The Social Mind

Construction of the Idea

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CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge, United Kingdom

CAMBRIDGE UNIVERSITY PRESS
The Edinburgh Building, Cambridge CB2 2RU, UK http://www.cup.cam.ac.uk
40 West 20th Street, New York, NY 10011-4211, USA http://www.cup.org
10 Stamford Road, Oakleigh, Melbourne 3166, Australia
Ruiz de Alarcón 13, 28014 Madrid, Spain

First published 2000

Printed in the United States of America

Typeface Palatino 10/13 pt. *System* DeskTopPro$_{/UX}$ [BV]

A catalog record for this book is available from the British Library.

Library of Congress Cataloging in Publication Data
Valsiner, Jaan.
The social mind : construction of the idea / Jaan Valsiner and René van der Veer.
p. cm.
Includes bibliographical references and index.
ISBN 0-521-58036-6 (hb). – ISBN 0-521-58973-8 (pbk.)
1. Intellect – Social aspects. 2. Intellect – Social aspects – History. 3. Discoveries in science – Social aspects. 4. Inventions – Social aspects. I. Veer, René van der, 1952– . II. Title.
BF431.V25 2000
302—dc21 99-28458
CIP

ISBN 0 521 58036 6 hardback
ISBN 0 521 58973 8 paperback

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CHAPTER ONE

The Development of Ideas in Science: Intellectual Interdependency and Its Social Framework

In this chapter we outline a general scheme of *intellectual interdependency*. Our coverage of the issue is based on the assumption that new understanding of phenomena in science is actively constructed by intentional persons, who are involved in a field of mutually communicable meanings, or ideas. Within this field, persons act in a goal-oriented manner: communication is directed toward personally desirable possible future state of affairs. Each of the persons is unique, and constructs knowledge from the basis of personal uniqueness, yet in ways that are related both to the interpersonally communicable ideas, and to the nature of the object world of the given science. These persons are also members of different social institutions, and assume social roles that are set up by these institutions. Thus, we try to make sense of a threefold relation: – *social institutions, scientists, nature* – of the object of the given science. Our focus in the latter case is the issue of humans (scientists) making sense of fellow humans. This is the crucial epistemological problem for the social sciences, where the distance between the subject and object of investigation is essentially absent. A social scientist who looks at another person (or social phenomenon, like social class, gender, etc.) inevitably can't escape the obvious fact that the roles of the researcher and the research participant are always close to being reversible. It often happens that the research subject investigates the thinking of the researcher, while not providing much evidence about one's own. Thus, the researcher is constantly under the uncertainty about his or her control over the research encounter. Furthermore, the closeness of the researcher and the research participant makes it easy to project into the other one's implicit assumptions about perspectives that can be taken (or ought to be taken, from the researcher's viewpoint) upon an object of inves-

tigation. The closeness of social sciences' research to societal sociopolitical or sociomoral local imperatives creates a framework for intellectual interdependency where particular decisions by scientists become implicitly guided in directions which emerge from the researcher being ''too close for comfort'' to the research participants. The issue of intellectual interdependency is undoubtedly complicated by that closeness.

Intellectual Interdependency as Constructive Communication

Intellectual interdependency entails *a process of construction of ideas* by persons, while *that construction of ideas is aimed in a selected direction* in the communicative process with other persons. The other persons are involved in a similar communicative construction of ideas, communicated with some orientation toward goal states. Intellectual interdependency is thus a state of affairs in the process of purposive communication efforts by persons and institutions, in which the constructed ideas are transformed into new forms. Such creation of novelty is possible due to persons' constructive internalization and externalization processes. Communication takes place between participants who are necessarily assuming different positions, and create their potentially different goal orientations. The state of intersubjectivity that emerges in communication can be transitory and often is purposefully illusory, since it is a constructed artifactual basis for maintaining the communication flow.

The communicative process entails construction and use of signs – in this sense, it is a semiotic process. A particular insight (X) of person A becomes externalized by him or her in the form of a semiotically encoded message (X′), and thus made publicly available to other persons. Among these other persons, X′ may be ignored by many, neutralized or downplayed by some, and taken seriously by a few others. Among the latter, X′ may become transformed in the internalization process and become different (e.g., person B turning X′ into Y), followed by constructive externalization of Y into a novel publicly available form (Y′), which in its turn is a message for A and other persons.

This process continues ad infinitum, constantly producing novelty. The interdependency notion emphasizes the developmental continuity between persons (A and B) and their ideas (X and Y), which are linked through externalized communicative messages (X′ and Y′). Intellectual interdependency is the process of construction of new ideas through

the transformation of old ones in a communicative process. As such, intellectual interdependency is a universal human phenomenon.

Our elaboration of intellectual interdependency is based on the notion of *bidirectional culture transfer* (Valsiner, 1989, 1994c); a view on human communication that is built on the notion of active construction (and reconstruction) of cultural messages by individual persons. This axiomatic notion is based on Karl Bühler's Organon model of communication (Bühler, 1934/1990), and involves a focus on the novelty constructed in the domain of abstractive generalization. Much of scientific discourse – as well as of sociological discourse about what is ''appropriate'' in ''scientific discourse'' – is devoted to questions of acceptability, or unacceptability, of generalization, and to the issue of the value of abstract concepts in the reasoning of scientists.

In the history of the social sciences, opposite tendencies of either eliminating abstraction and generalization from scientific discourse (e.g., focus on ''knowledge'' being ''local'' or ''context specific'') have alternated with tendencies to create high-level (often mathematical) abstractions that have lost all direct connections with the common-language meaning systems. Our communicational perspective on intellectual interdependency focuses on the inevitable uncertainty about the abstractness of any scientific communicative message. The same concept – X – as abstracted from its common-language ''parent'' (x), enters into a tension between the remaining set of connotations of the common language on the one hand, and the ideal infinite abstractedness (with potential for generalization), on the other. Thus, the everyday notion of ''attachment'' (a mother's feeling toward her child, or vice versa) continues to disallow extreme abstraction of the scientific concept attachment (used by psychologists who study that topic). On the other extreme, the notion of attachment could be defined purely abstractly (e.g., attachment of function F to that of W), without tension between the common language.

The present perspective does not overlook the role of collectively available means of communication – sign systems. Rather, it indicates how such ''shared'' systems are put to constructive practice by people. One person may construct a cultural message, directed at another, in his or her personal, idiosyncratic way, yet utilizing common cultural tools (e.g., commonly understood words of a language) for such encoding. The other person, while receiving that message, does not accept it as a ''given,'' but instead actively reconstructs it in accordance with his or her own personal position. That reconstruction

entails analysis of the message and its synthesis into a new (personal–cultural) understanding, which may be very different from the one intended by the creator of the message.

From here it follows that intellectual interdependency in the history of scientific ideas is not a tale about how equivalent (or similar) ideas are accepted and proliferated by communities of scientists, social institutions, and laypersons. Instead, it is a glimpse into the dialogical process of knowledge construction, in which scientists are involved in dialogues within themselves (Hermans, 1995), and with others. These dialogues do not entail the simple acceptance of ideas, but rather tension-filled processes of relating of different ''voices'' (Wertsch, 1997) or positions (Rommetveit, 1992). The definitive issue involved is the construction of new understanding on the basis of what was previously achieved by the given discipline. This process involves the proliferation of similar versions of understanding both as a result of the construction of novel ones, and as a basis for (still more) novel understanding. Furthermore, such construction of novelty is part and parcel of the goal orientation of scientists: ''The goals of scientific practice are imaginatively transformed versions of its present. The future states of scientific culture at which practice aims are constructed from existing culture in a process of *modeling* (metaphor, analogy) . . . the existing culture predisciplines the extended temporality of human intentionality'' (Pickering, 1995, p. 19).

The notion of science as the constant construction of new – yet predisciplined – understanding of phenomena is crucial here. This future orientation has its side effect of dismissing the history of the discipline as part of the construction of the new. Scientists are constant producers of novelty, even when they repeat (and often fight for) meaningful constructions that have been worked out by others. The latter can occur in a generalized form of an interpersonally accepted matrix of meanings within which to look for solutions (outlawing other matrices in which solutions are not to be sought). At a particular historical period of a science, different scientists may arrive at similar solutions to a basic problem. These phenomena – the feeling that ''this solution was just in the air'' (in some science, during some historical period) – are a case of intellectual interdependency where the scientific (or wider) community was oriented in a specific direction in its efforts to arrive at novel understanding. Often such phenomena can be directed by wider social processes in the given society (e.g., the construction of various kinds of ''activity theories'' in Soviet psychology under

the ideological orientation towards the Marxist notion of practice; Valsiner, 1988).

Assumed Intersubjectivity and Intellectual Interdependency

Intellectual interdependency is possible on the basis of socially shared intersubjectivity (see Rommetveit, 1992) of scientists within their community, as well as on the basis of the establishment of domains of intersubjectivity between the scientific communities and laypersons' or bureaucracies' discourses. The central focus is on the construction of the understanding of some (target) phenomenon by individual scientists within a loosely defined social group (the ''scientific community''), within social contexts of such intersubjectivity. The constructor of the new knowledge is necessarily a human being, a creative person, who may indeed be very much intertwined with his or her intersubjective world. In other terms, the role of a particular scientist in providing novel solutions is enhanced (rather than dismissed) by the social embeddedness of that scientist. One scientist may take a notion of others (X′, see above) and proliferate it as if it were given (X′ = X′). Another may take the same notion X′ and turn it into its opposite (Y′). A third may spend his or her career proving that X′ is ''wrong'' and finding many reasons why this is so. For example, consider our contemporary habit of deconstructing different views in psychology. A deconstructionist fails to provide new understanding, but can succeed in reorienting the field toward the rejection of the old one. The scientists in a given field may become active in tearing down the previous ways of creating knowledge, but fail to create new ones. All this (and greater) variety of relating to the previous notion (X′) is included in the notion of intellectual interdependency. Followers and critics, skeptics and propagandists, deconstructors and reconstructors, are all in relations of intellectual interdependency with their target understandings in a particular science. Within this corpus of knowledge, all of these different positions can be claimed to operate within the system of assumed intersubjectivity. In order to deconstruct a particular scientific tradition, it needs to be assumed that the deconstructor understands at least the basics of the object of such effort, sharing this with the proponents of the deconstructed tradition. As has been shown (Markova, 1994; Rommetveit, 1992) the assumption of intersubjectivity is a necessary, productive (but counterfactual) construction that enables the communication

process to proceed. At times, such intersubjectivity is achieved by the use of general concepts of an open-ended nature (e.g., the ''self'' vs. ''non-self'' contrast in immunology, borrowed from William James's psychology; Löwy, 1992). Intellectual interdependency between scientists is based on the assumption of a shared ''common ground,'' even if in reality the ground is not common at all. Instead, it is a heterogeneous field of varied personal positions, guided by the social institutionalization of sciences.

Realms of Intellectual Interdependency in Science

All sciences are both knowledge constructive and social-institutional systems. It is because of the latter that different realms of intellectual interdependency need to be elaborated. Thus, interdependency may be found between scientists interindividually (e.g., Dr. Smith thinks that Dr. Jones understands his or her argument), between scientists of a given discipline and these of another discipline (i.e., ''interdisciplinary'' relations; e.g., Dr. Smith thinks that physicists understand her ideas better than fellow psychologists), and between scientists as such (a social-institutional structure) and other social institutions within societies (e.g., Dr. Jones applies for government funding, persuading the appropriate bureaucracy that his project deserves it). This parallel multiple intellectual interdependency situates our analysis in this book in the realm of relations between the process of construction of ideas on the one hand, and the sociopolitical guidance of that construction, on the other.

Psychology may be a discipline in which the tension between those two sides – those of the scientist and his/her social context – are experienced in an extreme fashion. On the one hand, psychology as science strives toward constructing basic knowledge on the basis of ever-specific particulars of an empirical kind. At the same time, psychology is built on numerous sociomoral value presuppositions (Cirillo & Wapner, 1983) that make some research questions and ways of knowing adequate for the discipline, while ruling out possible alternatives. Thus, research on why children drop out from U.S. schools is built on the assumption that such drop-out is a sociomoral problem for the given society (at the given historical time). The opposite idea – that the schools are of such quality that the best adaptation for enterprising children would be to drop school – is not only weird, but morally wrong. However, the question of how adolescents may ''drop

out'' from their neighborhood drug-using gangs would be a morally legitimate research issue. When a similar issue is played out on stockholders' ''dropping out'' from backing a particular company (by selling its stock, in mere anticipation of its failure), that can be hailed as a positively valued business decision.

The moral evaluation is the reverse: the free-market ideology is valued in the one decision and denounced in the other. Psychological problems are formulated in a socially predisciplined way, which – as it is implicit in the background of the research questions – creates a tension for psychology as science. Psychology – for all of the time of its independent existence – has been torn to pieces (almost literally; divided into various sub-areas) between ''natural science'' and ''sociomoral ideology.'' Because its object of investigation is both a biological species and a cultural self-organizer, it cannot escape this tension by rejecting either of the two poles that create the tension. The alternative, a synthesis of the two at a superordinate level, has become difficult to construct in the context of the proliferating fragmentation of knowledge (supported by the ideology of empiricism), and the irreconcilability of different underlying sociomoral sentiments.

Science and Common Sense: The Role of Empiricism

In the case of the social sciences, particularly psychology, the issue of intellectual interdependency is crucial for the science if it tries to transcend the limits of common sense (and language; Valsiner, 1985, 1994b) and move beyond being a game of pseudoempiricism (Smedslund, 1995). According to Smedslund, most of the results of the empirical investigations in psychology are actually expressions of ''given truths'' already encoded into our thought through language.

Smedslund argues that the world is known to us through the acceptance of the logic of the psyche: *psychologic* (Smedslund, 1997). This logic is encoded in the language used. Thus, meanings of particular words (e.g., bachelor) set up implications that are necessarily true (e.g., a bachelor is a man who is not married), and do not require empirical evidence for us to begin to know that these implications are true. For instance, in order to find out if bachelors are unmarried men, we do not need to question an increasingly large number of unmarried men about whether they are bachelors. Empirical efforts of the latter kind are examples of pseudoempiricism in Smedslund's critical account. Claiming that it is necessary to prove empirically what we

know to be true by the meaning system of our language is pseudoempirical. For example, the empirical demonstration that men are showing higher levels of masculinity than women (and vice versa for femininity) is pseudoempirical. In their practice of ''doing science,'' psychologists are involved in an extensive self-fulfilling prophecy – empirically demonstrating the obvious – rather than constructing new knowledge. Pseudoempiricism dominates psychology, despite its obvious irrelevance.

How can such a state of affairs dominate a particular discipline? Wouldn't such a pseudoempiricistic production of information about what we know already lead to the extinction of the given area of science? Perhaps, pseudoempiricism can be utilized in psychology because of the different social functions that psychological research evidence carries, due to the discipline being caught in-between the natural sciences and sociomoral ideologies. Part of the ideological function of psychological empirical evidence is to provide the ''scientific halo'' for obvious common-sense truths. This is valued by social institutions, which can utilize such pseudoempirical evidence for the social legitimization of their political actions, now based on ''scientific evidence.''

If our description is adequate, then – at the level of the work of individual scientists – we can see discrepancies between the parallel communication channels of intellectual interdependency. A scientist can agree that a given project – involving a large number of subjects – is pseudoempirical. Yet he or she may do the study anyway, citing the need to communicate to his or her peers that the work done is ''trustworthy,'' rather than ''mere speculation.'' The peer-peer institutionalized communication channel (which regulates the boundaries of the notion of science in psychology) here overrides the interdependency of ideas between individual scientists. In terms of the social psychology of construction of norms (Sherif, 1936), this situation is to be expected. Social scientists build their norms of what amounts to ''science'' in their everyday research practices similarly to persons (in autokinetic experiments of Sherif) decided about the movement of light dots – by reaching consensus about inevitably personal positions. A consensus can reflect shared understanding about a clearly specifiable object – in which case consensual validation improves our individual understandings by social referencing. But equally possible, in the case of objects which are not immediately perceivable, is a scenario according to which consensus leads to construction of shared (and,

hence, socially fortified) illusions. Thus, pseudoempirical studies in psychology can be viewed as ''breakthroughs,'' the reviewers of the research results may praise the large work done, and hail the conclusions, yet fail to see that the conclusions are unconnected with the research results. The logical imperative of the interpretation becomes masked *as if* the interpretation emerged from the data, whereas in reality the data give an analogous picture precisely because the imperatives for the researchers' interpretation were the same as the ones for the many subjects tested. Furthermore, a governmental agency may institute a policy of providing research funds for projects with large numbers of participants. Again, a pseudoempirical trajectory for research is set into function. Intellectual interdependency between thinkers is canalized by local social norms of institutions as to what is admissible as scientific knowledge.[1] The definition of the boundaries of a discipline is a social-institutional enterprise, carried out by persons in their appropriated social roles.

In terms of intellectual interdependency, pseudoempiricism is but one indication that the system of thought called ''science'' (or *Wissenschaft*; compare with the weird-sounding verb of ''knowledg*ing*'' = constructing knowledge) is an intricate web of meaning-making activities, which involve understanding and ignorance side by side. The scientific meaning construction is filled with the use and invention of hierarchies of semiotic devices, as well as symbolic practices. The latter are at times publicly displayed, at others carefully hidden from the public view. During some historic periods political leaders (or parties) may interfere directly in the affairs of science, at other times such intervention may be hidden behind slogans of ''taxpayers' right to know,'' and sometimes the sociopolitical system leaves scientists to their own devices. The intellectual interdependency of ideas in science is embedded in the web of social-personal interdependencies of the makers, carriers, and users of these ideas.

[1] As is often the case, such local norms may be upheld by a religious fervent of righteousness. Advisors may crush the ''wrong ideas'' in their doctoral students, or senior figures in a particular field may try to force younger colleagues to ''return to the right path'' (e.g., see Mahoney, 1989, p. 140). Fortunately, such social regulation of ideas in science is doomed to fail, as scientists, historically ranging from Galileo to Soviet geneticists, have repeatedly demonstrated.

Elaboration of the Roots of Intellectual Interdependency

It can be said that the issue of intellectual interdependency in science has haunted us in many ways, while we were working on the history of ideas in psychology. The intellectual interdependency was there in the case of Soviet developmental psychology at large (Valsiner, 1988), as well as in the case of Lev Vygotsky (Van der Veer, 1984; Van der Veer & Valsiner, 1991). Whenever we tried to make sense of ideas in what was charted out to be ''Soviet psychology'' or ''the genius of Vygotsky,'' we would find an intricate web of intellectual interdependency with others. Hence, the need to make sense of that notion in general.

Back in 1988, we started by noting similarities between sociogenetic epistemology in general and the development of scientific ideas:

> There is an interesting parallel between the sociogenesis of children's thinking and the issue of intellectual influence in scientific discourse. Children are no passive copiers of adult behaviors or passive recipients of ready-made cultural tools. They try to make sense of their environment, test hypotheses, integrate the results of these tests into their 'body of knowledge,' and actively master cultural tools (sometimes putting them to new use). The same is true, in a much more deliberate and systematic fashion, for mature scientists. It is obvious that the theoretical thinking of a good scientist cannot be reduced to the sum of influences undergone by that person. First, that would amount to denying the active role of individual scientists transforming ideas in various subtle ways . . . Second, scientists actively select their sources of influence. At the basis of this selection process are both theoretical considerations and results of empirical investigation . . . Investigators should be seen as active co-constructors of gradually developing ideas. (Van der Veer & Valsiner, 1988, pp. 61–2)

The appeal of the comparison of scientists with children depends upon the particular valuation of children in the personal culture of the evaluator. If that is of the kind in which children are viewed as inexperienced persons who need to learn the know-how of the world, then our comparison may read as an insult. If, in contrast, the evaluator considers positively the inquisitiveness, experimenting energy, and openness to new ideas that can be seen to characterize children, our comparison may be taken as a compliment. We have emphasized the latter interpretation: looking at scientists as children is a compliment to the youthful energies of scientists, who, even in old age, remain inquisitive about the world. The loss of childlike playful inquiry

would amount to the end of knowledge construction. It is there where the *process of doing science* becomes transformed into social discourse *about* science, with all the ritualistic parapharnelia (ranging from Nobel prizes to decisions about what ''appropriate'' doctoral dissertation is like in the given discipline) being tools for the latter.

The tension-filled complexity that is the basis for our intellectual interdependency notion is not overlooked by other analysts of the processes of science. Pickering's (1995, pp. 22–3) ''mangle of practice'' emphasizes the dialectical relation of resistance and accommodation to new ways of acting (and thinking) in science. Pickering's theoretical construction allows him to concentrate on the real-life practices of scientists. In contrast, our effort here is to reconstruct the realms of developing scientific thought (rather than practices).

Multiple Participants and Multiple Goals

It became clear to us that intellectual interdependency entails multiple participants: not just scientists but also their grandmothers, their doctoral students, research assistants, or other laypersons, social institutions (universities, research institutes, popular media, governmental and private agencies), and scientific institutional categories called ''disciplines.'' All of these participants in the process of intellectual interdependency act in a goal-oriented manner. Scientists want to solve selected problems in their knowledge domains (as well as secure their employment). Scientific institutions attempt to guide disciplines and scientists in directions of their interest. Different disciplines try to maintain (and others gain) their symbolic power positions (e.g., psychology pretending to be a ''hard'' or ''natural'' science, rather than a ''soul science'' or *Geisteswissenschaft*).

Such multiple participation can be documented through the study of communicative messages that exist in the knowledge construction process. Different kinds of narrative forms are used for different goals (Valsiner, 1994a). Frequent rhetorical efforts to specify ''where the science is heading'' or ''where should it develop'' indicate the self-interested, goal-oriented discourse about science. Social institutions make public claims (and encode those into their actual funding practices), making claims about where sciences should go in the present and in the near future. This amounts to the goal-oriented social guidance of different sciences, where the reasons for the promotion of one

or another direction of a science are built on extra-scientific (economic, sociopolitical, etc.) grounds.

The academic world of the end of the twentieth century may be witnessing its own eradication (or at least the elimination of the historically developed relative autonomy from the sociopolitical world), all under the rhetoric niceties of ''making science accountable'' to governmental bureaucracies, and/or making it ''applied'' so that modern multinational corporations could appropriate it. The construction of basic knowledge by social institutions of science per se can become under siege from two sides. Efforts to control the directions by governmental institutions (of any country) and selective ''buy out'' of some directions of science by the contemporary global economic power (multinational corporations) may result in the loss of the relative autonomy of sciences from other institutions of societies that have been the benchmark of sciences since the Middle Ages. Becoming ''socially *accountable*'' for sciences means simultaneously becoming socially *controlable* by other social institutions, and, consequently, becoming politically driven.

As we here show, intellectual interdependency in science is not just a purely personal and intellectual phenomenon. It is simultaneously based on social power relations between the given science and the sociopolitical texture in which they are embedded. Nuclear physicists were heroes at the time of social utopias about new energy resources (followed by the nuclear bomb), and have lost that ''halo'' at a time when even mere transport of nuclear waste by rail or ship evokes explosive social protests. And psychologists, who after they had created their theoretical ideas, became tainted by either ''immoral'' (e.g., Watson, or Baldwin, in Chapter 4) or ''wrong'' political affiliations (e.g., Krueger, see Chapter 7) can be easily forgotten within their disciplines. Others, whose political affiliations have not been marked by moral condemnation (e.g., the history of A. N. Leontiev's activity theory in Soviet psychology, or Francis Galton's and Karl Pearson's contributions to statistics – separate from their hopes for eugenic purification of society) fare well in retrospective and politically corrected accounts of the history of the discipline. The sociopolitical side of human life is necessarily in the background of our construction of historical narratives and counter-narratives (Ahonen, 1997; Luczynski, 1997). The same sociohistorical event becomes narratively constructed in accordance with the direction of desirability that a particular social

institution ascribes to it. The person who encounters such narrative constructions can cordinate different stories (e.g., narrative and counter-narrative), if such dialogicality is in place. Yet one of the goals of narrative constructors of a story can be the eradication of the possibility for such dialogicality – the privileged (by a social institution) an account may be created in ways that disallow the construction of its opposing narrative. In some cases of historical narratives the latter has succeeded (in the U.S. – see Wertsch, 1997), in others failed.

Narrative construction of historical accounts in science has similar nature of it. Hero myths are created about scientists, while others become ''counter-heroes.'' For example, the simplification and stigmatization of the developmental ideas of Jean-Baptiste Lamarck began already in his lifetime, and has continued to our days (Burkhardt, 1984). Images of the ever-increasing length of the neck of the giraffe are easily evoked when Lamarck is mentioned, and bold developmental thinkers at times have to prove to their scientific colleagues that they ''are not lamarckians.'' The socially constructed counter-myth about Lamarck's ideas as ''not adequately scientific'' has eliminated the interest in these ideas. Yet the myth around Darwin is a positively valued story of a grand breakthrough in evolutionary biology. Similar myths have been constructed around the life and work of Lev Vygotsky (e.g., that he studied mother–child interactions) as we have pointed out elsewhere (Van der Veer & Valsiner, 1991). To summarize, discourse about science is not merely talking about science in its actual reality (of the process of knowledge creation) as it is: It involves talking about science from the position of whoever does the talking. The need to do such talking is often sociopolitical and does not contribute to science itself. This discourse is meta-scientific (discourse about science's discourse), and since the positioning of the participants in that talking is variable, we can describe it as multivoiced or polyphonic.

Discursive Battlefields: Why So Much Fuss about Wording?

The polyphonic nature of any scientific and meta-scientific discourses makes them necessarily heterogeneous, and value-laden. In contrast with the iron-clad image of the ''rationality'' of scientists – a message that is proliferated between sciences and the rest of society – scientists are human in being passionately devoted to their pet ideas.

Inside disciplines as wholes, the issue of semiotic codes for constructing knowledge is often non-neutral (see Thompson, 1993; on moral rhetoric embedded in economists' discourse about causality). While the issues of particular explanations (or descriptions) are being disputed in scientific discourse, in the background may lure implicit moral preferences about the subject matter being explained.

The whole vocabulary of the given direction in a discipline can become a discursive battlefield for the development of the given discipline. Psychology's conventionalization of ways of talking (e.g., frowning at the use of the plural ''thoughts,'' while accepting its synonym ''cognitions''; or contrast between ''observers'' with ''subjects'' and with ''research participants'') flavors the way of making sense of the issues from an ideological perspective. Issues about building a universal scientific terminology for psychology have been high on the agenda of intradisciplinary dialogues. For example, at the 6th International Congress of Psychology in Geneva, Claparède (1910) called for the definitive setting up of a nomenclature of psychological terminology. Recommendations by a special terminology commission called for an austere simplicity of terminological equivalents between languages (Baldwin, 1910). Efforts to emulate chemistry in the construction of a symbolic system were revealed in a proposal for a new sign system (Courtier, 1910). Construction of unified terminology led to heated discussions about the potential of Esperanto as a language that could unify psychology (de Saussure, 1910).

The major dispute of psychology in the course of the twentieth century has been the opposition between uses of terminologies with mental and non-mental implications (cf. Vygotsky, 1926b/1997). Starting from the North American context (see Chapter 5) and proliferating worldwide, the notion of behavior has been a consensually accepted and vigorously defended (and attacked) concept. As Carl Graumann has observed,

> The ease with which the superfluous word 'behavior' could, and still can, be added to any other word designating animal and human activities (from crowding to milling, from dating to mating, from littering to energy-saving behavior) is at least indicative of the belief in the ubiquitous potency of psychology alias behavioral science. . . . Behavior . . . originally was, and in the educational field still is, a moral concept. In its originally reflexive form it meant to conduct oneself in a proper manner, that is, according to moral standards. Only by virtue of this meaning does the imperative ''Behave!'' make any sense (Graumann, 1996, p. 88).

Building psychology on the root term of behavior eliminated reflexivity from further consideration (animals behave, rather than think), and the complex nature of the phenomena was lost (as behavior, in the generic sense, was turned into analyzable discrete units of the observed phenomena: ''behaviors''). The role of the behaviorist consensus in the American psychology of this century was based on the moral imperatives of pragmatist philosophies of different kinds, meant to take the place of religious belief systems (e.g., James, 1907, p. 301)

Social canalization of psychologists' thinking moved further in the 1930s, with the introduction of discourse about ''variables.'' The previous ''stimulus'' now became ''independent variable,'' while ''response'' was translated into ''dependent variable.'' This

> gave the language of dependent and independent variables a greater apparent degree of theoretical neutrality than the language of stimuli and responses. . . . Different interpretations of what variables represented were permissible, as long as all psychologists agreed that the units of their investigative practice were ''variables.'' Second, the language of variables could accommodate the practice of psychologists who were engaged in establishing correlations between measures – for example, personality traits – that had not been experimentally manipulated and hence were not expressible in the language of stimulus and response (Danziger, 1996, p. 23).

Thus, psychology's discursive battlefield first barred psychologists' thinking from the possibility to explain phenomena in mentalistic terminology, and consequently guided it into a pseudo-physicalistic discourse about ''variables.'' The latter is still accepted, despite the blatant mismatch between the implications of the meaning of ''variable'' (= something to be varied) and the indexical nature of psychologists' use of the term. As a result, psychology could create an image of ''natural scientificalness'' for itself (and for outsiders), while remaining internally a sociomoral discipline (Maiers, 1988). Further differentiation of the discursive battlefields in the discipline brought back the mentalistic explanatory terminology (through the ''cognitive revolution''), yet in a form that maintained distance from the commonsense mentalistic expression (e.g., ''cognitions'' versus ''ideas,'' ''affects'' versus ''feelings''), and in ways that maintained the terminology of ''variables'' (e.g., ''cognitive variables'').

The social fights about the prescriptions for, and meanings of, ex-

planatory terms used in contemporary psychology have a basic theme: It is the fight between maintaining the ''common-language-nearness'' of the terminology, or abstracting from it. This theme is known in the history of other sciences as well. Notions like ''force,'' ''horse power,'' ''purity'' (of chemicals), the permanence of the substance in lieu of a change in its form (Crosland, 1995), chemical ''reaction'' (Holmes, 1995) in the history of the physics and chemistry of the seventeenth and eighteenth centuries indicate a constant fight for overcoming animistic or moralistic (i.e., common-sense) reasoning and the organismic view on non-organismic substances (Bensaude-Vincent & Stengers, 1996; Klein, 1995). At the time, the chemical science struggled to overcome the common-sensical and perceptually immediate nature of the relation of alchemy to the chemical substances. Psychology has been struggling with similar issues throughout the twentieth century. Once these disciplines succeed, general science is born out of sociocultural knowledge complexes. The latter are certainly a domain for the struggle for dominance between scientific and political institutions.

Cultural Systems of Knowledge in Construction

Sciences (and scientists) operate in their particular social contexts, and their intellectual interdependency is constrained by these contexts. A scientist's thinking is always integrative of the habits of the common language, rules of the given science, and voices of his or her colleagues. History of science has not been profoundly cultural-psychological in the past, even if it needs to be. As Renn has remarked,

> The texts of the individual authors which are usually in the center of attention of historians of science only reflect very specific aspects of the socially available knowledge. And even these texts cannot be properly understood without taking into account their specific role in the larger cultural system of knowledge. In a given culture, knowledge about bodies in motion, for instance, is built up and transmitted by ordinary experiences with unspecific objects accompanied by every-day language, but also by specific, socially-determined experiences with the material artifacts of that culture, such as machines, experiences which are reflected in technical language, and finally also by appropriating and exploring the theoretical constructs represented by the writings usually studied in the history of science. Since the knowledge of an individual scholar partakes in some or all of these currents of the socially available knowledge in a

given culture, the individual knowledge itself is, as a rule, composed of various cognitive layers, each with its own specific structures (Renn, 1996, p. 7).

It is in the analysis of these ''cognitive layers'' and their counterparts in the social world of the scientist that the focus on intellectual interdependency entails. The cognitive structure of the intellectual interdependency is guided first and foremost by the cultural meanings of ''science'' (in contrast with other human enterprises) themselves. Not surprisingly, religious changes within societies can be traced to leave their substantive marks on the ways in which science becomes conceptualized (Merton, 1936). It may be possible to trace the divide between Anglo-American empiricist focus in science, and Continental-European primacy on theoretical discourse, to the differential histories of religions in the different cultural areas. While looking at the background role of Puritanism in the framing of science, Merton noted

> It may well be that the Puritan ethos did not directly influence the method of science and that this was simply a parallel development in the internal history of science, but it is evident that through the psychological compulsion toward certain modes of thought and conduct this value-complex made an empirically-founded science commendable rather than, as in the medieval period, reprehensible or at best acceptable on sufferance (Merton, 1936, p. 8).

The ''faith in *empirical* science,'' in whatever form it occurs, is primarily a faith and only secondarily empirical. It specifies the direction of inquiry, the desired realm within which scientists should act. The role of the Puritan/Protestant ideology in directing sciences toward the concrete can be seen via its opposition to medieval scholasticism. Likewise, the advent of behaviorism in the United States in the beginning of the twentieth century was an ideological movement away from the context of Protestant theological speculations. In a way, behaviorism turned the pietist focus upon pietism itself.

Who Is Doing the Talking?

Talking about intellectual interdependency of science involves taking a stance – a perspective – upon that science. However, this immediately distances any statement about a science from that science itself. Any statement *about* science – moralistic, futuristic, critical, or glorifying – is a statement of some ideal position relative to science. Hence it

belongs to the realm of the social organization *of* science, not *into* science as such. Thus, what is attempted in this book is a meta-level analysis of the social canalization of ideas of the person as a social agent in the social sciences. In that sense, our effort might qualify as an example of a sociogenetic epistemology of science, carried out on the materials of the social sciences. As such, our effort does not belong to psychology (or other social sciences) per se. These are meta-scientific statements, i.e., they belong to the realm of the social organization of science. When this level of discourse is made into the target of investigation, we can talk about a discipline of the *developmental sociogenesis* of scientific ideas. Discourse about science is of value in its own right – only that value is in its being an object for investigation, not reflection of the state of affairs in a given science.

Who is likely to create discourse about science? The knowledge created within a given discipline is by its nature non-neutral as to the goal orientations of different institutions in a society. First, and historically foremost, sciences produced know-how that would lead to material gains in producing and distributing goods. The organization of society on its sociomoral side was sufficiently removed from the potential products of science and, hence, science could be perceived as a social institution in and by itself. This may be sufficiently well described by the representations of ''paradigm'' and ''paradigm change'' (Kuhn, 1970).

In the recent two decades, the study of the social organization of science has received increasing attention from sociologists and historians (Latour, 1987; Pickering, 1992, 1995; Renn, 1996; Shapin, 1995; Woolgar, 1988). Investigation of intellectual interdependency borders on these research foci, yet it differs from these in a substantial way. We are interested in the development of concepts in their social contexts, in the process of communication between scientists and societies. The focus of our investigation remains on the individual scientist and his or her creative efforts (and their successes and failures). The social embeddedness of these efforts is given careful consideration as the supportive basis for successes or failures, yet it is the personal creativity in a discipline that is the ultimate location of novelty construction. That creativity, however, is always embedded in the texture of the social guidance efforts of a science.

It may have been up to the scientists to interact about the substance of science, without having to take the social politics into account. Yet in conjunction with changes in the types of societies (moves toward

''democratic'' organization of society) and sciences' moves into the realms close to sociomoral arenas of institutional activities, this autonomy of the institution becomes lost. Sciences discover the need for *popularizing persuasion*, i.e., showing off certain kinds of their ''successes'' to the laypublic or powerful interest groups, gaining support via such communication. This necessity becomes important only if the lay populace acquires social mechanisms of control over the given discipline, either as a ''client'' to its applications, or as a potential power source over its support by governments. Thus, the pressure to persuade the laypublic about the effectiveness of psychotherapy is substantial, while that about the precision of psychophysical experiments is not. Yet the latter may need explaining to a grant review panel.

Contemporary research grant obtaining in U.S. federal funding agencies is a good example: even when peer review entails institution-mediated communication with (anonymous) peers, the final decision about funding is based on institutional decisions on the basis of ''priority ratings.'' Organizing the institution of a science by way of a ''peer community'' (which discusses the contents), while retaining the control over the actual provision of support, is a natural tactic for any institution (the ''professional power'' is obtained by purchasing the services of selected ''experts'' for the institutions in consultant roles).

This example leads us to the question of the structural organization of ''the scientific community'' and to the role of social institutions in setting it up. The scientific community is not a group of equal persons who are operating a club that functions on the basis of democratic governance. Different forms of the organization of institutions in the given society at the given historical epoch inevitably leave traces in (or give full form to) the way the given science is institutionally organized (e.g., through conventionalization of discourse; Bazerman, 1987). At the same time, it is the scientists themselves who actively assume the expected institutional roles and use them for the advancement of the social status of their particular knowledge.

At different historical periods the linkage between extra-scientific institutions and sciences is more explicit than at others. Thus, the ''great break'' in Soviet philosophy (Valsiner, 1988, pp. 90–5) that carried over to psychology. It led to the demise of paedology as the interdisciplinary investigation of children, and with it of the cultural-historical school of thought. This ideological transition was institution-

ally organized by coordination of the ''vertical'' (scientific institutions–political powers) and ''horizontal'' (competitive actions in scientists' peer-groups, and inter-''schools'' warfare) communicational processes. It was not ''Stalin's tyranny'' superimposed upon social sciences from above, but rather a social opportunity, provided ''from the above,'' that led competing peer groups to denounce one another in competition to ''win'' a better position for themselves. It was the ''next-door neighbor'' (or a competing scientific group) who was the initiator and henchman of the ''Stalinist purges'' in everyday life and in ''Soviet psychology'' in the 1930s. Similar ritualistic coordination of the two channels of communication occurred in the Soviet Union during 1947–1951, through different waves of reorganization in philosophy, biology, and linguistics (Kojevnikov, 1996).

All these changes were organized by public institutional rituals of ''discussion,'' during which ''criticism'' and ''self-criticism'' was publicly practiced by participating scientists and public officials. The effort to guide the scientific knowledge construction practices toward fit with the ideology in the Soviet Union were in principle not different from the institutionalization of psychology in Nazi Germany, and from the advent of behaviorism in North America (see Chapter 5).

Professionalization of a ''Quasi-object''

Intellectual interdependency acquires new nuances in a world filled with rituals of professionalization, advertising of credentials, and competition for rewards. Usually it takes some time for a discipline to construct its own institutionalized system (e.g., for American sociology this is said to have taken four decades; Kuklick, 1980, p. 209; see also Chapter 5). As a result, the practical activities of scientists become institutionally determined:

> At the highest, most general, level of the organization of the society, it sets up conditions that determine who among the population becomes involved in one or another area of science. For example, in all societies, becoming a scientist in a particular field involves a lengthy process of education with a selection of the appropriate candidates built into it (in the form of examinations, theses, degrees, honorary insignia, etc.). Furthermore, the content matter and language of exchange of information within a thus 'socially legitimized' science is constrained both by the society and the scientific community itself which may make its 'boundary

maintenance' an important task. Undoubtedly, the form of conventions of scientific discourse and boundaries maintained between disciplines are constantly in the process of dynamic change (Valsiner, 1988, p. 11).

When seen from this light, contemporary institutional insistence upon ''interdisciplinarity,'' ''multidisciplinarity,'' and ''research productivity'' constitutes a marker of enforcing change in the ways science functions. This is the social-institutional discourse about science, which – after providing convenient ''modernist'' and ''postmodernist'' labels for mixing scientific and meta-scientific discourses – ends up in fragmented talk about *quasi-objects* (see Latour, 1993). Quasi-objects are objects ''in between'' the realms of nature and society, belonging to both, yet not distinguishing either. The notion of such objects is an effort to fight ''dualisms,'' which is a social representation that organizes much of social sciences' discourse.

If we were to use Latour's terminology, the whole of psychology could be viewed as a quasi-object. That role is inherently contradictory, and instead of Latour's preferred notion of fusing the realms of nature and society, our coverage here emphasizes the inclusive separation of the two sides, natural and social, of the quasi-object. Latour's efforts to overcome dualistic views on the world have failed, as the application of quasi-object status to one's object of investigation effectively replaces making sense of the systemic functioning of the object by the assignment of an appealing (but imprecise) label to it. By declaring psychology to be a ''quasi-object'' we have only attached a Latourian label to it, while the fermenting processes that go on behind the label, and that make so many psychologists intoxicated by the ''crisis'' in their science, continue.

Secular Sanctity of Science

Science as an example of human activity has become intensively investigated by the sociology of scientific knowledge, where largely empirically based ethnographic descriptions, oriented toward describing what happens when scientists are involved in their work, prevail (Gilbert & Mulkay, 1984; Latour, 1987; Latour & Woolgar, 1979; Pickering, 1992). These studies have demonstrated how scientific activities are versions of human activities, and cannot be ascribed a completely separate status.

Scientific institutions make use of forms of socially representing