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Günter Mey and Katja Mruck

**A traveller in psychology**  
An interview with Jaan Valsiner

*Günter Mey*, Technische Universität Berlin, Psychologie im Institut für Sozialwissenschaften - HAD 40,  
Hardenbergstr. 4-5, 10623 Berlin; e-mail: Mey@zrzsp3.gp.tu-berlin.de

*Katja Mruck*, Institut für Klinische Psychologie, Psychologische Diagnostik und Gemeindepsychologie  
WE 09, Freie Universität Berlin, Habelschwerdter Allee 45, 14195 Berlin; e-mail: Mruck@zedat.fu-berlin.de

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## Forschungsbericht aus der Abteilung Psychologie im Institut für Sozialwissenschaften

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### **Redaktion**

Dr. Günter Mey

### **Redaktionsadresse**

Technische Universität Berlin  
Psychologie im Institut für Sozialwissenschaften  
Skr. HAD 40  
Hardenbergstraße 4-5, D-10623 Berlin  
Tel: 030 / 314-25286, -24770  
Fax: 030 / 314-79474  
e-mail: Mey@gp.tu-berlin.de

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### Summary

In this interview, which was mainly conducted by e-mail, the emphasis is placed on Jaan Valsiner's projects (Part 1), and some of his experiences with various psychologies in different countries (Soviet Union, Germany, Brazil) are presented (Part 2). This is followed by more generalized reflections regarding "different countries, different psychologies, different psychologists" (Part 3), and a short digression about the concerns of cultural-psychology (Part 4). Part five contains Jaan Valsiner's views about the relationship of "mainstream" vs. "non-mainstream", "old" vs. "new" psychology, also dealing with practices of academic-psychological publications and teaching (Part 6). The interview closes with a short look at "future and development".

### Zusammenfassung

In dem Interview, das in wesentlichen Teilen per e-mail geführt wurde, werden zunächst die Arbeitsschwerpunkte von Jaan Valsiner (Teil 1) und einige seiner Erfahrungen mit (unterschiedlichen) Psychologien in verschiedenen Ländern (Sowjetunion, USA, Deutschland, Brasilien) vorgestellt (Teil 2). Es folgen dazu verallgemeinernde Überlegungen (Teil 3) und ein kurzer Exkurs zu den Anliegen einer kulturpsychologischen Forschung (Teil 4). Teil 5 befaßt sich mit Jaan Valsiners Vorstellungen von dem Verhältnis "mainstream" vs. "nicht-mainstream", "alter" vs. "neuer" Psychologie, die dann an Fragen zur akademisch-psychologischen Veröffentlichungspraxis und Lehre vertieft werden (Teil 6). Das Interview schließt mit einem kurzen Ausblick ("Future and development").

*key words:* cultural psychology, development, methods, methodology, American psychology, European psychology, German psychology; e-mail-interview

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In order for intelligence to work, it must be motivated by an affective strength. No one will ever solve a problem if he is not interested in it. The impulse for everything lies in the interest, in affective motivation.

(Jean Piaget 1977)

*Dedicated to Dietmar Görtliz on his 60th birthday.*

## 1 Introduction

Jaan Valsiner was born in 1951 in Tallinn, Estonia. He studied English at the *Teacher's College* in Tallinn, and then psychology at *Tartu University* from 1971 to 1976. Upon graduation, he worked there until 1980—first as a junior and then as a senior research scientist and after finishing his Sc.Ph.D-thesis (1979) as an assistant professor. Jaan Valsiner emigrated in 1980. After spending six months as a visiting professor at the *Justus-Liebig-Universität Gießen*, Germany, Valsiner moved to the U.S.A. First, he worked at the *Institute of Child Development* at the *University of Minnesota*. Then, he was Assistant Professor at the *University of North Carolina at Chapel Hill* from 1981 until 1988, Associate Professor there until 1993 and Professor from 1993. In August 1997, he began working at the *Francis Hiatt School of Psychology* at *Clark University* in Worcester, Massachusetts.

Since 1982, Jaan Valsiner has spent time at various institutes as a visiting professor: at the *Institute of Psychology of the Italian Research Council* in Rome (1982, 1983, 1984, 1985; a total of ten months); at the *Rijksuniversiteit Leiden* in the Netherlands (1988 for four and 1995 for two months), at the *Max-Planck-Institut für psychologische Forschung* in Munich, Germany (for six months), at the *University of Melbourne*, Australia (1990 for three months), in Brazil at the *Universidade de Brasilia* (1991, since 1995 about three months a year) and finally for one year at the *Institut für Psychologie* at the *Technische Universität Berlin*, Germany (1995-1996).

Jaan Valsiner's activities are so varied that it makes it difficult to describe the main emphasis of his research: during his years in Estonia, he worked predominantly on questions regarding developmental psychology in early childhood and on questions about ecological psychology. Most of his work dating from this time is written in Russian or Estonian. Since moving to the the U.S.A., he has continued to work in these research fields, but, in addition, he is concerned with methodological problems. Furthermore, he takes an avid interest in questions pertaining to cultural and historical psychology.

During the last years, he participated—very often in co-operation with other scientists—in various projects and organized numerous conferences and symposia.<sup>1</sup> Beyond that, Jaan Valsiner is a member of the editorial board of several journals<sup>2</sup> and has been editor of the journal "Culture and Psychology" since 1995.

He has contributed many articles to books and journals; beside these publications we would like to cite here only some books: First, his very highly acclaimed book—a revised second edition of which was published last year—"Culture and the development of chil-

dren's action" (1987; Chichester: Wiley, 2nd.ed. New York 1997). Equally mentionable are "The individual subject and scientific psychology" (New York: Plenum Press, 1986) and the three volumes of "Child development within culturally structured environments," which he edited: Volume 1: "Parental cognition and adult-child Interaction" (1988); Volume 2: "Social construction and environmental guidance of development" (1988); Volume 3: "Comparative-cultural and constructivist perspectives" (1995). (These were all published by the Ablex Publishing Corporation; Norwood, New Jersey.). Then, "Developmental psychology in the Soviet Union" (1988; Brighton, U.K.: Harvester), the work done in collaboration with Rene Van der Veer: "Understanding Vygotsky; a quest for synthesis" (1991, Oxford: Basil Blackwell, which was also published in Portuguese in 1996), and finally "The Vygotsky Reader" (1994, Oxford: Basil Blackwell).

In 1995, Jaan Valsiner received the "Fullbright Serial Senior Lecturing Award" in Brazil and the "Forschungspreis für Geisteswissenschaftler" from the Alexander-von-Humboldt Foundation.

*Background: From a first meeting to the interview with Jaan Valsiner (Günter Mey)*

I met Jaan Valsiner in Germany in the summer of 1991. Along with Dietmar Görlitz and Hans Joachim Harloff, he was organizing an international symposium titled "City as a Frame of Children's Development" of which I was to be the conference assistant. During the time the symposium was held as well as during the preparation of the results I saw him regularly although never for very long. A more intensive exchange of ideas took place during his one year as Visiting Professor at the Technische Universität Berlin in 1995/1996.

The ensuing meetings and discussions, chiefly concerning the editing of our joint book as well as many short e-mails, proved a lively exchange of ideas. At the beginning, the questions were predominately of an editorial nature. However, little by little, psychological notes crept in and finally the idea was born to turn these small excursions into an interview and a theme in itself.

After Jaan Valsiner left the Technische Universität Berlin, I had the idea about conducting an interview. So, I asked him—basically too pointless to even mention: by e-mail—,

whether or not he would be interested in having Katja Mruck and myself interview him about his viewpoints regarding psychology. Some possible topics were the relationship between "old" and "new" psychology, "different cultures - different psychologies", his methodological understanding, and so on. I also attempted to make it clear to him that a biographical aspect would be of great interest to us. At first, Jaan Valsiner hesitated—a typical reaction of his—which I always felt to be somewhat coquettish "I've nothing to say." After some back and forth our plans became more concrete in December 1996. He was in Berlin for one week to organize the Ernst-Boesch-Symposium. We invited him to dinner and he was accompanied by Ingrid Josephs. The conversation lasted more than five hours, but after awhile we "slipped" from our planned interview strategy into a more

general discussion about psychology, which proved very interesting but did not quite correspond to our idea of conducting an interview with Jaan Valsiner. When I met him at the institute the next day, he was of the opinion—as were we—that it had undoubtedly been a pleasant evening but perhaps not as productive as had been hoped. He offered to continue the interview, but (because of his limited time) to do it per e-mail. At the beginning we were not quite sure whether or not to agree to this—a personal interview via the computer screen—but, we finally agreed because it appeared to be the most suitable form: if I think about him sitting in his office at the institute in Dove Street—which he lovingly refers to as "my little office near the faxroom"—in front of the computer and in contact to his many research colleagues by e-mail; a situation which first conjured up the idea of a "traveller in psychology" in my mind, someone who is (almost) always available and always interested and involved in questions of psychological research. In addition to organizational reasons, we felt this interview form to be an appropriate solution because it corresponded to our "step by step" development of dialogue. We decided to begin with the interview in January and to ask the questions as they arose. For the final sequence (starting in August 1997), we asked several colleagues whether they would be interested to become involved with the interview; we included questions from Alfred Lang (Universität Bern, Switzerland), Rolf Oerter (Ludwig-Maximilians-Universität, Munich, Germany) and Carl Ratner (Humboldt State University, California, U.S.A.)

Our discussions serving as a background and enriched by cross-references, an interview text emerged which shows Jaan Valsiner's different research themes and research fields. It was not only our intention to discover something about the different psychological directions due to Jaan Valsiner's various research contacts and his constant travelling about the world but perhaps, by going off the path or heading down dead-end-roads, to discover some hints how a psychology could be paved by using his experiences. Beyond exploring our branch of study, we also hope, within the framework of an interview, to show the relationship between personal and theoretical explanations. We would like to reclaim for academic psychology that which long has been assumed for conceptualizations of therapists: that the models can be seen as an expression of the author's personality. In this sense, the purpose of the interview is, besides all the other themes it touches upon, to let Jaan

Valsiner, the person become transparent; the person who is hidden behind the theory and who is to be found in it and it intends to show a personal document about "The Person and His Work."

## 2 The interview with Jaan Valsiner

### *Part 1: Research activities*

*Let us start with an a-typic narrative question: If you have only little space—how do you explain your person and your approach to someone, who never heard about you or about your work, who never read texts written by you?*

*Jaan Valsiner:* I am a sociogenetic developmental personologist.

*A nice paraphrase. Now let's try to get some of the nuances of this characteristic of yours more understandable by telling something about your actual projects—research, books, and so on. What are the main themes and research-fields you are occupied with, and what are the intentions and the goals of these projects?*

*Jaan Valsiner:* There are many projects going on in parallel, with my involvement in each being variable, and also very variable are specific topics. However, all the projects—in some general ways—are oriented towards the goal of *understanding how human beings are interdependent with their socio-culturally organized environments*, and how development is possible in this process of interdependence. Roughly, these different projects can be divided into the following categories:

First over the last years there are some projects on child and adolescent development with the focus on the ways, in which constraints on acting, thinking and feeling guide ontogeny<sup>3</sup>. Beside my earlier works about parenting<sup>4</sup> and "injury prevention tactics"<sup>5</sup> and my actual interest in the semiotic regulation of the self and "The Guided Mind"<sup>6</sup> you can see it in the topic of children in macroenvironments, which started back in Estland, was largely dormant in the 1980s, and re-emerged in my enthusiasm about becoming involved in the "Herten"-activities.

A second group of projects uses psychologists as research target, e.g. psychologists' reasoning on the basis of empirical data is one of these. This got its basis from a dispute with Jan Smedslund,<sup>7</sup> and was central for the book "The individual subject and scientific psychology"<sup>8</sup> on the role of individual subjects in psychological research. By the present time this topic arrives at the question of co-constructive nature of any method.<sup>9</sup> A rather important point is to discuss developmental psychology in contrast with its non-developmental counterpart. Here the recurrent question is why is developmental psychology oriented towards studying anything else, but development.

And at last you will find a group of my work, which includes the history of psychology in specific contexts as a target of psychological analysis. One direction is the social construction of history of psychology, which emerged from the study of USSR, but extended in the direction of *how histories are constructed in general*—as I lay it down in "Explorations in socio-cultural studies" together with Angela Rosa.<sup>10</sup> It included an experiment with psychologists—suggestion of the label "Co-constructionism" as an alternative to personal and social versions of constructionism.<sup>11</sup> Another line growing out of my interest in Developmental psychology in Soviet Union is my engagement—together with Rene van der Veer—in the work on Vygotsky<sup>12</sup> as well as some "side stories".<sup>13</sup> The work on Vygotsky has become appreciated,<sup>14</sup> yet for what I see to be mostly for the wrong

reason, a kind of

Vygotsky-fashion in different countries in the 1980s. A more internationally "balanced" project with Rene van der Veer is under way now: In our book "The social mind"<sup>15</sup> we cover the intellectual history of the *idea* that human personality is a social entity, from about 1880s to 1930s, with some linkages to the present time. I am playing with the idea of writing "Developmental Psychology in America"—a companion analysis of why developmental ideas have failed to develop in North America's psychology over the 20th century. But this project is still in its very embryonic state.

All in all, you see that I have never focussed on "doing research in area X," but rather have addressed problems that at the given time were intellectually interesting and challenging. There are topics I have tried out and purposefully abandoned—intelligence testing, testing in general, statistical inference use in psychology, sample-to-population generalization—, which can be proven—theoretically—to be "dead-end streets" for developmental knowledge construction. Yet it is not interesting to me to "deconstruct" these perspectives—which I have found unfruitful—but I am rather trying to build alternatives, as much as is realistic. For that, I read actively across disciplines, anthropology, sociology, history, etc., including—accessible—brands of formal modelling efforts. I basically do not read the "normal science type" literature *inside* psychology: when I see that an author in a paper has *not* provided description of the *phenomena* together with the *data* or if data are reported in terms of p-values or merely \*\*\*-significance markers, I would not expect to rely on these findings. As a result, I find rather little *basic knowledge* in the "accumulated literature" in psychology, and find the careful description of phenomena of earlier times' psychologists most refreshing. For example, Arnold Gesell's films on child development<sup>16</sup> are more informative than the easily assembled and underutilized videotapes of infants in very different field contexts.

*If a particular area is interesting, but proves to be too complex for a fruitful understanding, I move away from it—hopefully temporarily. The focus on children's "accident prevention" was of that kind: My colleagues found the topic potentially very "profitable" and encouraged me to get into it, but after revealing the simple theoretical paradox in the issue—accident prevention and accident non-happening are indistinguishable, so it is impossible to show any role in the prevention for sure—I have abandoned activities in that area.*

*You said, you never were interested in "doing research in area x," you never limited yourself to only one research-theme . . .*

*Jaan Valsiner: I do not "do research in area X," because the designation of such areas is arbitrary, and often driven by empiricistic concerns. Well, I can be perceived to have done research "on children's mealtimes"<sup>17</sup>, but the reason I did that had nothing to do with mealtimes per se, it would be rather perverse activity to look at how other people eat, not eating oneself . . .*

*What would happen to Jaan Valsiner if he would do research in one area, if he worked upon only one theme and if he stayed in one country?*

*Jaan Valsiner: If I were to "do research in area X," I would probably be satisfied by accumulation of data about X, and would not bother to ask difficult questions . . . It would*

be a *via regia* for success before retirement . . . I would be quite bored, intellectually, in such predicament, but of course would not show this to anybody . . . instead would write review articles about the grand successes in the area of "research on X."

*It is a little bit suprising that nowadays one scientist is interested in so many fields. Did you in earlier times also work on different fields or did you in the beginning of your work engage only in one theme or in a few themes?*

*Jaan Valsiner:* Well, in the *very* beginning I surely was interested in one topic; which was the nature of human intelligence: As a student at Teacher's College I once saw announcement that IQ can be tested, went for it . . . got no news about my IQ other than that it is a mystical concept . . . tried to read about it, and there I was—eager to study psychology . . . This was in 1969/70 in Tallinn, in 1971 I left the Teacher's College and entered Tartu University to study psychology.

*Your interest in psychology starts with intelligence testing? What results did you get?*

*Jaan Valsiner:* A very basic result—"intelligence testing" is not a scientific practice, despite all appropriate and consensually approved "as if scientific" garb. I still use intelligence testing as an example of unintelligent inventions in psychology, as "straw man."

*How did you use this new knowledge?*

*Jaan Valsiner:* I soon understood that all the limits that are set upon demarcating boundaries of sciences, and of different approaches within science, are merely organizational, and what matters is integration of *ideas*—and not of categories of sciences, or sub-sciences. Already during my student years I cared little about boundaries between sub-areas of psychology. As I could not decide on one of the three "available" areas in Tartu University for specialization to choose—"industrial", "social", "clinical"; the choice had to be made after two years of study"—I worked out for myself with support from the Department a "special study program" in "general psychology" with a special focus on communication and psycholinguistics, which created a motivation to combine the more interesting parts of knowledge from different areas, and not be "committed" to any. Needless to add that this combination worked through my fully independent study—I never visited a single lecture on psychology during my third, fourth, and fifth year as student, and had to take exams (still) with the professors being surprised that I turned up. Instead, I was reading into contemporary (mostly American) psychology in selected interest areas, especially communication: mostly nonverbal; environmental psychology; mother-infant interaction; some on development of visual perception; also on recognition of facial expressions, emotions on the human face. Meanwhile I was organizing research interests and activities of my fellow students, because I was the "de facto" advisor for students in my own cohort as to their graduate thesis . . . in parallel with doing my own—a review of American studies on mother-infant interaction. Even now some of my fellow students "accuse" me for spoiling their lives, by insisting to stay in psychology and do research . . .

So, from very early on I had no respect for accepted organizational structure of psychology, and broke it in my ways and under the local circumstances: Tartu University Psychology Department was a *very free* place in the 1970s for young students to develop

. . . Since that time and travelling all over the world, I have not encountered a comparable place, referring to freedom of exploration for young students . . . But we were poor, we had no technology, and we had to rely on own resources. There were no hopes for jobs to be guaranteed by the Soviet system (there were no "given" jobs *as* psychologist in the planned economy of USSR at the time)—*yet* we were eager to build our own kind of psychology, and in our own ways . . . And so we were a bit of self-selected "hostages" to American psychology, rather than European traditions . . . ; yet we knew the European ones thanks to good basic education.

Interestingly, the "ideological overdetermination" of the Soviet system was having its "blind spot" in the context of Estland in the 1970s, and in any case was limited to overcontrolling *political*, not scientific actions by students in the University. Of course, the line between political/scientific was not always simple to draw in reality—especially in the "social sciences"—and there were incidents of political persecution of some sociologists, on locally invented grounds. And I ran into the danger of such persecution, which led me to decide to leave the USSR if I only could, which happened, luckily, in 1980. But this feeling developed more or less in the second half of the 1970s (1975-1977), my early student years were not filled with any worries that my efforts in psychology somehow might be suppressed by the "Soviet system".

*What was your concrete reason to leave respectively to decide to leave the USSR?*

*Jaan Valsiner:* The decision by the vice-chancellor of research of Tartu University in 1977, not to allow me to send a review paper to America for purposes of publication. He considered me—at tender age of 26—"too young to be competent to publish internationally." I disagreed—in myself—, and three years later did not lose the opportunity to leave the Soviet Union. I do not tolerate administrators' arrogance towards young researchers, especially when it is based on power.

## ***Part 2: Visiting different psychologies***

*Well, you succeeded in opposing this arrogance, and you left USSR. It would be very interesting, if you specify some of your following experiences on your way through the countries and through psychology, too!*

*Jaan Valsiner:* I came to Giessen in 1980 and was told that some psychology students had opposed my being invited with reasons like "if he is a refugee from Soviet Union, he would teach us the wrong kind of Soviet psychology." The pro-soviet feelings of West-German students in 1980 were quite surprising for me—into psychology it carried over in the form of uncritical acceptance of ideas emanating from Soviet psychology. Such acceptance (by some) was not different from the acceptance of American ideas (by others). They differentiate their semi-rigid worlds around different "fixed anchor points"—some find it in Vygotsky, others in analysis of variance, still others in "humanistic psychology", etc. So my reaction to both kinds of acceptances indeed made me express ideas that were oriented towards reflexivity upon these acceptances. I moved on to America—to find a local acceptance of pre-set ideas, e.g. those of "data analysis" to be done in the "right way" in American psychology . . . For me, all these are the same phenomena—the crucial contrast

is between people who follow given ideas, and those who play with ideas, practices, etc., trying to construct new understanding. The latter I could consider to be science, the former only is a part of the social-institutional world. From that standpoint, the often told story of "progress" in American psychology—the measurement of IQ of army recruits during World War I—can be seen as a social sign of ending of a fruitful search period in psychology in the United States which was there at the turn of the centuries. Or, similarly, the emergence of the wide following of Piaget, and canonization of his ideas, would amount to the beginning of the end of a thoughtful research programme. Likewise, psychology in Soviet Union stopped its creative directions once it became overtaken by "the right" general ideas of a version of marxist philosophy.

*As we will return to your notion of practising science afterwards, now we are interested in hearing a little bit more about your concrete experiences here in Germany. During our "real life"-interview in december, you told something about the first time in Giessen and—I hope, I remember correctly—high technical surroundings, but nothing to work with it, so that there was only a little output. What concrete experiences did you make when you stayed here in the beginning of the 1980s? Were conflicts with some left-wing students still going on during your first German stay?*

*Jaan Valsiner:* . . . The first time in Giessen from April to October in 1980 was my first encounter with Germany, which I appreciated very much. As it was a temporary stay—just to help me to find a place to settle down—it involved some very transitory research efforts, for which I will forever be grateful to Professor Klaus Scherer who made it all possible. He organized a visiting position for me—but the student representatives on the appropriate committee were against inviting me, with the just mentioned remarkable argument. Fortunately, these student representatives were in the minority in the voting committee, and I was invited anyway—a good lesson of democracy—in order to get things done one has to neutralize political absurdities! I was surprised and filled with irony—to encounter such strong left-wing tendencies in West Germany was beyond my expectations. And of course I never stopped being surprised at how many students were distributing communist leaflets in front of the Mensa . . . and I thought to myself "what on earth do these young people *know* about communist world, and its intricacies." Well, they did not know, nor did they want to know . . . provincialism can easily be hidden behind nice slogans of political activism . . . However, the other side of the intellectual encounters in Giessen—with colleagues and the three students who *did* take my (obviously "not the right kind") Soviet Psychology course—was very pleasant and productive. These were people who wanted to learn and who were not narrowly focussed on ideological issues. I must admit that I have always disliked rigid ideological claims, which are pushed upon others—whether these are communist, capitalist, or any kind. The first stay in Germany opened my eyes to see beyond the easy "East" (= totalitarian societies) and "West" (= free societies) contrast: ideals that in Estland under Soviet rule were not taken seriously (even if played out in public games), were taken with religious fervent by the left-wing German students. So where was the "free" vs. "totalitarian" contrast, I failed to see that in the minds of people in 1980 . . .

*After this short time in Germany, you went to the United States, and you met another kind of beliefs, I remember. What were your first experiences in this country?*

*Jaan Valsiner:* Before my arrival in the United States, a Swedish friend had told me that this is a "very religious country . . . you have to believe in something in this country." I did not believe it then . . . but as I did arrive and got to know the life in the United States, I realized that he was exactly right. One needs to believe *in* some *existing* belief system (including a belief in scepticistic belief system), otherwise one is viewed to be "outside" of the accepted scope of mutually co-existing groups. This of course has historical roots—the United States is a country where religious freedom was granted to *different* groups escaping from persecutions in other parts of the world, and who in North America could establish *parallel* locations of co-existence. So what is tolerated is *belonging* to another group, but that does not mean acceptance of the belief system of the other, rather—the acceptance is that of the *right* of the other to co-exist with mine. This organization of society was noted by different commentators—Alexis de Tocqueville in the 19th century, and George Herbert Mead in this. It shows the social dynamic of how American psychology functions.

So, arrival in the United States made me to see all that. During a job interview in early 1981, I was "grilled" by a well-meaning faculty member for half-hour on the topic whether I am a "social developmental psychologist" or "cognitive developmental psychologist." Even then I had the stamina to claim that I am *both*, trying to unite the two . . . she smiled saying "o.k., yes, I understand, *but* are you *really* this one or that one." I did not get the job, but got another one—in North Carolina—where I stayed for 16 years from 1981 till 1997.

The very positive experience of America was of course the tolerance of academic autonomy. I took it for granted, because in Estland I had experienced this in full: after first two years of psychology studies, I got a "special study program" which freed me from sitting in lectures, and I could concentrate on self-study of psychology. So it was surprising to me that such autonomy was surprising to either other visitors to the United States—especially younger psychologists from Germany, so well groomed by their powerful professors, and by Americans who seemed nervous about evaluative feedback. I always get into trouble with calling my own self names that are not usual in United States—once, facing my computer screen and as I made a stupid mistake, I called out aloud "how stupid I am!" The American graduate student who was next to me felt the need to provide some psychotherapy on me—that I am not really stupid at all, but that it is not serious, etc. And I wondered—why such care? I was perfectly ready to call myself stupid when there was a reason for it, *without* that hindering my basic self-esteem. But this was and is an unusual way to relate to oneself in the United States.

*But you didn't stay only in Europe or in North Amerika, later you visited for example Brazil, too.*

*Jaan Valsiner:* To set the record straight—I started visiting Brazil in 1989, and have been back a number of times, three of these including stays of about three months per time. I became fascinated by the conference papers I saw in the program of the 1987 ISSBD-conference<sup>18</sup> in Tokyo, where I myself did not go; wrote for papers, got the papers and letters, and established correspondence. The first face-to-face meetings with the Brazilian psychologists happened in the summer of 1989 in Finland at the Jyvaskyla ISSBD Conference, and the Brazilian academic system—first at the initiative by Maria Lyra from Recife—found ways to invite me to Brazil.

I appreciate the freedom of thought in psychology among my Brazilian colleagues with whom I have developed close academic work and friendships. The eagerness for new approaches is by far greater in Brazil than in Europe or North America, psychology students are generally well prepared, and ready for new work. Of course—and especially as Brazilians themselves would talk of their "third world country" complex—there are many problems, difficult to solve. However, in interacting with psychologists in Brazil, I do not find any "third world" there. Instead, I think that the intellectual productivity—perhaps for the selected group of persons I link with—is very high. I keep going back to this beautiful country, enjoying the interchanges there. I am more eager to give short intensive courses or colloquia in Brazil—getting more interesting feedback than in USA or Germany.

*If you summarize these different experiences and contacts—what is the most important benefit for you visiting psychologies in different countries?*

*Jaana Valsiner:* I appreciate the experience of learning by way of experiencing joint work with colleagues within their life/work worlds. I do not travel as a tourist, I go to do some joint work. One needs that change of positions in practice, in order to make sense of the realities of different psychologies. Such information cannot be obtained even from the best written book.

*As German psychologists we are especially interested in your experiences in Germany, so what are your impressions visiting our country again during the 1990s?*

*Jaana Valsiner:* I think that German psychology is still very potent to return to its role as world leader of the discipline which it was 100 years ago, only if it gets rid of its excessively North American orientation. Presently it is perhaps not in the best state—or, more precisely, it is very uneven. There are very innovative approaches, in parallel with repetition of North American "games of psychological research." Unfortunately, different approaches seem to fight one another with ideological fervor, see the contrast between "mainstream" psychology and "Critical psychology"—aggressive stigmatization by both sides, which of course does not help any development of ideas.

*Mentioning "Critical psychology": What was your greatest benefit staying one year in Berlin?*

*Jaana Valsiner:* The greatest benefit was to live in the middle of Europe, while being involved in multiple projects that interest me in various and quite different ways. The "Children in the city" topic is interesting as it involves a principally undefinable macro-context ("city") and very real human beings (children) who are both autonomous and dependent relative to that context. I have long been interested in the notion of "dependent independence"<sup>19</sup>, so Dietmar Görlitz's project is very unique and scientifically full of promises that ordinary psychological research in laboratories etc. does not entail. The stay in Berlin also gave me a glimpse of the realities of life of German academic psychologists as they are approaching year 2000—such a fashionable cliché used nowadays!—or what that mysterious "European unity" is like when it comes to real-life work in psychology, especially in universities.

I also benefitted from learning more closely what other research groups in Berlin are doing—contacts with Martin Hildebrandt-Nielshon's group in Free University was giving

interesting information about how psychology in Germany moves from its "critical" to an "empirical" form; and the life-span psychology studied at the Max-Planck-Institut für Bildungsforschung became conceptually understandable to me—I could see the core of it, behind the multitude of very many empirical expositions.

*What is the core behind the multitude?*

*Jaan Valsiner:* I am now talking about Paul Baltes' Wisdom Project, rather than about that Max-Planck-Institut as a whole. The core is admirable—to demolish the very unintelligent view that exists in occidental common sense and political ideologies that old-age people are a "waste" for society. Rather, what Baltes seems to do is to restore the ideological stance of traditional societies—where old-aged persons, even if physically frail and indeed with some mental decay—are nevertheless positively valued and politically centrally important persons in local political life. Now, since politicians in any society one year or another understand that they can grow only older (rather than younger), the acceptance of the notion of wisdom of the elderly should have easy appeal to them, and to others in a given "modern" or "post-modern" European or North American society. So, the emphasis on wisdom is very clever—it is appealing to the anxieties of all who are growing older, and it clearly is a research orientation that psychology has overlooked. Now, the difficulty I see in that orientation is its borrowing of the traditional psychology's methodology, and trying to link it with productive concepts of selective optimization, and the like. Standardly accepted methods of psychology are not well-suited for adequately solving the problem of what is the psychology of growing old like. Thus, the Wisdom Project is very productive in terms of producing empirical data, well-accepted in the international system of psychological research, yet not in a position to solve the problem. The issue needs a fresh, rather than a traditional solution.

### ***Part 3: Different countries, different psychologies, different psychologists?***

*Collecting your experiences with different countries and different psychologies—What do you think about similarities and differences in psychology—or more concretely: what are in your opinion the main differences and similarities between Soviet or American or European psychology?*

*Jaan Valsiner:* It is not so simple a task to specify "Soviet", "American", "European", or "Asian", "Antarctican", "Greenlandish" etc. etc. psychologies. As all scientific systems, psychologies in different countries or cultural areas are mutually interdependent, both synchronically (at this time) and historically (diachronically). Thus, present-day "American" psychology may be best exemplified by the working style of some research groups in Germany, while "German" psychology of 1890s was very indicative for some of psychology in the United States. "Soviet psychology" was a curious mis-nomer at the times of the Soviet Union—its roots were profoundly within the Continental-European thought traditions, while its own declarations were often of the kind that presented their few new ideas as a profound "revolution" in psychology. Same may be applicable to whatever is called "American" psychology in the present time—the actual ways of thinking and

research practices may be not too different, yet the promotion of all fashions in psychology, created by American designers, can be very active.

In other terms: The understanding of psychological issues is trans-societal, while talk about psychologies in different countries are—like any cross-societies' comparisons—working in service of separation of one society from others ("we" versus "they"), while actually integrating them. Of course the history of a given society plays a major part in talk about psychologies—it would be quite a thought experiment if one were to consider that Freudian ideas could have become popular in the middle of rural America, or that behaviorism as a social fashion could have proliferated from Vienna outwards.

Yet persons who could generate ideas of both kinds may happen to live in either place, yet their ideas may disappear into an oblivion, by not being amplified by the discourse systems of the given science.

*Knowing it is difficult to concrete, let me insist: What are the main differences and similarities between American and European psychology?*

*Jaan Valsiner:* Latter in some European countries have taken the usual American focus on "publish or perish" (or better: "publish, publish, and perish under the load of publications") to an institutionalized extreme, which is even unheard of in America. Interestingly, in the short time of about one hundred years, the dominance patterns between Europe and America have been reversed—a century ago, American students flocked to German universities to get psychology education; nowadays German and other European psychologists seem to look for chances to study or work in America. If Wundt's laboratory floor plans were the main exports from Germany to the United States hundred years ago, then now it is the export of "Techniques of management of psychology" that goes the other way round. Then, the theoretical ideas might have been left behind the practical borrowings, now the reasons for American social discourses about psychology may pass by the European importation of fashions from the new world.

*Once you mentioned that German scientists talk with—maybe too—few words too much, on the other side, American psychologists say with many words nothing—their articles often seem empty . . .*

*Jaan Valsiner:* Well, nice saying it, may be . . . but it is largely rhetorical, rather than true. The contrast is not so clear, but in a more elaborate version there is some truth in it—in the American academic system, the "symbolic capital" of publication for the same of publication is more directly accentuated than in Germany. Hence much of the writing that provides no new solutions, but just adds "to the literature"—the mass of papers nobody these days can read carefully, gets eventually published. In Germany, a similar pressure (and possibility) may still be less advanced. But see the case of the Netherlands—where the evaluation system of universities' "work groups" emphasizes publication of little papers in English in international journals. So there are chances to find relevant ideas in Germany . . . yet there are also many examples of meticulous reviews of the literature that do not advance new ideas. So, the contrast is not so much between countries, but between individual scientists.

*This sounds as if psychology in different countries is more or less equal, and differences occur on the level of single scientists. I understand this in the course of an international psychology—is it right? But as a cultural psychologist in a way you pronounce the impact of culture, therefore: can you try to specify something about the relationship between psychology and culture?*

*Jaan Valsiner:* Hmm . . . but culture *is* equal, between persons and societies as well. All of them are *organized* by cultural means, surely differently from one another, but equally in the sense of culture and human activities being mutually constituted by the same general model. Here I differ cardinally from "post-modernists" who would emphasize the local nature of cultural phenomena, and the lack of general laws—in contrast. I consider cultural psychologies to work towards establishing general laws for human cultural existence—not prescriptive or normative laws, but general principles of making sense of the world.

Coming back to psychologies in different countries: The core knowledge base of the discipline is international, the individual-psychological reasons why persons in different countries perform the symbolic act of "becoming a psychologists" may be similar; yet the specific sets of meanings operating in everyday life may vary from society to society. Also social institutions of psychology are built along similar lines: when an institute or department of psychology is established in a country—be this Greenland or Antarctica—the ways in which that may happen may be borrowed from elsewhere, and is hence international. Well, Antarctica's psychology departments may be too much of a fiction, but when United States psychology departments were established about 100 years ago, there was clear effort to replicate the floor plans and apparatus of Wilhelm Wundt's laboratories in Leipzig.

*After this glimpse at countries, psychologies and cultures it impressed me at a first look how many countries you visited, how many colleagues you collaborate with . . . At a second look I considered if you are a little bit "afraid" to work too long on one theme, to stay too long in one country, and to work too long with one team. What's about this impression?*

*Jaan Valsiner:* The impression is quite wrong—I am not "afraid" of any country or team too much, in fact I am quite individualistic in any country or team I work in. The length of my stays in different countries is an artefact of my job requirements—I can't visit other countries for more than briefly only during my summer vacation, or when special circumstances allow it, like the Humboldt-Prize year in Berlin, Fulbright-arrangements in Brasilia. If I had my way, I would divide my time equally between North America and Europe, semester in each, and spend the vacation time in other countries, e.g. Brazil. Such move is very productive for my thinking and work, and (hopefully) for people whom I visit.

I do not work "in teams," I work with persons, who sometimes happen to create teams. I never rely fully on any of these collaborations for my own purposes—these are nice additions to my main work, which is always mine or shared with very few close collaborators. If I work "in teams," these are teams I create myself. One could, of course, analyze this that I am afraid of working "in teams", . . . maybe that is true . . . , but at least for me the main issue is whether projects get done or not. Often I find "teams" concentrating on locally relevant issues, or narrow problems, and working inefficiently—and surely I will

not be closely involved with these. Then, there can be teams which are more productive together than each person in it, e.g. the French Bourbaki group in pure mathematics, and I would be glad to create such team, some day, somewhere . . .

*Talking about your working style, let us have a look to your earlier years: You were a "sportsman" before you became a scientist. Firstly, what were your goals in the former field, and secondly, are there similarities and differences between the young (sports-)man Jaan Valsiner and the little bit older academic man Jaan Valsiner?*

*Jaan Valsiner:* Similarities are very clear. I work in academia in the same way I trained for (limited) success in decathlon when I was 13-19 years of age—in parallel on many projects, and all the time. Sports gave me a different view on how one works diligently for often ephemeral results; yet the motivation is strong. One learns to lose, and win, while continuing towards improvement. One learns to become independent from evaluations of others that are not based on substance or knowledge of the issues—somebody's praise for performance which you yourself consider mediocre does not have effects on you. The only of evaluation is general ideal future goal orientation. It could be illusory, because never attainable, but it keeps you on course.

*Some passages in this interview and in other contexts suggest, that you don't like "followers". Can you tell something about it? Maybe analogous to the remark of Sigmund Freud not to be a Freudian—would you say "I'm not a Valsiner follower!"? What does it mean for elaboration of (your) theories, what is the (non-)sense in following?*

*Jaan Valsiner:* Yes, I am definitely *not* a "Valsiner-follower." After inventing some ideas—e.g., the system of "constraints" as in "Culture and the development of children's action"—

I moved away from talking in terms of ZFM/ZPA/ZPD<sup>20</sup> for some five or six years, not wanting to make these fragile concepts into an orthodoxy for myself, and I am only tentatively returning to these notions in recent years. I accept that most of our constructed ideas—especially in the theoretical sphere—are probably poor solutions, so it is better to try new versions, than to try to prove by empirical work that the first solutions were "correct." Personally, it is simply boring to see others following the same ideas I am expressing, the critics of the ideas may give you a new impetus for developing these further, even if a critic wants to crush your ideas fully, there *may* be a reason to look at *how* he or she does it . . . and them maybe reject the criticism. My ideas are not "applicable" to anything, without re-thinking them for the particular purposes.

*What does this mean e.g. in the case of your "following" Vygotski?*

*Jaan Valsiner:* I think that I—together with Rene van der Veer—have quite powerfully shown how shallow are most of the contemporary efforts to "follow Vygotsky." Following of this kind is not different from the mentality of the left-wing students handing out marxist leaflets in front of the Mensa in Giessen. I can understand why such followers come into existence, but have no sympathy with their activities. Construction of scientific ideas is a subjective process like artists sketching many versions of a drawing, before arriving at one that fits. Copying each of the sketches by ardent followers would amount to stagnation of ideas, not their development.

*A rather clear demarcation. Your answer, as well as some of your remarks in articles indicate that you are afraid of systems, or more correctly, of orthodoxy—is it right?*

*Jaan Valsiner:* "Afraid of" . . . perhaps disgust about perfectly intelligent people starting to play role of followers of one or another "system," turning it into a fixed system. Vygotsky and Piaget have suffered that fate. I would like to know how many leading psychologists would be ready to say that they transcend their predecessors (in ideas, not number of published papers), and how many of them would actively encourage their doctoral students to go beyond what the professor him- or herself is saying? Such intellectual transcendence is a guarantee for new solutions as well as many dead-end-streets; following never produces anything new, perhaps social fame at times.

*But do you think it is possible to build or develop a theory without being (at least a little bit) orthodox?*

*Jaan Valsiner:* I fail to understand your question—"being orthodox"? If you mean "traditional" psychology, then my own work is hyper-traditional—it is based on the standards of "purity" of the 1920s and 1930s, and not on the (more imprecise) standards of 1950s to the present. I find myself advocating some conservative ideas such as *presentation of raw data* or data as close to their "raw form" as possible, which are clearly old-fashioned. And I do it in an "orthodox"—in the sense of arrogant, dominant, or whatever—way.

In contrast, I have no grand pleasure to follow what passes as "theory" in the "post-modern" social sciences. It often seems to be propagation of some (journalistically made relevant) social representations, and juggling their relations. The biggest problem seems to be the ideological acceptance of the grandness of "applied topics"—in the name of such, thinking can be put aside, and replaced by superficial talk about difficult issues. At a congress of psychology I recently attended, the number of posters and papers dedicated to *psychological* issues of AIDS seems to have been bigger than number of AIDS sufferers. This seems to me to be profanation of psychology as science—running after "popular topics" of *no solution* by psychology, and thus bringing the profession to the center of publicity.

*On the one hand, you refuse working on—as you titled it: 'postmodern' psychology, which relies on actual themes, e.g. AIDS; on the other hand it seems to be important for psychology to talk about actual themes. Is this a dilemma?*

*Jaan Valsiner:* No, I do not see this as a dilemma. I do not consider AIDS to be an "actual theme" for psychology, because it is a topic fabricated by the social system of a particular kind, e.g., U.S. middle-class dominated system, which superimposes its life ideologies on others, it is a kind of "missionary effort." Thus, let us ask—why should psychologists study AIDS and not Ebola or influenza? Or any other illness? What is so special about AIDS—*other than* what journalists constantly tell us about it? And what they tell is of course a horror story, but then—would psychologists be in any position to solve the problems of AIDS? My guess is no (same answer for influenza, a problem we even do not take interest in!). So why study such questions, where there are many others that psychologists may find more fitting for their investigation—like *construction of horror scenarios* per se? Psychology of journalists, politicians, or anybody else who at times purposefully constructs

a publicity-catching horror scenario, to gain political capital from its success? The psychological issue about AIDS is not AIDS per se, but our *ways of talking and feeling* about AIDS, this could be a way to sum up my point.

*Nevertheless, you worked—more or less—sometimes on actual themes, see your engagement in research upon accident prevention, see some topics of the Hertzen-activities etc.—What is the difference between this and the so-called postmodern science?*

*Jaan Valsiner:* I never pretended to try to solve the practical question of accident prevention. In fact, the issue was (and is) of interest as a paradoxical theoretical problem for psychology—one that psychology is very poorly equipped to solve! Namely—accident *prevention* entails producing an event that does not exist (i.e., *no* accident), while most of psychology has been trying to predict or explain why/how events that *do exist* come to be so. I turned away from the "accident prevention" area at the time when it was suggested to me by some other American psychologists that this could be a great success area for my work. I still keep interest in its theoretical issues, but would not start applied programs for "training parents how to avoid accidents for children." No doubt, this topic is very actual—yet I do not know how to "train" anybody in this way.

*Maybe it would be good at this time to ask what you exactly understand under the terminus "postmodern"?—What does it mean, going postmodern ways?*

*Jaan Valsiner:* Good question! Possibly I have been using these labels too widely. I basically meant the world of our time, where mass communication mediates between persons and experiences, and where knowledge is supposed to be fragmented, local, discursively constructed, and where generalizations of basic kind are assumed not only to be impossible but also unnecessary, in the social sciences.

#### ***Part 4: Cultural psychology***

*Cultural psychology is one very important topic of your current work. Can you describe the main concepts of cultural psychology, which are currently the most popular (for you)? What do you think, cultural psychologists will take in their research/thinking in the near future?\**<sup>21</sup>

*Jaan Valsiner:* I see many of the people who (now) label themselves "cultural psychologists" struggling with issues that are not new to philosophers: *Mutual constituting of person and the other, social discourse, narratives, national and personal identity, activity, social representations, social participation, semiotic mediation, myth*, etc. This (incomplete) list indicates how diverse are the different concerns of "cultural psychologists." In many ways, such diversity is healthy, as long as there is no uniform, canonized *one* cultural psychology, development of ideas is still possible. Yet it makes it impossible to predict what that diverse crowd of rather vocal psychologists would actually do in the future. Maybe it all is a temporary fashion, built on the basis of social opposition to "cognitivism" or any other "-ism" . . . if that is the case, then cultural psychological talk sooner or later becomes extinct. I hope myself that it is not a fashion, and if I am correct,

then the urgent need is to build up empirical methodology for the different cultural psychologies. So far such efforts have been few, and inefficient.

*How did you yourself come to the topic of cultural psychology?*

*Jaan Valsiner:* By coincidence. The "new" field of "cultural psychologies" of the 1990s simply happened to "suck in" what I was doing in the 1980s, not bothering about whether it is or is not "cultural psychology."

*So you worked as a cultural psychologist even as it was not modern to talk about culture and psychology.—Is in this way the term "cultural psychology" only a new terminus technicus? And what do you connect with cultural psychology?*

*Jaan Valsiner:* I think it is a term that builds upon many oppositions. First, the old fight between meaningless-objects-oriented psychology (e.g. Ebbinghaus' nonsense syllables in memory experiments) and meaningfulness-accepting psychology (e.g. Frederick Bartlett, Muzafer Sherif). Secondly, there is the opposition with cross-cultural psychology. Thirdly there is the implied fight about methodology—cultural psychologies at times call for qualitative methods (being unable to create the general logic of those) in opposition to quantification. For me, "cultural psychology" is a generic label that fits many different orientations that deal with issues of meaningfulness of psychological entities.

*Do you think that cultural psychology can ever become a systematic, coherent psychological paradigm? And do you think it would be a good thing for cultural psychology to become a paradigm?\**

*Jaan Valsiner:* That knowledge construction in different cultural psychologies needs to become *systematic* is beyond any doubt for me. Yet I would be a bit uneasy to suggest it could become a *paradigm*, with the connotations (à la Thomas Kuhn) for "normal science" that this entails. The current fascination with cultural psychologies seems to build (or at least does for me) exactly on its function as "revolutionary science"—it builds on the tension between psychological phenomena (and their cultural nature) on the one hand, and the inability of the canonized "methods of psychological science" to consider these, on the other.

*And do you think that it is possible and / or desirable that psychology, especially cultural psychology, should be formalized in a way comparable to natural sciences? Can ever construct basic mathematical equations that describe human socio-cultural development? \*\*<sup>22</sup>*

*Jaan Valsiner:* My inclination is to say *yes* to both questions—the "could" one and the "should" one. Or, more appropriately—it all depends upon the kind of mathematics that is being utilized for such purposes. Psychology as a discipline is notoriously out of date in its understanding of what amounts to mathematical models, as it has canonized the ideology underlying linear statistical models as *the* epitome of "scientific method." Quite clearly, human socio-cultural development cannot be modelled statistically, since the phenomena of development misfit with the axiomatic assumptions of the statistical thought system. Hence there is a big gap—one that can be overcome by replacing statistics with other formal models which might fit with the notions of development.<sup>23</sup> Should socio-cultural

psychology try to do that?—If it is serious about general knowledge construction, then yes. Mathematics help to capture those aspects of knowledge that may be difficult to conceptualize in general (philosophical) verbal descriptions. Furthermore, mathematical models allow for deductive power, which psychology usually lacks. Of course we talk here of different versions of qualitative mathematics, not of the old quantification-based application of mathematics.

*What influences has your experience with the Soviet occupation of your homeland had on your views of cultural psychology?\**

*Jaan Valsiner:* In the narrow sense, such occupation framed my studies of psychology, resulting in very positive education. Because of the occupation, we all had to study officially prescribed versions of "Soviet psychology," which was a rather obscure mixture of some of the best traditions of Russian psychology, and pseudo-marxist slogans. Needless to say we—in Estonia—studied it with a critical attitude (not expressed publicly, of course!), and never took its ideological suggestions really into our ways of constructing psychological knowledge. In contrast, such pressures led us—us here refers to a group of young students in the 1970s, who were largely self-studied in psychology—to look for Western psychology carefully, get to know it well, and build our own ideas in a very internationally connected way. I think my knowledge of "American psychological literature" in the areas of my interest was better in Estonia in the 1970s than it is now in the United States. We treasured new knowledge, read all the papers we could get, and took the Western empiricist ethos for granted. So, as a result of the fact of Soviet dominance in Estonia—politically—, we over-idealized the West and were sceptical of the promoted Soviet psychology, yet getting to know it. More generally, the formative experience under the Soviet rule helped to develop my healthy scepticism about the inherent benevolence of social institutions. The frequent idealization of the notion of "community" in the Anglo-Saxon social thought that I encounter in the socio-cultural psychologies of our time seems strange to me, given my development under Soviet conditions. We understood very well that all the nice Soviet institutions that declared themselves to "do good for the people" were not of such kind, and one needed to be wary of them.

*Till this time we talked more or less in a general way about your cultural concepts. Let us concrete here some issues of your own work. One of the most provocative ideas in your theoretical conception, is the proposal to understand internal structures and cultural structures both as of semiotic character . . . However, semiotic is a field as problematic as modern psychology. Customarily the problem of meaning itself is understood in a dualistic perspective. This is manifest in the central idea of interpretation—of signs—which implies that some mental entity, usually a mind, interprets some material entity, that thereby becomes a sign. Neither Vygotsky nor Bühler have specified a semiotic in a clearly non-dualistic manner and your use of the term semiotic is rather broad.\*\*\*<sup>24</sup>*

*Jaan Valsiner:* Before we can go in this field, I think it would be good to clarify a little bit: I do not accept the contemporary witch hunt on "dualism" as a reasonable analytic stance. In fact, all "dialectical" thinkers are "dualists" if "dialectics" is rigorously defined—i.e., the unity *and* contradiction of *opposites* within the same *whole*. The negative connotations linked with the use of "dualism" term are of course many, so I prefer to talk about

*duality*—this is an *inclusively separated*, or differentiated structure that allows to look at the relationships between the separated parts—e.g., parts of a whole, A and B, can be studied as to the nature of their relations *if* A and B are at first differentiated from one another, or in my terms inclusively separated. There is no need to eliminate such duality, in fact it cannot be eliminated, as that would eliminate any possibility to investigate the system—in which A and B are considered "merged", not separated. Hence the assumption that Vygotsky or Bühler even tried to create semiotic in a "non-dualist" manner would be suspect. Signs can be constructed only if they relate to something else, and hence duality is a necessary precondition for any semiotic system.

*So we would like to ask questions as to the nature and functioning of the semiotic process you have in mind when thinking of co-construction of the person in culture. First: Are all parts of an ecological systems of semiotic nature or only some? In the latter case, what do you think to be the character of the other structures. The question pertains to internal as well as to external structures of any one person.\*\*\**

*Jaan Valsiner:* Sure, only part of the "ecological system" in human case is semiotic; not all. The semiotic level is a structural level one step higher up than the functional structures of human relations between the organism and the environment. As internalization and externalization work as parallel (opposite) yet linked transformation processes, there is constant reorganization of *both* the external structure of the world, and of the intra-personal structure of subjectivity. Psychology cannot be built without accepting human subjectivity—psychic reality knowable only to the person, as a component of ongoing relating with the world. The Subject-Object *inclusive* separation is a pre-condition for any psychological analysis.

*What makes an internal and an external structure a semiotic one? When does this occur and what are the effective conditions?\*\*\**

*Jaan Valsiner:* Sign construction occurs at moments of increased uncertainty about the present and future-expected relations of the person to the world. It is not necessary when the person and the world relate without misfit—we do not create signs to reflect upon the micro-process of ordinary walking, we take that for granted that we just walk, *but* the very moment our ordinary walking is hindered in some way, we may begin to create signs to make sense of the new situation. Thus, *disequilibrium* in person-world ecological systemic relation feeds into sign construction, and re-equilibration can lead to sign loss, or abandonment.

*How do semiotic structures interact, if there is no mind other than the semiotic system itself which is to interpret signs? This pertains again to internal and to external signs themselves and in addition to internals interacting with externals.\*\*\**

*Jaan Valsiner:* Surely there is the organism, the active person who *constructs*, *re-constructs*, *pre-constructs*, and dismisses signs, in accordance with goal orientations at a here-and-now setting. Semiotic processes are person-centered in the sense of the person setting out in one or another goal-orientation, and on the basis of current relations with the world. Sign systems operate via person's construction of them, they do not have life of their own, without linkage with persons.

*What does interpretation of a sign mean in an ecological system consisting (exclusively or in part) of signs?\*\*\**

*Jaan Valsiner:* "Interpretation" is one form of Subject-Object relation in the construction/reconstruction process of signs. If we were merely interpreting signs, there would be no function of these signs in relating to our movement further, in our life-worlds. Human beings would be seen as old maidens gossiping about one another rather than making something more pleasurable. This is not to denigrate the relevance of interpretation, only to show its limits. The person not just *interprets* (by oneself, and by others) constructed signs, but constructs the specific structure of one's personal relation with the world by way of constructing signs. This specific structure is that of heterogeneous *affiliating and distancing* profile between the Subject (person) and the Object (the world). Many contemporary socio-cultural perspectives in psychology try to eliminate the Subject-Object distinction, and thus close the door to any notion of distancing. In contrast, I argue that signs *simulateously* link the person with the world *and* distance the person from the world.<sup>25</sup> Different forms of Subject-Object relations emerge as hierarchies of semiotic organizers (signs in relation to other signs that either block some action, or enable it). We can speak of both "liberation semiosis" (signs make us free) and "semiotic entrapment" (signs block certain ways of acting, feeling, or thinking). The duality of the two processes—relating and distancing—is seen as inevitable and necessary function of signs. I think different socio-cultural perspectives have been trying to solve that problem by reducing the function of signs only to one (relating) or the other (distancing) sides. The conceptual third solution I adhere to is the use of concept that unites the two opposites.

### ***Part 5: Mainstream and non-mainstream, old and new psychology***

*Till now, we heard a little bit about your way through psychology, and you underlined that you always are rather interested "to go your own way". Some of your remarks suggest that you seem to work "against the wind" of mainstream psychology? Is this right? And what do you think about the contrast "mainstream" vs. "against mainstream"?*

*Jaan Valsiner:* Of course I do not have a clear answer here myself . . . but . . . maybe it is good to start from the question "what is mainstream"? We usually talk of it as if it is a *homogeneous* set of beliefs, practices, prescriptions which are equally followed by all of its "practitioners" or "followers," and then there are these "courageous outsiders" who "deviate" from the "mainstream." Even if this picture *can* be a sociological view of psychology—probably fortified by our reconstruction of psychology's history, e.g., there was "introspectionism" as leading paradigm, then came "behaviorism" as "mainstream," then "cognitivism," then "ecological psychology," etc.—this picture is probably only adequate in way of a gross approximation. Maybe it fits at the level of *politics of science* where certain directions "come in" and others "go out". But surely that all does not represent the heterogeneity of *psychologists' individual interests* who are working, in parallel, in the same research unit or department. The persons look at *ideas*, relate positively to some, and

negatively to others. So in interpersonal sphere, there is no strict mainstream vs. non-mainstream distinction, but a very heterogeneous picture. Now, *in my own work*, despite of its criticism of some directions of others, I do not think in terms of mainstream or non-mainstream; my thinking is about idea complexes: e.g., what is "non-developmental" is easily dismissed from my interest sphere, but *not* because it is "wrong", rather because it is something of little interest; or my irony about "sociobiology" is about the readiness of sociobiologists to mix up levels of analysis, thus creating a "soup" that nobody can eat, except themselves.<sup>26</sup> Thus I can sometimes stand on the same position on which "mainstream-persons" stand, e.g., my criticism of psychologists' uses of statistics can be similar to that of a "purist" statistician, and I can be very critical of the self-pronounced "cures" in psychology as "deconstructionism" or "post-modernism". So I am not fighting a war, I am just critically analyzing different ideas, and sure, some of these are "dumped" in full. I guess this complex position makes it possible to work in parallel, and at times together, with persons of very different standing on the mainstream-non-mainstream distinction or continuum. *Technically*, the crucial point is *output productivity*. I do not waste my time and energy worrying about how sad the situation in psychology is, but will try to analyze it and go further.

*Integrating different fields and different disciplines' perspectives seems to be very modern; but on the other side, your pattern looks "unmodern", too, because in my opinion the most scientists are specialists on one field.*

*Jaan Valsiner*: Yes, my pattern is very unusual within the "modern" scientific world, even if the latter pays lip-service to "interdisciplinarity", by which is usually meant to persuade "the other" disciplines that my discipline has better ways of doing things, so you should "join in". The real interdisciplinarity is in the process of actual knowledge construction, and one should ask an "interdisciplinary scientist" (who labels oneself that way): "Don't tell me who you are, tell me what you *read*, and I can then tell you who you are!" Only then will the specifics of possible "interdisciplinary synthesis" come forth. *One more point*: Behind my constantly changing and multiple interests, is still *one major theme*—fascination with the question of *how can human beings* get along with themselves, and with others, under environmental contexts which look solid, yet may be ephemeral.

*As you are interested—as a main theme—in understanding of development between individuals and culture and especially fascinated about the question of how human beings get along with themselves and others: their is another very important part of psychology, traditionally dealing with this question—psychoanalysis. You never did mention psychoanalysis—are you not interested in or what other reasons has your not mentioning?*

*Jaan Valsiner*: Indeed, I am not interested in psychoanalysis—as explanatory framework—, but I am interested in the phenomena that psychoanalysts work with. For instance, the question of subconscious phenomena stands unresolved since Pierre Janet (and later Sigmund Freud) made it into a central issue of psychological analysis. Or—the documentation of the rehabilitation processes of children who had experienced harsh life conditions in concentration camps during World War II (Dorothy Burlingame's and Anna Freud's work) continue to provide very good realist testing challenges for these of our psychological theories that look down at the simplistic interpretations of psychological phenomena in

terms of "penis envy." However, our non-psychoanalytic explanatory concepts are not any better than the ones propagated by psychoanalysis (for instance, compare "libido" and "cognitive capacity"—in their buildup these are the same). Only psychoanalytic concepts seem to catch our affective reactions, whereas cognitive ones seem neutral . . . So maybe (speaking meta-theoretically) our (including my) rejection of psychoanalytic explanations proves psychoanalysts' basic point . . . In this case, it may be my own fear of psychoanalytic constructs that hinders me from making use of these, and of course this is based on my lack of secure attachment to my mother at age twelve months, and emergence of an attachment "working model" that is either anxious or ambivalent as to different approaches coming from psychoanalysis. In an apparent act of repression of my unconscious desires, I simply claim that for me different psychodynamic theoretical explanations have not been intellectually interesting. This is a statement reflecting personally constructed entity (which "interest" is), and for me the issue stops there. A statement "I am not interested in X" allows me to think in other directions, and not be bothered with the natural question that an interpreter of my ideas would follow—but why not? The author's and interpreter's perspectives necessarily differ.

*What do you think are "good" theories/models/approaches in contemporary psychology? And if you relate your own approach to these, how do you evaluate your own approach?*

*Jaan Valsiner:* I do not operate by evaluating theories as "good" or "bad", theories are just theories . . . My own theory has some value (sometimes it seems to me) because it is simultaneously general and phenomena-close. Contemporary psychology is wary of such, these were more usual some decades back. But the value also means many unsolved questions, which I cannot deal with. So, it is a mixed bag of constructions . . .

*On the one hand, you mentioned that you don't (want to) evaluate theories as good or as bad . . ., on the other hand, you are not interested in psychoanalysis . . . So let me take this to reformulate my question: In what kind of theories are you interested in, in what you are not interested in?*

*Jaan Valsiner:* "Interested in" refers to the issue of what constructive linkages I can make between what I am building and what others suggest could be used. My lack of "interest" does not equal stigmatization of these as "bad" or anything—I deeply respect psychoanalysis' capacity to deal with complexity, and sensitivity to nuances of phenomena. In other terms—I am interested in these theories that for my present purpose seem to allow productive linkages with what I try to do.

*In the context of this question: How do you get your themes?*

*Jaan Valsiner:* Through combining observation of everyday life events and very general theoretical ideas. Once these meet, I have what I could call a "researchable project", e.g., the studies of mealtimes, children's bye-byes, etc. in the 1980s, study of tattoos and body decorations in the 1990s. The theoretical orientation sets the stage where to look in everyday life.

*What was your last empirical work in a traditional sense?*

*Jaan Valsiner:* My last empirical work is my *current* work, on autodialogues in construction of self, together with Seth Sargan and Ingrid Josephs. This has been going on in the last one and a half years, first results should soon be available.<sup>27</sup> But, of course, this is not "traditional" kind of work, it is an explicit effort to create consistency between theoretical and empirical sides of research, with theory dominating.

The last "traditional" one in the sense of full-fledged use of statistics, for instance, arised from a conference paper in 1982 in Austin, Texas.<sup>28</sup> After that some of my co-authors have found it necessary to use statistical methods of analysis, I have agreed with their need, but in my thinking that use has had no relevance. Statistical ways of data analysis are a dead-end street for contemporary psychology.<sup>29</sup> Psychology's phenomena and statistics rarely fit one another. Quetelet made a rather unproductive extension out of astronomy into "social physics," and Galton and Pearson made it into a trade . . .

*In the context of your criticism on statistical use one of your main criticism is that developmental psychology is a non-developmental psychology! What do you mean with this paraphrase?*

*Jaan Valsiner:* What I mean by it is very clearly written down in my book "Culture and development in children's action". What is labelled "developmental" in psychology is *often times* mere comparison of age groups, without any specification of how children develop. This is actually merely "child psychology," not "developmental psychology." In contrast, any perspective in psychology or biology that *directly investigates transformations in time* is developmental. In psychology, such efforts are very rare.

*What could help going forward to a developmental psychology?*

*Jaan Valsiner:* Answer here is simple—consistency needs to be restored between theorizing, methods construction, and phenomena, all based on clearly specified basic assumptions. Non-developmental methods will never reveal processes of development (understood as processes of transformation of form). Psychology's socially institutionalized empiricistic ideal—a recent social construction, as Gerd Gigerenzer et al. have shown—needs to be returned to the case of theoretical and empirical thoughtfulness.<sup>30</sup> Developmental psychology historically grew out of *embryology*, and not from psychology (psychophysics), so it is closer to developmental biology than to non-developmental psychology.

*You are a critic on methods and on methodology, you are a critic on theories (e.g., sociobiology), you are a critic on developmental psychology, and so on. But how do you deal with the phenomenon that there is no great change: the psychologists work again and again with this unfruitful methods . . ., they again and again engage in non-developmental psychology?*

*Jaan Valsiner:* Hundred years is too short a time for solving basic questions of the given science. Chemistry took about 300 years to move from alchemy to chemistry; psychology seems still stuck in an analogue of alchemy.

*After we talked about different aspects of mainstream and non-mainstream, let's have a look more concretely on "new psychology": In different countries yet some "rethinking" of*

*psychology occurs—see the books of Smith, Harré and Langenhove<sup>31</sup>, or here in Germany the foundation of the "Neue Gesellschaft für Psychologie" [New Society for Psychology], which prefers an other understanding of theories, and also the use of other methods. What do you think about these tendencies in psychology? What needs this psychology to become stronger than it's at the moment, or is there any sense at all for this "new psychology" in the over all quantitative-oriented psychology?*

*Jaan Valsiner:* I would prefer to separate two sides of your question about the "new psychology"—one is the social-institutional negotiation (or fight, if one prefers that term) between the "old" and "the new" for a socially recognized and established place in the given society (or discipline). The other is the question of "how new is new"—what substantive prospects are there with the "new" variety, as it fights for its existence. Not all "new" is necessarily new, nor is "old" necessarily old. First, I think that the social-institutional negotiations between "old"/"new" are largely social phenomena, and can be analyzed like any social movements within a society. That psychology often ends up inventing "new" psychologies speak rather unfavourably of the restrictiveness of the "old" version in its socio-political organization, as well as about the unreadiness of psychologists to play with concepts, rather than follow any of them with some missionary zeal. I see *both* rejection of the "new" by the "old" psychology (e.g., denunciation of the "new" as "soft science"), and the rejection of the "old" by the "new" (e.g., denunciation of the "old" for its refusal to accept "qualitative methods") as equally fruitless (yet real!) social phenomena. What we have here is a social dominance fight—fight about axioms on ideological grounds—which in itself has no role in science. It does not "preserve the purity" of the accepted version (i.e., the "old"), neither does it lead to breakthroughs in the "new" direction, since the proponents of the "new" move to form a strict ingroup/outgroup relation between "we" and "them", similar to that formed by their more powerful opponents of the "old" kind.

From that viewpoint it is only a social phenomenon that in Germany there are two (mutually opposing) societies of psychology—or that in the United States there occurred the split between APA [American Psychology Association] and APS [American Psychology Society]. These are sociological, not epistemological phenomena. Even if in that dispute—e.g., using the question of quantitative versus qualitative methods as a means of such dispute—different productive nuances of substance are highlighted, no solutions can be provided from a clear "partisanship" point of view. In a wider sense—much of contemporary "new" psychology spends its energy on fighting different virtual opponents (e.g., "positivism", "behaviorism", "dualism") without a positive constructive programme that would replace the criticised problems. It is largely *deconstructive*, rather than *reconstructive*. Let us consider the "qualitative *versus* quantitative methodology" dispute, which in my opinion is misconceived in a number of ways:

At first, often, the quantitative methodology is made to equal "the scientific method," while qualitative one is seen as its opposite (or at least a "soft" version). This is quite unrealistic—belief in the safety in the precision of numbers is a political/social construction, and has nothing inherently scientific in it.—Theodore Porter has analyzed this beautifully in his study of the meanings of objectivity.<sup>32</sup>

The quantitative/qualitative distinction is—as a second issue—often equated with nomothetic/idiographic distinction. This is demonstrably inadequate, e.g. Gordon Allport's "morphogenetic science" of individual cases was meant to be nomothetic.

Lastly, the opposition is often seen as fight of mutually irreconcilable opposites, while in reality the two are easily combinable, *depending upon the criteria* used for science. If the latter criteria were to include *adequacy to the original phenomena*, then most of psychological research is scientific if it devises theory- and phenomena-adequate formal-qualitative models, rather than uses statistical models (e.g., ANOVA-type models, general linear model) by forcing those onto the phenomena. Automatic prescription for the use of "the statistical method" as the guarantee for scientific status of a study is a blatantly non-scientific act by any researcher.

Interestingly, on that point I and many purist statisticians would agree—I argue from the side of need *not to do violence* to the phenomena being studied, they—from the side of *not violating the axiomata* on which the statistical models are based. These basic assumptions of statistics are violated in psychologists' research practices every day . . . and a social consensus is formed that such violations do not matter.

*So, in sum:* The issue is not an opposition of the "old" and the "new", but construction of methodologies—which ever these may be—that fit the phenomena we are interested in.

*You criticized data-fixation and you abandoned themes like testing, inference statistic etc. Instead of this, you think it is crucial important to start research near to the characteristics of phenomena. Your critics, and your stressing priority of phenomenon before methods is also near to traditional qualitative positions—though you stressed your position to be near to puristic statistic positions. Do you also accept other qualitative arguments as induction instead of deduction, reflecting the researcher and research subject relation as important for gainig and interpreting data, and so on? And what are the conclusions, you drew from your methodological position for doing research?*

*Jaan Valsiner:* I can't understand your question. My criticism of "data fixation" is in the context of *methodology as cycle*, not a criticism of data orientation as such. From that perspective, I am close to "purist" scientists in other areas (physics, chemistry), and even to applied mathematics (which statistics is). The idea of "induction *instead of* deduction" is absurd from the viewpoint of such methodology cycle; the talk about "dialogue" between subject and researcher is an exaggeration of the data-constructive reality, independent of if we think of it in such hermeneutic terms, or not.

*Though rejecting the separation between old and new, in the context of your journal Culture & Psychology you talked about moving towards the goal of restoring theoretical discourse in the area of New psychology.—What does it mean? What does "new psychology" mean for you, for your work?*

*Jaan Valsiner:* "New" here is of course a bit utopistic label . . . there is no "new" without "old" (= my point about relevance of history of psychology for its future). As an idealization, the "new" for me means a synthesis of ideas that transcends both previous "-isms" (behaviorism, cognitivism) and newly created ones (vygotskianism, etc.).

## ***Part 6: (Re-) writing and (re-) teaching psychology***

*Let me ask first, before talking about some more general aspects of writing in psychology, what is your "favourite" among the many articles and books you wrote or edited? What is the most important one and why is it favourite to you?*

*Jaan Valsiner:* I think the crucial paper I started from—and keep going—is the one on variability in the *Journal of Mind and Behavior* in 1984.<sup>33</sup> The question of variability is central for me. Because variability contains the information about issues of development, which is my interest.

The most important book is "Culture and the development of children's action" published in 1987 by Wiley. Re-writing it for the second edition I noticed how the basic ideas—expressed then in a youthful arrogance—are by now better "grown", and yet on a trajectory that forces much of psychology to re-organize its premises.

*In psychology, a lot of books and papers are published. Why it is necessary to publish such a lot? Is it important for the development of theories, or/and an impression of scientific politics — "publish or perish", or the other way around, if someone wrote a lot, he or she exists?*

*Jaan Valsiner:* Well, it is not necessary to write much, at all! It is simply that I get carried away by different ideas, and as I feel that some of these are worth interest by others, I make an effort to get them to be available in the "public domain," rather than "write into my drawer." As to scientific politics—I do not care about that, the mere fact of my publishing what I want, in my own way, should be sufficient to satisfy the minimal needs of such politics.

*What are your ways to avoid such scientific politics mechanisms?*

*Jaan Valsiner:* I do not bother to try to become accepted in "established journals," as I am creating my own publication outlets, so from 1985 to 1995, it was only edited volumes, and then I created my own journal, "Culture & Psychology," under appropriate circumstances. My main reason for not trying to publish in "regular" journals is simply lack of time and need—I am busy with the other publication projects. I do not give any great value to the "refereed nature" of journals: I consider "refereed nature" to be an institutional construction of *local conformity*—editor and three reviewers establish a "consensual truth" that manuscript X is very good—and I will not accept that "blind conformity" can guide science.

*But you go in it?*

*Jaan Valsiner:* For instance, I have never taken the good demand of my colleagues "publish in good peer-reviewed journals" seriously . . . if I have published in some of them, it is largely a co-incidence, rather than a plan. There are many things I have not published—especially results of empirical projects—as I do not see value of these projects beyond what I had learned while doing it. Psychologists in general publish too much, and most of what is published is either trivial or boring. Jan Smedslund's point of psychology being largely "pseudoempirical"—proving empirically what is already known in common sense—seems very true to me, and a very powerful critique of psychology, even as I may differ with Smedslund as to suggested solutions.

*You mentioned similarities between Smedslund's and your position about "pseudo-empirical", but there are some differences between him and you, too, according to the problem to find a way out of the pseudoempirical state of psychology?*

*Jaan Valsiner:* I agree with Smedslund in his ways of defining "pseudoempirical" as pre-given by our common language/sense, but I do not share his solution—looking for variety of "common-sense theorems"<sup>34</sup>.

I think that this proliferation of publications for the sake of getting published (= get a "token" of "symbolic capital" to accumulate one's academic value—playing along with Pierre Bourdieu) cannot last forever, and will soon be corrected by economic forces . . . that is if psychologists themselves can't find a way to reduce the flow of publications. New journals which are specialized can help—I established *Culture & Psychology* where we meticulously fight with the pseudoempiricism—with some (but not full) success.

*Your journal Culture & Psychology is now in its third year. You started with clearly defined aims and summarized in your first issue: "Ideally, Culture & Psychology will be a sophisticated professional and interdisciplinary journal, where the quality of intellectual contributions of the published papers will be high, and the use of incomprehensible jargon from different disciplines will be reduced by the editorial process."<sup>35</sup> So, after this three years, what are your successes, what are your failures, and maybe, what changes are necessary to get nearer to your goal that Culture & Psychology should be more than only one of many other journals on the market?*

*Jaan Valsiner:* Over the last three years we are keeping to these aims, step by step moving towards the goal of restoring theoretical discourse in the area of *new* psychology. At the same time, the thoroughness of empirical work in that area seems to get better. At first, it was very bad (hence our 95% rejection rate), it looked like people who liked to talk in terms of "culture" (or its synonyms) thought that the empirical side is unimportant, or can be traditionally, or is merely "rich description." None of this has been easily acceptable in *Culture & Psychology*.

*Let me resume: You talked about "Publikationsflut" in psychology and that economic powers will stop it, if psychology won't. What could be a creative way to cope with this problem without intervention from economics?*

*Jaan Valsiner:* Simple changes . . . a department would evaluate a researcher on the basis of *one single* paper published in a given year, irrespective in which place (journal, chapter, etc), and which the person considers the best for the given year. And other such things . . . require one-page curriculum vitae for all job applicants.

*As well, you talked about evaluation feedback of teaching and research in United States, a field, also German psychology has do deal with, though some criteria seem to be a little bit old-fashioned, for example the just mentioned quantitative output of articles. What could German or European psychology learn from American and especially from your experiences?*

*Jaan Valsiner:* I just asked one of my Brazilian friends who is world-wide known in biological sciences: "What would happen to a science if it evaluates itself by number of publications?" His answer was simple: "It will become extinct, because there will be no

intellectual innovation." I agree with him. For German (or other European) psychologists to emulate the science-administrative practices of the United States would guarantee huge success in nominal productivity, yet lack of focus (and lack of criteria) for evaluating breakthroughs (if these happen). Instead of "publish or perish" controversy, psychology has actually published consensually accepted stuff, and either perish anyway, or—if lucky—become "beamed" [civil] problem. I do not think the administrative system of science is meant to care about this—which were the latest intellectual breakthroughs in psychology, anyway?, but individuals might.

*In different ways we talked about re-thinking and re-writing psychology, what's about re-teaching psychology? Actually, here in Germany we find some discussions about "teaching" psychology (or teaching at university in general). The main critics are that studying lasts too long, university helps only little preparing for practice; and so on . . .*

*Jaan Valsiner:* I do not accept such rhetoric—"practice": whose practice?; "too long": for whom? from whose point of view? Psychologists still struggle for defining their social roles in the given society, and it can be relative to the success of that definition efforts that "practice" needs to be understood. If the role becomes well-defined, the lament over "preparing psychologists for practice" either is useless, or constitutes a case of displacement from actual preparation—are we talking about other professions: janitors, cooks, taxi

drivers, artists, lawyers, medical doctors—in terms of "not good preparation for practice?" Or if we do, it probably means something in the role of these professions is changing: consider the extreme idea that janitors one day need to operate high-technology equipment for their job, and then indeed we may hear stories about how "poorly" they are prepared for practice. In general, speaking developmentally, we are all poorly prepared for any (new) practice, including that of living (in the sense of ascending and then descending the "staircase of life"). Psychology (or other) education being "too long" (or "too short")?—again, I see this as a question about the criteria for comparison. Reality of length of education may depend upon simple questions—"who pays for it?"—and who benefits from such payment. If an institution that benefits from such payment decides whether the education it provides is too long or short, there is a temptation to state it is "too short" (= to make it longer), if a government agency needs to pay for it, it may invent the idea that it is "too long", etc. etc. I doubt that psychology as a speciality has much to do with these decisions—there are now programs in the United States that try to produce Psychology Doctors (Psy.D.) in a short time (by cutting out all research orientation from the course of study), while on the other hand the "regular" psychology education becomes longer and longer (Ph.D. level in US is increasingly a long study process, especially if the goal is academic life).

*In this debate many people call for a reform of university and some of them think US-universities to be a good model. What means good teaching for you? What do you like according to the American way of teaching, what is critical to you, and what are good or bad ways you met in others countries, maybe Brazil, Germany etc.?*

*Jaan Valsiner:* Indeed, it is true that "American models" of teaching psychology (and not only psychology) proliferate all over the World . . . like Hollywood movies do. The teaching aids (textbooks, etc.) used in America look impressive, and are forcefully

promoted by their publishers. Yet the "American way" may be a coincidence—a way that has historically emerged in the history of U.S. higher education, has found its place there at the intersection of the school-university distribution of educational roles on the one hand, and the widening of the part of population that gets college-level education, on the other. Like "American democracy" at large, it need not be easily exportable to other societies (for democracy, the only fitting example is the history of Liberia), yet the appeal of that way can be high in many countries.

Takeover of the "American way" of teaching psychology at university level is first (and foremost) complicated by the differences between the educational systems. First of all, United States *undergraduate* level teaching of psychology is *not* for students who specialize (over 5 years) in psychology as such and can work as such, after Diplom as it is in

Germany. United States "undergraduate psychology major" with a Bachelor of Arts or Bachelor of Science degree is *not* in any position to work as a psychologist. Rather, such degree is a general college degree with *some* extra interest demonstrated in psychology, and demonstrated by way of taking a few more courses in psychology. The undergraduates in principle *can* put together their own individual version of education in psychology (by selecting different courses), yet their selection is limited by whatever is offered, and whatever ways that is taught. So the given university's psychology department *de facto* determines what is possible by setting up course requirements and limits what students can get. Sometimes these requirements lead to absurd results—at University of North Carolina there exists an upper limit (for psychology majors) on how many psychology courses they *are allowed* to take (to count towards their degree), paired with limited selection of upper-level courses, this effectively has limited the educational choices for the more interested and intellectually alert undergraduates.

The "American way" of teaching also includes—differently from Germany—a multitude of assignments, exams, etc. during a regular semester. This can have a very positive effect—getting students to produce something in the given topical area—but if it becomes a series of little tasks performed for the sake of accumulating the final exam result, it can be counterproductive. So it all depends upon *what*, *how*, and in *what framework* it is taught in a particular course, the same feature of "the American way" may in the hands of one professor lead to very nice results (e.,g., good papers), while on another occasion it may be useless paperwork. The "American way" is pluralistic (which includes *both* its benefits and problems), whereas the German university contexts are quite different, so are Brazilian, Estonian, etc. ones.

Maybe behind the "American way"-*"European ways"* comparison in teaching styles is actually the curious issue of "post-modernist" fragmentation of knowledge in any social science, and also in psychology. The materials taught in psychology have become organized by content topics ("literatures" that exist in areas X, Y, or Z), rather than by systematic presentation of basic knowledge. This shift is evident if you compare contemporary American-based textbooks with German ones from the 1920s; in the latter the authors tried to systematize knowledge while also reviewing the "literature"; in the current texts the focus is on "socially hot topics" rather than systematicity. Of course the latter is understandable if we look at the administrative evaluation practices of university

professors by their departments—by the end of the semester, students are required to rate the performance of the instructor on some rating scales, and the average of these ratings is used by universities to evaluate the professor's teaching quality. Nothing can be more absurd than that, e.g., in University of North Carolina, in psychology evaluations the question "*how much did you learn in this course?*" is not even asked, yet there are questions about how the person entertained the audience; as it opens the evaluation process to the wide public opinion, and takes it as an indicator of quality. In reality, public opinion always represents the middle ground (mediocre side) of the reception of any knowledge, and cannot be basis for evaluation. Yet the use of public opinion in this function is central in the "American way"—clergymen (preachers) in 19th century US were evaluated by way of how many followers they could gain by their preaching and modern university professors are evaluated by how many dollars they can "preach out" of different agencies for their research . . . independent of the nature of the research.

So—most textbooks of psychology in US are taking into account this social background of undergraduate teaching, and provide instructors with topics that can be presented in a "hot way". Also the textbooks are pre-adapted to fit the prevalent way of testing the students—by "multiple choice" exams. This means that knowledge construction by the textbook proceeds (by students) through guessing what kinds of questions from the text might appear in exams, and what kind of associations between question and "the right" answer there may be in the text. This fragments knowledge construction even further—technical knowledge may be usefully built this way, but not conceptual knowledge.

All in all—psychology teaching has no easy solutions "made in USA" available, maybe it is the case that solutions might be produced in some other country, where the patterns of work and knowledge construction are more systematic.

### ***Part 7: Future and development***

*Very soon you will change to a new university—to Clark, Worcester. What do you expect from this new job and life stage? What does it mean for the development of Jaan Valsiner?*

*Jaan Valsiner:* I hope this move will be good both for me, and for Clark University. I expect to investigate the rich history of psychology at Clark—it was perhaps the very key role-playing university in psychology in the United States hundred years ago . . . in 1909 it accepted visitors like Sigmund Freud, Carl Gustav Jung for its 20th anniversary conference; it was the place of publication of main psychology journals in the United States. Clark has been one of the very few places in the United States where developmental psychology has existed well, and currently its synthesis with cultural psychology should bring interesting results.

*What kind of changes will occur—especially: Will you travel as often as you did in the past? Are you going to create a research group? What else?*

*Jaan Valsiner:* I will travel as much as is interesting and productive, probably as much as up to now, which is actually not too much . . . I travel not very often during the regular semesters, and only for short times . . . I will establish a research group, trying to bring out the most important features of Clark's intellectual history, unite it with the work of students in the group, and with that of international visitors. This has been my goal all along, but it was not possible to do that as productively as I wanted in North Carolina. I hope Clark proves to be more productive.

*Between our interview and the one of Karen Klaue<sup>36</sup> in 1989 eight years passed—some questions are more or less equal, some of your answers are more or less equal . . . What are the main differences according to your thinking between these two times, and how did your positions change in these last years?*

*Jaan Valsiner:* Well, this is a curious issue . . . possibly I will have to admit I have not developed substantively since 1989 . . . a sad thought, yet possible, as I now think of it. Maybe it is only now (1996-97) that I feel moving ahead again . . . At the level of topics, there of course has been the move from interest in the first two years of life to (by now) adult development, but excluding aging . . . I have no interest in that, perhaps in 20 years I will develop it . . .

*We talked about many themes . . . more or less about the scientist Jaan Valsiner. Sometime it seems, that there is only the scientist Valsiner, not the private man; an impression that more general not only arose during the context of interviewing, because it seems that you always work and work . . . Only in a few moments I met the private person, too.—What do you think about my impression?*

*Jaan Valsiner:* The private me is very much involved in work, and all my work (which is publicly visible) is in fact private. I feel like a strip-teaser in many ways, uniting the public and the private. Yet, as any strip-teaser could tell others (if s/he wants to . . . usually not), there are ways to maintain privacy behind the public=private realm. And I do that too.

*If you have a look back to the last decades of psychology, what are the main progressive, what are the main regressive developments? Additionally: what do you expect for psychology in the future, for example for the next decade?*

*Jaan Valsiner:* Sorry, I do not want to speculate on this topic.

*Why you don't want to "speculate" about progressive or regressive ways during the last decades or about psychology's future?*

*Jaan Valsiner:* I can't predict where the discipline is going, why waste energy on making up nice stories where it should go. I am not interested in telling anybody where they should go, but of course I do not conceal what I think of the constructed efforts in the current developments.

### 3 Some afterthoughts about the interview

The unusual feature of this interview—that it was conducted by e-mail—is only partially recognizable. While editing the interview, it developed into a flowing text with a specific dramaturgy differing greatly from the one and a half year time span during which the interview was conducted. The text appears to be—if it needs to be categorized and using traditional criteria—more of a journalistic than scientific product.<sup>37</sup> In the course of the final editing (which Jaan Valsiner authorized), questions and answers were "smoothed" over, several things were abridged or added and the chronological order of the questions was changed. The latter developed because during the interview certain topics were outlined which we wanted to "speak" about and did speak about. The subsequent questions were treated with more depth at a later point.

Because it is difficult to understand exactly how this text arose, some afterthoughts regarding interviews per e-mail may be added here: the interview with Jaan Valsiner was somewhat more drawn out than we had originally supposed and planned. We think that the reasons for this are first of all organizational: during this time span, Jaan Valsiner stayed at different locations (he was in Germany two times, once in England, Sweden and he spent three months in Brazil) so he was not always available; he was busy with other projects at the same time and he asked us if we could continue the interview at another time which, in turn, did not always correspond to our time schedule. Some of the interruptions were due to technical reasons, but sometimes we also wondered what Jaan Valsiner's silence meant (or the other way around, his co-operation). We thought our questions (depending on what we were dealing with at that moment) were perhaps too general or too personal for him. It also appeared to us as if the initial enthusiasm did not last the entire time. Possibly sometimes our questions seemed unnatural to him, too external due to our scientific interests. Sometimes, his answers seemed abrupt and dispassionate to us. The insecurities could not be dealt with more or less directly, as is often possible in real life interviews and the result of this were "meta-mails" between the continents: "What has happened . . . with our interview? We wonder, if you are not happy about our questions—your answers are very short. Have you lost your interest in answering the questions?" Jaan Valsiner then responded promptly, indicating that everything was fine: "Nothing has happened . . . I am as happy with it now as I have always been. Only the last few questions were such that I felt no need for extended answers." These short answers and loops allowed us to express our insecurity. The fact that he subsequently pointed out to us that the reason he gave such brief answers was because of his involvement in other projects and contexts is what permitted us to continue with the interview. Without these insecurities and above all without the possibility of immediately trivializing the negotiations, it appears to us as though it is of great advantage to be able to exchange ideas over such an extended period of time and thus not have to react immediately. Furthermore, we were able to take time for subsequent questions and compare them with the already existing answers. These opportunities give this special interview form its distinctive appeal and charm. On the other hand, not only do the people asking the questions have a longer time to thoroughly think things over and work on them—the same holds true for the person answering the

questions. Sometimes, it can be very important and exciting to surprise or even irritate the interview partner with a certain question (and likewise, to be surprised or irritated by a counterquestion yourself) but as the e-mail-time elapses, it buffers surprises and irritations. We feel this apparently happened in those cases where Jaan Valsiner left his "territory". Sometimes we even had the feeling that by making the suggestion of conducting the interview by e-mail, he intended to do just exactly that. But perhaps we have merely constructed this idea about Jaan Valsiner: that in spite of being a very communicative person, he is, in fact, a lonesome thinker, an individualist in the truest sense of the word with all the positive and negative connotations which this entails and is also touched upon in some of the interview questions.

It appears to us that conducting an interview per e-mail can be carried on endlessly and not only because of the aforementioned peculiarities. There is always a next question and explanations can be "forced". A real interview is over at some point because all questions seem to have been asked, the participants have no more ideas or because they are merely exhausted. It appeared (and appears) to us that it is far more difficult to finish the interview form we chose. However, that the interview was finished is mainly pragmatic: we wanted to bring it to an end so we could concentrate on new projects. This step was not an easy one to take because while we were editing the interview, many new points for questions and discussions arose. (The mere answer of our first question about projects on which Jaan Valsiner is presently working on parallel to this one, could have led to more detailed subsequent questions than were generally asked.)

In addition to the already previously mentioned unendlessness of an e-mail interview, it also allows for something else: it opens the possibility of including others in leading the interview; something we only attempted in a limited way. It is possible to go far beyond the traditional framework of a two person discourse which could lead up to a scientific discussion. Perhaps this would do away with the "rules" that have to do with the roles pertaining to the interviewer and the interviewee. If this idea would be carried out even further, then the e-mail interview could at least contain a "virtual" aliveness and thus encourage other forms of scientific discourses. Our main fear regarding the e-mail interview was that the situation itself would be too artificial. We are surprised at how apparently successful we have dealt with this danger in a productive manner in spite of the aforementioned "interview difficulties". Indeed, that it was successful to this point could possibly be attributed to Jaan Valsiner. He used this frame-work, took up all the questions, turned them around every which way, thereby giving us the opportunity to participate fully and to some extent forget that we were separated by so many hours and miles.

This e-mail interview has been a memorable experience for us because it has, at least, made it possible to overcome these "barriers" intellectually. We hope the reader feels the same way. It has opened up new communication possibilities which were not clear at the time Jaan Valsiner came to Berlin and insisted on an e-mail address. Back then, it was "merely" a faster and more comfortable form of communication, but in the meantime it is being made full use of and has received another dimension because of this interview.

## 4 Notes

- 1 Together with Jens Asendorpf (Max-Planck-Institut für psychologische Forschung) Jaan Valsiner invited to the "Bernried Workshop" in summer 1989 in Bernried (see Asendorpf, J. B. & Valsiner, J. [Eds.] [1992]. *Framing stability and change: An investigation into methodological reasoning*. Newbury Park, CA: Sage).  
 In 1992 he organized in co-operation with Dietmar Görlitz and Hans Joachim Harloff (Technische Universität Berlin) the international conference "City as a frame for children's development" in Herten (Nordrhein-Westfalia). Beside a German publication *Entwicklungsbedingungen von Kindern in der Stadt: Praxisbeiträge der Herten-Tagung* [Developmental conditions of children in the city. Contributions on the practical nature from the Herten conference], edited by D. Görlitz, H. J. Harloff, J. Valsiner, B. Hinding, G. Mey, R. Schröder & U. Ritterfeld (Berlin/Herten: Stadtverwaltung Herten 1993), the theory centered discussion will be published in: D. Görlitz, H. J. Harloff, G. Mey, & J. Valsiner (Eds.) (1998). *Children, cities, and psychological theories: Developing relationships*. Berlin/New York: Walter de Gruyter.  
 In 1993 he arranged together with Alan Fogel from the University of Utah and Maria Lyra from the Universidade de Pernambuco (Brazil) a workshop in Serrambi, Brazil (see Fogel, A., Lyra, M. & Valsiner, J. [Eds.] [1997]. *Dynamics and indeterminism in developmental and social processes*. Hillsdale, NJ: Erlbaum.).  
 Likewise in 1993 he took part in organizing a series of seminars at Carolina Consortium on Human Development on the topic of "Comparative approaches to the study of development" (see Tudge, J., Shanahan, M., & Valsiner, J. (Eds.) (1997). *Comparisons in human development: Understanding time and context*. New York: Cambridge UP.).  
 In autumn 1995 he organized together with Helena Hurme (Abo Akademi, Finland) in Vaasa (Finland) a workshop on open-systemic nature of development and needs of methodology.  
 In 1996 he arranged together with Paul B. Baltes (Max-Planck-Institut für Bildungsforschung Berlin) and Lutz Eckensberger (Deutsches Institut für Internationale Pädagogische Forschung; Frankfurt on the Main, Germany) a symposium in honor of Ernst Boesch 80th birthday at the Max-Planck-Institut für Bildungsforschung in Berlin. (Contributions of the symposium are published in *Culture & Psychology* 3[3], 1998.)  
 Lastly, in 1997 he conducted with Ingrid Josephs (Otto-von Guericke-Universität Magdeburg) and Siegfried Hoppe-Graff (Universität Leipzig) in Leipzig a Humboldt-Stiftungs-Seminar-kolleg on "Dialogical models for the explanation of developmental processes."
- 2 Children's Environment Quarterly (until 1995); Early Development and Parenting; Journal of Social Distress and Homeless; Mind, Culture, & Activity; Journal of Human and Environmental Sciences; Human Development.
- 3 Beside some single work of Valsiner himself some publications result from collaboration with Laura Benigni and with Cynthia Lightfoot, and from conference activities with Angela Branco, too:  
 Valsiner, J. (1987). *Culture and the development of children's action*. Chichester: Wiley.  
 Valsiner, J. (1985). Parental organization of children's cognitive development in their home environments. *Psychologia*, 28, 131-143.  
 Valsiner, J. (1984). Construction of the zone of proximal development (ZPD) in adult-child joint action: The socialization of meals. *New Directions for Child Development*, 23, 65-76.  
 Benigni, L. & Valsiner, J. (1984). Il corpo del neonato e i suoi confini sociali. In L. Gandini (Ed.), *Dimmi come lo vesti* (pp. 89-135). Milano: Emme Edizioni.  
 Valsiner, J. & Lightfoot, C. (1987). The process structure of parent-child-environment relations and the prevention of children's nonintentional injuries. *Journal of Social Issues*, 43(2), 61-72.  
 Branco, A. U. & Valsiner, J. (1992). *Reading in contextualist perspective: A Vygotskian*

- approach*. Paper presented at the Symposium "Joint Book-reading and emerging literacy" at the Conference on Human Development, Atlanta, GA, April 10.
- Branco, A. U. & Valsiner, J. (1993). *Dynamics of social interaction strategies among young children: the emergence of cooperation and competition within structured contexts*. Paper presented at the XII. Biennial Meetings of the International Society for the study of Behavioural Development, Recife, Brazil, July 19-23.
- Valsiner, J. & Branco, A. U. (1994). *The study of the social interaction from a methodological co-constructivist approach: an analysis of coordination of goal orientations*. Paper presented on the Symposium "Evaluative Research on Social Interaction Process" at the 23rd International Congress of Applied Psychology, Madrid, July, 21.
- 4 Lightfoot, C. & Valsiner, J. (1992). Parental belief systems under influence: Social guidance of the construction of personal cultures. In I. Siegel, J. Goodnow, & A. McGillicuddy-DeLisi (Eds.), *Parental belief systems*. (2nd ed., pp. 393-414). Hillsdale, NJ: Erlbaum.  
The focus on parental beliefs continues in the 1990s in collaboration with Angela Branco; a new handbook chapter is in press.
  - 5 Gärling, T. & Valsiner, J. (Eds.) (1985). *Children within environments: Towards a psychology of accident prevention*. New York: Plenum.
  - 6 "The guided mind" will be published in 1998 (Harvard University Press) and entails empirical collaboration with Ingrid Josephs (Otto-von-Guericke-Universität, Magdeburg, Germany) and Seth Sorgan (Clark University; Worcester).
  - 7 Valsiner, J. (1985). Common sense and psychological theories: The historical nature of logical necessity. *Scandinavian Journal of Psychology*, 26, 97- 109.  
Valsiner, J. (1986). Transcending common sense in psychological theorizing: A developmental perspective. *Scandinavian Journal of Psychology*, 27, 184-189.
  - 8 Valsiner, J. (Ed.) (1986). *The individual subject and scientific psychology*. New York: Plenum.
  - 9 See chapter 7 of "The guided mind," and also chapter 3 in "Culture and the development of children's action" (2nd ed., Wiley).
  - 10 Rosa, A. & Valsiner, J. (Eds.) (1994). *Explorations in sociocultural studies. Vol. 1. Historical and theoretical discourse*. Madrid: Fundacion Infancia y Aprendizaje.; especially here the four following chapters:  
Rosa, A. & Valsiner, J., Introduction: An overture on history and theory (pp. 13-18).  
Valsiner, J. Reflexivity in context: Narratives, hero-myths, and the making of histories in psychology (pp. 169-186).  
Valsiner, J., James Mark Baldwin and his impact: social development of cognitive functions (pp. 187-204).  
Rosa, A. & Valsiner, J., Coda: discourse, meaning and knowledge - a reflection on the socio-cultural approach within the crisis of modernity (pp. 247-254).
  - 11 Valsiner, J. & Hill, P. E. (1989). Socialization of toddlers for social courtesy. In J. Valsiner (Ed.), *Cultural context and child development: Towards a culture-inclusive developmental psychology* (pp. 163-179). Toronto-Göttingen-Bern: C. J. Hogrefe and H. Huber.  
Valsiner, J. (1989). Collective coordination of progressive empowerment. In L. T. Winegar (Ed.), *Social interaction and the development of children's understanding* (pp. 7-20). Norwood, NJ: Ablex.  
Valsiner, J. (1989). *Human development and culture*. Lexington, MA: D. C. Heath & Co.;  
Winegar, L. T. & Valsiner, J. (Eds.) (1992). *Children's development within social context. Vol. 1. Metatheory and theory*. Hillsdale, NJ: Erlbaum.  
Winegar, L. T. & Valsiner, J. (Eds.) (1992). *Children's development within social context*.

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*Vol. 2, Research and methodology.* Hillsdale, NJ: Erlbaum.

- Valsiner, J. (1992). Co-regulation of interaction: goal-oriented redundant construction of development. *Human Movement Science, 11*, 481-487.
- Valsiner, J. (1994). Uses of common sense and ordinary language in psychology, and beyond: a co-constructionist perspective and its implications. In J. Siegfried (Ed.), *The status of common sense in psychology* (pp. 46-57). Norwood, N.J.: Ablex.
- Valsiner, J. (1994). Culture and human development: a co-constructionist perspective. In P. van Geert, L. P. Mos, & W. J. Baker (Eds.), *Annals of Theoretical Psychology*. Vol. 10 (pp. 247-298). New York: Plenum.
- Valsiner, J. (1994). Co-constructionism: what is (and is not) in a name? In P. van Geert, L. P. Mos, & W. J. Baker (Eds.), *Annals of Theoretical Psychology*. Vol. 10 (pp. 343-368). New York: Plenum.
- Vasconcellos, V. M. R. & Valsiner, J. (1995). *Perspectiva co-constructivista na psicologia e na educaao*. Porto Alegre: Artes Medicas.
- 12 Van der Veer, R. & Valsiner, J. (1989). Overcoming dualism in psychology: Vygotsky's analysis of theories of emotion. *Quarterly Newsletter of the Laboratory of Comparative Human Cognition, 11(4)*, 124-131.
- Van der Veer, R. & Valsiner, J. (1991). *Understanding Vygotsky: A quest for synthesis*. Oxford: Basil Blackwell.
- Van der Veer, R. & Valsiner, J. (Eds.) (1994). *The Vygotsky Reader*. Oxford: Basil Blackwell.
- 13 Valsiner J. (1989). Environmental psychology in the Soviet Union. *Studies in Soviet Thought, 37*, 61-71.
- Valsiner, J. (1991). David Joravsky. Russian psychology: A critical history. *American Historical Review, October*, 1241-1242.
- Valsiner, J. (1992). Vygotsky's sociohistorical psychology and its contemporary applications, by Carl Ratner (book review). *Early Development and Parenting, 1*, 56-58.
- Valsiner, J. (1994). From energy to collectivity: A commentary on the development of Bekhterev's theoretical views. In L. H. Strickland (Ed.), *V. M. Bekhterev's Collective Reflexology* (pp.xiii-xxiv). Commack, NY: Nova Science Publishers.
- Valsiner, J. (1996). Social utopias and knowledge construction in psychology. In V. A. Koltsova, Y. N. Oleinik, A. R. Gilgen & C. K. Gilgen (Eds.), *Post-Soviet perspectives on Russian psychology* (pp. 70-84). Westport, Ct.: Greenwood Press.
- 14 "Understanding Vygotsky" appeared in Portuguese translation [Vygotsky: uma sintese. Sao Paulo: Edicoes Loyola] in Brazil in 1996.
- 15 "The social mind" will be published in 1998 (New York: Cambridge University Press).
- 16 In 1938, for example, Arnold Gesell made two films:  
*Prone progression in the human infant*. New Haven: Photographic Library, Yale Clinic of Child Development (35mm- and 16mm-film).  
*The tonic neck reflex in the human infant*. New Haven: Photographic Library, Yale Clinic of Child Development (16mm-film).
- 17 Valsiner, J. (1987). *Culture and the development of children's action*. Chichester: Wiley.
- 18 Conference of the International Society for the Study of Behavioural Development
- 19 Valsiner, J. (1984). *The childhood of the Soviet citizen: Socialization for loyalty*. Ottawa: Carleton University Press.
- 20 ZFM = zone of free movement, ZPA = zone of promoted action, ZPD = zone of proximal development

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- 21 The questions which are marked with \* are suggested by Carl Ratner (Humboldt State University, California; U.S.A.). The contact to him grew out of a meeting when he stayed in Berlin at the conference of the International Society of Theoretical Psychology. The question whether he was interested in participating in the interview ensued by e-mail in August 1997.
- 22 The question marked with \*\* was formulated by Rolf Oerter (Ludwig-Maximilians-Universität, Munich, Germany). Günter Mey met him at the 13. Tagung Entwicklungspsychologie [13. Conference Developmental Psychology] in Vienna in September 1997.
- 23 See for explanation chapter 5 in *Culture and the development of children's action* (1987) or chapter 3 of the second edition (1997).
- 24 The questions marked with \*\*\* were asked by Alfred Lang (Universität Bern, Switzerland). Günter Mey met him when he stayed in Berlin at the Ernst-Boesch-Symposium. The question whether he was interested in becoming involved in the interview arose by e-mail in August 1997.
- 25 In "The guided mind"; see note 6.
- 26 Valsiner, J. (1989). On the glory and misery of sociobiological perspectives: A selfish book review. *Developmental Psychobiology*, 22, 413-417.
- 27 In "The guided mind"; see note 6.
- 28 Valsiner, J. (1982). *Strategies of dyadic problem-solving with infants in a simulated laboratory situation: Adults' strategies of baby-calming*. Paper presented at the International Conference on Infant Studies, Austin, Texas, March, 18.
- 29 See note 23.
- 30 Gigerenzer, G., Swijtink, Z., Porter, T., Daston, L., Beatty, J., & Krüger, L. (1989). *The Empire of chance: How probability changed science and everyday life*. Cambridge: Cambridge University Press.
- 31 Smith, J. A., Harre, R., & Van Langenhove, L. (Eds.) (1995). *Rethinking psychology*. London: Sage.  
Smith, J. A., Harre, R., & Van Langenhove, L. (1995). *Rethinking methods in psychology*. London: Sage.
- 32 Porter, T. M. (1986). *The rise of statistical thinking, 1820-1900*. Princeton: Princeton University Press.
- 33 Valsiner, J. (1984). Two alternative epistemological frameworks in psychology: The typological and variational modes of thinking. *Journal of Mind and Behavior*, 5, 449-470.
- 34 See note 7.
- 35 Valsiner, J. (1995). Editorial: Culture and Psychology. *Culture & Psychology*, 1, 5-10.  
Valsiner, J. (1996). Editorial: After the first year. *Culture & Psychology*, 2, 5-8.
- 36 Klaue, K. (1989). Child development revisited by a dissenter. An interview with Jaan Valsiner. *New Ideas in Psychology*, 7, 331-344.
- 37 Michael Haller made interesting remarks about journalistic interviews, especially about interviews which are published in the the weekly German magazine "Der Spiegel", see *Das Interview. Ein Handbuch für Journalisten* [The interview. A handbook for journalists]. Munich: Ölschläger (1991). In addition, see his short essay "Wir danken Ihnen für dieses Gespräch" [Thank you for the interview] in the special edition "50 Jahre Der Spiegel. 1947 - 1997" [50 years Der Spiegel. 1947 - 1997].