Forgetting, celebrating and reconstructing past science

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Contribution to the meeting
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— 1 —

Until half a century ago or so, the sciences – at least the natural sciences, the humanities and social sciences present problems to which I shall return – were supposed by most philosophers and historians of science to be by their very nature cumulative and progressive. They were in no need of anamnesis, everything was retained straightaway (except mistakes, which could be safely buried). The general public, though sometimes scared by the technological and world-shaping outcome of the applications of science, still tends to think so. For good reasons, we must admit. To the general public, the proof of the pudding is the eating, and if we measure science by, say, the ability to predict the trajectories of planets and spatial vehicles or the development of new antibiotics (or to explain the mechanisms by which we make older ones inefficient), then science does progress.

Whether science is *cumulative* turns out to be a different question, and this is indeed where doubt set in around 1960 among philosophers of science – some of those established as such, and most famous of all, a historian of science failing to find linear progress and turning philosopher in the process of understanding *why*, namely Thomas Kuhn, in *The Structure of Scientific Revolutions* from 1962, and in a paper on "The Function of Dogma in Scientific Research", presented

in 1961 and published in 1963.[1]

What remained from Kuhn's argument in general discourse was little more than the term "paradigm" (rarely understood exactly as Kuhn intended it, but even his usage was somewhat blurred) and perhaps the idea that successive paradigms were "incompatible", understood by critics more radically than intended, and therefore emphasizing the non-cumulative character of scientific practice even more strongly than Kuhn himself. His view on the matter was summed up in the formulations that scientific development was Darwinian in nature, "from more primitive beginnings but toward no goal". This denial of progress toward a goal named "truth" hinged on his identification of truth with *an ontology*, a list of constituents of the world. In 1718, indeed, Stahl introduced phlogiston – a material principle of combustion – as a means to bring order to the multitude of combustion and reduction phenomena, and half a century later Lavoisier pushed the same principle away in order to make place to oxygen; Newton introduced the gravitational force as an explanation how the planetary system works, and Einstein's theory of General Relativity replaced it by the geometrical properties of space itself. In both cases we do have steady progress in the predictive power of the discipline, but speaking of steady improvement of the ontology seems absurd.

Kuhn first meant "paradigm" to designate the "great book" (e.g., Ptolemy's *Almagest* or Newton's *Principia*) – or, in more recent times, the standard set of textbooks – on which everybody in a discipline is trained, and this is where oblivion comes in. Training is indeed more than just accepting as true a set of statements, as everybody knows who has thought about how you are trained to swim, to run a bicycle or drive a car. Training reaches the level of your mind which makes you (re)act without thinking about why you do so. In order to understand why you act as you do on the foundation of training requires quasi-Kantian *critique*, which is always difficult and slows down work – Newtonian physicists never bothered much about Kant's *Kritik der reinen Vernunft*. Training, however, is not necessarily cumulative: having been trained to drive a car, you may certainly extend your training by trying to drive on ice; but beware not to rely on your training in running a motorbike when starting car training – better forget it for safety!

Now let us have a look at the dynamics Kuhn sees in scientific development "*from* primitive beginnings but *toward* no goal" (I shall permit myself to present

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[1] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, [2]Chicago, 1970, [1]1962. *Idem*, "The Function of Dogma in Scientific Research", pp. 347–369 *in* A. C. Crombie (ed.), *Scientific Change*, London, 1963.

a "creative" and evidently very brief version[2]). The starting point is a field of interest – maybe (Kuhn's example) electric phenomena, maybe "the situation of women". Initially there are almost as many approaches as there are workers, nobody really understands why others are doing as they are, and there can be little progress; this is the "pre-paradigmatic phase". Then one approach seems to hold particular promises, and others begin to emulate; Kuhn's example is Franklin's theory of electricity and his experiments, in women's studies of the early 1970s Germaine Greer's *Female Eunuch* had a similar role. This leads to the second sense of "paradigm": *what you take over* when being trained from a particular book or textbook tradition: a set of classificatory and explanatory concepts, methods and tricks of trade, expectations and norms as to what the discipline should be interested in and how explanations ought to look, which handbooks and journals are important, etc. This constitutes a *discipline*. The elements of the paradigm are only in part learned from analytical explanation, in part they are genuine skills learned from non-verbalized training; and even that which is verbalized often has the character of *values* rather than rules that can be followed strictly.[3] Workers interested in the same real-life object but not sharing the paradigm in this sense do not belong to *the same* discipline in the epistemological sense, even though they may be employed at the same institute. They may still be able to learn from each other, but with the same difficulty as one learns from disciplines looking at different objects, through particular spectacles.[4]

Once a paradigm in this sense has been established, systematic and cumulative work can start. One worker's results and questions can be taken over by other workers, if needed questioned with understanding, because not only explicit statements but all their connotations, conceptual presuppositions etc. are shared. In this way, the explanatory power of the paradigm is exploited to

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[2] This "creative" interpretation is fully unfolded in my *Human Sciences: Reappraising the Humanities through History and Philosophy* (Albany, New York: State University of New York Press, 2000), pp. 277–394, *passim*.

[3] Emphasis on training and on the guidance by values rather than rules are the two main features that differentiate Kuhn's approach from others with whom he is often compared. This difference was rarely understood at the time – and in 1992 Kuhn himself had forgotten ("The Trouble with the Historical Philosophy of Science"; History of Science Department, Harvard University).

[4] In order to qualify this statement one should observe that "*the* paradigm" is not made of one piece: it is a nested structure. Some components of the training and scholarly norms of a linguist are shared with literary scholars, others with psychologists, still others with none of them.

the full; gradually, the range of problems it has solved increases – Kuhn sees the paradigm as an instrument for "puzzle solving", and the paradigm itself is sharpened as a tool in this process (it «accommodates»). But exactly because it allows those who are well trained in the paradigm to anticipate which kind of problems it allows to attack,[5] it helps to shelve and forget other interests which once were part of the inspiration for the field – once the paradigm of comparative linguistics had put the study of etymologies on a more secure base in the early nineteenth century, not only the methods used by the etymologist of the sixteenth and seventeenth centuries but also the questions that had occupied them came to be viewed as scholarly obscenities (as they still are today, if they have not become historical curios).

At some moment, intractable anomalies begin to abound, problems which the paradigm "promises" to be able to solve but which in fact resist understanding. Systematic work under the paradigm has exhausted its possibilities, and at the same time located the points rather precisely where it fails.[6] The discipline gets into a state of mild panic, the limits of the permissible widen, and the situation begins to resemble that of the pre-paradigmatic phase – until one of the swarm of innovative proposals has so much success that it serves to establish a *new* paradigm which replaces the old one. The process repeats itself, and new collective oblivion ensues, now hitting even the concepts and norms of the previous paradigm. Those who then grow up with this paradigm and are trained in it of course learn about the triumphs of their predecessors, but these triumphs are now told in the terms of the present cognitive framework. If confronted with original writings from before the "revolution" they tend to misunderstand what these are speaking about and looking for. The two paradigms are, in Kuhn's word, "incompatible", and understanding one from the standpoint of the other is as translating from one language into another; workers under the new

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[5] In this respect, the doctoral advisor is supposed to embody the paradigm, knowing in advance which problems should yield to good work, and which represent blind alleys which it would be irresponsible to ask or encourage the student to take up.

[6] In a passage from the *Dialektik der Natur* (MEW 20, 335f; as far as I know newer taken up by the philosophers of science of the socialist countries), Engels already noticed this dynamics: "In chemistry, only a century's work according to the phlogistic theory supplied the material which allowed Lavoisier to discover in the oxygen that Priestley had produced the real counterpart of the imaginary phlogiston substance, and thus to throw over the whole phlogiston theory". Another "Kuhnian" observation is packed into the quotation: Joseph Priestley produced oxygen, but understood it within the framework of the phlogiston theory as "phlogiston-free air"; only Lavoisier "invented" oxygen, thus engendering what developed into a new paradigm. Our ontology, the objects we see, depend on our paradigm.

paradigm, if not unusually attentive, tend to do so "naively", falling into the traps of "false friends" which any conscious translator knows so well.

This description is already highly abstract when we discuss the natural sciences; no wonder (notwithstanding the reviewers' amusement at the time) that Kuhn, when entering into the thicket of historical details in a book from 1987 about Max Planck and the early history of quantum mechanics, did not use the word "paradigm" a single time in its 400 pages. Within *the humanities,* Kuhn's picture may still give us some insights, but these will have to be thought through and sifted cautiously – Kuhn already pointed out that in the humanities training is not made from textbooks but from reading of original work, where no *one* approach normally has the monopoly (perhaps not quite as true today, but still a fairly adequate picture). In particular, we may think of the proliferation of fields called "studies": "English studies", "Women's studies", "post-colonial studies", "queer studies", "science studies", etc. What these have in common is to be defined from interest in some part of the external world, not through anything like a paradigmatic approach. They are by nature pre-paradigmatic – and since those who work in them usually already have a scholarly training, inter-disciplinary. For a while, a particular approach may achieve almost paradigmatic status – I mentioned *The Female Eunuch* – but as a rule, this approach is not eventually replaced by a new one because its possibilities have been exploited to the full, thereby creating an internal pressure for renewal and alternative; in a more colourful terminology, it is not exploded from within. Instead, it comes under attack because any *particular* approach tends to forget and repress problems from the field which, at second thoughts or because of a new political and socio-cultural situation, seem no less important than those interesting the quasi-paradigmatic approach (for example, in the case of women's studies, questions of working and educational possibilities and legal restraints, which Greer's sexual emphasis had driven to the periphery). Often, to say it crudely, changes of approach, interest and emphasis in the humanities tend to depend *even less* on internal cognitive dynamics and *even more* on fashion or adaption to external demands than in the natural sciences. That does not mean that oblivion of earlier approaches does not take place in the "studies" fields. However, since they were not created by a highly technicalized discipline, the barriers for understanding them are lower. For workers in women's studies today, the difficulties in understanding Simone de Beauvoir's *Deuxième sexe* will at most be a combination of the difficulties encountered by her readers in the year the book was written (1949) with those of reading any intellectual text from that year at 60 years' distance; such difficulties are quite different from those of a young geneticist who tries to grasp what things are really about in a paper

from a genetics journal from 1949. There is oblivion and oblivion.

Moreover: The oblivion of the geneticist may be necessary, in so far as skills are better wielded when you do not need to reflect; when working on the connection between sequences of amino acids in DNA strings, protein production and genes, the geneticist should not be bothered by questions and fumbling antedating the double helix and the cracking of the DNA-code – these would only disturb working concepts. At least when training the next generation of workers, it is important for efficiency not to teach all the meanings a concept (etc.) has had in history – that is, everything it does not mean any longer.[7]

The worker in a non-paradigmatic field needs forgetfulness of preceding approaches to the same extent and for the same reason as neglect of other intellectual fields – one cannot be actively interested in everything without becoming superficial.

On the other hand, both kinds of oblivion have a common effect: the field may forget where it came from, and thereby perhaps sever the connection to its roots in larger society. This does not necessarily happen, new roots or legitimizations may also arise or be constructed; but the invective "purely academic" illustrates the risk.

To sum up this first section: Science regarded as a depository of results remains in progress, and even accumulative. No paradigm shift bereaves Watt's steam engine of its degree of efficiency, and successive paradigm formations and paradigm shifts in thermodynamics and materials science have each contributed to improve the efficiency of steam engines. But science as an *intellectual* endeavour is not. By climbing on the shoulders of predecessors (giants or otherwise), scientists transforms them into stone plinths or at best pretty caryatids; past scientists are no dialogue partners.

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Until the 1950s (often but not always in subsequent decades), *history* of science supported the accumulative-progressive view of philosophers of science. Of

[7] There may of course be other reasons for such cognitive repressions, more rooted in institutional history than in epistemology. In the 1960s, it was impossible to make a thesis on "intuitionist" philosophy in the Mathematical Institute of the University of Copenhagen, since Harald Bohr – whose bust looked at everybody in the institute library – had belonged to the opposite, "formalist" school. In the Niels Bohr Institute it was equally impossible to have a serious interest in "hidden variables", since this was at odds with Niels Bohr's "Copenhagen interpretation" of quantum mechanics.

course it might present personal history, with or without integration into social, institutional and cultural context and with or without integration with technology (mostly without, it must be said). As a history of ideas, however, what it depicted was the caryatids of contemporary science – what a Kepler or a Newton had done was presented in modern formulae. If (say, but indeed true for both) they had developed their thinking on the partial basis of ancient harmonics (musical theory), that was at most mentioned as a curiosity, not explored; but mostly it was not mentioned, being irrelevant to that accumulation of still relevant results to which they had contributed.[8] Paradoxically as that may seem, historiography was thus a tool for oblivion – for the kind of oblivion inherent in the practice of the science of its time. Yet the paradox is that of any court history, by nature a *justification of the present rule*, and the history of science was largely written from a perspective similar to that of a court historian. The court in question was of course not that of political rule but that of prevailing science, and what was produced was the picture of the professional past which could please or flatter the practising scientist – "the royal road to me", in a formulation which goes back to my friend Ivor Grattan-Guinness but has since become a familiar jibe among historians of science of a new breed.[9]

This new breed came, if not to dominate the field then at least to have some importance from the 1970s onward. Their (better, "our" – I suppose I belong to the race) hermeneutics is not that of Gadamer – his concept of *Horizontverschmelzung* from *Wahrheit und Methode* (according to which the present conceptual world is a kind of fulfilment of the historical development and hence ultimately the key in which the past should be read) is indeed a justification of the traditional historiographic of science. Instead it tries to reconstruct the scientific concepts of the past from the web of texts (scientific, but not only scientific) from a particular epoch – to some extent following Wittgenstein's principle "don't ask for the meaning, ask for the use", at least trying to formulate past meanings on the basis of past use, and trying to avoid importing present-day meanings without being aware of it. We may call it "anamnestic", since it tries to recover memory of the past which had been repressed.

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[8] I have colleagues who might protest here and point to Alexandre Koyré as an exception. To some extent at least, it is true, Koyré (a run-away theologian) rather transformed early Modern science into a plinth for contemporary *philosophy*.

[9] We may also apply Georg Lichtenberg's aphorism: "Such works are mirrors. When a monkey looks into them, no saint can stare back".

Grattan-Guinness goes so far as to deny that what we are speaking of here be *history*, classifying it instead as "heritage". In that case, however, one may ask whether (for example) Livy wrote history.

At times those who make “anamnestic” history of science are as aware as other humanists and as philosophers of history that what we can achieve is only a *reconstruction*, an abstraction from which many aspects of the past have disappeared, and which may even be mistaken in some of the aspects it retains. They are also conscious that we can only reconstruct, that is, explain the past, by making use of our own language and concepts: not naively, not by using the concepts of actual scientific practice at face value, not by one-to-one mapping between past and present concepts, but so to say network-to-network – but still rooting our explanations (and the questions we ask to the past) in the world we live in. Others seem to be blissfully ignorant of these paradoxes of the historiographical endeavour (or at least write as if they were) and believe they can tell the truth about the past “on its own conditions”, or to find out *wie es eigentlich gewesen*, in the all-encompassing sense which is often attributed to Ranke though it was not his – being just as convinced to be right as the workers under the paradigm in one of the hard sciences and just as unable to understand other approaches than their own.

Many factors have gone into the rise of this trend. One (but only one) is the emergence of a professionalized environment. The pertinence and quality of the work of a historian is judged in the first instance by other historians of science, not by active scientists. A second instance judges the relevance of positions in the history of science, and here practising scientists – or rather formerly practising scientists who have climbed the administrative ladder – may have their say; but this *is* a second instance, and a less powerful influence on the research style of the single worker than the professional peers.

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Given that (say) chemists have limited influence on how historians of science approach the history of their field, we may ask what they get out of the historians’ attempt to overcome their professional oblivion. A curing anamnesis? Curing for what?

This seemingly simple question – like almost any seemingly simple question – is a crystal with many faces. I shall reflect upon a few of them.

In the daily work of the practising scientist, the answer is that history (of any kind) means little. Normal science is not trite routine; as lived by the participant of course it involves much routine, but the moments of getting new insight, new ideas, is felt to be what is important and distinguishes the undertaking. However, these insights etc. are produced within the framework

of the prevalent practice and mode of thought ("paradigm"), and here neither the projection of these on the past nor views of the past which might cast doubt on their absolute validity is of any help.

When crisis approaches, one might believe historical insight to have better possibility to intervene. Not only is the amount of innovation greater;[10] it is also less constrained by the prevalent paradigm. Kuhn claimed indeed that one of the things that characterize this situation is increased willingness to take up past ideas. New ideas may certainly be inspired from anywhere, also by distorted or less distorted versions of past theoretical structures. But they might as well come from a fairy tale which impressed one as a child, from experience in the kitchen. Newton's falling apple is a lie, but nothing would prevent it from having been true; Einstein, when writing his first paper on the Special Theory of Relativity, *might* have been inspired from the discussion of the nature of time in Aristotle's physics – thoughts are developed in parallel for quite a while – but he was not.[11]

Traditional history of science, like court history in general, had a different function: legitimization of the present and its power relations. Historiographic sensitivity to differences, the awareness that the past cannot be reduced to a less developed version of the present but was a world on its own, makes legitimization less straightforward. If we cannot claim that everything in the past points toward us, then it seems less obvious that we are on the only possible and therefore the right path.

That sensitivity to differences and acceptance of their legitimacy which characterizes "anamnestic" history of science does not convey a message that science progresses along the only possible road. This may displease scientists less than it displeases those who disguise controversial activities as scientific (and hence inescapable) progress or necessity – be it agriculture based on genetic

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[10] Even though much is still routine – if not, the new paradigm would not be created by the workers in the field, since their professional routine would not count. In a beautiful aphorism: "In normal science, 95% of everything is routine; during a scientific revolution, only 90% is routine" (Donald T. Campbell, at the "Symposium on Evolutionary Epistemology", Ghent 1984).

[11] I may add two personal confessions. My inspiration for the methods that allowed me to crack the Old Babylonian mathematical terminology came (so I discovered *a posteriori*) from guiding student projects about thematic analysis of novels, based on structuralist methods; a particular Babylonian method I suddenly understood (and this time I knew it at the moment, and it went into my naming of the trick) when applying a concept I knew from my past in high energy physics. Both innovations drew on tools coming from my professional cognitive past, as "new" ideas often do; but that is not the same as history, in particular not as history which has only been studied but not lived intensely.

manipulation, be it bombings calculated mathematically and hence claimed to possess “surgical precision” because of their reliance of infallible mathematics. It may also displease those who dismiss scientific results as “mere theory” or science as a whole as “nothing but another belief system” when wanting to teach Genesis as biology or to pump the atmosphere full of carbon dioxide. “Anamnestic” history of science does not give us a picture of the scientific endeavour as a military march along a straight line, nor however as blind groping and random walk in all directions. Kuhn may show that Planck did not understand the implications of what he was doing when he was taking the first steps toward quantum mechanics; but Planck still provided a starting point for what has since then been adopted by every physicist, though integrated in different frameworks depending on whether they accept the “Copenhagen interpretation” or not, and without which our computers and diode lights simply would not have been made.

Working scientists thus cannot or can only accidentally take advantage of history (of any kind) in their scientific practice. Many, of course, are not interested in historical questions at all. Those who are, however, do not in general react negatively to the “anamnestic” style. They may disagree with particular interpretations, and they may have good reasons – interpretations are always under discussion, so this is a different matter than disagreement with the undertaking as such. What they get at the personal level may be a richer feeling of professional identity. What the profession gets is an opportunity that its members reflect once more upon their role and upon the relation between what they are actually doing and what were the extra-scientific concerns from which the discipline started once, members more conscious of the responsibility of the discipline and the scientific enterprise in general. Anthropology and cognate disciplines may serve as an example: the appearance of Edward Said’s *Orientalism* in 1978 had a strong impact on many members. Recent work on the intertwinement between the development of nineteenth-century (French, but not only French) anthropology and the effort of the colonial powers to “divide and rule” by sharpening whatever ancient ethnic conflicts could be identified within the submitted populations at least deserves the same – and might sharpen the awareness of the profession of its use for similar purposes today. So, if history of science while overcoming its traditional role of celebrational court history does not become a “purely academic” concern itself which has lost sight of its original aspirations, then it may contribute to producing academics who are more responsible citizens, both of the *république des savants* and of the *république tout court*.

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I shall close my talk with some observations concerning the field where much of my own work over the years has been made: the history (and once, the didactics) of mathematics.

Together with medicine, mathematics is probably the academic field which has the strongest tradition for interest in its own history, and which most often pays for positions for historians within its own institutes.

In contrast to medicine, which has had no difficulty in recognizing at least the major revolutions in its history and points to these as triumphs (such as the rejection of the Galenic bodily humours and the miasma theory), mathematics has a long tradition for seeing itself as a discipline in steady progress, always keeping what it has once brought under its dominion. In the 1970s, after the impact of Kuhn's work on scientific revolutions, this was held out as what distinguished mathematics from other field; mathematics, thus we may interpret the view, never errs, and therefore never had to retract – it just learns more and more, and orders its knowledge better and better.

In the meantime, historians of mathematics have refuted this view, as it turns out without creating excessive scandal among mathematicians. None the less, just as in the case of the physical sciences there is something alluring in the old view – we may still recognize mathematics in papyri and clay tablets from the Bronze Age, and crack their procedures by means of (the simplest level of) the mathematics of our own days – which means that they are somehow valid in the same sense. I shall not try to discuss why this is so (it is probably just as impossible to state the answer exactly as in the case of the physical sciences) nor claim that it is the full truth (much of my own work on Mesopotamian mathematics has aimed to show that it is not). What is important for our present discussion is the impact of this continuity in *teaching*.

Nobody would teach medical students the Galenic theory of humours, neither in the original nor in some updated version, and nobody would do anything similar with the phlogiston theory in school chemistry. On this account, mathematics is much more akin to literary theory and historiography. One may base an introductory university course of text analysis on the principles of Aristotle's *Rhetoric* and *Poetics*, and one may even use the original texts (a colleague of mine did so a few years ago – evidently in translation and not in ancient Greek). It is also natural to confront those who undertake the study of history with Herodotos, Thukydides and Livy – three wonderfully contrasting historiographic models. School mathematics does not act so radically (nor would anybody base pre-university teaching of literature and history on the ancient

authors), but there is still a high degree of continuity. When I was 14, I was not only confronted with a slightly updated version of Euclidean geometry but also with the whole range of topics from commercial arithmetic which I have now encountered in my work on Italian "abbacus school" mathematics from the late Middle Ages (my algebra, however, made use of the letter formalism created in the seventeenth century). Until the nineteenth century, reform movements in mathematics education often returned to Euclid's *Elements*, at least books I–VI, as the best basis (while the next reform movement kicked him out again – "À bas Euclide, plus de triangles", in Dieudonné's dramatic words from 1960).

The historian's traditional task at the mathematical institute has therefore not only been to write court history, to describe "the royal road to us", but also, perhaps mainly, to give some kind of inspiration to the teaching endeavour. Some didacticians have even believed in the "genetic method", the pedagogical version of Haeckel's law (individual development resumes historical evolution), and thus held that children should develop mathematical conceptual structures in the same order as they have arisen historically; others were less radical, but many still thought history might be a good wall against which to throw the ball.

Literal belief in the genetic method presupposed unacknowledged oblivion, oblivion not only of the different conceptual structures by which past mathematics was carried but also lack of awareness that something has been forgotten. Anamnesis might really be needed, it seems. It probably is – but it result is somehow catastrophic. While showing that past conceptualizations were different from ours, it also makes us see how these drew on *experience* which is no longer ours, and in particular not those of modern school children, and were limited by lack of kinds of experience which our children have encountered already before starting school. This shows the futility of the strict version of the genetic method, it makes it clear that it is a Lichtenberg mirror.

But anamnestic history may still be useful for didactics. Firstly, it may teach didacticians something about how the construction of conceptual structures are conditioned by the experiential world of the learners. Secondly, if its results are communicated to teachers and children, it may broaden their view of what mathematics is and what it could be. Some fifteen years ago, I had written a short paper on the notion of "broad lines", an idea of lines carrying an implicit breadth (permitting a measurement of areas and lengths by the same unit just as we measure cloth in length units because the breadth is given), and showing that this idea was widespread in early practical geometry. The Italian journal editor to whom I sent it later told me that he was to make a lecture in a middle school the day after he had received it, and chose to tell about these broad lines. "The children enjoyed it. They were enthusiastic that mathematics could be

thought in a different way than the teacher said".

Mathematics is certainly a field where the joy of discovery on the part of the learner is mostly supposed never to go beyond what the teacher already knows, and anamnestic history may therefore be an eye-opener, a support for tolerance. I am not convinced *a priori* that mathematics is the only field like this.