

Scientific revolution without any revolution whatsoever:

The psychology of the 21st century as ‘eternal return’ to its roots

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In French, “Bande-annonce” means nothing else but a “trailer”: a short movie clip a few minutes long created for promotional purposes in order to advertise for a new cinematographic piece to be shortly released on the screens of movie theaters. Yet, if read through the eyes of a German-speaker, this very phrase—with certain liberty taken—can be interpreted as something like an classified advertisement (“Annonce”) for a new book, a volume (“Band”) to appear. And true, a new book is to come out in more or less distant future. So, this article can be regarded as an announcement of the new book of this kind. In the meantime, let us have a closer look at the terminological monstrosity in the title: the “cultural-historical gestalt psychology”. This phraseological creature hardly walks and will never fly: obscure, mysterious and repulsive, it might weakly crawl at best, if only survives. The reader of these lines is pleaded to forgive me the irony of saying all this, but the phrase is really awkward and most likely confusing, indeed. And yet, here it is: here it stands as the subject matter of this very paper that not only presents an intellectual provocation (hopefully, in the positive meaning of the word, like in the expression “thought-provoking”), but also offers what I believe is a really new and exciting pathway in this science of humans.

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The co-occurrence of these very words in this particular order is not entirely incidental, though. In fact, there are two reasons I use this very phrase in the title of this paper. First, I have already used it earlier on a number of occasions, such as (Yasnitsky, 2012e, 2016a), and it would be a real waste of the effort if I resisted the temptation to revisit it and use it once more. Second, the subject matter of this paper is most closely related to a discovery of very curious transnational contacts, mutual visits and international travels, correspondence, ideas exchange and even scientific cooperation between the Soviet scholars of the so-called “Vygotsky—Luria Circle” and the German—American scholars of the so-called “Gestalt psychology” movement. Those earlier publications on this topic highlighted the strange scholarly symbiosis, hinted at the “beginning of a beautiful friendship” between intellectuals across the state and disciplinary borders and, in a phrase, merged the two intellectual traditions that are hardly associated in the minds of the majority of global international academic community. In this sense, the earlier research and this terminological coinage did the due work and established the phenomenon. Yet, the time is up to move way further—and deeper—than that.

Formally, virtually all individual scholars of this double-headed intellectual domain belonged to and identified as representatives of a specific scientific discipline: Psychology. Thus, this study seems to be of primary interest to psychologists, all the rest do not need to bother, it seems. Nothing can be further from the truth, though. In fact, in order to meaningfully discuss the subject matter at stake I suggest a mental exercise: depart from the discipline of psychology as we know it nowadays and assume somewhat different perspective and a different frame of reference. As a matter of fact, I have a really good word in order to refer to this vantage point. The word is: “anthropology”. Again, I have two reasons for doing so: the methodological and the theoretical one. Let us discuss these two, one by one.

Context and background

This paper can be best understood as “half-way house” (in the sense of an old North-American phrase for a hotel or an inn midway between two towns—more like a “motel” in contemporary sense) where a traveler made a stop in order to have some rest and reflect on the book in progress that, as it has been announced in the preceding paragraph, is to come about some day. The unfinished manuscript of the book was once presented as an invited talk and is currently available online.

Then, considering the targeted readership, the potential readers of this paper predictably consist of two major groups: first, these are specialists in gestalt theory (its history, ideas and their applications), Soviet studies researchers, especially in the field of the history of Soviet psychology, including the so-called “Vygotskian” scholars; second, it is hoped (and intended) that other scholars might be interested, who identify themselves far outside these relatively narrow fields and quite possibly even outside the confines of psychology research proper—such as disciplined anthropologists, linguists, philosophers and even, possibly, cognitive studies specialists. The representatives of the latter group—unlike those from the former one—might well (and, perhaps, are even advised to) skip the first part of the paper that deals primarily with the historical issues of the “cultural historical gestalt” and proceed directly to the second one that attempts to present some ideas on the actual contents of this new (or not so new) theoretical proposal, albeit very superficially, fragmentarily and in a few propositions only—suggesting further research, theorizing and a larger synthesis on the basis of these.

It seems a truism to claim that psychology as scientific discipline is in deep crisis. The earlier efforts to overcome this crisis—such as the “Soviet Marxist psychology” of 1930s, “cognitive science” and “humanist psychology” of 1960s or the “cultural-historical activity theory” of its North American and Western European variety of 1970s-90s—in retrospect do not appear as satisfactory as originally planned. Therefore, it seems quite appropriate to focus on the new ways of dealing with this crisis, yet in the context of the 21st century. The proposed pathway is the development of a trans-disciplinary (or even a-disciplinary) field of knowledge that for the lack of a better name or a specific label has been earlier described in terms of an “anthropology/psychology/philology”.

The starting point is the decade old proposal for an integrated “cultural historical gestalt psychology” (Yasnitsky, 2012c, 2012a, 2012e, 2012b, 2016a), yet now it will be discussed here in much broader terms (that is, first, as a “theory” without any specific disciplinary affiliation) and, ultimately, under a different label of “pragmatic anthropology”. Furthermore, this initiative seems to lead to yet another proposal: the bold and ambitious call for a considerable reassessment of the history and the historical meaning of the “Gestalt psychology” of the “Berlin school” —especially, through the idiosyncratic lens of “Soviet Marxist psychology” of the interwar period. The discussion of this proposal is exactly and exclusively the main topic of this very paper.

Personally, my interest in this topic stems from my earlier work on the so-called “Kharkov school” (alternatively, “Kharkov group”) of Lev Vygotsky’s associates and their followers in Kharkov, Ukraine in 1930s (Yasnitsky, 2008, 2009a; Yasnitsky & Ferrari, 2008b, 2008a) that was then pretty soon superseded by an interest in the larger “Vygotsky Circle” as the topic of my doctoral dissertation at the University of Toronto (Yasnitsky, 2009b) and then, even the larger circle of scholars that included the peripheral members (as I referred to them then) of the Vygotsky Circle outside the Soviet Union. These were primarily German, later (i.e., after 1933) American psychologists of the Gestalt tradition (Yasnitsky, 2012c, 2016a). The discovery of the increasing proximity of the Soviet scholars and their foreign peers was quite surprising, exciting and thought-provoking, to say the least. Indeed, considering the seemingly eternal fragmentation of psychology into numerous “psychologies”, whose representatives can hardly meaningfully and productively communicate with each other, the studies revealed the intense transnational dialogue between these two large groups of scholars that at first sight had virtually nothing in common. Besides, consider the obvious fact that they were separated and divided by state borders, linguistic constraints, scholarly traditions, political regimes and ideologies of the countries of their residence, their research topics, and, finally, the methodological and philosophical backgrounds of their research. And yet, apparently, they did have a common language (meaning: the language of science, a *discourse*, rather than natural ‘linguistic’ language such as German, Russian or English) that allowed them communicate their ideas and scholarly insights across the multiple divides. This discovery definitely required a further in-depth investigation into what I somewhat provocatively termed back then as “cultural historical gestalt psychology”.

Lev Vygotsky, the members of his Circle and, broader, the tradition of Soviet Marxist psychological research provide an important new perspective on the history and theory of gestalt. Indeed, this tradition of research in the Soviet Union was strong and important. Overall, it counts for a full-fledged “Soviet gestalt psychology”, for the lack of a better term. The scholarship as an integrated and

interrelated whole has never previously been systematically studied and definitely deserves further exploration in the uncharted waters in the hope it will give us new exciting insights into its new, productive and promising developments in this 21st century. This is the first “take-home message” of this paper, yet not the only—and definitely not the main—one.

Despite the importance of the “Soviet gestaltist” perspective, it turns out it is Kurt Lewin (1890-1947) who is an actual central figure of this paper as an involuntary broker and an important link between the two scholarly traditions. On the one hand, he had numerous personal and intellectual ties and interconnections with of the “Vygotsky-Luria Circle”; besides, his work served as the source of inspiration for a few Soviet scholars even outside this Circle and well after Vygotsky’s death. On the other hand, a peripheral member of the “Vygotsky-Luria Circle”, he was also at the same time one of the core individuals within the “gestalt network” and an active participant of the gestaltist movement: first in Germany, then, after 1933, in the United States. Indeed, Kurt Lewin and his work can hardly be adequately understood unless in the context of the activities of a larger network of gestaltist scholars and their foundational ideas and principles.

Therefore, the second and, perhaps, the main message that this paper conveys is the discussion of the question of how this seemingly unexpected discovery of “Soviet gestaltism/Lewiniana” is going to impact our traditional understanding of the history of the “Berlin School” (rather: “Berlin Circle”) of intellectuals and researchers, and, furthermore, the meaning of gestalt theory as such.

I suggest, a productive way to treat these issues is the perspective of “informal personal networks” and “circles” as their hubs. In the anticipation of the due argumentation and the final conclusion, I would like to give the reader a hint: our traditional views will need to undergo a change, and perhaps quite a considerable one, indeed. If so, then, this is going to become the second and, perhaps, the most important “take-home message” of this paper.

Finally, I would like to point out that this paper is written in English with the English readership in mind. A number of the claims I make might look like truisms, for instance, from the perspective of the German readers, who might be well familiar with these proposals as substantially discussed in the literature in this language. Yet, on the other hand, these ideas might appear as fairly novel to the intended English readership with no German language reading skills. I still believe this topic is of primary importance now and, furthermore, I am certain the time has come for a systematic large-scale analysis and discussion of the history, theory, methodology, and empirical findings made within this transnational intellectual tradition and its promise in the context of the 21st century.

The discussion presented in the first part is based on my earlier—somewhat tentative and preliminary—text on “Field theory” (Yasnitsky, 2014a) that was written for and published in a multivolume “Encyclopedia of Critical Psychology”. The word “critical” in the book’s title is not incidental, and that small paper was intended to discover the unusual facts and perspectives, the “anomalies” that are frequently ignored in the traditional narratives. Now, I would like to revisit this earlier research, question some of its proposals and expand its findings beyond the limits of its original, relatively narrow topic.

Anthropological perspective

“Anthropology” that we have known it as a descriptor of an institutionalized scientific discipline among other—possibly related—disciplines can mean virtually anything and include a wide range of scholarly topics and problems of very diverse nature. On the other hand, scholars from apparently distinct disciplines (including Anthropology and Psychology, mind the capitalization) sometimes study very same (or extremely close) topics and problems; furthermore, they might well use very similar methods of research. That is definitely confusing. Indeed, quite often people—especially scholars—invent words and then start fights in order to understand what they mean, how they are different from other words with a similar meaning, and what the relation—if any—of these words to the so-called “reality” is. The whole battlefield is lying ahead of us, enshrined with a few figures of well known and academically decorated scholars—the actors and the spokespersons, the already dead and those still alive—under whose banners are gathered other academics, less prominent ones, willing to fight for specific idiosyncratic interpretations of the words (typically, ending with “-ism”, “-ity”, or “-tion”) and their meanings in the service of increasing their “scientism”, “sciencism”, “scientificity”, “sciencization” or “scientification”, not to mention the assumed radical distinctions between the five (or even more). How this normally happens in scientific discourse—and social practice—is described, for instance, in the beautiful book of Michael Billig under the telling title ‘Learn to write badly: How to succeed in the social sciences’ (Billig, 2013). Everyone is invited to join the fight, if only we are ready and eager to submerge into the discussion of these terminological and linguistic distinctions. The only risk is, one can hardly predict when we—alive or otherwise—will be able to emerge out of this battle, what the result would be, and whether we would need it for the purposes of this specific little paper. There is an alternative, though.

The alternative is not in joining forces with specific most respected Great Men of Science (no gender or sexist implications) and their devoted followers and, furthermore, confining ourselves within the disciplinary borders of specific institutionalized field of knowledge, but on the contrary: decisively break away from any disciplinary loyalty and self-identification in pursuit of the Truth in and about the ‘science of the human’ in its broadest sense.

Thus, in this paper, I propose the perspective of anthropology (mind the non-capitalization) and conceive its meaning based entirely on its original Greek etymology. This way, ‘anthropology’ is nothing else than the ‘science of man’ (again, no sexist or gender connotations implied). Then, I find it reasonable to stick to the notion that Anthropology (as an institutionalized scholarly discipline) is constituted by the four sub-fields of Archeology, Linguistics, Biological (alias: Physical) and Cultural/Social Anthropology (the well-known—illustrative and playful—metaphors of “stones”, “tones”, “bones” and “thrones” for the four, respectively, might help remembering these sub-fields of Anthropology and understand the differences between them). Thus, we understand that Anthropology—if construed this way—might equally deal with a *historical past* (and archeological findings) and the *present*; the *ethnically-related* studies (like those of the studies of languages, contemporary and extinct), but does not have to limit itself exclusively to ethnicity research, therefore, might study *a range of cultural and social groupings*; it might equally get involved in the *materiality* (like

that of archeological, anatomical or physiological nature), but is also inseparable from the *ideology* (that inevitably comes with the interest in languages and culture). Besides, although it is commonly assumed that anthropological studies are focused primarily on social groups and larger super-individual formations, I would argue that the field of anthropology can be easily extended to a study of singular, *individual cases of specific persons*—which will certainly make much sense if only we agree on the postulate of the social origin of human mind—also known as “sociogenesis” (Valsiner & Van der Veer, 2000)—and essentially social character of human thought, will, emotions and actions, even in the situations when a person is left to an apparently solitary and, seemingly, socially independent course of existence and action. In order to clarify what exactly I mean by making this claim and give the readers some food for further thought in this direction I would like to quote these nice lines from a prominent thinker of the 19th century, whose psychological and anthropological insights have been considerably (and regrettably) overclouded by his sociological and, even worse, political ideas:

Social activity and social enjoyment exist by no means *only* in the form of some *directly* communal activity and directly *communal* enjoyment, although *communal* activity and *communal* enjoyment— i. e., activity and enjoyment which are manifested and affirmed in *actual* direct *association* with other men— will occur wherever such a *direct* expression of sociability stems from the true character of the activity's content and is appropriate to the nature of the enjoyment. But also when I am active *scientifically*, etc. — an activity which I can seldom perform in direct community with others—then my activity is *social*, because I perform it as a *man*. Not only is the material of my activity given to me as a social product (as is even the language in which the thinker is active): my *own* existence *is* social activity, and therefore that which I make of myself, I make of myself for society and with the consciousness of myself as a social being².

(Marx, 1975, p. 298)

This is a most beautiful description of essential “sociality”, or “sociability”, of a human being as such and the source of eternal inspiration, among a number of similar ones by a wide range of other thinkers, yet to the same effect. Now, let us get back to the proposal of a four-partite composition of Anthropology. The introduction of these four subdivisions is commonly attributed to German-American scholar Franz Boas (1858–1942), who is acknowledged as the founder of Anthropology as an academic discipline on the American soil. For the purposes of this specific study, the “stones” and the “bones” as such are of no use, it seems. Yet, by the force of a mental exercise we can turn these into some other comparable entities that might be quite helpful as stepping stones and research tools in our study of the “cultural historical gestalt psychology” as the main subject of this paper. Thus, should we ask ourselves what

² In the original German: “Die gesellschaftliche Tätigkeit und der gesellschaftliche Genuß existieren keineswegs *allein* in der Form einer *unmittelbar* gemeinschaftlichen Tätigkeit und *unmittelbar* *gemeinschaftlichen* Genusses, obgleich die *gemeinschaftliche* Tätigkeit und der *gemeinschaftliche* Genuß, d.h. die Tätigkeit und der Genuß, die *unmittelbar* in *wirklicher Gesellschaft* mit andren Menschen sich äußert und bestätigt, überall da stattfinden werden, wo jener *unmittelbare* Ausdruck der Gesellschaftlichkeit im Wesen ihres Inhalts begründet und seiner Natur angemessen ist. Allein auch wenn ich *wissenschaftlich* etc. tätig bin, eine Tätigkeit, die ich selten in *unmittelbarer* Gemeinschaft mit andern ausführen kann, so bin ich *gesellschaftlich*, weil als *Mensch* tätig. Nicht nur das Material meiner Tätigkeit ist mir – wie selbst die Sprache, in der der Denker tätig ist – als gesellschaftliches Produkt gegeben, mein *eignes* Dasein *ist* gesellschaftliche Tätigkeit; darum das, was ich aus mir mache, ich aus mir für die Gesellschaft mache und mit dem Bewußtsein meiner als eines gesellschaftlichen Wesens.” (REF: MEW, B. 40, 1974, S. 538).

constitutes the “archeology of human sciences”, then, keeping in mind the seemingly immaterial intellectual production (i.e., thoughts, ideas, hypotheses, theories, etc.) as the essence of the scholarly craft, I would offer Archival Studies and Textology for an answer. The similarities between the two and Archeology (each to its purposes) are obvious, it seems, therefore, the “stones” turn into the “archival manuscripts” and “written materials”. The bones of Physical/Biological Anthropology, then, may be translated in “photographic materials” that depicted and preserved the body images of the physical stature, gesture and facial expression of our characters as they lived. As a research and an author, who happened to pen a full-scale monographic biography of at least one prominent scholar (Yasnitsky, 2018) and on the basis of my personal experience of dealing with the challenge of writing about human beings, I must say all these have been very important to me in my effort to understand the thought, the life and the psychological make-up of the protagonist of my story and other man and women around him.

Then, having clarified the relevance of the anthropological “stones” and “bones” within our research methodology, we have virtually no difficulties in dealing with the “tones” and “thrones”. These two metaphors cover the wide range of topics such as: the social and cultural aspects of human life, their linguistic and discursive characteristics, power relations and political issues, profoundly personal and interpersonal (such as social ties, interconnections and communications) and essentially psychological matters (for instance, the issues of personal attachment, envy, competition, ambitions, angers and fears, love and hate, curiosities and drives, interests and repulsions, respect and disgust, etc.)—all these should and will play the main roles in this story (if not explicitly all of these, then will certainly figure somewhere on the background). All these constitute the deliberately *anthropological methodology* of this study as distinct from the methods of the traditional—and, more often than not, rival—disciplines such as psychology proper; social, cultural and/or intellectual history; sociology (of science; alternatively: social studies of science, the SSS; the social studies of knowledge, the SSK, etc.) or even anthropology/ethnology/ethnography as such. Even more precisely, perhaps, this study qualifies as a *cultural anthropology of science*.

The author of this paper would probably—proudly and boastfully—claim the original authorship of this terminological coinage, has he not—quite with relief—discovered the use of this phrase in earlier publications of similar nature—not to be mentioned here, unless the reader (and the author) is ready and eager to get involved in the terminological and disciplinary fights and battles over the meanings of the words and phrases. The answer is negative: we seem to have agreed not to do so; not in this very paper, at least. This is the first reason for an “anthropological turn” this paper takes: the methodological one. Yet, there is another reason why the use of *anthropology* is particularly pertinent in this case. Let us briefly discuss it.

Considerable work has been done that demonstrates numerous and diverse *social* interconnections between a number of prominent Soviet Russian, German and American scholars of mid-20th century: these are somewhat briefly discussed in the ‘Methodological considerations’ section of this paper. Yet, the effort will be wasted unless the true essence of these interconnections is revealed and substantially discussed: the *intellectual* interconnection between these beautiful minds. In fact, this paper is the call for a discussion of this very intellectual interconnection, its theoretical impact and, especially, its promise for the future development of this science in our 21st century context. Thus, perhaps the main

argument of this presentation is that the interconnection between the protagonists of this story and their ideas can be best understood not within the disciplinary field of *psychology* (e.g., developmental, cultural, humanistic, clinical, or neuropsychology), as fragmented and disconnected as it stands now. Moreover, the disciplinary borders of psychology as we often understand it these days will obscure very important aspects of the main object of this study: the intellectual legacies of the thinkers about human life, who are featured in this paper. That would be a terrible and unforgivable loss. Therefore, I am making hereby a bold claim that this integrated project—as a whole and even in its main constitutive components—is an *anthropology*, but, quite possibly, not the one that we have known before; as a consolation for the loss of the exclusively psychological perspective, we could think about it as *psychology/anthropology*, instead. Before we proceed any further, a few extra considerations of methodological nature need to be discussed.

Linguistic considerations: language, style and discourse

In essence, this paper is mainly about “German psychology”, which is declared here on purely linguistic grounds as the scientific legacy written and communicated in German. This way, the international scholarship of researchers and thinkers from Germany, Austria, and—even broader—a wide range of continental European authors who authored their works in German must be included. In its extreme, even translations into this language belong to this domain of “German psychology”, from consistently linguistic standpoint.

Yet, the paper is still in English. Thus, I will deal here mainly with the English sources and the idiosyncratic Anglophone perceptions, predilections and interpretations. This is a conscious and deliberate limitation of this paper: I explicitly pronounce this here, in the hope that subsequent discussion of this piece of writing among international scholars of different, not necessarily English-speaking circles, will help rectify this limitation, quite possibly, in their own languages. Whether we like it or not, English is the *lingua franca*—the common and the dominant international language—of scientific research and communication in the current, 21st century. It has not always been so. One might be surprised to discover that from the second half of 19th century until, roughly, the period of 1920s and 1930s (i.e., between the two World Wars) was the time of a “linguistic triumvirate” in international academia that saw the proliferation of the three main languages of international science—German, English and French (M. D. Gordin, 2015); furthermore, quite for a while it was German that dominated the publication landscape and was particularly important in the field of psychology, anthropological, medical and human sciences. Not anymore. The *Zeitgeist* in academia over the last half century or so has definitely changed: after a relatively moderate *ouverture* of the early Cold War era, we apparently reached the *crescendo* by the 1970s, then, passed a period of a *perestroika* in the linguistic *repertoire* in the languages of science; as a result, *in lieu* of the “linguistic pluralism” of the previous era, the *Migdal Bavel* (i.e., the Tower of Babel) in the *parlance* and *discourse* of the international scientific research and communication has been finally (and successfully) built, the “linguistic imperialism” of English as the *koiné* and *lingua franca* has become a *fait accompli*, thus—*de facto* and *ipso facto*—the *Blitzkrieg* is triumphantly over, manifested by the *coup de grâce* to the “linguistic triumvirate” of the preceding

epoch, albeit to the yet unknown *finale*. Indeed, times come and go, and *sic passit gloria mundi*: mind the eternal wisdom of the *Kohelet* (i.e., the biblical Ecclesiastes).

In somewhat more comprehensible vernacular of the contemporary scholarship, this means that the dominance of English does not have to stay forever, and there are several reasons for the possible change. Specifically, in international academia this change might be triggered by a “sudden discovery” of different linguistic domains and cultures in scientific research that have something really important to offer and, thus, fill the gaps in the international scholarship as it stands now. This is one of the key take-home messages of this very paper: the time for a closer look at the non-English sources and ideas has definitely come, and these can be found in the scholarly cultures outside of the Anglo-Saxon world, primarily in the polyglot German—Russian scholarship, in many ways still obscure to the exclusively English-speaking reader or the English reader not familiar with either of the two intellectual traditions (regarded in isolation from each other) or the unified whole sum total of these. In the field of the science of the human that is discussed here under the label of “psychology/anthropology”, a range of other cultures in academia have also a lot to offer to the Anglophones such as the Francophone, and, increasingly eager and avid Hispanophone and Lusophone ones (i.e., those associated with the French, Spanish/Castellano and Portuguese languages), but these are beyond the scope of this paper. This is only the first, but definitely not the last, linguistic consideration that I would like to discuss here.

The first point concerned the natural languages that people speak (and write), the intellectual and cultural traditions (especially, in science), not to mention the power relations in this global world of ours; the latter subject has not been explicitly mentioned here, yet only subtly hinted at. Now we proceed to the second point. As it has already been established above, the centerpiece of this article is the ideas that scholars deal with. Ideas are immaterial indeed, yet they have immediate material and objective representation: words and phrases. Wilhelm von Humboldt (1767-1835) famously claimed and taught that a single word is a metaphor that has its “inner form” and as such is a *myth*, a *legend*, or a *narrative* (that is, something that can be ‘said’/‘pronounced’, ‘read’ and ‘told’/‘related’, respectively, judging from the perspective of the etymology of these originally Greek and Latin words). These, in turn, when substantially elaborated and explicated in proper scholarly parlance, can unfold into ideas, hypotheses and, further, scientific theories of all sorts. In order to materialize ideas in this sense, we need instruments and materials: in this instance, the linguistic ones. Yet, the toolkit each specific natural language of humanity has to offer its users provides them with quite different tools—and critically different, in specific cases.

Indeed, languages—their phonetic and graphic systems, vocabularies, grammars, rules of style, etc. differ. Furthermore, depending on the extent the speakers (and writers) in a language have contributed to its adaptation to expressing formal, abstract thinking generally—and systematic, rigid scholarly thinking in relation to particular problems, fields of knowledge, later, specific scientific disciplines—languages also differ considerably. Considering the latter point, for the purpose of an illustration, one can think about specific languages that have had no (or minimal) written tradition of scholarly writing (including theological and philosophical works, of course)—or even, no written tradition of their own whatsoever—and compare these with well-developed, age-old cultural traditions of the kind as represented by a number of languages such as (Ancient) Greek, Latin, Chinese, Arabic, Sanskrit, English,

German, French, or Russian, to mention just a few, prominent throughout the history of human civilization. Similarly, in this context, one can avoid recalling the idea that languages shape, impact or even—taken to its extreme, controversial, much disputable and contested—predetermine the actual content of the thought of their native speakers. This proposal is well-known in linguistics (and not only) as the so-called “Sapir—Whorf” hypothesis of linguistic relativism, which is strongly associated with the two names of its originators, the American scholars of the mentioned Franz Boas’ Circle: the linguists/anthropologists Edward Sapir (1884-1939) and Benjamin Lee Whorf (1897-1941), the student of Sapir and, via the latter’s mediation, Boas. In order not to get distracted from the main line of argumentation in this paper, we should certainly avoid further in-depth discussion of the essence, history and the contemporary status of this scholarly puzzle, especially since scholarly literature on this matter is really immense and an interested and avid reader is welcome to get familiarized with it; for some introductory reading see, for instance, (Whorf, 1956a). Yet, while fully sympathetic with this hypothesis and its implications, I would like to remind the reader about the idiosyncrasy of *academic discourse* (as a somewhat artificial subset of a *natural language*) that notably distinguishes it from the native *language* as it is presented to all of us as early as the first days of our lives.

Scientific, scholarly discourse, from purely linguistic (particularly, sociolinguistic) standpoint, is a special genre—a “functional style”, in some nomenclature (Chloupek & Nekvapil, 1993)—of thinking-and-writing that does not come about naturally: it emerges gradually, starting from the age of high school the earliest and develops throughout the years of schooling during the after-school period of enculturation into the scholarly environment with its idiosyncratic norms, mores, values, ideas, methods and research techniques, and ways of academic writing composition and publication. All these are mastered through a continued exercise and persistence; furthermore, even not all university graduates are guaranteed and assured fully acquiring all those skills and crafts of scholarly discourse active production—unlike their ability to passive reception (i.e., reading and comprehending), at best—by the end of the first stage of their formal learning in academic settings: for a great many of young scholars this ability is still to be developed in the course of their post-graduate studies and scholarly activities. Furthermore, the discourse of scholarly research, from philogenetic standpoint—that is from the perspective of the historical development of the human species (as opposed to the ontogenetic perspective, namely, the individual development of a person throughout his or her lifetime)—is also dynamic and is being deliberately, consciously, and not infrequently even forcefully and artificially developed by the individual intellectuals, writers and the entire scholarly communities that these intellectuals represent and consist of. One of the earliest and the most memorable, perhaps, examples of such deliberate and creative linguistic activity in the history of European civilization was Marcus Tullius Cicero, who—as early as two thousand years ago—was complaining about the inability of his native Latin to express the full depth of Greek wisdom and, at the same time, was actively developing the linguistic toolkit that would enable him (and his learned compatriots and other users of Latin) to speak and write academically in his native language. Thus, the active use of a national language to do philosophy and science and the conscious agency toward transforming the linguistic tools (primarily, the vocabulary and style) seems to present an argument that somewhat undermines the implications of the Sapir and Whorf hypothesis in terms of its determinism and the pervasiveness of its assumed relativism, at least, as long as scholarly craft is concerned.

And still, if not in its most radical forms, the Sapir-Whorf hypothesis holds. We have experienced—and keep experiencing in our everyday practice of scholarly work in multilingual research communities—the difficulties of translation. For a few illustrations, the reader is invited to ponder the complexities of the interrelations between German “Seele” and “Geist”, Russian “dusha”, “psikhika” and seemingly their English equivalents “mind”, “soul”, “cognition” and “psyche”. The methodology of science must be deeply affected by the ambiguities of the duo of English “subject” and “object” in their application to the trio of Russian “predmet” (as the ‘topic’ or the ‘object’ of research), “ob’ekt” (as the ‘what’, the ‘thing’) and “sub’ekt” (as the ‘who’, the ‘actor’ or the ‘agent’) or, even worse, German “Subjekt”, “Objekt”, “Gegenstand”, XXXXX. Further complications come with another cluster of equally essential psychological terms such as “Handlung” and “Tätigkeit” (German), “aktivnost”, “deistvie” and “deiatel’nost” (Russian), and “act”, “action”, “behavior”, “activity”, “agency” and even “activeness” (English). Besides, the reader of this of this journal hardly needs to be reminded about the seemingly everlasting difficulty of rendering a simple German word “Gestalt” in virtually any other language. Finally, the interested reader can find an extensive discussion of the “loss” that keeps occurring in English translations of the whole semantic field of “consciousness—meaning—sense” and its extremely well developed vocabulary of derivatives that, grammatically, are represented by semantically closely interrelated verbs, adjectives, action-nouns, etc. in Russian (Yasnitsky & Van der Veer, 2016a). In contrast, I would remark, these terminological complexities of Russian might well be unknown to a German-language reader, if only properly translated with full awareness of the importance of all these terminological distinctions and similarities: the simple reason is that German appears to provide its users much of the linguistic toolkit that can be found in Russian, but is still missing in English. With these examples as some food for further thought I would like to conclude my second linguistic consideration. And yet, there is another—the last one.

The third linguistic consideration concerns primarily the sociological, rather, sociolinguistic matters: necessarily, albeit briefly. Languages obviously exist in and belong to social contexts of their use. As long as societies are concerned, one can hardly avoid the topics of power, ideology, politics, and propaganda of various sorts. True, scholars consciously and deliberately develop their discourse in order to be able to talk about the worlds of ever new ideas, theories and discoveries: this is the essence and the primary obligation of their craft as such. Yet, that said, it is not only and exclusively scholars and intellectuals, who keep always developing new discourses and adjusting them to new realities. Politicians and political activists of all sorts—although for very different reasons—do the same: for the most recent and contemporary examples think about the proliferation of the *politically correct* parlance that is being increasingly demanded these days. Apparently, the postulate of political correctness urges some societies to artificially change natural languages and adjust them to political agendas of the ideologies of the time and place. This process can be assessed differently. Yet, one is invited to think about the extreme examples of the radical political interference in language use and change, such as those in Nazi Germany or, say, Soviet Union. Specifically, the Bolshevik activities and closely associated scientific research in the Soviet Union have gradually developed an idiosyncratic “doublespeak” of Soviet science that possessed an incredible plasticity of operating—and manipulating—scholarly discourse to discuss scientific matters in equally politically correct and scholarly meaningful ways. The backdrop of these sociolinguistic developments was virtually total inability understanding the intricacies of the virtually

inseparable truth-and-lie telling in Soviet scientific discourse for those foreigners outside the socio-cultural context of Soviet Communist autocracy unfamiliar with the ground rules of these language games. Further elaborations on and discussion of these matters can be found in scholarly literature such as (Gerovitch, 2002; Kremontsov, 1997; Yasnitsky, 2009b).

In conclusion of this section (the one on ‘linguistic considerations’) of this very paper, I would like to propose an amendment to the claim that I made at the very end of the previous section (the one on ‘anthropological considerations’). Thus, it appears, from the disciplinary standpoint, we are dealing neither with a four-partite Anthropology (that is composed of Archeological, Physical/Biological, Linguistic and, finally, Cultural/Social sub-fields) and Psychology, nor even with a *psychology/anthropology*, as somewhat adventurously proposed above. More precisely, the phenomenon we are presently dealing with can be best described as *psychology/anthropology/philology*—or, rather, *anthropology/psychology/philology*—where ‘philology’ (*φιλολογία* in Greek, *filologiya* in Russian and *Philologie* in German) is understood in its original meaning of the “love for word”, namely, an integrative scholarly discipline dealing with languages (alternatively, Linguistics), literatures (alternatively, Literary Criticism or Literature Studies), speech/discourses (alternatively, Rhetoric, Communication and Narrative Studies), and cultures—modern, contemporary and classical, historical ones.

I argue that only from this perspective we will be able to approach a holistic understanding of the human species in the past and present, and to predict its further evolution on the path to the potential future over-man (also known as the *Übermensch*), if only it survives as such in the more or less reasonably close historical perspective, of course.

Anthropology/psychology/philology as the science of the human

This proposal might strike someone as novel (therefore, daring, overambitious, provocative, arrogant, outrageous, etc.), yet, there is nothing of novelty here. Indeed, it is deeply rooted in the centuries-long tradition of thinking about humankind with its roots as far back as the end of the 18th—early 19th century, specifically, the work of Wilhelm von Humboldt, a high-rank state official as well as a “linguist”, “philologist”, “ethnographer” and “anthropologist”, “psychologist” and “philosopher”. Apparently, all these disciplinary distinctions are totally meaningless, given the high interconnectedness of his ideas in all these—subsequently different disciplinary fields in the later era—and the essentially holistic standpoint of the person. Then, the same very attitude and scientific methodology can be found in the work of the legendary “founder” of the so-called “empirical” or “experimental” psychology, Wilhelm Wundt (1832-1920): most remembered and glorified (especially, in North America, it seems) for the founding of the first psychological laboratory in Leipzig in 1879, but virtually totally forgotten as the author of the monumental (roughly, 5-6 thousand pages long) *Völkerpsychologie. Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythos und Sitte*. In order not to mislead the reader, I prefer to keep the word *Völkerpsychologie*—originally coined and actively promoted by post-humboldtian scholars Moritz Lazarus (1824-1903), and Heymann Steinthal (1823-1899), in the mid-19th century—as it stands in

the original. Yet the subtitle translates into English approximately thus: ‘The investigation into the laws of the development of language, myth and cultural/behavioral/social norms’. The scope of the topics covered in this truly *opus magnum* that came out in 10 volumes of 1900–1920 (of a few editions—revised, reedited and reordered) really impresses and hardly leaves one in doubt about what the author truly meant by the sphere of interests of a “psychology”—apart from the “physiological psychology” (*physiologische Psychologie*), of course. These are: (a) language (two volumes: 1.-2. Bd. *Die Sprache*); (b) art (another volume: 3. Bd. *Die Kunst*); (c) myth and religion (three volumes: 4.-6. Bd. *Mythus und Religion*); (d) society (another couple of volumes: 7.-8. Bd. *Die Gesellschaft*); (e) right (a volume on the law, the legal system; 9. Bd. *Das Recht*), and (f) culture and history (the final volume of the whole set: 10. Bd. *Kultur und Geschichte*). Sadly and most characteristically of the “social construction of knowledge”—the topic that is further discussed in the next section of this very paper—the 10-volume *opus magnum* has never been translated (into English), therefore, virtually never read in the context of English-speaking psychological community. Finally, a still doubtful reader (if any, at this point) should also be reminded that in terms of his institutional affiliation, Wilhelm Wundt, the “founder of contemporary psychology” (whatever that means), was employed for a decade in 1860s-70s in Heidelberg University as a professor of Medical Psychology and, indeed, *Anthropology*: the position of the “*Professur für Anthropologie und medizinische Psychologie*”, according to its title in German. In retrospect, the idea of anthropological origin of psychology would hardly seem novel even in the Wundtian times, that is, a century or so ago. *Finis coronat opus*: the reader might probably need to get reminded about the final work and, arguably, the definitive *opus magnum* of Immanuel Kant (1724-1804) that was published under the title *Anthropologie in pragmatischer Hinsicht* in 1798, prepared on the basis of the material of his most popular and widely attended lectures that Kant presented to his audiences at the Albertus Universität in Königsberg for almost 25 years; for the text see, for instance, a new republication of the book (Kant, 2000). For discussion of Kant’s “pragmatic anthropology” (*pragmatische Anthropologie*) and the argument for its promise in the 21st century research and practice see (Cohen, 2009, 2014; Lorini & Loudon, 2018; Sturm, 2009). The “anthropological interlink” between Kant and Wundt is firmly established both in the writings of the latter and in the subsequent contemporary discussion of the matter; see, for instance, (Fahrenberg, 2008). Yet, on the other hand, it might also be well argued that the effort of Wilhelm Wundt, the founder of *experimental psychology* as a distinct scholarly discipline, did not quite meet Kant’s requirements for his *pragmatic Anthropology* as an integral science of the humans.

The founding of a *Psychological Institute*, the invention and the introduction of *psychological laboratory* isolated from the world in all its exciting diversity and complexity as we experience it every day, and the establishment of *psychology* as allegedly a unitary and integrated, independent scholarly discipline might thus have been the most *tragic* mistake in the history of the Human sciences (that is, *sensu* the “sciences about humans”) that caused, perhaps, the most terrible blow to this intellectual field as a holistic enterprise; maybe not *the most* tragic one. Thus, in this context I can hardly resist the temptation to place here a quote from Benjamin Lee Whorf:

Psychology has developed a field of research that may no doubt be useful or valuable in itself, but it throws little or no light on problems of the normal human mind or soul. The person who

wishes to understand more fully the laws and, so to speak, topography, of the inner or mental life is as much thrown back on his own difficultly acquired store of wisdom and his native judgments, intuitions, sympathies, and common sense as though the science of psychology did not exist. Such a one, for instance, is the teacher, educator, sociologist, anthropologist, trainer, coach, salesman, preacher, manager, diplomat, executive: anyone who must deal with human intangibles, especially the man concerned in leadership of any sort. If he seeks aid from books, he will get far more information about this field from literature not intended to be scientific, that is, from the best works of the novelists, playwrights, and poets, than he will from any textbook of psychology. There are certain courses that psychology has elected to follow that have estranged it, perhaps permanently, from the truly mental field (Whorf, 1956b, p. 40).

These words of a prominent—albeit somewhat controversial—anthropologist/linguist resonate with those of another intellectual, a psychologist and, incidentally, the founder of Gestalt psychology. In his famous and programmatic talk ‘Über Gestalttheorie’, that the great Max Wertheimer (1880-1945) gave on December 7, 1924 at the meeting of the Kant Gesellschaft, he made a scorching remark: “People speak of idealism as opposed to materialism, thereby suggesting something beautiful by idealism and by materialism something gloomy, barren, dry, ugly. Do they really mean by consciousness something opposed to, let us say, a peacefully blossoming tree? ...Frankly, there are psychological theories and even plenty of psychological textbooks which, although they speak continuously only of conscious elements, are more materialistic, dryer, more senseless and lifeless than a living tree which has probably no consciousness in it at all” (Wertheimer, 1944, pp. 95–96).

Curiously, a blend of mainstream anthropology and psychology (specifically, quintessential gestalt psychology) can be found in the roots of the so-called “humanistic psychology” that has been often presented as the “third force” in the discipline of psychology in an opposition to the other two dominant forces in this field of knowledge and practice: the psychoanalytic post-Freudian tradition and North-American behaviorism (in Vygotskian terms, the “depth” and the “surface” psychologies, respectively). Carl Rogers (1902–1987) and Abraham Maslow (1908–1970) are traditionally credited for the introduction of the “third force” in 1950s-60s, yet it is the latter, who is primarily of interest to us in the context of this discussion: Abraham Maslow, the “10th most eminent psychologist of the 20th century”, according to an often quoted survey study published in the “Review of General Psychology” (Haggbloom et al., 2002). Indeed, it was under the strong influence and supervision of psychologist Max Wertheimer and anthropologist Ruth Benedict (1887–1948; yet another student of Franz Boas and a member of his close-knit Circle)—in the words of his biographer Edward Hoffman, Maslow’s “two favorite mentors” in New York—that “Maslow’s groundbreaking studies of self-actualizing people” were launched. The studies resulted in Maslow’s elaboration on the idea of “self-actualization”—as a matter of fact, borrowed from another prominent thinker and leading scholar of the German-American gestalt movement, Kurt Goldstein (1878–1965) and his original notion of *Selbstverwirklichung* (Goldstein, 1934, 1939)—and the “hierarchy of inborn needs” model (by mid-1940s). A decade later, Maslow presented these in copious publications on “the specific qualities he found among self-actualizers, including their frequent peak experiences, attraction for creative work, and yearnings for world betterment” (Hoffman, 2008, p. 441). A few remarks that the biographer makes, though, are pertinent and of import. Thus,

Maslow is rightfully praised for revitalizing religious psychology in a way no American academician had done since the days of William James [1842–1910]. Yet it is important to note that Maslow ultimately felt more comfortable studying entrepreneurs and business organizations than mystics, sages, and exalted states of consciousness. Indeed, this predilection explains the intellectual route Maslow took in his final years—and it may well have been due to the zeitgeist. Why? Because American culture in the 1960s increasingly came to associate mysticism with hedonism in a way that he abhorred. The hippies not only dismayed Maslow; they disgusted him (Hoffman, 2008, p. 442).

The “peak experiences” though, for Maslow, always carried some sort of—if not really socialist, yet—social-transformative flavor, it seems:

Maslow accelerated his involvement with corporate entrepreneurs... It was not for financial gain, for Maslow had always preferred a simple lifestyle—not even once traveling abroad. Rather, it was because they genuinely conformed to his sense of humanity’s “best” and “highest” people: hardworking, creative, innovative, achievement oriented, and concerned with world improvement. It is not surprising that Maslow sharply criticized Eastern monks and monasticism in general as “selfish.” He often revealed that his ethical ideal was the Hasidic *zaddik* or the Buddhist *bodhisattva*—who are actively “in the world” to raise the common good (Hoffman, 2008, p. 442).

Still, one has to keep it mind that “when Maslow decided on a psychology career, he was initially drawn to behaviorism... Although Maslow repudiated behaviorism quickly enough, he retained a strong loyalty to positivism throughout his life” (Hoffman, 2008, p. 440). Yet, Maslow’s lifetime predilection for positivism and behaviorism did not prevent him from eventually publishing his book “Religions, Values, and Peak Experiences” that, along with its second edition’s introduction of May, 1970 (written just a month before his death on June 8, 1970), can be considered as his “swan song”. This work is most remarkably reminiscent of Lev Vygotsky’s proposal—never fulfilled, though—of “peak psychology” of his own (again, as radically distinct from *Tiefenpsychologie*, the “depth psychology” of the Freudians or the “surface psychology” of the behaviorists). However, the main difference between Maslow’s and Vygotsky’s approaches is the objects and the ultimate goals of their proposals: while the former was apparently mostly concerned about the “last man”—*der letzte Mensch* of Friedrich Nietzsche—which seems to perfectly explain the popularity of Maslow’s ideas in North America. In contrast, the latter was obviously fascinated by the post-Nietzschean “overman” (*der Übermensch* of Nietzsche’s *Also sprach Zarathustra*), albeit understood in the footsteps of the Bolshevik prophet and political leader Leon Trotsky—his radical and clearly utopian prophesying—as a “new man”, or a “superman” of the Communist future (Yasnitsky, 2018, 2019b).

A critic might argue that this is all just a history—and “water under the bridge”, so to say—and the state of the arts in this science has very much changed since then. In response, I would argue, not so much. Consider an illustration of what I mean.

Cognitive Studies and CHAT as a failed integrative science

Let us mentally transfer to the end of 1940s in the United States and the state of the arts in the local domestic psychology: “national”, as they call it, which in the actual social practice of this discipline, quite unfortunately, often—way too easy—translates into “international”. American psychology of that time is frequently described as, arguably, dominated by the behaviorist mindset, and it was exactly this mindset that a new generation of scholars challenged in their research and practice. In retrospect, this is the origin and rationale of the contemporary Cognitive science and the full-blown so-called “cognitive revolution” of the late 1950s (as it was not labeled until early 1970s). In the context of this discussion, it is worthwhile to mention that by admission of an Austrian-born American psychologist—a founder and an activist of the cognitivist movement in the US—the ideas of cognitivism (at the launch of this intellectual project, at least) were deeply rooted in the legacy of European continental and German-American psychology, specifically, the gestaltist tradition (G. Mandler, 2002); in fact, not only was this tradition acknowledged in memoir literature retrospectively, but also it was actively used and explicitly referred to during the earlier period of the birth, social construction and institutionalization of the new, cognitivist trend as early as the mid-1960s, at least (J. M. Mandler & Mandler, 1964). Similarly, another founder of the American Cognitive science on a number of occasions reminisced about the remarkable similarity between the Soviet “Vygotskians” and their American cognitivist movement peers’ “battle for consciousness” that, in the Soviet case, originated in the context of domestic scholarly revolutions of 1920s and 1930s (Bruner, 1985, p. 22)—which clearly poses the Soviets in the avant-garde of the discipline, at least, from the North American perspective, given that “the “normal science” of American psychology during this period was clearly behaviorist” (Sokal, 1984, p. 1242)—and, in the post-WWII period, was reportedly waged in parallel in these two political “Superpowers” and their academic cultures (Cole, Bruner, & Sacks, 2013): in some ways similar, in many ways quite different—divided by geography (mind the Ocean) and politics (mind the Curtain; the “Iron” one, of course). Incidentally, the latter author, thinker and academic activist has left us a number of quite revealing writings and recorded oral statements—at least, partially—of a memoir character, such as (Bruner, 1983, 1985, 1990, 1995; Cole et al., 2013). So, it seems worthwhile to have a deeper look into these in order to get a better understanding of not only a historical past of this field of knowledge, but its present state and, quite likely, the course of its future development. After all, this is all what we eventually need history for; fairly useless, otherwise.

In his autobiographic memoirs of early 1980s, Jerome Seymour Bruner (1915-2016)—the classic of American psychology, a founder of Cognitive Science and, again, a militant fighter for the study of *consciousness* (whatever it might mean)—presented a story of fairly triumphant “revolution” in American psychology from behaviorism to cognitivism as, presumably and by analogy with the legacy of Carl Rogers and Abraham Maslow, could count as another, alternative variation of a “third force” in psychology in its opposition to post-Freudian and behaviorist theories alike. The author should by no means be blamed for what is called a “presentist” and a victorious narrative. By his own confession, Bruner is hardly the best objective witness or an unbiased judge on these matters, given his direct involvement and, to his great credit, self-acknowledged partisanship:

I was so engrossed by and involved in the transformation that it is still difficult for me to disentangle my own biases from the events that were unfolding. And I am enough of a child of the times to recognize that it is impossible, as Yeats says, 'to tell the dancer from the dance.' Several historians of that changing scene have even alleged that I had a major part in what happened, which should make me suspect as a commentator... We were all caught up in history. And I can only tell the tale from the perspective from which I viewed it. It was often a beleaguered perspective (Bruner, 1983, p. 68)

Besides, one should always keep in mind the idiosyncrasy of the genre: autobiography, almost by the very definition, is a narration about the time of the person's life when the author was young, full of hopes and energy, the sky was brighter, the grass greener, figuratively speaking, and so on. For somewhat similarly elevated retrospective accounts of the story see the publications of other "founding fathers" of the Cognitive science, such as (H. Gardner, 1986; Miller, 2003), augmented with another reminiscence of the kind that sheds the light on the role of the Soviet tradition in the American cognitivist tradition in the making, specifically, the alleged legacy and popular image of Lev Vygotsky—the visionary and pioneer of "cognitive development" research and a predecessor of cognitivist movement in the USA—as it was socially constructed at the time in America and across the Ocean (Bruner, 1985). Thus, for instance, it was in this publication that we find Bruner's confession that he liked Vygotsky's "instrumentalism" (that in essence was probably not that far from the domestic American pragmatism, for instance, of John Dewey, which might be the chief reason for the popularity of Vygotsky among the Americans and their intellectual allies). By "instrumentalism" though, we most likely should understand Vygotsky's stance and intellectual legacy of his earlier "instrumental period" of 1920s—reductionist, mechanistic, and eclectic—which resulted in "Vygotskian" scholarship that, in turn, Vygotsky remarkably criticized, revised and even rejected in his later, presumably, "holistic" and "structuralistic" writings of 1930s (i.e., a few years of 1930s until his death in mid-1934). The later period of Vygotsky life of early 1930s, though, as the reader should be reminded, was considerably influenced by and oriented to gestalt-psychology that Vygotsky on a number occasions presented—either correctly or not—under the label of "structural psychology", "*strukturnaia psikhologiya*": mostly that of Kurt Lewin and his numerous students, but also some others. The second important proposal that Bruner makes here goes thus:

Vygotsky's book [not quite correctly translated as "Thought and language"—as opposed to "Thinking and speech", which is way closer to the sense of the original Russian "*Myshlenie i rech*"] *finally* appeared in English in 1962... *By then I had learned enough about Vygotsky* from accounts of his work from Alexander Romanovich Luria, with whom I had become *close friends*... And I read the book not only with meticulous care, but with growing astonishment. For *Vygotsky was plainly a genius*. Yet it was an *elusive form* of genius, his (emphasis added; Bruner, 1985, p. 23).

Thus, here is the restatement of the said, somewhat simplistically and in a nutshell: previously totally brainwashed by the older and "close friend" Luria—"Luria and I became fast friends almost immediately" (Bruner, 1983, p. 145)—on the subject of "Vygotsky the genius of psychology", Bruner eventually actually read Vygotsky's book and did nothing but confirm Luria's zealously propagandist

claim: Vygotsky was a genius indeed, but Bruner was clearly at a loss pinpointing where exactly this genius was hidden from the scholarly laypersons: yet, hidden and “elusive” it was! The irony of this statement appears to have been hidden from Bruner himself, who made an attempt to localize the “genius” in the ideas that their author voluntarily and actively rejected in his later work. Yet, the statements of the kind did serve well the service of (a) the social construction of the mythical and glorious image of Vygotsky abound in virtually everything written about this fairly obscure thinker and social activist of the Bolshevik era, otherwise, and (b) the virtually endless quest ever since for “demystifying” and, after all, “understanding Vygotsky”. Note the two “virtually” in the previous sentence: a great deal of de-mystifying (and related de-mystification) has taken place quite recently (Yasnitsky, 2018; Yasnitsky & Van der Veer, 2016b; Yasnitsky, Van der Veer, Aguilar, & García, 2016), along with “questioning Vygotsky’s legacy” in terms of the opposition of it as a “scientific psychology” versus a “heroic cult” (Yasnitsky, 2019a). Yet, as long as Bruner’s advocacy for Vygotsky is concerned, the next logical step in this direction can be found in his later writing of a memoir character, where the author is openly speculating about the “Russian consciousness” that, pretty much in age-old spiritualist tradition, allegedly resides in the scholars and thinkers from Russia and those from the former Soviet Union (the USSR), an extinct state by the time of the publication of the essay (Bruner, 1995). After this minor digression into “Vygotskiana”, let us get back to Bruner’s views on Cognitive science: the field he (among others) created and, as we will see right now, demolished, from a purely intellectual standpoint at least.

Bruner’s later publication of early 1990s—unlike his earlier ones of 1980s—reveals quite a novel assessment of the history, the course and the outcome of American “cognitive revolution. It was Bruner’s book “Acts of Meaning” (1990) that has brought the good me argue, was so slow, relatively uneventful and hardly ground-breaking that it does not really qualify a news and the revelation that can be succinctly formulated in a series of statements and propositions, thus:

1. Bruner, among a handful of other like-minded individuals in Boston area, was among the visionaries and the initiators of new trends in psychology from mid-1940s. Quite in the spirit of the “battle for consciousness”, they (specifically, Bruner and his long-time collaborator from 1960, George Miller) “tried to persuade a generation of Harvard and Radcliffe undergraduates that to know Man you must see him against the background of the animal kingdom from which he evolved, in the context of the culture and language that provide the symbolic world in which he lives, and in the light of the growth processes that bring these two powerful forces into concert” (Bruner, 1990, pp. xv–xvi).
2. In 1950s, a few new trends crossed their paths and formed a new intellectually rebellious movement that could be first and foremost characterized by an anti-behaviorist stance in order to overcome the positivist, mechanist—at least construed and presented thus—behaviorism in American psychology, by many accounts, dominated by this intellectual force.
3. The pivotal year was 1956 when *A Study of Thinking*, a highly influential and ground-breaking book by Jerome Bruner, Jacqueline J. Goodnow and George A. Austin, came out (Bruner, Goodnow, & Austin, 1956); on the other hand, a major inaugural scientific forum took place. In his memoirs of 1983 Bruner refers to George Miller (cf., for instance, Miller, 2003): “his ‘birthday’ for the ‘cognitive sciences’ is

September 11, 1956, the second day of the Second Symposium on Information Theory held at MIT” (Bruner, 1983, p. 121). As an outcome, “what happened, of course, was that psychology’s world turned topsy-turvy not by psychology itself, but by modern theories of computation, of linguistics, of anthropology, indeed, even of biology” (Bruner, 1983, p. 60). Yet, “I was not there”—briefly adds the memoirist (Bruner, 1983, p. 121).

4. The new movement would not have its name, a label, until mid-1960s when the attribute of “cognitive” got attached to and associated with this movement, its representatives and their intellectual legacy. The major impulse to this terminological coinage and its institutionalization was provided by Bruner and Miller in 1960 when they established the Center for Cognitive Studies at Harvard University. The “cognitive studies”—as distinct from “psychology” proper—was conceived as an interdisciplinary project. Furthermore, the project is, perhaps, better understood as an a-disciplinary or trans-disciplinary (that is, transcending—if not entirely denying—disciplinary borders) rather than multi- or inter-disciplinary one, indeed: “the boundaries that separated such fields as *psychology*, *anthropology*, *linguistics*, and *philosophy* were matters of administrative convenience rather than of intellectual substance” (emphasis added; Bruner, 1990, p. xvi).

5. By mid-1970s the “cognitive” intellectual movement took its full strength, and it is only then that “Cognitive Science Society” was formally established in 1973 and a “Cognitive revolution” was explicitly proclaimed, retrospectively and triumphantly. As an aside, a remark is in place that, according to some commentators, the virtually two-decades long revolution was so slow and virtually uneventful (if compared with major events in political history of humankind; for instance, think about French Revolution of the 18th century or Russian Revolution of 1917-18) that it is best understood as an evolution, at best (G. Mandler, 2002, 2011).

Yet by the end of 1980s Bruner got ultimately disappointed with the outcomes of the “revolution” and acknowledged the failure of the project from the perspective of its original proposal as it was conceived roughly four decades before. In 1990 he attributed the failure to a few crucial factors and reasons. These can, possibly, be summarized and grouped together into three main clusters of problems.

First, the original “consciousness”—as in “battle for consciousness”—was interpreted as “mind” that, in turn, transformed into “cognition” which, eventually, degraded to “information”—as in “information processing”. This series of subtle linguistic, discursive and phraseological mutations and substitutions was gradual and seemingly inconspicuous, albeit lethal, eventually³. In his 1983 memoirs Bruner provides a somewhat abbreviated citation (on p. 129), apparently, even despite Bruner’s intention, to this very effect: “The confusion and barrenness of psychology is not to be explained by calling it a ‘young science’; its state is not comparable with that of physics, for instance, in its beginnings. ...For in

³ In fact, the phrase “lethal mutations” (used in a very similar sense to that in which it is implicitly used in this paper) is borrowed here from the great British-American educationist Ann Lesley Brown (1943–1999) and her long-time collaborator Joseph C. Campione. They reported the “lethal mutations” that occurred to their original, Vygotsky-inspired, educational practice—well-known in America under the labels of “Reciprocal teaching” and “Fostering Communities of Learners”—in the process of the dissemination of the visionary and ground-breaking innovation among wider circles of educational practitioners and thinkers (Brown & Campione, 1996).

psychology, there are experimental methods and *conceptual confusion*” (Wittgenstein, 1953, p. 232^e). Yet, the terminological and conceptual issues reflected deeper problems.

Thus, secondly, the original unity of six scholarly disciplines within Cognitive science—as it is being proclaimed to these very days (the disciplines being the four mentioned psychology, anthropology, linguistics and philosophy, augmented with computer science, or artificial intelligence, the AI, and, as seemingly somewhat later newcomer in subsequent years, neuroscience)—was considerably undermined by the computer-minded experts and, then, brain researchers. So, the original interdisciplinary unity, dialogue and balance in fact never held true. Yet, it is the winners who write histories: virtually no trace of anthropology, cultural studies or philosophy can be found in the Cognitive Science as it is often presented in some historiographies of this “information-processing” science—to the exclusion of Jerome Bruner’s name as a pioneer of this intellectual movement—and, for that matter, in the Cognitive Science as such, as it stands and is practiced now.

Then, thirdly, the problem goes even deeper than that. The original message and the mission of the “new science” was to eliminate behaviorism and positivism as the underlying philosophy of this science. This, in fact, never happened. The main reason for this, truly dramatic failure, is the rapid and victorious spread of the cognitivism as a mass movement. Indeed, regardless of the intentions of the few top visionary thinkers and the “founding fathers”—although, far from all of them—the majority of those rank and file scientific workers, who eagerly joined them, never reflected on their own behaviorist upbringing and academic enculturation, therefore, did not give up their quintessentially positivist mindset, values, attitudes, methods of research and data interpretation. Again in his *Essays in Autobiography*, Bruner cites his collaborator George Miller’s words:

I somewhat wonder, however, whether we really won the war. What seems to have happened is that many experimental psychologists who were studying human learning, perception, or thinking began to call themselves cognitive psychologists without changing in any obvious way what they had always been thinking and doing—as if they suddenly discovered they had been speaking cognitive psychology all their lives. So our victory may have been more modest than the written record would have led you to believe (Bruner, 1983, p. 126).

The history of Cognitive Science might be an exciting topic on its own, of interest to those involved in this field of studies now as well as to the historians of science. Another interesting—albeit unwritten yet—story of a similar kind is waiting for a researcher of the history of the “double-headed” inseparably interrelated process of “Vygotskiana” social construction and dissemination in the United States along with parallel establishment of comparative ethnographical studies on literacy in West Africa launched by the scholars from Rockefeller University’s the Laboratory for Comparative Human Cognition (LCHC), in New York in 1970s. The most notable agents of this scientific and publishing activity were Sylvia Scribner and Michael “Mike” Cole, whose paths parted by the end of the decade when the latter moved to University California, San-Diego in order to re-establish the LCHC there. A few publications came out of

⁴ In the German original: “Die Verwirrung und Öde der Psychologie ist nicht damit zu erklären, daß sie eine ‘junge Wissenschaft’ sei; ihr Zustand ist mit dem der Physik z.B. in ihrer Frühzeit nicht zu vergleichen. ...Es bestehen nämlich, in der Psychologie, experimentelle Methoden und *Begriffsverwirrung*” (Wittgenstein, 1953, p. 232).

this “double-headed” activity, yet there are just two that are worth specific mentioning in this context: First, the renowned and most criticized pseudo-Vygotsky’s book that came out under his name under the title “Mind in Society” in 1978 and launched the so-called “Vygotsky boom” (alternatively, critically referred to as “Vygotsky cult”) among American educationists and, to considerably lesser extent, psychologists (Vygotsky, 1978); second, a book by Scribner and Cole based on their literacy studies in Africa that came out in 1981 and—by decision of the African Studies Association (ASA), whose membership is primarily composed by the representatives from the United States and Canada—subsequently earned its authors the prestigious Melville J. Herskovits Prize of 1982, until most recently named so after anthropologist, another student of Franz Boas as well as the founder and the first President of the ASA, in 1957-58 (Scribner & Cole, 1981).

Yet, the actual agenda of the activities in California in 1980s gradually yet dramatically changed since Scribner’s departure from the project, the reestablishment of the LCHC on the west coast of the continent, and the unexpected—according to the memoirs of the mastermind and the first coeditor of the translation and the “social construction” of the pseudo-Vygotsky book of 1978 (Cole, 2004)—beginning of the “Vygotsky boom” in the United States. Indeed, the anthropological dimension of the originally unified research project was considerably suppressed, whereas the “Vygotskian”, “social constructivist” trend expanded and notably shifted to the area of learning and instruction, mostly in the domestic context of the United States. As an outcome of these later developments there emerged “sociocultural”, “sociohistorical” or, even worse, the so-called “cultural-historical activity theory” (CHAT)—not to mention yet worse, totally monstrous and absolutely disastrous “socio-historical-cultural activity theory”—that, upon closer examination, is hardly cultural or historical, and definitely not a theory, but rather an umbrella term for a wide range of very diverse studies and activities mostly in Education and, indeed, Cognitive Science—in its reduced and positivistic version, alienated from virtually any anthropology, philology or philosophy whatsoever, not to mention the roots of all these in the German classic philosophy and Romantic tradition of late 18th-early 19th century⁵.

Later, the agenda of the LCHC was creatively developed and exported to Western Europe and further advanced there—subsequently, worldwide—under the leadership of Yrjö Engeström at the Center for Research on Activity, Development and Learning (CRADLE) at the local university’s Department of Education in Helsinki, Finland since the Center’s inception in 1994. This initiative was developed under the label of “activity theory”, and focused on research and social interventions within the so-called “activity systems”. Engeström famously proclaimed his (and his collaborators’) contribution as the “third generation of activity theory” (3GAT): the first two allegedly being the legacy of Vygotsky and, from mid-1930s, but mostly in the post-WWII period, of Vygotsky’s former associate Aleksei N. Leontiev (1903-1979) and the group of his followers, respectively; the contemporary stage of the theory development is being currently discussed elsewhere as presumably the contribution of the “fourth generation of activity theory” (4GAT). This advancement even further distanced this field of research and social practice from the original proposal of “the struggle for consciousness” in psychology, furthermore, even departed from the discipline of psychology as such. Indeed, by the admission of a prominent advocate for and eager proponent of the “activity theory” in the United States, unlike the original legacy of Vygotsky and

⁵ For the collective “polyphonic autobiography” of the LCHC see online resource: <https://lchcautobio.ucsd.edu/>

Leontiev that cannot be understood unless as some kind of *psychology* (that is, ultimately, the science of personality in cultural and social context), “as *activity theory* is applied to larger and broader social groups, it requires further development in order to function as interventionist *sociology*” (Spinuzzi, 2019, p. 155).

Furthermore, consider the relatively recent call for “expanding Vygotsky’s (CHAT) project” in the spirit of “transformative activist stance” (Stetsenko, 2011), the proposal of “bringing together theory and practice” by means of “revisiting Vygotsky for social change” (Neto, Liberali, & Dafermos, 2020) or the proclamation to “regenerate and repoliticize CHAT to meet the environmental, economic, social, and now public health crisis generated by late capitalism”. The latter quote is so curious that it deserves more of our interest; it further develops thus:

Given the current political polarization with the emergence of both far-right nationalism and movements for democratic socialism in the U.S. and beyond, we feel that CHAT should take inspiration and lessons from a similarly tumultuous period of Vygotsky’s when war, reaction, and revolution were the order of the day. Vygotsky’s support for and participation in the tremendous social experiment in Russia provided the foundation for his emancipatory science of the mind and activity, as an antidote to traditional science in support of hegemonies and hierarchies of the status quo. That is, revealing how Vygotsky’s approach was forged in the crucible of revolution and came to embody his passionate activism, can help to advance similarly non-neutral science needed today in the struggle for social equality and justice⁶.

Cumulatively, all these proposals, statements and proclamations come from the radical left intelligentsia within the core of the “cultural-historical activity theory” (CHAT), who effectively introduce quintessentially neo-Bolshevik (bordering on neo-Stalinist) discourse in scholarship and push this scholarly tradition even further: from relatively innocent *interventionist sociology* to radical left-wing *political activism* in contemporary world that, by the authors’ radicalist admission, can be adequately compared with the “similarly tumultuous period of Vygotsky’s when war, reaction, and revolution were the order of the day”. All initiatives have their value and might find their application, but this one is seemingly of value primarily in the sphere of political struggle for “equality and social justice”, has potential of application primarily in the revolutionary social practice of fighting capitalism and the so-called “neoliberalism”, and, from scholarly perspective, promise to even further marginalize and voluntarily yet unwillingly stigmatize this intellectual tradition, its advocates, proponents, and the alleged historical “founding fathers” (such as Lev Vygotsky), if only these militant proposals to be taken earnestly and zealously, of course.

The readers of this paper might wonder why they should be concerned with the history of the Cognitive Science or, for that matter, the destiny of the so-called Cultural-Historical Activity Theory. I would insist the historiography of these episodes of relatively not so distant history is important for a number of reasons:

⁶ See the source online: <https://re-generatingchat.com/chats-crisis-of-re-generation-from-the-russian-revolution-to-the-trump-era/>

First, it sheds some light on the popular image of Lev Vygotsky (to be discussed further) and, briefly, his legacy as construed in North America during the Cold War period. Second, pretty much in agreement with, probably, the main message of this paper, it provides a very illustrative example of a transdisciplinary merger of psychology, anthropology, linguistics/philology (and philosophy too, as will also be discussed below). Third, the glorious launch and the eventual fate of the Cognitive Science in North America, at a higher level of abstraction, can be read as an earlier attempt at restoring Continental European (and British) psychology in its rights in America. Fourth, it demonstrates the power of horizontal social interconnections within informal social networks in scientific communities (on that, see a discussion in the immediately following section of this paper) and, on the other hand, the weaknesses and the pitfalls of the institutionalization of these informal networks in the vertically organized, hierarchical formal establishments and social institutions, such as scientific societies, foundations, and associations. Finally, the effort was wasted and the attempt failed, from Jerome Bruner's standpoint at least. This way, this episode is to serve a very good lesson to us now: as an example of what needs to be done and, on the other hand, what mistakes of the past need to be avoided this time.

Now that we know *what* needs to be done and *how not* to do it, the time has come to discuss *how* this can be achieved, it seems.

Methodological introduction

Apparently, this project aims at a holistic (all-round, organismic and integral) knowledge of human beings in their entirety and totality. Yet, let us face the obvious truth, the knowledge of totality—if only possible—can hardly be reached, unless in speculative thought (such as in philosophy or theology). For an empirical science, however, the ideal of totality is unreachable: the richness of the concrete objects, phenomena and processes and the whole multitude of interconnections between them in its entirety is by definition beyond the scope of any specific scientific discipline and, even broader, field of knowledge. Yet, there is a solution to this seemingly insurmountable problem. It is within certain limits, artificially constructed and predetermined borders that a resemblance of a holistic understanding of the world is possible, to a first approximation, at least.

That scientific knowledge is socially constructed is a truism these days; and this paper is definitely not for those, who disagree with this. Knowledge is not “discovered” (as if it stays “covered” beneath under a protective “cover” of some sort of a mystery of the nature), but is “constructed”, driven by our own beliefs, deeper motives, highly selective interests, specific questions we as researchers ask, our methods of investigation, choices of research tools, and interpretative practices. Then, the ideas we come up with are materialized by means of vocabularies, rules of grammar and stylistic conventions in accordance with specific genres of verbal expression, provided to us within specific languages we speak and write with. This even further complicates the matter. Propelled to its extreme, this idea of “knowledge construction” is getting dangerously close to denying the possibility of any scientific knowledge whatsoever as such. Indeed, if anything what we know is “constructed”, how can we know the differences between scientific knowledge, ideology, propaganda, myth or beliefs, the latter being more

often than not irrational and self-contradictory? Thus, from methodological standpoint, there is a minimalistic imperative that scientific knowledge—as constructed as it is—be grounded in empirical material of any kind. Let us see how this relates to the topic of our discussion and translates into clearly identifiable guidelines for further research.

In intellectual history or a theoretical treatise, virtually any couple (or more) of names, ideas, theories, etc. can be placed beside for the sake of comparative analysis, interpretation and discussion. Thus, for instance, René Descartes can be coupled with virtually any philosopher or psychologist in order to compare their intellectual legacies and discuss the challenges of the well-known (and notorious) so-called “Cartesian dualism”. This is what, in fact, quite often happens in various studies and publications that seem to have already created the genre of comparative studies in the manner of “X and Y”, where the two (or more) names are seemingly united by a series of commas and virtually meaningless conjunction “and” between them. This approach has certain merits and might be quite of use for certain purposes. Yet, it is quite vulnerable to criticism on the grounds of the comparability of the names in principle and their respective contributions, not to mention the extent of magnitude of the legacies and their impact on the intellectual capital of the humankind. For the sake of illustration of these critical considerations, I would offer a couple “Vygotsky and Marx”, which incidentally is a title of recent book (Ratner & Silva, 2017), whose scholarly quality has been critically examined in a few reviews of the book just on these very grounds. Therefore, some extent of arbitrariness—unlike in the case of coupling the names of “Marx and Engels”—is always a problem to any author performing in this genre of scholarly writing that inevitably requires an explicit discussion of a rationale for the choice of the names of the individuals (and their ideas) and their interrelation. There is a solid alternative to this approach, though.

In this paper, I do refer to a great many names of thinkers from the history of the sciences of the humans. Yet, the names are not randomly chosen; moreover, there are all reasons to consider the bearers of these names within a single unit of analysis, as a unitary whole. In order to do so, I propose a loose (in the sense: not clearly defined, yet intuitively quite clear to the reader) notion of “informal personal network” of individuals, as well as the other one: a “circle” (*Kreis*, in German; *krug* or *kruzhok*, in Russian). This approach, however, is not novel at all. Historically, we have known quite a few of “circles” (even those self-identified as such) of intellectuals, for instance the “circles” of linguists and philologists in interbellum Prague, the Czech republic (Jakobson, 1933; Toman, 1995), molded upon the image of similar and in a way like-minded circles (*kruzhki*) in Moscow (Jakobson, 1976, 1979; Walker, 2002) and St.Petersburg/Petrograd, Russia. Furthermore, the organizational model of a “linguistic circle” appeared so efficient and compelling that it triggered a number of other initiatives of the kind worldwide such as *Cercle Linguistique de Copenhague* (*Lingvistkredsen*, in Danish), Linguistic Circle of New York (with the subtitle *Cercle Linguistique de New York*), the *Cercle Linguistique de Tokyo*, the *Slovak Bratislavský lingvistický krúžok*, *Circolo Linguistico Fiorentino*, *Bucharest Cere de poetka si Stilistică* or the Linguistic Circle of Canberra: all these were presented and discussed in a characteristically titled paper by Roman Jakobson, “An Example of Migratory Terms and Institutional Models (On the Fiftieth Anniversary of the Moscow Linguistic Circle)” (Jakobson, 1965). There is substantial literature on Russian and Soviet literary “circles” (Walker, 1999, 2002), as well as the scholarly “circles” in natural sciences (Alexandrov, 1997; M. Gordin, 2006). In humanities and social

sciences, we have known about the so-called “Bakhtin Circle” (Brandist, 2002; Shepherd, Brandist, & Tihanov, 2004), as well as “Vygotsky”, otherwise, “Vygotsky-Luria Circle” (Yasnitsky, 2009b, 2011, 2016b). The densely interrelated and interconnected psychological community in the “Red Vienna” of 1920s-1930s can certainly be viewed through the lens of an “informal personal network” with numerous “circles” within it; some food for thought in this direction can be found, for instance, in (S. Gardner & Stevens, 1992). Finally, the *Wiener Kreis* (the Vienna Circle) proper as well as the “Berlin Circle” (however, self-identified as “*die Berliner Gruppe*”) of logical positivism (alias: logical empiricism) are very well known in the philosophical world and hardly require any introduction.

In principle, a “circle” is a constitutive part of a “network”, yet what makes it distinct from the latter is the proximity of the connections, the higher extent of intimacy (in the sense of personal attachment, psychological mutual involvement, loyalty and cooperation) and the intellectual interdependence between the individuals with a “circle”. Sometimes, a “circle” is described in terms of a “school” (of scholars), which is hardly justifiable, given the normative misuse of the word used for a discussion of a formalized and institutionalized hierarchical system of client-patron interrelations within a rigid social establishment. For the countless examples of “scientific schools” of the kind see, for instance, the propagandist and presentist historiographies of Soviet science, where virtually any administrative scientific authority in power of virtually any caliber (head of department, dean of faculty etc.) is proclaimed an innovator and a creator of a “unique approach” (a theory, a scholarly movement, a method of research, etc.) shared by a swarm of his (or her) former graduate students and associates. These individuals are the “clients” of their “patron” by virtue of being officially hired by the person with the authority of employment decisions within the administrative unit he or she is in charge with. Cumulatively, this administrative unit is typically proclaimed an “X’s school in Y”, where “X” stands for the name of a scholarly bureaucrat in power, and “Y” is the name of the respective scholarly discipline.

In sum, in this study we are dealing with (a) “informal” (as opposed to “institutionalized”, “formalized” or official)—therefore, quite likely, clandestine, inconspicuous, not readily revealing themselves) (b) “personal” (as opposed to “impersonal”, “scientometric”, “bibliometric”, “social”—like in the sense of “social network analysis” (SNA),—“mathematical”, or “graph theory”) (c) “networks” (as contrasted with “scientific schools”, “[invisible] colleges”, “cliques” or “clans”). The “circles” can be best understood as some sort of hubs within these networks. The “networks” and “hubs” are socially and intellectually constructed indeed: informal as they are they cannot be justified *exclusively* on any formal (e.g., cross-citations, patterns of collaboration or co-publications, etc.) or institutionalized grounds (such as official employment, shared membership in associations and societies, etc.). The interconnections are personal, therefore, more often than not, hidden, covered and in need of discovery and reconstruction. And yet, on the other hand, the interconnections are real in the sense grounded in the historically justifiable, real life events, processes, people’s actions and interrelations. Therefore, these are not randomly constructed at the discretion of the intellectual and the author, who thinks and writes about them. These are empirically valid as historical facts, again, as constructed as they necessarily are. This approach seems to provide a feasible compromise between the extremes of the Scylla and Charybdis of the *construction* of the fact and the requirement of scholarly rigor of an *empirically grounded* research.

What is interesting about the “informal personal networks” and, especially, the “circles” as the hubs within them, is that these, as units of analysis, can (and should) be regarded from three perspectives at the same time. First, from *anthropological* perspective, these function as some kind of “tribes” with their own values, habits, customs, communications, systems of power interrelations of various kinds, shared beliefs and ideas. Second, from *linguistic* and *discourse analysis* perspective, in the process of communication they develop their own idiosyncratic “languages” and “discourses” in order to talk about the issues of interest to them. This is particularly true of cases of creative and innovation-minded circles such as those artistic and intellectual ones. Finally, from *psychological* perspective, the circles constitute psychological unities of a special—inter- and supra-individual kind, where, quite true to the gestalt-theoretical principles, the whole is definitely larger than the total sum of individual members. These members are glued together, apart from anthropological and discursive characteristics, by an array of quintessentially psychological features, both of cognitive and emotional nature, such as shared interests, curiosity and ideas, passions and affections, the similarities and conflicts of egos and ambitions, interpersonal jealousies and altruisms, and so on and so forth. All these, in turn, trigger common actions and activities; these, in turn, result in ideal outcomes and their material implementations such as arguments, theories, manuscripts, oral presentations and written publications, to mention but a few. Considering all the above, a “circle” looks like an interesting theoretical and methodological proposal for a complex analysis of human beings at a higher level of generalization, even outside of the contexts, in which they traditionally have been discussed in the literature. Thus, this notion seems to be in principle equally applicable to the analysis of sport teams, theatrical production or even the situation of a poker game. Clearly, it is still premature to judge the range of possible applications of this conceptual and theoretical framework to concrete empirical material, especially given the initial stage of its conceptualization and development. Yet, understanding “circle” in consistently and rigorously gestalt-theoretical terms, this might be a potentially very interesting avenue for future research and genuinely important contribution of gestalt theory into, narrowly, the field of social psychology, and, much broader, the holistic and transdisciplinary science of human beings that has been proposed and advocated for on a number of occasions somewhere else.

General characteristic of the transnational Vygotsky-Luria Circle

The personal composition and the range of activities of the “Vygotsky Circle” (perhaps, more precisely, “Vygotsky-Luria Circle”) has been quite thoroughly—albeit not definitively: there is still some work that needs to be done on the subject in more or less distant future—researched in the recent decade or so. Thus, for the sake of the time and space economy in this paper, I would rather refer to these, quite numerous publications, mostly of my own such as those on the larger geographically dispersed Circle (Yasnitsky, 2009b, 2011, 2016b) and its offshoot in the capital city of the Soviet Ukraine (until 1934), Kharkov, in the decade of 1930s. The Kharkov group’s activities did not involve much of Vygotsky’s personal participation and contribution, though, unless on rare occasions (Akhutina, 2012) and developed mostly under the supervision of Vygotsky’s former associates such as Alexander Luria, Mark Lebedinskii and Alexei N. Leontiev (Yasnitsky, 2008, 2009a; Yasnitsky & Ferrari, 2008b, 2008b). The interested reader is invited to get familiarized with the details and intricacies of the deeds and the actions of a couple of dozens of Vygotsky’s closer and somewhat more distant (sometimes indirect) associates from these earlier studies that might give the reader an idea of the foci, the scope of research and theoretical status of their psychology during the interwar period, i.e., roughly, until 1939. Then, on Luria’s activities and the smaller, specialized circle of his associates in Moscow in the second half of 1930s we know from a study of his report on his “defectological” research that was published in the materials of a conference in Paris in 1937 (*Le premier congrès international de Psychiatrie infantile*, Paris, 1937); Luria did not actually attend the conference in France and his presentation was published *in absentia* (Lamdan & Yasnitsky, 2013). Another curious historiographic publication, although available in Russian only as yet, presented the outcome of this period from Luria’s perspective, as evident from his international correspondence of August, 1941, as strange and counter-intuitive the mere fact and the possibility of such correspondence in the earliest months after the Nazi Germany invasion in the Soviet Union might appear (Yasnitsky & Lamdan, 2017). In addition, as a nice historiographic source on Luria, his work, ideas and collaborators in the interbellum period we have an excellent study originally made as a doctoral dissertation (Proctor, 2016) and released recently as a monograph, highly advisable to anyone interested in the intellectual biography of Alexander Luria (Proctor, 2020).

Among other various issues of interest, these studies demonstrated very close and systematic interlinks between Lev Vygotsky and Kurt Lewin (broader, the gestalt psychologists in Germany and, later, the United States): they shared their students and collaborators, corresponded and even on quite a few occasions met personally, collaborated directly and exchanged their publications, etc. Yet again, this research has also been done and published: the more detailed and developed overview of this larger Circle of Soviet and German-American researches’ shared interests and activities is available in Russian, again (Yasnitsky, 2012c), although a fairly extended four-page long summary of the paper also exists in a few other languages such as English (Yasnitsky, 2012a), German (Yasnitsky, 2012b) and Portuguese (Yasnitsky, 2012f); there is also an informal Spanish translation of this shorter text, available online⁷. Of certain interest in this respect is a reconstruction of Luria’s trip and activities at the IX International Psychological Congress in the United States in 1929, including Luria’s meeting—among many others—with Kurt Koffka, their travels along the East of the continent and even sharing an accommodation

⁷ See https://vygotski-traducido.blogspot.com/2013/05/yasnitskiy_22.html

during the trip; this study was also published in Russian (Yasnitsky, 2012d). Yet, a larger version of this larger study on the “Soviet-Gestalt” psychology was also published somewhat later as a book chapter in English; the title of the chapter speaks for itself: “A transnational history of ‘the beginning of a beautiful friendship’: the birth of cultural-historical Gestalt psychology of Alexander Luria, Kurt Lewin, Lev Vygotsky, and others” (Yasnitsky, 2016a). The highlight and the climax of this curious “beautiful friendship” in its inception was, perhaps, Luria’s plans to organize a “Topologische Meeting” someplace in the Soviet Union (i.e., in Kharkov or Moscow) around 1936 as it got clear from a handwritten postcard sent from Moscow and signed by Luria, as well as two former Lewin’s students there: Bluma Zeigarnik and Gita Birenbaum. In turn, the card was an invitation to take part in the prospective “Meeting”, sent to the United States addressed to Kurt Lewin and another couple of his former Berlin students, the residents in the USA at that time, Tamara Dembo and Maria Ovsiankina (alias, Rickers-Ovsiankina):

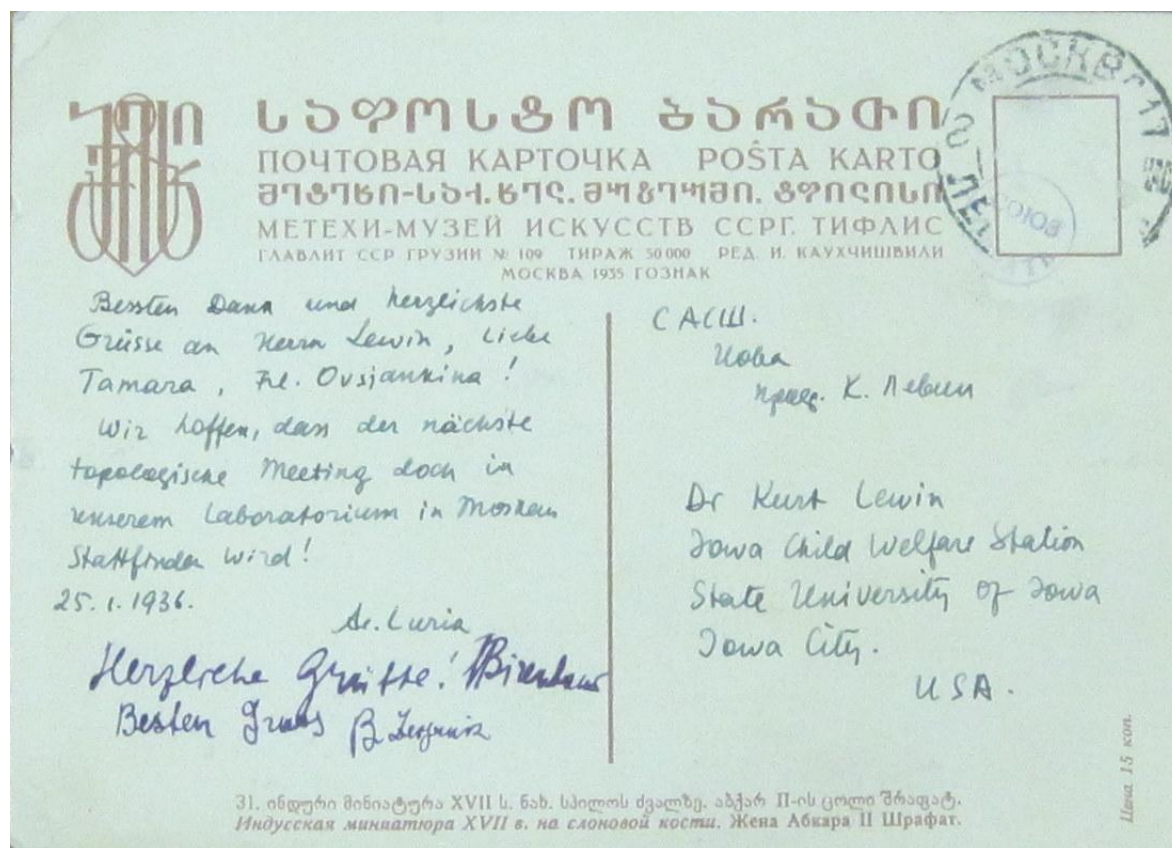


Illustration. A postcard with the invitation to a Soviet “Topologische Meeting” of 1936 from Luria, Zeigarnik and Birenbaum to Lewin, Tamara[Dembo] and Owsiankina, in German.

A real plan or a case of wishful thinking, this meeting never realized, though; one is tempted to presume mainly political reasons as a major obstacle. A specific episode of this “beautiful friendship”—Alexander Luria’s and Kurt Koffka’s expedition to the Central Asia—was thoroughly studied and discussed in detail in a series of important publications (Harrower, 1983; Lamdan, 2013, 2019; Lamdan & Yasnitsky, 2016; Proctor, 2013; Tupper, 2020; Yasnitsky, 2013) that presented the economic reductionism and vulgar Marxist bias of the Soviet scholars and the “Koffka-Luria controversy” on their contradictory and

mutually exclusive assessment of the different groups of local population's performance on optical illusions. Curiously, these historiographic publications apparently inspired a group of cognitive science scholars in the Netherlands for a most recent conceptual replication study that essentially resolved the controversy: the study refuted Luria's findings of the alleged impact of literacy on the literate subjects' submission to optical illusions (and, in turn, non-submission of those illiterate ones) and indirectly demonstrated the correctness of Kurt Koffka's research conclusions (Arunkumar, Van Paridon, Ostarek, & Huettig, 2021)⁸.

Vygotsky Circle intellectual legacy and the "Soviet Gestalt psychology"

Lev Vygotsky has long been considered as a pioneer, visionary and genius—"the Mozart"—of psychology. The vague, mystical and somewhat spiritualist claim of the "genius" notwithstanding, the rest he definitely was: the *leader* of a group of Soviet scholars, who were enchanted by his inflammatory and contagious proclamations about the future "new psychology" as a "science of superman"; the scientific *futurist* and avant-garde *visionary*—albeit a utopian one—of new horizons in psychological research; and the *pioneer* of a "Marxist psychology" that he never, though, realized in a theory, system of thought of his own or as a research practice. Indeed, an attempt at a truly "cultural-historical" field research in the settings of the Soviet Central Asia that Vygotsky and Luria undertook in early 1930s revealed their vulgar interpretation of Marxism in the spirit of economic reductionism and resulted in a virtual disaster.

The popular perception of Vygotsky and his legacy has considerably changed in the second decade of the 21st century, though. This partly happened due to the recent "revisionist" publications on Vygotsky and his legacy that reflected and, on the other hand, shaped the "revisionist revolution", first announced as early as in 2012 (Yasnitsky, 2012e). Yet, the full-blown "revisionist revolution in Vygotsky Studies" did not in fact come about until 2015-16 when the so titled book was published in English (Yasnitsky & Van der Veer, 2016c) and, somewhat revised and with an addition of a whole new section, in Spanish (Yasnitsky et al., 2016). This book summarized a decade of 2006-2016, when a series of archival, textological, historiographic, and theoretical studies were done. In fact, one can even argue that it is exactly during this decade that scholarly rigorous and systematic field of research called "Vygotsky Studies" as such was born.

Overall, the optimism of earlier "Vygotskiana" and its triumphant international advancement until the end of the first decade of the 21st century has notably changed into a situation that by the end of the second decade of the century can be described as the crisis and disillusionment. A series of various publications with the proposals to "revisit", "reintroduce", "rethink", "reevaluate", and even "question", etc. Vygotskian legacy are indicative of the crisis, yet hardly help to overcome it, it seems. Indeed, the popularity of Lev Vygotsky appears to be shrinking as manifested by the statistically proven decrease of scholarly citations to the works of this author and the loss of the interest in his personality (and ideas)⁹.

⁸ See also the preprint of the paper, officially "in press" as of May 2021, online:

https://pure.mpg.de/rest/items/item_3318525_2/component/file_3318526/content

⁹ For the statistics of the declining trend see: First, Google Scholar's citation rate of Vygotsky's published works worldwide (i.e., in a whole range of languages): <https://scholar.google.com/citations?user=L4S0dT0AAAAJ>. This

In an interview at the end of 1980s, Piotr Gal'perin, a noted Soviet psychologist of Alexei N. Leontiev's post-WWII "clan" in the Moscow State University and former—somewhat peripheral, though—associate of Vygotsky, gave his pessimistic, furthermore, given the "sacral" status of Vygotsky's name within certain circles of intellectuals, even blasphemous and sacrilegious assessment of his legacy and its future:

To those in the West who are so enthusiastic now about Vygotsky I want to say that they are considerably delayed in turning to him. In the meantime, we have made some progress, not so much from a theoretical point of view, but, I should rather say, from a historical one. In the West this process must, apparently, still be experienced; but eventually, they will also become disappointed in Vygotsky (Haenen & Galperin, 1989, p. 15).

Here it is: the time for the disappointment has eventually come, it seems. Vygotsky was hardly a theorist in the sense of traditional, old-school systematic thinker of any scale. True, Vygotsky was consistently loyal to and obsessed with a few themes such as a "theory of consciousness", a deliberately and consciously created "superman" (the "new man" of the Communist future), and the "new psychology" that would not only account for, but also provide the "know-how" for the emergence of the new human superman-like type. Yet, this is where his consistency ends. For instance, a "theory of consciousness" — as Vygotsky elaborated it throughout the last decade of his life (i.e., essentially, his entire career in psychology in 1924-34) under the pressure of external influences—underwent a series of profound theoretical mutations and encompasses at least three radically different theoretical standpoints, or even rather paradigms, such as "objectivist/reflexological", "reactological/instrumental" and "gestalt/holistic" (Zavershneva, 2014), which is definitely a very alarming sign for an allegedly serious theorist of any sort. Furthermore, more often than not, his writing style is pretty erratic as if the author was immediately reacting *ad hoc* on something that had just happened in the scholarly world, for instance, a conference, book publication or even an official decision of some kind of supervisory or governmental authority: thus, he would often engage himself in a discussion of some kind of matter at some point—and would never return to this discussion or problem at a later time. Furthermore, in his private documents, correspondence and even actual publications he would constantly refer to ambitious projects of considerable magnitude that would never come about in the long run. Indeed, we know about quite a few Vygotsky's book projects that would remain either unfinished and abandoned at best or even never launched at all (Yasnitsky, 2018; Zavershneva & Van der Veer, 2018).

Yet, that said, the full potential of Lev Vygotsky as a historical personality, intellectual and, last but not least, a still popular, "cult" figure and public image has not fully been exhausted yet and can still be productively reused for the purposes of further advancement of science. There are several ways that Vygotsky can still be of use. First, notwithstanding a great many of ideas expressed by the "buzz-words"

seems to reflect the changing status of Vygotsky's legacy within academia over the last three-four years or so. Second, English Wikipedia statistics of visits to the entry "Lev Vygotsky" (i.e. the "popular vote" perspective, so to say) over roughly the last five years, at least:

https://pageviews.toolforge.org/?project=en.wikipedia.org&platform=all-access&agent=user&redirects=0&start=2015-07&end=2021-04&pages=Lev_Vygotsky . Both charts appear straightforward, self-explanatory, and convincing enough, it seems.

erroneously or incidentally attributed to Vygotsky as his original or unique contribution to the intellectual treasures of the humanity—see the discussion of the former in (Yasnitsky, 2019b)—there are a couple of ideas that occupied certain space within his writings and are still seemingly of use in the relatively narrow and specialized spheres of human knowledge and practice. What immediately comes to one’s mind is the notion of the “inner speech” with its roots in German thought, specifically, Humboldtian philological tradition, from where it found its ways into the “cultural-historical” tradition and, further, the works of Soviet researchers in the field of psycholinguistics (Bertau, 2014; Werani, 2014). The second idea of considerable interest is the vague notion of the *zona blizhaishego razvitiia* (the “zone of the closest development”) with its three different yet interrelated meanings in Vygotsky’s texts (Kozulin, 2014) that, as I already claimed earlier, was not only mistranslated—or, rather, artificially translated as the “zone of proximal development”, only as if in order to deliberately obscure and mystify pretty straightforward meaning of the phrase (Yasnitsky, 2019b)—but also grossly misused in the educationists’ narratives about everyday classroom learning context (to the detriment of the very idea of psychological “development”—the third word of this very phrase—quite distinct from “learning”, as it is understood in virtually every discussion of the matter in the last half century or so). In contrast, the main and the most direct application of the notion in the sphere of education, as I argued earlier (Yasnitsky, 2014b), is the progressive educational practice of “dynamic assessment”, still pretty much in its infancy from the standpoint of students’ academic performance assessment in the context of publicly funded schools and other educational establishments in North America, at least. Yet, an introduction of *dynamic assessment* (quite opposite to free-form essays or standardized tests) in education has the potential of entirely revolutionizing and considerably advancing the sphere of educational philosophy and practice; for further discussion see (Kozulin, 2014; Valsiner & Van der Veer, 1993, 2014). These two topics are of interest to specialized experts in psychology and education, but are hardly related to this discussion. Yet, it seems somewhat early to totally dismiss Vygotsky: he still has something valuable to tell us on the topics of interest to us in this paper.

A brief characterological comment on Vygotsky’s psychological portrait is in place here. Traditionally, the academic culture of Imperial Russia was very much oriented toward Western Europe and lengthy research trips to Germany, less frequently to France, were virtually a precondition for any Russian post-graduate scholar aspiring a doctoral degree even if the degree would be subsequently awarded by a domestic university. Thus, on the one hand, we are dealing with some kind of “inferiority complex” of the scholarly community in Russia that would always be catching up with the Western (predominantly, German, from linguistic standpoint) academia; a few remarkable exceptions such as Ivan Pavlov in physiology and medicine, Vladimir Vernadsky in a range of natural sciences or Dmitry Mendeleev in chemistry only prove the rule. However, one needs to keep it in mind that each of these truly stellar Russian scientists had studied under the guidance of Western leading researchers in their respective fields before they earned their own world fame of topmost scholars. On the other hand, the Russian revolution of 1917-18 added something new to the status quo and somewhat changed it, if not in the traditional “hard sciences”, then in the social sciences, without any doubt. The Bolshevik overtake of the power in the end of 1917, their eventual victory in the Civil war, and the establishment of the first ever socialist state was a crucial factor that contributed to the change. This change can perhaps be described as “arrogance” of the world leaders in social practice of gigantic reconstruction of the entire fabric of the

state towards a classless society to the benefit of the former minorities, underprivileged, and those socially outcast under the capitalism. Lev Vygotsky, entirely the child of his time and the product of the Zeitgeist, combined both: in his mind, scholarly thought and writing.

The “inferiority complex” turned Vygotsky into an avid and somewhat eclectic reader of everything that he would reach and considered as representative of the highest quality foreign science publications. Some of these were available to him in Russian translations, some he had to read in the original, yet some of these latter were made available to the Russian readers upon Vygotsky’s initiative as an editor, translator or a volunteer entrepreneur of the Bolshevik kind. In these activities we see Vygotsky acting as an international “broker” in science and a “spokesman” for the Western European scholarship in the context of the early Soviet Union. Curiously, it is exactly this social role of Vygotsky that might well be usefully put into motion once again: now, in the context of the international scholarship of the 21st century. During his lifetime, Vygotsky was often—among other things, including politically-motivated matters—criticized for his eclecticism and uncritical borrowing from the foreign scholarship. Indeed, quite a number—in fact, the majority of Vygotsky’s major publications published during his lifetime—were either educational course materials, or second-hand compilations and reports of foreign achievements in psychology and allied sciences, or popular expositions of foreign scientific works to the mass population of undereducated readership in post-revolutionary Soviet Union (Van der Veer & Yasnitsky, 2016). As an outcome, the six-volume collection of Vygotsky’s works (available in Russian as well as in English and Spanish/castillano translation) reads now as a “Who is who” in world and European psychology. Given Vygotsky’s—fading but still relatively high—popularity and fame this is quite a useful feature of his written works that “advertise” for a great many thinkers and scholars of the past, really great, important and actual, but quite undeservedly forgotten, omitted or overlooked by the contemporary “publish or perish” academic community.

An offshoot of this function of Vygotsky as a “spokesman” in favour of the great scholars of the past is his ability to tell us even something new about these individuals and their ideas than we believed we have known before. Consider, for instance a very interesting confession that Vygotsky left in one of his unfinished manuscripts (published only posthumously and posited as his most important methodological work in psychology). In this text Vygotsky reflects on his only foreign trip (in the summer of 1925 to London, via Berlin) and his encounters with German psychologists (most likely either Max Wertheimer or Kurt Lewin or both, in Berlin): “When the eclectic and unprincipled, superficial and semi-scientific theory of Jameson is called Marxist psychology, when also the majority of the influential Gestalt psychologists regard themselves as Marxists in their scientific work, then this name loses precision with respect to the beginning psychological schools which have not yet won the right to “Marxism.” I remember how extremely amazed I was when I realized this during an informal conversation. I had the following conversation with one of the most educated psychologists:

What kind of psychology do you have in Russia? That you are Marxists does not yet tell what kind of psychologists you are. Knowing of Freud's popularity in Russia, I at first thought of the Adlerians [i.e., the followers of Alfred Adler]. After all, these are also Marxists. But you have a totally different psychology. We are also social-democrats and Marxists, but at the same time we are Darwinists and followers of Copernicus as well.

I am convinced that he was right..." (Vygotsky, 1997, p. 341). This is very interesting statement, to say the least. Both Vygotsky's own assessment of the majority of German gestaltists' identity as "Marxists" and, reportedly, Marxist identity (apart from and in addition to the apparently non-conflicting identities of "Darwinists and followers of Copernicus") of his Berlin interlocutor—as a representative of the gestalt movement, in all likelihood—appears equally unusual, novel and thought-provoking.

Yet, there is a back side to Vygotsky's "inferiority complex" and popularization of the foreign, Western research, and it has much to do with the other "demon" of his: the self-assuredness and arrogance of the post-Revolutionary militant Bolshevik activist in science. As some curious twist in Vygotsky's mind characteristic of the great many of his contemporaries among Russian intelligentsia in 1920s-1930s (and even, in fact, in many instances, up to these very days), his true fascination with the West and uncritical borrowing from the Western scholarship go hand in hand with acute criticism of the Western "bourgeois" science, in quintessentially Bolshevik parlance. In order to get a taste of this sentiment consider an entry that Vygotsky—a newcomer in the field of "defectology" (i.e., a study as well as special and corrective education of children with impairments of different kinds)—that he left in his personal notebook during his trip abroad to a congress on the 8th International Conference on the Education of the Deaf in London in 1925:

In essence, Russia is the first country in the world. The Revolution is our supreme cause. In this room only one person knows the secret of the genuine education of the deafmutes. And that person is me. Not because I am more educated than the others, but [because] I was sent by Russia and I speak on behalf of the Revolution (Vygotsky in Zavershneva & Van der Veer, 2018, p. 63).

This kind of militant—and arrogant—attitude presented itself in a great deal of criticism that Vygotsky expressed at different times and on different occasions toward his compatriots and foreigners alike. The critics of Vygotsky are often blamed with unfair, biased, distorted, and politically motivated intellectual aggression. Ironically perhaps, all these labels are apparently quite applicable to Vygotsky himself, as already mentioned, entirely the product of his time. Yet, the criticism of Vygotsky (in both senses, as an object and the subject of critique) is not necessarily unfair and biased: in many instances it appears well-deserved and quite to the point. Thus, in this case we are dealing with another potentially very useful characteristic of Vygotsky (as not a theorist, but still a sharp intellectual and the subject of critique, of course): Vygotsky the "*critic*". Specifically, we should be very much alert to Vygotsky's criticisms of gestalt psychology's claims and achievements: relatively well-read in the published and even some unpublished works of the gestalt authors (although serious lacunae in Vygotsky's familiarity with the gestalt movement as a whole are quite conspicuous and regrettable), furthermore, their great admirer (especially, in the very last years of his life in 1930s), Vygotsky in his social role of a "critic" might still teach us something important about their scholarly legacy and this intellectual tradition.

Thus, here we deal with the third social role that Vygotsky and his associates played in relation to the gestalt movement. This role can be perhaps characterized as that of "*critical follower*" or a "*critical replicator*". The latter elements of these phrases suggests a second-hand, epigone performance. Yet, the former, namely, a critical stance is important as a potential expansion of the original gestaltist thought

in different social circumstances and, even more importantly, on a different worldview/cultural, philosophical, theoretical and axiomatic base. In this sense of particular interest are the first studies and publications that former Kurt Lewin's students did in Moscow under the supervision of Vygotsky upon their move to the Soviet Union (Birenbaum & Zeigarnik, 1935; Zeigarnik & Birenbaum, 1935); of similar interest are the studies that Vygotsky and his associates did in the footsteps of Lewin and closely following the methods of Lewinian research in Soviet Russia (Solov'ev-Elpidinskii, 1935; Vygotskii, 1935). In addition, a most curious publication came out just a year after Vygotsky's death (Zankov, 1935). This was a book by Vladimir Zankov: Vygotsky's former graduate student, associate and, at certain point, an administrative superior at the Experimental Defectological Institute in Moscow. Incidentally, this very author somewhat earlier served also a co-translator into Russian of Wolfgang Köhler's classic on the intellect of the anthropoids known in English as "The mentality of apes" (Keler, 1930; Köhler, 1921, 1925). Thus, in his book of 1935, Zankov not only discusses Lewin's ideas at considerable length, but also—on page 36—provides a reference to the work of Lewin's student Paul Köpke titled "Satisfaction in normal and in feeble-minded children". What is remarkable is the fact that Köpke's study has never been published as such and we only know about it by its description and discussion that can be found on a number of occasions in Lewin's first English book titled "A dynamic theory of personality" that came out in the United States just the same year as the one by Zankov, in 1935 (Lewin, 1935). This curious finding yet another time suggests us the most direct, personal and informal transnational contacts and scholarly communications between the Soviet researchers of the Vygotsky Circle and their gestaltist German-American peers.

In addition to these, one is tempted to mention in this context a series of studies conducted by those Soviet scholars, who were not in any sense associated with either Vygotsky or Luria (or their associates and collaborators), but still continued the Lewinian-style experimentation in the Soviet Union. In the interwar period this was a circle of Vladimir Miasishchev (1893-1973), Vladimir Bekhterev's (and his closest associate Alexander Lazurskii's) former student and his intellectual (as well as administrative, as the Director of former Bekhterev's Psychoneurological Institute in 1939-60) heir in the field of clinical psychology and psychoneurology in Leningrad¹⁰ (Karvasarskii, Podsadnyi, Cherniavskii, & Chekhlatyi, 2012). A few of the members of the Miasishchev Circle were former associates of Bekhterev as well. These studies were presented in a series of publications of the individual members of the circle (Averbukh, 1936; Khvilivitskii, 1935; Meerovich, 1935b, 1935c, 1935a; Meerovich & Kandratskaia, 1936; Meerovich & Plotnikova, 1936) that he summarized in a couple of publications of his own (Miasishchev, 1935, 1936). These empirical studies in the field of clinical and medical psychology are absolutely obscure and hardly known until these very days even in the land of their origin.

A few studies of 1930s conducted under the supervision of Vygotsky's former associate Aleksei N. Leontiev (after Vygotsky's death in 1934) also reveal participation of Lewin's former students such as a virtually unknown Nina Kaulina, who launched, but never completed her doctoral research under Lewin in Berlin. Yet, the 1935 study in Moscow's Gorky Park—the manuscript of which was discovered and published only quite recently under the two names of a Moscow and a Kharkov researchers (Leontiev & Rozenblum, 1999)—was accomplished with Kaulina participation, and arguably presents a particularly

¹⁰ Former St. Petersburg, renamed Petrograd in 1914, then Leningrad in 1924.

interesting example of some version of Topological psychology, although less formalized than in Lewinian original proposal that he published, notably, a year after the completion of the study in Moscow (Lewin, 1936). This suggests that, quite true to the meaning of Luria-Zeigarnik-Birenbaum postcard that they sent to the United States in early 1935, investigations in the spirit of Lewinian Topological psychology were developed in parallel in the United States and the Soviet Union alike. Incidentally, another study that fully qualifies as a “topological” one was conducted in the Kharkov Palace of Pioneers, also in 1935; a somewhat brief and quite informal report on the study was not published until the end of WWII (Leontiev, 1947). This very paper with a presentation of a collaborative study done by its author in his mid-30s, was much later republished by the aging author in his mid-70s (and just four years before the author’s death in 1979) in a collection of his much later articles that originally came out in 1970s in the main Soviet philosophical journal “Voprosy filosofii” (The issues in philosophy) and were collected under the same book cover of the main Soviet political and philosophical publishing house, the *Politizdat*—the most official and politically correct publisher in the USSR; the discussion of the composition of the book can be found in (Yasnitsky, 2020). This republication of an obscure and, possibly, well forgotten much earlier paper (Leontiev, 1975)—the only republication of this kind in this book—suggests its supreme importance to Leontiev as a rationale for the deliberate inclusion of the only text a few decades old in his “swan song”, the lifetime concluding oeuvre and the final book that would come out under the author’s name. This article—subsequently, book chapter—was titled “Psychological issues of cognizance in learning and instruction”, where “learning and instruction” stand for Russian word “*obuchenie*” with an indiscriminate meaning of both these English words combined in this Russian one. Then, “cognizance”—as related to but clearly distinct from “consciousness”—stands for Russian “*soznatel’nost’*” (as related to but clearly distinct from “*soznanie*”): for the subtle yet crucially important differences within highly lexically rich semantic field of “consciousness”/“meaning”/“sense”, their derivatives in Russian and the proposed terminological toolkit for discussing these highly nuanced issues in English see the specialized discussion in (Yasnitsky & Van der Veer, 2016a). The interconnection of these two themes appears very important and highly promising, namely, the gestaltist/Lewinian research orientation and the focus on “consciousness”/“meaning”/“sense” in the context of the psychology of action (*Handlung*, in German; *deistvie*, in Russian).

Indeed, a curious example of this very complex of themes and ideas figures prominently in yet another study that was conducted in the 1930s in Kharkov by the former associates of the late Vygotsky. In this study on involuntary remembering (in Russian: *neproizvol’noe zapominanie*; “remembering” in the meaning of “encoding” in the information-processing cognitivist slang) conducted by Piotr Zinchenko (subsequently, the leader of academic psychology in the post-WWII Kharkov), the involuntary remembering was demonstrated as a function of activity, furthermore, the extent of the involvement in this activity; later and independently of Zinchenko, a very similar idea was proposed in the classical cognitivist studies on the “levels of processing” and their impact on information retention, or remembering (Cermak & Craik, 1979; Craik & Lockhart, 1972): it was much time later that the authors of these studies explicitly acknowledged and discussed at length the remarkable similarities between the two approaches (Craik & Lockhart, 2008). Yet, the original Zinchenko’s study—highly reminiscent of Lewinian experimental research model of deception-based studies (that is, the type of research, in

which the participants are not only aware of the true goal of research, but also are given a false explanation of the meaning of the study like, for instance, in the classic study of Stanley Milgram on obedience, one the best-known psychological studies ever)—represents a truly curious blending of Soviet Marxist “activity approach” and quintessentially gestaltist terminological framework that combines the ideas on activity, meaning, consciousness and cognizance with those apparently borrowed from the psychology of perception research. Thus, for Zinchenko it is not the figure and the background that his subjects were supposed to perceive, but the objective goal (as a “figure” in certain sense) and the background material of activity that they performed. The major finding of Zinchenko’s study was that the “figure” of problem-solving (and not related to memorizing) activity was uniformly remembered, and relatively well; yet the “background material” of this activity, quite surprisingly, was also remembered, albeit to a considerably lesser extent. This research was first presented at a conference in Kharkov in 1938—a transcription of the stenographic report remained unpublished until this century (Zinchenko, 2009)—then published next year in a local publication outlet (Zinchenko, 1939). The study proved truly ground-breaking and paved a way to a great deal of further psychological research and writing, such as (Zinchenko, 1961). An English translation of this classic of Soviet psychology is also available, although in a poor translation that confuses “memory” with “remembering” and largely distorts the essential distinction between these two notions (Zinchenko, 1983). A much later replication study conducted in a similar settings (such as, for instance, the geographic locale, in Kharkov) generally corroborated Zinchenko’s research findings of 1930s, but also revealed a number of interesting differences that might possibly be explained by historical and cultural development of psychological performance on the task over the period of a few decades that separated these two studies (Yasnitsky, Falenchuk, Mazhirina, & Ivanova, 2008).

Cumulatively, all these studies definitely deserve more attention from scholarly community as a curious example of a variation of gestaltist/Lewinian research in the Soviet Union (the proximity and loyalty of Lewinian “topological” agenda to the original quintessential gestaltist proposal is a controversial topic and an issue yet to get clarified in a future in-depth theoretical investigation), yet done allegedly from a very different methodological perspective, namely, that of declaratively distinct “Marxist methodology”, whatever this might mean, and to varying extent following the visionary proposal of “activity approach” of Sergei Rubinstein as laid out in his pioneering article of 1933, published in original Russian under the title “The problems of psychology in the works of Karl Marx” a year later (Rubinshtein, 1934), subsequently translated in German and English and published a few decades after that (Rubinstein, 1981; Rubinštejn, 1987). With an important yet virtually unnoticed exception (Payne, 1968), Rubinstein’s work has been virtually unknown to the English reader until most recently and was de facto first introduced to the English readership as “the founder of Soviet Marxist psychology” in a special book chapter on Sergei Rubinstein and his scholarly legacy in the context of the 21st century (Yasnitsky, 2021).

In sum, it seems safe to claim that this tradition of research in the Soviet Union was strong and important. Overall, it counts for a full-fledged “Soviet gestalt psychology”, for the lack of a better term. The scholarship as an integrated and interrelated whole has never previously been studied and definitely deserves further exploration in the uncharted waters in the hope it will give us new exciting

insights into its new, productive and promising developments in this 21st century. This is the first “take-home message” of this paper, yet not the only one.

The second part of this paper is dedicated to the discussion of the question of how this seemingly unexpected discovery of “Soviet gestaltism/Lewinianism” is going to impact our traditional understanding of the meaning of gestalt theory as such, especially from the idiosyncratic perspective of “informal personal networks” and “circles” as their hubs. In the anticipation of the due argumentation and the final conclusion, I would like to give the reader a hint: our traditional views will need to undergo a change, and perhaps quite a considerable one, indeed. If so, then, this is going to become the second and, perhaps, the most important “take-home message” of this paper.

Before we progress any further, though, let us briefly overview the literature on the history (mainly) and theory (a little bit) of gestaltist psychology, at least the most remarkable, the *sine qua non* publications available mainly—but not exclusively—in English.

Scholarly literature on Gestalt psychology

To be sure, the literature on gestalt psychology is quite impressive, if publications only in English are considered alone. Then, publications in German definitely add to this multiplicity of sources and considerably add to the depth and the breadth of our understanding this intellectual movement. Thus, for the start, there are a couple of excellent overviews of holistic tradition generally (Harrington, 1996) and, specifically, the “gestalt psychology” in German culture (Ash, 1995) that should still be considered authoritative classics on this topic—until something better is published in terms of scholarly breadth, depth and quality of research. Hopefully, this will happen in more or less distant future. Then, a curious reader interested in the background on German academic culture has another couple of fascinating, unorthodox and in a number of ways counter-intuitive stories: the one on the rise and fall of “the German mandarines” (named so by analogy with traditional bureaucracy in China) in German academic community in 1890-1933 (Ringer, 1969) and the “professionalization of psychology in Nazi Germany” (Geuter, 1992). Perhaps not equally liked by everybody within academic community (especially, in Germany), these two monographs stand out as undeniably knock-out classics; another couple of most distinguished books might be of help, both in German, though: an edited volume on the history of German psychology under the Nazi rule (Graumann, 1985) and, broader, in the 20th century (Ash & Geuter, 1985). For a multitude of personal portraits and biographies of German scholars of the interwar period, i.e., the Golden Age of gestalt psychology we have a contemporary “who’s who in world psychology” (Murchison, 1932) and, written from the perspective of our times, a biographical lexicon of German-language psychologists, the so-titled Springer’s “Deutschsprachige Psychologinnen und Psychologen, 1933-1945” (Wolfradt, Billmann-Mahecha, & Stock, 2015). In terms of personal biographies of the protagonists, there are nice sources of variable quality and reliability such as the monographs on Wax Wertheimer (King & Wertheimer, 2005), Kurt Lewin (Marrow, 1969), and Kurt Koffka (Harrower, 1983): these are, generally, reach with archival documents and of interest to historians of psychology even today. To these may also be added an autobiography of non-gestaltist, but like-minded their Austrian-American contemporary Fritz Heider (Heider, 1983) and another edited book

on Lewin, with a number of publications, quite interesting from the standpoint of historiography of science, in either of its two Peter Lang's German editions (Schönpflug, 1992, 2007)¹¹. As long as Kurt Lewin's legacy is concerned, another source might appear quite helpful, especially to those readers, who lack the reading knowledge of German and thus cannot get familiarized with the original empirical studies of the group of Lewin's students that came out in the gestaltist journal *Psychologische Forschung* in 1922-34: some of these were translated into English and got published in (de Rivera, 1976), specifically (Dembo, 1976; Hoppe, 1976; Karsten, 1976; Rickers-Ovsiankina, 1976; Schwarz, 1976), originally published as (Dembo, 1931; Hoppe, 1930; Karsten, 1928; Ovsiankina, 1928; Schwarz, 1927, 1933), respectively. Incurably poisoned with cognitivist slang that penetrated the English texts in the translation of 1970s, the sources are still usable, at least in a first approximation to the original meaning of Lewinian empirical research program in Berlin. In addition, I would strongly recommend two critically undervalued yet equally crucial studies on the frontier between historiography and theoretical treatise on Lewinian legacy of his earlier period. Both were written as doctoral dissertations, in German and in English, the former eventually published as a monograph (Wittmann, 1998), the latter available as manuscript only (Perlina, 2015), luckily, available online¹². Finally, apart from the actual publications of the proponents of and advocates for gestalt psychology, mainly from Frankfurt, Giessen and, definitely not the least, Berlin during the Weimar Republic period—mentioned as such but deliberately not cited here in their entirety—I would like to also present a set of other, somewhat obscure (in the sense of merely forgotten and not often cited) sources of considerable interest in the context of this discussion. These are a couple of texts collections, both of pre- and after-WWII period that represent a wide range—and the width of the range—of German gestaltist studies such as a “source book” (Ellis, 1938) and the “documents” of gestalt psychology (Henle, 1961). A curious genre of its own is constituted by a few books written by the outsiders: the contemporaries of the German-American gestaltists and authors not necessarily entirely sympathetic to gestalt thought, therefore, more alert to its pitfalls and likely to express their criticisms, ever of great value if only well-thought, rational, and logical (Boring, 1929; Hartmann, 1935; Katz, 1950; Leeper, 1943).

To be continued.

¹¹ A great many of treasures on the history of German psychology can also be found with this book series titled “Beiträge zur Geschichte der Psychologie”, edited by Helmut E. Lück and Armin Stock; see online for the titles of over thirty volumes in the series so far: <https://www.peterlang.com/view/serial/BGP>

¹² See <https://edoc.hu-berlin.de/bitstream/handle/18452/18324/perlina.pdf?sequence=1>

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