Towards a theory of effective discrete systems

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■ ***Abstract***. *Many aspects of information processing, effective computations and programming are usually directly related to sets and functions. However, in spite of many formal similarities, some usual evidences involved in such an articulation hide underlying open problems, and conflict with some standard and fundamental mathematical definitions and conceptions. In this paper, I propose to understand these difficulties as the normal counterpart induced by the lack of a mathematical theory of effective discrete systems ; I present some remarks and ideas concerning the foundations of such a theory, and I briefly relate them to some difficulties implied by a too direct articulation between effective computations and mathematical functions.*

Introduction

From an historical point of view, the theories of effective computability have been elaborated before the concept of information processing has ermerged. These theories are mathematical theories, in the sense that they are supposed to only concern mathematical objects and the mathematician itself : they lead to mathematical theorems related to mathematical problems. At present, these theories are immerged in a rather different context : many real systems (related to various scientifical fields, not only computer science) are commonly considered as being information processing systems. Thus, from a theoretical point of view, if we want to consider the link between such real systems and the [mathematical] theories of effective computability (or more generally between these systems and any mathematical concept or theory), we cannot avoid to suppose that there is somewhere at least one cut separating what is properly mathematical and what is not. In fact, when we speak about the application of a mathematical theory to a field of real objects or systems, we speak about such cuts which, at the same time, separate and link. For example, physical phenomenas are not numbers, even if the correlated measurements lead to numbers ; similarly, no physical concept can be properly identified with a mathematical concept, even if the latter is involved in the mathematization (or the formalization) the former. These remarks are perhaps evident when they concern the articulation between mathematics and the well established experimental sciences, such as physics. However, one may remember that it took a long time to make precise the methodological principles leading to this fundamental cut ; this certainly means that such a cut has not been taken as evident during hundreds of years.

The field of information processing systems is in a rather singular situation with respect to this fundamental cut. From an experimental (or technological) point of view, an information processing system is already a kind of abstraction of a real (physical) system ; but from a mathematical point of view, the same information processing system is already rather a kind of particular implementation. This situation suggests the hypothesis that the traditionnal cut between mathematics and experimental sciences is not [directly] applicable in order to elaborate a fundamental theory in which the articulation between the experimental sciences, the field of information processing systems, and the mathematical theories would be sufficiently satisfying.

The aim of this paper is mainly to underline the lack of a [mathematical] theory for the effective discrete systems, exactly as, during the XVIIth century, the fundamental theory of NEWTON would not have been correctly founded if [the basis of] a [mathematical] theory of effective continuous systems had not been elaborated, mainly by LEIBNIZ and NEWTON itself. Correlatively, I briefly examine some usual evidences (concerning the articulation between effective computations and mathematical functions) which may be invoked to try to avoid this lack, then introducing some « difficulties ».

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Methodological remarks

Everybody knows what happened to the pythagorician belief concerning the harmony between the nature and the [rationnal] numbers : the hypothesis that all the nature is governed by [rational] numbers has been abandonned, although it is impossible to concretely falsify such an hypothesis, simply because it is concretely impossible to « observe » or « mesure » anything which *is* (or is necessarily related to) an irrational number. Everybody knows also what happened to the belief of an evident connexion between our usual conception of the space and the [euclidian] geometry : in spite of its concrete operativy, never rejected by any concrete argument, in spite (and, in fact, thanks to) the Euclide's effort to state a minimum number of axioms to found this geometry, it took about two millenaries to understand that this evidence was able to cover the invisible cutting between one extra axiom and the others. These two well-known events have been made possible by two major inventions : the invention of logical demonstrations (if an hypothesis leads to a contradiction, then it must be rejected), and the invention of the disjunction between a mathematical theory (axiomatized and formalized) and a [mathematical] model of such a theory. Many other examples could be found in the history of mathematics where an evidence has two faces : on one side, it allows a theory to be elaborated in spite of an unsolved underlying fundamental problem ; on the other side, it covers this problem, that is to say that it conserves the problem, empeaches the problem to be detected or correctly stated, thus empeaching *a fortiori* to solve it correctly. I recall these two events to recall that nobody can ignore such an antagonist effect of some particularly evident evidences :

> FIRST METHODOLOGICAL PRINCIPLE. No evidence concerning the articulation of a mathematical theory (or point of view) and a field of concrete application can be regarded as being free from underlying fundamental open problems, even if it impossible to find any appearent reason or concrete argument or proof to reject it (or its operativity).

In fact, a probably good heuristical aphorism would be : *consider any evidence concerning the articulation of a mathematical theory (or point of view) and a field of concrete application as a way of temporarily covering an undected fundamental problem until it may be solved*. Correlatively, one may suppose that the heuristical value of such an aphorism increases as it is focused on last recently developped fields of application. The fact a difficulty may be detected a long time before the correlated problem is stated and solved, in spite of the fact that the theory using the evidences that make it operative, leads to a second methodological principle :

> SECOND METHODOLOGICAL PRINCIPLE. The operativity produced by the evidences allowing the application of a mathematical theory (or point of view) to a concrete field of application is never to be understood as a proof (or a sign) of a well-founded theory.

This methodological principle is stated to underline that such an operativity doesn't mean that the theoretical effort concerning foundamental theories is now to be stopped ; on the contrary, I want to suggest that such an effort really begins with a sufficiently well established operativity. One may easily understand this idea as follows : it is senseless to try to elaborate the fundamental principles and concepts leading to the unification of a theoretical field if there is nothing (or quite nothing) to unify ! More precisely, such an elaboration becomes a theoretical necessity when a theoretical field developpes towards the unknown limits of the evidences implied by its operativity, thus putting into evidence that some facts or considerations ar not compatible with (or cannot be interpreted with respect to) these evidences. Such difficulties indicate that some evidences are to be suspected and rejected :

> THIRD METHODOLOGICAL PRINCIPLE. An evidence must be rejected, even if it is impossible to find any concrete or formal arguments or proofs against it, if it blocks the elaboration of a fundamental principle or concept leading to a more general (and/or unified and/or simple) theory.

In other words, one may understand that some essential evidences are strongly related with the underlying structure of theories, and that they cannot be directly touched by formal proofs or experimental corroborations. I suggest to understand such evidences as being singularities which condenses a high degree of complexity (in fact which condenses some aspects of the structure of the theory itself) in order to understand that the rejection of such evidences is equivalent to partially deploy (explain, uncondense) those condensed aspects of the structure of the theory. This, in turn, makes understandable that such evidences, as long as they are admitted without any discussion, may contribute to block the elaboration of unifying and fundamental principles.

These three methodological principles apply only to fundamental researches : in the current practice of sciences, it is sufficient to rely on well established evidences, principles and postulates. But this is correct only if the results that such a practice wants to obtain are compatible with those evidences, principles and postulates. On the contrary, a fundamental research is motivated by some problems which cannot be evitated or eliminated, nor correctly solved within such limits, the correct solution of which requiring the enlargement of such limits, that is a reexamination of the evidences, the principles and postulates determining a too restricted theoretical field for their resolution. One may now remark :

> GENERAL REMARK. The theoretical field of computer science plays an essential role in the actual scientifical context in the sense that any difficulty concerning the articulation between information processing, formal mathematics and the theories of calculability may have fundamental consequences on the evidences, the principles and the postulates actually governing our conception of the positives sciences in general.

This means that computer science is a kind of nodal point with respect to the possibility of reducing "something" in the reality (physical systems, biological systems, cognitive systems, etc.) to a calculus in the sense of the theories of calculability. Correlatively, no difficulty emerging in this area, even considered as being a detail, can be regarded as being without interest.

Effective discrete systems

Effective computability and effective computation

The situation of information processing raises two interesting fundamental questions. The first one concerns the difference between the *effective computability* (the possibility of finding an effective procedure — which is a writing — and the possibility of obtaining some desired result in a finite number of steps) and an *effective computation* (the fact that some given rules are effectively applied). One can remark that a mathematical machine is effective, in the sense that it corresponds to an effective writing (one is theoretically able to write it), and is not effective, in the sense that nothing happens (and no result may be obtained) if the rules are not applied « in the reality » :

> QUESTION. When a mathematician is applying rewriting rules to effectively compute some result (according to the definition of some mathematical machine), why don't we regard such an effective system (mathematical machine + mathematician) as a real system ?

One can easily imagine a kind of experience where the processor of a computer has been replaced by somebody who scrupulously applies the state transition rules specifying this processor. If such a computer is certainly slower than the current ones, I am nevertheless quite sure that everybody will agree that, from a mathematical point of view, the difference is only a matter of technology, or perhaps of implementation details. Consequently :

> REMARK 1. From a methodological point a view, if we agree that an effective computation is to be considered as the effect of a real system, we recognize that the articulation between such effective computations and mathematical theories cannot be admitted as evident, since it implies a fundamental cut between what is properly mathematical and what is not.

In other words, the theories of effective computability concerns two kinds of effectivity : (1) the « effective effectivity » of the initial state (we are able to write the writing associated to the effective procedure and the data) ; (2) the « potential effectivity » of the computation (in terms of a number of steps). But these theories do not deal with the « effective effectivity » involved in the computations (or state transitions) themselves.

Regarding a [real] system as discrete

The second question concerns the articulation between real systems (such as computers, for example) and what we have in mind when we regard these real systems as information processing systems. On one side, if we suppose evident the link between such systems and the theories of effective computability, it is unavoidable to at least suppose that those systems are discrete (because these theories are built upon discrete operations — rewriting rules — concerning [discrete] writings) and finite (because the finiteness is a condition for effective computability). On the other side, the fact that we regard a real system as a discrete and finite information processing system doesn't [necessarily] imply that this system is *really* discrete. In many cases, the main part of these systems is obviously continuous from a physical point of view. Consequently :

> QUESTION. Since it is equally obvious that information processing systems (which may be supposed evidently reductible to the theories of effective computability) are discrete [and finite], and that the underlying real systems are not [necessarily] discrete, why do we usually behave as if the fact of regarding such systems as discrete was evident ?

One may note that this question is especially interesting from several points of view, since, for example, it concerns one of the most fundamental condition of possibility for experimental sciences : the predictions allowing the experimental theories to be corroborated cannot be actually conceived without effective computations, even if these theories are supposed dealing with continuous phenomenas. I know that it could be objected that such an articulation is usually refered to the concept of approximation (of measuring, of representation, etc.). But, in the context of information processing, it is somewhat difficult to interpret what seems to be the most exact and perhaps the most mechanical part of the mathematics as an approximation of... of what ? Should we also assume that the effective application of an inference rule involved in a formal demonstration is in fact an approximation of... of what ? It is easy to understand that such ideas directly concerns some of the most fundamental postulates and principles on which mathematics and logics (as we actually conceive them, especially when formalized) are usally supposed to be built.

> REMARK 2. From a methodological point of view, the fact of regarding a real system (not [necessarily] really discrete) as discrete cannot be admitted as evident, since it reactivates, within a new context, one of the oldest mathematical problems : the articulation between the discrete and the continuous.

This remark suggests the existence of some underlying and strange singularity, since the fact of regarding something as discrete doesn't produce any tangible trace : on one side, this thing is [generally] not yet discrete ; on the other side it is already envelopped and hidden within the discrete point of view. When we work with a computer, for example, we forget the underlying continuous system to regard it as discrete. There is no tangible trace of such a transformation, which is nevertheless *effective*.

The theoretical field of effective discrete systems

If we now put these remarks together in order to try to understand their relations, we observe that the usual frontier between real systems and mathematical theories involves two different cuts which, by the effect of an accidental coincidence, seem to be the same one :

> REMARK 3. From a methodological point of view, we must make a difference between two kinds of articulations : the first one articulates real systems (generally understood as continuous) with [theoretical] effective discrete systems ; the second one articulates these [theoretical] effective discrete systems with mathematical theories or points of view.

With respect to many usual evidences, the two articulations seem to be the same one ; thus, the theoretical field of effective discrete systems vanishes into this accidental coincidence, in such a way that this field has no theoretical existence. By the fact, the direct connection between real systems (generally continuous) and the theories of effective computability (discreteness and finiteness) is a kind of dazzling short-cut which hides the underlying gap.

If one agrees with me that an apple being falling implies an effective movement, then one agrees also with me that the effective transition between two discrete states implies also a kind of effective « movement ». Clearly, the fact that the states are regarded as discrete (i.e. associated to writings) doesn't mean that, from a theoretical point of view, we are allowed to state that an effective transition between such states is « nothing » (as suggested by the usual conception of discrete and finite systems).

> REMARK 4. Because the [mathematical] theories of effective computability are adequate to their object (effective computability), these theories cannot be considered as being ultimately adequate to effective computations and, more genrally, to effective discrete systems.

The simple comparison between continuous movements and discrete state transitions makes obvious that the problem of elaborating a theory of effective discrete systems is facing the ZENO's paradoxes. But the solution elaborated in the context of continuous (or understood as continuous) phenomenas (mechanical, geometrical, etc.) doesn't seem to be [directly] applicable in the context of discrete (or understood as discrete) phenomenas.

A fundamental implication

Before a brief examination of some evidences related to these questions, I want to suggest a possible (and partial) explanation which tries to make understandable that a [mathematical] theory of effective discrete systems has not yet been elaborated. It is obvious that a mathematical « machine » becomes an effective discrete system if (and only if) an effective discrete system — a simulator of such a « machine » on a given computer, for example — is added to the mathematical « machine » itself. From a mathematical point of view, such an addition, which involves a real system, is irrelevant. Then, the only remaining issue is to invite the mathematician itself to do that job. As already remarked above, the theoretical consequences are exactly the same. The main difficulty has now been reached :

> REMARK 5. If it is not impossible to elaborate a [mathematical] theory of effective discrete systems, then any effectively applied formal process (computation, inference, etc.) is to be articulated with such a theory.

Among the consequences implied by the mathematical formalization is the importance given to effective formal processes. Theses processes, which obviously concern [discrete] writings, are not only potentially effective processes. In many cases, theses processes must be effectively applied by the mathematician. For example, it is not (alas !) sufficient to read a set of axioms to obtain immediately the knowledge of all the theorems which can be demonstrated. The effectivity required by a demonstration cannot be reduced only to a problem of finiteness (the number of steps of the demonstration) ; something must be effectively done by a mathematician so that a formally derived theorem becomes a meaningfull theorem for him. Another example concerns the fact that the knowledge of the complexity of a given program (potential effectivity related to a number of steps) is not the same thing than the knowledge of the result produced by this program from a given data. From the point of view of potential effectivity, an elementary step is valued as 1 ; but from the point of view of « effective effectivity », one effective transition cannot be properly reduced to strictly finite considerations.

It is now possible to perceive another short-cut when we try to put together all these remarks. On one hand, we usually believe that mathematical formalization is founded upon strictly finite (and discrete) considerations, and that there is « nothing » under such a ground ; on the other hand, as soon as we take into account the fact that many formal processes are effective (and not only potentially effective) like any other effective discrete system, it becomes unbelievable that this foundation of mathematical formalization could be the very ultimate ground :

> REMARK 6. It is not impossible that the elaboration of a theory of effective discrete systems is actually blocked because of its potential implications concerning the foudations of mathematical (and logical) formalization.

This hypothesis makes perhaps understandable that the efforts to elaborate the foundations of a theory of discrete information processing always have always vanished into the theories of effective computability. It also perhaps explains why the coincidence of effective computability and effective computation seems so evident, although it is probably the accidental effect of some underlying and yet undetected singularities on which mathematical and logical formalizations are built.

Endless regressive problems

Levels of discretization

I have made an allusion to ZENO's paradoxes. Some simple remarks concerning our most basic practice of computers [1] bring a lot of arguments to corroborate such a point of view. It is well-known that we can regard a computer (which is a real system) as an effective discrete system if (and only if) we choose (or we determine) a level of discretization (or a level of observation, or a level of abstraction). Clearly, it is impossible to speak about a discretization [of a real system] without implying a correlated level [of discretization].

> REMARK 7. In the field of effective discrete systems, an irreductible (or elementary) transition is only irreductible relatively to a particular level a discretization.

More precisely, the choice of a level determines which transitions are to be regarded as irreductible and *vice versa*. For example, at the machine language level, each irreductible transition corresponds to the effect of the interpretation of one machine language instruction ; but we know that each instruction may be cut into a sequence of microinstructions, each microinstruction being regarded as irreductible relatively to the microprogramming level. More generally :

> REMARK 8. When we try to reduce an effective discrete system to finite considerations, we « forget » : 1. its « effective effectivity » ; 2. the fact that such a reduction is conceivable only in relation with the choice of a level.

As far as I know, the actual theories of effective computability are neither [explicitely] founded on some « level of finiteness », nor related [explicitely] to any fundamental and mathematical concept corresponding to such levels. Thus :

> REMARK 9. It is not impossible that the theory of [discrete] level transitions and the theory of effective discrete systems are actually blocked for the same fundamental reasons.

The expression « level transition » covers a wide variety of actually open problems, such as : [levels of] discretization, [levels of] observation, [levels of] specification, [levels of] representation, [levels of] abstraction, [levels of] languages, etc. Those problems are so important (both theoretically and practically), especially in the field of information processing systems, that the effective discrete systems problem is perhaps secondary with respect to the [discrete] level transition problem. However, if the specification of an effective discrete system implies the choice of a level, and *vice versa*, the two problems probably imply each other :

> REMARK 10. It is probably impossible to solve the effective discrete systems problem without solving the [discrete] level transition problem, and *vice versa*.

Effectivity and endless regressions

It would be much difficult (and perhaps impossible) to rub out the word *level* in the fields of effective discrete systems and computer science. Although such a concept is probably incompatible with our usual conception of [mathematical] finiteness, we use it as an evidence, that is to say as a way of temporarily hiding some underlying and yet unsolved fundamental problem :

> QUESTION. Is it always possible to cut a relatively irreductible transition into a sequence of irreductible transitions relatively to an underlying level ?

The ZENO's arguments apply in the discrete case : an effective discrete state transition will never be equivalent to a collection (even infinite) of states, for the simple reason that a transition always takes place *between* states. Thus, if we agree that the decomposition of any effective transition leads to a sequence of effective transitions (relatively to an underlying level), we agree also that, from a theoretical point of view, such a process of decomposition, which is also a process of level transition, must be conceived as *endless* :

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1. Theses remarks may be translated to any effective discrete information processing system.

THEORETICAL EQUIVALENCE. Any « irreductible » and effective discrete state transition is theoretically equivalent to the achievement of the development of an endless regression.

Although the concept of « achievement of the development of an endless regression » is obviously self-contradictory (if the regression is supposed endless, it is contradictory to speak about the achievement of its development), this conjectural equivalence is nevertheless coherent : if we agree that it is impossible to properly reduce an effective discrete transition to any motionless writing (or formula), then, since any scientifical theory concretely consists in [motionless] writings (written on books, papers, etc.), it is perfectly coherent to build the theorical concept of effective transition thanks to a postulate which guarantees that, from a theoretical point of view, no [motionless] writing may be said equivalent to (or an adequate representation of) an effective transition. It is obvious, for example, that a writing like `{a→b}` (standing for : rewrite `a` as `b`) is not (and will never be) [the same thing as] an effective transition ; the same remark applies to writings like `a→b` (standing for : a discrete transition from the state `a` to the state `b`) where the arrow, even if we read it as an indication of some effectivity, is, from a formal point of view, nothing else than a *letter*. The above theorical equivalence is not surprising ; in fact, it is similar to well-known classical schemes like, for example,

`1 = 1/2 + 1/4 + 1/8 + ... ad infinitum`

The theoretical equivalence means also that, from a theoretical point of view, it is impossible (or it is contradictory) to suppose the existence of some ultimate (or « absolutely irreductible ») effective transition between two discrete states. Such an impossibility, involved in many LEIBNIZ's ideas, has a particular implication in the context of effective computability :

REMARK 11. The CHURCH thesis may be related to the problem of finding a unique and « absolute » (or ultimate) mathematical machine to which all universal mathematical machines could be reduced.

If one had found such an « absolute » mathematical machine (and mainly CHURCH itself), the CHURCH thesis would probably no longer be a thesis :

THESIS OF THE ABSOLUTE MATHEMATICAL MACHINE. From a theoretical point of view, the hypothesis of a unique « absolute » (or ultimate) [effective] mathematical machine must be rejected.

In other words : if a machine is [said] effective, then each of its effective « irreductible » transitions is equivalent to the achievement of the development of an endless regression ; consequently, this machine is not « absolute » (the ultimate one, the last one). The puzzle of open fundamental problems is perhaps now becoming a little more coherent : whereas it is usually assumed that the mathematical (and logical) formalizations are built upon the supposed ultimate (and perhaps « absolute ») ground of finiteness, it appears that the effectivity required to obtain any theorem (or result of computation) in such formalizations involves endless regressive processes (decomposition and level transition). The most fundamental short-cut is now obvious :

REMARK 12. The finiteness of the considerations to which it is actually usual to reduce effective discrete systems (including mathematical and logical formalized theories) may be understood as a particular way of « solving » endless regressive problems.

It is perhaps unusual to understand [mathematical] finiteness from such a point of view. However, one may note that all formal theories are in fact discrete (since formal writings are themselves discrete) ; correlatively, the elaboration of [transfinite hierarchies of] meta-theories may be understood as an other [mathematical] way for « solving » endless regressive problems, thanks to the mutual implication which links together the discrete state transition problem and the [discrete] level transition problem.

Mathematical functions and effective computations

Functions and effectivity

The above remarks suggest that it is highly probable that some singularities play a fundamental part to make the field of effective discrete systems avoidable within the actual scientifical context. However, this suggestion doesn't mean that these singularities never appear, and that there is absolutely no trace of their existence. On the contrary, I shall now briefly examine a few number of basic evidences concerning a usual way of considering the articulation between effective computations (or discrete information processes) and [the usual conception of] mathematical functions.

It is not unusual to regard functions as if they were effective. But, from a mathematical point of view, such a consideration is only a way of speaking. Let us examine the usual definition of a function. We call *function* a triplet

$f = (G, X, Y)$

where G, X, Y are sets satisfying the following conditions :

(1) $G \subseteq X \times Y$
(2) for all $x \in X$, there exists one and only one $y \in Y$ such that $(x,y) \in G$

From a mathematical point of view, nothing in this definition allows the interpretation of a function f as a kind of discrete transition from the set X to the set Y, or the interpretation of a pair (x,y) as a kind of discrete transition from the data x to the result y. Suppose that some abstract object "w" is [equal to] the pair (x,y) :

```
abstract objects       "w"
________________       / \
   formalism          w  =  ( x  ,  y )
________________
  transitions         ?       x ──▶ y
```

Should we consider equally evident to associate the transition $x \rightarrow y$ to the denotations w and (x,y) of "w" ? From a mathematical point of view, the pair (x,y) is [a denotation of] *one* abstract object (noted here "w"), not [a denotation of] the relationship *between two* abstract objects. Note that if such interpretations were mathematically founded, it would be necessary to at least suppose that any conceivable mathematical function is obviously discrete ! Note also that the cartesian product doesn't imply the idea of [discrete] transition :

> REMARK 13. The *direct* identification of an effective discrete system with a mathematical function conflicts with several basic definitions, postulates and interpretations involved in the mathematics *as we actually conceive them*.

This remark doesn't imply that such evidences are not an efficient way of speaking (from a didactical point of view, for example), sometimes used by the mathematicians themselves ; it doesn't imply that no operative result may be obtained thanks to such an evident identification ; it simply means that such an identification is, in the best case, nothing more than a kind of conjecture. Like in physics, such conjectures may lead to operative results, but only within a constrained domain of applicability which is alas generally not precisely known (for the obvious reason that evidences are always invoked to hide underlying and unsolved problems) ; correlatively, it may happen that such conjectures lead to unexplained « difficulties », to « surprising » theorems, to unsolvable problems, etc., when used without sufficient care outside their domain of applicability, until they are rejected and replaced by more satisfying conjectures.

In the case of effective computation and effective discrete systems, the explanation of this identification purloins the above remarks concerning the effectivity involved in the mathematical formalisms (and implemented by the mathematician itself) : when we operate on formal writings, we effectively apply [discrete] operations to transform writings into other ones ; but these effectively applied operations over formal writings do not properly reflect the [supposed] motionless world of abstract mathematical objects. The point is simply that :

> REMARK 14. The effectivity which is required by the use of a formalism (and which is implemented by the mathematician itself) cannot stand for a mathematical theory of discrete effective systems.

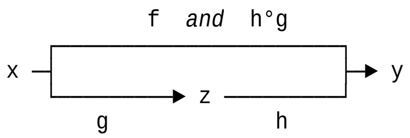
If one pays sufficient attention to his own pratice, one may be convinced that, in several circumstances, mathematical formalisms, because they require an effectivity which is similar to the effectivity involved in effective discrete systems, may be used as a kind of *simulation* of these systems, instead of their [unelaborated] *mathematization*.

Functionnal composition and sequential transitions

It is not unusual to use the composition of functions as a mathematical equivalent of sequential transitions. From a mathematical point of view, if an equality like

`f = h°g`

is true, a contradiction is introduced as soon as we suppose any kind of difference between the function [denoted by] `f` and the function [denoted by] `h°g`. Even in a diagram like



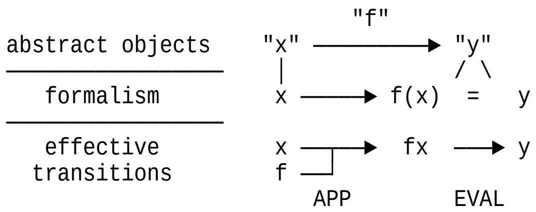
there is only one arrow for `f` and `h°g`, since these functions are the same one ; correlatively, if we associate the other path to `h°g`, we imply that the functions `f` and `h°g` are not equal, and thus we imply a contradiction :

> REMARK 15. The *direct* identification of a composition of functions with a sequence of effective transitions conflicts with several basic definitions, postulates and interpretations involved in the mathematics *as we actually conceive them*.

This remarks simply means that, from a mathematical point of view, we are not allowed to read `h°g` as `g` « and then » `h`. Correlatively, a function is not a transition in one step, and the composition of functions doesn't mean that the same function is sometimes in one step, and some other times in several steps. Considering the graph `G` of a function `f` (in the above definition), what would mathematically mean, for example, « a subset of the cartesian product `X x Y` in four steps » ?

Effective computations and the identity of a result

Some difficulties arise when we try to precisely articulate the usual mathematical point of view (functions, sets, equality, etc.) and the states and transitions required by the point of view of effective computations. Let us examine the following diagram in which `APP` stands for *apply*, and `EVAL` for *evaluate* :



Consider the third level (effective transitions) : when we have just applied a function `f` to an element `x` of its domain, we don't have the result `y` itself, but a kind of expression of the result, symbolically noted here `fx`, where the letter `f` stands for a representation (effective procedure, program, etc.) of the [abstract] function `"f"`. This expression `fx` may be understood as the initial state of an effective computation (arrow `EVAL`) taking the writing `fx` as a data and producing the writing `y` as a result.

> REMARK 16. If we consider the fact of applying a function as a function `APP`, this function `APP` must be itself applied, so that this problem is an endless regressive problem. For the same reasons, if we consider the fact of evaluating the result of a function as a function `EVAL`, this function `EVAL` must be itself [applied and] evaluated, so that this problem is also an endless regressive problem.

One may be tempted, as suggested in the above diagram, to suppose that the expresion `fx` (third level) corresponds to the denotation `f(x)`, knowing that `f(x)` and `y` (second level) are equal, equality which is usually interpreted as the fact that `f(x)` and `y` denote the same abstract object `"y"`. We must then suppose that the effective computation `EVAL` corresponds to the abstract identity of `"y"`, since `f(x)` and `y` are equal. In this case, `EVAL` doesn't correspond to the function `"f"` itself. But, although `f(x)` and `y` are equal (second level), it is impossible, from the point of view of effective computations (third level) to admit that the evaluation `fx→y` is the same as `y→y` ; thus we are obliged to suppose a difference between `fx` and `y` (third level), difference which implies a contradiction since `f(x)` and `y` (second level) are supposed equal. Furthermore, if the expression `fx` corresponds to the denotation `f(x)`, the fact of applying (arrow `APP`) a function `"f"` is, in some way, associated with the applied function `"f"` itself.

> REMARK 17. In the above diagram, the *direct* correspondence between the effective computation `fx→y` and the equality `f(x)=y` conflicts with several basic definitions, postulates and interpretations involved in the mathematics *as we actually conceive them*.

The unrefered expression of a result

Consider now the following diagram which tries to avoid [some of] the above difficulties :

```
                            "f"
abstract objects     "x" ----------> "y"
_________________     /               / \
   formalism        x               f(x) = y
_________________
   effective        x -┐-> fx -----------> y
  transitions       f -┘
                        APP       EVAL
```

There must exist an arrow between `x` and `fx` (third level) : in the expression `fx`, if the letter `f` (which stands for a representation of the function `"f"`) is not an empty word, then the writings `x` and `fx` (third level) are necessarily different. There must exist an arrow between `fx` and `y` (third level) because the evaluation `EVAL` is supposed to be an effective computation (at least in one step). Since the writing `fx` is, in general, different from `x` and `y` (third level), it is obvious that the evaluation `EVAL` doesn't [directly] correspond to the function `"f"`. Furthermore, as remarked above : (1) if we state that `f=EVAL°APP`, then any difference between `f` and `EVAL°APP` implies a contradiction ; (2) supposing that `APP` and `EVAL` are functions implies at least two endless regressive problems. Moreover, the above diagram suggests that the function `"f"` has been cut into two parts, `APP` « and then » `EVAL`, so that the expression `fx` (third level) is not refered to any mathematical object :

> REMARK 18. In the above diagram, since the expression `fx` (third level) has no mathematical status, it is impossible to suppose that `APP` and `EVAL` are functions, or any other mathematical object, so that no articulation has been established.

The fact of applying a function and the identity of a data

However, one quite strange hypothesis may be proposed in order to give a mathematical status to the expression `fx` :

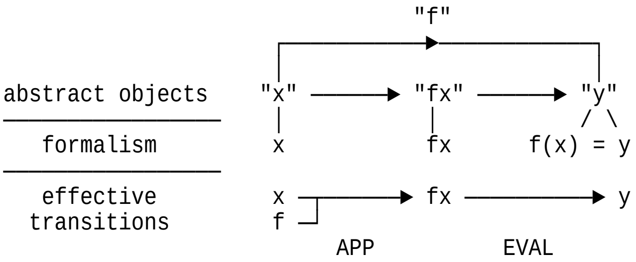
```
                              "f"
abstract objects     "x" ----------> "y"
_________________    / \              / \
   formalism        x  =  fx        f(x) = y
_________________
   effective        x -┐-> fx -----------> y
  transitions       f -┘
                      APP       EVAL
```

On this diagram, the expression `fx` (third level) is associated to a denotation `fx` (second level) of `"x"`. Thus, `x` and `fx` (second level) may be said equal (with respect to the usual interpretation of the mathematical equality). Correlatively, since `fx` (second level) denotes `"x"`, and since `y` (second level) denotes `"y"`, the arrow `EVAL` (third level) corresponds to the abstract arrow `"f"` between the abstract objects `"x"` and `"y"`. The difficulties are now concentrated on the arrow `APP`, which implies a difference between `x` and `fx` (third level) corresponding to the equality `x=fx` (second level), thus implying a contradiction. Besides, the difference between the writings `x` and `fx` (third level), which is the [letter `f` standing for the] representation of the function `"f"`, vanishes into the identity of the abstract object `"x"` thanks to the equality `x=fx` (second level).

> REMARK 19. In the above diagram, the *direct* correspondence between the fact of applying a function `f,x→fx` and the equality `x=fx` conflicts with several basic definitions, postulates and interpretations involved in the mathematics *as we actually conceive them*.

The requirement of abstract representions

The last diagram I shall briefly comment tries an other way for giving a mathematical status to the expression `fx` of the result. In the following diagram, the abstract object `"fx"` may be easily understood, for example, as an abstract word belonging to some well choosen set of words over a given alphabet :



The abstract object `"fx"` is different from `"x"`, and the arrow `APP` becomes more understandable. The abstract object `"fx"` is different from `"y"`, and the arrow `EVAL` may be easily associated to the abstract arrow between `"fx"` and `"y"` ; in return, the arrow `EVAL` is not associated to the abstract arrow `"f"`. Note again the difficulty concerning the equality `f=EVAL°APP`.

This diagram is not far from some usual conceptions where the abstract representations (in terms of abstract words, for example) are a kind of reflected image (with a one-one correspondence) of the concrete writings involved in effective computations. But, what problem are we trying to solve ? The initial problem is to propose [the basis for] an articulation between mathematical functions and effective computations. What does the above diagram propose ? It proposes to « solve » the initial problem concerning *one* function by cutting it into two instances of the same problem, because there are now *two* functions. The ZENO's argument applies, and the above diagram is only starting the development of an endless regressive problem. If we assume that the initial problem is not meaningless, then nothing has been solved :

> REMARK 20. If we try to articulate effective computations and mathematical functions thanks to intermediate abstract representations : (1) we fall down into several endless regressive problem implying ZENO's paradoxes, (2) nothing is solved, and (3) we conflict with several basic definitions, postulates and interpretations involved in the mathematics *as we actually conceive them*.

Conclusions

The remarks briefly presented in this paper must be understood in a highly positive way for at least four main reasons. The first one concerns the lack of a [mathematical] theory of effective discrete systems :

> FIRST CONCLUSION. If there were no difficulty at all to articulate the effective discrete systems and the usual [conception of] mathematical objects, the hypothesis of a lack of a [mathematical] theory of effective discrete systems would be unfounded.

In a way, those difficulties corroborate the hypothesis. The second reason is related to a very fundamental consideration :

> SECOND CONCLUSION. The more we confirm that no appropriate articulation between effective discrete systems and the usual [conception of] mathematical objects may be found, the more we are guaranteed that a mathematical theory of effective discrete systems is possible.

This conclusion is not a paradox. It means a kind of independance between such a theory and our actual conception of mathematics. It means also a kind of resistance which notifies that the underlying structure of effective discrete systems, which cannot be properly captured thanks to some usual and insufficient evidences, requires a real and fundamental research to be theorized and mathematized. The third reason concerns the precise problematical point :

> THIRD CONCLUSION. The difficulties do not arise because some newly discovered field of real discrete systems conflicts with a well established mathematical theory of effective discrete systems ; on the contrary, they arise because we try to avoid the lack of such a mathematical theory by enforcing an impossible articulation thanks to inappropriate and unfounded evidences or ways of speaking.

In this sense, each difficulty gives us the indication that the appropriate articulation has not yet been discovered. I feel important now to underline that such a situation is a very « classical » one : the history of mathematics shows that many among the most fundamental progresses have been strongly connected with an effort for rejecting particularly « evident » evidences. Anyhow, from a methodological point of view, it is never sufficient to invoke evidences to articulate mathematical theories and reality, either to justify such theories, or to obtain operative applications. The fourth reason concerns the presence of endless regressive problems :

> FOURTH CONCLUSION. From a fundamental point of view, it seems more coherent to compare an effective discrete state transition to an effective [continuous] movement, than to reduce such a transition to finite considerations, and then to « nothing » ; correlatively, it seems possible to articulate in one unifying theory the discrete state transition problem and the [discrete] level transition problem.

Probably, a theory of effective discrete systems depends on the possibility to elaborate a reinterpretation (and a generalization) of some very fundamental ideas of LEIBNIZ in order to make them appropriate to a discrete (and perhaps not numeric) theoretical field. In this case, the field of effective discrete systems and the field of [effective] continuous systems should probably be conceived as two different developments (or instances) of a unique underlying and more fundamental structure.

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CANTOR, Georg
[1872-1899] *Correspondance avec R. Dedekind*, Traduite dans : J. Cavaillès, Philosophie mathématique, Hermann, Paris, 1962.

CHURCH, Alonzo
[1956] *Introduction to Mathematical Logic*, Princeton University Press.

CURRY, Haskell B.
[1963] *Foundations of mathematical logic*, New York, Mac Graw Hill.

EINSTEIN, Albert
[1916-1955] *Correspondance avec Max Born*, Le Seuil, Paris, 1972.

FREGE, Gottlob
[1879-1925] *Ecrits logiques et philosophiques*, Traduction et introduction de C. IMBERT, Le Seuil, Paris, 1971.
[1884] *Les fondements de l'arithmétique*, Traduction et introduction de C. IMBERT, Le Seuil, Paris, 1969.

GÖDEL, Kurt
[1944] *La logique mathématique de Russell*, The philosophy of Bertrand Russell, Tudor publishing Company, New York, 1944 (trad. française in Cahiers pour l'analyse, n° 10, Le Seuil, Paris).

HOLTON, Gerald
[1973] *L'imagination scientifique*, Gallimard, Paris.
[1982] *L'invention scientifique*, Puf, Paris.

KUHN, Thomas S.
[1983] *La structure des révolutions scientifiques*, Flammarion, Paris, 1983.

LEIBNIZ, Gottfried Wilhelm
[1686] *Discours de métaphysique (suivi de la correspondance avec Arnauld)*, Vrin, Paris, 1984.
[1710] *Essais de Théodicée*, Garnier Flammarion, Paris, 1969.
[1714] *La monadologie*, Delagrave, Paris, 1983.

MARKOV, A. A.
[1962] *Theory of algorithms*, Académie des sciences, Moscou, URSS.

POPPER, Karl R.
[1963] *Conjectures et réfutations*, Payot, Paris, 1985.

SERRES, Michel
[1968] *Le système de Leibniz et ses modèles mathématiques*, Puf, Paris.

VAUDENE, Didier
[1992] *Une contribution à l'étude des fondements de l'informatique*, Thèse de doctorat d'état, Paris (rapport LITP n° 92-75).

WITTGENSTEIN, Ludwig
[1921] *Tractatus logico-philosophicus*, Gallimard, Paris, 1961.