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From Paraconsistent Logic to Universal Logic

FROM PARACONSISTENT LOGIC TO UNIVERSAL LOGIC*

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«The undetermined is the structure of everything»

Anaximander

1. Paraconsistent Logic (Paris, 1989-91)

1. 1. Discovery and interest

My first contact with paraconsistent logic was a one page article in the French psychoanalysis magazine, *L'âne*, entitled something like «Paraconsistent logic: a logic for the inconscious». This was in fact an interview with da Costa. It was of quite general nature, paraconsistent logic was presented in a totally informal way, just as a logic violating the principle of contradiction.

But it was enough to strongly attract me. Why? Some people are attracted by paraconsistent logic, via contradictions, i. e. they think that contradictions are fundamental and therefore are naturally interested in a logic which does not reject them, but deal with them.

This was not my case. I was attracted by paraconsistent logic because I was interested in the question *What is logic?* Traditionally the principle of contradiction is taken as a fundamental pillar of logic. The idea is that reasoning is not possible without it. Paraconsistency goes against this idea. And if paraconsistent logic is rightly a logic, therefore what are the ground principles of logic, if any?

At this time I was studying logic at the department of mathematics of the University of Paris 7. Daniel Andler was giving there a graduate course on non-classical logics including modal logic, temporal logic, non-monotonic logic, etc. Linear logic was also in the air. But none of these logics attracted me as much as paraconsistent logic did.

It is clear that a logic like e. g. *linear logic* is far to be as challenging as paraconsistent logic. Informal motivations for linear logic are based on a few attractive and funny examples involving cigarettes and food, but they are not connected with a serious philosophical analysis (much the same as the penguin case for non-monotonic logic). Moreover there is a big gap between these informal motivations and the technical aspects of Girard's logic. Until now there are no convincing intuitive interpretations of linear logic operators.

Linear logic is tightly connected with the so-called structural rules of sequent calculus and it had a key role in the emergence of the new research field of substructural logics.[1] Of course there is a huge amount of nice technical problems related to linear logic. But it is true also for Kripke semantics and matrix theory.

1. 2. First Researches

I looked for da Costa's works at the university's library and started to work with few materials (the very sketchy notes of the CRASP, and some papers published in NDJFL).[2] My objective was to study thoroughly the paraconsistent logic C_1.

My attention was directed to the common ground between C_1 and classical logic. These two logics are very different and my intuition was that the very essence of logic should not lie in any of their specific differences but on their common features.

The semantics for C_1 is at first sight very strange, because it is a blend of known and unknown materials. On the one hand it looks like semantics for classical logic because it is *bivalent*, on the other hand it looks very different because it is *not truth-functional* and in particular you cannot start with distributions on atomic formulas and then extend them to bivaluations on the whole set of formulas.

The common feature is that both are characteristic functions of maximal sets. In fact when you have a logic, you can always consider the class of characteristic functions of maximal sets, this makes sense even in the case where they are not at the same time homomorphisms, like in the classical case. Moreover this notion of maximal set can be defined in a purely abstract way. Often a maximal set is called a maximal consistent set and its definition depends on negation. But this must be different in paraconsistent logic, because a theory can be inconsistent without being trivial (one can in fact found paraconsistency on this distinction). The common ground notion of paraconsistent logic and classical logic is the *abstract notion of non trivial maximal set.*

This notion palys a key role in the completeness theorem of many logics. Studying a lot of non-classical logics, I saw plenty of completeness theorems and apparently there was an invariant kernel and this was related to *Lindenbaum's extension lemma* saying that every consistent set can be extended in a maximal consistent one.

The completeness theorem often appears as a kind of magic link connecting two different ontological fields: proof and truth. A close study of Lindenbaum's lemma helps to understand better this magic. Moreover if the notion of proof is defined with a sequent calculus instead of an Hilbert's style system, the completeness effect is not so spectacular.

Sequent calulus was quite popular at that time in Paris mainly because of linear logic. I remember a course of Girard presenting simultaneously and comparatively classical,

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1. On substructural logics see Dosen/Schröder-Heister (1993). This field is in fact not new, just the name for it. For example Avron (1988) shows that there are some striking resemblances between linear logic and relevant logic.

2. Da Costa's works were first published in the *Comptes Rendus de l'Académie des Sciences de Paris* (CRASP, first note (da Costa 1963), the referenes of other notes can be found e. g. in D'Ottavinao (1990)), through Marcel Guillaume (see Guillaume 1996). At this time I wrote to the latter who kindly sent me a joint work of him with da Costa published in Brazil that I was not able to find in France. Later on da Costa started to publish in *Notre Dame Journal of Formal Logic* (NDJFL) where a lot of papers on paraconsistent logic have appeared along the years.

intuitionistic and linear systems of sequents and giving very intuitive hints on how sequent calculus works and on the cut-elimination theorem. I got very interested in the subject and learned it thoroughly by myself reading Gentzen's original paper.

Therefore it was natural for me to try to build a sequent system for C_1 and prove *cut-elimination* for it. There had been an aborted tentative in the late sixties for doing this by Raggio[3]. I built a set of sequent rules for C_1 using an *intuitive transposition of semantical conditions*. Only four years later I was able to prove a general completeness theorem which explains why this intuitive transposition was working.

I then verified that monstrous rules with three premisses and without the subformula property were not conflicting the sophisticated machinery of the cut-elimination theorem, showing that necessary conditions for this theorem are of a quite general nature, and that in particular the subformula property is not one of them.

My study of C_1 was presented in my Master thesis supervised by Daniel Andler, at the department of mathematics of the University of Paris 7.[4] I then started a PhD with him in the same line and projected, with his support, to go to spend sometimes in Brazil with da Costa. I was lucky to meet da Costa just at this time, in January 1991, when he was visiting Paris and I was needing intuitive interest and a formal letter in order to go to Brazil.

I saw da Costa for the first time when he was presenting a memorable lecture in Paris during which, so enthusiastically animated, he performed a spectular jump, nearly breaking his legs. I was introduced to him after the lecture and the contact was quite good. I gave him a kind of abstract of my Master thesis about C_1 which he liked very much and we met again several times. He asked me why I was interested in paraconsistent logic and was satisfied with my answer. My trip to Brazil was projected for august.

In fact just before meeting da Costa, my interest had already shifted definitively from paraconsistent logic to *general logical stuff* (at this time I had no name for this kind of thing, I had heard about *general abstract non sense* for category theory and I liked the expression).

This had arised mainly due to two influences. The first one was a line of research developed by da Costa himself under the name *theory of valuation*, which I knew through his paper with A. Loparic: «Paraconsistency, paracompletness and valuation» (see Loparic/da Costa 1984). In this paper there is a first part which is a general form of completeness theorem which is then applied to a particular logic, inspired on C_1, which is both paraconsistent and paracomplete (i. e. neither the principle of contradiction nor the principle of excluded middle hold). After easily working out a sequent version for this system[5] I was eager to understand the essence of this general theorem, which would take me about one year.

The other one was the study of a little book by Curry, *Leçons de logique algébrique* (Curry 1952). I spent one month with it in the West Indies and came back quite enlightened. In this book Curry presents, among other things, a study of four kinds of negation. To carry out this study he develops a quite general framework based on such general notion as

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[3]. See Raggio (1968). Raggio was a former student of Bernays who worked on cut-elimination for natural deduction before Prawitz. At the same time of my work, W. A. Carnielli built a tableau system for C_1 and proved cut-elimination for it (see Carnielli 1990).

[4]. See (B 90a), the main results of it were later published in (B 93a).

[5]. This turns into my first published paper (B 89).

relational algebra and gives results such as a general form of the *replacement theorem*.[6] Over the years I kept on meditating on Curry's book which was an important source of inspiration for me.[7]

2. Abstract logic (Brazil, 1991-92)

2. 1. Saturation and valuation

I arrived in São Paulo, Brazil, in August 1991, where I was to stay about one year.

Arriving there I started to work on a series of problems that will turn into my paper «Recherches sur la logique abstraite: les logiques normales» (B 98h) which is itself a preliminary draft of the first chapter of my math PhD (B 95e), published separately as (B 95f).

At this time I already had decided to work only at the general level, and to use the terminology *Abstract Logic* to emphasize this and the fact that I was working independently of any specifications of languages and logical operators. I used the expression «logic» both as a generic term and also as a specific term. I defined an abstract logic to be a consequence relation on a given undetermined set. I stated this definition with no axioms for the consequence relation, even if my work was concerned mainly with what I called *normal logics* in which the three basic properties (reflexivity, monotony, transitivity) hold. My motivation and my terminology were taken from Birkhoff's famous notion of *abstract algebra*, that I found in *Lattice theory* (cf. Birkhoff 1940), which is just a set with a family of operations. My idea was already that the basic foundations of logic were not more principles for the consequence relation than principles for connectives, like the principle of contradiction. I reached the idea that we must throw out all principles altogether, that logic *is not grounded on any principles or laws*. In fact at this time I also launched the notion of *Alphabar logics*, which are abstract logics for which the *law of autodeductibility* (a formula is a consequence of itself) does not hold.[8]

An intuitive example of such a logic was given to me as an adaptation of da Costa's paraclassical logic.[9]

Within this framework of abstract logic I was tackling the general completeness theorem of da Costa's theory of valuation, according to which *every logic is two-valued*. The central notion in this theorem is the notion of *saturated set*[10] and not maximal set and I was

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6. According to Curry, this is a special case of an even more general result to be found in MacLane's PhD (cf. MacLane 1934).

7. I discovered later on that da Costa had also been influenced by this book, in particular to develop the algebaic counterparts of his C-systems, which he called «Curry algebras» (see da Costa 1966).

8. This work was later published (B 97e) in a joint paper with D. Krause a disciple of da Costa working mainly in Schrödinger logics, i. e. logic for which the principle of identity does not hold in general, the motivation being that according to Schrödinger the micro-objects of quantum physics do not obey this law (see da Costa/Krause 1994). The principle of identity is also one fundamental law of logic whose study and rejection have attracted me over the years (see B 96b).

9. On this logic see de Souza (1997).

10. A set of formulas is saturated iff there is a formula not deductible of it but deductible of any extension of it. A saturated set is maximal iff it is saturated for any formula outside of it. Saturated sets are also called relatively maximal sets, especially in the Polish school.

wondering why. I succeeded to find an answer that satisfied me after a precise and detailed study of the abstract version of Lindenbaum's extension lemma. I distinguised four kinds of Lindenbaum's extensions (two involving the concept of maximal set, two the concept of saturated set), all equivalent in classical logic but that I proved to be all distinct at the abstract level. Moreover I succeeded to prove that *the semantics of saturated sets is minimal.*[11]

I also tried to make a connection between the fact that all saturated sets ar maximal (in such a case a logic is said to be *absolute*, intuitionistic logic is not absolute) and the presence of certain logical connectives. By doing so I wanted to give an abstract characterization of logical operators. I succeeded to prove that both the existence of a classical negation or of a classical implication imply that a logic is absolute.[12]

This work was purely abstract in nature and no concrete examples of logics were given, nor did I dealt with the notion of systems of deductions (rules and proofs). It was complemented by a paper that I wrote with da Costa, which was the fruit of our collaboration during this year.[13]

The starting notion of da Costa's theory of valuation is a highly idealized version of an Hilbert's style system of deduction, simply called a *calculus*. The nature of the objects is not specified and the rules are just pairs with *no restrictions of recursivity or cardinality*. It is easy to see that in fact such a calculus, due to the definition of Hilbert's style notion of proof is the same thing as a normal abstract logic. Da Costa's definition fits better if one has the intention to apply general stuff to concrete Hilbert's style logical calculi.

By valuation, da Costa means generally any bivaluation, i. e. function which attributes true or false to formulas. His theory of valuation is a kind of generalization of his semantics for C_1 (see da Costa/Alves 1977), based on the fact that once truth-functionality is dropped, bivaluations can be used as a semantics for any calculus.

My paper with da Costa has two parts. One dealing with generalities, including some results without proofs about abstract logics, but also some abstract results about rules and proofs, definitions of these extended in order to catch Gentzen's style systems as well as Hilbert's ones. The second part is on applications and shows how concrete cases of completeness can be elegantly and easily obtained from general results. An important point is that with this method it is possible to give a proof of the completeness theorem for classical logic connective by connective (therefore this theorem is the disjoint union of all completeness theorems for classical connectives). There is a sketchy indication of how to apply this method

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[11]. This result was not new in the sense that there is an algebraic version of it which is known for years. However the logical version of this theorem apparently was not known or properly understood (cf. Suszko 1977), nor the consequences of it, for example the fact that intuitonistic logic cannot be characterized by maximal sets. This result shows also that the standard semantics for propositional classical logic is minimal, since it is made of maximal sets and classical logic is absolute.

[12]. David W. Miller proved independently the result about implication. He visited da Costa during my stay in São Paulo and turned to be interested in my work, due to the fact that at this time he was working on the question of the quantity of maximal extensions of a set. The expression «absolute logic» was suggested to me by David Makinson.

[13]. See (B94c). This paper is the first extensive exposition of da Costa's theory of valuation. A shorter and simpler exposition is to be found in Grana (1990).

for logics of any order.[14] It also includes da Costa's result about the characterization of truth-functional bivalent logics.

In another paper writing at this time (B 90b) I show how it is possible to generalize da Costa's methods for C_1 in order to construct a family of paraconsistent, paracomplete and non-alethic logics. Before arriving in Brazil, I already had the idea of extending naturally C_1 in a logic strictly stronger that I called C_1+. I didn't wrote at this time a paper devoted exclusively to this logic because I had already lost interest for the study of such or such system for its own sake. Therefore I presented C_1+ in a paper among many other logics all generated by the same guiding ideas.[15]

In the same paper I also introduced the notion of *non truth-functional many-valued semantics*. My initial idea was to construct a non truth-functional three-valued semantics for C_1 in order to get the subformula property. The equivalence between this semantics and the standard one was given by a theorem showing how *to reduce any semantics to a bivalent one*.[16] As this example shows, such a reduction theorem does not necessarily mean that non bivalent semantics are useless. They can be useful, for technical reasons or philosophical interpretations.

2. 2. Logic as structure

During my stay in Brazil I was to realized that my views on abstract logic were strongly connected with other works and ideas, mainly with Bourbaki and the Polish school of logic.

Da Costa was interested in Bourbaki since his youth. As it is known A. Weil, J. Dieudonné and A. Grothendieck spent each one about two years at the University of São Paulo during post-war time. They contributed strongly to the development of modern mathematics in Brazil. Da Costa's master, E. Farah was a close friend of Weil and the first Brazilian to work on set theory.[17] During the late eighties, da Costa's interest for Bourbaki was renewed by his research program, developed with F. A. Doria, on the axiomatization of Physics, which leads them to various incompleteness results for physical theories[18].

Therefore when I arrived in São Paulo, the Bourbakian concept of structure was in the air and da Costa spoke many time about this subject and indicated us bibliographical references such as Corry (1992) which very rightly points out an important heterogeneity between the Bourbakian informal notion of structure as it is presented in «The architecture of mathematics» (Bourbaki 1950) and the formal definition presented in *Theory of sets* (Bourbaki 1968). In my opinion this duality reflects perefectly that Bourbaki's idea to take the notion of structure as the fundamental notion of mathematics is independent of his formalist option

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[14]. This has been developed in more details in my philosophy PhD (B 96a).

[15]. An individual study of C_1+ was later on presented in (B 95c) and also in my math PhD (B95e).

[16]. This result is presented in (B 98h) (B 96a) and (B 95e). The relation with da Costa and Suszko's reduction results is discussed in (B 96c).

[17]. Farah proved the equivalence between the axiom of choice and the general distributivity law (see Farah 1955).

[18]. See e. g. da Costa/Doria (1991). They in fact mainly use Suppes predicate which is a kind of adaptation of Bourbaki's notion of strcuture (see da Costa/Doria 1994).

chosen in *Theory of sets*, which can be considered as an accidental feature motivated by the circumstances of the time and which was later on rejected by his main promoter (see Chevalley 1985). In fact «The architecture of mathematics» ends with a rather anti-formalist tone with the quotation of Lejeune-Dirichlet's motto: «to substitute ideas for calculations».

Learning more about Bourbaki, my impression was that my idea of abstract logic fitted perfectly well with the mathematical spirit of the General expressed by the slogan *From the general to the particular*.[19] And reflecting on Bourbaki's bright idea which revolutionized mathematics, my idea was to consider, within the architecture of mathematics, *logical structures as mother fundamental structures* but different from the three Bourbakian ones (algebra, topology, order).

In fact at this time I discovered at the library of the university of São Paulo a book by the French logician Jean Porte published in 1965 (the year I was born) and entitled *Recherches sur la théorie générale des systèmes formels et sur les systèmes connectifs*, with the same leading idea. Porte wrote:

> «Formal systems» considered here will be some mathematical structures (the word «structure» is taken here with a meaning close to the one given by Bourbaki, but slightly different), not much, not less «fundamental» than the class of algebraic structures for example. (Porte 1965, p. 2)

In many other points Porte's objective and methodology were the same as mine with my «abstract logic». His idea was to work in the spirit of abstract modern mathematics avoiding denotational and terminological complications often met in the formalist approach and trying to dissipate confusions by establishing a general framework providing clear stucturalist definitions of the basic notions of logic.

Porte didn't have a name for his general theory but he rejected the name «metamathematics» in particular because, as he wrote (Porte 1965, p. 3) his work was not restricted to formal systems representing mathematical reasoning.

Porte's book includes a lot of results of Polish logic. It is a bright exposition of the main achievements of the Polish school, such as Lindenbaum's results on matrices, at a time when these works were not well known abroad. But the book contains also a lot of Porte's own contributions. It is much in the spirit of the Polish school (as Porte says, p. 4, like Tarski, he will allow himself to use all the methods of reasoning that the standard mathematician uses) improved by a straight Bourbakian structuralist perspective.

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[19]. I learned a lot about Bourbaki in Brazil but of course I already had heard of him before! In fact I was part of the generation of school boys who have been Bourbakized by ultra-bourbachic pedagogues. But when I went at the University of Paris, the Bourbakian ideology was already widely dismissed. People were making the bill of the alleged disastrous effect of modern mathematics and the high-school programs had been changed in order to come back to 19th century pre-Bourbakian mathematics and get rid of abstract non sense, viewed as anti-democratic (sic). Moreover Bourbaki was not well considered among French logicians who had been persecuted by him. However, as an exception, my first course on set theory was given by M. Eytan and was based on Bourbaki and category theory (this course was considered as a monstrosity and was later on suppressed for «technical» reasons).

However Porte's work appeared to be actually quite distinct from what I was doing, especially due to the fact that he was working with more specific structures (distinguishing three classes of logical structures).[20]

The other important discovery for my work was *Polish logic*. The expression «Polish logic» is ambiguous, in fact it does not denote logic in Poland but a kind of logical studies which are mainly developed in Poland and not well-known abroad. R. Wójcicki has summarized these works in his book *Theory of logical calculi* (Wójcicki 1988). A first version of this book was written when he was in Brazil in the late seventies and published by Ossolineum under the title *Lectures on propositional calculi* (Wójcicki 1984).

The connection between da Costa and Polish logicians is old and is due in particular to Jaśkowski. Jaśkowski is a famous logician of the Lvov-Warsaw school of logic who is known as the creator of natural deduction (independently of Gentzen) and also for his result about intuitionistic logic (soon after Gödel's result showing that intuitionistic logic cannot be characterized by a finite matrix, Jaśkowski in his 1936's paper showed that it can be characterized by a infinite class of finite matrices). But in 1948 Jaśkowski also wrote a paper which is now considered as an important step in the history of paraconsistent logic. Jaśkowski's work on paraconsistent logic was rediscovered by da Costa, and he started, working jointly with some Polish logicians, the study of this forgotten work of Jaśkowski. During the seventies da Costa and other Brazilians such as A. I. Arruda went to Poland and Polish logicians, like J. Kotas, L. Dubikajtis and R. Wójcicki, went to Brazil.

Da Costa was therefore acquainted with the main concepts of Polish logic such as matrix theory and the theory of consequence operator. He used to present the consequence operator as an equivalent formulation of his notion of calculus.[21]

This is therefore through da Costa that I myself got soon acquainted with the basic notions of Polish logic. It seemed to me that it was very close in spirit to what I was doing and that I should investigate it seriously. Hence, as soon as december 1991, I had already decided that my next destination after Brazil would be Poland.

In August 1992, I went to the IX Latin-American Symposium on Mathematical Logic in Bahía Blanca, Argentina and presented there a little lecture on my work with da Costa on the theory of valuation (B 93c). By coincidence there was there a prominent Polish logician, Stanislas Surma, who presented a very interesting talk (see Surma 93). I had a conversation with him on the train back to Buenos Aires and as I told him I will soon be in Poland, he draw me a map of logic in Poland (names of cities and logicians). Unfortunately the difficult Polish language didn't help my memory and when I arrived in Poland I didn't remember anything.

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20. Porte's book is quite unknown and had no influence. It was several years ahead of his time. Porte himslef spent most of his career in Algeria. I tried to contact him in Paris but he was already in a senile state.

21. The interplay between the Brazilian school and the Polish school was in fact limited, for example Kotas/da Costa (1980) is rather a juxtaposition of valuation and matrix than a work of synthesis. The terminology is generally different, with some random similarities. Funny enough, Wójcicki used as a key word «logical calucli» in the title of his books rather than the tyical Polish expression «consequence operator» which shows up only timidly in the subtitle of his 1988's book. On the connection between da Costa and Suszko's results on bivalent semantics see Batens (1987).

3. From abstract logic to universal logic (Poland: 1992-93)

3. 1. More abstraction

I arrived in Wrocław, Poland, by the beginning of October 1992. This Silesian town was given back to Poland after second world war, after several centuries of German occupation, known then as Breslau. Anyway soon after the war it became one of the most important university centers of Poland in particular due to the fact that most of the city of Lvov, which itself became part of Soviet Union, was transported there.

Famous logicians started to work in Wrocław right after the war, J. Łoś (whose famous monograph (cf. Łoś 1949) on matrix theory which is the first extensive exposition of Lindenbaum's results about matrices was published by Wrocław University Press) and also R. Suszko, J. Słupecki, etc.

I was received there by the director of the department of logic, Jan Zygmunt, a man with a huge knoweldge of the Polish school of logic, keeping the old tradition of this school alive and who turned to be a very good guide for me.

Arriving in Wrocław I continued to develop the idea of abstract logic by presenting it and discussing it.

I wrote soon two «philosophical» papers in French about my conception of abstract logic: «De la logique formelle à la logique abstraite» (B 94a) and «La logique abstraite au sein de la mathématique moderne» (B 93d), this last one being a lecture presented at Lodz University that I was happy later on to see published in *Ruch Filozoficzny*, the journal founded by Twardowski in 1911 and where Łukasiewicz in 1920 presented his famous paper on many-valued logic.

Already in Brazil, I had found out that the Polish logician Roman Suszko had also used the expression «abstract logic» in « series of works carried out at the end of the sixties and the beginning of the seventies, together with two American mathematicians D. J. Brown and S. Bloom.[22] By an «abstract logic» he meant a consequence operator defined on an algebra. It was a slight generalization of the notion of *structural consequence operator*, notion which has been canonized by himslef and J. Łoś in their famous paper «Remarks on sentential logics» (Łoś/Suszko 1958).

The basic logical structure which Polish logicians are working with is indeed not really fixed. The fundamental point is to consider a logic as a consequence operator rather than as a set ot tautologies (keeping Tarski's original idea). Properties of this consequence operator may vary as well as the set on which it is defined. The standard approach is to consider rather a structural consequence operator than an abstract logic in the sense of Suszko. Moreover, even if it is not explicitly said, the replacement theorem is also generally required in addition to the theorem of substitution, in Polish terminology: a logic must not be only structural but also *self-extensional* (see Wójcicki 1988, p. 200).

In fact, as it is known, when Tarski first developed the theory of consequence operator at the end of the twenties (cf. Tarski 1928), he didn't specify the structure of the underlying set, taking such a set to be just a set of «meaningful sentences» in the sense of Lesniewski.

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[22]. The main results of these investigations are to be found in Brown/Suszko (1973) and Bloom/Brown (1973). Suszko was in a sense quite an isolate figure in Poland and his work on «abstract logic» has not been pursued there, neither in the USA, but it was recently revived by the BarcelonaDATE s logic group (see Font/Jansana 1996).

Without doubt the notion of consequence operator as developed by Tarski was inspired by *topology,* which was highly popular at this time in Poland (cf. Kuratowski with whom Tarski worked).

From the Bourbakian perspective, Tarski's original proposal falls into topology and Suszko's abstract logic appears as a «carrefour de structures» (algebraico-topologic), as well as Łoś-Suszko's notion of structural consequence operator. In all the cases, logical structures are a by-product of the three fundamental Bourbakian structures.

My proposal was clearly distinct, because I was considering logical structures as different from the already known structures and because by so doing I was defining them in a very abstract way, in particular without stating any axioms for the consequence relation.

One can find indeed examples of logic which are not structural such as the logic of P. Février[23] or not self-extensional (this is the case of the paraconsistent logic C_1). Moreover there is no good philosophical reasons to consider that the domain of a logic should be an algebra. The fact that logical operators are represented by functions is a mathematical representation that can be rejected: in natural language, there are sentences which are distinct negations of one given sentence, therefore negation appears rather as a relation than a function.[24]

As for the axioms for the consequence operator, what did Tarski when he developed the theory of consequence was to axiomatize the notion of logical consequence as defined by Hilbert's style notion of proof. For such a notion, Tarski's axioms hold. But when we generalize the notion of proof, this is not necessarily the case.

In fact in Polish logic there seems to have a confusion between proof-theoretical notions and concepts related to the theory of consequence operator. This happens mainly because proof-theory did not develop by its own in Poland but was incorporated within the theory of consequence operator.[25] People working outside of Poland inspired by the theory of consequence operator but substituting a consequence relation denoted by the Fregean symbol $\vdash$ for the consequence operator, usually denoted by *Cn*, have went worse into the confusion. These two concepts are in fact equivalent, but the confusion arises when people are mixing the concept of consequence relation together with Gentzen's sequent calculus as a general setting and employing the Fregean symbol as well for Gentzen's sequents, and using the same names for structural rules of sequent calculus and axioms for the consequence relation (reflexivity, monotony, cut).[26] The matter is even worse when one generalizes the consequence relation, keeping the Fregean symbol to denote it, to relation admitting sets of formulas on

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23. P. Février developed a three-valued logic in order to deal with Heisenberg's indeterminacy principle (see Février 1937). This logic has been rightly considered as «quasi-formal» by J.-L. Destouches (see Destouches 1948). Discussion about this can be found in (B 95 g).

24. For a discussion of these topics, see (B95b). In (B 96a) I proposed to consider the domain of a logic as any kind of structure, results which do not depend on this structure being properly abstract results.

25. More generally, the *metamathematics* of Hilbert was replaced by the *methodology of deductive science*, with different objectives and methods, in particular, by contrast with Hilbert, Tarski allowed himself to use any mathematical tools at the «meta» level.

26. As it is known, Gentzen originally used the arrow for sequents (under P. Hertz's influence). For this discussion see (B 99b).

both sides (the so-called multiple conclusion logic), which resembles even more to Genzten's concept of sequent. This is for example as D. Scott proceeds in (Scott 1974b).

This leads to a confusion between two notions of different natures: the notions of *rule* and of *law*. For example when someone calls *cut rule* the law of transitivity for the consequence relation, he is making a confusion which leads to a misundertsanding of the cut-elimination theorem which shows in fact that a transitive logic can be generated by a sequent system without the cut rule. Scott for example wrote that «In many formalizations a great deal of effort is expended to eliminate cut as a primitive rule; but it has to be proved as a derived rule. In general, cut is not eliminable.» (Scott 1974b, p. 414). But as it is known one cannot prove cut-elimination for LK by proving that cut is a derived rule of LK without the cut rule, simply because the cut rule is not a derived rule of this system. The cut rule in this system is a *permissible non derivable rule*. The cut-elimination theorem shows therefore that there are some permissible rules which are not derived rules.[27]

Discussing this with Zygmunt and trying to clear up all these confusions, I wrote a paper entitled «Rules, derived rules, permissible rules and the various types of systems of deduction» (B 99b) intended to be, among other things, a remake of D. Scott's paper «Rules and derived rules» (Scott 1974a). In this paper I tried to develop a kind of *abstract proof theory*, defining the central notions of proofs and rules at the abstract level, i. e. independently of the nature of the objects. This has some connections with Hertz's *Satzsysteme* (Hertz 1929), work which was the basic source of inspiration of Gentzen's sequent calculus and cut rule.

3. 2. Universal algebra and universal logic

I decided to change the name abstract logic for universal logic at the beginning of 1993. It was a consequence of learning more about *universal algebra* and its connection with Polish logic.

I soon realized that universal algebra was very popular among people working in Polish logic. Suszko himself considered «abstract logic» to be a part of universal algebra and it seems that it turned out to be a common idea in Poland, as suggests the following comment by S. L. Bloom: «Roman taught us the Polish view of logic — as a branch of universal algebra (a novel outlook for us)» (Bloom 1984, p. 313).

The connection in fact goes back to the golden years of the Polish school of logic when Tarski and Lindenbaum transformed the notion of matrix, introduced originally by Łukasiewicz for many-valued logic, into a central tool for a general theory of zero-order logics (i. e. sentential logics). By thus doing they were developing universal algebra independently of Birkhoff. We must also recall that logic was first introduced in Poland via *algebra of logic*, as Wolenski notes (Wolenski 1989, p. 82).

Birkhoff developed his notion of universal algebra to unify two disjoint approaches: Noether's school with groups and rings on one side and algebra of logic and lattice theory on the other side, as well explained in (Birkhoff 1976)[28].

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[27]. It seems that it is also a confusion between permissible and derived rules that Łukasiewicz made in his odd paper about intuitionistic logic (Łukasiewicz 1952), as pointed out by Legris/Molina (200?).

[28]. (Birkhoff 1987), Birkhoff explains that he took the expression «universal algebra» from Whitehead (1898) but recalls that the creator of this expression is J. J. Sylvester; Corry (1996) erroneously states that it is Whitehead. Birkhoff also says that it is in (Birkhoff 1940), his famous *Lattice theory*, that he decided to use this expression to denote a general study of algebras. The first systematic exposition

No doubt that there is a strong connection between logic and universal algebra. Algebra of logic is one fundamental root of *abstract* algebra, because Boole was the first to deal with algebras whose objects are not quantity, and of *universal* algebra because the laws for logical operators such as involution are totally different from the laws for numbers; one therefore can understand why Birkhoff's unification was not possible by stating some «universal laws» which would hold for all algebras. As explained by Scott (1974b), Tarski developed *model theory* via the kind of universal algebra which has emerged in Poland as a general metatheory for zero-order logics, which transformed itself in Poland after the war into the *mathematics of metamathematics* (cf. Rasiowa/Sikorski 1963). Later on universal algebra and model theory were applied back to the general theory of zero-order logics leading to *algebraic logic*.[29]

Despite all these relations between logic and algebra, I think that to consider a general theory of logics as part of universal algebra is wrong. In fact many people who are doing that are confusing universal algebra with the general theory of structures. Polish general approach to zero-order logic is highly mathematized comparatively to a standard Western approach according to which zero-order logic is presented in a rather linguistic informal way. But to make an extensive use of mathematical tools for the study of logic does not necessarily mean algebraization. It is true that algebraic tools are important but they are not the only ones. Moreover, if a wide class of logical structures can be reduced to algebraic structures via factorization, it is not the case of all logical stuctures, in particular those in which there are no non trivial congruence relations (*simple logics*), like what happens with the logic C_1, as shown by Mortensen (1980). In my paper «Logic may be simple» (B 97h) I discuss all this in details and argue that there are no good reasons to reject such simple logics out of the sphere of logic.

As Suszko's notion of abstract algebra was understood as part of universal algebra and as this expression was therefore already used with a different meaning, I thought bettter to shift the terminology and the expression *universal logic* seems to me perfectly appropriate. Universal logic stands in the same position with regards to the multiplicity of logics as universal algebra with the multiplicity of algebras. Moreover, as my original idea of a naked logical structure was inspired by Birkhoff's definition of algebraic structure, I thought a good idea to use a similar terminology in logic as the one promoted by Birkhoff in algebra, who is «universally» recognized as the father of modern universal algebra.

The terminology «universal logic» shows clearly that universal logic is different from universal algebra (and in particular not part of it), but at the same time shows also the spiritual connection.

I think that the independency of universal logic with regards to universal algebra is much of the spirit of the Polish school of logic itself whose success was borne out the consideration of logic as an autonomous field as recalled by Wolenski and Zygmunt: «the logicians of the Warsaw school always emphasized the autonomy of logic as a discipline and

of the subject was (Birkhoff 1946) whose title is simply «universal algebra».

29. Czelakowski (1980), Blok/Pigozzi (1989) and Font/Jansana (1996). The road leading from the algebra of logic to algebraic logic is an interesting object of study for the historian of modern logic which has yet to be fully examined. Curry stands in the middle of the road, he was the first to use the expression «algebraic logic» in (Curry 1952) and not Halmos as erroneously stated in (Blok/Pigozzi 1991, p. 365), but what he meant by it was still close to algebra of logic. Halmos introduced this expression rather to denote the algebraic treatement of first-order logic, but nowadays the expression «algebraic logic» is used to include both the zero and the first-order levels.

this ideology, regardless of its justification, was one of the pillars of the success of the Warsaw school of logic» (Wolenski/Zygmunt 1989, p. 403).

4. Universal Logic onwards (Around the world 1994-99)

4. 1. A lecture, a paper and a PhD

Back from Poland by the end of 1993, I stayed a couple of months in Paris where I developed some philosophical ideas related to universal logic in a short monograph entitled «universal semantics» (B 98c). In june 1994 I went to Czech Republic to present a lecture entitled «Universal Logic» at an international conference on logic at the Liblice castle. The reception was good and I wrote the full version back to São Paulo in august, improved by some comments of R. Sylvan who was visiting Brazil at that time. It was subsequently published in the proceedings of the conference (B 94b).

The paper contains in a first part, a full description of what I mean by universal logic, including reference to the Bourbakian architecture of mathematics and a short story of universal algebra,[30] illustrated, in a second part, by the example of an improved abstract form of the completeness theorem I was able to present, just having found an important result working on my PhD.

This result connects rules of sequent calculus with conditions for bivaluations in such a way that it is possible to instantaneously derive from it various completeness theorems. This result is purely abstract and does not depend neither on self-extensionality nor on truth-functionality. I achieved this result by putting together da Costa's theory of valuation, Lindenbaum-Asser's abstract form of Lindenbaum's extension lemma (Asser 1959) and abstract sequent calculus. The hint of my theorem was given to me by the study of Gentzen's first paper dealing with Hertz's *Satzsysteme* (Gentzen 1932). This theorem arrived at the right time in order to link works that I was putting together to form my math PhD, which I decided to entitle *Recherches sur la logique universelle* (*Excessivité, négation, séquents*) (B 95e).

Excessivity was the word that I had decided to use instead of «saturation» or «maximal relativization», because I thought «saturation» improper due to the fact that this terminology was already used in model theory with a different meaning and «maximal relativization» was much too lengthy. Moreover I found appropriate to introduce a virgin name to denote a concept that my researches had revealed fundamental. In particular the above central result depends on the fact that excessive sets respect rules of a certain class of systems of sequents. To specify this class and also for a general version of the cut-elimination theorem given there, I presented a deconstructional analysis of the sequent calculus.[31] These general results are applied to the paraconsistent logic C_1+. In this work I therefore follow the Bourbakian motto «from the general to the particular», in an inverse route that led me from paraconsistent logic to universal logic. My *study of negation* does not limit to paraconsistent logic but extends to a reformulation of Curry's theory of negation (Curry 1952). I was able to prove an interesting result showing that intuitionistic negation collapses into classical negation if we slightly modify the morphological feature of negation by admitting not only positive negations but also

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30. I was therefore jointly presenting two different tendencies, Bourbaki and universal algebra, which historically, for some odd reasons, have been conflicting.

31. I prefer the terminology «sequent system» than «sequent calculus», because a sequent calculus is not necessarily a calculus, in the algorithmic sense, if it is undecidable. More generally, I think that the word «calculus» in logic is inappropriate. It suggests that logic'algorithm, a thesis dismissed by the fall of Hilbert's program.

negative ones. I was led to this result by observing that the two forms of the *reductio ad absurdum* are quite the same (one increases the number of negations, the other one decreases it) and that therefore there were no good reasons that they should induce two different negations (this part of my PhD has been published as B 94d).

4. 2. Los Angeles

After finishing to write my PhD and send it to Paris, I stayed a while in Brazil where I concluded a translation of one book of da Costa in French including a preface and two appendices written by myself (one about paraconsistent logic (B 97a), the other about the theory of valuation (B 97b)).

At the beginning of 1995, I left São Paulo from Paris and then flew to Los Angeles where I was to stay a couple of months at UCLA. I attended lectures and seminars there both at the philosophy and mathematics departments but I was surprised to see so few logic, I mean logic for its own sake. Y. Moschovakis righlty described me the situation by a joke saying that there was no logic at UCLA, but on one side philosophy of language on the other side a lot of set theory.

Anyway I presented a talk at the math department «Universal logic: some results in abstract completeness.» A Polish logician, emigrated to the US, told me that of course he had heard about the theory of consequence operator when he was in Poland, but that he rejected it due to the fact that it fails to capture non-monotonic logics. I think that this rejection is common nowadays due to the success of these logics. However I don't think that it is a definitive argument against Polish logic. Most of the results of consequence operator theory can be in fact adapted to the non-monotonic case and Wójcicki wrote a paper apparentlty just to prove that (Wójcicki 200?). Non-monotonic logics just show, in my opinion, that Polish logic must be widened into a true universal logic.[32]

I wanted also to present a talk at the philosophy department on the comparison between category theory and set theory as foundations of mathematics which would include comments on Bourbaki and universal algebra. But D. A. Martin told me that it would be a mess because on one hand people of the philosophy department would not understand the talk due to their very poor knowledge of this matter and on the other hand only «big names» were able to attract people in a lecture at this department. I realized therefore that analytic philosophy was not so much different than continental philosophy in the sense that in both cases the man is more important that the stuff he is speaking about. The argumentation of the analytic philosopher is not enough rigorous to have a value by its own, independently of who expresses it, as it may happen in mathematics. I realized also that analytic philosophers were using terms from logic without knowing their exact technical meanings, and that therefore they were speaking rather metaphorically, in a way not so much different to Lacan, Deleuze or Derrida.

I left L. A. at beginning of july 1995 at the time when the airport was under threat by the Unabomber and arrived in Paris to defend my math PhD. I left Paris after escaping for short of the bomb who killed many people in the RER subway at *Saint-Michel*.

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[32]. Apart of Wójcicki's work on non-monotonic logics, there are some works by G. Malinowski where the axioms for the consequence operator are weakened (Malinowski 1990).

4. 3. The world of possible logics

Back to Brazil, I worked on two papers «What is many-valued logic?» (B 97f) and «What is paraconsistent logic?» (B 99e)[33] which are in a spirit of a project I had with da Costa to write a book entitled *The world of possible logics*[34] in which the most famous non-classical logics would be presented from the perspective of a general framework. The idea is really to use this kind of perspective to clear out the many confusions related to each given logic.

For example, people generally think that intuitionistic or modal logic are not many-valued but they are not really able to sustain their assertion, to turn explicit the matrix backround of it. Even less are they able to explain, if by chance they know it, the challenging result according to which it is possible to give a bivalent semantics to most logics including Łukasiewicz's logic L_3 (see Suszko 1975).

As regard to paraconsistent logic, generally people just give a negative definition of it, the exclusion of the *ex-falso sequitur quodlibet*. But such a negative definition is meaningless as long as it is not complemented by a positive one. However there is not only one possible answer because several positive criteria may be incompatible together. I think that we can make a good job in this direction only if we have a general framework which allows us to compare rightly the various logical and metalogical properties. Working on this direction, I was able to show that there are no De Morgan full paraconsistent negations which are self-extensional (B 98b).

In a dialectical interplay, I worked on general problems and particular logics, and developed further paraconsistent logics (B 97g) including a self-extensional one (B 00a). I also used da Costa's theory of valuation to study connectives which are *between conjunction and disjunction* (B 98i). This is related to some problems in Biology on which I was working with M. V. Kritz at the LNCC. I think that nowadays logic is more and more connected with all the fields of knowledge[35] and that universal tools will help us to built *the right logic for the right situation.*

Another interesting question which links clearly abstract questions of universal logic with concrete cases is the question of *translations* between logics. As it is known classical logic can be translated into intuitonistic logic which at the same time is included in classical logic. How to explain this paradox? What is the exact status of «translations» between logics? Are they embedding? Working with an example of a logic even simpler that intutionistic logic in which classical logic can be translated,[36] I showed that the question was not simple and was involving different notions such as the concept of identity between mathematical structures

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[33]. These two papers were presented respectively at the 27th International Symposium on Multiple-Valued Logic (Antigonish, Canada, May,1997) and at the First World Congress on Paraconsistency (Ghent, Belgium, July 1997). My researches on many-valued logic started with a discussion with da Costa and O. A. Bueno (B 96c) about (Malinowski 1993).

[34]. This nice title was suggested to us by Michel Paty.

[35]. N. C. A. da Costa since more than ten years has started to work on the connections between logic and physics, logic and biology, logic and economy, etc. (see da Costa 1997).

[36]. I found this logic by studying the paracomplete dual of C_1 and mentioned it in my math PhD (B 95e).

in general and the difference between real and nominal defintions (it is da Costa, once again, who introduced me to the subtility of the theory of definitions, see (B 98f)).

I also started to develop the philosophical side of my universal logic's project which shows in particular that the formalist approach cannot any longer seriously be sustained; see (B 98e), (B 99f).

5. Universal logic in perspective

5. 1. A theory of our time

Universal logic corresponds to the spirit of our time. The number of new logics has increased these last years due to the need of computer sciences, artificial intelligence, cognition, and all the stuff of our cybertime. There is a need for systematization in order to put an unifying order in the chaos of the multiplicity.

Several books and papers have been recently written in this spirit presenting various methodologies and technical tools. Gabbay edited a book (Gabbay 1994) which collects a series of essays which are supposed to answer the question *What is a logical system?*

In his Mathematical Review of it [96k: 03008], Walter Carnielli rightly points out that the book misses a central question, the question of translations between logic. I think he is perfectly right. We must unify the «inconsistent multiplicity» of logics, to use Cantor's expression, in a Category of logics, and study the morphims between logics, of which translations are particular cases. This is certainly a key point for a general theory of logics.[37]

Another trouble with the book is the formulation of the question. The shorter question «What is a logic?» would be better. The expression «logical system» tends to focus on logics considered as proof-theoretical formal systems. It is much out of date and too narrow a view for full abstraction, as shown clearly by Barwise and Hammer's paper (Barwise/Hammer 1994) dealing with diagrams, an old visual approach to logic, which was considered in the past heuristical at best, but which has been taken seriously recently.[38]

Despite of this, the proof-theoretical tendency is quite strong nowadays, in particular due to the crucial role of sequent calculus in linear logic, and in substructural logics in general. Some people are mixing this framework with the consequence operator's one and this is generating some confusions in the same line has those found in Scott years ago.[39]

What is a substructural logic? One can say that it is a sequent system lacking some structural rules or whose sequents have not the same structure as the classical ones (cf. the intuitionistic case). Very good, very clear. But we must distinguish this system from the logic,

[37]. The translation problem was not eschewed by the Polish school, people such as Wójcicki worked on it and Suszko and his collaborators were probably the first to work on a «category of logics». In Brazil, the logic group of Campinas has few years ago taken this subject as a main subject research (see Carnielli/D'Ottaviano 1997).

[38]. In order to get an intuitive idea about the paraconsistent negation of C_1 I worked with diagrams (B 98d).

[39]. confusions have proliferated recently, see e. g. (Dunn/Hardegree 200?). The expression «substructural» was put forward by people working within a proof-theoretical framework with probably very few knowledge of Polish logic in which the expression «structural» is used since many years with a totally different meaning. On the other hand, Gentzen's work was not well-known in Poland and people were no aware that Genzten already used the expression «structural rule» in a different context.

the consequence relation, it induces. This system can lack weakening rules, and the consequence relation can be monotonic (in fact monotony always holds for a consequene relation induced by a system of sequent, due to the very defintion of «induced», which however can be modified). So what is a non-monotonic logic? Is a non-monotonic logic substructural or not? Is linear logic non-monotonic?

Gabbay in his own approach[40] to the subject does not avoid the confusions. For him, at first, a logical system is a consequence relation together with a proof system generating it, he says that he is compelled to this second part due to «the central role which proof theoretical methodologies play in generating logics» (Gabbay 1996, p. 3). Gabbay takes a second step by dropping monotony for the consequence relation and considering instead of a simple proof system, what he calls a LDS proof system (Gabbay 1996 p. 11).

No doubt that *Labelled Deductive Systems* is a nice technique which has innumerable applications, as shown by Gabbay in his book. However it is not simple enough to be as general as one may need. Firstly, why dropping just monotony? Secondly this proof-theoretical approach has some drawbacks. For example the complement of the underlying consequence relation of a logical system in the Gabbay's sense, cannot always be considered as the underlying consequence relation of a logical system (i. e. the case of first-order logic). Mathematically speaking, this class of logical systems is not closed under complementation. It is also not closed for a lot of other operations on a class of structures.

The proof-theoretical approach is limited and there are no good reasons to give priority to it. One may prefer a *semantical approach*. This is the case of Epstein (1990)[41].

Although Epstein and Gabbay's approaches are based on two different methodologies, their works bear the same defects. On the one hand some general machinery is introduced with few important significative abstract results, on the other hand they present a huge quantities of examples to which their general methodologies apply more or less happily. Moreover working only on one side of the logical business, completeness is not a central question, which seems rather odd.

In view of these works we can say that the present state of research in the systematization of logic is much the same as the pre-Birkhoff period of universal algebra, well-illustrated by the «monstrous» book of Whitehead, which collects a lot of things together, without a serious methodology and without important results; as noted by Grätzer, Whitehead «had no results», though he «recognized the need for universal algebra.» (Grätzer 1979, p. vi).

If we want to go further on, I think we must follow the method that has always shown to be right in the history of mathematics: we must jump into abstraction. We must stop for a while to be preoccupied by such or such logic and work the abstraction for its own sake. This is what Birkhoff did with universal algebra and this is what must be done in logic in order to develop a real universal logic.

The general theory of logics which is emerging is of course in some sense contrasting with the traditional line of reserach in logic that can be «labelled» by the expression

[40]. Presented as one chapter in (Gabbay 1994) and fully developed in (Gabbay 1996).

[41]. A general abstract semantical approach can already be found in (van Fraasen 1973). A less abstract semantical approach is related to «Abstract model theory» which includes such results as LindströmDATE s theorem, see e. g. (Barwise 1974).

foundations of mathematics. One can say that the new trend is about *foundations of logic*[42]. Gabbay presents these two tendencies as opposed:

> Unfortunately, the traditional logic community are still very conservative in the sense that they have not even accepted non-monotonic reasoning systems as logics yet. They believe that all this excitement is transient, temporarily generated by computer science and that it will fizzle out sooner or later. They believe that we will soon be back to the old research problems, such as how many non-isomorphic models does a theory have in some inaccessible cardinal or what is the ordinal of yet another subsystem of analysis. I think this is fine for mathematical logic but nor for the logic of human reasoning. (Gabbay 1996, pp. 3-4).

But is mathematical logic not the logic of human reasoning? Maybe reasoning about transfinite is beyond computers minds but Cantor's paradise has been investigated by human brains. Anyway, I don't think that the gap between foundations of mathematics and the general theory of logics is so big. There are some connections as shown by the equivalence between the abstract formulation of Lindenbaum's extension lemma and the axiom of choice (see Dzik 1981). Even if in Poland this duality has increased after the war, people like Łoś and Suszko have made important contributions on both sides, pursuing the way of their masters, and in particular Tarski, who considered logic as a whole.

5. 2. Universal logic and philosophical logic

Nowadays the gap between mathematical logic and philosophical logic is striking. *Philosophical logic* (an ambiguous expression) itslelf is divided in two parts. On the one hand it is the study of non-classical logics such as relevant logic, modal logic, etc. If at the beginning these logics were motivated by philosophical preoccupations (hence the name), nowadays most of the works are of purely technical nature without even a pinch of philosophy. The adjective «philosophical» is in fact used here sometimes negatively, to qualify these works, by logicians working in «hard» foundations of mathematics and who are looking for mathematical recognition and don't want their work to be confused with something they regard as easy meaningless games for philosophers. But if it is true that these games exist, work in e. g. relevant logic, even at the propositional level, can be as difficult, mathematically speaking, as «hard» foundations.

On the other hand there is *philosophy of logic*, which has concentrated mainly on questions of reference (related to the famous «denotational» works of Frege and Russell), and which has turned in fact into *philosophy of language* where technical terms are used only metaphorically, the technical knowledge of these philosophers being in general reduced to truth-tables. And this may generate confusion.

I will give just an example. A lot of «philosophical logicians» are speaking about *intensionality* and it is standard to say that modal logics are intensional. But how can someone claim that a self-extensional logic is intensional? Explanation: on the one hand there are some people who are doing technical work on modal logic and know that these logics are *self-extensional* but they don't know exactly what is the problem of intensionality and use this name to conform to the usually way of speaking, on the other hand there are some people who

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[42]. Curry was already using this expression: he wrote a book entitled *Foundations of mathematical logic* (Curry 1963), which was a kind of augmented version of (Curry 1952) which, as we have seen, also bears a prophetic title.

know «Sinn und Bedeutung» and «On denoting» by heart but are not aware that current modal logics are self-extensional and what this means.[43]

One (maybe the original) reason why modal logics are called «intensional» is because the modal operators are *not truth-functional*. But if non truth-functionality may be considered as a necessary condition for intensionality, it is not at all a sufficient condition, as show indeed modal logics which are self-extensional: self-extensionality clearly conflicts with intensionality as the name rightly points out.[44]

What is needed for good philosophical discussions about logic is clear definitions of the central features of logic. Therefore we can see why universal logic can be useful if not indispensable. The definitions philosophers need involve *mathematical abstract conceptualization* rather than symbolic formalisation. This is what they are maybe not aware of after logicism and formalism which gave a deformed vision of mathematics, according to which mathematics is a game which consists mainly of the manipulation of strings of signs following specific rules.

To understand truth-functionality, one must learn matrix theory, to understand such result as Gödel's result showing that intuitionistic logic cannot be characterized by a finite matrix, the reason why intuitonistic logic is said to be non truth-functional. To understand self-extensionality, one must know what is a congruence relation. Someone who doesn't understand these notions cannot seriously speak about the intensionality/extensionality problem.

Universal logic can give a new direction to the philosophy of logic, because it provides via modern mathematics, rigour and abstraction, without which philosophy of logic is only metaphorical discussion, bad poetry in the sense of Carnap.[45]

5. 3. Paraconsistency and universal logic: a final word

G. Priest thinks that paraconsistent logic is the most important event in logic in the XXth century because it is kicking out a principle which was taken as the basis of reasoning during more than two thousands years. He uses the word *transconsisten*t (Priest 1987) by comparison with the tranfinite's phenomenon (funny enough paraconsistent logic has been used also to defend a finitist point of view, see e. g. (van Bendegem 1993)).

In some sense he is right, the philosophical import of paraconsistent logic cannot be ignored, but I don't think that paraconsistent logic is the new paradigm.[46] What we know nowadays, after paraconsistent logic, is that logic is not founded on the principle of contradiction, that logic is still logic without this principle. In this sense logic is truly transconsistent. Paraconsistent logic has clearly shown that triviality is more fundamental than

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[43]. On this question see (B 93f).

[44]. Unfortunately this name is not very used outside Poland.

[45]. Suszko liked to say that «abstract mathematics can be a genuine philosophy». His ideas about philosophy of logic are similar to the one defended here, cf. (B 00e). In this paper we show how the mathematical concepts developed by the Polish school of logic can be a basis for a new approach in philosophy of logic, less formal or symbolic in style, but conceptually more mathematical.

[46]. Nor a blend of paraconsitency and relevancy, or any other system which will play the role of a «universal» system; see (B 99d).

consistency, as da Costa (1958) already strongly emphasized, and has thus led us to more abstraction.

In the work of Vasiliev[47], considered with Łukasiewicz as the main forunner of paraconsistent logic, we can find also some bright ideas, although his work is not technical in nature. Vasiliev argued that the principle of contradiction is *empirical*, that it is not a real fundamental formal principles of logic. He said that his *Imaginary Logic* which is a logic without the principle of contradiction just showed this, that this principle is accidental, independent (in the same way that Lobatchevski had shown with its Imaginary Geometry that Euclide's parallel postulate is). What Vasiliev said is that logic is grounded as a deeper level, which he called metalogic.[48]

Łukasiewicz himself started his investigations which would lead to matrix theory and the general study of zero-order logics by accurate criticisms to Aristotle's defense of the principle of contradiction (Łukasiewicz 1910).

All this shows that paraconsistent logic has played a fundamental role towards universal logic, by dismissing the last and the more sacred principles of logic, showing that logic is grounded at a more abstract level, where no principles hold.

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47. On Vasiliev see (Bazhanov 1990) and (Arruda 1990).

48. One could think that it will be a good idea to call «metalogic» what we have called «universal logic», but on the one hand the suffix «meta» has different meanings and has been already used in such expression as «metaphysics» and «metamathematics» with a meaning not corresponding to our intention, on the other hand the expression «metalogic» is already used and has already been used in various different ways. In fact one can find it, even before Vasiliev, but with a similar meaning in Schopenhauer. On this question see (B 92) and (B 93b).

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