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EVENT ANALYSIS

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TRANSPARENCY IN SCIENTIFIC COMMUNICTION: FROM LEIBNIZ'S DREAM TO TODAY'S REALITY[1]

The traditional assumption that scientific communication should and can be "transparent" is examined. It is shown that this assumption is questionable in extra-scientific, cross-scientific, as well as in intra-scientific communication - as revealed by current practice in research funding agencies, inter-disciplinary projects, and communication within a discipline. It is argued that "opacity" is not the exception, but rather the rule in scientific communication and that this fact must be taken into account in any realistic model of scientific communication. Some suggestions for developing such a model are made.

Key Words: funding research, Leibniz, scientific communication, sociology of science, transparency

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[1] This paper is based on two lectures: the Keynote Address I delivered on October 23, 2002, at the Università della Svizzera Italiana (Lugano), to the meeting "Scienza della communicazione – Communicazione della scienza", commemorating 50 years of the Swiss National Research Fund; and my Inaugural Lecture as "Leibniz Professor" at the Center for Advanced Studies, of Leipzig University, delivered on November 11, 2002. I wish to express my gratitude to both institutions for honoring me with their invitations, and to Peter Schulz (Lugano) and Georg Meggle (Leipzig), for making them possible.

Introduction

Communication is a crucial component of scientific activity (as of virtually any other domain of human activity, especially in this «communication age» in which we live). As researchers and as citizens, we should all be concerned with the communication *of* science as well as with communication *within* science. In this paper, I will deal with one of the key aspects of this topic – the question whether scientific communication is or should be "transparent". The view that this is or should be the case is often taken for granted both by scientists and the general public. I will challenge this view and suggest that we should learn to live without the illusion that scientific communication is or should be transparent.

This idea is closely related, if not derived from, the traditional epistemological conception according to which scientific method is the privileged tool we have for penetrating beyond appearances and discovering the true "nature of things", in terms of which all observable phenomena should be ultimately explained.

Applying the scientific method should, thus, yield a fully intelligible representation of the world, which in its turn should be transparently communicable.

The trouble with this enticing ideal is that it does not correspond to actual practice.

Again and again we experience the fact that the "true picture of the world" remains veiled for everyone but a small group of initiated experts in a narrow domain. Is this only a technical problem having to do with the phenomenon of specialization and with the inevitable complexity of the language(s) of science, as it is often suggested?

Theory and practice

Leibniz contested the traditional separation between theory and practice, and argued for a close connection between them:

> An able professional, who knows the reasons for what he does, possesses – without mastering Latin or Euclid – more theory than a *demy-sçavant enflé d'une science imaginaire* and without any experience, for the latter does not have *toute la theorie qu'il faut.*
>
> ...
>
> Il faut se mefier de la raison toute seule, et il est important d'avoir de l'experience ou de consulter ceux qui en ont, car l'experience

> est à l'egard de la raison ce que les épreuves (comme celle du novenaire) sont à l'egard des operations arithmetiques.[2]

And Kant argueed that when one is led to defend one's theory by claiming that what is true in theory need not be true in practice, one should better check what's wrong with one's theory.[3] I will follow Leibniz's and Kant's lead in viewing the gap between ideal and praxis regarding the transparency of scientific communication as a sign of concern and a trigger for further thinking on this issue.

Three levels of "scientific communication"

It will be useful, in what follows, to distinguish between three kinds or levels of scientific communication, for each has its special problems with transparency:

1) intra-scientific communication: communication between scientists working in a specific field,
2) cross-scientific communication: communication between scientists across different fields, whether connected (interdisciplinary communication) or disconnected (cross-disciplinary communication), and
3) extra-scientific communication: communication between scientists and non-scientists: politicians, funding agencies, and the public at large.

My main contention will be that scientific communication is hardly transparent in any of these levels. This fact has only recently begun to be systematically investigated by the communication sciences. As the share of science and technology in human life grows, it should be better understood and taken into account both by scientists and by non-scientists, who take decisions about the funding and use of scientific research.

What is communicative transparency?

Intuitively, an utterance or a piece of text is "transparent" if its intended audience is capable to grasp its meaning without any special difficulties.

[2] Gottfried Wilhelm Leibniz [= A], *Sämtliche Schriften und Briefe*, Leibniz-Ausgabe der Deutschen Akademie der Wissenschaften, Darmstadt, Leipzig, Berlin, Akademie Verlag, 1923— , (A VI 4, 712-713). Translations of Leibniz's texts are my own.

[3] Immanuel Kant, *Über den Gemeinscpruch: das mag in der Theorie richtig sein, taugt aber nich für die Praxis*, in *Werkausgabe*, hrsg. W. Weischedel, Frankfurt-am-Main, Surhrkamp, 1996, Bd. XI, pp. 125-172.

In particular, there should be no problems of interpretation of unusual or "difficult" words, symbols, graphs, and formulae, no hard to process syntactic structures, and no implicit allusions, suggestions, or other forms of "hidden" meaning particularly hard to be detected by the intended audience. Maximum transparency, in this sense, occurs when there is a one-to-one correspondence between the meaning the speaker/writer intends to convey through his/her utterance/text and the meaning the audience actually "understands". We might formulate a working definition of this intuition as follows:

> Discourse addressed by a Speaker (S) to an Audience (A) is maximally transparent if the meaning S intends to convey to A (through his/her discourse) is recognized by A without any problem of interpretation.

It is important to notice that the notion of transparency captured by this working definition is heavily context-dependent – and rightly so. For what is transparent for one audience (e.g., a linguistically and *scientifically* competent audience) in a given context may not be transparent for another (e.g., a linguistically and *artistically* competent one) and vice-versa.[4]

Consider for example a graph or diagram – one of the preferred vehicles of scientific communication. On the face of it, nothing more transparent than that, for it sort of "bears its meaning on its sleeve". For a graph to be understood it is sufficient to understand the labels attached to its different columns, boxes, arrows, and symbols. As you have certainly experienced, however, this is not always the case. I, for one, confess that often it takes me a lot of effort to understand not only what certain graphs are supposed to "mean", but also how their presence is supposed to support the argumentative flow of the paper. Probably for an expert in the field the graphs would be much more "transparent". But then, what about graphs that "show" or "reveal" to an inventive expert a new idea no one of his colleagues, familiar as they are with these graphs, has detected this idea in them? In so far as the new idea was "there", in the graphs, it was not there "transparently", for it took its discoverer nothing less than a gestalt shift to take notice of it.

[4] For a more detailed discussion of the notion of transparency, see M. Dascal, *Pragmatics and the Philosophy of Mind*, vol. 1, Amsterdam, John Benjamins, 1983, *passim*, as well as M. Dascal and J. Wróblewski, "Transparency and doubt: understanding and interpretation in pragmatics and in law", *Law and Philosophy* 7, 1988, pp. 203-224.

Such gestalt shifts can transform the meaning of even the most semantically transparent signs. You certainly would not doubt that René Magritte's well known picture of a pipe represents an object everybody in our culture will easily recognize. And yet, by inserting under the pipe the phrase "Ceci n'est past une pipe", the painter created a co-text in which the picture could no longer be taken to straightforwardly and transparently represent a pipe. In this new co-text, we are forced to question our prior understanding and seek a novel interpretation.

In the jargon of linguists and communication scientists, what these examples show is that transparency is not a *semantic*, but a *pragmatic* notion, for it has to do with language-in-use, in a specific co(n)-text, rather than with language *tout court*.

The ideal of a transparent "language of science"

And yet, ever since the 17th century scientific revolution, the ideal of a transparent form of scientific communication was pinned down exclusively on semantics, overlooking the pragmatic nature of this notion. In this, science followed in the footsteps of Luther, who contended that the biblical text is transparent, i.e., understandable by any reader in any language, without the help of "authorized interpreters". Needless to say that Luther's thesis fitted his political agenda.

So did Galileo's analogous thesis. Only he defended the absolute transparency of the language of nature, as opposed to the language of the Sacred Scriptures, which often required elaborate metaphorical interpretation. Galileo spoke about nature as a *book* whose interpretation depended only upon the discovery of the language in which it was written. He who possessed the "key" to this language or code (i.e., its "dictionary" or set of semantic rules) would have no problem whatsoever in understanding nature and communicating transparently his findings.[5] The language, of course, was mathematics:

> La filosofia è scritta in questo grandissimo libro che continuamente ci sta aperto innanzi a gli occhi (io dico l'universo), ma non si può intendere se prima non s'impara a intender la lingua, e

[5] I have called the model of interpretation based on a semantically transparent ideal, such as suggested by Galileo, "cryptographic" – as opposed to the "causal", the "pragmatic" and the "hermeneutic" models of interpretation. See M. Dascal, "Models of interpretation". In Maxim Stamenov (ed.), *Current Advances in Semantic Theory*, Amsterdam, John Benjamins, 1991, pp. 109-127.

> conoscer i caratteri, ne' quali è scritto. Egli è scritto in lingua matematica, e i caratteri son triangoli, cerchi, ed altre figure geometriche, senza i quali mezi è impossibili a intenderne umanamente parola ; senza questi è un aggirarsi vanamente per un oscuro laberinto.[6]

In the same line of thought, the Royal Society of London commissioned, in the 1660's, its member John Wilkins to develop a Universal Language for science. It should not be – *pace* Galileo – composed of geometrical figures and mathematical equations, though its aim was to represent scientific knowledge in a correct, transparent, and universally intelligible way. Its aim was rather communicative: it would overcome the linguistic barriers that plagued the circulation of ideas in Europe at the time, and would lead to the acceleration of scientific progress.

Though communicative in purpose, the project presumed that enough scientific knowledge about the nature, structure, relations, and classification of things had been accumulated so as to permit the creation of a semiotic system capable of representing nature comprehensively and transparently – hence its title: "Real Character". The addition of new knowledge was foreseen and would be performed through the addition of pre-designed types of squiggles to the basic symbols. Wilkins published his monumental work in 1668, and received the Royal Society's thanks and blessings.[7] It was quite an achievement of ingeniousness, but its failure was clamorous. For neither the Royal Society nor Wilkins took into account the fact that our knowledge of nature might change in more than a modest incremental way. Nor did they take into account the *pragmatic* conditions under which any language, including the alleged Universal Language of Science must fulfill in order to be

[6] *Il Saggiatore* (1623), p. 121. In Galileo Galilei, *Opere* (a cura di Ferdinando Flora). Milano-Napoli: Ricardo Ricciardi, 1953, pp. 91-352. The editor points out that Leonardo da Vinci had already affirmed the essential role of mathematics for the knowledge of nature (p. 121 note 4). As for the "double language" thesis mentioned above – language of scriptures (coming from the Holy Ghost) and language of nature (coming from God) – and their, respectively, non-literal and literal nature, see page xvii of the editor's Introduction, as well as Galileo's letters to Castelli (21 December 1613) and to Cristina di Lorena (1615), where Galileo makes extensive use of his biblical hermeneutics skills. The fact that many of the scientists and philosophers of the 17th century had to devote quite a lot of their intellectual energy to biblical hermeneutics is, in my opinion, indicative of the fact that, until roughly the end of that century, the battle between "faith" and "reason" for supremacy had not been resolved.

[7] John Wilkins, *An Essay towards a Real Character and a Philosophical Language*, London, Gellibrand & Martin, 1668.

actually used. As a result, Wilkins's ontologically naive and purely semantic gem was never used.

Leibniz's dream(s)

As a son of his century, Leibniz shared Galileo's and Wilkins's dreams concerning the language and method of science. But, as he used to do, he was both more ambitious and more realistic than each of them. The result was an intricate network of semiotic, linguistic, logical, rhetorical, and methodological projects designed to cover the ensemble of needs of the search for knowledge in all domains, subordinated to a master plan he often called "General Science". Obviously, I can only offer you here a glimpse of some of his extremely rich and innovative ideas in this field.

Leibniz admired Wilkins's work and partially followed his general classification of things when elaborating his own extensive tables of definitions. But he considered that the enormous effort required to produce a "Real Character" should yield "the most powerful instrument of reason" rather than just a means for the transmision of extant knowledge:

> Nihil enim hominibus evenire majus potest quam perfectio functionum mentis; scripturam autem rationalem ajo potissumum rationis instrumentum fore, minimumque ejus usum censeri debere commercium inter gentes lingua dissitas.[8]

This instrument of reason would comprise *both* a "method of judgement" and a "method of invention", i.e., it would provide a reliable tool both, for assessing the validity of extant knowledge *and* for creating new knowledge:

> Characters ... must serve invention and judgment, like in algebra and arithmetic.[9]

For this, it should be based on a complete analysis of concepts, leading to an "alphabet of human thoughts", coupled with a "calculus ratiocinator" capable of operating on such an alphabet in order to produce all the possible combinations of complex thoughts. For Leibniz, mathematics – hailed by Galileo as *the* language of science, is nothing but a very limited example of his much more ambitious project – the "Characteristica Universalis" (C.U.) – which should cover all domains of human thought and action.

[8] Gottfried Wilhelm Leibniz [= B], *Der Briefwechsel des G. W. Leibniz in der Königlichen öffentlichen Bibliothek zu Hannover*, ed. E. Bodemann, Hannover, 1889 (B, 101).
[9] Gottfried Wilhelm Leibniz, A II 1, 428.

This much is the familiar "calculative" version of Leibniz's dream, popularized by those who, like Frege, the Logical Positivists, and many others, found in it inspiration or support for their own dreams. What is less known are its more realistic counterpart-dreams in Leibniz's own thought. For he soon realized the intrinsic limitations of the C.U. model and the practical difficulties of implementing it. As against Wilkins's optimism, Leibniz was aware of the very problematic state of human knowledge at the time, in spite of (or perhaps due to) the enormous progress of the sciences in the 17th century. He compared knowledge to

> a huge magazine, loaded with merchandises of all kinds, but all mixed up and unordered, without the possibility of gaining access to any of them by means of numbers, letters or any other index, without any inventory, without any record of input and output, from which some light could be drawn.[10]

How could such a magazine serve as the basis for a systematic classification of knowledge and for the conceptual analysis that was to yield the full "alphabet of human thoughts"? A pre-requisite for this was the creation of an Encyclopedia – a project to which Leibniz devoted much thought and effort. Unlike other such projects, however, the leibnizian encyclopedia was subordinated to his idea of a "General Science". Its task was to collect, in an organized and usable way, the totality of available knowledge, or order to use it as a guide for future discoveries. Once complete, its lacunae would indicate what remained to be investigated,

> car en decouvrant tout d'une veue toute cette region d'esprit, déja peuplée, on remarqueroit bientost les endroits encore negligés et vuides d'habitans. La geographie de terres connues donne moyen de pousser plus loin les conquestes des nouveaux pays. On envoyeroit des colonies pour faire des plantations nouvelles dans la partie la moins connue d'Encyclopedie ...[11]

But the realization of the encyclopedia would require the cooperation of many scholars, would take a long time, and would therefore require a provisional classification of knowledge that would evolve along with the progress of the work. Rather than a cumulative and absolute representation of the nature of things, the leibnizian encyclopedia turned out to be, precisely because of its ambition of comprehensiveness, an ever provisional dynamic representation and organization of our knowledge *as it is*

[10] Gottfried Wilhelm Leibniz, A VI 4, 440.
[11] Gottfried Wilhelm Leibniz, A VI 4, 696.

known to us at each stage of the progress of science. For the totality of sciences is in fact like an

> ocean sans interruption ou partage, bien que les homes y conçoivent des parties et leur donnent des noms selon leur commodité.[12]

Such an open-endedness characterizes not only the content basis of the "method of invention" that is to be provided by the encyclopedia, but also its methodical apparatus. The closure and completeness, typical of the deductive procedures of a calculus, restrict the scope of rational invention to formalizable domains and cannot therefore be equated with a *general* method of discovery. Formalization itself depends upon the possibility of unequivocal interpretation of natural language expressions – which cannot be taken for granted. Furthermore, many domains require tools, such as probabilities, presumptions, hypothetical reasoning, weighing pros and cons, and other means of reaching conclusions, that are no doubt reasonable though not necessary or certain.

> Two are the ways of *heuretics* or the inventive art, the one *demonstrative*, the other *indicative*. The former proceeds demonstratively, i.e., by definitions and axioms. … The *indicative* way is no doubt the way of invention that not so much discovers as suggests …[13]

While the main example of the former is mathematics, the *Topics* are mentioned as an example of the latter. Leibniz's grand dream was to develop a "New Logic" that makes room for both the "softer", dialectical character of the latter and the "harder", logico-deductive character of the former. It should be clear that the inclusion in this model of a "soft" component, with its hermeneutic component and non-conclusive inference procedures, amounts to the acknowledgment by Leibniz that scientific communication cannot always be maximally transparent.

Leibniz nevertheless shared the basic assumption of his century – which the 18th century also shared and built upon – namely, the unity of science. This was predicated upon a belief in the unity of reason, which in its turn implied the unity of method, the unity of scientific language, the universal intelligibility of science, and the appropriateness of the scientific approach for treating every subject matter. These beliefs informed the *praxis* of scientific communication throughout the 18th century,

[12] Gottfried Wilhelm Leibniz [= C], *Opuscules et fragments inédits*, ed. Louis Couturat, Paris, Presses Universitaire de France, 1903 (C, 350).
[13] Gottfried Wilhelm Leibniz, A VI 1, 279.

and gave substance to the abstract idea of a "République des Lettres" or community of all scholars. The scientific journals of the 17th and 18th centuries indeed presume that their readers can understand and intelligently discuss the most recent results in all fields of investigation, which are reported and discussed in their pages next to each other.[14] Both this "encyclopedic reader" praxis and its underlying belief in the unity of science will fade away in the following century.

Can specialized science be unified science?

The Spanish philosopher José Ortega y Gasset pointed out the paradoxical nature of the phenomenon of scientific specialization. According to him, this trend in scientific research began to gain momentum in the 19th century, when a person of culture was still defined as 'encyclopedic'. "The 19th century – he writes – begins under the leadership of persons that live encyclopedically, even though their production is already typically that of experts".[15] By the end of the century, however, the intellectual leadership of Europe was already in the hands of a new type of scientist:

> a person who, from all that one must know as a cultivated person, knows only a specific science, and, within it, knows well only that small portion which he actively investigates. He sees as a virtue the fact that he does not know anything that lies outside the narrow landscape he cultivates, and calls "dilettantism" the curiosity for the ensemble of knowledge.[16]

Such an individual, "closed in the narrowness of his visual field", makes his particular scientific niches, "which he alone knows", progress; paradoxically, he thereby contributes also to the "encyclopedia of thought, which he studiously does not know". For Ortega, this paradox is possible due to what he calls "mechanization":

> Mechanization. A large part of the things that must be done in physics and in biology consists in mechanical thinking jobs,

[14] For some examples, see Marcelo Dascal and Cristina Marras, "The *République des Lettres*: a Republic of quarrels?". In M. Dascal, G. Fritz, T. Gloning, and Y. Senderowicz (eds.), *Scientific Controversies and Theories of Controversy* (Technical Report 3 of the research project Controversies in the *République des Lettres*, Giessen, 2002, pp. 3-19.

[15] José Ortega y Gasset, *La rebelión de las masas*, Madrid, Revista de Occidente, 1934, pp. 166ff. Translations of Ortega's quotes are mine.

[16] Ortega y Gasset, *ibid.*

> which can be performed by anyone. For the purposes of endless research tasks it is possible to divide science in small segments, to enclose oneself in one of them and to ignore the rest. The firmness and precision of the methods permits this provisional and practical disarticulation of knowledge. One operates with one of these methods like a machine, and there is no need to have clear ideas about the machine's meaning and foundations. Thus, most scientists push the general progress of science isolated in their laboratories, as bees in their cells.[17]

On this view, none of the experts that are extremely capable in doing their jobs has the slightest idea how what they do combines with what the other experts do in order to produce "the big picture". Actually, they don't even try to communicate with each other about their jobs, for they wouldn't be able to understand each other. The unity of science turns out to be a matter of luck, the result of divine intervention or of an "invisible hand" that coordinates individual efforts for man's benefit.

Writing about 50 years later, J. R. Oppenheimer also rejects the "false image" evoked by the expression "the unity of science", which the logical positivists in the mid-20th-century tried to promote, "the image of a few fundamental truths, of a few methods, techniques, and critical ideas, from which all scientific discoveries would derive, as if from a center, the access to which would provide the explanation of atoms and galaxies, genes and sense organs".[18] Instead, he considers the unity of science as being of a "virtual" nature:

> All [science's] parts are offered to everyone – and this is not a merely formal invitation. There are in the history of science many examples of fruitful contact between two sets of techniques or ideas developed in separate contents, leading to the discovery of a new truth. Sciences fecundate each other and develop through common contact and initiative. Hence, if the scientist can benefit from taking stock of what happens in a specialty different from his, it does not follow that he has to study all the remaining specialties. So, the unity (of science) is virtual – the unity of things which, if put together, can clarify each other. It is neither global, nor total, nor hierarchical.[19]

[17] Ortega y Gasset, *ibid.*

[18] Justus R. Oppenheimer, *Science and the Common Understanding*, New York, Simon and Schuster, 1966, p. 98.

[19] Oppenheimer, *ibid.*

Full of inter-disciplinary and open-mindedness fervor, Oppenheimer only forgot to spell out the extent of the difficulties involved in the "fecundation" across different disciplines he expects to emerge, virtually.

The extent of cross-disciplinary opacity

There is no point in engaging in the worn-out complaints against specialization, for this would hardly change the situation. It is better to try to understand how much opacity it generates.

Though rarely a celebrity, the specialist or expert is, today, our real cultural hero. For he or she is universally perceived as the producer, guardian, and applier of our most important treasure: knowledge. Naturally s/he strives to preserve and enhance this treasured status in all possible ways. One of these ways is the development, by each sub-discipline, subfield, or sub-sub-field, of its own "language". By this I do not mean only a very specific technical terminology, but also of a specific conceptual framework, of specific ways of arguing, of identifying and solving problems, of defining research aims, and so forth. Each sub-discipline thus becomes rather esoteric for "outsiders" – including those that practice another subdiscipline within the same "mother discipline".

Translation may be possible across such barriers, but it is not at all easy. For – I submit – there is a sort of "linguistic relativity" between the languages of different scientific fields similar to the one the linguist Benjamin Lee Whorf detected between different natural languages.[20] Whorf stressed that such differences go very deep, so much so that speakers of different languages think according to the different "worldviews" the structure of the languages they speak provide for them. Far-fetched as it may seem, this suggestion might explain why *true* inter-disciplinarity is so difficult to achieve. It is simply too difficult to really *learn to think* in terms of a deeply different language, so that participants in inter-disciplinary teams tend to fall back in their familiar disciplinary conceptual/linguistic patterns as soon as they face serious problems of transparency in understanding their co-researchers coming from other fields.

The depth of the linguistic/conceptual barrier within sub-fields of subdisciplines of the same discipline is well attested in mathematics – the field the classical ideal saw as the archetypal case of universal scientific transparency.

[20] Benjamin Lee Whorf, *Language, Thought and Reality*. Cambridge, Mass., The MIT Press, 1956.

The mathematicians Philip Davis and Reuben Hersch have proposed a way to estimate the number of mathematical subspecialties, characterized as specific areas of mathematics which holders of a Ph.D. in that domain are competent to understand.

They asked "How many mathematics books should the Ph.D. candidate in mathematics know?" And replied: "Sixty to eighty volumes".[21] Then, taking the 60.000 volumes of the mathematics library of Brown University as a good representative collection of mathematical knowledge, they continued:

> Dividing 60,000 books by sixty, we find there should be at least 1,000 distinct subspecialties. But this is an underestimate ... [A more accurate estimate] would show mathematical writing broken down into more than 3,000 categories. In most of these 3,000 categories, new mathematics is being created at a constantly increasing rate.[22]

And mathematics is indeed rapidly expanding, as shown by a comparison between the classification of its subdivisions in 1868 and 1979, which jumped from 38 to 3400 sub-specialties (see Appendix A).

The problem is not only the number of sub-disciplines but the gap between them. According to Davis & Hersch, a professional mathematician cannot really understand more than two or three neighboring fields, and even this not perfectly:

> Consider, say, the two fields of nonlinear wave propagation and category-theoretic logic. From the viewpoint of those working in each of these areas, discoveries of great importance are being made. But it is doubtful if any one person knows what is going on in both of these fields. Certainly ninety-five percent of all professional mathematicians understand neither one nor the other.[23]

This is, of course, a very serious problem for whoever believes in the possibility of overcoming opaqueness in cross-scientific research and communication. It is also a serious problem for whoever has to compare and evaluate funding requests for projects even in adjacent scientific areas.

[21] Philip Davis and Reuben Hersch, *The Mathematical Experience*, Harmondsworth, Penguin Books, 1983, p. 17.
[22] Davis and Hersch, *ibid.*
[23] Davis and Hersch, p. 22.

Is the allocation of research funds arbitrary?

Based on their experience in mathematics, Davis & Hersch's reply to this question is an appalling "Yes!":

> Under these conditions, accurate judgment and rational planning are hardly possible. And, in fact, no one attempts to decide (in a global sense, inclusive of all mathematics) what is important, what is ephemeral.[24]

The "importance" of a field – i.e., the amount of funding it will get – is judged by such formal measures as the amount of activity in it (publications, departments devoted to it, grants received, number of students, etc.), which is in turn a function of the amount of funding it has. Under the assumption that content cannot be compared across fields, no attempt is made to determine whether two hundred theorems or experimental results in one field are indeed worth more money than one finding in another. The evaluation of the results in each sub-field is, so to speak, entirely "autonomous", and the ranking or reputation of experts in each sub-field – which plays an essential role in such an evaluation – is determined essentially within the field itself.[25]

If this grim picture is true within mathematics, it is even truer when decisions regarding the allocation of funds across all the domains of knowledge are concerned. The crucial problem of the agencies and institutions that have to make such decisions is the lack of criteria that apply, across domains, to *content.* In this respect I can rely upon my experience both as Dean of Humanities and head of one of the committees of the Israel National Research Foundation. The fact is that such agencies take for granted the existing disciplines, sub-disciplines, and sub-sub-disciplines that are "active" and their contingent institutional allotment to faculties, departments or institutes. They then distribute the financial "cake" among them according to the presumed costs of their activity, without making any serious effort to compare – formally or content-wise – between disciplines, and usually willingly yielding to the pressure of some of these disciplines' well-organized lobbies.

[24] Davis and Hersch, *ibid.*

[25] On the role of reputation in the assessment of academic achievements, see M. Dascal, "Reputation and refutation: the negotiation of merit", in E. Weigand and M. Dascal, *Negotiation and Power in Dialogic Interaction*, Amsterdam, John Benjamins, 2001, pp. 3-17.

In many such agencies it is customary to distinguish administratively between three "divisions": Exact sciences & technology, Biomedical sciences, and Humanities and social sciences. Each of these is, in turn, subdivided and sub-subdivided employing the labels typical of the currently familiar disciplinary matrix. The categories and sub-categories reflect, of course, the contingent level of activity and interest of each field in different countries, as a quick glance at recent Israeli and Swiss classifications shows (see Appendix B).

Submitted projects are administratively placed in sub-category pigeonholes and committees are appointed to evaluate and rank them. Needless to say that the members of such committees cannot be experts in all the sub-fields grouped in a sub-category. They resort to expert evaluation in each sub-field, but the experts consider only each project individually. The comparative task is left to the committee, which has little or no tools to perform it objectively. What to say of the high level cross-category committees that have to base their decisions only on the standard evaluation forms they receive from the lower level ones?

At no point in the decision process the members of all these committees meet with each other, across disciplines or fields, to discuss criteria, importance, problems, needs, etc.! How can they be sure, under these conditions, that the way in which they fill the evaluation forms, the meaning of their actual evaluations, the content of the criteria that guide their proposed ranking, are "transparent"? And even if they met and discussed, would this ensure that they would, in the course of some brief meeting, really understand each other, coming as they do, not only from different disciplines but perhaps also from at least two radically different "cultures"?[26]

Intra-scientific communicative opacity

If we move now to communication within a determinate field, we might expect that here, at last, transparency should prevail. Experts communicating with experts in the same narrowly defined field, publishing in and reading the same specialized journals, should have no problems understanding each other. I am sorry to disappoint you, but this is not the case either. Recent studies of the language of science as provide convincing

[26] See C. P. Snow, *The Two Cultures and a Second Look*, Cambridge University Press, 1963.

evidence regarding the problems of transparency *within* any given scientific field.

Let me mention, first, the acknowledgment that metaphor is not extraneous to scientific language and to scientific thought, or at most ornamental to it, as pretended by the classical picture. Since the pioneering studies of Mary Hesse in the 1950's, it is now widely recognized that metaphor plays a cognitively essential role in science.[27] Scientific theories and the scientific "research programmes" they belong to are shaped by the "root" or "core" metaphors their proponents choose in order to express their basic insights. And scientific controversies often arise due to the choice of conflicting metaphors. An example should suffice to illustrate the importance of metaphor in science.

Brian Greene, in his excellent attempt to make intelligible "superstring theory" to a wide audience, writes:

> Einstein showed the world that space and time behave in astoundingly unfamiliar ways. Now, cutting-edge research has integrated his discoveries into a quantum universe with numerous hidden dimensions coiled into the fabric of the cosmos ... Although some of these concepts are subtle, we will see that they can be grasped through down-to-earth analogies. And when these ideas are understood, they provide a startling new perspective on the universe.[28]

Greene warns the reader, however, against «the illusion of the familiar», i.e. assuming that the «large», «extended» spatial dimensions we are familiar with are the only ones.[29] The analogy given to show that there might be non-easily perceived or perceivable dimensions is that of a garden hose. Seen from a quarter mile, the hose looks like a one-dimensional line, without its curled girth. For the ant that lives on the hose, however, the curled girth is an essential "dimension" in its life.

Being able to see things from a new perspective – which is what metaphors are all about – is thus essential for performing the gestalt switch necessary for conceiving the very idea of "coiled dimension", which lies at the core of super-string theory. Through this switch, the meaning of the term 'dimension' is modified so as to induce us to encompass both, the

[27] Mary Hesse, *Models and Analogies in Science*, South Bend, Ind., University of Notre Dame Press, 1966.
[28] Brian Greene, *The Elegant Universe: Superstrings, hidden dimensions, and the quest for the ultimate theory*, New York, W. W. Norton, 1999, pp. x-xi.
[29] Greene, p. 184.

familiar space-time coordinates and the unfamiliar new entities, even though we do not quite know exactly what properties they share. Regardless of whether the new concept can be cashed out in precise mathematical equations [notice that 'cashing out' is also a metaphor], it is doubtful that one could understand and communicate this new meaning of 'dimension', even if one actually were a specialist in this field, without the liberty of indeterminacy granted by capturing it through insightful analogies and metaphors. Analogies and metaphors, however, are examples of linguistic devices that, although illuminating, can hardly be considered "maximally transparent", in terms of our working definition.

The other linguistic fact about scientific communication worth mentioning, even briefly, is the "rediscovery" of what may be called the "rhetoric of science" – the title of a well-known book by Alan Gross.[30] In another recent book, Gross and his colleagues study the changes in the style and terminology, as well as the standardization of the format of what we are familiar with today as "the scientific article".[31] This study demonstrates that "scientific style", along with its adjunct means of persuasion such as graphs and statistical tables, evolved significantly, and spread throughout the thousands of hyper-specialized scientific journals. Presumably this evolution resulted from a spontaneous collective effort of editors for achieving more intra-disciplinary transparency. Viewed as a rhetorical device, however, isn't this imposed style rather a means to pre-determine the kinds of acceptable argumentative strategies according to some ideal of what science and scientific discourse should be like? In this sense, "the scientific article" represents just one among other rhetorical options of rational persuasion. Its purported transparency, then, might be nothing but a veil hiding the methodological – if not metaphysical and political – positions of the "scientific establishment". Unveiling this veil no doubt requires more than the usual amount interpretative effort.

Extra-scientific communicative opacity

I will leave aside the important and fascinating problem of the "popularization" of science, and focus on what seems to me the main issue of the "transparency" of science for the public at large.

[30] Alan G. Gross, *The Rhetoric of Science*, Cambridge, Mass., Harvard University Press, 1990.

[31] Alan G. Gross, Joseph E. Harmon, and Michael Reidy, *Communicating Science: The Scientific Article from the 17th Century to the Present*, Oxford University Press, 2002.

Three established disciplines devote themselves today to the study of the object "science", and play a decisive role in elaborating the image of science that, ultimately, will be filtered out to the public at large: The philosophy of science, the history of science, and the sociology of science.

The philosophy of science or "theory of science" has been (justly) accused of "selling" science to the public by idealizing and legitimizing it.[32] The philosophy of science indeed seeks to account for the "objectivity" and "validity" of scientific knowledge, which are, ultimately, the grounds for society's reliance upon science and its generous financial support of science. The history of science and, more recently, the sociology of science, are not concerned with justifying or selling science, but with describing it as it is. And the picture of science they present is quite different from the methodical, systematic, and judicious idealized image of what science should be according to the philosophical standards of the theory of science.

The immediate – and crucial – problem of transparency for the public and for decision-makers posed by this situation is: which of these images of science to buy? Upon which of them to base public decisions to build or not to build nuclear plants, to allow or not to allow for genetic engineering, to finance or not to finance this or that research project. No doubt the answers to these practical questions comport a decisive ethical component. But they must also be based on some reasonable understanding of both the contents and workings of science.

Living with science, without transparency

A rather pessimistic answer, based on the study of science as a human activity among others, comes from the quarters of the sociology of science. As a human activity science is plagued by all the foibles of human action. Self-interest, power-seeking, arrogance, distortion of the facts, and insecurity – all masterfully camouflaged by a pretense of self-forgetfulness, truth-seeking, modesty, objectivity, and certainty. On the basis of these observations, some sociologists insist that we should set aside the idealized image science and the philosophy of science try to project (and with it, the ideal of transparency), and focus on the dire realities of scientific activity. In particular, we should focus on how scientific controver-

[32] Helmut F. Spinner. *Popper und die Politik*, Bd. 1, Geschlossenheitsprobleme. Berlin: J. H. W. Dietz Nachf., 1978, p. 147.

sies are "resolved". We would then see that transparency and pure rationality are far from being the central and only players in the scientific game. This should suffice for combating the pretense of experts to be the owners of the truth and its exclusive spokespersons. And this is "what everyone should know about science" – as the subtitle of Harry Collins and Trevor Pinch's THE GOLEM goes. According to these authors,

the assumption that "if the person in the street knows more science – as opposed to more *about* science – they will be able to make more sensible decisions" about nuclear power stations, environment, etc. "ranks among the great fallacies of our age. Why? – because PhDs and professors are found on all sides in these debates.[33]

Since we have shown that scientists at the research front cannot settle their disagreements through better experimentation, more knowledge, more advanced theories, or clearer thinking. It is ridiculous to expect the general public to do better.[34]

> What to do, then? Well, simply to treat science as no more and no less than expertise and scientists as no more and no less than experts. As such, they are "neither Gods nor charlatans; they are merely experts like every other expert in the political stage".[35]

Once their pretensions to omniscience are thus deflated, everyone will know how to deal with non-transparent science, for "the citizen has great experience in the matter of how to cope with divided expertise".[36]

To be realistic about science, however, may lead to a more optimistic outlook. And Leibniz's second dream, the one I called "realistic", might help us to feel better than the sociologists' picture. To be sure, Leibniz sides with Collins & Pinch in rejecting, against Descartes, certainty as the necessary foundation and result of scientific knowledge. Instead, he stresses its provisional character, its ever evolving nature. Furthermore, like Collins & Pinch, Leibniz was an acute observer of the scientific (and other) controversies of his time, as well as an active participant in them. He was aware of the many not quite rational factors that interfered in such controversies and prevented them to be solved by the ideal method of having the contenders sit down, formulate their positions in terms of the Characteristica Universalis, and then calculate who was right. At

[33] Harry Collins and Trevor Pinch, *The Golem: What everyone should know about science*, 2nd ed., Cambridge University Press, 1994, p. 144.
[34] Collins and Pinch, pp. 144-145.
[35] Collins and Pinch, p. 145.
[36] Collins and Pinch, *ibid.*

times he was extremely skeptical: even if men possessed the marvelous C.U. method, he argued, their quarrelsome character would prevent them to use it:

> Let us suppose, just for fun, that we could find the truth, that one could establish uncontestable principles, that it is possible to have a sure method to deduce from them important consequences, and that God himself sends to us from heaven this new Logic. I am convinced that, in spite of that, men would not cease to quarrel, as they usually do.[37]

But he firmly believed, if not in the absolute rationality, at least in the reasonableness of men, especially those who praised knowledge. Through the development of the "softer" logic mentioned above, another, less demanding and pretentious method of handling controversies would emerge. By applying it, apparently irreconcilable quarrels would be eventually transformed into manageable controversies, and the opposed positions would be made intelligible to both sides, so that *Verständigung* – if not reconciliation – could be achieved. He did not hesitate to advertise the merits of this "other method", as he had earlier (and also later) advertised the C.U. method:

> I once talked about this method with a great Prince, who objected at first that many others had already proposed supposedly new methods, but that no advancement had been achieved thereby. I called his attention immediately to the difference between my promise and theirs: for they always promise very easy methods, by which they hope to convince their adversaries in a short time; whereas I declare that the method I undertake is very difficult, and that it requires great dedication and a great deal of time.[38]

No doubt the application of this method would require a lot of interpretative work by the contenders and by the "judge of controversies", and would yield nothing comparable to the glamour of perfect transparency. But it would be humanly feasible and would certainly contribute more to the achievement of science's ultimate aim – the happiness of humankind

[37] Gottfried Wilhelm Leibniz [= D], *Conversation between father Emery the hermit and the Marquis of Pianese, minister of state of Savoy, which yielded a remarkable change in the minister's life* (= A VI 4, 2245-2283); translation and commentary in Gottfried Wilhelm Leibniz [= D], *The Art of Controversies and Other Writings on Dialectics and Logic*, ed. M. Dascal and Q. Racionero (Forthcoming).
[38] Gottfried Wilhelm Leibniz, *On Controversies* (= A IV 3, 204-212); translated in D.

– than the instant light-producing, miraculous methods proposed by others (and by Leibniz himself).

Well, you may wonder, what should we – concerned as we are with the lack of transparency in intra-, inter- and extra-scientific communication, and having to make important decisions on the basis of such non-transparent communication – do? Suppose we accept your analysis and your thesis that opacity is inevitable: what are the practical implications that follow from it? In short, what do you recommend as a reasonable and responsible way of "living with non-transparency"?

First of all – and most importantly – setting aside the illusion of transparency does not mean espousing opacity as a norm. For it does not mean giving up the two basic principles of the ethics of communication: the duty to make oneself understood and the duty to understand. Under the prevalent condition of lack of transparency, these duties are in fact more important – and more difficult too – to fulfill. Scientists must be aware that they cannot presume, by virtue of some special property of the language they use, to be easily understood (by colleagues in the same building, as well as by scientists in the next building and laymen), just as they do not easily understand colleagues and laymen. The same applies to the latter. To use esoteric technical jargon is, in many cases, not to fulfill the duty to make oneself understood and to deliberately hide behind opacity instead of making the required communicative effort. To dismiss allegedly "difficult" explanations is, likewise, not to make a reasonable effort to understand, and amounts to all to easily accepting a veil of opacity.

Although transparent understanding is unlikely to ever be maximal, it is a regulative idea worth striving for, not because it is achievable, but because of the intrinsic cognitive (and also human) value of such a striving. By making the effort to understand what is at first unintelligible, one clarifies one's difficulties in understanding and is thereby able to raise (for oneself or for the speaker) the relevant questions that may resolve such difficulties and improve one's understanding of the issues in question. By making the effort to reply to such questions (or, better, to anticipate them) one realizes not only what was problematic *for the addressee* in one's earlier formulation, but also – to a large extent – *for oneself*, and thus clarifies one's own thought.

The direct, real, and active presence of the addressee is extremely valuable in the process of overcoming – as much as possible – the barrier of opacity. Quite often the possibility of asking a simple clarification question or of raising some objection can clear up gross misunderstandings.

Unfortunately, the time reserved for "discussion and questions" on most academic and other occasions is often taken more as a conventional ritual than as essential in the process of communication. Written documents (recall the "evaluation forms") are taken to be more "objective" than oral exchanges. And formulae in a symbolic language are considered to be clearer than their counterparts in natural language. All of these – and similar assumptions – are questionable and, since – as we have seen – they do not eliminate or significantly decrease opacity, they should not be taken for granted, and the opposite assumption should be given a fair chance to demonstrate their utility in this respect.

Again, all of this implies much more communicative effort on the part of all involved in communicating science than is usually made. More meetings, more preparation for such meetings, more hours to reach conclusions and decisions, more different languages or conceptual structures to learns, less automatic procedures – all meaning that a larger share of our limited cognitive resources will have to be devoted to the communication of knowledge. Why all this trouble if – as many believe – the communication of knowledge is completely peripheral, if compared to the principal business of science, which is the production of knowledge? This belief, however, is mistaken. Not only due to the cognitive gains involved in the attempt to communicate properly, which I have indicated above; but also because knowledge growths not by beginning from zero at each stage, but by learning from, debating with, criticizing and, ultimately, building upon "the shoulders" of the knowledge produced by preceding generations, a knowledge available to us, albeit not transparently, through… scientific communication!

APPENDIX A: SPECIALTIES IN MATHEMATICS

CLASSIFICATION OF MATHEMATICS IN 1868 *Jarbuch über die Fortschritte der Mathematik*	CLASSIFICATION OF MATHEMATICS IN 1979 *Mathematical Review*
– History and Philosophy – Number Theory – Differential and Integral Calculus – Analytic Geometry – Theory of Functions – Mathematical Physics – Algebra – Probability – Series – Synthetic Geometry – Mechanics – Geodesy and Astronomy – ...	– General – History and biography – Logic and foundations – Set theory – Combinatorics, graph theory – Order, lattices, ordered algebraic structures – General mathematical systems – Number theory – Algebraic number theory, field theory, polynomials – Linear and multilinear algebra, matrix theory – Commutative rings and algebras – Algebraic geometry – Associative rings and algebras – Algebraic topology – Manifolds & cell complexes – Global analysis, analysis on manyfold – ...
38 subcategories	**Approximately 3400 subcategories**

APPENDIX B: ISRAELI AND SWISS CLASSIFICATIONS OF RESEARCH DOMAINS AND SUB-DOMAINS

ISRAEL RESEARCH FUND (2001/2) AREA OF EXACT SCIENCES AND TECHNOLOGY	FONDS NATIONAL SUISSE (2001) MATH, SC. NAT. ET DE L'INGENIEUR
– Astrophysics and Particles, Nuclear and Plasma Physics – Engineering and Technology – Chemistry and Materials Science – Sciences of the Environment – Mathematics and Computer Science – Physics of Fluid State	– MATHEMATIQUES – ASTRONOMIE, ASTROPHYSIQUE ET RECHERCHE SPATIALE – CHIMIE Chimie physique, Chimie inorganique, chimie organique – PHYSIQUE Phisique theorique, Physique nucleaire, Phisique des particules elementaires, Phyisique de la matiere condensee, Physique des plasmas, Autres – SCIENCES DE L'INGENIEUR Genie civil, Ingenieur des machines, Dynamique des fluides, Construction electrique, Sciences de materiaux, Informatique, Genie chimique, Microelectronique et optoelectronique, Autres – SCIENCES DE L'ENVIRONEMENT Pedologie, Geomorphologie, Meteorologie, Climatologie, physique de l'atmosphere et aeronomie, Hydrologie, limnologie et glaciologie, Autres – SCIENCES DE LA TERRE Geologie, Geophysique, Geochimie, Geochronologie, Paleontologie, Mineralogie, Autre

ISRAEL RESEARCH FUND (2001/2)	**FONDS NATIONAL SUISSE (2001)**
AREA OF OF LIFE SCIENCES AND MEDICINE	BIOLOGIE ET MEDECINE
– Immunology – Ecology – Botanic – Biochemistry, Biophysics and Biotechnology – Molecular Biology and Molecular Genetic – Cell Biology and Physiology – Zoology – Microbiology and Parasitology – Neurobiology – Medicine	– SCIENCES BIOLOGIQUES DE BASE: Biochimie, Biologie Moleculaire, Biologie Cellulaire et Cytologie, Genetique, Embryologie, Biologie Genetique, Microbiologie Experimentale, Biophysique – BIOLOGIE GENERALE: Botanique, Zoologie, Anthropologie et Primatologie, Agronomie et Sciences Forestieres, Sciences de l'Environnement, Ecologie – SCIENCES MEDICALES DE BASE: Recherches sur les Structures, Neurophysiologie et Neurologie Cerebrale, Cardio-angiologie, Endocrinologie, Physiologie: Autres Secteurs, Pharmacologie et Pharmacie, Microbiologie Medicale – MEDECINE EXPERIMENTALE: Recherches Experimentales sur le Cancer, Pathophysiologie, Immunologie et Immunopathologie, Etude du Comportement – MEDECINE CLINIQUE: Medecine Interne, Chirurgie, Cancerologie Clinique, Pharmacologie Clinique, Dermatologie, Gynecologie, Pediatrie, Neurologie et Psychiatrie, Oto-rhino-laringologie, Ophtalmologie, Medecine Dentair, Medecine Tropicale, Biomedical Engineering, Cardiologie Clinique, Endocrinologie Clinique, Pathophysiologie Clinique, Immunologie et Immunopathologie Clinique – MEDECINE PREVENTIVE: Troubles Psychiques et Maladies Psychosomatiques, Toxicomanies, Cancer, Troubles du Metabolisme, Maladies Infectueuse, Maladies Osteo-articulaires, Troubles respiratoires, Methodologie de l'Epidemiologie

ISRAEL RESEARCH FUND (2001/2)

AREA OF HUMANITIES
AND SOCIAL SCIENCES

- Art and Musicology
- Archeology
- Geography
- History
- Education
- Economy and Business Administration
- Political Science and Communication
- Bible and Talmud
- Law
- Sociology
- Statistics
- Literature
- Social Work
- Philosophy and Jewish Thought
- Psychology
- Languages and Linguistics

FONDS NATIONAL SUISSE (2001)

SCIENCES HUMAINES ET SOCIALES

- PHILOSOPHIE, SCIENCES RELIGIEUSES ET SCIENCES DE L'EDUCATION
  Philosophie, Sciences des Religions et Theologie, Histoire de l'Eglise, Pedagogie et sciences de l'Éducation, Psychologie
- SCIENCES SOCIALES, ECONOMIQUES ET JURIDIQUES
  Sociologie, Sciences Politiques, Economie Politique, Economie d'Entreprise, Sciences Juridiques, Geographie Humaine et Economique, Ecologie Humaine
- HISTOIRE
  Histoire Generale, Histoire Suisse, Sciences de l'Antiquite
- ARCHEOLOGIE, ETHNOLOGIE, ETUDES DES ARTS ET URBANISME
  Pre-histoire, Archeologie, Ethnologie, Histoire de l'Art, Musicologie, Theatre et Cinema
- LINGUISTIQUE ET LITTERATURE
  Langues Germanique et Anglaise, Langues Romanes, Autres Langue