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A MATHEMATICAL-INFORMATION PERSPECTIVE

This is the first step of one part of a research program about the meaning of computerization, and it is cast in the form of a "reader". This is to be understood as a statement of intent and the purpose is to stimulate and capture suggestions for the improvement of the research idea.

A computer can be considered to be, among other things, an electromechanical machine, an economic capital, a communication channel, a logical symbol manipulator, or a mathematical machine. In this section we shall dwell on the last mentioned perspective.

The mathematical computer

At least from the historical point of view it is obvious that the possibilities and the problems of application or use of computers are closely related to the possibilities and problems of applications of mathematics (Zellini, 1988, the foreword). It is, the least to say, intuitive that mathematics, in its wide sense, should be relevant to the understanding of the presuppositions and consequences of the administrative use of computers, considered as mathematical number machines and symbol manipulators. It is convenient to remember, in the age of the fashionable artificially intelligent computerized expert-support-decision tools, that mathematics, "the queen of science", by itself and through its controversial relation to logic, is discipline which in the context of a long history and high reputation has been the field of many important and still relevant speculations and findings about the nature of supported human thinking. Even today, in the pragmatist tradition, we are reminded that in Greek mathematics simply meant learning, and that mathematics can be seen as a way of learning to decide or to prepare for decisions through thinking: "In many ways it was unfortunate that philosophers and mathematicians like Russell and Hilbert were able to tell such a convincing story about the meaning-free formalism of mathematics.... Set and classes provide one way to subdivide a problem for decision preparation; a set derives its meaning from decision making, and not vice versa. (Churchman, Auerbach, & Sadan, 1975, p vii.)

In which way all this relates, for example, to the present discussions about the capabilities of artificial intelligence can be seen by means of the historical and conceptual bridge furnished by pragmatism (Peirce, (Hartshorne, & Weiss, 1932-1933, pp.27, 36). Problematic as it may be, such bridge is also an introduction to the rather more sophisticated branch of pragmatism known as empirical idealism (Singer, 1924, esp. pp.285-293 on "the mathematician and his luck"; Singer, 1959) and further to the school of social systems theory (Ackoff, & Emery, 1972; Churchman, 1971).

Nevertheless it is not necessary to follow the pragmatist tradition in order to appreciate the merits of our research proposal of relating mathematics to important currents of continental European culture (Zellini, 1988). Such cultural currents are not well known in our Anglo-Saxon tradition but, through the influence of Charles Sanders Peirce and others, they have many points of contact with pragmatism (Ivanov, 1984).

One important hypothesis of our proposed research program is that we can gain in depth understanding of what is happening today with the ongoing industrialization and worldwide societal diffusion of embodied computer computer science, and what should be done about it, if we get a better understanding of the historical relations mentioned above. They are, after all, still alive in today's problems.

The claims of CASE

Searching carefully it seems possible to find at least thirty years of "practical" mathematical background and presuppositions for research on computers and information. Starting from the mathematical basis of computer programming in the 50's and 60's (Information processing, 1960), during the 80's some arguments were advanced in favor of the strengthening of the formal and logical basis of research and applications in the field of computer and information science. This concerned at least the Swedish scene (Bubenko, 1980; Bubenko, 1982a; Bubenko, 1982b; Bubenko, 1983). Such late emphasis on increased formalization of information systems analysis, on mathematical-logical models for formal analysis of correctness and completeness has been advocated as a programmatic campaign against inefficiency and low quality of scientific work. Increased mathematization and strict formalization with strict definitions and "communicable knowledge" are then seen as a tool for avoiding pseudoscientific speculations, in analogy with the popular positivistic view of the advancement of physical science. Displaying a curiously defensive attitude it is also claimed, at the same time, that mathematization does not imply a natural-science, technical, and in-human bias, since mathematics is neutral, and is only a common tool to all disciplines, and a precondition for applied science, for accumulation, integration and communication of usable knowledge. Increased mathematization would prevent, it is claimed, the diffusion in the computer market of "miracle tools" that promise design and implementation of data processing systems in a fraction of the time required by traditional methods. Therefore, the highest priority for future serious research work should be given to the establishment of a "standard notation" for modeling, seen as a prerequisite for cumulative research and success on other research issues. For less experienced users a friendly but still stringent high level specification and interaction language should be devised.

Such strong claims are, paradoxically, not based on any stringent or mathematical argumentation, and, as a matter of fact, there are several researchers who definitely do not agree with the above claims (Ehn, 1988, p. 148; Sørgaard, 1988, part 6, pp. 44-45). "Mathematicism", then, seems to be a very weak philosophy with very strong implications. It is, therefore, very natural in the frame of our proposed research to inquire into the nature of such new miracle tools in the form of standard notation for modeling which are supposed to substitute the older miracle tools.

These new miracle tools have been lately represented by the confidence in two notable trends in research and development of methods: the increased used of formal techniques also in the very early system development stages, and the increased use of deductive and rule-based techniques (Bubenko, 1988). These trends are supposed to support "languages for capturing and describing knowledge of the application domain (its structure and behavior), and of the information requirements in early development stages": deductive and rule-based approaches work on the basic idea "to capture and to explicitly express business rules and constraints in a declarative style, rather than to implicitly embed them in processing procedures or transaction descriptions". The additional use of a temporal dimension will allow "to reason about the state of the system at any point of time", possibly dealing not only with changes of the contents of a database, but also with changes of the schema describing the contents of the database and the constraints, as well as the changes reflecting changes in the applications. Quite different conceptions of formal and informal handling of the problem of change have been presented in the literature (Forsgren, 1988; Forsgren, Ivanov, & Nordström, 1988; Ivanov, 1972; Ivanov, 1987) and, in particular in the software tradition (Parnas, 1972; Parnas, 1976; Parnas, & Clements, 1986; Parnas, Clements, & Weiss, 1984; Sørgaard, 1988, part 6).

This discussion should obviously be related to that important so called functional feature of computer aided software engineering, CASE, the feature of design support. It is envisaged as including support for transformation of specifications from one "level" to the another, "view integration" which is required "to combine the local specification efforts of a number of work teams working in parallel", and where "the restructuring implies the semantics preserving rearrangement of a specification in order to improve it according to a set of quality (rules), performance (rules), or other kinds of rules, or according to a designer's restructuring directives (Bubenko, 1988, p.6). CASE environments are judged to be advanced if they allow to develop a CASE tool for an arbitrary method which is the more advanced the more it can handle advanced computer modeling concepts such as "constraints, derivation rules, operations, and preconditions..., rules for checking the consistency, completeness, and quality of the designed objects and their relationships": The CASE tool building requires the method's constructs to be strictly and formally defined (ibid., pp, 10-11, 17). Lately this position has been consolidated in an outline of a program for research on information systems (Berztiss, 1989).

In a paradoxical contrast to the above claims of what is required in the future by means of today's research, stands the acknowledgement of the fact that experience of the use of this type of tools in projects of realistic size is still quite limited, and that many of the commercial tools still seem to be "toys" which are not suited for use in projects of realistic size and complexity (ibid., pp. 10, 12). Obviously this supports the research view which opposes this vague program of mathematization based on an unstated view of one kind mathematics in terms of formal systems. Experience and reason (Churchman, 1971, chap.2; Parnas, et al., 1986) indicate why it is legitimate to embark on alternative research without the expectation that, for instance, we will be able to achieve a formal software development process in which the programs are derived from specifications.

An unstated mathematical background

From what has been said above about the formalist-mathematical position, it seems that it is not so much mathematical as it is a oversimplified version of a formal-logical position in the spirit of traditional mathematical logic. In what follows, however, we will anyway pursue the mathematical interpretation of the arguments, while the logical or mathematical-logical interpretation is left to another section of the research proposal. By doing so we acknowledge that the issue of the relation between mathematics and logic is not settled, that they cannot be vaguely considered to be identical and therefore cannot be subsumed under the one same label of one discipline, and that mathematical logic can, at best, be considered as only one particular type of logic (Church, 1962, offers a contribution to this issue).

The "mathematizing" trend in the development of computer and information systems which was described above claimed the purpose and capability of supporting languages for "capturing" and describing knowledge of the application domain, its structure and behavior, and for capturing information requirements in early development stages. Deductive and rule based approaches, or conceptual modeling, were supposed to capture and to express in an explicit way, in a declarative style, business rules and constraints. The additional use of a temporal dimension would allow to reason about the states and the changes of the system.

Dwelling on the notion of states and changes of a system it has been noted (Rosen, 1985b) that the Newtonian idea of "state determined system" contains some problematic basic assumptions about causality. Causation cannot be reduced to simple mathematical relations between propositions which describe events, with a segregation of different classes of causation into independent mathematical structures. If the assumption of independence is relaxed, as it should be e.g. in the case of biological modeling, mathematical images become like webs of informational interactions which contain the set of "state-determined" systems as a subset, and where the behavior of one of these webs can be approximated, albeit locally and temporarily. But this is a new notion of approximability which is only local and temporary, and this "explains a great deal about why we have been able to go as far as we have with the non-generic Newtonian picture, and why we have never been able to go further with it". What is required, then, is to develop the mathematical science of simple systems into a science of complex systems: "Namely, by loosening the Newtonian shackles, we can introduce a category of final causation" (Ibid, 1985#,p.175). The matter can be consolidated through the study of other interesting literature. (Ackoff, et al., 1972, pp. 19-31, 248ff; Churchman, 1971, chap. 3 and 10; Geach, 1981, pp. 128-138 on "intentionality"; Grenander, 1983; Rosen, 1985a; Rota, 1973).

We have here also an interesting connection to the history and theory of statistics in terms of what we write elsewhere about the role of the unique-single case in psychological research, versus Buckle and Quételet's Laplacean conception of universal determinism for cultural phenomena, making mass-phenomena the sole object of the science of society (Hayek, 1941, pp. 318-319; Lottin, 1912, esp. pp. 313-317, 397ff, 440ff, 501ff, and the reference to social mathematics on pp. 374ff.)

Our proposed research will proceed through a detailed understanding of the assumptions of the Galileian-Newtonian mathematics which is considered as origin and model for many common formal mathematical systems, including the characterization of the realm the biological as an indicator of analog problems in the realm of the social (Dessauer, 1954; Henshaw, 1986, concerning correspondence about Rosen; Koyré, 1954; Portmann, 1954; Rosen, 1985a; Rosen, 1985b; Rosen, 1986; Strong, 1957; Wedde, 1984). A hypothesis of the research proposal would be that if any formal models of biological-social phenomena are to be processed in terms of knowledge bases and rule systems, the specific nature of mathematical modeling in relation to "states" and "time" must be interpreted in the light of the above kind of criticism. The formal models will then probably have to be expanded in unknown dimensions, e.g. in terms of finalistic or teleological "interactivity" (Ackoff, et al., 1972, pp.65ff. 160ff) as in some suggestions which have been proposed in the context of quality-control of information, man-computer interaction, interactive systems planning, and computer supported constructive qualitative conversations (Forsgren, 1988; Ivanov, 1972; Nilsson, 1987; Nilsson, 1988; Whitaker, & Östberg, 1988).

One main point in the proposed.research, in fact, will be to relate such detailed understanding of the assumptions at the roots of mathematics to the kinds of mathematics and mathematical logic which stand closer to computer science. Unhappily, many works on mathematics and mathematical logic for computer scientists (Harel, 1987; Levin, 1974) stand really at a summarizing discoursive textbook-level, and it is difficult to find discussions in depth of the mathematics of computer and information science. Even the few classics (Turing, 1963, esp. pp. 24-25; von Neumann, 1956; von Neumann, & Goldstine, 1947) seem to be quite ahistorical and they do not really touch upon the theoretical issues of mathematics proper. This is by no means uncommon, as it may be noted in many types of professional contributions in the formal sciences (Church, 1941; Hoare, 1969; Iverson, 1981). It is also interesting to note that attempts to develop "an outline of a mathematical theory of computation" for a supposedly mature field of science were made as late as in 1970 (Scott, 1970). Contributions that cast some light on the basic scientific theoretical assumptions of pioneers of computer science are rare, and they seem to become still more rare in relation to the increasing number of writings on mathematical formalisms in computer science (Minsky, 1967; Winograd, 1979, see their bibliographies).

This last mentioned tendency uncovers the superficiality of much thinking and training in the computer field which lay at the basis of the Christopher Strachey's disarming definition of computing science so late as in the year 1977 (Fontana dictionary of modern thought)

> *Computing science.* The study of the use and sometimes the construction of digital computers.... It is a fashionable, interesting, difficult, and perhaps useful activity. Unfortunately, in spite of appearing to be a mathematical or physical science, it has so far a pitiably small body of generally accepted fundamental laws or principles which are likely to remain valid even for the next 20 years, and consists instead almost entirely of ephemeral "state of the art" information. A more appropriate title at this stage of its development would probably be "computer technology".

As an example, in a Swedish university in the beginnings of the 80's, after the advent of object oriented simulation languages but before the advent of functional and logic programming languages, programming was thaught on the basis of the concept of *algorithm* which was introduced as a word originated by the Arab mathematician Abu Jafar Mohammed ibn Musa al-Khowarizmi (around year 825), and having the following definition: "a method for performing a task, where the method is expressed in terms of a finite sequence of rules, operations". The introduction went on remarking that this obviously is a very general definition including e.g. cooking recipes, but: "we naturally are interested in algorithms in the form of computer programs for processing of digital data, with the closer definition of 'a sequence of operations which in a finite number of steps leads to the solution of a data processing task'". It was, then, remarked that the precise formulation of algorithms requires the development of artificial languages or algorithmic languages which, through compilers, can bridge the gap between the original language in which the problem had been formulated and the computer's internal language. Advices about how to construct algorithms or how to solve problems were a referral to "thumb rules and experience" as represented by "heuristics".

So much for the depth of the mathematical understanding of the nature of programming. With such a kind of approaches, supplemented by more ambitious references to obscure "abstract machines" and the like (Doyle, 1982), a whole generation of computer scientists and university professors has been brought up with a very particular and limited view of the meaning of formalism (Borillo, 1984; Mathiassen, & Munk-Madsen, 1986; Naur, 1982). This generation usually is very heuristically based on experience but it does not relate the understanding of the old algorithmics and of the new programming languages to any deeper understanding of mathematics, formalisms, functions. Attempts to reach a deeper understanding, however, are exemplified by many outside the area of computer science (Dessauer, 1954; Rosen, 1985a; Rosen, 1985b;

Stenlund, 1987; Stenlund, 1988; Strong, 1957; Verene, 1982). Neither there is a relation of mathematics to "reality" or operationalization, in spite of the frequent references to operations (Margenau, 1962, cf. the original Bridgman's operationalism as described by Stevens in 1935).

The dangerous infinite and playfulness

More fundamentally, the thing which is ignored is the historical ongoing debate about the treatment of the *infinite* in mathematics (Zellini, 1985a; Zellini, 1985b), an issue which is deeply related to the possibilities and limitation of discrete mathematics, and which is often hidden behind eclectic, popular, playful, pedagogically attractive but superficial allusions to the "mistique" of the problem (Brown, 1969; Hofstadter, 1979; Mandelbrot, 1982; Maor, 1987; Pearce, 1978; Pennick, 1980; Purce, 1974; Rucker, 1987; Zichichi, 1988). Such state of things leads in turn to a very unfortunate lack of respect for the discipline of mathematics which has sometimes prompted reactions that, however, keep paradoxically the same superficial level of debate (Hansson, 1983).

The nature of the mathematical playfulness that contributes to this superficiality can itself be made an important object of our research. This idea is an original contribution of our research which was advanced (Ivanov, 1983; Ivanov, 1989) almost simultaneously with the results of certain "anthropological" research on the interaction of children and adults with computers (Turkle, 1984). Research about playfulness, in a rather different key, is proposed in another section of our reseach program as it regards the use to which both mathematics and computers are put. The quest, in our case, will be directed towards the "serious" treatment of aspects of mathematics as related to computers and computer models, aspects which, when ignored, encourage unmotivated claims about the applicability of mathematics and computers outside the limited fields of Newtonian natural science (Marchetti, 1983; Peterson, 1975). In the context of social science and social systems research it seems that such serious treatments of mathematics are, if possible, even more rare than in the context of natural science (Bosserman, 1981, in contemporary production; Cobb, & Thrall, 1981; Lottin, 1912, historically). In fields which stand closer to formal science and computer science modern problematic developments of mathematics are sometimes observed, but in mild uncommitted and optimistic terms (Cohen, 1983). It happens more seldom that fundamental issues are raised about the relation between computer programming and mathematics (Chaitin, 1974, Chaitin, 1987 #1065, is a most interesting contribution in this respect) which could be contrasted with early conceptions (Gorn, 1963; Korfhage, 1964). It is still more rare to find mathematicians who set aside irresponsible playfulness in order to formulate strong explicit criticism of the misuses and misunderstandings of mathematics in science in general, and in computer science and scientific computing in particular (Ingelstam, 1970; Schwartz, 1962; Truesdell, 1984).

There are today paradoxically serious (postmodernistic?) attempts to defend the legitimacy and fruitfulness of playfulness (Ehn, 1988; Papert, 1980). In another section of our research proposal we suggest the conditions for a genuine research about playfulness by extending certain recent attempts (Carse, 1986) and relating them to mythical ritual behavior, which would eventually include the character of the child archetype and "puer aeternus"(Hillman, 1971; Hillman, 1979; Ivanov, 1986, pp.135-136; Jung, 1953-1979, CW 9:1, §§ 259ff). In any case there are plenty of historically important research directions which are not easily related to playfulness. One interesting direction which is today ignored in spite of being implicit in the discussions on the use of computers is the school of "economy of thought" (Jourdain, 1914; Rignano, 1913a; Rignano, 1913b; Rignano, 1913c; Rignano, 1915a; Rignano, 1915b;

Rignano, 1915c); Peano, 1915; Kennedy, 1980]. The idea of economy of thought, besides of its possible connections to philosophical utilitarism, could be an entrance door to rich historical material for supporting our research about the contact points between calculation, computers, mathematics and logic, and about the chaotic development that in the last decades is leading to a "tower of mathematical Babel (Davis, 1987).

Historical aspects of research methodology

It is apparent that the quest for the meaning of mathematization or of mathematical formalism and symbolism in the quest for knowledge raises many difficult issues. In part they have already been addressed in past times where today's computers were simply represented by the question of computation or calculation. A long run research program which must choose among alternative degrees of mathematization in its work must therefore incorporate an inquiry into the debates and arguments about the issue. Nevertheless, we will not exaggerate in being too "philosophical", and we may disregard, for the time being, some early speculations about mathematics in general, and Platonism in particular, the concept of "form" which stands at the root of the possible meanings of formalism, "the Greek mind", etc. (Boodin, 1957; Kitto, 1957, pp.192ff.; Koyré, 1954).

Some philosophical precedents of the debates may, however, be particularly interesting as they touch upon the strivings towards greater mathematization of inquiry. One example will suffice about one of the early philosophers of the scientific revolution leading to our age (Hobbes, 1962), who criticizes the tendency to formalize with symbols and, on one occasion, accuses one of his contemporaries for mistaking the study of symbols for the study of geometry (ibid., p. 187). He observes that "the symbols serve only to make men go faster about, as greater wind to a windmill", and that "no logic in the world is good enough to draw evidence out of false and unactive principles" (ibid., p. 188). On another occasion, in the essay "Lessons on the principles of geometry..." addressed to "the egregious professors of the mathematics, one of geometry, the other of astronomy", the use of symbols is also opposed by observing that (ibid., pp. 247f, 329):

> But are not you very simple men, to say that all mathematicians speak so, when it not speaking? When did you see any man but yourselves publish his demonstrations by signs not generally received, except if it were not with the intention to demonstrate, but to teach the use of signs?.... Symbols are poor unhandsome, though necessary, scaffolds of demonstration; and ought no more to appear in public, than the most deformed necessary business you do in your chambers....
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> ...Symbols, though they shorten the writing, yet they do not make the reader understand it sooner than if it were written in words. For the conception of the lines and figures (without which a man learneth nothing] must proceed from words either spoken or thought upon. So that there is a double labour of mind, one to reduce your symbols to words, which are also symbols, another to attend to the ideas which they signify.

This would in our times be echoed by others (Keynes, 1952, p. 19n). An evaluation of such words and disputes certainly requires that they be set into the broader context of empiricism versus rationalism, and other contexts such as "the past struggles between symbolists and rhetoricians in elementary geometry", touching also upon the function of signs, of intuition, psychological studies of symbolisms, etc. (Cajori, 1929, vol. 1, pp. 426-431 and vol. 2, pp. 284ff, 314, 327), and of "devices that appealed to the eye and thereby contributed to the economy of mental effort" (ibid., vol.1, p. 265), i.e.

something which in our age of computer graphics could be called the integration between geometry, aesthetics, and economy.

It will, then, probably be noticed that the creative work on programming languages, computer programming, or software engineering, relies heavily on the creative use of "the history of mathematical notations" (Cajori, 1929) which, in turn, is interleaved with the history and philosophy of mathematics supplemented with geometry, logic, statistics in the early history of computers in the form of calculating machines, planimeters, integraphs, and other mechanical aids to calculation (Cajori, 1980, pp. 485f; Smith, 1951-1953, vol. 2, pp. 156-207).

It could be suggested that these historical aspects are something which can and should be studied apart from the substance of the discipline, in our case mathematics and computer science. This runs, however, counter to one influential interpretation of the nature of history, which leads us to be sure that "no subject loses more than mathematics by any attempt to dissociate it from its history" (Cajori, 1980, quoting a statement by J.W.L. Glaisher on the title page). Our hypothesis is that it is treacherous to attempt to avoid the supposed "genetic phallacy" (Toulmin, 1977, shows the phallacy of this phallacy) and to strive for a higher degree of mathematization or formalism in computer science without combining the study of mathematics with the study of the history of mathematics. It is such a combination that would allow an adequate understanding of the meaning, possibilities and dangers of mathematization. It would also contribute to the avoidance of the risks associated with the delivery of a "powerful tool" in the hands of immature scientists, in analogy to the delivery of machine guns or bulldozers in the hands of children.

Unfortunately it is nowadays possible to study the history of anything, and in particular of mathematics, without coming in contact with the important problematic aspects of such history when it is conceptualized as "accumulation" of knowledge, where debates are glossed over. According to such a view of history, in the spirit of a kind of "social Darwinism", it appears as obvious that the "right history" in the one which contains only those developmental steps of the discipline which lead to its present interpretation: the rest is only a superfluous story of past mistakes which show how smart our generation is in comparison with prior generations of scientists. In such a perspective it becomes important to know how to choose of build up an adequate historical account of mathematics. Certain approaches are mainly summarizing and, therefore, only superficially informative for our research purposes. At any rate, they do not enhance, and still less interprete or take position about the controversial aspects of the historical development, even if they sometimes offer extensive bibliographies and exciting overviews of curiosities, and are presented in an attractive elegant style of writing (Bell, 1945; Kneale, & Kneale, 1965, concerning logic; Struik, 1959). We believe that the study of administrative, organizational or social use of computers requires a wider and deeper approach and the inclusion of a more detailed and comprehensive history of mathematics. It should also include "elementary" aspects in the sense of including commercial, actuarial mathematics oriented towards accounting, bookkeeping, auditing, and classical 18th century statistics (Smith, 1951-1953, vol. 2, pp. 552ff). What seems to be elementary may give insights into the nature of modern problems. It is suggested by the fact that "relations between algebra and geometry (Smith, 1951-1953, vol.2, pp. 320ff) remind us of the seldom recognized scientific basis of today's graphic computer processing (Körner, 1960, p. 105, on "diagrammatics").

It is obvious that the historical quest will unavoidably lead our attention to certain questions of philosophy of mathematics. In the Anglo-Saxon tradition such a philosophy has a relatively low historical content in spite of touching. upon the important relation between mathematics and reality, tools versus machines, conceptual thinking, etc. (Körner, 1960, pp. 29, 101, 176ff; Körner, 1979, pp. 38-91). In this tradition, history of science gives way to philosophy of science or theory of science. The loss os historical detail and historical spirit is perhaps compensated by a clearer focus on certain issues, e.g. the question of logicism and formalism versus intuitionism (Körner, 1960), which are critical for our quest about the function of mathematics in computer and information science. It will then be noticed that the research problem tends to "inflate", motivating a temporary strategic retreat into simpler overviewing literature (Mathematics [-as a calculatory science -foundations of -history of],), 1974 #440]. Such a strategic retreat should then be completed by diving, also at an "encyclopedic" overview level, into particular issues which have become relevant because of the late pressures towards mathematization or rather formalization and mechanization of systems development. Among such issues we may count the relativity of standards of mathematical rigor (Wilder, 1973) and, more broadly, mathematics in cultural history (Bochner, 1973) which illuminate fundamental questions about the meaning of form and symbol processing. In Sweden this type of studies has had a very weak tradition, but there some attempts have been made (Olsson, 1988a; Olsson, 1988b; Stenlund, 1987; Stenlund, 1988; Wallin, 1980).

Research method in detailing the study

The proposed object of research stands already at the frontiers of what "method" should mean in its relation to mathematical and empirical reality. The idea itself of having a method constitutes a particular chapter in the history and study of science (McRae, 1957). It is therefore rather paradoxical to ask which should the method be for furthering the studies which have been suggested here. In such a situation it will be legitimate to continue our proposed research by shifting gradually from the field of so to say impersonal objective philosophy and history to particular essays and testimonies of people who have reflected on the nature and development of mathematics and of the study of form. In its extreme aspects it could be called a shift from system towards biography. It may concern works which range from philosophical-historical reflections (Davis, & Hersh, 1981, pp. 34ff on "the ideal mathematician"; Kline, 1985; Melzi, 1983; von Wright, 1983; Weil, 1970-1974, italian ed. 1978, pp. 76ff, 113ff, 200ff.; Weyl, 1949; Weyl, 1985; Whitehead, 1911; Wittgenstein, 1978) to more popular and computer-focused versions of such reflections (Pagels, 1988), and to biographical notes (Hodges, 1983; Johnson, 1977; Quillet, 1964, pp. 61ff, 207ff; Reid, 1970) including the extremes of noting chronical "don juanism" and even homosexuality in particular mathematical minds (Hodges, 1983; Wilson, 1988, pp.215-229). Such extremes may, however, be relevant to the study of relation of mathematization to the cognitive and emotional functions of the mind. We have also fiction literature and essays in the original broad sense of the word leading the thoughts to an European continental tradition of where science sometimes is still integrated with philosophy, politics, literature, art and religion (Carse, 1986; Nyman, 1956, pp. 239 on Alfred Korzybski's "general semantics" movement of the Non Aristotelian Society; Zellini, 1985a; Zellini, 1985b; Zellini, 1988). To the latter area belong also such approaches as the "logic of poetry" (Larsson, 1966) with its probable connections of logic to rhetorics and dialectics (Reichmann, 1968; Weil, 1970-1974) elaborated in fiction (Pirsig, 1974), and recently revived in the philosophical literature (Barilli, 1983; Fisher, 1987). To the same last area belong also those contributions which stand at the frontiers of cultural criticism proper (Guénon, 1982, chapters 13-14, concerning the postulates of rationalism, and mechanicism-materialism"), and of critical semi-fiction (Musil, 1952; Rényi, 1973) including finally some literary works of Poe (Poe, 1969,p.302).

Such contributions are often the work of people having varied degrees and qualities of knowledge about present academic formal science but displaying a concern for the deeper meaning of mathematics in the context of human thought. As such they are therefore relevant for our own quest about the importance of the mathematical aspect when studying presuppositions and consequences of the massive use of computer technology, the dependency of the answers upon the particular field of application, what should be understood as being a "field", etc.

The creative-intuitive dimensions

The direction of our research, following the above reflections, points out that mathematics can be seen as a culturally contingent way of thinking having particular psychic dimensions. In the mainstream of today's research this is explicitly recognized in the reference to such concepts as "human information processing" (Attneave, 1959, is an early exponent), "artificial intelligence" and "cognitive science", especially when dealing with expert or (decision) support systems. Up to now, however, there seems to be a tendency to avoid historical fundamental issues (Ivanov, 1988). In the history of the development of mathematics many of the psychological or psychic dimensions, whenever they were noticed, were associated to a vaguely understood "creativity" or "intuition" (Lerda, 1988, is a late expression of such an understanding). These dimensions have been generally considered to be intellectually intractable, and therefore they were judged as just interesting or, at best, potentially useful only from the educational point of view. As such they could be important as a source of expedients for making the study more enjoyable or easier for people who lack the motivation or the capability to follow the details of the analytical arguments. In this sense the intuitive dimension has been considered in certain works (Hilbert, & Cohn-Vossen, 1952, cf. pp. iii-iv).

It is natural to see the same attitude and argument today lurking behind the conceptions of the scientific status of graphic data processing and the processing of computerized visual images on the high resolution screen of work stations. An intangible and evanescent so called tacit knowledge or personal knowledge stands there for the relationship between computer mathematics and screen geometry. It is seldom recognized that this relationship between mathematics and geometry stood at the center of the debates around the romantic "Goethian" conception of science (Bortoft, 1986; Steiner, 1926/1988; Steiner, 1937/1982) and around Newton's versus Goethe's theories of color (Goethe, 1970).

The romantic approach, however, or at least the literature about it, does not cover in sufficient detail one central question for our research, i.e. the *reasons* and the particular form for the ongoing dissociation between sensations and emotions in mathematical thinking, whether it is only a question of these two categories of sensations and emotions or whether there are other ones, where these categorization comes from, etc. What seems to be required in the context of computer science is a more rigorous approach which shows clearly what impact these categorical presuppositions have on the development of mathematical theories, in a way that is similar to the difficulties which characterized the birth of the intuitionistic school of mathematics (van Stigt, 1979). Some help in this respect could be also obtained from studies of the historical and psychological nature of numbers, formalism versus axiomatics, presuppositions of the theory of sets, etc., paving the road from philosophy of science to analytical depth psychology (Bachelard, 1975a, chap.2, esp. pp. 19ff and 41; Bachelard, 1940, about "O mathématiques sévères"; Bachelard, 1975b, chap.1, pp. 62ff; Bortoft, 1986, pp. 79f about multiplicity vs. unity; Meschkowski, 1975; Spengler, 1981-1983/1918, esp. vol.1, chap.2, pp. 53ff on the meaning of numbers, and pp. 8ff, 426ff on nature-knowledge; von Franz, 1974). Some approaches lead in particular to anthropology and religion, treating the role of experience and intuition, the ritual origins of geometry and counting etc. (Freudenthal, 1962; Pennick, 1980; Seidenberg, 1962a; Seidenberg, 1962b). They are interesting in the context of our research since it is obvious that many "irrational" attitudes to, and uses of, computer support can be interpreted as gambling and ritual behavior (Ivanov, 1983; Ivanov, 1989; Turkle, 1984).

Among works which more explicitly refer to psychology and to the problems of mathematical thinking, intuition, and the "unconscious" there are some more technically-biologically oriented (Klir, & Lowen, 1989), as well as well known pioneering classics (Hadamard, 1954). There are also works within the more general current of the psychology of personality and of learning (Rychlak, 1977) which stand epistemologically very close to the systems theory adopted in our research program (Churchman, 1971).

Towards cultural criticism

It can be seen that all the above gradually transforms, or rather widens the original mathematical issue of our research turning it into an issue that today would be considered as more legitimate under the label of cultural criticism. In the academic community today such label is often considered as an expression of contempt. Labeling something as being cultural criticism, journalistic essaism, or even philosophical, are ways of explaining away problems by claiming that even if they happen to be important, nevertheless they do not belong to the realm of science and should not be institutionally supported by the universities. This appears also to have been the paradoxical fate of all metaphysics after Kant. It is one aim of our research to contribute to the understanding why the philosophical implications of problems and crises in the development of mathematics have, today less than ever, practical and theoretical impact. So late as in 1972 a well known and respected scientist who had worked at the frontiers of mathematics claimed the great undecidability theorem of Gödel had not yet been absorbed by philosophy, though its ultimate impact would undoubtedly prove to be shattering" (Morgenstern, 1972).

In contrast, it can be said today that the mentioned crisis in the foundations of mathematics has not left any trace, for instance in the ongoing research in computer and information science. Maybe this is an echo of H. Weyl's suggestion that there is a "Darwinist" line of argument lurking behind the basic conjectures of D. Hilbert's mathematical program: Hilbert's trust in the human *psychological* propensities which he took to be embodied in the procedures of modern classical mathematics, and which he directly argued for by appealing to their "practical success", appears ultimately to be based on faith, in "...the reasonabless of history, which brought these structures forth in a living process of intellectual development" (Weyl, 1927/1967, quoted by Deftelsen, 1986, p.37n) . Perhaps it was this very same requirement of faith that led Hilbert's contemporary, the mathematician Paul Gordan, to comment on Hilbert's solution of a problem in the theory of invariants by announcing in a loud voice "This is not mathematics. This is theology" (even if it was later tempered by the additional graceful concession that "I have convinced myself that theology also has its merits" (Reid, 1970, pp. 34, 37).

It can be the case that the matter has no "practical importance" today when the computer enhances constructive procedures. Such procedures may, however, rely implicitly on a trial and error experimentation which has given up the controllability of the long run consequences of the logical network build-up (Churchman, 1971, chap.2). It has been noticed that numerical mathematics has recently undergone an enormous "development" in terms of study of efficient algorithms in the discrete in order to solve "approximately" problems defined in the continuum. This is based on the great foundational premise of the end of the nineteenth century, i.e. the conviction that analysis could be arithmetized *as matter of principle* (Zellini, 1985b, p.255). In this way we come to witness how practical efforts of computer-based mathematics, that is now mushrooming into efficient algorithms for parallel machines, is based on foundational-philosophical premises and on a concept of approximation which is violently challenged in some professional districts of the discipline (Rosen, 1985b; Truesdell, 1984) even if sometimes this is done in a much more limited "technical" sense (Grenander, 1983). In the context of our research we might need to explore the hypothesis that the perceived triumphs of computerized discrete mathematics are essentially late superficial byproducts or spin-off effects of the real earlier triumphs of Newtonian physics. Such mathematics and related object-oriented thinking today, however, should raise many difficult and painful questions in what concerns their applicability to the biological and social fields of research (Chargaff, 1971; Oppenheimer, 1956) including the related moral aspects in Kass, 1972; Schwartz, 1962].

In other words, the apparent utilitarian triumphs of recent applied mathematical science enhanced by computer technology may be mainly a meretricious way for profiting of past achievements. It would be a using up the philosophical-scientific heritage after having freed oneself from the peers' scrutiny and social control thanks to the extreme degree of specialization (Cohen, 1983) and thanks to the independent financing obtained from big industry and big military state. Consequently, the "improvisational heuristics" of degenerating research programmes, including even the "ability to suspend judgement in the face of disconfirming evidence", will allow "proposals to be made without regret even when they have highly implausible aspects, or when tests are not likely to be possible in the foreseeable future" (Holton, 1984).

If the above turn out to be true, then, we should question the commonplace statements about the advantages of a mathematization program for information science, and perhaps of computerization in general. The essential, enduring, central mathematical issues of the infinite, sets, multiplicity versus unity, order-complexity-chaos and randomness, are certainly too important to be gambled away in the recent playful type of mathematics (Hoffman, 1988; Hofstadter, 1979; Pagels, 1988, all in varying degrees). These issues should be rescued for the purposes of our research program, in a serious sense, synthesizing what has been called different categories or types of mathematics (Browder, 1976), a seriousness obtained through a further development beyond the attempt of conceptualization by means of games (Carse, 1986; Eigen, & Winkler, 1985).

In any case, some important efforts have been made in order to understand instead of explaining away, e.g. regarding why so called crises, as in the foundations of mathematics, do not necessarily lead to deeper reflection (Zellini, 1988). A possibly fruitful analogy, of course, is that crises in the effects of the use of computers will not necessarily lead to reflection and better understanding of the underlying problems. It may, therefore, be necessary in the course of our research to share knowledge of cultural criticism of mathematics and of "quantitative knowledge" (Bochner, 1973; Guénon, 1982; Ingelstam, 1970; Schwartz, 1962; Spengler, 1981-1983/1918; Tuchel, 1982, less specifically mathematical). Of particular interest will be that kind of criticism by authors who are practicing or scholarly mathematicians and attempt to focus their

attention on the details of the discipline itself (Rosen, 1985a; Rosen, 1985b; Rota, 1973; Zellini, 1985a; Zellini, 1985b; Zellini, 1988).
The cultural criticism of mathematics may be coming. Two recent, apparently well diffused and easily available works that have not yet been integrated in our own work (Barrett, 1987; Davis, & Hersh, 1986) indicate that the interest for these matters may be on the rise. This obviously does not relieve us from responsibility, but rather encourages us to contribute to this strife from the inside of computer and information science.

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