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**VYGOTSKY CIRCLE DURING THE DECADE OF 1931-1941:  
TOWARD AN INTEGRATIVE SCIENCE OF MIND, BRAIN, AND EDUCATION**

by

Anton Yasnitsky

A thesis submitted in conformity with the requirements  
for the degree of Doctor of Philosophy  
Department of Curriculum, Teaching and Learning  
Ontario Institute for Studies in Education  
University of Toronto

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Doctor of Philosophy 2009

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Department of Curriculum, Teaching and Learning

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**ABSTRACT**

This dissertation presents a study of the scientific practices of the circle of Vygotsky's closest collaborators and students during the decade of the 1930s-and including the early 1940s (until Germany's invasion of the Soviet Union and the beginning of the Great Patriotic War in 1941). The notion of Vygotsky Circle is introduced in this work and is explicitly distinguished from a traditional—yet frequently criticised—notion of “the school of Vygotsky-Leontiev-Luria”. The scientific practices of the Vygotsky Circle are discussed here as the unity of a) social and interpersonal relations, b) the practices of empirical scientific research, and c) discursive practices of the Soviet science—more specifically, the “Stalinist Science” of the 1930s. Thus, this study analyzes the social and interpersonal relations between the members of the Vygotsky Circle and the evolution of this circle in the social context of Soviet science during the decade of 1930s; various practices of empirical scientific research conducted by the members of the Vygotsky Circle were also overviewed. Finally, discursive practices of the Soviet scientific “doublespeak” were discussed and illustrated with several examples borrowed from publications of the time.

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Dedicated to

Tatiana,

Inna,

Irina,

Irena,

Maria,

and—the least but,

hopefully,

not the last—

Katerina

## **Prologue:**

### **Why do we need yet another study on the history of Vygotskian psychology?**

Lev Vygotsky (1896-1934) was a seminal thinker and one of the founders of Soviet psychology, whose work appears to be among the most commonly referenced in the studies that investigate a wide range of cultural and social factors and their influence on human development today. The “Vygotsky boom” (Cole, 2004; Garai & Kocski, 1995) dates back to 1978 when the book entitled *Mind in Society* was published under Vygotsky’s name (Vygotsky, 1978). The book has since become a classic work and an almost mandatory reference for a number of research traditions in the human sciences that claim Vygotsky’s legacy as one of the main sources of inspiration of their work. This publication was followed in 1987-1999 by the major publication of a six-volume collection of Vygotsky’s works that made essential Vygotsky’s writings finally available in English and further contributed to an ongoing “Vygotsky revolution” that, despite the considerable and increasing popularity of Vygotsky’s ideas and their international success worldwide, has not yet been fully assimilated by the mainstream psychology as it has been practised in North America (Harre, 2006).

Yet, there are quite a few problems with the common perception Vygotsky’s legacy. First, interestingly enough, Vygotsky never actually wrote his most famous book in the West, *Mind in Society*: the book is a compilation of the fragments taken from different Vygotsky works written during different periods of his scientific career. On a number of occasions in their preface the editors of the book confess that they “constructed” some chapters of the book, whereas other chapters “summarize” or

are “based on” Vygotsky’s actual writings (Cole, John-Steiner, Scribner, & Souberman, 1978). Furthermore, the editors explain that “at several places [we] have inserted material from additional sources in order to more fully explicate the meaning of the text... In a few cases passages have been taken from the work of Vygotsky’s students or collaborators which provide concrete examples of experimental procedures or results which the original text describes with extreme brevity... In putting separate essays together we have taken significant liberties. The reader will encounter here not a literal translation of Vygotsky, from which we have omitted material that seemed redundant and to which we have added material that that seemed to make his points clearer.” And, finally, the editors add: “We realize that in tampering with the original we may have distorted history; however, we hope that by stating our procedures and by adhering as closely as possible to the principles and content of the work, we have not distorted Vygotsky’s meaning” (Cole, John-Steiner, Scribner, & Souberman, 1978, p. x).

Second, what is perhaps Vygotsky’s the most famous concept in the West, the “zone of proximal development”, is far from central to the entire system of Vygotsky’s thought and occupies just a few dozen pages within the six-volume collection of Vygotsky’s works ([Chaiklin, 2003](#)). Still, it remains “one of the most used and least understood constructs to appear in contemporary educational literature” ([Palincsar, 1998](#)), and “there is a danger that the term is used as little more than a fashionable alternative to Piagetian terminology or the concept of IQ for describing individual differences in attainment or potential” (Mercer & Fisher, 1992).

Third, the quality of Vygotsky’s works published in the Soviet Union between 1950-1980s—mercilessly edited and censored—is often notoriously poor; his ideas

are at times distorted, and translations add even more complexity to the task of understanding Vygotsky's legacy (Van der Veer, 1997).

Fourth, up until quite recently, a great many of Vygotsky's texts remained unavailable to the international reader: the translations were scarce and often fragmented. Even the relatively recent edition of the six-volume collection of Vygotsky's works does not represent the entire original scope and magnitude of Vygotsky's scientific interdisciplinary work. The most recent archival and bibliographic research conducted in private archives of Lev Vygotsky revealed enough material for a prospective 15 volume collection of complete works of Vygotsky (Zavershneva, 2007a, 2007b, 2008a, 2008b).

Fifth, historiography of Vygotskian psychology is represented by a number of "hagiographic" accounts of Vygotsky's life and work, whereas critical studies of Vygotsky's theory within its cultural context remain rare. More specifically, the role of Vygotsky's group in development and dissemination of his thought in a wide range of closely interrelated domains and areas—including the integrative study of child (*paedology*), education and child development, pathology of child development and special education (*defectology*), degradation of mental functions and mental sickness, speech pathology caused by brain lesions, psycholinguistics and neuropsychology,—is often underestimated in scholarly literature.

Sixth, whereas the vast majority of these ideas and research perspectives are implicit in Vygotsky's own texts, they are developed at considerable length in the works of his students and collaborators. These studies were conducted and published during the last years of Vygotsky's life (early 1930s), but for the most part after his death (from 1934 onwards). The links between Vygotsky's ambitious project and the

later development of his ideas and their interrelations in a wide range of applied fields still remain undiscovered and underrepresented in scholarly literature.

All these problems set the background of this study that aims at investigating the development of Vygotsky's thought in its entirety during the last years of his life and immediately after his death. The main actors in this study are Vygotsky as well as the group of his collaborators that I present and discuss here as the Vygotsky Circle.

## **Chapter 1.**

### **Deconstructing historiographical narrative:**

#### **“The school of Vygotsky-Leontiev-Luria”**

The early history of the Vygotsky school in the 1930s has been variously referred to by Russian and North American scholars alike as a “blank spot in the historiography of psychology” (Sokolova, 2003), or a “major hiatus in American knowledge about Soviet psychological research” (Cole, 1977). On the other hand, there is Russian historiographical tradition of the history of the Vygotskian psychology that has been developed during the post-war period from the late 1940s onwards. The detailed analysis of the historiography of Vygotskian psychology in the making is beyond the scope of this work. However, several snapshots on this “traditional” historiography are necessary to set the stage for this study. The contemporary history of post-Vygotskian psychology is often recounted in terms of “the school of Vygotsky-Leontiev-Luria” or “the trojka” (the three) that along with “the pyaterka” (the five) of Vygotsky’s students (Zaporozhets, Bozhovich, Levina, Morozova, and Slavina) arguably constitute the main group of Vygotsky’s scientific heirs. I take this story as a point of departure and the focus of my analysis of Soviet and contemporary historiography of Vygotskian psychology.

During his lifetime Vygotsky failed to establish himself as a leader of an institutionalized scientific school in the human sciences associated with his name, unlike, for instance, Ivan Pavlov or Vladimir Bekhterev, both of whom not only founded scientific theories and rival research schools, but also set up “factories” for massive production of scientific knowledge (Todes, 2002). However, for his contemporaries the name of Vygotsky was associated with his theory of human

development that was at different times variously referred to as an “instrumental”, or “cultural” psychology, or a “theory of cultural development”, or a “theory of cultural development of higher mental (psychical) functions” (Van der Veer & Valsiner, 1991). This terminological diversity and fluidity reflects the constant search for the adequate descriptors for the original research program and theoretical innovation introduced by Vygotsky and his associates. Vygotsky died on June 11, 1934, which is when the history of Vygotskian psychology starts. The first written contributions to this emergent historiography of Vygotsky’s psychology (that perhaps tell us more about their authors than the object of their papers) are obituaries and memorial speeches written or delivered by his closest collaborators soon after Vygotsky’s death (A. N. Leontiev, 1934; A. R. Luria, 1935a, 1935b, 1935/2003).

During his lifetime Vygotsky and his associates frequently became the object of critique, most often scientifically ungrounded and politically motivated critique. However, it was only after the July 4, 1936 publication of the notorious Central Committee of the Communist Party decree of against “paedological perversions” in the system of Narkomproses (i.e. Ministry of Education) that a campaign of public critique of Vygotsky’s theory was launched. As a result, although Vygotsky was never officially banned, he—posthumously, two years after his death in 1934—became a *persona non grata* in academic circles for almost two decades, and the number of references to Vygotsky’s name and his published scientific works dramatically decreased (Vygodskaya & Lifanova, 1996a, 1996/1999).

The situation changed radically in 1956 when a volume of Vygotsky’s works (Vygotsky, 1956) along with several articles with the analysis of Vygotsky’s contribution to psychology were published (A. N. Leontiev & Luria, 1956). This book was followed by another volume that came out four years later (Vygotsky, 1960)



under the editorship and with a preface by Soviet leading scholars, including Leontiev, Luria and Teplov (A. N. Leontiev, Luria, & Teplov, 1960). These publications seem to be most closely related to the change of the political climate in the country after the groundbreaking speech *On the Personality Cult and its Consequences* given by Nikita Khrushchev on February 25, 1956 at the 20th Congress of the Communist Party of the Soviet Union (February 14-25, 1956). Thus, on June 25, 1956 only a few months after the Party Congress, Vygotsky's first afterwar book was submitted to the publishing house, successfully passed censorship, and signed off to press on October 25 of the same year. The change is also evident in the publication of the second, 50-volume edition of *Bol'shaya Sovetskaya Entsikolpediya* (Great Soviet Encyclopedia): the name of Vygotsky is never mentioned in the first 49 volumes, or more specifically, in the main article *Psychology* (pp. 238-244 of volume 35 that came out in 1955). Notably, this article refers to a great many other Soviet and foreign psychologists (Marxists and non-Marxists alike), such as Wundt, Ebbinghaus, Titchener, James, S. Rubinstein, Kornilov, Smirnov, Teplov, Zaporozhets, Anan'ev, Ladygina-Kots, Voitonis, Kravkov, Leontiev, Bozhovich, and Zankov. Two years later, however, chapter XV of *Bol'shaya Sovetskaya Entsikolpediya* (Great Soviet Encyclopedia) volume 50, on *Science and Scientific Organizations* [in the USSR] (pp. 430-566) asserts that "studies by Vygotsky (1896-1934) and the group that he supervised constituted a considerable stage in the history of Soviet psychology" and that "Vygotsky was one of the first among the Soviet authors to emphasize the importance of the problem of consciousness for materialist psychology... Not only did he manage to surpass the limits of the 'behaviouristic' view of a human being, but also overcame the idealistic conception of consciousness as a special, self-contained world of purely subjective phenomena" (p. 560); and that "Vygotsky demonstrated

the leading role of education in children's mental development and conducted the first ever studies of children's acquisition of scientific concepts" (p. 561). Overall, these publications provided overviews of Vygotsky's scientific legacy and, by the standards of that time, a relatively mild critique.

The period of 1956-60 can thus be called as the beginning of Soviet historiography of Vygotskian psychology. Another significant contribution to that historiography was a two-volume edition of *Psikhologicheskaya nauka v SSSR* (Psychological science in the USSR) that came out in 1959 and 1960. This foundational publication featured virtually all the trends in theoretical and empirical psychological research of the day. This publication set the "gold standard" of discussion of Vygotsky's theory in the Soviet Union for the next decades. Thus, in the research overview of the Soviet developmental studies that Leontiev for the volumes, he credited Vygotsky as an original and innovative thinker, while pointing out that Vygotsky's general theory of psychological development had never been completed in his lifetime and that several of Vygotsky's hypotheses were subsequently developed by Soviet Marxist researchers. Quite characteristically, Vygotsky's work is discussed at length on a couple of pages of Leontiev's chapter and is also briefly mentioned on a few other occasions, while the rest of Leontiev's chapter is abundant with references to Marx and Engels. Thus, Leontiev ultimately presents a moderate criticism of Vygotsky from the perspective of Marxist general psychological theory. Quite in contrast with Leontiev's account, several other chapters of this book authored by Luria, Zeigarnik, Solov'ev and others present overviews of special and applied fields of psychology (including Soviet research on abnormal and pathological development, and child and clinical psychology), in which Vygotsky's contribution to these special fields is discussed at considerable length and is lavishly praised. In any

case, Leontiev's activity theory was positioned at that time as the mainstream version of Marxist psychology, was used as an umbrella term for a wide range of studies in the Vygotskian tradition of the 1960-70s, serving as a protective shield for other Vygotskian psychologists in the Soviet Union (V. P. Zinchenko, 2003).

This trend began to change around the mid-1970 to early 1980s with a series of memoirs, public presentations, and interviews of the main protagonists of the history of Vygotskian psychology in the Soviet Union (A. N. Leontiev, 1976/1986; A. N. Leontiev & Leontiev, 1976/2005; A. N. Leontiev & [Luria, 1976](#); A. R. Luria, 1979, 1982) as well as a number of publications by their followers and students (Davydov & Radzikhovskii, 1980, 1980/1985, 1981; Radzikhovskii, 1979). Quite a few of these appeared around the time of the death of Alexander Luria (in 1977) and Aleksei Leontiev (in 1979) (e.g., Zaporozhets & Elkonin, 1979), which might account for the laudatory and elevated rhetorical style of these publications, and the sometimes uncritical and rather heroic depiction of the protagonists: Among all these, two memorial edited volumes on the scientific legacy of the two scholars are especially notable as the two foundational and comprehensive collections of articles on the life and scientific legacy of Luria and Leontiev authored by their former colleagues and students (Khomskaya, Tsvetkova, & Zeigarnik, 1982; Zaporozhets, Zinchenko, Ovchinnikova, & Tikhomirov, 1983).

My main point is that the "canonical" version of the history of Vygotskian psychology is based on several accounts made by the "founding fathers" and their later reconstructions and interpretations. In fact, this canonical story was first presented by Aleksei A. Leontiev-jr.—himself a prominent psychologist and psycholinguist—in his chapter in a memorial volume in honour of his father (A. A. Leontiev, 1983). A.A. Leontiev-junior's narrative was mainly based on a series of

A.N. Leontiev-senior's personal archival materials, first published in 1988 under the title *Materials on consciousness* (A. N. Leontiev, 1940-41/1988) and interviews A.A. Leontiev made with his father in the fall of 1976. The transcript of the handwritten notes taken by A.A. Leontiev during these interviews was published only recently (A. N. Leontiev & Leontiev, 1976/2005). The impact of A.A. Leontiev's 1983 publication was considerable since this account has been repeated and disseminated by many later authors (Ivanova, 1995, 2002; A. A. Leontiev, Leontiev, & Sokolova, 2005; Sokolova, 2001, 2007) and until recently considered to be virtually the sole—or certainly the most authoritative—source on the history of the Vygotskian school.

A.A. Leontiev's account has several key narrative elements. The core of the narrative is the story about the 1924 meeting in Moscow of the three founding fathers, Lev Vygotsky, Alexander Luria and Aleksei Leontiev, and formation of the first alliance of the “trojka” (“the three”). Then, a second alliance was formed by the five first-generation students of “the triumvirate”, named “pyaterka” (“the five” in Russian), which included Alexander Zaporozhets, Lidia Bozhovich, Roza Levina, Nataliya Morozova, and Liya Slavina. According to this canonical narrative, the group of these eight individuals formed the “school of Vygotsky”. Typically, this narrative does not provide us with any information about the later fate of “the pyaterka”, but we are told that at the end of 1931 a group of Vygotsky's followers that included a part of the trojka (Luria and Leontiev) and a part of the pyaterka (Zaporozhets and Bozhovich) left Moscow for Kharkov, then capital of Soviet Ukraine, to start their work in a new organizational unit, Sector of Psychology at the Ukrainian Psychoneurological Academy (UPNA). There, in Kharkov the Muscovites met with a group of local scholars, the Kharkovites, including Piotr Gal'perin, Piotr Zinchenko, Vladimir Asnin, Kseniya Khomenko, and Grigorij Lukov, who formed an

alliance that later became to be known as the Kharkov group or the Kharkov school of psychology. According to A.A. Leontiev's narrative, Luria headed the Psychology *Sektor* (Sector) at UPNA, whereas a position of a head of the *Otdel* (Department) of Child and Genetic Psychology was offered to A.N. Leontiev. However, A.R. Luria reportedly soon returned to Moscow; and after his departure A.N. Leontiev became the head of the Psychology Sektor and supervised virtually all administrative and research work from then on. A series of recent publications added the name of M.S. Lebedinsky as one of those scholars invited from Moscow to Kharkov as well as other minor details (e.g., A. A. Leontiev, 2003/2005, p. 32), but the structure of the narrative, the sequence and the interpretation of the main events remains basically identical across the vast majority of "traditional" publications in this topic.

The history of "trojka da pyaterka" ("the three and the five" in Russian) and the interpretation of the history of Vygotskian psychology along the lines of A.N. Leontiev's activity theory is not the only interpretation of the history of the Vygotskian research project after Vygotsky's death. The idea of "the school of Vygotsky - Leontiev – Luria" did not appear until the end of the 1970s, and a number of researchers questioned its validity now and then, and doubted the legitimacy of the claims of the continuity of Vygotsky's original framework in the research program of activity-oriented psychological studies. For instance, Luchkov and Pevzner (the latter, as it happened, another of the first-generation of Vygotsky's students) argued that "we have every reason to doubt the appropriateness of the phrase the "activity theory of Vygotsky - Leontiev - Luria", as well as the juxtaposition of these three names with respect to this theory. Furthermore, in the school of Leontiev, it has recently become usual to talk, generally, about "the school of Vygotsky - Luria - Leontiev" ... which is also illegitimate, in our opinion" (Luchkov & Pevzner, 1981, pp. 251-252).

However, the full-scale revision of the “trojka da pyaterka” version of the history of Vygotsky school started around the end of the 1980s-beginning of the 1990s and seems to have coincided with the processes of *perestroika* in the Soviet Union. The traditional history of Vygotskian psychology evoked much criticism from authors who questioned Leontiev’s role as leader of the school of Vygotskian scholars and challenged the claim that Leontiev’s activity theory was a direct continuation of Vygotsky’s theory of cultural development. The critique of the “official” historiography can often be recognized by the authors’ insistence on the split between Vygotsky on the one hand and Leontiev and the Kharkov group that reportedly departed from Vygotsky’s research program, on the other. Another characteristic feature of the “revisionist” historiography of Vygotskian psychology is the authors’ insistence on the rupture in Vygotsky’s theory development in the works of his students and followers—despite recognizing the continued influence of Vygotsky on his students (Kozulin, 1984, 1986, 1990; Orlov, 2003; Valsiner, 1988; Van der Veer & Valsiner, 1991; Veresov, 2007). Some scholars even discuss Leontiev’s and Luria’s personal betrayal of Vygotsky and “his case” that allegedly took place some time around the end of 1933, approximately half a year before Vygotsky’s death (Van der Veer & Valsiner, 1991; Vygodskaya & Lifanova, 1996a, 1996/1999). However, despite considerable criticism of the “school of Vygotsky-Leontiev-Luria”, scholars in favour of the canonical interpretation of the history of Vygotskian psychology are still very active and skillfully defend against the revisionists by providing new evidence in support of the direct lineage from Vygotsky’s cultural-historical psychology to Leontiev’s activity theory (A. A. Leontiev, 2003, 2003/2005; A. A. Leontiev & Leontiev, 2003; A. A. Leontiev, Leontiev, & Sokolova, 2005; Sokolova, 2001, 2005, 2007; Voiskunskii, Zhdan, & Tikhomirov, 1999; Zhdan, 2007).

There are a number of problems with both the canonical history of Vygotsky-Leontiev-Luria school and its critical reassessments from the 1990s onwards. Neither version proves sufficient in its effort to explain the processes taking place in the 1930s, and both abound with gaps and inconsistencies that I discussed elsewhere (Yasnitsky, 2008; Yasnitsky & Ferrari, 2008a, 2008b). These numerous problems with the history of Vygotskian psychology related from the perspective of the “Vygotsky-Leontiev-Luria school” seem to be deeply rooted in an outdated methodology of historical research widely criticized as “traditional”, “Whig” or “Whiggish”, or “insider’s” historiography<sup>1</sup>. These “Whig” histories of Vygotskian psychology tend to share several common features regardless of their authors’ ideological orientations and affiliations with specific psychological schools. First, the traditional history most often results in presentist and progressist reconstruction of the “development of activity theory” by Leontiev and his associates or, alternatively, the lamentation for the lost opportunities for the development of Vygotsky’s theory. Second, these rationalistic histories of ideas often disregard the cultural and social circumstances of the scientific enterprise. Kurt Danziger in his classic book on the history of Wundt’s psychology discusses the distinction between the methodology of the “insider’s” and the “outsider’s” history, and it seems that all the drawbacks of the traditional history of Vygotskian psychology can be attributed to the insider’s perspective in historical studies:

Historical studies of the sciences tend to adopt one of two rather divergent points of view. One of these typically looks at historical developments in a discipline from the inside. It is apt to take for granted many of the

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<sup>1</sup> The alternative to the traditional “Whiggish” history is sometimes presented as the research program of the “new history”. Whereas this approach is clearly quite promising and advantageous for the needs of constructing the social history of science, the distinctive characteristics of the alternative “new history” are far from clear and still require further theoretical and methodological elaboration (Lovett, 2006).

presuppositions that are currently popular among members of the discipline and hence tends to view the past in terms of gradual progress toward a better present. The second point of view does not adopt its framework of issues and presuppositions from the field that is the object of study but tends nowadays to rely heavily on questions and concepts derived from studies in the history, philosophy, and sociology of science. A history written from the insider's point of view always conveys a strong sense of being "our" history. That is not the case with the second type of history, whose tone is apt to be less celebratory and more critical. In the case of the older sciences, histories of the second type have for many years been the province of specialists in the history, philosophy, or sociology of science. This is not, or perhaps not yet, the case for psychology, whose history has to a large extent been left to psychologists to pursue. Accordingly, insiders' histories have continued to have a prominence they have long lost in the older sciences (Danziger, 1990, p. vii)

Indeed, the history of psychology in the Soviet Union was typically studied not by the historians of science but by psychologists themselves. Most often such "internalist" histories are scientific biographies written by the relatives, students or the "scientific heirs" of the great scientist of the past. Third, the vast majority of these studies follow the tradition of "Great Men" histories, or, in other words, represent hagiographic historiography that is characterized by distinctly celebratory (or, otherwise, accusatory and denunciatory) accounts of the history of ideas against the background of personal life-stories of a few protagonists, typically, Vygotsky, Leontiev, or Luria. Finally, virtually all these insiders' hagiographic histories seem to share the common conception of the interrelation between state and science in



Stalinist Soviet Union as the “two opposing entities locked in uneven conflict, with the state in the role of dictator and oppressor, and the scientists as victims, trying to defend their autonomy” (Krementsov, 1997, p. 4). (In fact, the presupposition of the symbiosis between the state and science in the Soviet Union from 1920s onwards, their interdependency, gradual fusion and cultural unification seems to be very productive in our effort to understand the complexities of Vygotsky’s and post-Vygotskian psychology development in the Soviet Union). Another consequence of the present day fascination with a great figure of a protagonist—be they one of the “Founding Fathers” (Vygotsky, Luria, Leontiev) or their “inferior”, and significantly less known disciples in the West (e.g., Galperin, Elkonin or Zinchenko)—is that the figure of a “solitary thinker” viewed from a very close distance obscures the monumental edifice of the ambitious project of reforming the ensemble of the human sciences launched by Vygotsky, and to certain extent developed by his students. Both the interrelatedness of numerous smaller research projects carried out by members of Vygotsky Circle, and the focus on their specific practices and experimental designs, can only be understood with respect to the social practice of the Vygotskian science at that time. In this sense, the excellent work done by Krementsov in his *Stalinist Science* (1997) is a great help to any researcher attempting to understand the intricacies of the inner workings of Soviet science and its official, public life at that time. One should particularly keep in mind such factors as “the merging of the scientific community and the party-state control apparatus on the level of both institutions and individuals; the subordination of science-policy decision-making to the priorities of the apparatus; the centralized, pyramidal, rigid, hierarchical structure of scientific institutions; the fierce competition among various groups within both the community and the party-state agencies; the tight administrative control over

institutional structures, appointments and certification of scientific personnel, research agendas, the international and domestic scholarly communications; the translation of the communities interests into “Newspeak” of party bureaucracy; the militant style of scientific criticism; and the peculiar party “etiquette” that defined the required rituals of scientific behaviour”. Furthermore, most importantly, these features of the Stalinist science were universal and mandatory, and “*there was not a single scientific or scholarly discipline in the Soviet Union to which they did not apply and whose fate was not shaped by them*” (Krementsov, 1997, p. 8). This perspective radically changes our perception of the scientific *modus vivendi* of the time and helps us understand many problematic issues of the history of Vygotskian psychology, such as the interrelations between the members of the Circle and the real meaning of their public mutual criticism and self-criticism, the stylistic differences between public presentation and inter-group reports or private notes, the issues of authorship of texts and ideas, the choice of experimental designs and of the experimental data interpretation, strategies for publication—and non-publication,—the meaning of groupings and sub-groupings within the Vygotsky Circle, and many more such issues. In the last years of his life, Vygotsky was allegedly reflecting on the possibility of expressing psychology in terms of conflict, collision and drama. Indeed, the history of Vygotskian psychology understood as the history of scientific practice—that is, as the combination of experimental designs and practices, moral economy of the group, and social techniques and technologies—will help us uncover perhaps the biggest drama of Vygotskian psychology. This is the drama of how Vygotsky’s light and free style of scientific thinking and writing was transformed into the self-censored thought and convoluted “Newspeak” of the members of Vygotsky Circle after his death.

## Chapter 2. Methodology of the study

The methodology of this study is shaped by the idea that, “remarkably, Vygotsky’s approach *de facto* embodies, in its real life history, the very theoretical principles central to it, such as the inseparability of knowledge and action, theory and practice, and the collaborative nature of cognition” (Stetsenko & Arieviditch, 2004, p. 58). Thus, for instance, according to Vygotsky’s famous dictum, each mental function in human cultural development “appears on the stage twice, in two planes, first, the social, then the psychological, first between people as an intermental category, then ... as an intramental category. ... Genetically, social relations, real relations of people, stand behind all the higher functions and their relations” (Vygotsky, 1931/1997, p. 106). Still, Vygotsky’s theory of cultural development of higher mental functions can not be directly applied to the needs of research in the history of science, so a certain approximation of Vygotskian principles of his theory of human development must be adapted to the study of the history of science. Following the proposal of a cultural-historical approach to the history of Vygotskian psychology (Cole, 1996; Stetsenko, 2003, 2004; Stetsenko & Arieviditch, 2004; Valsiner & Van der Veer, 2000), this study contrasts with the traditional account of the history of the school of Vygotsky-Leontiev-Luria by investigating its “cultural-historical school as a collaborative, multi-generational, value-laden, and ideologically-driven investigative project that stretched far beyond the confines of science in its traditional mentalist guise” (Stetsenko, 2003, p. 96). This task places a special emphasis on two interrelated research issues: first, the collaborative nature of Vygotsky’s project, and, second, the scientific practices of Vygotsky’s group. These two issues are briefly discussed below.

### *Vygotsky Circle*

Most studies of Vygotsky-Leontiev-Luria et al.'s collaborative project were developed under the banner of research on the “scientific school”, and, according to their taste and affiliation, authors tend to discuss the school of Vygotsky alone, of Vygotsky-Leontiev, of Vygotsky-Luria, or of Vygotsky-Leontiev-Luria. However, there are several problems with referring to Vygotsky's collaborative research project as a “school”. Schools often have a hierarchical organization with the leader (“Founding Father”) at the top of the pyramid, loyal members of the school and to the school's leader, the same geographical locale and institutional affiliation of school members, and a controversial and combatant attitude towards representatives of other competing schools. Scientific schools are typically understood as “small groups of mature scientists pursuing a reasonably coherent programme of research side-by-side with advanced students in the same institutional context and engaging in direct, continuous social and intellectual interaction” ([Geison, 1981, p. 23](#)).

On this view, the designation “scientific school”—while quite applicable, for instance, to the scientific groups of I.P. Pavlov or V.M. Bekhterev, or even, arguably, to A.N. Leontiev's activity theory and the postwar Department of Psychology of Moscow State University under his leadership in 1950-70s—can hardly be applied to the collaborative research of Vygotsky and his associates. In striking contrast to later reconstructions of the “Vygotsky school”, Vygotsky's collaborative project has virtually none of the characteristics of a proper scientific school: for instance, it did not have a strong authoritative leader (from the administrative point of view Luria was most often superior, or at least equal to Vygotsky), a single institution (the group

was dispersed between more than a dozen institutions in three cities of the country<sup>2</sup>) or a unified and hierarchically controlled research program. Various groupings of scholars and their interpersonal connections are typically discussed not only as “schools”, but as “scientific” or “research schools”, “research groups”, “scientific “communities”, “invisible colleges”, “solidarity groups”, “scientific networks” etc. ([Geison, 1981, 1993; Servos, 1993](#)). In this study I prefer to use the notion of “circle” borrowed from Bakhtinian studies to emphasize the crucial ideas of dialogue and polyphony in scientific research. Thus, Bakhtinian scholars pointed out that the *kruzhok* (circle) was the major form of intellectual life in Russia since the 1830s, the most notable examples of circles being the Vitebsk circle of Bakhtin (that also included Medvedev, Voloshinov, and many others), Kazan’ Psychoanalytical Circle of Luria, the Petrashveskii *kruzhok*, known as Petrashvetsy (among whose members was F.M. Dostoevsky), the formalists of OPOJAZ, the Moscow linguistic circle of Roman Jakobson, Grigory Vinokur and others, and the Prague linguistic circle (organized in Prague by the immigrants from the Soviet Russia) (Brandist, 2002; Shepherd, Brandist, & Tihanov, 2003).

In this study, I introduce the strict notion of the “**Vygotsky Circle**” There are several historical precedents of reference to “Vygotsky Circle” in scholarly literature (e.g., [Blanck, 1990, pp. 39-40; Bruner, 1995, p. 84](#)), but these seem to be used indiscriminately and non-terminologically as a mere substitute for “group” or “milieu”. For example, somewhat different meaning of the word “circle” is used by Anna Stetsenko in her introduction to Vygotsky’s *Tool and sign* when she reminds us that

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<sup>2</sup> For instance, Vygodskaya and Lifanova reconstruct a non-comprehensive list of *some* of the institutions where Vygotsky was employed from 1924 to his death in 1934 (Vygodskaya & Lifanova, 1996a, pp. 133-134). This list comprises most impressive eighteen items for Vygotsky alone!

Vygotsky wrote *Tool and Sign* in close collaboration and in lively discussions with a number of people... They formed the so-called Vygotsky Circle, which included several brilliant women, and they carried out research projects collectively. It is quite revealing, in this respect, that even the authorship of *Tool and Sign* is disputed; there is some reason to believe that Vygotsky wrote it together with Luria... Whatever the case of the authorship of this particular work, the ubiquitously collaborative nature of Vygotsky's project in general must be emphasized, especially because it has often been underestimated or even ignored in previous accounts of his heritage (Stetsenko, 2004, pp. 502-503).

In a similar vein Oskana Bulgakowa discusses the “circle” of Sergei Eisenstein that included both Vygotsky and Luria (Bulgakowa, 2007).

Ironically, perhaps the most precise depiction of Vygotsky Circle—or, for that matter, “Vygotsky Circles”—was given by none other than the “godfather” of the “school of Vygotsky-Leontiev-Luria” construct: by A.N. Leontiev himself! Thus, in his 1976 interview an important slip of tongue takes place: Leontiev says “In 1927-29, up to 1930 there started gathering *circles* around Vygotsky” (A. N. Leontiev & Leontiev, 1976/2005, p. 373).

However, all these references to “Vygotsky Circles” in the scholarly literature are fairly incidental, and a systematic analysis of this Circle has never been done. By analogy with fairly extensive Bakhtinian scholarship and in contrast to the hierarchical, directed, centralized and institutionalized image of a “school”, the notion of *Vygotsky Circle(s)* I introduce here seems to provide much better opportunities for studying problematic cases of disputed authorship, collaborative work in the conditions of the high mobility of the members of a completely distributed research

group, temporary and overlapping alliances of these scholars, trans-disciplinary scientific and social transformative projects, and the prevalence of tacit knowledge within the group under conditions of censorship, and increasing state control over science and political repression. Of particular interest and importance is the roughly ten year period of 1931-1941 as the formative years of what we now know as Vygotskian psychology (in its various social, cultural, historical, and activity-oriented guises): specifically, the work of the group Vygotsky's associates in three cities of the former Soviet Union—Moscow, Kharkov, and Leningrad. The history of the Kharkov fraction of Vygotsky Circle was briefly investigated in our previous research (Yasnitsky, 2008; Yasnitsky & Ferrari, 2008a, 2008b), and this study presents the first systematic exploration of the entire network of Vygotskian scholars and the dynamics of its change before the WWII.

### ***Scientific Practices and Doing Science***

Science can hardly be separated from its social and cultural context. This is particularly true of the Soviet science of the Stalin's era. Yet, the vast majority of those rare studies on the topic present either a traditional rationalist "history of ideas" or a fairly old-fashioned biographical narrative about a protagonist, or a combination of the two. As a result, Vygotsky is commonly presented as a revolutionary "thinker", yet virtually nothing is known about the actual empirical experimental studies he did or supervised in a wide range of research, educational and clinical contexts.

Vygotskian experimental science still remains a mystery to most of us. This is a loss to the history of psychology, but even a greater loss to the psychology as a contemporary practice. The history of "Vygotsky Circle" is bound to be a variation of a social history. In this study I present a somewhat novel program of research on the

history of Vygotskian psychology that requires shifting focus from a biography of ideas to the actual practices of scientists; that is, to a focus on actually doing Vygotskian science in its sociocultural context. This work follows the lead of those historians of science who treat science as social production (Clarke & Fujimura, 1992; Knorr-Cetina, 1981; Latour & Woolgar, 1979; Pickering, 1992; Shapin & Schaffer, 1985), and emphasize investigating the complexities of the inner workings of research groups and their scientific practices ([Kohler, 1994](#); [Todes, 2002](#)). Generally, speaking of scientific practice, historians and sociologists of science primarily mean *experimental* practice, and neglect the *social* practice of science which “produces formal and informal organizations, patrons, careers, roles, and policies” (Krementsov, 1997, p. 308). Clearly, the social practice of science is intrinsically linked to the interpersonal dimension of science as a network of actors, which is sometimes described as a *moral economy* as “a web of affect-saturated values that stand and function in well-defined relationship to one another”, where the *moral* component of the term “refers at once to the psychological and to the normative” (Daston, 1995, p. 4).

In his study of “Stalinist science” Krementsov analyses three major components of the social practice of Soviet scientists. These are language, public behaviour, and styles of criticism (Krementsov, 1997, p. 6). For the purposes of a history of the cross-disciplinary research project of the scholars of Vygotsky Circle in the circumstances of Stalinist science of the 1930s, I also discuss three components of Vygotskian research project: (1) empirical studies, (2) interpersonal and social relations of the protagonists, and (3) discursive practices. I attempt to cover both empirical and social dimensions of scientific enterprise, however, at the present stage of historiographical scholarship a thorough analysis of empirical research is virtually



impossible. Therefore, I place considerable emphasis on the interpersonal relations, career making strategies, and discursive strategies of the members of Vygotsky Circle.

### ***Historical and Interdisciplinary Study of Vygotskian Developmental Theory***

According to a fundamental principal of Vygotsky's research program, a scientific study of any phenomenon is impossible unless it is the historical investigation of the development of that phenomenon and the dynamics of its change (Van der Veer, 1997). This is the actual meaning of the "historical" component of Vygotsky's theory of "cultural-historical development of higher mental functions". In this study, I follow this methodological principle as closely as possible. Indeed, this study is first of all a research on the history of science, in the sense that the object of my investigation is constituted by the events and processes in historical past. Then, there is yet another meaning of the principle of the historicity employed in this research, namely, that each phenomenon is of interest as a dynamic entity in its development rather than a static unit. This is the reason why one of the main foci of my study is the investigation of the processes of change, to the extent available data and analytic techniques allow for such investigation. Such emphasis on historical past and the processes of change has its clear advantages and obvious disadvantages.

The primary advantage of historical method applied in this study is that it allows us to pose the question of the origin of scientific theories and concepts and investigate these as processes within their social and cultural context rather than fixed and isolated rationalistic entities. Also, such historical research understood as the study of scientific practices involves a wealth of data that normally remains beyond the scope of the analysis of internalist studies of science and promises to shed light on the

social processes of scientific enterprise that are typically excluded from traditional historiography. Yet, a purely historical approach has its limitations that I can not avoid, at least at this stage of research development. Thus, it is fairly clear that social practices constitute a very important, but still not the main component of scientific work, and scientific knowledge production still remains the primary goal of any scientific worker. In this sense, I view my study of scientific practices as a very important effort at investigating the scientific work of the group of Vygotskian scholars in its development from the interwar period until our time. Yet, this research is complementary to the traditional research in the history of ideas as well as to the contemporary practical applications of Vygotsky's ideas in a wide range of contexts such as in clinical, educational, industrial and organizational settings , and I anticipate that in the future these two many lines of research will merge into intergral whole.

The variety of applications of Vygotsky's theory is complemented with the multitude of perspectives from which the phenomenon of Vygotsky Circle can be analysed. This brings us to the issue of the interdisciplinarity of this study at full-scale: not only is the object diverse and multifaceted and diverse, but also the methods of its adequate investigation need to be borrowed from the wide range of scientific disciplines as diverse as social psychology, philosophy and history of science, linguistics and semiotics, and sociology. Such multitude of task determines my choice of analytical techniques and the form of material presentation. This study is an attempt at providing a holistic view on the phenomenon of Vygotsky Circle as an intergral whole and their practices in their development in the interwar period, bringing together the elements of historical, sociological, psychological, linguistic, and semiotic analysis. Such presentation promises to provide a "big picture" of the phenomenon at stake, yet is necessarily limited in its depth with respect to any

specific scientific discipline involved in my analysis. This is another limitation is this pioneering study, and only future systematic interdisciplinary research will advance our understanding of the workings and the ideas of Vygotsky Circle at substantial depth and breadth.

### ***Research Data***

As Raymond Bauer wrote in 1952, “it is axiomatic in the field of Soviet studies that one is never right; he is only wrong with varying degrees of vulnerability. In this area more than in most the writer must make the choice between avoiding risks and eliciting the maximum of meaning from the material” (Bauer, 1952, p. ix). Unfortunately, the situation has not changed dramatically since then, at least in the field of the historiography of psychology. There are very few studies on our topic, either in Russian or in English, and this study is the first ever attempt to investigate the history of the Vygotsky Circle during the 1930s-1940s.

My research is based on the study of the rare publications of that time and rare ***archival materials*** that include many previously unknown texts from and about the early Vygotskian research in Soviet Ukraine that I discovered during several trips to the Ukrainian cities of Kharkiv and Kiev in 2006-2007 (Yasnitsky, 2008; Yasnitsky & Ferrari, 2008a).

This study is also based on an analysis of ***pre-war period publications*** that came out between 1931 and 1941. These typically include scholarly monographs and publications of scientific articles in Soviet journals *Sovetskaya nevropatologiya*, *psikhiatriya i psikhogigiena* [Soviet neuropathology, psychiatry, and psychohygiene], *Sovetskaya psikhonevrologiya* [Soviet psychoneurology], and *Sovetskaya pedagogika* [Soviet pedagogy], as well as a number of international scientific journals, e.g.

*Character and Personality*, *Journal of General Psychology*, *Pedagogical Seminary and Journal of Genetic Psychology*, *Psychologische Forschung*, and *Wiener Klinische Wochenschrift*, chapters in edited volumes such as *Novoe v uchenii ob agnozii, apraksii i afazii* [Advances in the teaching about agnosia, apraxia, and aphasia] and *Voprosy psikhologii glukhonemykh i umstvenno-otstalykh detej* [Some issues of psychology of the deaf-mute and mentally retarded children], conference proceedings, for instance, at such forums as *Pervyj Vseukrainskij s'ezd nevropatologov i psikhiatrov* [First All-Ukrainian convention of neuropathologists and psychiatrists] in 1934 in Kharkov, *Premier Congrès international de psychiatrie infantile* [First International congress on child psychiatry] in 1937 in Paris, a series of the *scientific sessions of Kharkov Pedagogical Institute* held in 1938-1941, and *Pratsi respublikans'koji naukovoji konferentsiji z pedagogiky i psykhologiji* [The proceedings of the republican (Ukrainian) scientific conference on pedagogy and psychology] in Kiev in 1941. Many original articles were published in Ukrainian and few of those written in Russian have been included in later republications, nor have many been translated into English.

Another invaluable source for this historical study is a series of **recent publications**, which present the groundbreaking materials from the **personal archives** of Vygotsky (Vygotsky & Puzyrei, 2004, 2007; Vygotsky, Vygodskaya, & Zavershneva, 1932/2007; Zavershneva, 2007a, 2007b, 2008a, 2008b), Leontiev (A. N. Leontiev, 2003), Luria (A. R. Luria, 2003), Galperin (2009, in press) that dramatically change our understanding of the work of Vygotsky and his school during the 1930s. There is an increasing scholarly interest in Soviet psychology of the 1930s, and many previously unpublished materials closely related to the work and life of the members of Vygotsky Circle were published in Russian during last decade or so (A. A.

Leontiev, Leontiev, & Sokolova, 2005; A. N. Leontiev, 1994, 2003; A. R. Luria, 2003; E. A. Luria, 1994; Voiskunskii, Zhdan, & Tikhomirov, 1999; Vygotskaya & Lifanova, 1996a, 1996/1999). Quite a few of these recent publications came out in English during the last decade in the *Journal of Russian and East European Psychology* in thematic issues on L.S. Vygotsky, A.N. Leontiev, A.R. Luria, N.A. Bernshtein, P.I. Zinchenko, L.I. Bozhovich, and others.

Even so, resources on the history of this period of the development of Vygotskian psychology remain extremely scarce and not reliable. There are a number of 1980-2000s republications of the works of Vygotskian scholars of 1930s, which are typically considerably edited and abbreviated in comparison with the original publications of the 1920-1940s. Van der Veer, who back in 1997 noted that “it must be realized that reliable archives of Vygotsky’s writings open to the general public or interested expert do not exist”, was perhaps the first to question the reliability of available Vygotsky’s texts. By extension, I argue that the issue of sources reliability poses one of the major problems to the entire domain of the historiography of Soviet psychology, especially during the initial phase of its development and institutionalization in the interwar period (i.e. in 1920-1940s). Indeed, the archives of the protagonists of Vygotsky Circle—for instance those of Vygotsky, Leontiev, Luria, Elkonin, and Bozhovich—remain stored in the private apartments of their heirs and are generally not open to a researcher. The situation remains virtually unchanged over the last decade, and up to now “there is no Vygotsky Center which on demand sends facsimile copies of original manuscripts, papers, and letters to interested researchers. There is no Vygotsky Library where we can consult copies of Vygotsky’s publications as well as those of his contemporaries. What exists are the family archives which by nature are accessible to only a very limited group of people and the

private archives of different people interested in Vygotsky. This situation poses a problem to those who want to study Vygotsky's work thoroughly" (Van der Veer, 1997, p. 1). Furthermore, even the published sources are questionable as trustworthy data since foundational texts, most notably, the works of Lev Vygotsky underwent considerable editing—if not censorship—in the Soviet publications of 1950-80s that lead to notable omissions and even distortions of the meaning of the original texts, which was noted by quite a number of researchers (Peshkov, 1999, 2008; Tkachenko, 1983; Tulviste, 1987; Van der Veer, 1997; Zavershneva, 2009, in press).

Van der Veer provided an overview and analyzed the reliability of Vygotsky's works. He distinguishes between three groups of sources such as (a) manuscripts not published during Vygotsky's lifetime, b) works published posthumously, and c) republications. The virtual lack of scholarly work on the sources from group (a) in the middle of 1990 signalled by van der Veer has recently been addressed by the group of Russian scholars lead by Ekaterina Zavershneva whose recent groundbreaking publications in Russian (e.g., Zavershneva, 2007a, 2007b, 2008a) will come out in English translation in the *Journal of Russian and East European Psychology* by the end of this year. The posthumous editions of Vygotsky's works of 1930s that constitute group (b) are not entirely reliable for the reasons outlined by van der Veer: these publications—with the only exception being Vygotsky's *Thinking and speech* (1934)—were made on the basis of students' shorthand notes and stenographic reports of Vygotsky's University lectures and conference presentations and had not been authorized by its author for publication. Relatively reliable, these publications still need to be dealt with with caution. The situation with the sources of group (c) is much more problematic: van der Veer remarks that "Soviet republications of original texts published in 1920s or 1930s tend to be notoriously sloppy and long lists of errors

could be compiled. Most of these errors are of minor importance and regard, for example, the orthography. Others are most annoying and concern deleted references to authors now out of favor (e.g., Trotsky, Watson) and the suppression of unacceptable terms (e.g., pedology)... We may conclude that the status of works published under the name of Vygotsky has not always been sufficiently clear and that scholarly editions have been exceedingly rare". Furthermore, "the Russian editors of Vygotsky's *Collected Works* [of 1982-84] have done little to solve the problems listed above. They did not attempt to unearth thus far known writings by Vygotsky, did not question the reliability of the texts used, made use of unreliable republications (e.g., in the case of *Thinking and Speech*), and in republishing original publications introduced mistakes of their own. This does not diminish the value of the work they accomplished (e.g., the tremendous number of useful notes), but it should make us realize that the critical and scholarly reception of Vygotsky's writings is still in its infancy" (Van der Veer, 1997, p. 2). Minimal progress has been made up to now in textological analysis of the versions of Vygotsky's published texts and comparing these with the available Vygotsky's manuscripts, and rare—and therefore most valuable—exceptions (e.g., Zavershneva, 2009, in press) only confirm van der Veer's diagnosis of "textological infancy" of Vygotskian scholarship that he made more than ten years ago.

In this study, however, unlike van der Veer, I was primarily interested not in Vygotsky's texts per se or, for that matter, on the scientific texts of his collaborators and associates, but on the wide range of historical documents—that naturally include scholarly publications as well—that shed the light on the social history of the group of scholars of Vygotsky Circle in the interwar period. Thus, in addition to scientific

publications of that time, the sources that I used in this study fall under several general categories:

1. **Research overviews** found in a number of publications of 1930-1960s describing the main problems and themes of their research (Galperin, 1934; Goldenberg, 1934; Kolomiets', 1934; Lebedinskii, 1933a, 1936a; A. R. Luria, 1960; Rokhlin, 1936; Rybnikov, 1940; Shmelkin, 1934; B. V. Zeigarnik & Rubinshtein, 1960). Notably, we know about quite a number of studies done in interwar period only through later their rendition by other scholars of the Circle. Some of these studies, like Bozhovich' research on the child's speech and practical intellectual activity that was known until recently only as a second-hand account (Zaporozhets, Zinchenko, & El'konin, 1964, 1964/1971), were eventually published on their own (Bozhovich, 1935/2006a, 1935/2006b, 1935/2006c). However, several studies remain fairly hard to date and no manuscripts are known. These are, for instance, the studies by Zaporozhets and Asnin on sensation formation reported by A.N. Leontiev (A. N. Leontiev, 1947a), a series of psychological studies in educational settings by Zinchenko, Bozhovich, Ginevskaya, and Lukov reported by Leontiev and Zaporozhets (A. N. Leontiev, 1947b, 1947/1975, 1947/1978, 1947/1983a; Zaporozhets, 1941a), Filip Bassin's research on concept formation in schizophrenia, according to the accounts of Luria, S.Ya. Rubinshtein and Zeigarnik, completed in mid-1930s (A. R. Luria, 1960; B. V. Zeigarnik & Rubinshtein, 1960).

2. **Secondary literature**, including compiled *lists of authors' publications* and *commentaries to the volumes of selected works* of the key figures typically prepared by their students or collaborators. For example, in addition to Vygotsky's comprehensive bibliography compiled by Lifanova (Lifanova, 1996; Vygotskaya &



Lifanova, 1996a), bibliographies are available for Bozhovich, Elkonin, Leontiev, Luria, and Zaporozhets (see Bozhovich, 1995; Elkonin, 1989; Homskaya, 1992/2001; A. A. Leontiev, Leontiev, & Sokolova, 2005; A. N. Leontiev, 1983; A. R. Luria, 2003; Zaporozhets, 1986). Another important resource is *biographic and memoir literature* by the scientists themselves or members of their families (Elkonin, 1983, 1983/1984; Galperin, 1983, 1983/1984; Ginevskaya, 2005; Haenen & Galperin, 1989; A. A. Leontiev, 1983, 2003, 2003/2005; A. A. Leontiev, Leontiev, & Sokolova, 2005; A. N. Leontiev, 1976/1986, 1999; A. N. Leontiev & Leontiev, 1976/2005; A. R. Luria, 1979, 1982; E. A. Luria, 1994; Morozova, 1983; Vygotskaya & Lifanova, 1996a, 1996/1999).

3. **Archival materials**, including official documents (e.g., orders, research plans and reports, personal files, stenographic transcripts) from the main organizations within which research was done, for instance, Kharkov State Pedagogical Institute, Ukrainian Pedagogical Research Institute, and Ukrainian Psychoneurological Academy. Several other important archives (e.g., the archives of the Institute of Preschool Education, Institute of Psychology, Moscow Region State Archive, etc., all in Moscow) that have not yet been systematically explored by historians of science most likely also contain important documents on the history of Vygotsky Circle, and remain to be investigated in the future.

4. **Manuscripts** of that time, some of which still remain unpublished (Galperin, 1936; Lebedinskii, 1938a; A. R. Luria, 1941; G. I. Voloshin, 1939; Zaporozhets & Lukov, 1936). A number of other important manuscripts have been reported to exist (Bassin, 1938; A. R. Luria, 1937a, 1940b; Zaporozhets, 1934, 1937, 1939b; Zaporozhets & Asnin, 1934), but unfortunately I was not able to gain access to them.

5. *Publications in foreign languages* outside of the Soviet Union, which include the 1920-30s publications of scholarly works of Soviet researchers (Lebedinsky, 1936; Lebedinsky & Luria, 1929; A. R. Luria, 1932a, 1936, 1937b; Rosenstein, 1930/1932; Salkind, 1930/1932; Vygotsky, 1934d, 1939), necrologies and research reports (A. R. [Luria, 1931b, 1931c, 1932c, 1933, 1934, 1935a, 1935b](#)), papers on the progress of Soviet science authored by their Western counterparts who traveled to the Soviet Union or had contacts with Soviet scholars (Bauer, 1952; Berman, 1939; Dewey, 1928/1984, 1929/1990; Kasanin, 1939/1944, 1944; Schultz & McFarland, 1935; Viteles, 1938; Williams, 1934a, 1934b; Zacharoff, 1931) and private correspondence such as correspondence between Alexander Luria and Max Wertheimer (King & Wertheimer, 2005), Wolfgang Köhler (Van der Veer & Valsiner, 1991), or Kurt Koffka ([Harrower, 1983](#)).

The multitude and diversity of data require their assessment against the problem of reliability of sources on the history of Soviet psychology. In this study, I developed a fairly informal scale of reliability of sources that I used throughout the work as a general guideline that helped me determine the reliability of materials. The most reliable data includes: *statistical and meta-information* (e.g., publication titles, publishing house chosen for publication, the dates when the volume was submitted to censorship institution and to the publishing house, the number of copies printed, and lists of editors' and authors'), *official Party documents* (like the decree of the Communist Party on "paedological perversions" of July 4, 1936), *personal notes*, and *correspondence* with closest friends and foreign scholars. The reliability of these data hinges on either their impersonal and universal character (e.g., publication statistics or Party decrees) or, on the contrary, personal and sincere character of exposition (e.g., personal notes and private correspondence).

At the other extreme are *utterly formal documents* authored by Soviet scholars such as publications in *Bol'shaya Sovetskaya Entsiklopediya* [Great Soviet Encyclopedia] (A. N. Leontiev, 1940a; A. N. Leontiev & Luria, 1941; A. R. Luria, 1940a) or their *correspondence with authorities* and topmost Communist Party officials. The rest of the documents can be viewed as being closer to one extreme or the other. Thus, for instance, the lack of understanding of certain processes and realities of life in Soviet Union that is often evident in ‘Westerners’ writings is sufficiently compensated by their authors’ eagerness to truly reflect upon their Soviet experiences, which makes this type of data comparatively reliable for an historical research. In contrast, the wide range of texts produced by Soviet scholars for publication or public presentation are relatively less reliable due to censorship and—even more importantly, self-censorship—in the Soviet Union from the early 1930s onwards.

With these general guidelines in mind, a researcher still needs to develop a toolkit of fairly idiosyncratic interpretative strategies in order to assess the relative reliability of any specific source. For instance, one of the main strategies for assessing memoir data that I used in this study was systematic comparison of several individual accounts of the same events presented by different participants from different angles, whenever such data was available. Other strategies included comparing different versions of the same document (for instance, a scientific article), analysis of publications statistics, and various methods of graphical representation of the network data on several dozens of Vygotsky Circle actors.

### *Structure of this narrative*

The outcome of this study is yet another narrative. I truly believe the Vygotsky's famous dictum that each mental function appears on the stage twice, or Shakespeare's even more famous dictum that "all the world's a stage", and find the metaphor of life as drama particularly appealing for presenting this investigation. Therefore, the subsequent discussion is presented as series of chapters that cover the topics of the *Scene and Setting* (socio-historical context of the Vygotsky Circle), *Protagonists* (Vygotsky Circle, its members, their interrelationships and careers, and the dynamics of the Circle change), *Action* (empirical research done by the Circle of Vygotskians), and the *Script* (discourse, discursive practices and strategies).

### **Chapter 3. The Scene and Setting:**

#### **Soviet human and behavioural sciences in 1920-30s**

The development of Soviet psychology took place amidst rapid post-revolutionary changes and is determined by the larger social processes that can hardly be separated from it. Soon after the Bolshevik uprising in October of 1917, Russian science received unprecedented privileges, state support, and lavish funding. The Soviet science of the 1920s also enjoyed relative academic freedom, especially in comparison with the monolith Stalinist hybrid of state and science of the 1930s (Krementsov, 1997). As a result, a great many subfields of humanities, psychoneurological and behavioural sciences flourished during the decade of the 1920 hosted and developed by a great many of scientific societies—to a great surprise of the Westerners who discovered a most impressive development of Soviet science by the end of the 1920s and beginning of the 1930s. Formal proof of the international recognition of Soviet behavioural and human sciences is demonstrated by several international conferences held in the Soviet Union in the 1930s: such major international scientific forums as the VII International Congress of Applied Psychology (or Psychotechnics) held September 8-13, 1931 in Moscow; the XV International Physiological Congress, August 9-17, 1935 in Leningrad and Moscow; and the VII International Congress of Genetics scheduled for 1937 in the USSR. The first international recognition of Soviet psychology is in 1929 when a representative group of ten Soviet scholars—the third largest group of Non-American scholars (after Great Britain's twenty two and Germany's seventeen members)—was delegated to the IX International Congress of Psychology held at Yale University, September 1-7. As a token of recognition of the rapid growth of Soviet psychology, Murchison's

“Psychologies of 1930s” presents a section “Russian Psychologies” that for the first time includes three chapters on the theory of higher nervous activity, the reflexological school, and “psychology in the light of dialectical materialism” authored respectively by I.P. Pavlov, A.L. Schniermann, and K.N. Kornilov (Kornilov, 1930; Pavlov, 1930; Schniermann, 1930). “Although the Russian experiment may present a confused picture in certain political and economic ways the contributions to science, specially to psychology, is in many ways striking and original,” reported Dr. Ross McFarland in 1935 on the basis of his observations of scientific practice in Soviet Russia during the summer of 1934 ([McFarland, 1935](#)). More or less similar attitudes towards Soviet research was shared by quite a few American specialists in such areas as psychotechnics (industrial psychology), psychohygiene (mental hygiene), and defectology (psychology of abnormal child development and corrective pedagogy), many of whom had paid a visit to the Soviet Russia in 1930s in order to investigate the scientific practices there, and published their reports on Soviet advancements in these areas (Berman, 1939; Schultz & McFarland, 1935; Viteles, 1938; Williams, 1934a, 1934b; Zacharoff, 1931).

It is important to consider what motivated these advances and what stood behind this most impressive international success of the Soviet science—specifically, the unprecedented success of the human sciences by the beginning of the 1920s. I would argue that sciences in the Soviet Union first and foremost owe their rapid development after 1917 to the *Zeitgeist* of the revolutionary transformation of the entire social framework shared by the leftist intelligentsia. The revolutions of the 1917 reshuffled the entire construction of pre-Revolutionary Russian society, and it was then that leftist intelligentsia gained access to the resources and to certain extent the mechanisms of power in the country in the early 1920s.

Furthermore, driven by the need to restore and reorganize the national economy of the “first ever state of the workers and peasants”, the Bolshevik Party and government introduced a utilitarian policy towards science that played a key role in the national economy. This policy determined a mutual interdependence between state and science in Soviet Russia that continuously and gradually increased over the decade of the 1920s. This convergence of state and science in the 1920s led to the close symbiosis of the two, and ultimately to the emergence of the science-state hybrid by the end of the 1930s, known as the Stalinist Science. On a practical side, the scientists were granted considerable privileges and benefits from the state as early as the first years after the revolution—the so-called period of the “War Communism”—and a great many of scientific, educational and publishing projects were launched at that time: for instance, according to the official Soviet statistic reports, the number of scientific institutions in the country after the revolution increased from 289 (in 1914) to 1263 (in 1929), and an even more impressive 1908 scientific institutions in 1932 (Krementsov, 1997).

Thus, psychological research at that time is conducted under the auspices of a number of scientific, educational and medical institutions, both old and newly established. The oldest of these organizations like the *Psychoneurological Institute* (in Leningrad, founded by V.M. Bekhterev in 1907) and Institute of Psychology (in Moscow, founded by G.I. Chelpanov in 1912, opened in 1914) were founded in the pre-revolutionary period yet reorganized and frequently merged with other institutions during the Bolshevik era. The examples of the new institutes opened after the revolution are *Akademiya Kommunisticheskogo Vospitaniya* (Academy of Communist Upbringing; founded in 1919 in Moscow, reorganized and transferred to Leningrad in 1934, where it was merged with the local pedagogical institute in 1944),

*Ekspperimental'nyj Defektologicheskij Institut* (Experimental Defectological Institute; founded in Moscow in 1929), *Ukrainskaya Psikhonevrologicheskaya Akademiya* (Ukrainian Psychoneurological Academy; founded in Kharkov in 1931-2, reorganized into the *Ukrainian Psychoneurological Institute*) in 1936-7, and the *Kharkov Pedagogical Institute* (Department of Psychology founded in 1933)—to mention but a few.

It was in mid-1920s that Vygotsky, inspired by the idea of creating a New Man, popular among intelligentsia and Party officials of the time, started his project of radical revision of the state of the contemporary psychology and wrote a manifesto for a new psychology for a New Man (Zavershneva, 2009). This manuscript entitled “Historical meaning of psychological crisis” (Vygotsky, 1927/1982, 1927/1997) was not published until early 1980s yet it had considerable influence on the development of Vygotskian research and cultural-historical theory formation. There, Vygotsky discusses three proposals for a new psychological theory to be based on a unity of theory and practice, new methods of psychological research, and a unified theory. In essence, Vygotsky’s new psychology encompasses four levels that include 1) Marxist philosophy and dialectical materialism, 2) general psychological theory proper, 3) middle-level applied disciplines like paedology or psychotechnics, and, finally, 4) social practice in medicine, education or industry. However, psychology was not the only disciple that had the ambition of unifying these four levels. Psychology was developing against the background of other, more or less well clearly defined disciplines and sciences. Thus, several disciplines and intellectual movements were competing with psychology for the status of the general theory of psychical processes and for dominance over entire field of human sciences, most notably psychoanalysis, reflexology, and reactology.



Philosophy	General Theory	Applied sciences	Social Practice
Marxism: ▪ dialectical materialism & ▪ historical materialism	▪ Psychology ▪ Psychoanalysis ▪ Reflexology/ theory of higher nervous activity ▪ Reactology	▪ Paedology ▪ Defectology ▪ Pathopsychology and psychotherapy (clinical or medical psychology) ▪ Psychotechnics (industrial psychology and psychology of labour) ▪ Psychohygiene (mental hygiene) ▪ Pedagogy	▪ Medicine ▪ Education ▪ Industry ▪ The military

Table 1. Human sciences in the USSR in the 1920s

The pre-revolutionary contention between subjectivist (Chelpanov's "empirical psychology") and the objectivist (e.g., Pavlov's physiology of higher nervous activity, Bekhterev's reflexology) orientation in pre-revolutionary psychology was largely overcome by mid-1920s in favour of the former. Thus, in 1923-4, between the First and the Second Psychoneurological Conferences a group of objectivist psychologists headed by K.N. Kornilov, P.P. Blonskii and others managed to align with the call of the time to build a Marxist psychology. They displaced the leadership of Institute of Psychology in Moscow, the leading psychological research institution in the country, and the newly appointed senior officials of a number of other educational and academic institutions established in the 1920s clearly proclaimed their objectivist orientation in academic and applied research.

Dissatisfaction with the limitations of the extremes of both subjectivist and objectivist

camps inspired several attempts at compromise between the two, most notably Kornilov's "reactology" (supported in mid-1920s by the group of newly recruited employees of the Institute of Psychology including Artemov, Dobrynin, Gellershtein, Vygotsky, and Luria—although their fascination with the promise of reactology was fairly short-lived). Another powerful alternative to the subjectivist-objectivist dualism in psychology was psychoanalysis (Ermakov, O. & V. Schmidt, S. Spielrein, Luria, and others). Despite their different histories and particular theoretical and philosophical inspirations (if any), all major theorists of the 1920s were positioning themselves as Marxists. This led to a number of Marxism inspired theoretical proposals, for instance, reactology as "the Marxist psychology" (Kornilov, 1923, 1924, 1930), Marxist reflexology (Bekhterev, 1925), and Marxist psychoanalysis (Fridman, 1924; A. R. Luria, 1924, 1924/2002). However, most often the apparent fascination with Marxism was mainly a rhetorical device used to justify utterly different and even opposing scientific ideas. This tendency to use quasi-Marxist discourse in science prevailed throughout the entire decade of the 1920s and dramatically increased in the 1930s (Krementsov, 1997).

Along with these general and theoretical proposals a number of special and applied disciplines coexisted, mutually enriched each other, and competed for resources. These include *zoopsikhologiya* (animal, evolutionary and comparative psychology) (Wagner, Severtsov, Borovskii, Ladygina-Kohts, Vojtonis), *pedologiya* (paedology) (Zalkind, Molozhavyi, Blonskii, Vygotsky, Zaluzhnyi), *defektologiya* (defectology, i.e., the psychology of abnormal development and rehabilitation, special education, and corrective pedagogy) (Rau, Danyushevskii, Sokolyanskii, Vygotsky, Azbukin, Zankov, Solov'ev), *psikhotekhnika* and research on kinematics (i.e., psychotechnics, or industrial psychology) (I. Spielrein, Gellerstein, Gastev,

Bernstein), *psikhogigiena* (i.e., psychohygiene, or mental hygiene) (Zalkind, Rosenstein, Rokhlin), *psilkhoterapiya* (i.e., psychotherapy) (Bekhterev, Protopopov, Platonov). These middle-level theories and psychological subdisciplines were often inspired by the general theories, which led to the creation of a number of terminological hybrids like “reflexological defectology and paedology” of Sokolyanskii and Zaluzhnyi, “psychoanalytical pedagogy” of Vera Schmidt and psychoanalytical *Detskii Dom* (Children’s House) in Moscow, “psychoanalytic paedology” of Zalkind, or the “socio-cultural defectology” of Vygotsky and his associates. In contrast, despite its proclaimed goal of providing a universal approach to human beings, reactology actually produced a series of experimental studies that failed to expand outside psychological laboratories. Furthermore, a great deal of cross-disciplinary exchange between psychology and humanities took place, for instance, with linguistics, with literary theory (e.g., formalists of Moscow linguistic circle and Leningrad OPOYAZ, the scholars of Bakhtin circle, and N.Ya. Marr’s “semantic paleontology”), and with theories of theatre and the nascent cinematography (Stanislavsky, Eisenstein, Meierhold, and Kurbas). In some of the human and behavioural sciences (e.g., defectology, mental hygiene, or psychotherapy) the influence of medical sciences like psychoneurology and psychiatry was particularly strong, since most specialists in these fields were medical doctors or individuals with vast experience of clinical and health care work.

In sum, the period of 1920s can generally be characterized by the foundation of original scientific schools, the integration of Soviet science into international research efforts, and intensive novel, often cross-disciplinary, theoretical and applied research by Soviet scholars working in close conjunction with professionals in medical sciences, education, and industry.

However, the so-called Great Break announced by the Bolshevik Party at the end of the 1920s and related changes in the structure of the supreme power in the country, national policy, the economy and industry, all dramatically changed the entire landscape of the human and behavioural sciences in the country.

Perhaps one of the most notable precursors of the change to science policy at the end of 1920s was the elimination of the two contending groups of Marxist philosophers known as “mechanicists”, and, later, those associated with A.M. Deborin, founder and the first editor of the journal *Pod znamenem marksizma* (Under the banner of Marxism). The latter group was criticized in “public discussions” during the campaign against “menshevizing idealism” in philosophy. The elimination of Deborin’s group put an end to free philosophical discussion in the country. As a result, official Soviet Marxism mutated into an essentially positivist, dogmatic, and prescriptive discipline in the 1930s (Kogan, 2002; Ogurtsov, 1989; Yakhot, 1991). Quite illustrative of this transformation of Soviet Marxism is the fact that the basic idea of “fading away” of the state that formed the core of Marxist social philosophy was “corrected” by its opposite: the idea of the increase of class struggle in a country of developing socialism that later served as basis and legitimization of the Great Terror of 1936-1938. In education and science, the Great Retreat (Timasheff, 1946) and revision of basic Marxist principles and practices of the 1920s was reflected in the introduction of strict and rigid organizational forms, and restoration of the pre-revolutionary school system of education and of pre-revolutionary academic degrees and titles (January, 13, 1934 decree of the *Sovnarkom*, the Council of People's Commissars, the government of the early Soviet republic).

These large scale social processes later also inevitably involved science. Thus, the idea of a key and instrumental role for science in social transformation was not

removed, but was subordinated to yet another social idea, namely, of power and control of the Party and its social institutes. From the end of 1920s, new mechanisms of state control over the science were introduced, and the centralization of science significantly increased. The key instruments used to implement Stalinist Science were: state censorship; rituals of so-called “self-criticism” and politically motivated “public discussions”; the institutionalization of a new state-science bureaucracy; the creation of a new elitist ruling quasi-class of the Party *nomenklatura* (Voslensky, 1984); and dissemination and standardization of the rhetoric of Party “newspeak” in scientific discourse. These processes resulted in a major paradox for Soviet science of the 1930s: the declaration of the exceptional role of science in society (which implies relative independence of science from other social institutions) and total state control over science (including the total dependence of Stalinist Science on its Party patrons). The situation was even more aggravated by the growing international isolation of the Soviet Union from mid-1930s onwards, which dramatically contrasted with the international success and growing international recognition of the Soviet science.

Thus, the decade of 1930s changed the interrelations between the four levels of scientific research at which psychology operated as theoretical and applied science and social practice. Introduction of the principles of *partijnost*’ (literally: Party-ness) and *practicality* were instrumental in this transformation of the paradigm of psychological research in the Soviet Union. General theories of human psyche were generally overtaken by the dogmatic version of quasi-Marxist positivism that required the elimination of some theories and the transformation of others, according to the principle of *partijnost*’. Psychoanalysis was the first to fall. Its influence gradually decreased when its patron Lev Trotsky failed in his struggle for leadership of the Communist Party and virtually disappeared after his expulsion from the Party and the

government, followed by his exile from the country at the end of the 1920s. Another major blow to the multiplicity and diversity of general theories of human psyche was a series of “public discussions” about Kornilov’s reactology and Bekhterev’s reflexology in early 1930s, after which these disciplines were in effect banned. Finally, unlike reactology and reflexology, Gestalt theory—another psychological theory with ambitions of a being general psychological theory that was somewhat popular in Russia in 1920s—was never able to gain much support from the Russian scholars that would compare with its influence on German or, to smaller extent, American psychology ([Scheerer, 1980](#)). Anan’ev, a zealous reflexologist in 1930 and a loyal psychologist just a year later, in 1931, left us perhaps one of the best characterization of the process of elimination of general theories in human sciences in early 1930s writing that “in psychology, no schools must be allowed other than the only one, the one based on the works of the classics of Marxism” (Anan'ev, 1931).

In conjunction with the principle of the *partyiness*, the idea of practical transformation of the world seems to have gained dominance in the 1930s in the form of a campaign for the *practicality* of science, through critique of the “the separation of theory from practice”. But, as Krementsov points out,

here, “practice” referred not to experimental work in a particular field, but rather to the ‘practice’ of the construction of socialism. According to the critics, science should only be engaged in solving problems “presented by practice”. Scientists were criticized for “fruitless theorizing”, “separation from reality”, “confining themselves in ivory towers”, and similar sins (Krementsov, 1997, p. 47).

The status of scientific theory from then on was increasingly regarded as auxiliary, leading to the deemphasizing of both general and middle-level applied

theories. Instead, amalgamations of quasi-philosophy/general theory and social practice/applied disciplines were introduced. Quasi-philosophy and social practice respectively played leading role in each of these two hybrids.

<b>1920s: four levels</b>	<b>End of 1930s: two hybrids</b>
Level 1. Marxist philosophy Level 2. General theory Level 3. Mid-level applied theories Level 4. Social practice	Level 1. Marxist philosophy and general theory Level 2. Social practice and applied disciplines

Table 2. Transition from four-layer to two-layer structure of scientific knowledge and social practice in 1920-30s

The turning point in the history of the applied behavioural sciences was the notorious decree of the Central Committee of the Communist Party on the “paedological perversions in the system of Narkomproses” of July 4, 1936. This was the first direct and straightforward interference of the Party in scientific issues on grand scale as deciding about the existence of a particular scientific discipline. This decree banning the use of testing in human sciences effectively prohibited such applied research disciplines as paedology (*de jure*) and psychotechnics (*de facto*), both of which extensively used tests in their practices. Representatives of these disciplines migrated to relatively more secure niches in psychology and pedagogy. The disciplines of psychotherapy and psychohygiene, little in demand in the Soviet Union of the 1930s, seem to have lost their official support and gradually disappeared. By extension, psychology substantially lost its pre-decree status of being one of the leading disciplines of the human sciences and was on the verge on extinction.

However, one should not underestimate the factor of strong personalities. After the loss of their respective leaders and the spokesmen of the disciplines Lev Rozenshtein (died in 1934), Aron Zalkind (died in 1936), Lev Rokhlin (arrested in 1937), Isaak Spilrein (arrested in 1935, executed in 1937) and Aleksei Gastev (arrested in 1937, executed in 1938) neither psychohigiene nor psychotechnics produced a spokesman proficient in the Party newspeak of the time, skilful in negotiations with the patrons of science, and capable of persuading the key decision-makers to support and sustain research in the field.

The result of these processes and their effect on psychology is presented by an important eyewitness to these events Raymond Bauer, in his book *The new man in Soviet psychology* published in 1952, just a year before Stalin's death. Bauer reported that

From the early days of the Bolshevik regime to the present [i.e. by early 1950s] the discipline of psychology in the USSR has undergone a number of fundamental changes—one might even refer to them as revolutions—which cannot be understood except as a function of social and political conditions in the Soviet Union. Psychology has changed from empirical, relatively independent science, which strove to arrive by investigation at general laws that could be applied to social living, to a relatively unempirical, applied science which is enjoined to make theory follow practice. In range of activity, Soviet psychology has narrowed from an extremely broad discipline which studied animal and human, normal and abnormal, child and adult subjects, to one which focuses most of its attention on the study of normal, human children. In the earlier period the range of application of psychology extended to the full breadth of industrial psychology, medical and abnormal psychology,



criminology and delinquency, the study of social attitudes, child and educational psychology, and testing and guidance in the schools. In addition, a wide range of theoretical work was done and a variety of theoretical positions tolerated. Today, virtually all of applied psychology is confined to developing techniques of education and training the New Soviet Man. A limited amount of work is done in such areas of application as medical psychology, vast fields of application are forbidden, and there is, for practical purposes, only one theoretical position allowed (Bauer, 1952, pp. 4-5).

Under these circumstances, by the end of 1930s psychology lost its ambitions of generating a general theory and split between an unempirical hybrid quasi-philosophical discipline studying the human psyche and consciousness, and empirical psychological research that remained auxiliary to such applied fields as pedagogy, defectology, medicine and clinical practice of rehabilitation. This ambiguous state the Soviet psychology was reflected in its organizational structure and the institutional affiliation of psychologists from 1940s onwards. Thus psychologists were affiliated with either the Academy of Pedagogical or of Medical Sciences (founded in 1943 and 1944 respectively). However, the first academic department (*otdelenie*) of psychology in the Soviet Union that started preparing professional psychologists (formed in 1943) was administratively a part of the Faculty (*fakul'tet*) of Philosophy of the Moscow State University.

Philosophy & General Theory	Applied sciences & social practices
<p>Marxism:</p> <ul style="list-style-type: none"> <li>▪ dialectical materialism</li> <li>▪ historical materialism</li> <li>▪ general psychology</li> </ul>	<ul style="list-style-type: none"> <li>▪ Defectology, special education &amp; corrective pedagogy</li> <li>▪ Pathopsychology &amp; clinical practice</li> <li>▪ Psychohygiene &amp; sanitary hygiene</li> <li>▪ Pedagogy</li> <li>▪ Psychotechnics &amp; personnel training, specifically, military personnel</li> <li>▪ Child psychology &amp; Psychology of perception</li> </ul>

Table 3. Psychoneurological sciences in the USSR in the 1930s

The entire Vygotskian psychology as a theory and a social practice and its course after Vygotsky's death can hardly be understood outside of this extremely complex socio-historical context of Soviet science of the 1920 to the early 1940s. Numerous and intricate mutual influences and interconnections between several Vygotskian scholars only add to the complexity of the task of writing a history of Vygotskian psychology. Thus, at some point Vygotsky, who started his scientific career with literary theory and brilliant analysis of Shakespeare's *Hamlet* and other studies that resulted in his doctoral thesis *Psychology of Art* (1925), reveals close affiliation with some ideas of Pavlov and Bekhterev and refers to himself as a "bigger reflexologist than Pavlov himself", while later he becomes one of the leading researchers of Kornilov's reactological Institute of Psychology. Alexander Luria, a major figure in Vygotskian psychology, founded the Kazan Psychoanalytical Society and even received a welcome letter from none other than Sigmund Freud. In Moscow, Luria was the secretary of Russian Psychoanalytical Society and—along with

Vygotsky—remained a member until 1930. They even wrote an introduction to a volume of Freud's works that came out in 1925. Still in Kazan, Luria was employed at the local *Institute of Scientific Organization of Labour*, was one of the founders of the journal *Problems in the psychophysiology of labour* later renamed *Problems in the psychophysiology of labour and reflexology* after Bekhterev agreed to Luria's offer to join the editorial board of the journal. In Moscow, both Vygotsky and Luria worked closely with leaders in psychotechnics of the time like Spielrein, Gellerstein and Bernstein. They also collaborated with several prominent specialists in psychohygiene (mental hygiene) like Lebedinsky and Rokhlin. Several of Vygotsky's works on concept formation in schizophrenia remain classic studies in psychiatry (Hanfmann & Kasanin, 1942; Kasanin, 1944), whereas Soviet defectologists referred to Vygotsky as the initiator of the scientific study of the child's pathology of development in the country (Bein, Vlasova, Levina, Morozova, & Shif, 1983; Zankov, 1971). Finally, Vygotsky was considered one of the leading paedologists of his time, which was the reason his works were banned for more than twenty years after his death. These are just a few examples of interconnectedness of Vygotsky Circle with the entire network of the Soviet human and behavioural sciences in the 1920-30s. However, the ties within the main actors of the Circle were considerably stronger, more principled, and more durable than occasional alliances with outsiders. The interconnections between the main protagonists and the Vygotsky Circle and its transformation into several Circles of Vygotskians after Vygotsky's death in 1934 are discussed in depth in the next chapter.

## Chapter 4. Protagonists: Making science as social relations

This chapter describes the interpersonal and professional interrelationships of Vygotsky and his group and the dynamics of group change. The first Circle formed after Vygotsky moved to Moscow in 1924, therefore, my analysis needs to go beyond the 1930s. The development of the Vygotsky Circle is traced throughout the period of 1924-1941 and is divided into several phases.

4.1. **Phase One (1924-27)**, *prehistory of Vygotsky Circle: multiple interconnections, Vygotsky in search of a new research program and a team*

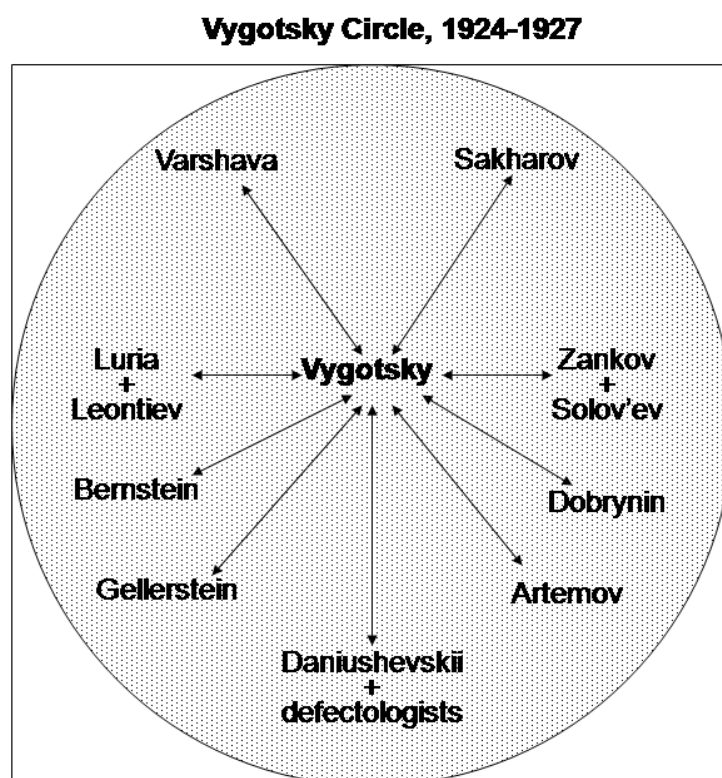


Figure 1. Vygotsky Circle in 1924-1927

The first phase of Vygotsky's Circle starts with Vygotsky's move from Gomel' to Moscow, and is characterized by his initial activities at the Institute of

Psychology and in various defectological institutions. During this period he made his only foreign trip in the summer of 1925 to a conference in London on the problems of the deaf. After his return from that European trip, in the fall of 1925, Vygotsky was hospitalized with tuberculosis and spent several months of 1925-6 in the hospital. At the end of 1925, on the basis of his unpublished dissertation *The psychology of art* that Vygotsky most likely wrote before 1924—that is still while in Gomel—he was awarded a doctoral degree without defence, in absentia. During this period Vygotsky also wrote his seminal methodological work *Historical meaning of the crisis in psychology* (1926).

Vygotsky's major academic contacts and collaborators during this time were his first graduate students (Zankov, Solov'ev, Sakharov, Varshava) and collaborators at Moscow Institute of Psychology such as Artemov, Dobrynin, Bernstein, Gellersein, and Luria. Two books were written in collaboration with the latter group (Vygotsky, Artemov, Bernstein, Dobrynin, & Luria, 1927; Vygotsky, Artemov, Dobrynin, & Luria, 1927).

Another major area of Vygotsky's activity was defectological work that was done chiefly in collaboration with I. I. Danilushkevskii, his friend from Gomel'. Luria and Leontiev were continuing investigation of affect by using Luria's combined motor method, a research project initiated by Luria in which Vygotsky did not seem to have been interested or to have participated—at least not during his first years in Moscow. Vygotsky's main research interest in 1922-27 was the investigation of dominant reactions in collaboration with his students Zankov, Solov'ev, Sakharov, and Gagaeva (Van der Veer & Valsiner, 1991, pp. 33, 128), and defectological work with Danilushkevskii and others. Also, during this period Vygotsky initiated his classic research on concept formation in collaboration with his student Leonid Solomonovich

Sakharov (1900-1928). Sakharov's research was most likely completed by the summer of 1927: according to van der Veer and Valsiner, on May 10, 1927 Sakharov died under unknown circumstances (Van der Veer & Valsiner, 1994, p. 96). Another of Vygotsky's collaborators during his first years in Moscow was Boris Efimovich Varshava (1900-1927), whose premature and tragic death in July, 1927 Vygotsky mourned in the introduction to the only their collaborative publication, a *Psychological dictionary* that came out in 1931:

Varshava was just at the beginning of his path as a psychologist-researcher and a writer. He made only first steps on this road that was interrupted by his death. But these first steps left no doubt to anybody who knew him as to *what kind of way* was waiting for him. This way was one of creative scientific work and a struggle for the complete reconstruction of psychological science on a new methodological foundation, and he chose this way was very early and decisively...

We tend to measure the extent of the loss of those who worked along with us by the extent of what they achieved, accomplished, completed. And this is correct. But the opposite is correct, too: to measure by what remains unrealized. This measure will be great if we apply it to the death of Boris Efimovich Varshava. What he could have achieved is incomparably more than what he has achieved. The measure of what he was destined to carry out and what is left unrealized is greater than the measure of his achievements. And this is the true meaning of the tragedy of his death (Vygotsky, 1931).

The projects undertaken by many of Vygotsky's colleagues and collaborators in 1924-1927 do not leave the impression of being interrelated pieces of a unified research program. However, by the end of 1920s these disparate lines of research

converged and became increasingly intertwined, leading to emergence of new research alliances and new teams of collaborators.

#### 4.2. Phase Two (1927-1931): *Luria-Vygotsky Circle formation*

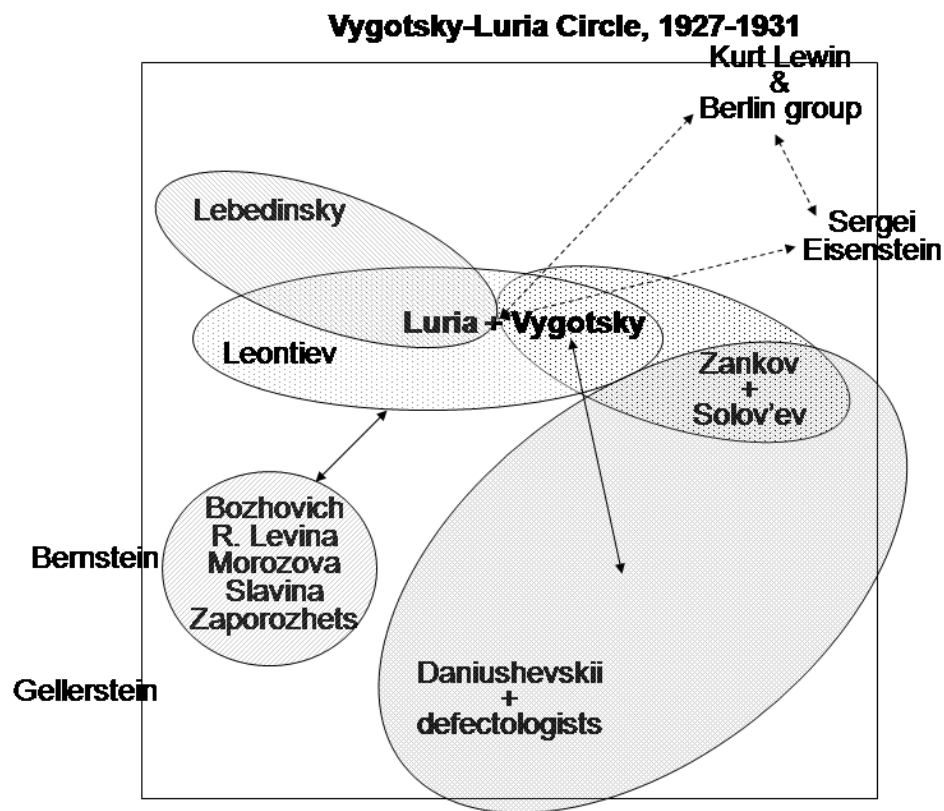


Figure 2. Vygotsky-Luria Circle in 1927-1931

According to the traditional historiographical narrative, Vygotsky's psychology was first developed by the members of Vygotsky's "trojka". In contrast, I argue that we have no compelling evidence of the *trojka*, the trio of Vygotsky-Leontiev-Luria during the last decade of Vygotsky's lifetime (1924-1934), and it is more appropriate to single out the duo of Luria and Vygotsky and their numerous personal and professional connections and interrelations that, collectively, may be referred to as **Vygotsky-Luria Circle** of the end of 1920s and first half of the 1930s.

There are a number of reasons for selecting the duo of Luria-Vygotsky as the intellectual and organizational core of a team developing cultural-historical theory in 1920-30s, and if only the number of publications alone is indicative, then from the sheer number of collaborative publications it is clear that Vygotsky and Luria were closely connected indeed. For instance, in mid-1920s they wrote two introductions to the Russian translations of psychological books (Vygotsky & Luria, 1925a, 1925b); then, participated in preparing educational materials and handbooks (Vygotsky, Artemov, Bernstein, Dobrynin, & Luria, 1927; Vygotsky, Artemov, Dobrynin, & Luria, 1927); co-authored a paper on egocentric speech that Luria presented at the IX International Congress of Psychology held at Yale University, which Vygotsky was unable to attend (Vygotsky & Luria, 1929/1930), and a chapter *Tool and symbol in child development* that was to appear in Murchinson's *Handbook of Child Psychology* in 1930 (Vygotsky & Luria, 1930/1994). Finally, their collaborative book was written in the end of 1920s and published in 1930 (Vygotsky & Luria, 1930, 1930/1993). In contrast, the only paper that came out under the names of both Vygotsky and Leontiev was the introduction to the book on psychology of memory written by the latter under the supervision of the former (A. N. Leontiev, 1931). It is also interesting how the contemporaries—mostly the outsiders and critics—perceived and attributed the Vygotsky's group and theory. Thus, for instance in the critical account by Talankin at his speech given in June of 1931 in Kharkov attacks the group of Vygotsky and Luria and calls for Marxist critique of their cultural-historical theory (Talankin, 1931, 1931/2000). Later, in 1934 Razmyslov severely criticized Vygotsky's and Luria's cultural-historical theory of psychology (Razmyslov, 1934, 1934/2000). Finally, in 1936 an author that identified (or rather *did not* identify) himself with the just two letters G.F. also mentions the school of Vygotsky and Luria in the context of its



critical discussion in the light of the Party decree of July 1936 (F.G., 1936). Judging from their personal correspondence and the facts of their biographies the connections between Vygotsky and Luria remained very close until the very last days of Vygotsky's life. For instance, in the fall of 1931 Vygotsky and Luria resumed their studies in medicine: they both got accepted as the students at the Medical Department in the Kharkov, and were spending much time together preparing for their exams (E. A. Luria, 1994). At the same time they were involved in intensive research in clinical settings conducted in parallel in Kharkov and Moscow (Vygotsky & Puzyrei, 2004).

The Vygotsky and Luria circle includes all their co-workers and like-minded individuals of that time, and their acquaintances; the exchange of ideas in a circle normally takes place through more or less informal communications and personal discussions (e.g., Vygotsky's "internal conferences" and his correspondence). The **periphery of the Circle**, however, is formed by the individuals who can not fully qualify as students or collaborators of either Vygotsky or Luria, but whose work and ideas must have been instrumental in the development of their theory of development of higher mental functions and whose influence on Vygotskian psychology—despite the lack of any formal records such as scholarly references to published works or coauthored papers—must have been pervasive and profound. In-depth analysis of the interrelations between Vygotsky-Luria group and these peripheral members of the circle remain beyond the scope of this research on the Vygotsky Circle, yet a cursory overview is needed in order to demonstrate the complexity and the multitude of the professional connections of this scientific alliance if only to delimit the "inner circle" of Vygotsky and Luria<sup>3</sup>.

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<sup>3</sup> I focus only on the two individuals: movie director and film theorist Sergei Eisenstein and Gestalt psychologist Kurt Lewin. However, a number of other individuals are of great importance for the fullest reconstruction of Vygotsky's "peripheral circle" of his "invisible college". These are psychotechnic and physiologist Nikolaj Bernstein, poet Osip Mandelstam, Georgian psychologist

**Sergei Eisenstein** (1898-1948), a revolutionary Soviet film director and film theorist, first met Luria around 1925-beginning of 1926 (Bulgakowa, 1989, pp. 180-181) soon after theatrical release of Eisenstein's *Bronenosets Potemkin* (The battleship Potemkin) in order to discuss psychological problems of "theory and psychology of expressiveness" that were of great interest to him at that time. Luria most likely introduced Vygotsky and Eisenstein to each other, and the three eventually became close friends (E. A. Luria, 1994, p. 121). In the late 1920s, Eisenstein—along with Vygotsky and Luria—participated in psychological studies on human movement under hypnosis (in December, 1928) (E. A. Luria, 1994, p. 122) and the investigation of the phenomenal memory of the famous mnemonist Shereshevskii:

He [Shereshevskii] presents a unique example of a man who—along with absolutely full and perfect development of a normal human being—preserved as well the entire set of early mechanisms of perception. Over the years, professors Vygotsky and Luria were doing a wide range of psychological observations and experiments with him. In turn, I managed to have a series of most interesting discussions with him about the problem that interested me: synaesthesia that was represented in him in utmost completeness (in 1928 and in 1933) (Eisenstein, 1937/2000, p. 385).

In the 1950s Shereshevskii recalled the voices of Vygotsky and Eisenstein and described Vygotsky's "crumbly, yellow voice" and Eisenstein's voice as "entire composition, a bouquet ... listening to him, it was as though a flame with fibers protruding from it was advancing right toward me" (record of November, 1951) (A. R. Luria, 1987, p. 24). The discovery of the only surviving manuscript of Vygotsky's

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Dimitry Uznadze (Usnadze), philosopher and psychologist Sergei Rubinstein, philosopher and literary theorist Mikhail Bakhtin and the scholars of the so-called "Bakhtin Circle", and psychotechnic Solomon Gellerstein, to mention but a few.

doctoral dissertation *Psikhologiya iskusstva* (Psychology of art) in Eisenstein's personal archives attests to the fact that the contacts between these three individuals in 1920s, before Eisenstein's trip to Europe and North America (August, 1929-May, 1932), were remarkably intense. In the early 1930s, after Eisenstein's return to the Soviet Union, another research project was conceived: film director and theorist Sergei Eisenstein, philologist academician Nikolai Marr, and two psychologists Lev Vygotsky and Alexander Luria planned to systematically analyze the "problems of the nascent language of cinematography", but the untimely death of Vygotsky (in June, 1934) and Marr (in December, 1934) put an end to this ongoing research (Eisenstein, 2002b, p. 136). However, even after Vygotsky's death and Luria's short tenure in Kharkov's Ukrainian Psychoneurological Academy, personal and professional contacts between Eisenstein and Luria continued. The evidence for that can be found in Eisenstein's diaries, for instance, the entry of December 29, 1938 about the schizophrenic case of "Tyoma" that Luria demonstrated to Eisenstein on December 26, 1938 (Eisenstein, 2004, p. 672) or his reference to their experiments with Luria in his diary entry of July 24, 1940 (Eisenstein, 2002a, p. 431).

**Kurt Lewin** (1890-1947), a German psychologist of the Berlin school of Gestalt psychology, is another important node in the very complex network of interconnected scholars and thinkers. The first contacts between the Moscow and Berlin psychologists were established in 1925 when both travelled to Europe, Luria in July-September, 1925 (E. A. Luria, 1994, pp. 43-44) and Vygotsky, in the summer of 1925 (Van der Veer & Valsiner, 1991; Zavershneva, 2008a). During his next foreign trip, to the USA (via Germany and the Netherlands) around August, 1929, Luria met his Berlin colleagues again and even took part in Tamara Dembo's psychological study on the dynamics of anger (Dembo, 1931)—as an experimental subject himself

(E. A. Luria, 1994, pp. 44-45; Yaroshevskii & Zeigarnik, 1988). Another opportunity for Luria to converse with his foreign colleagues, including Kurt Lewin and other representatives of the Berlin school opened up very soon, at the IX International Congress of Psychology held in Yale September 1-7, 1929, when on September 4 (Cattell, 1930, p. xxii) Kurt Lewin's presentation on *Die Auswirkung von Umweltkräften* [The impact of environmental forces] and Vygotsky and Luria's paper *The function and fate of egocentric speech* were assigned to the same session on Child Development. Luria was mostly likely instrumental in organizing the meeting of Eisenstein and Lewin in Berlin in 1929-1930. In fact, Oksana Bulgakowa published two letters from Luria written during his 1929 trip to the IX International Congress of Psychology in New Haven, addressed to Lewin (dated September 20) and to Eisenstein (dated October, 24—sic!) that contained mutual recommendations to each other (Bulgakowa, 1989, p. 190). It seems though that the letter to Eisenstein is dated incorrectly, and that it was actually written on *September*, 24. For one thing, in this very letter Luria describes his plans to stay in Berlin through October, 25-November, 5 (that is, on his way back to the Soviet Union from the Congress in North America), Also, a series of Luria's published essays that he wrote during his transatlantic trip are dated October 11-13, 1929 guarantees that Luria must have arrived in Europe in mid-October, 1929 (E. A. Luria, 1994, pp. 50-55). The meeting between Luria and Eisenstein reportedly took place and correspondence between the three ensued (Bulgakowa, 1989, p. 177). The meeting was important for both Lewin and Eisenstein and resulted in mutual influences between: thus, Lewin's field theory and topology found its place in Eisenstein's his theory of cinematography, whereas Eisenstein inspired Lewin as a film-maker and might even have participated in the production of Lewin's demonstrational films (Van Elteren, 1992). In her memoirs, Luria's daughter

argued that “not only was Sergei Mikhailovich [Eisenstein] well familiar with Luria’s experiments, but also involved Aleksander Romanovich [Luria] himself in film production. From the correspondence between Luria and Lewin we know that Luria was making films about children’s counting, attention and memory as involved in counting operations. Aleksander Romanovich put an effort into the organization of International association of scientific cinematography and founded a psychological laboratory in the Institute of cinematography, where his assistants were supervising graduate theses at the Department of Directing” (E. A. Luria, 1994, p. 124). Quite characteristic of the interrelationships between Lewin’s and Vygotsky’s research programs, when two of Lewin’s “Russian” students, Gita Birenbaum and Blyuma Zeigarnik, returned to Moscow in the early 1930s, according to Zeigarnik, they started working with Vygotsky “literally the next day” (Yaroshevskii & Zeigarnik, 1988).

When Lewin’s short-term teaching contract in North America was finished, he decided to return to Germany through Japan and Soviet Union, countries where his former students were living at that time. On leaving Japan, he learned from Japanese newspapers about Hitler’s accession to power at the end of January, 1933, but could not grasp the full meaning of this event until he reached Moscow, where he met his German-speaking friends and former students. Lewin remained in Soviet Union for several months and finally reached Berlin in May, 1933 in order to pick up his family and leave Germany for the United States in August, 1933, never to return (Marrow, 1969, pp. 66-68). In Moscow, Lewin again met his friends including Luria, Eisenstein, Vygotsky, Zeigarnik, and Birenbaum, and had numerous personal as well as professional encounters and discussions. On a number of occasions Lewin met with Vygotsky in his Moscow apartment and had heated and most passionate discussions in German (Vygodskaya & Lifanova, 1996a, p. 299). Recent studies based on the

unpublished materials from Vygotsky's personal archives reveal a close affinity between Vygotsky and Lewin, who was the major source of inspiration and the principal opponent for Vygotsky so that "discussion with Lewin became one of the main engines for Vygotsky's thought development during the last years of his life" (Zavershneva, 2007b).

From the end of the 1920s for a number of organizational, social, political, professional, and—last but not the least—personal reasons, research in the "**Inner Circle**" of Vygotsky and Luria was conducted by several relatively independent groups. The independence and the isolation of these groups varied. Typically, in the traditional, "Leontievian", version of the history of psychology the separation Vygotsky's former graduate students Zankov and Solov'ev from Vygotsky was one of the elements of the history of the construct of the "trojka" as the alliance of the most devoted Vygotsky's students with their teacher. Vygotsky's letter to Leontiev of July 2, 1929 was considered proof of Vygotsky's critique of Zankov and Solov'ev and the evidence of their departure from Vygotsky's research program. Thus, Vygotsky wrote:

Zankov and Solov'ev are the most difficult part. There has been no response. They are coming again tomorrow. First they want everything to be at a single institution, and that it be a clinic if Zankov is to go there, and this greatly ties my research interests to practical issues; then they want to split the work among institutions. In a word, things are not going well. That is just between us. Nevertheless, a decision has to be made on this one way or the other (Vygotsky & Puzyrei, 2007, p. 25).

Based on this, one might be persuaded to believe that Zankov and Solov'ev were "ambiguous about Vygotsky's research program" and that they were "hesitant

and doubtful” about it (A. N. Leontiev & Leontiev, 1976/2005, p. 372). Interestingly enough, at the same time Leontiev unwittingly spoke about the “struggle for the place near Vygotsky” (A. N. Leontiev & Leontiev, 1976/2005, p. 373). This struggle is perhaps best illustrated by an episode reported by A.N. Leontiev, when, after Vygotsky’s death, the former “Kharkovite” students of Vygotsky were not allowed to take part in the mourning ceremonies that were organized in June, 1934 by Zankov, Solov’ev, and their associates at the Experimental Institute of Defectology in Moscow (A. N. Leontiev & Leontiev, 1976/2005, p. 378; Vygotskaya & Lifanova, 1996a, p. 328). Quite a different version of the history of interrelations between Vygotsky and his former “Kharkovite” colleagues is presented by Vygotsky’s daughter Gita Vygotskaya and her collaborator Tamara Lifanova, who argue that it was Leontiev who betrayed Vygotsky, accusing him of an unethical separation from his former teacher some time around 1933 (Vygotskaya & Lifanova, 1996a). In turn, the son and the grandson of A.N. Leontiev in a number of publications argue that the idea of Leontiev’s separation and betrayal of Vygotsky is inconceivable and totally false (A. A. Leontiev & Leontiev, 2003; A. A. Leontiev, Leontiev, & Sokolova, 2005). I will try to verify these two opposing versions of this controversy on the basis of the available historical evidence in the next section dealing with the period of 1931-34.

Also during the late 1920s Luria and Vygotsky initiate research in clinical settings, typically in collaboration with medical specialists (neurologists, psychiatrists, etc.), such as M. S. Lebedinsky. This research was typically conducted under the auspices of the G.I. Rossolimo Clinic of nervous diseases of the 1<sup>st</sup> Moscow University (Lebedinskii, 1931; Lebedinsky & Luria, 1929; A. R. Luria, 1932a). The alliance of Luria with Lebedinsky seems to have been instrumental in sustained clinical research in Vygotsky’s Circle from the end of 1920s.

Another group of Vygotskians during the period is formed by the five students of the Second Moscow University later known the “pyaterka”. This group was involved in experiments supervised by Vygotsky, Luria, and Leontiev; they all studied at this University at the end of 1920s, and graduated in 1930. After their graduation the members of the “pyaterka” were appointed to mandatory jobs in different locations (the so-called *raspredelenie*), mainly outside of Moscow, and ceased to exist as a unity or a research team.

#### 4.3. Phase Three (1931-1934): Vygotsky-Luria Circle and the beginning of specialization and separation

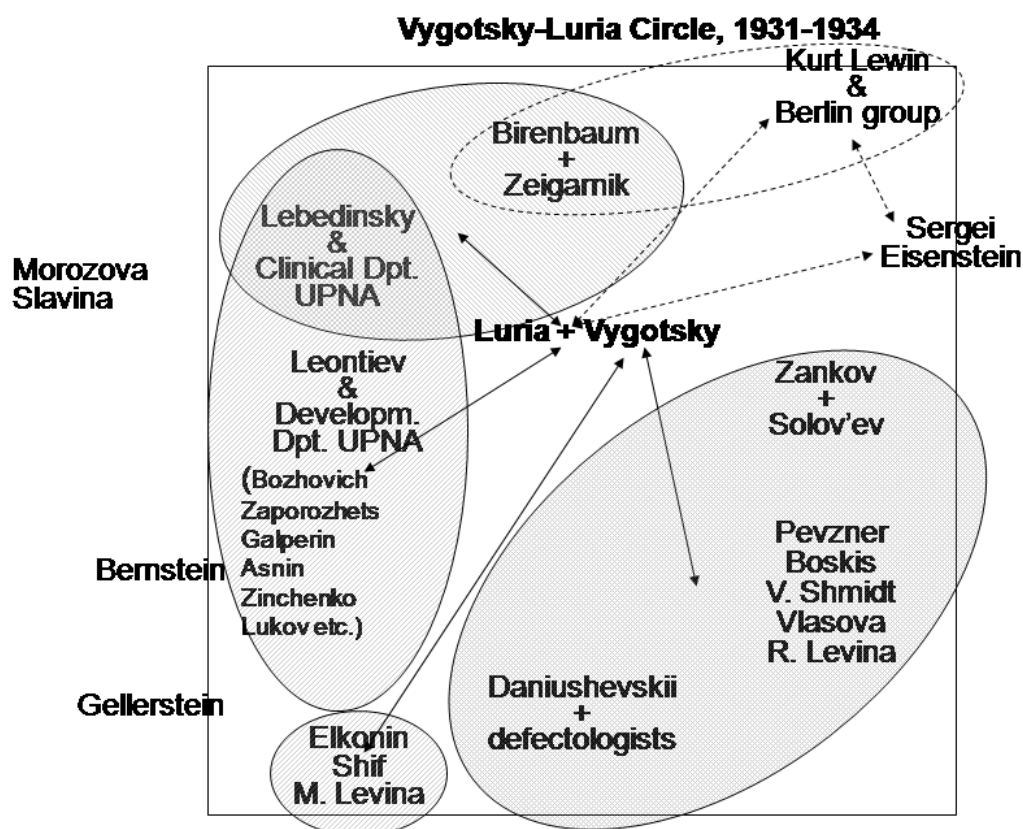


Figure 3. Vygotsky-Luria Circle in 1931-1934

During the period of 1931-34 several groups of Vygotskians were working in parallel in a number of institutions in three cities of the Soviet Union: Moscow,



Kharkov, and Leningrad. However, the internal unity of this geographically and thematically extended research project was guaranteed by Vygotsky and Luria's role as mediators between these groups and as coordinators of a whole range of studies of normal development, pathology and clinical research.

The version of the separation of Zankov and Solov'ev from the Vygotsky group in 1929 presented in the "Leontievian" historical account is essentially undermined by discoveries in the Vygotsky's archives that have recent been published in Russian. According to Vygotsky's notes, in 1932-3 the alleged "separatists" kept taking part in Vygotsky's "internal conferences" (research meetings that he was organizing for his closest students and collaborators). Furthermore, at least one study that was done at that time by Solov'ev, under direct supervision of Vygotsky; Solov'ev replicated research on "instantiation" (*Sättigung*) originally conducted by Kurt Lewin's student Anitra Karsten (Karsten, 1928) and even published a paper with his report on the findings (Solov'ev, 1935). This study was presented at one of the research meetings of the larger Vygotsky Circle in October, 1932. Leontiev also presented a research report and participated in discussion, but we still do not know all the names of the rest of Vygotsky's colleagues who attended the meeting (Vygotsky, Vygodskaya, & Zavershneva, 1932/2007; Zavershneva, 2007b). Furthermore, both Zankov and Solov'ev appear on the list of the individuals that Vygotsky was planning to invite to the department of Psychology that he was organizing in the All-Union Institute of Experimental Medicine (VIEM), in early 1934, only months before his death—a list that includes the names of Solov'ev, Zankov, Veresotskaya, R. Levina, Slavina, Shif, but neither Leontiev or Luria (Vygodskaya & Lifanova, 1996a, p. 129). Furthermore, the obituary that came out immediately after Vygotsky's death was signed by a group of Vygotskians and by a number of highest officials from VIEM

(Vygodskaya & Lifanova, 1996a, p. 141): Again, the name of Leontiev can not be found here —unlike those of Zankov, Solov'ev, Luria, Daniushevskii, V. Schmidt, Geshelina, Vlasova, Zeigarnik, and Birenbaum. Finally, after Vygotsky's death, his *Thinking and speech* was prepared for posthumous publication by his students Zankov and Shif (Kolbanovskii, 1934, p. v), and , another posthumous volume was published in 1935 by the two Vygotsky's students, Zankov and Shif, in collaboration with El'konin (Vygotsky, 1935c). The latter book presents quite remarkable collection of Vygotsky's works of the 1930s on the problems of the interrelation between learning and development that featured perhaps the most well-known of Vygotsky's theoretical innovations, the “zone of proximal development”. Finally, another 1935 book written by Zankov was dedicated by him to the memory of his teacher, and heavily relied on Vygotsky's ideas on children's defective and pathological development (Zankov, 1935)—yet another argument in support of conflict between Zankov and Leontiev rather than of a separation of Zankov and Solov'ev from Vygotsky and mainstream Vygotskian research.

The roots of this conflict may be traced to an episode overlooked by historians of psychology and still waiting further investigation. In 1928 Leontiev's article on remembering in children with pathological mental development came out in the journal *Voprosy defektologii*, only the third work of this author ever accepted for publication at that time (A. N. Leontiev, 1928). Two years later Zankov published a paper with the results of his study on the same topic in a volume that he coedited with D.I. Azbukin and Vygotsky (Zankov, 1930). In this paper Zankov presented the findings of his study and a scorching critique of Leontiev's 1928 study, lambasting his negligence of experimental procedures precision, carelessness with statistics, vague style of reporting findings, and, last but clearly not least, Leontiev's totally distorted

account of mnemonic processes in mentally retarded children. Leontiev never responded to this critique. Furthermore, no references to this Zankov's study can be found in the publications of Leontiev's students, for instance, in the monumental and most pedantic book by P.I. Zinchenko on psychology of involuntary remembering that overviewed virtually all literature on the topic to date, including several other of Zankov's 1930-40s publications on memory (P. I. Zinchenko, 1961). In sum, regardless of whether the episode with Zankov's scientific critique of Leontiev's early study was the cause or merely an indicator of the ongoing conflict, there are reasons to believe that the opposition between the "Kharkovite" (Leontiev, Luria, etc.) and the "defectological" (Zankov, Solov'ev, etc.) groups of Vygotsky's followers was based on a personal conflict and strong hostility of the key protagonists rather than on purely scientific, theoretical grounds.

Until 1931, Vygotsky's closest and direct collaborators were working in a number of educational and research institutions in Moscow, but the end of the year was the turning point in the history of the group. That is when many of Vygotsky's former colleagues left Moscow for **Kharkov** (the capital of Soviet Ukraine) to head the research at the Psychology Sector of the newly formed *Ukrainian Psychoneurological Academy* (UPNA). This group of 'Kharkovites' is commonly referred to as the "Kharkov school of psychology" and is most well known for its work on the foundations of the so-called Leontiev's activity theory.

Vygotsky also might have planned to move to Kharkov at some point and—along with Luria— even started his studies at the extramural department at the Medical Institute, but his move to Kharkov never took place, and he was invited—and instead, in fall of 1931, he accepted the invitation to lecture part-time in **Leningrad**.. Following the invitation to teach in Leningrad that Vygotsky received from Sergei

Rubinshtein, the head of the department of psychology at the local Herzen State Pedagogical Institute, a third center where Vygotskian studies were conducted in the 1930s was formed. From 1931 until his death Vygotsky frequently commuted to Leningrad to lecture and supervise the research of the Leningrad group of his students that included Daniil El'konin, Zhozefina Shif, F.I. Fradkina, M.A. Levina, and several others. This research considerably decreased after Vygotsky's death, and the subsequent departure of several his Leningrad students to Moscow (Shif, M. Levina), and resumed only some time after 1937.

In **Moscow**, Vygotskian studies continued in the field of defectology under the auspices of the Moscow *Experimental Defectological Institute* (EDI), where a major group of Vygotsky's students was working from the institute's inception, others, like Shif from Leningrad, later joined the group, which included such long-time Vygotsky collaborators as Daniushevskij, Vlasova, Zankov and Solov'ev and their students, as well as the newly admitted defectologists Pevzner, Boskis, R. Levina, Morozova, Bejn, Veresotskaya, Eidinova, Geshelina, and M. Levina, and others who started their work at EDI in this period. These new collaborators were quite diverse and included quite a few fugitives from other academic and applied disciplines that were by then ostracized or formally banned such as former psychoanalysts (V. Schmidt, Geshelina), or Vygotsky's Leningrad students M. Levina and Shif who were severely criticised in Leningrad for pursuing cultural psychology.

Another line of Vygotskian research in Moscow in the early 1930s was conducted by Kurt Lewin's former Berlin students Blyuma Zeigarnik and Gita Birenbaum who returned to the Soviet Union in 1930-1 and started collaborating with Vygotsky on psychological research in clinical settings. In 1931-32 Birenbaum and Zeigarnik worked at the Clinical Department of the *Institute for the Research of the*

*Higher Nervous Activity* (IVND), and later, moved to the VIEM where from 1933 until Vygotsky's last days they worked under his supervision and collaborated with a group of researchers at the Psychiatric Clinic, mostly medical doctors—psychiatrists and neurologists—that included N.V. Samukhin, E.S Kaganovskaya, and A.M. Dubinin. The work of the Moscow group of clinical psychologists was paralleled by the research in Kharkov, at the department of clinical psychology of the *Ukrainian Psychoneurological Academy* (Luria, Lebedinsky, Gal'perin, Zaporozhets, Kozis, Margolis, and Voloshin).

During the last months of his life Vygotsky was hectically working to organize a psychological department—hypothetically, a department of clinical psychology—at the *All-Union Institute of Experimental Medicine* (VIEM) (Vygodskaya & Lifanova, 1996a, pp. 129, 318—319), most likely with the Psychological Sector of the UPNA in Kharkov as a model for this new organizational unit. According to Vygotsky's archival documents, he was going to form another research team and invite a number of individuals including Solov'ev, Zankov, Veresotskaya, (M. or R.) Levina, Slavina, and Shif (Vygodskaya & Lifanova, 1996a, p. 129). Unfortunately, this project was never realized: in May, 1934, due to throat haemorrhage that was caused by chronic tuberculosis Vygotsky was prescribed bed rest until his hospitalization and death in June, 1934. This is the situation A.R. Luria described in his speech at the L.S. Vygotsky memorial meeting in Moscow at the *Dom Uchenykh* (House of Scientists) on January 6, 1935: “This death was even more tragic given that Lev Semenovich died amongst everybody's devotion and love, for the first time in his life being on the verge of bringing all his plans to life and gathering the organized group of researchers he had been dreaming about all his life, and who could undertake the realization of everything that was hidden in this brain of a genius” (A. R. Luria, 1935/2003, p. 275).

4.4. **Phase Four (1934-1936): *The Circles of Vygotskians and disintegration of the original research program***

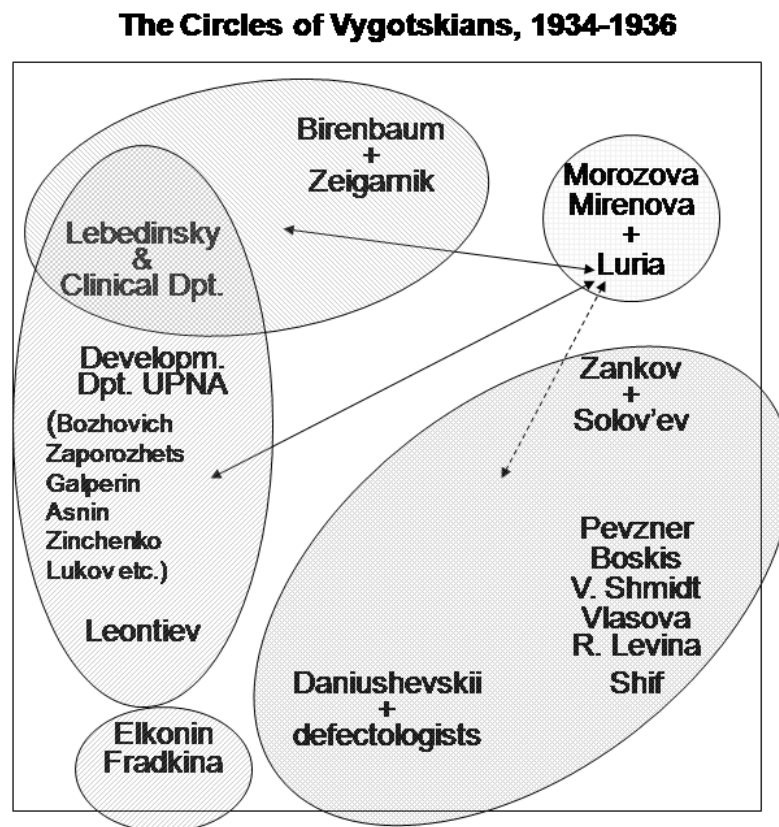


Figure 4. The Circles of Vygotskians in 1934-1936

After Vygotsky's death, several groups (or for that matter, several circles) of researchers formed. They were typically institutionalized in specific research or educational organizations, and comprised more or less constant groups of researchers during specific periods of time. In addition to these circles, a number of nomads migrated between groups and remained relatively independent or even distanced themselves from any research or institutional affiliation. The two major circles were comprised by Vygotsky's students at the Defectological Institute and scholars of the

so-called Kharkov group (Kharkov school) that remained virtually unchanged from the preceding period.

After Vygotsky's death and up until 1936, there is no evidence of any close ties between Luria and Leontiev, who seem to have remained relatively distant from each other, making their own careers. In 1934 both Luria (in March) and Leontiev (in the end of the year) returned to Moscow. Luria joined Medico-biological (from 1935 Medico-genetic) Institute where he headed Psychological department and collaborated with Anna Mirenova, Nataliya Morozova, Faina Yudovich, and Viktor Kolbanovskii. Incidentally, the former was the Director of the Institute of Psychology (in early 1930s. Institute of Psychology, Paedology, and Psychotechnics) in 1932-37. From October, 1934, Luria was also serving as the Head of the Department of Clinical Psychology at the All-Union Institute of Experimental Medicine (VIEM). Leontiev also left Kharkov and in October 1934, like Luria, was hired by the Institute of Experimental Medicine, where he headed the Laboratory of Genetic (i.e. Developmental) Psychology. Around this time Leontiev also became a Professor at *Vysshij Kommunisticheskij Institut Prosvescheniya* (VKIP, Higher Communist Institute of Enlightenment). Formally, he also remained employed by the Ukrainian Psychoneurological Academy in Kharkov (until December 1936) and Kharkov State Pedagogical Institute (until December 1, 1937), and occasionally traveled to Kharkov to supervise the work of his colleagues (A. A. Leontiev, Leontiev, & Sokolova, 2005, pp. 59, 63). At that time Leontiev also worked in the *Vysshij Kommunisticheskij Institut Prosvescheniya* (VKIP; The Higher Communist Institute of Enlightenment) in Moscow.

Vygotsky's students published several works in the fields of defectology and clinical psychology during this period, with clear recognition of Vygotsky's

supervision or collaboration (Birenbaum, 1934; Birenbaum & Zeigarnik, 1935; Kaganovskaya & Zeigarnik, 1935; Levina, 1936; Samukhin, 1935; Samukhin, Birenbaum, & Vygotsky, 1934; Zankov, 1935; B. V. Zeigarnik, 1934; B. V. Zeigarnik & Birenbaum, 1935). These papers are truly invaluable in the presentation of Vygotsky's clinical work, often considered obscure and, generally, atypical of his mainstream cultural-historical research. In fact, it is normally believed that "a composite picture of Vygotsky as clinical practitioner cannot be reconstructed" (Van der Veer & Valsiner, 1991, p. 77). But this corpus of scientific publications seems of particular promise of solving the mystery of Vygotskian clinical research and practice, and shed the light on the last stages of development of Vygotsky's thought by the time of his death in 1934. However, only indirect, camouflaged references to Vygotsky were possible after the decree of 1936, which certainly complicates the task of historical reconstruction.

During this period the Leningrad group of developmental psychologists from the Herzen State Pedagogical Institute established first personal and professional contacts with the Kharkov group and became increasingly closer to the Kharkov developmentalists.



4.5. **Phase Five (1936-1941):** *Formation of a new center and the beginning of the “Kharkov school of psychology” and the Vygotsky-Leontiev-Luria School*

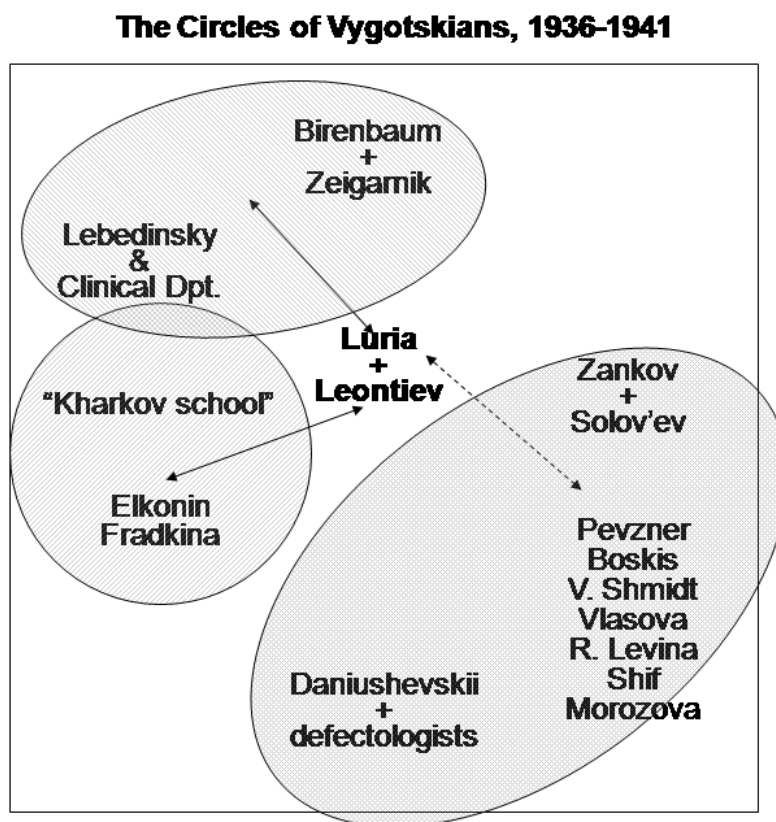


Figure 5. The Circles of Vygotskians in 1936-1941

The notorious decree of the Communist Party on “paedological perversions”—which officially banned studies in paedology (i.e. the interdisciplinary study of the child)—also had a considerable negative affect on all psychological research nationwide (Joravsky, 1989). For instance, dramatic changes in the status of local psychologists took place in Kharkov. From the end of 1936 to the beginning of 1937, the *Ukrainian Psychoneurological Academy* was reorganized and renamed the *Ukrainian Psychoneurological Institute* (P. V. Voloshin, 1994) and its Psychological Sector significantly reduced. Out of the three Departments of the Psychology Sector,

only the Department of Clinical Psychology survived. As a result psychologists like Filip Bassin or Anatolii Rozenblyum moved to other cities (Moscow and Poltava, respectively), whereas others like Pyotr Gal'perin, a Medical Doctor by education, had to resume full-time clinical practice (Haenen & Galperin, 1989). By the end of 1936, Luria had to resign from all his chief positions and “disappear” in the relative quietness of his new internship at N.N. Burdenko's Clinic of Neurosurgery in Moscow that he qualified for as a recent Medical Department graduate, in isolation from the social pressure. This internship (1937-39) probably helped him survive the years of the Great Terror. Then, and in 1939, he joined the Institute of Experimental Medicine where he headed the Laboratory of Experimental Psychology (E. A. Luria, 1994, p. 89).

Generally, the two years of 1936-1937 of the Stalin's Great Terror can be best described as a period of the utmost instability, anxiety and disorientation. The period is particularly poorly documented and notoriously underrepresented by the historians of Soviet psychology; quite illustrative is the fact that the data on the directorship of the only psychological institute in the country is controversial and inconsistent. According to different sources, Kolbanovskii remained the director of the Institute of Psychology until 1936 or 1937, whereas Kornilov resumed directorship in 1937 or 1938. In his 1989 book David Joravsky remarked that it is not clear whether “there was a transient occupant” between appointments at the leading position of this institution (Joravsky, 1989, p. 523). Unfortunately, not much has changed in the historical scholarship on the topic since then. The situation is aggravated by such accounts as Leontiev's report that he returned to the Institute of Psychology in 1936 (A. N. Leontiev & Leontiev, 1976/2005, p. 381), or, alternatively, in 1937 (A. N. Leontiev, 1999), and his special mention that this happened after Kornilov took over

the Institute from Kolbanovskii, which, according to most accounts must have taken place not sooner than in 1938. An historian of science must be particularly attentive to details and rely on his intuition and judgement in reconstruction of this historical period and in making sense the events of the time. On the other hand, I argue that this period seems to be particularly important for the development of the entire course of the Soviet psychology development. Let us see why this is so. The careers of the two protagonists, Alexei Leontiev and Alexander Luria, are particularly interesting in this respect and deserve a closer analysis.

On the basis of fairly fragmentary evidence available to date we know that both Leontiev and Luria were very active and had made quite impressive careers by their mid-thirties when the disastrous July 4, 1936 decree prohibiting paedology was issued. A series of discussions of the decree ensued. Criticism of Vygotsky and his associates, one of the leaders of paedology at the time, concluded with a notorious booklet by E. Rudneva *Paedological perversions of Vygotsky*. This publication that came out in January of 1937 virtually replicated the title of the Communist Party decree and culminated the major offensive against Vygotsky and his followers. There is circumstantial evidence to show that both Leontiev and Luria were forced to quit their jobs around 1936-1937. We can also assume that Luria had to flee from Moscow and found a relative refuge in the Caucasus area. From the publication of Luria's correspondence with Max Wertheimer we know, for example, that his letter to Wertheimer of September, 1936 was sent with the return address in Teberda (Caucasus region) (King & Wertheimer, 2005). Likewise, it was in Tbilisi, Soviet Georgia, under unclear circumstances, that Luria defended his doctoral dissertation, in 1937. Perhaps, resigning from his professorship and fleeing from the capital were not the only social strategies for physical and professional survival used by Luria and

Leontiev. Thus, we know about a paper by Leontiev on “The teaching about environment in pedagogical works of L.S. Vygotsky (a critical investigation)” that was most likely the basis for an oral presentation that he made some time during the turbulent years of 1936-7. For instance, such presentation could have taken place in 1937 when, after a considerable break, Leontiev returned to the Institute of Psychology. This paper was originally located in the archives of the Institute and was not published until the late 1990s when it was incidentally discovered by the well-known Russian scholar Irina Ravich-Shcherbo. Leontiev’s critique of Vygotsky in this paper might have served the purpose of distancing him from the then outlawed former teacher. Another strategy that both Leontiev and Luria must have applied was to establish personal connections with decision-makers and key individuals in power at that time; in the absence of any official or archival documents, we can only speculate about such possible connections. For example, Alexander Luria’s father, Roman Al’bertovich Luria, was one of the most successful medical professionals of the time, and the personal doctor to quite a few of the elite inhabitants of the Moscow Kremlin, including the Prosecutor General of the USSR Andrei Vyshinskii (E. A. Luria, 1994). Another of Alexander Luria’s possible connections with the Communist Party leadership was Lev Sheinin, an aide to Vyshinskii in late 1930s, with whom Luria collaborated during his studies on the traces of affective reactions in suspected criminals (A. R. Luria, 1928/2003, 1929/1930). This research resulted in the publication of Luria’s first major book *The nature of human conflicts* (A. R. Luria, 1932a), which was instrumental in designing the lie detector as we know it today. It is still unclear who exactly helped Luria and Leontiev establish personal connections with key figures in power, but it is fairly clear that both scholars were successful in building such connections with powerful patrons of the time.

It is not incidental that it was in 1939 that both Luria and Leontiev were appointed to supervisory positions in research and educational institutions in Moscow and Leningrad. As Krementsov points out

by 1939, Soviet scientists understood perfectly well the principles of operation of the Stalinist science system and had learned to use that system to their own advantage. They knew that the real power was concentrated in the highest party bodies—the Central Committee and its Secretariat—and they petitioned party bosses in numerous letters. For their part, the party bosses read scientists' petitions and relayed them with their own remarks and notes to the lower level of the party hierarchy “for consideration” or “for implementation,” and sometimes “for archiving”. These second-hand echelon bureaucrats prepared concrete decisions and sent them back to the top for approval. The behaviour of both the top officials and their subordinates was shaped by their own interests and agendas, and thus by considerations external to the scientific questions raised in the petition (Krementsov, 1997, p. 80).

It is clear that from that the end of 1930s onwards only those individuals who thoroughly understood the real meaning of Soviet science policy and the internal mechanics of decision-making in the country could make scientific careers in the Soviet Union.

In 1940 Leontiev defended his dissertation on the “Genesis of the psyche”, largely based on empirical research on the origin of sensation conducted in Kharkov and Moscow in mid-1930s under his supervision. Along with S.L. Rubinshtein and B.M. Teplov, an opponent at Leontiev's doctoral dissertation defence was Leon Orbeli, a great Pavlovian scholar and one of the most influential figures in Soviet scientific hierarchy before the WWII. By 1940 Orbeli, considered Pavlov's scientific

heir, was the Head of the Institute of Physiology of the Academy of Sciences of the USSR (from 1936), I.P. Pavlov Institute of Evolutionary Physiology and Pathology of Higher Nervous Activity (from 1939), Member of the Academy of Sciences of the USSR (from 1935); furthermore, in 1942 Orbeli was appointed Vice-President of the Soviet Academy of Sciences and in 1943 – the Head of the Military Medical Academy in Leningrad. It seems that connections with Orbeli as well as with a number of other prominent scholars (and, most likely, Soviet officials) of the highest rank in the hierarchy of the Soviet scientific establishment was one of the key factors that facilitated Leontiev's future success as one of the leaders of Marxist Soviet psychology. Furthermore, as a token of recognition of their trustworthiness and loyalty, both Luria and Leontiev were invited to coauthor two articles for most prestigious *Bol'shaia Sovetskaia Entsiklopediia* (Great Soviet Encyclopaedia), particularly such important ones as *Psikhologiia* (Psychology) and *Rech'* (Speech) (A. N. Leontiev & Luria, 1941; A. R. Luria & Leontiev, 1940). In addition, they contributed an independent article each: both Luria's *Psikhoanaliz* (Psychoanalysis) (A. R. Luria, 1940a) and Leontiev's *Psikhika* (Psyche) (A. N. Leontiev, 1940a) came out in 1940. All these publications of Luria and Leontiev in this multi-volume—perhaps the most prestigious edition of the time—marked the inclusion of their authors to the Soviet scientific *nomenklatura* of the highest rank. Hence, not surprisingly, during the war Luria and Leontiev were appointed heads of the hospitals in the Urals region. Fairly soon thereafter both joined the Communist Party of the Soviet Union (Luria in 1943, Leontiev in 1948), the move that, according to “the existing rules of the game” should most likely be interpreted as a precondition of social success, given that “Party membership served as an obligatory stepping stone to

any serious aspirations for career advancement in the Soviet Union” (Cole & Levitin, 2006, p. 270).

Thus, from the end of the 1930s Luria and Leontiev became the new organizational centre of Vygotskian psychology. Also, this is the origin of the “school of Vygotsky-Leontiev-Luria”—a term that would not be introduced until the late 1970s. As the new center, Luria and Leontiev served as mediators between different groups of scholars who continued the Vygotskian line of research in the country. For instance, the remains of the Leningrad group and the Kharkov group received a considerable support from A.N. Leontiev around mid-1930s when they established personal and professional ties between the two groups in Kharkov and in Leningrad (thus, for instance, Leontiev served as an official supervisor of the dissertation of El’konin, from Leningrad, at his defence in 1935). The ties between the Leningrad and Kharkov groups especially strengthened after 1939 when Leontiev—like Vygotsky in 1931—started commuting between Moscow and Leningrad where he spent ten days of each month lecturing at Leningrad Herzen Pedagogical Institute. In turn, Luria was instrumental in establishing correspondence and exchange between clinical and defectological researchers (see, e.g., A. R. Luria, 1937b) that, in turn, considerably contributed to his own studies on brain and the then-nascent discipline, neuropsychology.

## **Chapter 5. Action: Making empirical science**

In this section I survey the experimental designs and empirical studies conducted by the members of the larger Vygotsky Circle in the 1930s-early 1940s. If there ever was a period of relatively free development of Vygotskian research, it was abruptly interrupted in 1936 by the Communist Party decree. The dramatic decrease in the number of references to Vygotsky's work after 1936 considerably complicates any analysis of the intellectual development of his ideas in the second half of the 1930s. The sources for a history of this empirical research are extremely limited: a great many of archival materials were destroyed during the Great Terror or the WWII (Ginevskaya, 2005; A. A. Leontiev, Leontiev, & Sokolova, 2005; Vygodskaya & Lifanova, 1996a, 1996/1999; A. V. Zeigarnik, 2007); even of those that remain, the main archives have not been systematically explored and for a number of reasons, still remain mainly unavailable. Therefore, publications or manuscripts that describe research reports remain the main source of information, but there are a number of problems with this group of sources: the primary problem is the fact that not much was published on a regular basis in the 1930s, and some very important of studies conducted during this period were published only after the war, or even not until after Stalin died (e.g., A. R. Luria, 1976; A. R. Luria & Yudovich, 1959); another major obstacle is the difficult accessibility of these texts. Furthermore, during the decade of the 1930s, Soviet science developed very specific discursive practices that produced many ambiguous texts, designed to hide their real intended meaning from the institutions of state censorship (see next chapter). The rarity and relative inaccessibility of these publications, their deliberately obscured meaning, and very special discursive practices, the virtual absence of scientific references in them, and the highly specialized character of the available materials, require an in-depth analysis



by specialists in related fields to uncover the real meaning of these texts and their relation to Vygotsky's legacy. In this study, I limit my task to providing an overview of the published texts known to date, as a guide for future research.

A Vygotskian psychology of the cultural development of higher mental functions interpreted development as a bi-directional process involved a program of interlinked and mutually enriching research on the development and pathological degradation of higher mental functions. However, for practical purposes, the wide range of Vygotskian studies done by Vygotskys' students and collaborators can be divided into several groups, according to their subject domains and the institutional affiliation of the researchers. In its most explicit form this program was pronounced in Vygotsky's speech given on April 28, 1934, forty days before his death (Vygotsky, 1934/1960a). These two strands—research on normal development (general and child psychology) and developmental pathology (defectological and clinical research)—seem to cover the entire landscape of Vygotskian studies before 1941, as discussed below.

*Pathology of development: Clinical psychology and defectology*

Vygotskian psychology of the second half of the 1930s is sometimes described as a “lacuna” or a “gap” in our knowledge of the history of the science. This characterization is particularly true of the Vygotskian study of pathology. Indeed, the history of the research in clinical psychology in Vygotsky’s tradition is not yet written, and this study presents only a preliminary and very brief history of Vygotskian clinical psychology. This history begins with the travel of the American doctor, Jacob Kasanin, to Soviet Russia.

*Clinical Psychology*

When Jacob S. Kasanin, M.D., an American psychiatrist of Russian origin, a Senior Research Associate at the Boston Psychopathic Hospital and Director of Mental Hygiene of the Federated Jewish Charities in Boston planned his trip from Boston to the Soviet Union in 1930 he could hardly have predicted that he would later become the “Columbus” of Vygotskian clinical psychology for international, primarily, North American science. It is not clear what exactly motivated his trip, but we can hypothesize that several meetings at the IX International Congress of Psychology held September 1-7, 1929 in Yale University had sparked his interest in the recent achievements of Soviet scholars back in the USSR. However, born in the Russian Empire and coming from a family of expatriates, Kasanin might have had other, more personal, reasons for visiting the Soviet Union.

According to his own testimony, once in Moscow, Kasanin—who might have become acquainted with Alexander Luria during the IX International Congress of Psychology the year before—met with Vygotsky (Kasanin, 1939/1944), who

presented Kasanin with his recent work on schizophrenics and demonstrated the *Sakharov-Vygotsky* method of investigating conceptual thinking (Sakharov, 1930, 1930/1994; Vygotsky, 1934a, 1934e). Kasanin was greatly impressed with the Vygotsky group's work that, subsequently, seems to have considerably influenced his own thought (Windholz, 1947). Kasanin was instrumental in introducing Vygotsky's ideas on pathological development and clinical psychology to the North American scientific community.

Thus, Kasanin's Moscow acquaintance with Vygotsky and his ideas led to one of Vygotsky's first journal publications in the West. In his letter to A.R. Luria of June 1, 1931 Vygotsky wrote: "I received a letter from Kasanin saying that my article will be published" (Vygotsky & Puzyrei, 2007, p. 33). However, the article or some version of it called "On the problem of the psychology of schizophrenia" first appeared in Russian in 1932 in the journal *Sovetskaya nevrologiya, psikiatriya i psikhigigiena* (Vygotsky, 1932) and only in 1934 did its English translation come out (Vygotsky, 1934e). Later, Kasanin initiated the publication of yet another important work by Vygotsky, namely, the translation of the last, summative chapter of Vygotsky's *Thinking and speech* (Vygotsky, 1939) published posthumously in the original Russian five years earlier (Vygotsky, 1934a). By the time of its English publication, the book had been unofficially banned in the Soviet Union in 1936 due to its author's affiliation with paedological research. Vygotsky's research on concept formation appeared to be the first of his work that garnered some attention and was, to some extent, disseminated in North America. Kasanin's own research, following in the footsteps of Vygotsky and his team, was instrumental in the international dissemination of his ideas.

Thus, Jacob S. Kasanin extended Vygotsky's research, and along with Eugenia Hanfmann, another American researcher of Russian origin replicated Vygotsky's work in clinical settings in North America. In 1936, Kasanin invited Hanfmann to work on a study of concept formation (Simmel, 1986), in which they applied the research method for studying concept formation to diagnosing schizophrenia, creating what is nowadays relatively well-known in the West as the *Hanfmann-Kasanin test*. The new test was presented and discussed in a series of journal publications (Hanfmann & Kasanin, 1937; Kasanin & Hanfmann, 1938) and conference presentations (Kasanin, 1944), culminating in a monograph by the two authors published in 1942 under the title "Conceptual thinking in schizophrenia" (Hanfmann & Kasanin, 1942).

Interestingly enough, in his introductory footnote to the 1934 English publication of Vygotsky's "To the problem of the psychology of schizophrenia" (1932), Kasanin commented that "for the past five years Vygotsky [sic], together with Professor Luria, has been doing extremely interesting work on the psychology of schizophrenia, utilizing the experimental technic of the Gestalt psychology. The article was written at my request over three years ago, and since then a great deal more work has been done" (Vygotsky, 1934d, p. 1063). It is easy to calculate that, according to Kasanin, Vygotsky wrote the paper around 1931 (as confirmed by Vygotsky's letter to Luria, see above), although clinical research in Vygotsky's Circle was launched around 1929. Later, Kasanin clarified this somewhat enigmatic reference to the "extremely interesting work" of 1929-1931 and the "great deal more work" done since 1931. In fact, several references found in later publications by Kasanin and Hanfmann shed some light on the development of the original research Vygotsky presented to Kasanin in 1930 in Moscow. For instance, in their 1942 book

*Conceptual thinking in schizophrenia* (Hanfmann & Kasanin, 1942), Hanfmann and Kasanin discuss several studies published from 1934-5 that were conducted by members of Vygotsky's Circle, such as B. Zeigarnik, G. Birenbaum, and N. Samukhin. However, Kasanin and Hanfmann do not present the entire story of the clinical research done by Vygotsky and, after his death, especially by his students in the 1930s.

From Vygotsky's correspondence with Luria we know of most of the intensive research in clinical settings done during the last years of Vygotsky's life. Thus, in his letter to Luria of November 21, 1933 Vygotsky reported a series of such studies virtually ready for publication:

Finally, regarding the series. If they are going to publish it in a real sense and periodically (it has to be from issue to issue), we need to take it in no uncertain terms. I have: (1) a classification of aphasias; (2) Birenbaum and Vygotsky, aphasia and dementia; (3) Birenbaum and Zeigarnik, agnosia; (4) Vygotsky, written speech in brain disorders; (5) Vygotsky, grammatical disorders, and so on, and so forth, *ohne Zahl* [German: "without number"] as our patient says when asked how many fingers she has on her hands. I will submit the first article by mid-December, and we will prepare three or four in reserve. Like Lewin's *Gestalttheorie*, and so on, it is essential that there be an overall title for all of the studies (Vygotsky & Puzyrei, 2007, p. 48).

Unfortunately, we still have no clear evidence of these studies, and the manuscripts of the great many of Vygotsky's studies that were reportedly ready for publication have not been found yet (Akhutina, 2007). However, we know that the Moscow group of Vygotsky's students and collaborators continued the studies he had initiated in clinical and pathological psychology (Birenbaum, 1934; Birenbaum &

Zeigarnik, 1935; Dubinin & Zeigarnik, 1940; Kaganovskaya & Zeigarnik, 1935; Samukhin, 1935; Samukhin, Birenbaum, & Vygotsky, 1934; B. V. Zeigarnik, 1934, 1940, 1941; B. V. Zeigarnik & Birenbaum, 1935). These studies led to wartime efforts in several military hospitals to restore motion and higher mental functions, conducted under the supervision of Luria (Galperin, 1943; Galperin & Ginevskaya, 1947; A. N. Leontiev & Ginevskaya, 1947; A. N. Leontiev & Zaporozhets, 1945; A. R. Luria, 1947, 1947/1970).

### *Defectology*

Vygotsky himself was interested in the pathology of development from early in his scientific career. In 1924, he defined his scientific and professional goal as “educating blind-deaf-mute children” (Vygodskaya & Lifanova, 1996b, 1999), an interest he subsequently developed in his work. Vygotsky’s main research on the pathology of development and in clinical psychology were studies of aphasia and of dementias like Parkinson’s or Pick’s diseases (Samukhin, Birenbaum, & Vygotsky, 1934; Vygotsky, 1935b, 1935/1956, 1935/1983), schizophrenia (Vygotsky, 1932, 1932/1956, 1933, 1934e, 1934/1994), and, relatedly, the localization of brain functions (Vygotsky, 1934b, 1934/1960b).

In particular, the role of language in human development was investigated through clinical cases of speech pathology—specifically, aphasia (Galperin & Golubova, 1933; Kozis, 1934; Lebedinskii, 1933b, 1934, 1936b, 1941; Lebedinsky, 1936; A. R. Luria, 1932/1933, 1940b, 1943, 1947, 1947/1970). These studies, and Vygotsky’s later work on the localization of brain functions (Vygotsky, 1934a), significantly contributed to Luria’s neuropsychology (A. R. Luria, 1966). Vygotsky’s study on schizophrenia was developed through several studies of concept formation

(Bassin, 1938), action structure (Lebedinskii, 1940; Lebedinskii, Artyukh, & Voloshin, 1938, 1938/1939; Zaporozhets, 1939b) and speech disorders in schizophrenia (Bassin, 1938; Lebedinskii, 1938b; Tatarenko, 1938).

Unlike Vygotskian clinical psychology, the defectological work of Vygotsky and his students at the *Experimental Defectological Institute* (EDI) is exceptionally well documented. Traditionally, Soviet defectology included such research strands as typhlopsychology (research and rehabilitation of the blind), surdopsychology (the deaf and mute), logopsychology (pathologies of speech development), oligophrenopsychology and pathopsychology (mental disturbances and retarded mental development). All these fields were covered and thoroughly explored by Vygotsky's students at EDI. This research and clinical practice is reflected in a series of books published from the 1930s to the early 1940s. Thus, Vygotsky's ideas on the unity of affect and intellect served a foundation both of defectological studies of *mental retardation* (Danyushevskii & Zankov, 1941; Vygotsky & Danyushevskii, 1935; Zankov, 1935, 1939; Zankov & Danyushevskii, 1940), and research on "*difficult children*" (*trudnye deti*) (Pevzner, 1941; Pevzner, Zankov, & Shmidt, 1933; Vygotsky, 1936). These became the classics of Soviet pathopsychology and oligophrenopsychology as well as research on the corrective education and the psychology of upbringing (M.S. Pevzner, N.G. Morozova, L.I. Slavina, L.I. Bozhovich, et al.).

Another major lineage of Vygotskian research are his studies on the role of speech in the child's development, the phenomenon of the inner speech (R. Levina), and the idea that the entire system of psychological functions in the child is transformed with the introduction of speech. Investigation of the effect of *speech pathology* on the child's development occupied a very special place in this research,

and is exemplified by defectological studies of deaf and mute children (Boskis, 1939; Danyushevskii & Zankov, 1941; Korsunskaya & Morozova, 1939; Morozova, 1944; Zankov & Danyushevskii, 1940; Zankov & Solov'ev, 1940) and of the problems of slow or abnormal development of speech, or reading and writing in normal children (Kupriyanova, Levina, Pravdina, & Samoylenko, 1941; Levina, 1936, 1940). This research was foundational for the post-war defectological research and practice in the fields of surdopsychology (I. M. Solov'ev, Zh.I. Shif, R.M. Boskis, N.G. Morozova) and logopsychology (R.E. Levina), and was closely connected to Alexander Luria's pre-war research on *brain pathology*, dynamic localization of brain functions, and rehabilitation (first published after the war) that constituted the core of the pioneering Lurian cultural-historical tradition in *neuropsychology* that originated in the work of Vygotsky (Vygotsky, 1934c, 1934/1960b, 1934/1997). Indeed, Vygotsky and Luria were the founders of the discipline (Akhutina, 1996, 1996/2003, 2004).



***Normal development: general, developmental and child psychology***

The main line of Vygotskian empirical research on normal human development was continued in a number of Ukrainian scientific and educational institutions by the members of the so-called Kharkov school. Despite certain criticism to Vygotsky's work expressed by the Kharkov psychologists, the Kharkov school's empirical studies are fundamentally grounded in Vygotskian research program and share common root in Vygotsky's cultural-historical psychology of higher mental functions. Furthermore, the organizational structure of the *Psychological Sector* of the *Ukrainian Psychoneurological Academy* (UPNA) in Kharkov was most likely designed according to Vygotsky's framework of developmental research.

For Vygotsky, studying human psychical life scientifically meant studying its historical development. Vygotsky's entire research program aimed to investigate the development of culturally mediated, or 'higher', mental functions. Vygotsky and his collaborators emphasized three main lines of development, namely: (1) the evolutionary development of *Homo Sapiens* as a biological species, (2) the historical development of modern, cultural man, and (3) the ontogenetic development of human children into adults (Vygotsky & Luria, 1930). As a product of all three lines of development, cultural man "may be understood and explained scientifically only by analyzing *the three different paths that make up the history of human behavior*" (Vygotsky & Luria, 1930/1993, p. 36).

By the summer of 1934, representatives of all three Departments of UPNA's Psychological Sector shared the assumption that "generation of higher intellectual forms of psychological activity over the course of the history of society is decisive for psychological development" (Galperin, 1934, p. 34). Thus, researchers at the UPNA

*Department of General Theory in Psychology* investigated the interrelations between psychology and physiology, and the place of psychology among other Human Sciences, the development and degradation of higher mental functions, and the methodology of distinctly psychological research. This work resulted in a number of theoretical papers written by the members of the School that still need to be assessed in relation to Vygotsky's theory (Asnin, 1938, 1941a/1980, 1941a/1981; Galperin, 1935; Lebedinskii, 1938c; A. N. Leontiev, 1940-41/1994). According to Galperin (1934), experimental research in UPNA's Psychological Sector had two major goals. First, the study of human thinking, speech, and practical activity—including their interrelations and relations to other psychological functions at different stages of human development, carried out at the *Department of the General and Genetic Psychology*; second, analysis of the degeneration of distinctly human higher mental functions caused by organic or functional damage to the human brain, carried out at the *Department of Clinical Psychology*.

The *Department of the General and Genetic Psychology* experimentally investigated all the three lines of development important to Vygotsky. The triad of “ape, primitive, and child” (Vygotsky & Luria, 1930, 1930/1993) was roughly mirrored in the three Laboratories of Animal, General, and Child experimental psychology (Galperin, 1934). Thus, experimental research on the psychology of underdeveloped rural population of Uzbekistan conducted by Luria in his two Central Asian expeditions (A. R. Luria, 1931a, 1932b, 1933, 1934, 1976), aimed at experimental research on “primitive” people under the conditions of rapidly changing social environment. This research was received very negatively by officials in the Soviet Union in the early 1930s and, as a result, Luria's academic career, his freedom, and even his life, were threatened—which may explain his move to the *Ukrainian*

*Psychoneurological Academy* by the end of 1931 (E. A. Luria, 1994). For these political reasons, research on “primitive” people in Vygotskian cultural-historical psychology was put on hold for several decades until the studies of Luria’s prominent students Michael Cole, from the United States, and Peeter Tulviste (Cole, Gay, Glick, & Sharp, 1971; Cole & Means, 1981; Cole & Scribner, 1974; Tulviste, 1991).

However, the other two main lines of Vygotskian developmental research remained in relatively better standing with official Marxist science of that time, and a number of studies on animal and child psychology were conducted by the Kharkov researchers in the 1930s. With minor exceptions (Zaporozhets, 1941c) most of the psychological studies by Zaporozhets, Dimanshtein, Solomakha and Bassin on sensation and perception in animals done under Leontiev’s supervision were published in his doctoral dissertation prepared by 1939 and defended in May, 1941 in Leningrad. An off-shoot of these studies with animals was experimental investigation of the development of skin sensation to light carried out by Leontiev and his collaborators—in consultation with P. P. Blonskii, A. I. Bogoslovskii and M. S. Lebedinskii—in Kharkov (Asnin, Zaporozhets, Drobantseva) and Moscow (Poznanskaya, S. Ya. Rubinshtein) (Asnin, 1940; A. N. Leontiev, 1940b, 1940/1981a, 1940/1981b, 1940/1981c; Zaporozhets, 1941a, 1941a/1980, 1941a/1986a). According to Leontiev’s plan, sketched around 1940, all these studies were part of a major research project that was to continue until 1948-9. The results of these studies were to be published as a monumental three-volume monograph on “The development of the psyche (mind)” that Leontiev had already partly completed by 1941. Unfortunately, the unpublished materials perished during the war (A. A. Leontiev, Leontiev, & Sokolova, 2005). These lost manuscripts were never restored and only a very brief

summary of this project was published after the war (A. N. Leontiev, 1947a, 1947/1981a, 1947/1981b).

As in America and Europe, methods of psychological research on animals made their way into the research on child psychology done in Kharkov. The experiments of Wolfgang Köhler on the “practical intelligence” of apes (Köhler, 1921, 1921/1926) triggered a series of studies of children’s practical intelligence and tool-mediated practical action; these studies showed qualitative differences between the ape’s and the child’s use of use of auxiliary tools and instruments (Asnin, 1941a, 1941a/1980, 1941a/1981; Bozhovich, 1935, 1935/2006a, 1935/2006b, 1935/2006c; Galperin, 1937, 1937/1980; Zaporozhets, 1936, 1938a, 1938a/1986, 1939a, 1939a/1986).

Experimental studies in the fields of Child and General psychology made up a considerable part of research at the UPNA’s *Department of General Experimental and Genetic Psychology*. Within this broad topic, Vygotsky’s students in Kharkov focused on investigating mediated, or ‘higher’ mental functions—especially their genesis, structure and interrelations (Vygotsky, 1931/1960, 1931/1998). In addition to studies on children’s practical intellect, several studies were conducted on thinking, including: (1) a ‘post-Piagetian’ study of children’s reasoning (Zaporozhets, 1942; Zaporozhets & Lukov, 1941; Zaporozhets & Lukov, 1941/1980, 1941/2002), (2) “transfer” mechanisms in thought development (Asnin, 1936, 1941b, 1941b/1980; A. N. Leontiev, 1933/1994), (3) the effect of formal schooling on children’s worldviews (Bozhovich, 1936, 1937, 1945, 1945/1980; Bozhovich & Zinchenko, 1941, 1941/1980), and (4) the development of children’s visual-operational and visual-imagic thought (Asnin, 1941b/1980; Khomenko, 1941b, 1941b/1980).

A separate series of studies was conducted on the age-related characteristics of: (1) voluntary and involuntary remembering in children and adults (Galperin, 1938; P. I. Zinchenko, 1936, 1937, 1939/1983, 1939a, 1939b, 1939b/1980, 1941), (2) the psychology of motion (Asnin, 1939, 1941b, 1941b/1980; Galperin, 1937, 1937/1980; Kotlyarova, 1946), and (3) the development of perception (Ginevskaya, 1941; Gordon, 1941; Khomenko, 1939; Kotlyarova, 1940, 1946; Mistyuk, 1941; Zaporozhets, 1940, 1941a, 1941a/1980, 1941a/1986b)—specifically, the perception of art by children (Aranovskaya, 1940, 1945; Khomenko, 1938, 1940, 1941a, 1941c, 1941c/1980; Kontsevaya, 1941; Titarenko, 1941; Zaporozhets, 1948, 1949, 1949/1986).

Another of Vygotsky's essential contributions to developmental psychology was his work on the role of play (Vygotsky, 1933/1966, 1933/1967) and learning (Vygotsky, 1935a, 1935c) in human development. This line of research remained largely underdeveloped in Vygotsky's own writings, and is a facet of his theory most clearly articulated through collaborative work of his students. The Kharkov group focused mainly upon studies within lab settings (for discussion see Asnin, 1941a, 1941a/1980, 1941a/1981) as well as in natural settings of children's play and learning environments. The role of children's play on their development was also investigated by Vygotsky's students in Kharkov and Leningrad (Elkonin, 1978/2005; Fradkina, 1945; A. N. Leontiev, 1944, 1945; Lukov, 1937, 1938, 1939). A wide range of the studies on learning were conducted by Vygotsky's students at the *Ukrainian Scientific Institute of Pedagogy*, and at the *Pedagogical Institutes* of Kharkov, Poltava, and Leningrad (Asnin & Zaporozhets, 1936; Bozhovich, 1936, 1937, 1941, 1945, 1945/1980; Bozhovich & Zinchenko, 1941, 1941/1980; Elkonin, 1940, 1940/1980; A. N. Leontiev, 1947/1983b, 1947a, 1947a/1978; Mazurenko, 1941). Finally, a series of

classical postwar studies continued this early research on psychological environments and their effect on children's affect, motivation, and cognition (Istomina, 1948a, 1948b, 1948/1975, 1948/1977; Manuilenko, 1948/1975).

Another remarkable development of Vygotsky's theory by his students, in the late 1930s, was their increasing interest in the unconscious, and their research on *ustanovka* (what in German was originally called *Einstellung*, or in English 'set' or 'attitude')—a term they most probably borrowed from the Georgian school of Dimitry Uznadze (see, e.g., Piaget & Lambercier, 1944; Uznadze, 1939, 1966). Several studies of 'set' were published (Aks, 1941; Galperin, 1941a, 1941a/1980, 1941b), and the major unpublished work on *ustanovka* by Bassin—reportedly written by 1944 (Savenko, 2006)—seems an important landmark in the long standing interest in the unconscious as understood within the context of the Vygotskian paradigm (Bassin, 1968, 1969, 1978). Indeed, the unconscious as an integral part of cultural historical and activity-oriented psychology still needs its own detailed investigation.

## **Chapter 6. The Script: Making science as discourse.**

### **Discursive technologies (strategies and techniques)**

In the third part of my analysis of scientific practices of the Vygotsky Circle I discuss the discursive practices of this group of scholars—for good reason. Scientific discourse still remains one of the major sources of our knowledge about the actual scientific research (Schickore, 2008). This is especially true of research done by the members of Vygotsky Circle, given that many archival and other historical materials perished during the war, were confiscated or were deliberately destroyed by their owners during the Great Terror (see, e.g., A. V. Zeigarnik, 2007). Yet, there are typically quite serious problems with the unprepared reader's understanding of the scientific discourse created by the members of scientific community in the Soviet Union. The discourse of Soviet science generally and, particularly, the discourse of social and human sciences from 1930s onwards is most unusual and highly problematic for the Western reader, and one needs special skills and possess a great wealth of background knowledge about the time, place and the cultural codes of the social organization of science in order to “decode” the message of this discourse, and without knowing these rules and discursive conventions one can hardly understand the development of Soviet human and behavioural sciences from 1930s onwards. To the best of my knowledge, discursive rules of scientific texts production and their perception by laypeople, representatives of censorship bodies and the scientific community in totalitarian science of the Soviet Union of 1930s have never been subjected to a systematic and rigorous analysis by a consorted cohort of linguists, historians of science and semioticians. In the rest of the chapter I discuss several characteristics of scientific discourse and discursive practices of the Soviet psychology of the 1930s, specifically, the group of the scholars of Vygotsky Circle in

the 1930s. In the absence of systematic research, that remains the future task for full-scale interdisciplinary research, these characteristics of Soviet scientific discourse are presented here as a set of *hypotheses* about the mechanisms of texts production in Vygotsky Circle in the second half of 1930s. These hypotheses are introduced here on the basis of my extensive involvement with the scientific texts of the period and preceed future research that will be able to corroborate—or disconfirm—my preliminary conclusions.

One of the notable and rare examples of analysis of Soviet scientific discourse was done by Kremmentsov who focused on social practice of Soviet science and presented an overview of discursive practices of the Stalinist science. Following Orwell, Kremmentsov used the term “Newspeak” for the Communist Party jargon of the time, elaborated on how “scientists adopted the rhetoric, etiquette, and critical styles of the party-state bureaucracy, skilfully nurturing and exploiting the images and beliefs of their patron and partner to pursue their intellectual, institutional, and career objectives”, and discussed the processes of “translation of the [scientific] community’s interests into the ‘Newspeak’ of party bureaucracy” (Kremmentsov, 1997, pp. 6-8). The guiding metaphor that Kremmentsov used in order to describe the scientists’ agency in relation to the new emerging discourse of science is that of “translation”, stating, for instance, that “to what extent particular agendas were *translatable* and *actually translated* into party lingo often defined the outcome of discipline building, career building, and institutional struggle” (Kremmentsov, 1997, p. 81). Kremmentsov is certainly right when he talks about translation of academic discourse into a Newspeak of party lingo in the social situation of the scientific spokesmen appeal to the patrons in power. However, the situation of scientists’



communication with party bosses certainly does not cover the entire multitude of social situations in which scientific or science-related discourse was produced.

Thus, for instance, Krementsov analyzes scientific discourse of the same author presented at the same time in Party edition *Pod znamenem marksizma* (Under the banner of Marxism) and, on the other hand, in professional academic journals. The difference between the two is clear: the publications in the party edition were typically abound with Newspeak slang and criticism on the grounds of the loyalty to the dogmas of the classics of Marxism-Leninism, whereas professional publications remained relatively distant from the discussion of political issues and presents fairly traditional scientific discussion of the merits and drawbacks of the opponents' research on the rationalist grounds. This trend of the 1920s dramatically changed in the 1930s, though, after Stalin's Great Break, when the two styles of academic discourse converged and, ultimately, merged by mid-1930s (Krementsov, 1997, pp. 28-29). Another important analysis of scientific discourse of that time can be found in a recent article of Meshcheryakov who discussed what he refers to as a "suspicious criticism of Vygotsky" in the works of Piotr Zinchenko, one of the members of the Kharkov school of psychology. Meshcheryakov's analysis of Zinchenko's paper on involuntary remembering that was originally published in 1939 in Kharkov (P. I. Zinchenko, 1939a, 1939a/1983) reveals highly idiosyncratic discursive practices of the author that was, according to Meshcheryakov, motivated by censorship requirements for publication (Meshcheryakov, 2008). In addition, Vladimir Zinchenko analyzed personal notes for A.N. Leontiev's oral presentation in the early 1970s and revealed fairly similar duality in the text of the author—a prominent scholar and scientific administrator, one of the leaders of the official Soviet psychology by that time—who was struggling to reconcile his scientific priorities with the intricacies of

scientific policy-making (V. P. Zinchenko, 1991). Finally, materials of the First Scientific Conference of the Kharkov State Pedagogical Institute that were discovered in the Kharkov Region State Archive helped to uncover some of the mechanisms of scientific text production by the members of Vygotsky Circle in the 1930s (Yasnitsky, 2009). The subsequent discussion in this chapter is based on these publications as well as on some original materials.

Different authors concur on the necessity of analyzing a text within the situation of its communicative act and, furthermore, against the background of the rich texture of the social environment to which that text belongs to. The most basic form of representation of a text within a communicative act is in its relation to a) the author of the text, b) the problems, events, phenomena, etc. that the text deals with and refers to (or, in other words, the *referents* of the text), and c) the target audience of the text.

This basic scheme was developed by German and Austrian psychologist and linguist Karl Bühler in the beginning of the 20<sup>th</sup> century (Bühler, 1933). Indeed, in order to adequately understand a text, one should, minimally, take into account the interplay between the author, the extra-linguistic contents of the text, and the target audience, and how this interplay is reflected in discourse. However, the three components are still not enough to present an adequate description of a text in its social context of communication. In the 1960s Roman Jakobson added several components to Bühler's scheme and presented his model of communication act in relation to the discussion of the interrelated issues of linguistics and poetics (Jakobson, 1960). Jakobson's model seems to be particularly well suited for the purposes of the present analysis of the discourse of Stalinist science. Thus, according

to Jakobson, there are several constitutive factors in any speech event, in any act of verbal communication that Jakobson thus discusses in some detail:

The ADDRESSER sends a MESSAGE to the ADDRESSEE. To be operative the message requires a CONTEXT referred to (“referent” in another, somewhat ambiguous, nomenclature), seizable by the addressee, and either verbal or capable of being verbalized; a CODE fully, or at least partially, common to the addresser and addressee (or, in other words, to the encoder and decoder of the message); and, finally, a CONTACT, a physical channel and psychological connection between the addresser and the addressee, enabling both of them to enter and stay in communication (Jakobson, 1960, p. 353).

Jakobson also presented his model in graphic form:

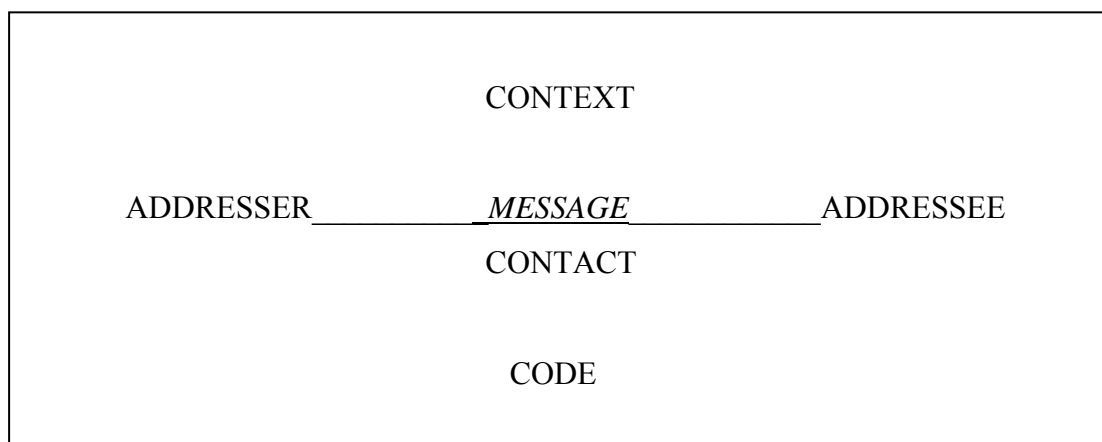


Figure 6. Roman Jakobson’s scheme of constitutive factors of a speech event (Jakobson, 1960)

Following Jakobson, I analyse Soviet scientific discourse, or the Message, in Jakobson’s terminology, by its relation to the constitutive elements of a communicative act, such as Context, Addressee, Addresser, Code, and Contact.

### *Context/Referent*

As we have seen so far, Stalinist science was a hybrid of science and quasi-Marxist ideology with its two guiding principles of *partijnost* and *practicality* (see Chapter 3 of this work), therefore, the topics of philosophy, theory, empirical research, and practical application of scientific research invariably constituted the context of Soviet scientific discourse of the 1930s. The representation of these topics varied significantly depending on the specific author's place in the social hierarchy of the Stalinist scientific *nomenklatura*, occasion of discourse production and presentation, intended audience, kind of public gathering (for oral presentations) or edition (for published works), specific socio-political situation at the time of presentation, manuscript submission or publication, and career goals at the time of presentation or publication, etc. In any case, one should keep it in mind that *each and every text without exception* produced from the mid-1930s until at least the early 1980s intended by its author for public use served the dual purposes of scientific knowledge production and exchange and, on the other hand, needed to conform with fairly strict agenda of Communist propaganda, and to express the loyalty to the Communist party ideology and its powerful bosses-patrons of science. Therefore, virtually all public scientific texts of the time can be localized somewhere on the continuum between ideology and science, but never at one extreme only. By extension, this explains the enormous importance for historians of science of those that authors did not intend to publish, and produced for their own purposes or for a very limited number of close friends or collaborators: for example, private notes, correspondence, various personal archival materials, or texts prepared for publication

abroad, in the context of the Western science (e.g., Lebedinsky, 1936; A. R. Luria, 1932a, 1937b).

### *Addressee/Target audience*

State control in Stalinist science was implemented in different ways, and one of them was Soviet publication policy and its censorship of all printed materials produced in the Soviet Union from the early 1920s onwards. This policy was carried out by the *Glavlit*, the Main Directorate on Literature and Press. Therefore, an author willing to publish his or her work had to take into account the fact that before its publication, the piece of writing was invariably submitted to a Glavlit official, whose responsibility was to read and copy-edit the manuscript, before finally authorizing its publication. This official was the person who made the decision on the actual publications of specific texts and editions, hence, the authors did their best not to displease the censor. In other words, the social institute of official censorship was mirrored in the individual authors' practice of deliberate self-censorship, which is how written scientific discourse of 1930s evolved towards further synthesis with the Party Newspeak.

However, unlike Krementsov, I tend not to emphasize similarities between the language of Soviet science and Bolshevik jargon, but rather the differences between the two. Thus, the distinctive feature of the communicative situation of scientific discourse at that time is what some scholars refer to as the “double” or “dual addressee” (sometimes distinguishing between the two, although mostly using these as synonymic terms). In case of Soviet science, any author who was writing a text for a publication—whether consciously or not—had to develop an orientation towards the “dual” intended audience of the text and present the message that would satisfy the

expectations of quite different if not even opposite types of readership—that is, the scientific community and the censor. Using Orwell’s phraseology, such implementation of Soviet “doublethink” in a verbal text—yet essentially different from Soviet “newspeak”—I refer to as the “doublespeak” of Soviet science (Yasnitsky, 2009); the difference between “newspeak” and “doublespeak” is below discussed at some length. The interrelations between the two addressees of doublespeak were determined by a specific situation of discourse production and depended on several key factors, including the location of publication, discourse genre, the author’s publication record and current standing with the authorities at the time of publication, etc. In other words, the importance of the specific type of addressee for the author should be determined judging on the basis of specific situation in each particular case of publication. Considerably fewer restrictions were placed on the discourse of the texts that were presented orally and not intended for verbatim publication. Therefore, although these points are difficult to document with complete assurance, comparing stenographic reports of oral presentations with the written and published versions of the texts presents historians of science with a very promising analytical technique that can help us better understand the mechanics of doublespeak discourse production in Stalinist science.

### ***Addresser/Author***

In a recently published work written about archival materials concerning the first scientific conference (session) of the *Kharkov State Pedagogical Institute* in 1938 I discussed the issue of authorship of the texts written by the members of the so-called Kharkov school of psychology. In my analysis of the stenographic reports of the conference materials and handwritten editing of the texts, I introduced the hypothesis

of collaborative anonymous authorship of texts produced by several anonymous authors, but finally published under the name of just one of the collaborators (Yasnitsky, 2009). Boris Meshcheryakov in his commentaries to this paper proposed to refer to this scientific practice as the phenomenon of “quasi-pseudonymity”. According to Kharkov psychologist Dr. E.V. Zaika (personal communication in the Fall of 2006), this hypothesis is supported by anecdotal evidence from the late Debora Dubovis-Aranovskaya, one of the members of the group in the 1930s. Unfortunately, the memories of Dubovis-Aranovskaya remain uncorroborated nor has any written account been preserved. Daniil El’konin also supports this hypothesis in his reminiscences of the experience of collaborative composition of an article by several scholars of Vygotsky Circle including such prominent figures as Zaporozhets, Luria, Galperin, and Elkonin himself that was later presented and published under A.N. Leontiev’s name alone (Elkonin, 1983, 1983/1984). Such practice seems to closely resemble the composing practices of the scholars of the Bakhtin Circle (Brandist, 2002; Shepherd, Brandist, & Tihanov, 2003), and the procedures and its meaning require further investigation.

### ***Code/”Doublespeak”***

By the term “code” linguists and semioticians normally mean the sign system and the set of principles that help one prepare speech and make linguistic choices at both structural and vocabulary levels during verbal expression production. This process is also referred to as “encoding”. The reverse of this, the process of “decoding” takes place during the reception of this verbal message, and one’s mastery of code secures his or her ability to adequately understand the meaning of the message. As a matter of fact, code, encoding and decoding can apply to both verbal or

nonverbal message production and reception, for instance, to a gesture. Code is a social convention and needs to be shared by different people in order for adequate communication to take place. In the words of Jakobson, code needs to be “fully, or at least partially, common to the addresser and addressee (or, in other words, to the encoder and decoder of the message)” (Jakobson, 1960, p. 353). The social aspect of code was emphasized by Basil Bernstein, who argued that

Different social structures may generate different speech systems or linguistic codes. The latter entail for the individual specific principles of choice which regulate the selections he makes from the totality of options represented by a given language. The principles of choice originally elicit, progressively strengthen, and finally stabilize the planning procedures an individual uses in the preparation of his speech and guide his orientation to the speech of others (Bernstein, 1964, p. 56).

With certain reservations the hybrid of party-state-science and the increasing control over science during 1930s can be regarded as the “external” reality of doing science in the Soviet Union. If so, then, doublespeak, the special code designed to simultaneously render the message to the scientific community and to deceive the censor was a reaction to this “external” oppression is the result of scientists’ deliberate invention and intense effort to both pursue science, and to survive, both physically and psychologically (Yasnitsky, 2009). From the perspective of the contemporary standards of Western scientific ethos one is tempted to blame Soviet scholars for hypocrisy and servility. In contrast, I argue that doublespeak was a progressive and efficient invention of the scientific community, an essential mechanism of resistance to oppression, and, paradoxically, an expression of freedom and intellectual independence. Indeed, the oppressive mechanism of Stalinist science



required obedience, loyalty, and stylistic unification, which was largely achieved on a superficial level. Generally, scientific discourse of the Stalinist science is characterized by relatively high proportion of ritualistic expressions and standard patterns of criticism and self-criticism, an abundance of “nomadic quotations” from the classics of Marxism-Leninism and the “founding fathers” of the scientific discipline, and other rhetoric devices (Krementsov, 1997). All these attributes of the texts make them fairly highly stereotypical and predictable. Such predictability characterizes, in terminology of Bernstein, a “restricted code” that is the opposite of a considerably less restrictive and less predictable “elaborated code” in a number of respects:

On a psychological level the codes may be distinguished by the extent to which each facilitates (elaborated code) or inhibits (restricted code) an orientation to symbolize intent in a verbally explicit form. Behaviour processed by these codes will, it is proposed, develop different modes of self-regulation and so different forms of orientation. The codes themselves are functions of a particular form of social relationship or, more generally, qualities of social structures (Bernstein, 1971, p. 125).

Yet, there is notable tension between the considerable predictability of Bolshevik party Newspeak and the high frequency of ritualistic political phraseology in Stalinist scientific discourse and the natural tendency towards elaborated code of uncensored scientific discourse. These two extremes delimit the continuum of scientific discourse of doublespeak. Also, they reflect the dilemma each and every author faced when preparing a piece of scholarly writing for publication. With respect to the “dual addressee” of any scientific texts of the time, the authors had to develop special strategies and techniques that allowed them to produce superficially loyal

texts, yet overcome politically motivated constraints on discourse production. Soviet doublespeak was worked out through a spontaneous, emergent, collaborative process that involved numerous authors and readers who had to master the new “Aesopic language” of Soviet science on the fly. The code of doublespeak for obvious reasons was never codified in any form, and required considerable intellectual effort from the authors and the readers alike. A couple of anecdotal examples that can be found in memoir literature are fairly illustrative of the phenomenon of doublespeak.

The grandson of A.N. Leontiev and well-known contemporary Russian psychologist, Dmitry A. Leontiev, recalled about his grandfather: “He was a person with a great deal of internal control, and he lived in such chronotopes, in such times when it was very important to considerably control what oneself thinks, says, and writes. There is even a legend, according to which, when asked “Aleksi Nikolaevich, why do you write in such a complicated, convoluted manner, using such long phrases that are extremely hard to untangle and comprehend? Why can’t you express yourself in a simpler, more straightforward and clear manner?” He replied: “Long ago life taught me to write so that nobody could deliberately take a quote of mine out of its original context and, use it as evidence against me to send me to jail”... He had a lot to keep silent about, and he perhaps was in fact keeping silent about a lot in his life. He kept silent among the relatives, at home, too...” (D. A. Leontiev, 2003). Another example is from the reminiscences about another Vygotskian and a former member of the Kharkov group, Filipp Bassin, who left Kharkov some time around 1935-6, and lived and worked in Moscow ever since: “In order to fully appreciate the brilliant mastery of F.V. Bassin as a theoretician and polemist one needs to know the epoch, in which he lived and worked, very well. This was the time when new paths in science, and even any diversion from the accepted stereotypes sanctified by the authority of

the classics of Marxism-Leninism had to be framed in accordance with ideologically acceptable phraseology. He was a person of such deep reflexion that he could afford to confront the fiercest Cerberuses of the Soviet censorship, the official and volunteer ones alike in a superficially friendly manner... An extreme ease and elegance, with which he would do this, were amazingly accompanied by his mockingly ironical appearance resembling that of Voltaire” (Savenko, 2006).

As quite a number of authors testified, including those quoted above, there was yet another aspect of doublespeak, namely, its ethical dimension. Playing games with censorship is not always an easy task and requires not only mastery of phraseology and political correctness, but also a great deal of courage and personal honesty. Thus, unlike in newspeak propaganda, risk-taking accompanied the doublespeak of an author attempting to send two opposite messages to the censor and to a professional audience. The distinctive feature of doublespeak is the author’s attempt to operate close to the border between the official discourse and “forbidden discourse”, the discourse of “the enemy”. Whether to choose a relatively secure newspeak jargon of the official propaganda or to attempt to overcome it by writing in doublespeak was always a matter of personal choice. The high price for taking risks and attempting to resist the ideological constraints might be the loss of one’s job, freedom, or even one’s life. Yet, the reward was even bigger: a clear consciousness and inner freedom, perhaps the most valuable possession for a human being.

Considering specific mechanisms of text production, I suggest that several hypothetical discursive strategies and techniques are notable in the discourse produced by the members of Vygotsky Circle in the interwar period. Several of such strategies and techniques are discussed below, although in many ways they remain speculative and await more careful documentation in future work.

### *Referencing strategies*

Dramatic change in the practice of referencing in published texts occurred around 1936 and should be understood as the direct consequence of the Communist Party “paedological decree” of 1936 and, even more notably, the Great Terror of 1936-1938. Whereas the texts published by mid 1930s contained numerous footnote references and a fairly impressive bibliography, later publications, especially those in applied disciplines like defectology, pedagogy, and educational psychology typically avoided any references other than those to the classics of Marxism-Leninism, the “Founding Fathers”, whose works were used as substitute for the lack of appropriate quotations from the writings of Marx, Engels, Lenin or Stalin, or to the authors’ close and direct collagorators, for instance to the studies of graduate students or research assistants. This pattern is observed in most publications of that time. For instance, Leontiev in his article for Great Soviet Encyclopedia entitled *Psikhika* [Psyche] that came out in 1940 (yet it was written before August, 1939 when the volume was submitted for censorship in Glavlit) provides ten references overall that include lengthy quotes from the works of Marx and Engels (two references), Lenin (five references), and Stalin (three references). Curiously, the latter author was quoted by the book of L. Beria, *K voprosu ob istorii bol'shevistskikh organizatsij v Zakavkaz'ja* [To the history of Bolshevik organizations in Trans-Caucasus region] the fifth edition of which came out in Moscow in 1939 (A. N. Leontiev, 1940a). No references other than to the works of the classics of Marxism-Leninism can be found in this Leontiev’s encyclopaedic article about one of the most fundamental concepts of psychology.

Non-referencing became an instrument of survival and of continuing scientific research, but in a demonstratively “atheoretical” fashion. Quite characteristically, the

first edition of multivolume *Great Soviet Encyclopedia* published in 1926-1947 reflected this trend: this edition that meticulously listed all members of its editorial board until 1936 suddenly omitted the names of editors and collaborators in several volumes that came out between the beginning of 1937 and the fall of 1938. An excellent example of non-referencing is presented in the dynamics of publication patterns of L.V. Zankov, one the first students and ardent followers of Vygotsky from the early 1920s. Thus, for instance, his relatively short article of 1930 that came out in a volume that he coedited with Vygotsky and D.I. Azbukin concludes with an impressive list of scientific bibliography of nineteen items and contains numerous references to Vygotsky's works throughout the entire text (Zankov, 1930). In contrast, in his later publications from the second half of 1930s the number of references drops substantially, the structure of published sources also notably changes. For instance, in his article of 1941 Zankov casually, in passing, mentions more than a dozen names of contemporary Western scholars but provides footnote references to only two of their works; his entire book of 1949 contains twenty three references out of which the first six are references to the published works of Marx, Engels, Lenin, Stalin, and Molotov. The name of Vygotsky never appears in either of these publications of his former devoted student (Zankov, 1941, 1949). Yet, quite in contrast with Zankov's publications of 1930-40s, in his paper that came out several decades later, Zankov, a leading Soviet defectologist and educator by that time, discussed scientific legacy of the "prominent Soviet scholar, unsurpassed researcher in the fields of general, child, pedagogical, clinical, and special psychology Lev Semenovich Vygotsky—one of the founders of national defectology", and emphatically pointed out that "*now* we can say that, over several decades, research on anomalous children and pedagogy for anomalous children followed the pathways that were indicated by L. S. Vygotsky"

whose work was foundational for many decades of defectological research and the revolutionary transformation of defectological practice (Zankov, 1971).

### *Quasi-critique*

In his paper on the scientific legacy of P. I. Zinchenko Boris Meshcheryakov discussed Zinchenko's well-known critique of Vygotsky presented in his article published in 1939 and the two possible ways in which this critique can be interpreted today:

The first version (the “defeatist” one) is that the negative criticism was primarily defensive in nature; it proved necessary to publicly repent past “sins” and demonstratively break with past views and associations—which would have been very much in the spirit of the times. The second version (the “self-sacrificing” one) is that the negative criticism was necessary to mask the genuine intention to preserve and defend Vygotsky's main ideas within science (Meshcheryakov, 2008, p. 17).

In his further textual analysis of P. I. Zinchenko (1939) work, Meshcheryakov emphasized the fact that, “in addition to criticism, Zinchenko spelled out the views of Lev Vygotsky and gave them a high scientific rating”. Then, according to Meshcheryakov, “the text has a rather rigid stratigraphy, where thick layers of incoherent criticism alternate with substantive and obviously positively assessed ideas from cultural-historical theory” (Meshcheryakov, 2008, p. 20). Furthermore, Meshcheryakov points out that

this was an extremely risky and, undoubtedly, courageous act. At the same time it must be recognized how ingeniously this act was planned. The censors and several generations of critics of Vygotsky's ideas were presented with a

tempting bait. The criticism played its part brilliantly and deserves our admiration, since no other portion of the article is as boring and obscure (although it did not fail to appeal to Vygotsky's critics). It would seem that the teacher's [i.e., A.N. Leontiev's] wise council is evident here (Mescheryakov, 2008, p. 18).

Although the mechanism of quasi-critique has not been properly studied in the literature to date, I hypothesize that Zinchenko's case is not unique, but rather a fairly common technique of scientific self-expression introduced in the era of Stalinist science and thereafter used on quite a few occasions by numerous authors. Just one example of this technique is the groundbreaking book on unconsciousness written by F. Bassin (Bassin, 1968) who, as already mentioned, superficially presented a seemingly harsh critique of the notion of the unconscious "from the Marxist standpoint", but in fact, by mere fact of this publication and its very detailed discussion of the literature on the unconsciousness, including the writings of Freud, reintroduced the problems of the unconsciousness to the Soviet scientific discourse and initiated a discussion of psychoanalysis that had remained outlawed for several decades (Rotenberg; Savenko, 2005, 2006).

### *Shadowboxing*

A boxing training exercising, in which one person is required to fight an imaginary opponent is called shadowboxing. A similar technique of intellectual shadowboxing was developed in 1930s and introduced in the repertoire of the Soviet scientific doublespeak. An interesting example of this technique can be found in an article by A. N. Leontiev's student, an active member of the Kharkov group, G. D. Lukov published in Ukrainian in 1939 in *Naukovi zapysky Kharkivs'kogo*

*derzhavnogo pedagogichnogo institutu* (literally: Scientific notes of the Kharkov State Pedagogical Institute). According to Lukov's article,

Vygotsky's views [with respect to the role of speech in the child's play] are contradictory. Vygotsky, on the one hand, claimed that "in [the child's] play anything can become anything", yet, on the other hand, on the basis of a series of correct observations, he delimits the will of substitution in the child's play, and looks for functions determining substitution in the function of the physical object. He says: "The similarity between a toy and the object that the toy substitutes for in a play is not important. The most important factor is its functional use, the possibility of performing an indicative gesture with it" (Lukov, 1939, p. 92).

Lukov probably referred to ideas that Vygotsky presented in their most developed form in 1933-4, for instance, during one of his presentations at the Leningrad Herzen Pedagogical Institute. Quite likely, the source was the presentation that was first published in 1966 in Russian in the journal *Voprosy psikhologii* (The issues of psychology). The English translation came out a year later, in *Soviet Psychology*. The text of the stenographic record remained unpublished in 1939, but in all likelihood it was Daniil El'konin from Leningrad who might have acquainted the Kharkovites with the text of Vygotsky's presentation: Elkonin, who actually published the text in 1966, owned the stenographic record and Vygotsky's own handwritten lecture notes in 1939, and was closely collaborating with Lukov on research on child's play in the late 1930s (Elkonin, 1978, 1978/2005). Lukov as well as other Kharkovites must have been familiar with Vygotsky's ideas on play and his actual wording. Therefore, one would expect fairly close rendering of Vygotsky's



ideas from the members of the Kharkov group. Let us see what Vygotsky himself actually said on the issues discussed in Lukov's article. Compare:

Lukov (1939)	Vygotsky (1933/1966)
"in [the child's] play <b>anything can become anything</b> "	Properties of things as such do have some meaning: any stick can be a horse, but, for example, a postcard can never be a horse for a child. Goethe's <b>contention that in play any thing can be anything for a child is incorrect.</b>
"The <b>similarity between a toy and an object</b> that the toy substitutes in a play <b>is not important...</b> "	Of course, for adults who can make conscious use of symbols, a postcard can be a horse. If I want to show the location of something, I can put down a match and say, "This is a horse". And that would be enough. For a child it cannot be a horse. <b>Properties of things are retained</b> , but their meaning is inverted, i.e., the idea becomes the central point. It can be said that in this structure things are moved from a dominating to a subordinate position (Vygotsky, 1933/1977, p. 89) (see also Vygotsky's quote above)
"...The most important factor is its functional use, possibility of performing an indicative gesture with it"	?

Table 4. Lukov's (1939) rendering of Vygotsky's (1933) discourse

Thus, some correspondence with the text of Vygotsky's presentation can be found for the two of Lukov's quotes, but the meaning of these quotes is changed to the opposite! Overall, one possible interpretation of the distortions of Vygotsky's

ideas in these quotations may be that this is an instance of a deliberate corruption of the quoted text, and another doublespeak technique in action. The purpose of this distortion of quotation is fairly clear: having ascribed to Vygotsky the ideas that are exactly opposite to what he actually said, the author is free to criticize the quoted “Vygotsky” using Vygotsky’s own original arguments!

Alternatively, for his critique Lukov might have used the text of Vygotsky’s “The history of the development of higher mental functions” authored by 1931<sup>4</sup>, where Vygotsky seems to have written quite contrary to what he expressed in his oral presentation of 1933, namely, that “for the child, in play any thing can be every thing” (see Vygotsky, 1931/1997, p. 135). First of all I would like to point out that for the reasons that I discuss in chapter 2 of this work under the header of *Research Data*, this quote should be taken, though, with certain caution, like any other posthumous publication and republication of Vygotsky’s works known for their questionable reliability, and special research is needed to verify if this is original Vygotsky’s phrasing or the result of a later edition or addition to the text. However, were it true that this quote turns out a correct representation of an original Vygotsky’s idea, it would present even a more interesting case of a remarkable evolution of Vygotsky’s thought that is so rarely documented and studied by contemporary Vygotskian scholars. As to Lukov, it is not clear whether this manuscript of Vygotsky’s work on the *History of the development of higher mental functions*, the first five chapters of which were first published only in 1960 under the subtitle *From the unpublished works* (Vygotsky, 1960)—unlike the text of his Leningrad presentation on play in 1933 (Vygotsky, 1933/1966, 1933/1967)—was available to Lukov and the Kharkov group scholars. Again, were it true that the Kharkovites were familiar with the two

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<sup>4</sup> I am most thankful to Dr. Matusov for this observation and owe it entirely to his attention to details and familiarity with Vygotsky’s texts.

texts of Vygotsky, rendering quite opposite messages, it is remarkable how Lukov in all likelihood chose as an object of his critique the one that presented the earlier thought of Vygotsky that he himself later denounced!

This is the meaning of shadowboxing in this context: both critique and further development of Vygotsky's ideas become possible at the low cost of using the ideas of just one author for two, radically different purposes. In sum, I believe that Lukov's article, with its critique of the reverse statement ascribed to the criticised author—or, alternatively, borrowed from his earlier works—represents a perfect example of yet another doublespeak technique of *shadowboxing* that worked equally well with well trained in the issues of political correctness, but ignorant with respect to science censors, and with scholars—at least those individuals of the Vygotsky Circle familiar with the context and the phrasing of Vygotsky's writings—who (unlike the censors) could recognize the trick and decode the real meaning of the author's message.

#### *References vs. overviews: the roles of laymen and spokesmen*

Often a completed study was available only to a very limited number of the members of the scientific “inner circle”. Sometimes such studies were published much later, but, importantly, the content of the actual empirical studies conducted by the lower-rank scientists was presented in a follow-up overview article written by a *spokesman*, most often a head of the department or a director of the institute. This way the charisma of the scientific and administrative leader was passed on to socially inferior scholars, the *laymen*. In turn, when publications of the *laymen* took place they almost invariably expressed their gratitude to the spokesman who was typically referred to as the supervisor of the study. The reference to a supervisor was normally provided in the first footnote on the first page of the published work, for instance, like

in a series of publications of the members of the Kharkov group in their publications of 1939-41 in *Naukovi zapysky Kharkivs'kogo derzhavnogo pedagogichnogo instytutu* [Scientific notes of the Kharkiv State Pedagogical Institute] in which their authors invariably indicated that “the work was completed under the supervision of A.N. Leontiev” in the very first footnote (Asnin, 1939; Ginevskaya, 1941; Khomenko, 1939; Lukov, 1939; Mistyuk, 1941; Zaporozhets, 1939a; Zaporozhets & Lukov, 1941). The same general pattern is observed in a series of publications of defectologists who indicate that their studies were conducted under the supervision of Zankov and Solov'ev (Zankov & Danyushevskii, 1940), or in the publications of Leningrad developmental psychologists who worked under the supervision of S.L. Rubinshtein and published their studies in the *Scientific Notes of Herzen Leningrad State Pedagogical Institute*.

The strategies of publishing and not-publishing as well as referencing and not-referencing were also instrumental in establishing the hierarchical system of client-patron system of Stalinist science. In most simplified form, this hierarchy comprised three levels: 1) the lower-rank researchers, 2) the leaders and chief spokesmen of the science and its negotiators with the authorities, and 3) the party bosses and key decision makers. For psychology before the war these leaders were such scholars as S.L. Rubinstein, K.N. Kornilov, A.N. Leontiev, A.R. Luria, B.M. Teplov, A.A. Smirnov, V.N. Myasishchev, G.S. Kostyuk, and D.N. Uznadze. For the scholars of the Vygotsky Circle, the interrelations between the lower-rank researchers and the spokesmen of the field can be illustrated by a number of publications of the time in which a spokesperson in his or her publication presented an overview of research conducted by a group of his or her subordinates. Quite often such publication would be the only presentation of these studies and such second-hand accounts are so far the

only source of information about the studies conducted in these research groups (A. N. Leontiev, 1947a, 1947b, 1947/1978; Vygotsky, 1930, 1934a; Zaporozhets, 1941a).

Historians of science in the Soviet Union of this period must often take into account the “retroactive impact” of early unpublished studies on the later works. The 70-year history surrounding the publication of the manuscript reportedly written by Bozhovich in 1935 (Bozhovich, 1935) is a perfect illustration of this “retroactive impact” and of the immense complexity in reconstructing the intellectual history of this period. To the best of my knowledge, the first reference to Bozhovich’s study can be found in the classic monograph published in 1964 (Zaporozhets & El’konin, 1964) and translated into English in 1971 (Zaporozhets & El’konin, 1964/1971).

Bozhovich’s work was also featured in Chapter 5 of the *Development of thinking* (Zaporozhets, Zinchenko, & El’konin, 1964, 1964/1971), where it is enigmatically referred to as a study done under the supervision of Vygotsky (p. 207); a few pages later it is mentioned in the context of work completed under the guidance of Leontiev (p. 212), and yet three pages later (p. 215) Vygotsky’s supervision is mentioned again (respectively, pp. 201, 217, and 220 of the English edition of 1971).

Characteristically, this work of Bozhovich (actually written in 1935 and first presented to the general reader almost 30 years later) was an important contribution to Vygotskian developmental psychology. It was clearly known to the inner circle of Vygotskian researchers—and most likely to Vygotsky himself—and definitely affected later studies in this tradition. It was finally published in Russian in the journal *Cultural-historical psychology* more than 70 years after it was written (Bozhovich, 1935/2006a, 1935/2006b, 1935/2006c).

Countless references to unpublished and therefore unknown empirical works of lower-rank researchers constitute a significant component of the discursive practice

of Soviet psychology and are one of the reasons why traditional Soviet psychology is nowadays often perceived as fairly unempirical or philosophically oriented discipline, especially in the West (see, e.g., Reese, 1999). This perception of Soviet psychology is the rationale behind quite a number of relatively recent replication studies done in the footsteps of the rare adequately presented empirical psychological studies in Vygotskian tradition (Towsey & Macdonald, 2009; Yasnitsky, Falenchuk, & Ferrari, 2008; Yasnitsky, Falenchuk, Mazhirina, & Ivanova, 2008). The psychological reasons for not-publishing in the Soviet Union were very diverse, ranging from the author's inability to persuade decision-makers in publishing industry to the author's own unwillingness to submit their work to the institutions of state censorship. But, as we have seen, by the end of the 1930s Vygotskians prepared a solid foundation for their institutionalization within the social hierarchy of the Stalinist science, and essentially followed the rituals of "discipline-building" in science.

***Contact/Orientation (Set, Einstellung) of the reader***

The last element of a communicative act is Contact, which according to Jakobson (1960), involves both physical and psychological connection between the author (addresser) and the reader (addressee). The role and the weight of these two components—physical and psychological—is clearly very different. Indeed, one establishes "contact" with a text as soon as one has the publication or manuscript in hand (for written discourse) or attends a conference presentation or a research meeting (for oral discourse). Psychological contact is considerably more problematic. In his discussion of message and its function in the structure of a communicative act, Jakobson operates on the notion of psychological set (*Einstellung*), which denotes both the addresser's and addressee's specific orientation towards message and the

ways of its decoding. I propose that the notion of set, the reader's or listener's orientation towards specific semiotic system, and the implied rules of encoding it entails, is instrumental in adequately understanding of Soviet scientific doublespeak. Clearly, doublespeak poses considerable problems of "retranslation", and much research needs to be done in conjunction by linguists, psychologists, and historians of culture and science, in order to decode The Code. Yet, it is definitely worth the effort.

**Epilogue.** Toward a final synthesis:

Mind, brain and education in Vygotsky's system

Scientific investigation is never done: This is one of the most fundamental axioms of the craft. Even when a piece of scholarly work is published and the author has it as a material entity—an article, a thesis, or a book—it is still incomplete and imperfect. “Have no fear of perfection,” instructed Salvador Dali, “you'll never reach it”. Yet, there is a point when one needs to stop—at least for a little while—in order to produce a coherent piece of writing. This is perhaps another of the most fundamental axioms.

I stop here. An epilogue is a good place to reflect on what has been achieved, yet, it is perhaps an even better place to reflect on what has *not* been achieved and still needs to be done in the future.

What has been achieved? In this thesis I present the first systematic overview of the scientific practices of the entire Vygotsky Circle during the decade of the 1930s. In order to do so, I analysed the history of the notion “the school of Vygotsky-Leontiev-Luria” and demonstrated how this social construct developed from the 1930s until today (Chapter 1). I then explored the socio-cultural background of the Soviet human and psychoneurological sciences, and demonstrated how the formation of Stalinist Science affected Soviet psychology as a scientific discipline (Chapter 3). I operationalized the notion of “scientific practice” as the unity of a) social and interpersonal relations, b) practices of empirical scientific research, and c) discursive practices of the Soviet science (Chapter 2).

Three aspects of scientific practices of the scholars of Vygotsky Circle were discussed in Chapters 4 through 6. Chapter 4, traces the evolution of the larger Vygotsky Circle and studies the dynamics of its transformation from 1924 until the



beginning of Great Patriotic War in 1941. Chapter 5 presents an overview of virtually all experimental studies in Vygotskian tradition. Clearly, the scope of presentation was my priority, which might have affected the depth of analysis of the actual studies. Everything has its limits, and this is one of the limitations of the present research. Finally, Chapter 6, discusses the peculiarities of Soviet scientific discourse. A semiotic approach seems to provide researchers with the necessary theoretical framework and conceptual apparatus. In my study, I used ideas borrowed from the work of the great Russian-American linguist Roman Jakobson. These are clearly first steps in a larger project, and my somewhat superficial approach to the topic may be explained by the magnitude and the enormous complexity of the interdisciplinary material that I explored in this work of such ambition and breadth.

One of the main achievements of this research project is that it provided a perspective on how Vygotsky's ideas can be used in research on the history of science, specifically, on the history of cultural-historical psychology of the Vygotsky Circle. Such Vygotskian research on the history of science hinges on several key principles that include: historicism and developmental bias, investigation of intra- and inter-group dynamics of a scientific circle as a unit of analysis, and consideration of interconnectedness of scientific theories and abstract concepts with social practices of ideas generation, presentation and application. This research framework looks promising for investigating other collaborative research projects such as the group surrounding Jean Piaget in his study of genetic epistemology, the Berlin Circle of Kurt Lewin, the scholars of Bakhtin Circle, the work of Moscow, Prague, Copenhagen, and New York Linguistic Circles, and the work of the anonymous group of French mathematicians that published under the name of Nicolas Bourbaki.

What still needs to be done? Certainly, the next logical step is a deeper and more thorough historical research into the vicissitudes of Vygotsky's life and his scientific proposals, and we will be able to considerably revise what is known about the group of Vygotskian scholars from the 1930s onwards when the main archives open for the general public and become available to researchers. Unfortunately, the archives of key protagonists, including those of Vygotsky, Leontiev, and Luria are still kept largely in the private apartments of their descendants, who often seem largely unwilling to collaborate with historians of science. I truly hope this will change in the very near future. Another major avenue for future research is the merger of socio-historical investigation of scientific practices that has been undertaken in this study with the traditional intellectual history, the history of ideas, and the more philosophically oriented research on the theory and methodology of psychology. This future work is particularly important given the current interest in integrating the science of mind, brain, and education, all of which can be found in a highly integrated form in the work of Vygotsky and Luria and their students. Finally, I believe the basic attempt at linguistic and semiotic analysis that I presented in the chapter on discursive practices of Soviet science might be elaborated, refined or critiqued by the specialists in these disciplines willing to continue systematic exploration of discursive practice in science in particular social settings in order to develop a better understanding of the complexities of cultural processes of Soviet science.

I hope this study is just a first step in a long sequence of future research into the extremely complex phenomena of scientific circles, discursive practices and doublespeak of Stalinist science, still little understood in our contemporary scholarship. Such practices and groups affect our adequate understanding and therefore our ability of productively investigate, apply, and further develop one of the

most original and promising intellectual movements of the last century—Vygotsky's theory of the cultural development of higher mental functions.

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