

11

HOW LANGUAGE REALIZES

THE WORK OF SCIENCE

SCIENCE AS A NATURALLY SITUATED,

SOCIAL SEMIOTIC SYSTEM

*There, in front of us, where a broken row of houses stood
between us and the harbor, and where the eye encountered all
sorts of strategems, such as pale-blue and pink underwear
cakewalking on a clothesline, or a lady's bicycle and a striped
cat oddly sharing a rudimentary balcony of cast iron, it was
most satisfying to make out among the jumbled angles of
roofs and walls, a splendid ship's funnel, showing from
behind the clothesline as something in a scrambled picture—
Find What The Sailor Has Hidden—that the finder cannot
unsee once it has been seen.*

Vladimir Nabokov, *Speak Memory*

The chapters of this book have projected a few short moving pictures of language being used in science. Like all texts, these chapters have been constructed with as much intention and art directed toward the anticipated readers as the struggling writer can muster. The intention has been to share parts of a pattern, an understanding, which I have increasingly seen through contact with materials examined in the course of research. This pattern, although incorporating many patterns pointed out by previous authors, seems somewhat different in form and total mass than that perceived by others considering related problems. Why I think the pattern I have seen is important will, I hope, emerge in this and the next chapter, but first the entire pattern must be exhibited, by juxtaposing it with some other patterns, familiar and less familiar.

Put most baldly, the pattern I see addresses the problem of how language accomplishes the work of science. Such a discussion could be simplified if we could independently define the work of science; how-

ever, for reasons I hope to make clear, we cannot separate our view of the work of science from our view of the praxis by which the work is realized. Thus, we can best get an understanding of the various views of science and language by seeing them as unitary relations.

The Difficulty

From an everyday point of view, how language accomplishes the work of science is hardly a problem at all—or only a problem in the most practical sense of the word. From this perspective, language represents the objects of nature and their relations. As we discover new things we invent new words and we put those words in relation to represent the relations of the real world. Science tells us about nature; words and numbers are the symbols it uses to tell us. By representing nature symbolically, we can understand, predict, and manipulate it. The symbols give us a picture of the way things are. The only problem is the most practical one of making the symbols precise, unambiguous, univocal, to create a clear one-to-one correspondence between object and symbol. The prescriptions of technical writing manuals largely reflect this everyday perspective (see, for examples, Day; Fear; Houpp and Pearsall; and Mills and Walter).

From a commonsense point of view we have many reasons to credit such an account. The formulations of science—rules, laws, descriptions, knowledge—have provided us with detailed accounts of many natural events, accounts that seem tightly congruent with repeated experience and precisely predictive for future experience. Moreover, these formulations have given us unimagined dominion over the objects and creatures that surround us. These formulations allow us to conjure great forces, quicken those at death's door, and create new forms of life. Our trust in the congruence of these formulations with the ambient world goes beyond appreciation and spectacular display. We regularly trust our lives on airplanes and feel ourselves distinctly disadvantaged when our television or computer breaks down.

When we look at scientists themselves, we see so many of them working so intently to create new formulations and to create evidence for the correspondence between their claims and the phenomena they are exploring, that it is difficult not to share their conviction that they are describing something. Indeed, hard-headed corporations and real-politik governments have invested heavily in science's ability to create bottom-line economic power.

When we look to the formulations created by science as reflected in

symposia and published articles, we certainly see a very specialized development of language, distinct from our everyday conversation and newspaper reading. Unfamiliar words signify objects and phenomena from the microscopic and macroscopic limits of the universe, objects distinguished from each other and classified with a precision and taxonomic care having little to do with our everyday fuzzy naming of the objects of domestic life. Moreover, this specialized language of science seems constantly filled with evidence, numbers, observations, pictures, to ensure that the formulations correspond to real things. Fat scientific dictionaries, histories of the rise of scientific vocabulary, detailed handbooks of scientific writing, and the teaching of technical writing and scientific German as special subjects all reinforce our notion that scientific language is something special and privileged. Even such varied and opposed reductionists as Garfinkel (*Studies in Ethnomethodology*, chapter 8) and Skinner (*Verbal Behavior*, chapter 18) afford scientific language a special status separate from the turbulent, murky, and illusion-ridden language of the rest of the human world.

Yet from the perspective of our murky, deluded human world, we have always had good reasons to doubt such simple accounts. The Sophists early saw the fluidity and uncertainty of symbolic representations and thus the questionableness of whatever formulations we see as knowledge. Plato shared this perception despite his being cast as the Sophists' first and most formidable enemy in the saga of philosophic history. The cave allegory in the *Republic* is a critique of the shadowy representations by which we know the world; Plato only adds the difficult possibility of escape from the cave (514a–517c). This is the same problem Bacon grappled with in considering the idols that obscure our language (*The Advancement of Learning*). Although some Baconians—notably Sprat and Wilkins—may have believed in the possibility of a pure philosophic language totally expurgated of the idols, Bacon himself seemed to see the cleansing process as always a partial and incomplete process, so that we would always be burdened by the constraints of language. Nor could the naive linguistic realists identified as Baconians have held unquestioned sway after Swift's damning parody of the Royal Society in the third book of *Gulliver's Travels*. In the eighteenth and nineteenth century eminent scientists and philosophers of science repeatedly warned of the uncertainty of language and symbolic representations (Bellone).

Reasons for distrusting the direct correspondence between scientific formulations and nature have been in recent years rearticulated with great force, and with persuasive empirical evidence. The faint irony of empirical evidence being used to undermine naive empiricism has not

escaped the attention of a number of authors making the argument for the opacity of scientific discourse, and they have dealt with this awareness variously, with some considering it a great paradox and difficulty (see, for example, Woolgar, "Irony"; Mulkay, "The Scientist Talks Back"; and Oehler and Mullins, "Mechanisms of Reflexivity"). Yet, from the perspective to be sketched below, recognition of the opacity of language does not necessitate disowning empirical constraints on what we say.

The reasons to distrust scientific language are of several kinds:

1. All languages are semiotic systems, incorporating basic assumptions about the nature of reality (for example, Bloor). These assumptions color not only representations made within the language, but sensory perception about the ambient world (see, for example, Hanson). From this perspective it would seem that the work of science is to maintain and elaborate the existing semiotic system

2. Scientific formulations embody ideological components from outside the realm of science. From this point of view the work of science is to advance or provide foundation, legitimacy for larger social programs which themselves may simply be the result of class interests (see, for examples, the various essays in Barnes and Shapin, *Natural Order*).

3. Scientific language serves to establish and maintain the authority of science, largely through exclusion and intimidation. By establishing the special and elevated character of science, scientific communications accrete power to the scientific community (see Knorr and Knorr, "From Scenes to Scripts"; Gieryn, "Boundary Work"). Here the work of science is to advance itself.

4. Within the scientific community, scientific language serves the competitive interests of separate individuals and research groups. The language is partisan, argumentative, and manipulated for individual gain rather than an objective, dispassionate representation of things as they are (see Latour and Woolgar; Yearley; Pickering). Under this rubric the work of science is to advance the careers of individuals.

5. Scientific language is often fuzzy, incomplete, undefinitive. In particular the reference to actual events is obscured if not made fully obscure by the inadequacy of methodological description, the importance of inarticulate craft knowledge to produce results, the lack of precise replication of results, and the selectivity and emphases in the representation of results (see Knorr, "Tinkering"; Collins, *Changing Order*). This fuzziness leaves room for many kinds of social activity, with the apparent work of scientific discovery being only a screen.

6. In sum, scientific formulations are a human construction and thus are heir to all the limitations of humanity. Scientific formulations, giving

us no direct access to things in themselves, seem to do all the social work of being human with no overt means of doing the empirical work which has been considered the work of science. The appearance of reality projected in scientific texts is itself a social construction.

I have cast the modern formulations of the problem of language in the most radical form, and there are many who present less extreme positions. Some, claiming interest only in the social processes, simply postpone considering the empiricist issues. Others see the social processes somehow embedding empiricist procedures. Kuhn, for example, despite the rather radical uses he has been put to, insists he is a rationalist and empiricist. Yet, he has been unable to make that case forcefully enough to harness the widespread radical interpretation of his work. Currently, the radical positions put the issues most powerfully.

The Conceptual Source of the Difficulty

Our current inability to forge a convincing link between the socially constructivist critique of scientific formulations and the empiricist project has roots in how we have become accustomed to think about language in this century. When socially minded observers of scientific activity come to think about the role of language, our current concepts of language offer no strong clues about how language talks about anything other than itself. The main lines of twentieth-century linguistic inquiry have turned away from issues of how language interacts with the world of experience, although in recent years some linguists have shown increasing interest in how language constitutes the social world. Thus on the question of the nature of linguistic representation of the experienced world, linguists have only to offer some version of correspondence theory (that words do in some fairly direct way correspond to the objects of nature) or of social relativism (that every society creates its own reality through its symbol system.)

More specifically, what has been lacking is a unitary concept of signifying events simultaneously contexted within and realizing linguistic code, social relations, psychological cognition, and perception of the ambient world. Only in the recent attempt to elaborate the work of the Russian psychologist Lev Vygotsky and his followers has a strong enough model of language activity developed to encompass all these elements, and to enable us to see how in making statements we bring together many elements—cultural, social, psychological, and material—to accomplish our activities and create cognition, a cognition that can be

empirically conditioned. That empirical conditioning of cognition is highlighted by Ludwik Fleck's vision of scientific activity. The prescient work of both Vygotsky and Fleck was buried by the politics of the 1930s, but their recently rediscovered ideas point the way toward the understanding of scientific knowledge as a socially and individually constructed, semiotic, cognitive, empirical activity—a practical part of our being human in the world.

In his *Course in General Linguistics* Saussure, rightly considered the founder of modern linguistics, admits the complex reality of language, but finds this complexity far too much to contemplate with any clarity within any discipline (24–25). For the sake of analysis and the sake of establishing linguistics as an autonomous discipline, he separates *langue*, the linguistic code, from *parole*, the use of language in particular circumstance for particular purposes. He considers only the former, linguistic structure, as the proper study of linguistics. In so doing, he separates code from meaning, even though he recognizes that the sign is not an independent linguistic entity, but is a dialectical unity of signifier and signified (99–100). That is, sign systems not only embody meanings, they are embodied out of meanings. Words and meanings dialectically define each other. The immediate implication is that one cannot understand language without looking at the contexts in which it is used to convey meanings. Yet by distinguishing *langue* from *parole*, and limiting linguistic science to *langue*, Saussure has effectively ruled the fundamental questions of language out of bounds.

Three other Saussurean gestures heighten this context-free code orientation. First, to isolate the study of code from the study of the historical evolution of particular features (as characterized nineteenth-century philology), Saussure distinguished synchronic from diachronic study. Systematic linguistics would consider language only synchronically (40–43; 114–40). By ruling history out of bounds to systematic study, Saussure not only eliminates large-scale evolutionary studies, but also the examination of the brief historical moments in which code interacts with context to realize meaning and during which code evolves to meet communication needs. This antihistorical gesture effectively keeps the code orientation clean, at some distance from challenging data.

Second, in discussing the form of the sign, Saussure calls the sign arbitrary (100–102). The argument and examples that follow the designation of arbitrariness suggest only that the phonetic realization of the sign—the sounds—are arbitrary. Roosters go *cock-a-doodle-doo* in English and *kiekeriki* in German. Nonetheless, the slogan that the sign is arbitrary has been taken as justification for the divorce between code and

meaning (or use in context). From the text it is unclear how much Saussure himself was willing to use this more general claim to buttress his strategy of excluding *parole*, but certainly the claim of arbitrariness has eased the conscience and consciousness of many linguists to follow.

Finally, through an imaginative gesture, Saussure brings into creation an as yet unestablished but broader field of semiology, the study of sign systems (33–35). Semiotics was thus grounded in the model of the study of linguistic code separated from context of use and meaning, even though Saussure proposes that semiotics would study “the role of signs as part of social life” (33). This founding heritage has directed semiotics to consider sign systems as having autonomous structure and power.

Saussure's judgments about how best to make progress in the study of language have turned out to be quite shrewd. In looking closely at synchronic codes descriptively and structurally, linguistics has made great conceptual and concrete empirical advances, particularly at phonetic, grammatical, and syntactic levels. And this orientation was reinforced by such different kinds of linguists as Hjelmslev and Chomsky, who saw in the synchronic system not just an analytical fiction (an artificial cut to allow some clarity), but hope of a more substantial explanation of realities beyond the code. In explaining the rules that govern the code we might find the rules that govern meaning (in Hjelmslev's *Prolegomena to a Theory of Language*) or the rules that govern the mind (in Chomsky's *Language and Mind*). That is, code separated out and elaborated as an autonomous object has come to be seen as dominant. This tendency has also generally been followed in semiotics, where sign systems are seen to be determinant of consciousness, perception, and social behavior, rather than interactant with them.

This is not to say that there haven't been contrary observations, hybrid ideas, and minority traditions, but these have until recently tended to remain either vague or underdeveloped. Malinowski, Whorf, Sapir, and Firth got little beyond programmatic statements and/or preliminary investigations into the social embeddedness of language. Their undeveloped work was too easily reinterpreted in code-oriented ways, as forms of code determination of social/psychological realities. After all, the synchronic code seemed to have an elaborated, solid structure—something a linguist could analyze—while social and psychological phenomena seemed inchoate, and therefore open to be shaped by the structured linguistic or semiotic codes.

Thus from language and sign studies we tend to get either of two attitudes toward reference and meaning. First, within the majority code-oriented tradition, because the study of language structure is cut

off from problems of meaning and use and thus the relationship not looked into, it is simply assumed that there is some sort of not very interesting correspondence between words and meanings. Or alternatively, from a code-oriented reading of the minority tradition, since meaning and use seem to have no grounding equivalent to that found in synchronic code, they are free to be pushed around by the code—leading to a simple relativist position.

Linguistic studies of scientific language (or scientific register or scientific sublanguages) have come rather directly from the code orientation. They have been looking largely for the subset of syntactic and grammatical features used in scientific communications, considered fairly independently of use, context, or meaning (for example, Gopnik; Lee; Huddleston; Kittredge and Lehrberger). Relationships to meaning, use, and context are just not problems, and the implicit acceptance of some sort of correspondence theory of meaning need not even be raised.

For obvious reasons, these studies have been of little interest to the social relativist critics of scientific discourse, who have been concerned precisely with the social, ideological use of scientific language, but apparently reflected in syntactic, grammatical code. They have, however, found some greater affinity with literary philosophic work developing out of semiotics and transmogrifying into deconstruction—revealing the text only as a linguistic structure, a contrivance, having no inherent meaning, but creating sociopsychological realities out of its semiotic code. Both Knorr and Latour have shown particular interest in semiotics and deconstruction.

On the Way to a Solution

Recent developments in linguistics and related social sciences, however, have loosened the strict code orientation, thereby undermining linguistics as an autonomous discipline, having a separable matter for study. Sociolinguistics at first addressed the code descriptive task of identifying variation in the code and/or alternative codes among different groups distributed geographically and/or by class, but the variation found was so extensive as to call into question the notice of a stable/coherent code. Codes just ran into each other with no distinct boundaries (Hudson, *Sociolinguistics*, provides a critical review). Even more distressing, individuals seemed to speak no one code but have a repertoire of codes, with their choice of codes to use at any moment itself being a meaning-creating act (see, for example, Gumperz, *Discourse Strategies*; and LePage and Tabouret-Keller, *Acts of Identity*).

Similarly, the recently developed linguistic specialty of pragmatics has been fraying the edges of a firm code. Pragmatics is the study of how people use language in real life to do things, a topic seemingly beyond the edge of Saussurean linguistics. The topic first had to be domesticated enough to be brought into linguistics. This was done by Austin, who located Wittgenstein's concept of language in use (not far from Malinowski's observations on the social use of language among the Trobrianders) within certain sharply definable speech acts, which Searle further reduced to a series of rule-governed procedures. Thus framed, the concept of doing things with words seemed a code-consistent issue, opening up the new domain of communicative competence to parallel other code-based competences (see Searle, Kiefer, and Bierwisch; Leech). But this open-ended issue would not remain domesticated for long, as the observations of what people did with words started extending beyond crisp examples such as christening a ship and making a bet. Moreover, the action taken was not always crisply related to the linguistic forms used to realize the action. Social activity in language was seen to be a complex and creative force, not easily reduced to rule-governed behaviors.

Searle himself planted a major surprise when he argued that making reference itself was a speech act (*Speech Acts*). This problematization of reference impelled the study of deixis—that is, how one attaches one's talk to the surrounding world. At first deixis seemed a fairly containable subject, dealing with simple words like "this" and "that," but deixis too has been discovered to infuse all aspects of the language in complex ways (see Lyons, chapter 15). Thus the code again seemed unintelligible and uninterpretable and even unsystematic when separated from its contextualized use.

Increased attention to detailed developmental data, in part motivated by Chomsky's strong claims about the psychological implications of code structure, has as well revealed that language develops as part of the child's increasingly complex interaction with the world and people. Cognition, experience, and social interaction are all significant variables in language development, which can no longer be seen as an autonomous linguistic phenomenon.

And, finally, the great success of code-oriented linguistics in phonology and syntax has encouraged consideration of larger orders of organization, in the specialty at first called text grammars and then discourse studies. The change of nomenclature itself indicates how little the phenomena could be contained within a formal code-based model. Questions of textual interaction with cognition (schema, story grammars), social interaction (ethnomethodological approaches), and social

history (genre approaches) currently seem more promising in understanding textual organization. Even formal models seemed to require awareness of how texts were situated within task, social relationships, and communication channel (field, tenor, mode) to begin to account for variation in discourse patterns attributed to various subcodes or registers.

This exciting rediscovery of language's intimate dialectic with the lives of people in the world has hardly settled into any clear picture, appearing to reconfirm Saussure's warning that linguistics must isolate itself from these variables to gain any rigor. Thus while we are now starting to get much more detailed and vibrant pictures of separate linguistic phenomena, linguistics has not developed any sharply articulated model of language activity that could guide social studies of science.

The boldest and most influential attempt by a linguist to form an overall view of language activity has been by Michael Halliday, who argues that linguistic features are only surface realizations of larger social activities (*Language as Social Semiotic*). In his study of child development of language, for example, he sees the developing language system of the child as part of the child's growing system of social interaction (*Learning to Mean*). Only once that social system is formed is the child ready to adopt the socially given model of adult language. Accordingly, he interprets features of the code as realizations of communicative impulses and social interactions (*Functional Grammar*). And he argues that any communicative impulse may be realized in a variety of apparently different surface forms, which we cannot properly understand unless we see the connection to the underlying impulse. For example, in some contexts the question "How are you doing?" may be more closely related to the command "Have a good day!" than to the more superficially similar question, "How are you traveling?" ("Language as Code and Language as Behavior").

Despite Halliday's boldness in reestablishing meaning-making without context as prior to code, his formulations (which he considers within the Malinowski-Firth tradition) fall short of solving the puzzle presented by scientific use of language as currently perceived in social studies of science. First, while recognizing the evanescence of linguistic code, Halliday seems to have a much greater confidence in the firmness of social structure and culture as *a priori* frameworks from which to derive language behavior. When he talks of the influence of social structure and culture on language he has presented a synchronic vision of a well-ordered system, as though society—rather than logic and brain structure, as Chomsky might claim—offered a deep structure one could rely on (for example, *Language as Social Semiotic*, chap. 10). In this way he

not only elevates and reifies society as a primary principle more than current sociological thinking might support, he seems to be running contrary to sociological interest in how society is constituted through language.

No doubt, regularities and structured elements appear in both language and society, but I know no reason to believe that either is prior to or privileged over the other. Until we have positive reasons for believing otherwise, we must assume society exists no more firmly, nor no less firmly than language (and other symbolic and physical means of coordinating activity). They are simultaneously realized in the social language act. Linguistic and sociological regularities—realized and institutionally structured in successive acts—might be best accounted for as parts of mutual realization.

The second area insufficiently addressed to this date from the Hallidayan perspective is the influence of the material surroundings on the sociolinguistic interactions and activities—that is, in what way, if any, language can talk about the world or influence doings in the world. This issue has just not been raised within Hallidayan linguistics, as far as I know, although there is no reason why it should not. Until that is addressed we are left with a vision of language activity floating somewhere above the world, as in a middle-class living room, with attention only on social coexistence. The mutual construction of reality seems only a matter of free choice and social imagination, with all the work of the world handled by machines behind the woodwork.

My intent here is not to privilege practical boiler-room language over the elaborate imaginative constructions of the drawing room, but rather to avoid a separation of the two. Certainly consideration of how language is used in science brings questions of the connection between elaborate human intellectual constructions and material activity to the fore, if for no other reason than science has allowed us such unimagined mastery over nature. Yet the issue is not limited to discourse areas which take the natural world as their overt topic, as science does. Much is to be gained by seeing all forms of language as practical activity in the material world, no matter how complex and apparently removed from the production of goods and services. Even play—both child's and adult's—is an important part of our material existence, as psychologists, sociologists, ethologists, historians, and critics of the arts have often reminded us. Any attempt to understand language that does not pay sufficient attention to how language works as a social tool in the material world invites the extremes of materialist and antimaterialist reductionism that see potatoes as more real than books or books more real than potatoes. Whether one sees human constructions as arbitrary and immaterial

because they are just epiphenomenal by-products of less culturally conditioned material objects or arbitrary because society seems to ride above the material, one loses sight of the way human constructions provide our means of living in the world.

A Vygotskian Model of Practical Social Semiosis

A more crisply defined, and I believe ultimately more powerful, model of the role of language in human activity, society, and consciousness can be developed out of the work of the Russian psychologist Lev Vygotsky. This line of work, at first carried on in the Soviet Union, has in recent years also been carried on in the West. The following account of the practical use of language in science borrows deeply and freely from the work of Vygotsky and his followers. However, in applying these ideas (which have been largely elaborated through study of the development of higher cognitive functioning in children) to the problems of science's advanced system of literacy, I have transformed some of them, perhaps beyond recognition. But the influence of these ideas upon me has been so deep, I am no longer capable of offering a full archaeology of the sources of the model I am about to propose. In the following discussion I will identify and describe some discrete Vygotskian concepts, but in general I will not attempt to disentangle my own elaborations and transformations from ideas previously proposed in the Vygotskian literature, nor will I attempt to give a coherent account of Vygotsky's theories. For a less idiosyncratic exposition of the ideas, you may refer to Vygotsky's two books translated into English, *Thought and Language* and *Mind in Society*; Kozulin's history of Soviet psychology, *Psychology in Utopia*; Wertsch's commentary, *Vygotsky and the Social Formation of Mind*; or Wertsch's two edited volumes offering work in the Vygotskian tradition, *The Concept of Activity in Soviet Psychology* and *Culture, Communication, and Cognition: Vygotskian Perspectives*.

The following model of scientific use of language will suggest how the work of science can be accomplished through the unfolding social and empirical activity of individuals coordinated (cognitively and behaviorally) within groups. To start, language is a tool that helps us carry on cooperative activities (a frequent theme in Vygotsky's writing; see for example, *Mind in Society*, 19–30). But in order for cooperation to be successful, we must already share much, not just the meaning of words and the syntactical operations but how those generalized words apply in

this situation and how they are to be realized in action. (Wertsch offers a preliminary discussion of presupposition and intersubjectivity in *Vygotsky and the Social Formation of Mind*, chap. 5.) Written directions on observations to be shared through a microscope require congruence between the direction writer's and the direction follower's apparatus, defined by common terms and perhaps aided by standardization in design. But also it requires congruent craft skill in manipulating machinery, dyes, slide preparation—a craft knowledge that can only be to some extent spelled out in print. A joint language and organization of the visual field is necessary for one observer to be able to see what the other sees, to identify designated patterns and salient features. Much shared background knowledge and shared experience are necessary to create the shared perceptual schema. And finally the shared observation is aided by standard observational routines that organize the activity.

In the literary economy of scientific articles, much of this shared background is relied on—not just the shared technical words, but shared conceptual, practical, and social worlds. In books for neophytes more of these shared elements are made explicit, but still much that is tedious, difficult or perhaps impossible to reduce to shared print symbols is left unsaid. Similarly, in the realm of research, which by its nature lies just beyond the edge of the familiar and communally certain, the symbolic reduction of the world and action conveys less firm and stable meanings, for just those elements necessary for shared understanding have yet to be established.

Another kind of shared knowledge required is of the social interaction being engaged in through the language. Often, for example, students socialized into the authoritarian relationships of textbooks (which dictate the student's experience, perception, and general claims) have difficulties entering into the more active engagement offered by educational materials emphasizing student observation and the development of individual perceptual schema. Perhaps even more to the point for scientific research, research communication requires practical social understanding of cooperative endeavor, aggressive assertion, and agonistic competition. As in any competitive activity, one must grasp the limits of violence and cheating and understand the forces that would bring the game to the edge of disintegration or transformation to a different kind of activity. Only under certain conditions and certain mutual understandings can the mutual activity flourish, just as ice hockey can flourish as ice hockey under certain conditions and understandings; when other conditions and understandings reign, the game transforms into a public display of team street fighting.

Given personal investments of all kinds that scientists have in their

published claims, the maintenance of a cooperative, honest, problem-solving endeavor may often be threatened. Appeals to the rules of the game are almost necessarily self-serving resources (Would you complain to an umpire unless you had some interest at stake?), but mastering and developing allegiance to the interactional rules are an important part of socialization into scientific activity. Different individuals have different understandings of the rules of the game and make different adjustments to them. Different subcommunities vary or elaborate the interactive practices differently, with perhaps greater passion, cynicism, or avoidance of severe struggle. But whatever the interactive pattern is, the scientist must come to understand it. What fascination working scientists have for the sociology of science may come from the need to come to terms with this aspect of the communication system.

The Material Bases of Shared Understandings

Since communication depends on shared knowledges of so many kinds, we need to identify the source of shared understanding to establish the grounds of the communication and to identify the social range and cognitive degree of the sharing in any interaction. That is, who shares and with what degree of congruence? Which individuals are brought into a social understanding and how fully is intersubjectivity established? We need to unpack the mechanisms by which shared understanding is achieved locally, and then by which local sharings spread and maintain stability over larger collectivities.¹

The achievement of shared understanding can be examined in two different kinds of situations, both of relevance for scientific communication. First is of the neophyte becoming familiar with knowledge already shared within a community. Through interactions, such as with the mother-child dyad now so energetically being studied in developmental psycholinguistics (for example, Bruner), the neophyte's utterances are interpreted and recast so as to fit within the interactive patterns and linguistic formulations accepted within the adult community. A kind of negotiation goes on between the beginner (with some kind of expressive or interactional motive) and an accomplished speaker, until the beginner produces an utterance recognized as bearing meaning within the socially shared system. Often within such socialization situations

1. For another account of how shared understanding is achieved locally in the laboratory, see Lynch.

the neophyte's comments are interpreted through a broader and more charitable interpretation of the communicative system than would be granted to a speaker recognized as fully socialized.

Significantly, these activities usually embody some aspect of the material world that provides a reference point, constrains the language negotiation, and often defines successful completion of the activity. With a child, the material considerations may involve food and dry clothing to be obtained or a jigsaw puzzle to be assembled or a series of sounds to be played with; with a student of science, the material considerations may be of a textbook experiment to be carried out or a functioning machine to be explained or a printed equation to be explicated. In both sorts of cases, objects, which stand independent of the conversation constructed around them, take an essential part in the activity of the conversation and allow the neophyte to associate the symbolic interaction with concrete operations on concrete objects. (For the importance of active engagement with the material world for Vygotskian theory, see *The Concept of Activity in Soviet Psychology*, 37-71.)

The symbolic interaction shapes perception and meaning to be taken from these concrete objects by calling attention to particular features and placing them in symbolic relations to other features foregrounded as salient, as when an instructor identifies a piece of paper as litmus and tells the student to pay attention to color change when placed within various solutions. Such use of language establishes categories of significance; dialectically, the presence and character of objects make such categories of significance possible, constrain appropriate comments to be made, and provide meaning to the interaction. If there were no paper, or there were no chemical solutions to dip the paper into, or the colors did not appear to change in the predicted way, the interaction would go differently, have different meanings for the participants, and would provide a different kind of learning experience for the neophyte.

Language use in the communal enterprise of chemistry is taught and learned in textbook diagrams and charts to be memorized, in classroom discussion of the previous night's reading, in pencil problems to be solved, in the teacher's commentary on demonstration experiments, in getting particular bottles down from the shelf, in student groups with lab book on the table attempting to set up an experiment, in the teacher's comment on the experiment's write-up. Students learn not just names of chemicals, but when to use such names, how to label the results of experiments, how to determine whether their results fit the standard description, how to answer questions.

Even the well-known forms of laboratory fiction-making practiced by students—such as the fudge factor—require that the students under-

stand the discrepancy between the symbolic representations constituted by the students' activity (that is, the recorded results of the student experiment) and the representations the students are expected to reconstitute based on the prior experience of the expert community, codified in the textbook experiment (that is, the "right" results). The clear intent of student fudging is to hide their apparent manipulative incompetence in reconstituting the symbolic object according to the shared procedures and perceptual schema of the disciplinary community. By fabricating expected results through calculations based on textbook theory, students hope to hide their inability to do the experiment "correctly."

As students move up the hierarchy of expertise in their scientific communities not only do their technical vocabularies expand, but so do their ranges of contact with the subject materials, their abilities to manipulate these materials in congruence with the formulations of their disciplines, their abilities to formulate symbolic expressions in less teacher-constrained situations (that is, taking their linguistic constraints from the materials rather than from sentences fed them in class), and the ranges of interactive processes they are expected to handle with peers and mentors.

In the course of these interactions students gradually expand functional competence in language activity through what Vygotsky calls the zone of proximal development (*Mind in Society*, 84–91). At any stage of development, an individual can accomplish certain things on his or her own, whether uttering babble syllables or boiling a liquid. But that same individual can accomplish a broader range of activities with the cooperation of a more skilled individual, such as associating certain of those babble sounds with meanings, or boiling the liquid within a distillation apparatus. The expert intervention provides a scaffolding into which the neophytes' behaviors can grow. By actual physical manipulations, giving instructions, asking questions, or responding appropriately, the skilled partner provides a framework of meaning into which neophytes' impulses, behaviors, and language can shape themselves.

As the neophyte gains control of the structured meaning/behavior system transmitted through the scaffolding, she starts to incorporate parts of the scaffolding in her own behavior. She starts to repeat the phrases the adult utters, starts to grab toward the picture the adult points to in association with an appropriate word, starts to repeat to herself the instructions provided by the instructor or the lab manual (e.g., "First you connect the rubber hose to the glass tube. Make sure that . . ."). An important moment in the child's development for Vygotsky is when the child starts to develop an internal language so that these

self-instructions, regulating the child's behavior, go underground becoming invisible to observers and even eventually to the child.² In this way, gradually the neophyte becomes socialized into the semiotic-behaviorial-perceptual system of a community with language taking a major and multivalent role in the organization of that system, but with that system also shaped around concrete worldly activities. In terms of contemporary cognitive psychology, she will have developed the scripts, schema, and plans appropriate to participation in the community.

Thus the apprentice chemist learns to think and behave like a chemist, such that when she walks into a laboratory, she will perceive the surrounding material through the acquired framework of chemical formulations and will behave with respect to the material so as to reliably reconstitute phenomena accepted by chemists as reliably reconstitutable. She knows how to make recognized chemical phenomena appear to those who have the appropriate chemical perceptual framework. And finally she knows how to interact with chemists—to discuss the happenings in chemical laboratories in terms of significant chemical issues and so as to make an appropriate contribution to a communal endeavor.

But all this requires the cooperation of the material she is working with. If someone switches the bottles or the chemical nature of the universe changes without her awareness, she cannot make the anticipated phenomena reappear reliably, nor can she carry out the day's work with colleagues. Her language will break down into the common language of bafflement, where referents no longer seem to refer, anticipations do not hold, and symbolic relationships do not wrap tightly around ambient conditions. The language withdraws from intimate interaction with the control of the processes—one literally does not know where one is. Under such conditions language moves to questions such as "Why isn't X happening?" and "What is going on here?"

If only deception is involved, standard chemical tests can reassert order by putting the right labels on the right bottles. But if the material of the universe changes, the chemist will have to begin chemistry from scratch, with all previous knowledge serving at best as an uncertain analogy. That is, the semiotic-cognitive-behaviorial system ties language use procedurally to specific manipulations of materials, and if those ties do not hold, our language use in concrete situations breaks down.

2. Vygotsky's concept of internal language (as elaborated in *Thought and Language*) is a conceptual precursor to Polyanyi's tacit knowledge.

Constituting New Reliably Reconstitutable Phenomena

The second kind of situation in which shared understanding needs to be established is when change, growth, or instability occurs within the system of understandings already shared by fully socialized members of the community. This kind of situation is particularly central to the activity of the scientific community. Unlike some other social systems that seek stability and ritual regularity in their communications (such as churches, island tribes, or lower echelons of bureaucracies) and change only when forced by exigencies (such as climactic change, new populations to proselytize, or political revolution) scientific communities are by their nature committed to new formulations, new knowledge. If they have no new knowledge to create, they cannot be legitimately maintained. In that respect they are like legislatures; with no laws to be made, they would be adjourned or turned into shams.

Change in scientific formulations can come from many sources. Some sources can be from outside the scientific community such as political ideological movements (state Marxism has served as both a stimulus and a constraint within Soviet sciences), changes in other forms of communication (such as the rise of a periodical press), new means of communication (whether printing press or modem), or idiosyncratic individuals with complex personal histories that import foreign styles (as when physicists went into biochemistry, or Newton perceived physics as mathematics). Or the sources of change may come more directly from within the activity of a science—as when phenomena refuse to fit formulations or when a new idea developed for a narrow problem is seen to have much broader power, or when an individual, whose work is rejected, discovers new and compelling means to assert his position.

Whatever the source of the new impulses and new forms the acceptance of these new formulations and styles of formulations into the common stock (or disciplinary matrix, as Kuhn calls it) depends on the community. The community itself must see these formulations as more useful, productive, promising for its current set of problems as currently perceived and formulated. The new formulations must be perceived as realizing desirable lines for the group activity (that is, as part of a progressive research program, in Lakatos' terms). In this competition for intellectual survival (as Toulmin has elaborated in *Human Understanding*), formulations must be cast persuasively, and preferably compellingly.

But general and immediate capitulation is rare, for the new formula-

tion represents at first only the realization of the experience and cognition of one individual or small working group within the larger research community. Other members of the community would likely have interests in seeing, thinking, and talking about the phenomenon (or related phenomena) in other ways. Resistance to new formulations exists for reasons beyond narrow-mindedness and bull-headedness. Persuasion, rather than being a single, sudden event, can be a lengthy process of negotiation, transformation, and growth of the central formulations and related arguments. Other researchers with their own perceptions, experiences, and research goals are enlisted not by checking off an approval rating in a Gallup survey, but by somehow taking the new formulation into account in their own work, if even only as a target of criticism. Formulations survive only by entering the living body of scientific activity, influencing behavior, cognition, social relations, future experience, and new formulations.

New formulations entering the common stock of formulations influence future activity and thus enter into a dialectical process with experience through the medium of working scientists. A successful incorporation negotiation ends up with a symbolic representation of an object or phenomenon that can be reliably reconstituted by members of the community under appropriate conditions in appropriate relation to activities and other reliably reconstitutable phenomena as perceived through the shared perceptual screen of the field. The two most immediate points of contact between active experience and formulation—the experiment or observations reported in the article and replication attempts—have been most criticized as having a loose correlation between events and formulation, but in the long run they may not be the most decisive in incorporation or rejection. They are only the most obvious first steps, and there is no reason to assume a stable reliably reconstitutable object will emerge from such first attempts at formulation.

The original report of an experiment or observation will not necessarily establish for all lookers the existence and character of a phenomenon, though the authors might wish so. Rather it will only indicate that these authors have been able to constitute an object for themselves with enough conviction that they will hold it up for public inspection. Since they are holding up for inspection a previously unconstituted phenomenon and since their formulation is a new one, one would expect neither that such a formulation would be stabilized in its final form nor that the object would be easily reconstitutable. The authors, to give the impression that their formulation captures a robust and reliably reconstitutable phenomenon, may be selective in their report, telling only of those occasions when they were able to constitute the object and telling

only those key behaviors in their belief necessary to constitute the object. Moreover, they will be talking through a cognitive/symbolic framework that already incorporates the possibility (if not the reality) of the existence of such an object of the precise kind represented. Their own description may not be useful in helping others (or even themselves at a later time) in reconstituting that object.

The difficulties with replication, as pointed out by a number of observers, include that there is often little incentive to attempt an exact replication. Where replication is attempted, local differences in behavior, experiences, and craft knowledge influence the outcomes; that is, the active attempts to reconstitute phenomena represented symbolically may lead to different (or differently perceived) results. Further, the replication attempts might not be carried out by people with the same commitment to the claim/representation and the implicit perceptual/behaviorial world as that of the originators of that claim. In fact, finally, it would seem those most motivated to attempt replication may well be those who most distrust the reported results and would have least shared in this activity/language/perception matrix. That is, while certain stabilized framing elements of the disciplinary matrix may be shared, unstabilized elements will lead to variations in the created and perceived event. Replicators will understand the words differently, do the experiment differently, and see the results differently.

Great intersubjective fuzziness may therefore surround a newly proposed phenomenon. Much negotiation may be needed before a communally accepted formulation emerges that defines a reliably reconstitutable object. This negotiation may involve many different kinds of empirical experiences, and not just attempts at immediate replications. In cases of direct opposition, other kinds of experiments and observations may be offered, putting the phenomena in different contexts of activity and representative framework. Not only more sensitive tests or new equipment or experimental variations may be involved, but new ranges of data may be deemed relevant to determine the character of the phenomenon, as well as new kinds of formulations. In the course of this debate, the object may turn out to vanish from sight, turning out not to be reliably reconstitutable in the emerging terms of the discussion. The noose of language and activity may pull closed and discover it holds nothing, or the phenomenon may slip out of the noose. Or the stabilized phenomenon may turn out to be a somewhat different thing than first formulated. Or the negotiation may never be resolved, with the community splitting into subcommunities based on acceptance of the object.

Stabilizing of a reconstitutable phenomenon may occur in ways other than direct conflict and negotiation. Competing scientists may carry out

brief, successful, and unreported replication. They may accept the new claim as consonant with their own or the discipline's previous experience and current conceptual frameworks. They may find the formulation of the new phenomenon powerful in solving problems in their own work. In such events they may quietly accept the phenomenon as reliably reconstitutable, and will employ it in their own future work. If this new work, however, proves troublesome they may have cause to look back upon their reliance on this phenomenon as reconstitutable. They may be forced to reconceive their experiments and observations under alternative assumptions in order to have them work out. On the other hand, if the formulation proves a reliable assumption, it may be used in a wider range of changing theoretical contexts and empirical situations, thus transforming the object by making it part of different activities. Similarly, it may become more and more a fundamental assumption built in almost invisibly to activities and formulations at great remove from its original use.

A phenomenon may become so regularly used and so reliably reproducible that it is built into a machine. Every time the machine works as anticipated it reconfirms the reliable reconstitutability of the phenomenon. Every time I drive my car I am reconfirming the reliable reconstitutability of physical and chemical formulations about such things as explosive combustion, friction, and electrical current flow. Every time an oscilloscope is incorporated into an experiment, the success of the experiment relies on the reconstitutability of many electromagnetic phenomena.

Alternatively, the formulated phenomenon may prove of no interest to anyone else so that it is not reconstituted at all. It is not reliably reconstitutable, not because nature might not cooperate, but because scientists do not. Scientists must see the phenomenon as a significant one for it to enter the living body of scientific activity.

Active and Passive Constraints

Thus within the negotiation of meanings that turn individual proposals into intersubjective realities, we find ambient nature passively constraining possible meanings through the active experience that is inseparable from the language use. Claims that may appear crisp and certain to their proposers will only be fuzzy intersubjective speculations until they settle into a regularized use within repeated activities, and these activities will only be repeatable if they are conso-

nant with some regularity in the appearances or operations of the natural world.

Similar constraining processes occur in all discourse communities. Cult leaders' claims that the world will end tomorrow must contend two days later with cult members' perceptions of the continued existence of the world. Literary critical claims that a particular theme is central to a novel must contend with the words inscribed by the author as read by a reader. Various discourse communities appeal to various kinds of experience as touchstones for their negotiations of communal meanings. In some religious communities, for example, particular emotional states, identified and interpreted appropriately, serve to confirm and define the reality of a cluster of essential meanings. Such states are in fact encouraged through architecture, music, ritual activity, and rules for regularized prayer and group interactions.

Science, however, has taken empirical experience as its major touchstone, so that in the process of negotiation of meaning, empirical experience not only constrains the range of possible meanings but is actively sought in the attempt to establish stable meanings from the negotiation. Thus, whatever may be the source of statements, the fate of statements depends on the experience generated by them. In this way science has made nature its ally. The claims that endure do so precisely because (within the particular set of problems and activities considered important) they have been able to ally themselves closer to nature than their competition, so that in the long run, one set of terms rather than another proves more fundamentally useful in carrying on activities.

In the last three paragraphs I have been elaborating a Vygotskian perspective on cultural/semiotic evolution through concepts borrowed from Ludwik Fleck. In *The Genesis and Evolution of a Scientific Fact*, Fleck proposed that formulations of knowledge within a community (or thought collective) were influenced by two types of constraints. The first, active constraints, consisted of the elements of the thought style of the thought collective. In his analysis these elements of thought style actually turned out to be habits, patterns, and available means of representation—through language, drawing, or other symbolic media. This seating of thought within a collective drawn together through semiotic means places his ideas in the same general area as Vygotsky's.

Moreover, in proposing a second kind of constraint on formulations, what he calls passive constraints, he comes even closer to Vygotsky. Natural phenomena passively constrain the kinds of formulations you can make in the sense that once you begin formulating statements in whatever style of your thought collective, certain behaviors or features of nature will limit what you can properly say. Once you have estab-

lished, for example, a procedure for identifying the hardness of rocks and have developed a taxonomy of rock types, which rocks are labelled as harder than which others is no longer a matter of cultural discretion. Thus, formulating practices are constrained by the activities that bring the language user into active contact with nature.

According to Fleck, a scientific fact for a thought collective is the representation of that passive constraint within the stylized representational manner of the thought style. Moreover, Fleck suggests that the scientific community is marked by the active pursuit of passive constraints. That is, the thought style of science actively seeks to increase the relationship between representations and empirical experience.

This Vygotsky-Fleck model of formulating practices seems to me most fruitful for the issues I have investigated in this study and the data I have examined. By seating language use in a social/empirical/cognitive activity, this model allows us to see the multivalency of symbolic formulations and to give a plausible account of the kinds of work we know through our daily experience that science does. But it does not give undue status to the statements of science, which by their own nature can be nothing more than constructions of the humans who use them. Scientific formulations embody all the complex impulses and limitations of any human product. Such a model allows us to accept the deep insights of the recent social analysis of language use within science without being driven to the absurdity of considering scientific activity cut off from its concern with the natural world.

The Historical Analysis of Language Use

By situating scientific language use and cognition within specific social/empirical moments, this model suggests that scientific language needs to be studied as a historical phenomenon. (Vygotsky argued similarly for a historical/genetic analysis of language; see Scribner, "Vygotsky's Uses of History" in *Culture, Communication, and Cognition*, and Wertsch, *Vygotsky and the Social Formation of Mind*, chap. 2). To understand what scientific language is and does, we need to look at what kind of tool it is. We need to see when, how, and to what purpose it is employed in the concrete settings of human history. History is not just kings quarrelling, but apparatus being built, balls being released down ramps, astronomers looking at the moon and arguing over the different things they claim to have seen, political scientists interviewing southern voters, articles being written, articles being read. Thus in this book, I have offered accounts of what forces constrained and impelled

Newton, Compton, Oldenburg, Wundt, and a host of other scientists to use language (both reading and writing) in particular ways at particular moments. I have looked at their linguistic inventions as creative responses to their situations, investigations, and goals as they evolve in historical settings. But this kind of narrative of rhetorical moments only displays the first level of history, the single living moment.

But the model proposed and the data examined suggest that history makes history, so that I have looked at a second, third, and fourth levels of history. The second level is the history of an individual that defines the symbolic resources, experience, and perceptions of that individual coming to any particular moment. This corresponds to Vygotsky's autogenetic analysis. Accordingly, chapter 6 of this book describes how Compton's underlying conception and formulations in one article had been shaped by his history in trying to come to terms with a problem—both in his laboratory and in communication with the ideas and opinions of his colleagues. Chapter 4 similarly reveals how Isaac Newton had to work through many formulations in many situations in order to find the final public form in which to express the scientific meaning of some empirical experiences of forty years before. In the process of finding a satisfactory mode of public discourse, both Compton and Newton were creating intersubjective, reliably reproducible phenomena for their disciplines out of what first were only private experiences.

This creation of community-wide, intersubjective realities brings us to our third level of history—the genetic account of the community as a whole—those events that have lead to the momentary state of the debate or communal activity. This corresponds to Vygotsky's cultural history. We have seen this in the issues and claims and counterclaims, the negotiations going on in almost every chapter in this book. We see the development of arguments, the mutual construction of theoretical perspectives, the populating of the experiential/conceptual world with re-constitutable phenomena of varying reliability, states of negotiation, and intersubjective congruence. We have the emergence of procedures and formulations.

But out of this fluid world of ever-new and ever-different social action, interaction, and symbolic realizations certain regularities develop in the social forms—what Fleck would call the distinctive features of thought style, but which Vygotsky might see more broadly as the characteristic cultural forms. These regularities encompass when and how one would approach a test tube or a colleague, how one would go about reading a text, as well as how one would draw a diagram or frame an argument. An account of the emergence, evolution, and extinction of these regularities comprises the fourth level of history: the history of

cultural forms. The evolution of cultural forms shape, constrain, and create opportunities for the historical events seen through the previous three perspectives.

Previous structural examinations of scientific language have been at this level of analysis, but without recognizing the historical/cultural character of the forms studied. By identifying certain regularities appearing currently within certain limited locales and activities as characteristic of scientific language, linguists have given the impression that these regularities are timeless expressions of the essential character of science, and that these regularities give a grasp on the whole of scientific use of language. But when we view these regularities through the model proposed here, we become aware that we must account for the functional emergence of such regularities to understand what they are and what they do. We must see them as fluid to varying degrees and in relation to even more fluid elements, and must see them in relation to the complex activities that employ these regularities. Thus broad, ahistorical, static identification of features such as the standard five-part structure of the experimental report or the use of the passive voice and avoidance of the first person, are found inaccurate with the slightest amount of historical digging; moreover, such investigations tell us very little about how and why to use these features.

Rather, we need to understand why regularities emerge, evolve, and vanish; what the writers accomplish through the use of these features within the activity of the discipline; why these particular symbolic choices have seemed advisable to so many members of the community that they become regular practices; whether these habitual practices have become institutionalized; and what the effect is of regularities and institutions on science's ongoing work.

The Cultural Form of the Experimental Article and Its Impact

The studies represented in this book have looked at all four levels of history realized in the linguistic moment. But the central focus has been on the fourth—the history of cultural forms. In this case, the cultural form is the genre of experimental research article. In its emergence and continuing fluidity we see the impact of the other three levels of historical analysis, and in its normative stabilization and institutionalization we see the consequences of the genre for the other three levels. That is, cultural forms emerge and evolve through individual and

communal activity; in turn, cultural forms give shape and focus to continuing activity.

The framing themes of the historical narrative of this book are (a) that the features of the modern scientific article emerged as responses to (and realizations of) social and intellectual history within the emergent scientific community; and (b) that these larger communal regularities emerged out of the activity of individuals, attempting to accomplish their goals within their perceived situations. The growth of the scientific periodical press and the rise of scientific societies (both in seventeenth-century England and late nineteenth-century America), and the emergence of new disciplines and reformulations of fundamental problems (as with experimental psychology and political science) have created major shaping pressures on the genre. But it is the individuals (both towering figures and lesser souls) who perceive and respond to these pressures to remake the genre at each act of reading and writing.

Regularities occurred because individuals perceive situations as similar and make similar choices. Institutionalization and codification occurred because repeated choices appear to the collective wisdom (or wisdom of a few powerful actors) to be generally and explicitly advisable. The agonistic forum of the scientific journal made special demands on communication that made exploring the rhetorical possibilities of empirical representation a particularly attractive rhetorical resource. As the genre and the consequent literature took shape, they themselves became increasingly important social facts to be addressed in new texts. References, citation practices, and embedding of contributions in theory gave textual form to the increasing explicit intertextual activity of each individual author. The success of the genre in carrying out the business of the scientific community has also turned the genre into another kind of social fact, as an authoritative model to be emulated by other disciplines, interpreted through their own perceptions and problems.

Institutionalized patterns of representation not only shape the form of the utterance, but all the activity leading up to, surrounding, and following after the utterance. We have seen some of the argumentative assumptions built into generic features of the research article. In the case of Compton we have seen how his activity, his normative behavior, and his basic perception of the cognitive task he was engaged in were shaped by the form of the answer that he was seeking. We have examined how the patterns of argumentation impel the strategies of argumentation and the surrounding activity. Good science—both experimental psychology and physics—seems in part defined by the form of one's claims, and that desired form provides a goal for the activity. The history of the APA style sheet reveals it not only as an attempt to regu-

late form, but as a way to socialize neophytes into acceptable scientific practices and appropriate communicative relationships among professional researchers. We have also seen how the necessity to produce new statements even influences the reading of prior statements in fundamental ways.

But of all the stories recounted in this book, the most poignant one, to my mind, for revealing the utterance as crystallization of experience, realization of social action, and shaper of personal and social cognition is the story of Newton's search for the most persuasive, compelling form to create shared appreciation of his perceived experience—what he saw with his prism. In eventually finding that his material was amenable to a tightly sequential form, constraining and constructing the reader's reasoning, experience, and perceptual framework, Newton not only quietened his critics and won the argument; not only did he establish his "facts" as reliably reconstitutable phenomena for all to see; not only did he create a perceptual/behavioral/empirical complex so strong that he closed off serious investigation of alternatives for a century; not only did he invent a way of arguing that led to the even more mighty *Principia* that seemed an immovable mountain for two centuries; but, most powerfully, he provided a model for the form of scientific argument that influenced all of scientific practice.

The evolution of scientific use of language hardly ended with Newton, nor had it begun with him. But given the contemporary means, problems, social relationships, and activity of science, he organized them to create a shared, relatively stable semiotic universe which has only in this century been displaced by a communal creation. He dominated the history of science not just because he discovered a few major laws, but because in finding the way to articulate those laws he found a powerful, long-lasting (though ultimately and necessarily temporary) solution to the problem of how one should talk about the subject.

That debate over how to talk about one's subject continues in all disciplines today, and cannot be separated from the fundamental practices of those disciplines. If there is any essential message of this book it is in precisely this: in those communal endeavors whose goal is symbolic knowledge, the more we understand the way symbols are used in the activity, the better we can carry out that activity. In Vygotskian terms, ability to talk about our language behavior offers us a higher form of self monitoring and regulation of behavior.