Transcendental syntax 2.0

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How come that finite language can produce certainty — at least a sufficiently certain certainty, sometimes *apodictic* — in the presence of infinity? The answer can by no means be found in external reality, quite the contrary: it seems that the very purpose of *semantics* is to make this question untractable with the help of an *ad hoc* analytical *newspeak* in which one cannot even formulate the above question. Transcendental syntax comes from the constatation that logic is better off when there is no « reality » at all and thus restores the priority of syntax over anything else. What follows is the present state of a new programme.

1 The conditions of possibility of language

This first lecture is rather philosophical, too much indeed: I didn't find a way to distillate the philosophical issues in the more technical chapters 2 — 4. For those allergic to philosphy, I swear that a change of philosophical background is absolutely *necessary* to achieve logical maturity, including — indeed, especially — at *technical* maturity.

1.1 Introduction: philosophy

I just spent three months in Kyoto, at RIMS; the path to the University is known as *Testugaku no michi*, « The philosopher's path », after a famous japanese philosopher from the Meiji Era, Nishida Kitaro. This may explain that...

(i) Traditionnally, logic sits in between mathematics and philosophy. The present situation is schizophrenic:

- On one side philosophers illustrating their *deep* ideas with junk logics.
- On the other side mathematicians solving *shallow*, but difficult technical questions.

Like in any civil war, each side takes advantage of the errors of the other side; in particular, « technicians » don't care about philosophy: they are « aphilosophical », like some are apolitical. Which amounts at following — since there must be such a thing — the dominant approach to logic,

fregeism, the so-called *analytical* philosophy, which is the philosophy of those who don't care about philosophy.

(ii) Philosophy has an extreme influence on science, but this should be correctly understood, since it has strictly no technical contents; in particular, the idea of building logic from vague logical speculations — the aforementioned « deep ideas » — always leads to ludicrous developments, witness epistemic « logic » if you don't know what I mean! Notwithstanding its technical impotency, philosophy can assume a central *architectural* role. A bit like the role played by category-theory in mathematics: there are no real theorems about categories, moreover, diagrams suck! But categories provide us with a *structuration* of mathematical objects. I personally realised very early the importance of functors, direct limits, pull-backs: they were the source of coherent spaces, even if I never heard of monoidal categories before. In the same way we don't quite need diagrams, we don't need the *categorical imperative* to benefit from Kant and free us from the analytical (fregean) *aporia*.

1.2 Semantics

The (implicit) legacy of Frege, i.e., the philosophy of those who don't care about philosophy, can be summarised as follows:

(i) The language is about something *else*, the *denotation* (semantics); everything is in the « else ».

(ii) In particular the *subject* must be expelled[1].

(iii) If there is no reality — no semantics foundations — *everything is permitted*.

The argument for this is pure scientism: science should be « objective ». Let us observe that, outside logic:

(i) Objectivity has always been the primary concern of sorcerers: since it would be subjectivism to predict from reasoning, they seek « objective » signs in a deck of cards, in the bowels of a bird, etc.

(ii) Ancient astronomy expelled the subject and concentrated on the « objective » movement of planets, their denotations so to speak; the refusal to take the observer (the Earth) into account induced Ptolemy into the monstrous metastasis of *epicycles*[2].

(iii) Modern (e.g., quantum) physics dwells in a non-realistic world, in which physical variables have no *preset* denotations.

As to logic, let us cast an *a priori* doubt as to the relevance of this objectivism: logic is about language, which involves a *subject*, not about *objects* like physics; physics gave up objectivism, while logic sticks to this archaic conception!

[1]Whne something doesn't fit the objectivistic pattern, it is styled *intentional*, with the meaning of « bullshit ».

[2]Copernic was the first one to depart from this objectivism; this is why Kant styled his approach *copernician*.

(i) Aristotle observed long ago that the correction of a syllogism has little to do with its factual truth. He would, occasionally, use a semantic argument for a *refutation*, but never for a *justification*. The fact is that semantics — whatever quality they may have — are all *incomplete*: they fail to catch the specificity of language. We can indeed see « the » reality as a way of quotienting the language; there are various styles of quotients, of variable quality; this variability may even be linked to the freshness of the interpretation. In my book [9, 11] I distinguished three semantic layers, or *infernos* (= inferior):

Truth: layer -1 is that of usual *models*. Can be a valuable idea in algebra (model-theory), but strictly empty from the foundational viewpoint: explaining *Barbara* by the transitivity of inclusion is at least, circular. The Tarpeian Rock of this approach is *incompleteness*: as soon as we deal with serious matters (natural numbers), there is a shortage of models, basically a good one (styled « standard » for this reason) and monsters, e.g., $\mathbf{PA} + \neg\ \mathbf{Con(PA)}$ — a theory known to be consistent by Gödel's theorem — quotiented into a *non-standard model*... indeed a non-model, a « thing » of no interest[3]. Layer -1 provides an *external* explanation of logic, a schizophrenia between object (= model) and suject (= proof), in which « objectivism » has been pushed to its extreme: the identification of all proofs, considered as irrelevant subjectivistic encodings of the denotation « true ». But what remains of logic if we start with denying any status to proofs?

Functions: layer -2 is the realm of *categories*. The quotient makes sure that not all proofs are identified; *Barbara* becomes the composition of morphisms, what is highly non-trivial (associativity of composition forces some quotienting, hence Church-Rosser). However the diagrams (less pedantically, the functional equations) do not reflect the language: one side is more commutative (equal) than the other. However, the quality of this layer is to put together the formula and the proof, the object and the subject, as respectively object and morphism, in a unified setting.

Interaction: layer -3 deals with the dynamics which makes one side more commutative than the other. Here, the quotient is used to produce a compositional alternation of moves in *game semantics*. Its possibility rests upon properties of the language, typically of the cut-elimination process. However, something still escapes us, namely the *rule of the game*.

Semantics is useful yes, seldom as a technical tool, more often as beginner's help; semantics is the side wheels of logic. What helps you when you begin soon becomes a burden: in the same way one gets rid of side wheels to able to bike efficiently, one should get rid of these realistic prostheses.

(ii) The logical tropism is to build the language as the mirror of a fantasmatic reality and to erase all « irrelevant » information. The problem is that

[3]The analogy between incompleteness and non-euclidian geometry is a stupidity: there are perfectly good and concrete non-euclidian spaces, while nobody ever saw the tail of a non-standard models.

semantics may consider as irrelevant some artifacts of extreme syntactical importance. Typically, thirty years ago, people *formalising* data bases would only list the semantically relevant data (in contrast to the computer that would encode syntactic *garbage*, like begin/end of file). As a result, the associated formal systems would have trouble in deducing anything and be quite unable to deduce the slightest negative satement. In other terms, when modelising computation, they were referring to a fantasmatic computer free from all « useless » information. Common sense tells us that the sign of end of list is essential (think of web browsing); without this sign, we could not use internet. If our formalisation of internet dumps it, then it becomes extremely clumsy, nay faulty if we extend to an infinite domain: by the classic results of the thirties, there is no way to know that one cannot know... this is whence non-monotonic « logics » were created: a terrifying logical epicycle whose only justification was to avoid subjectivism, i.e., the recognition that a data base is suject-oriented. In the same style, this proposal to handle cognitive statements « I don't know » by... truth values[4].

(iii) If there is no God, everything is permitted, would say Dostoievsky; but this adamant God justifies the misdemeanours of religious fanatics: they created God at their image[5]. In the same way, the experience of philosophical logic is that of *ad hoc* realities, monkeying the syntax, typically Kripke models: semantics as *prejudice*, so to speak. One interprets the language (witten in italics) by the same thing (written in boldface), thus making (almost) no quotient at all. These quotients are, contrarily to the ones at work at layers -2 or -3, of poor quality, since they do not rely on any property (e.g., Church-Rosser) of the language; this poor quality is precisely what make them so precious for the adepts of the « publish or perish ».

This calls for an *autonomous* approach to syntax.

1.3 Deontics

Warning: « deontic » is the best expression I could find, better than the derivatives of « normative », with their negative conntations. However it suffers from the existence of various *deontic* logics, sort of bloodless red tape. What follows has nothing to do with deontic « logics ».

The fregean opposition between sense and denotation is philosophically extremely naive and part of the totalitarian regression of thought, possibly linked with the abominable political opinions of Frege. Indeed, the idea that the reality (the denotation) is prior to the language (the sense) forgets one detail: like in Voltaire's joke, reality is indeed a creation of the language, for the technical reasons just expounded, but also from a common sense remark: the language *formats* the reality. Frege's description of logic reminds me of this explanation of vision of my childhood: the landscape is projected on the retina of an ox... OK, so what? It still remains to understand how this retinian image is analysed.

[4]Truth values to a classical preformatting; alternative truth values thus can only produce bastard variants of classical logic. What would you say of alternative vehicles based on alternative horse-shoes?

[5]Voltaire: *Dieu a crée l'homme, mais celui-ci le lui a bien rendu.*

The question of formatting is absolutely essential. For instance, take Mr. W., supposed seller of decorations (Légion d'Honneur); justice can handle him *semantically*: guilty or not (layer -1), accomplices, price (layer -2), procedure of attribution (layer -3). But there is another way to handle the case by deciding that, as a close friend of the President, everything related to him is classified: this is the *deontic* approach: you cannot judge W.

In the same spirit, Orwell's *newspeak* is a preformated language in which some questions cannot be formulated; communists were very good at *newspeak*, e.g., « anticommunisme primaire », an expression implying that *any* criticism of communism is a sign of superficiality. The expression has been recycled into « antisarkozysme primaire »; maybe my approach will be styled « antifrégéïsme primaire ».

The deontic formatting is — whether one likes it or not — absolutely necessary; for instance, in order to avoid Richard's paradox[6], one must format, i.e., restrict the language. This formatting makes evaluation, i.e., semantics, possible. The denegation of deontics has a name, *essentialism*: essentialism claims that there is nothing like formatting, that everything is given in advance etc. Fregeism is an essentialist approach to logic, in which everything proceeds from the sky; in other terms, there is no justification to give for the rules of logic, they proceed from God (a.k.a. Reality) and any attempt at discussing them receives the same answer: « classified ». This arrogance is partly justified: as long as one deals with classical mathematics, one can perfectly accept everything without discussion, i.e., evacuate logic. But, *inside* logic, it's a cat of another colour: accepting the rules (especially non-classical ones) on the basis of God's revelation (semantics) is — to say the least — dishonest.

Essentialism (the hidden format) is at work at all semantic layers:

-1: Supposes that all questions receive an answer Y/N; how do we deal with $l = 2a + 1$ (l length of the ship, a age of the captain) without format?

-2: Categories are strongly essentialist; etymologically, « morphism » refers to the format. To the point that categories are unable to handle polymorphism, e.g., subtyping, records.

-3: The rule of the game; what is this third partner, this hidden « referee »?

In order to handle the referee, it necessary to introduce another layer, the *deontic* layer -4. The origin of this layer is to be found in multiplicative *proof-nets*, but one can find prefigurations in:

Prawitz: observed in the 1960s the symmetry between introductions and eliminations. We could say that introductions are those rules who « match » the eliminations (and *vice-versa*) w.r.t. normalisation. Due to the overformatting of natural deduction, this remark remains but a potentiality.

Herbrand: usually considered as a poor relative of Gentzen's *Hauptsatz*, the theorem is only concerned with classical quantification, for which it provides a semantic-free interpretation. People are usually puzzled by the fact that, in Herbrand's theorem, the universals depend upon the previous existentials, hence the side-wheel explanation about the refutation of

[6] The smallest number not definable in less than 12 words.

a refutation. Indeed, if in $\exists x \forall y$, we write $y = f(x)$, this is just to ensure that, when we find (through unification), a value $x = t$, then t does not depend upon y. This reversal of intuition is at work in proof-nets, in which conjunctions are handled like disjunctions and *vice-versa*.

Layer -4 yields, through various settings (GoI, ludics) a satisfactory explanation of the full evaluation process: the formatting (-4), the interactive evaluation (-3) enjoying compositionality (-2) and consistency (-1).

Prawitz's symmetry corresponds to negation (exchange left/right, hypothesis/conclusion), and really makes sense in a symmetric format like like linear logic. In that case, negation *is* the format. In other terms, to negate A is not to *refute* it, but to *recuse* it: instead of the hanging judge, the bleaching prosecutor. This remark is essential to vindicate one of the greatest philosophers, Hegel, and his *contradictory foundations*. From a fregean viewpoint, founding A on its negation is stupid, since A and $\neg A$ cannot coexist by consistency! The fact is that GoI, ludics, rest upon contradictory foundations without the slightest problem. The mistake about Hegel is to take negation in the *alethic*[7] (i.e., -1) sense, for which there cannot be any honest contradictory foundations; there are dishonest ones, e.g., paraconsistent « logic », making Hegel a sibling of Bernard Madoff… a real insult for such a great philosopher!

The point about layer -4 is that it is a dialectics between tests; on one hand, we have tests « for » (i.e., against) A: they try to deny the fact that something is a proof of A, on the the other hand, tests against $\sim A$. Now, these tests are not given by God-Reality, they are themselves testable, like in the Gospel[8] « Do not judge… ». Indeed a test against A is a sort of proof of $\sim A$ and, symmetrically, a test against $\sim A$ is a sort of test against A.

In a widely spread theory, the philosopher Popper proposed an explanation of truth by *falsifiability*, i.e., w.r.t. to a battery of tests. Popper, a failed logician in his youth, was surely reminiscent of Hilbert's programme, since the formulas that can be handled in this specific way are Π^0_1: universally quantified formulas of arithmetic, which include Fermat's last theorem, the quadratic reprocity law, Riemann's conjecture… as well as the Gödel sentence and consistency formulas. In terms of tests, these formulas are *recessive*: the more you check, the less you get. In accorda,ce with incompleteness which syas that not everything is recessive, of layer -4 is not Popperian, since, when a test and an counter-test disagree, one of the two is disqualified, butwe don't now which one. This is indeed delicate to tell since this relies on a subtle dialectics involving other tests and counter-tests, to the effect that the fact that τ is a test against A is undecidable, worse, of arbitrary complexity. I proposed the expression *epistate* (in Greek, a temporary judge, which could be judged for misjudgement) to account for the want of absoluteness of tests.

1.4 Enters Immanuel

It took me a long time and the building of contacts with the adequate philosophers to understand that I was « regressing » from Frege to Kant; strange regression, since it is rather an unwelcome by-product of totalitarism that put Frege's simplistic reductions on the front stage! From Kant, I will only retain

[7] From aletheia, meaning « truth ».
[8] *Matthew*, VII.1.

the idea of *conditions of possibility*; like most philosophers, he couldn't resist the temptation of systematisation; let us repeat that I am only a consumer of transcendentalism and not of categorical imperatives, like in category theory, I am a consumer of direct limits or pull-backs, but not a diagram addict!

Kant explains the coherence of perception — an experimental fact — by its *transcendental* conditions, i.e., the hypotheses that make it possible. Whether these conditions are necessary is a delicate question that may lead to unwelcome conclusions; but there is no problem with *sufficiency*. I propose to do the same for logic: coherence — not limited to consistency which is but the poor man's coherence —, i.e., cut-elimination, Church-Rosser, the disjunction property, are not accounted for by semantics — by no means at layer -1, partially at layers -2 or -3. The question is thus « What makes logic coherent? », the only possible answer being in the language itself: logical artifacts are constructed so as to ensure coherence. To find the — modestly, some — hypotheses making logic possible, this is *transcendental syntax*.

I daresay that I always did logic in this way, beginning with system **F** in 1970. Linear logic is a typical exercise in transcendental syntax: starting with an interesting layer -2 interpretation (incomplete or inconsistent like all such interpretations), I found the conditions of possibility for the new operations disclosed, what involved a not too bad layer -1 semantics — *phase semantics* — ; but what really gave the conditions of possibility of linear logic was proof-nets, which levelled with natural deduction.

There is a big difference being doing something and *being conscious* of doing it; this is why philosophy is important. In my case, the reference to Kant (and to Hegel, but I was already conscious of doing some sort of dialectics), gave me thwords, i.e., enabled me to attack the questions frontally; it also gives me ammunitions against this *fetishism* of reality known as fregeism. Psychologically speaking, I passed from a somewhat unpleasant defensive attitude — from the beginning, (system **F**) I had to defend my viewpoint against fregeans, typically Tarski's pupils — to a constructive one. By the way, *constructivism* is a typical kantian expression, meaning that reality is a construction, for instance the result of a formatting; it is what makes the ox see what he « sees » on his retina.

The idea of transcendental syntax is quite the opposite of what people do with Kripke models and similar constructions: they take the language as it is, call it reality, and state a completeness theorem. Here, the idea is to start with a good system and « forget » its syntax: the reason is that syntactical manipulations — although the only possible manipulations — are extremely involved. In particular, there is an entanglement between the actual information and the normative (deontic) apparatus which controls its use. Typically, what is the effect of writing twice the same formula at different locations? If there is a reason for doing so, only the oblivion of syntax can find it.

No system is quite good, let us give some examples, all of them coming from the transcendental approach.

Occurrences: there can be nothing like that. This idea of *bilocation* of a formula is pure semantic theology, with no condition of possibility. It comes from the fregean illusion that the language is about something else (an ideal denotation) that we can freely invoke. The only thing that makes sense is the location (hence the title *Locus Solum* [8]), and occurences are the result of *delocations*. As a result, the theoretical status of type

variables changes : a variable being a location, there can only be a variable in each type. If we need two variables of type A, this is because we must contract two « occurences » of A, indeed two delocations of A which are distinct. This change of viewpoint helps us to avoid meaningless questions, i.e., logical *aporias*. Typically, the question of superposition of proof-nets (additive case): what should we do with cuts, should we superpose them, and how should we do it in case of several cuts on the same formula? This question only makes sense from the dubious viewpoint of occurrences; if formulas come with their locations, the superposition can be done in only one possible way. This being said, we shall make use of the expression « occurrence », with the precise meaning of a delocalised copy, nothing to do with Fátima.

Locativity: in particular, contrarily to the category-theoretic dogma, most isomorphisms of logic are equalities. Typically, the tensor product enjoys $A \otimes B = B \otimes A$, so that functional application enjoys $F(a, b) = F(b, a)$, in apparent contradiction with category theory. Nobody can however discuss the fact that equality is more natural than an isomorphism (with sucking canonicity conditions). Moreover, categories are not that far: it suffices to work modulo delocation! This viewpoint is well-adapted to the handling of *records*, which appear as sort of locative products. Depending on the viewpoint, locative or categorical, such or such aspect of records will be enabled. An other example: Torino intersection types have nothing to do with any sort of reality, category-theoretic or not, they clearly deserve a locative treatment (you cannot intersect things which are up to isomorphism!), but pure lambda-calculus has not been handled in the right way (e.g., the name of bound variables should matter!), to the effect that the theory of intersection types seems half-baked.

Neutral elements: semantics creates monsters. For instance, there is no absolute need for neutral elements, but category-theory makes them appear, hence the neutrals of linear logic, together with the impossibility to draw decent proof-nets for them. Forget semantics and take the trancendental viewpoint: the tensor product is strict, i.e., literal; its neutral should also be strict, thus occupy no space. But how can we deal with something occupying no space? The only possible conclusion is that the neutrals do not make sense, at least not without some severe restriction on them!

Predicate calculus: there is already a problem with usual propositional calculus: the letters $P, Q, R, \ldots$ mean strictly nothing and only an adamant fregeist can imagine that P and Q could have distinct denotations! The correct way is to say that propositions are indeed universally quantified, thus second order formulas, the quantifier ranging over propositions. In the predicate case, the situation is more desperate, due to the presence of first order terms. The assumption than models are non empty is already a mistake from the viewpoint of transcendental syntax: of course, if we have in mind the sole encoding of « reality », this is presumably a minor simplification, otherwise we would not get $A \Rightarrow \exists x A$. But the price to pay for this « simplication » in terms of proof-theory is heavy: in the rule for existence, the variables of the witness t should be the eigenvariables of some $\forall$ below, what can be ensured with the convention of « non-empty

domains » only at the price of irrelevant red tape. In genral, first-order terms are problematic because they come from nowhere; the metaphysical solution consists in specifying an auxiliary domain, but this is yet another semantic nonsense. However, an answer — satisfactory in the main lines — exists, namely first order terms are proofs: this is the option of Martin-Löf's Type Theory[13]. The transcendental theory of dependent types and products is still to be written, with, as major difficulty, the handling of equality.

Equality: from the transcendental viewpoint, equality is badly mistreated. It is usually a matter of petty details: the most important operation is swept under the rug! One must say that semantics does not help, since $t = u$ means that t and u have the same denotation, in which case there is a proof (with empty contents). I am currently working on the transcendental conditions for equality, a complex endeavour. To give an example: the Leibniz definition $\forall X(X(t) \Rightarrow X(u))$ is, as it stands, a semantic nonsense. If we take t, u as they should be taken, i.e., objects with locations, it is not possible that they enjoy quite the same properties, since they can be distinguished by their locations. Of course, the purpose of formatting is usually to abstract from location, but there is more than a hint that the snake is biting its own tail: t and u enjoy the same properties partly because we have restricted to those properties which do not distinguish between them!

What has been so far expounded is not quite transcendental syntax, it should be rather called transcendental *semantics*, since layer -4 only yields the conditions of possibility of *evaluation*. This includes the conditions of possibility of cut-elimination and strong normalisation. However, transcendental syntax proper involves much more, namely everything related to *certainty*:

(i) In which sense is a proof a finite object?

(ii) How is it possible that a finite object gives us certainty about the infinite?

(iii) This certainty cannot be absolute (incompleteness); it is thus relative... relative to what?

2 From multiplicatives to GoI

2.1 Multiplicatives

The correctness criterion of proof-nets is a *deontic* achievement: among *proof-structures* (i.e., would-be proofs) it characterises the « correct » ones, *proof-nets*. Remember that they originate from the existence of an involutive negation; the original meaning of negation is not to... negate as would say the semantic parrot, but to swap the two sides of a sequent, the hypotheses and conclusion of a natural deduction. Linearity — which forbids repetitions of hypotheses — makes this swapping possible. This, by the way, sheds a new light on *Modus Tollens*, i.e., contraposition: double negation, by a to-and-fro between the two zones, enables contraction on the right, i.e., reuse of conclusions. The restriction

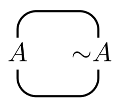
« one formula on the right » thus appears as a *formatting* impeding right contraction; in the absence of contraction (and weakening) *Modus Tollens* becomes innocuous... and devoid of interest.

Natural deduction rests upon an untold normativity: proofs are tree-like, thus the correctness can be locally checked by looking at the root of the tree and recursively ay the premises of the downmost rule. By the way, observe that natural deduction is satisfactory only for the *negative* connectives $\Rightarrow, \wedge, \forall$; this is because they naturally proceed from conclusion to hypotheses. The positive ones (existence, disjunction) hardly fit the tree format; their elimination rules are indeed topsy-turvied: one would need in that case upwards-rooted trees, but, due to the pregnancy of implication, the other format has been chosen. As a result of this inadequate format, the sucking theory of *commutative conversions* has at least one quality: it exposes the *format*, since these conversions do nothing but replacing one inadequate format with a less inadequate one!

Do to the pregnancy of the positive connective $\otimes$ and the involutive negation, linear logic is compelled to do what natural deduction could (almost) avoid, namely relinquish of the format of downward-rooted trees: no hypothesis, only conclusions! The success of the enterprise is so and so; however, the most important case, dealing with linear implication — the implicative part of usual implication —, i.e., *modulo* De Morgan, with the multiplicatives $\otimes, \invamp$, is perfectly satisfactory.

Everything being a conclusion, we face the problem of multiplicity of conclusions: although there are several terminal *links*, only some of them can actually be a last rule. The challenge is to make sure that there is at least one last rule — usually not unique. The correctness criterion associates to a proof-nets graphs (originally: trips); those graphs must be connected and acyclic (the trips should be cyclic; they can be seen as possible ways to « visit » the graphs). The first and most important result of linear logic is that this condition is equivalent to logical correctness. Originally, this was just a way to write linear logic without commutative conversions (unavoidable, since $\otimes$ is a positive operation almost as important as its adjoint $\multimap$. But this opened the issue of *normativity*, i.e., deontics[9]. In particular, this question of transcendental syntax: normativity, what for?

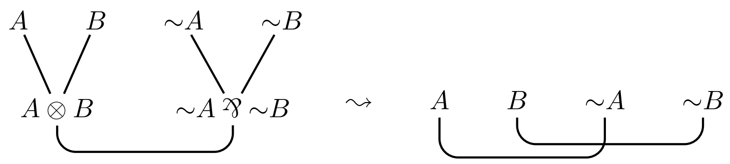
The factual answer: normativity ensures logical correctness is just as dumb essentialist as the tarskian « the role of negation is to negate »: it presupposes that we have no doubt as to logical correctness which proceeds from God-Reality. A non-metaphysical answer can be found by expanding the original remarks of Prawitz on the symmetry between introductions and eliminations. We know that, during the normalisation process, the size of the proof-net shrinks, but in the case of a « vicious circle » (combination axiom + cut, with no conclusion):



The deontic apparatus is thus a way to impede the creation of vicious circles

[9]Herbrand's theorem (*supra*) should have opened it long before; but the reductionistic ideology of consistency proofs didn't encourage subtility!

during normalisation; since vicious circles are excluded by the correctness criterion, it is enough to verify that normalisation preserves correctness. Indeed, the rewriting:



is *correct* (i.e., preserves the correctness criterion, not some metaphysical truth). Indeed, *switch* the reduced structure and remove the two cuts; this induces a graph made of several connected components. Observe that:

- The component of A contains none of the three other formulas:
  - If it contained B, there would be a cycle in the original net.
  - If it contained $\sim B$, a switching of the $\parr$ on « L » would produce a cycle in the original net.
  - Similarly, it does not contain $\sim A$.
- B stands alone too in its component.
- $\sim A, \sim B$ share a component; otherwise, the original net would be disconnected whatever switching we choose.

One can see this argument as a modern version of Prawitz's symmetry between introductions and eliminations. Indeed, the switchings of A anticipate upon the possible proofs of $\sim A$, they are *virtual proofs* of the negation.

2.2 Contradictory foundations

This notion of virtual proof can be made more precise. Indeed (assuming « η », i.e., atomic axiom links), a switching of A induces a sort of proof of $\sim A$. This « proof » can be recursively written in sequent calculus, starting from its conclusion $\vdash \sim A$. In the sequent $\vdash \Gamma$, formed with negations of subformulas of A, on chooses a compound formula D[10], so that $\Gamma = \Delta, D$:

- If $D = \sim(B \otimes C) = \sim C \parr \sim B$, one makes $\vdash \Gamma$ the conclusion of a rule « $\parr$ » applied to $\vdash \Delta, \sim C, \sim B$.
- If $D = \sim(B \parr C) = \sim C \otimes \sim B$ and if the $\parr$-link leading to $B \parr C$ has been switched on « L », then $\vdash \Gamma$ appears as the conclusion of a rule « $\otimes$ » applied to $\vdash \Delta, \sim B$ and $\vdash \sim C$; in other terms, the switching « L » gives the full context to the left premise. In case of a switching « R », the premises become $\vdash \Delta, \sim C$ and $\vdash \sim B$. Remark that there could be other splittings of the context, but that, in some sense, they are not needed: we are not producing « all » proofs of the negation, only « enough » of them[11].

[10] The choice of D only affects the sequent version of the *proof*, not its proof-net.

[11] This remark anticipates upon the recent idea that tests are only a selection of *virtual* counter-proofs.

This construction stops when one reaches literals. Here, the only choice is to accept the sequents as « axioms », which as the etymology suggests, is a purely arbitrary decision[12]. The correctness criterion can be rephrased as the convergence of the normalisation procedure between the proof of A and the « proofs » of $\sim A$ induced by switching. The sequentialisation theorem says that there are enough « proofs » of $\sim A$ to characterise the proofs of A.

The criterion can be restated in terms of « orthogonality » of permutations (« trip » formulation; with the graph formulation, orthogonality of partitions):

(i) Let us list the literals of A as the finite set $N := \{0, \ldots, N-1\}$ $(N \neq 0)$.

(ii) Our concern is about permutations $\sigma, \tau, \ldots \in \mathfrak{S}(N)$. These permutations may stand either for proofs ($\sigma(i) = j$ in case of a link between i and j, hence $\sigma(j) = i$) of A or switchings of A ($\sigma(i) = j$ when, starting downwards from literal i, the next literal to be visited (thus, upwards) is j; those permutations are not symmetrical).

(iii) If we say that σ, τ are orthognal, $\sigma \perp \tau$ when $\sigma\tau$ is cyclic (i.e., the $(\sigma\tau)^n(0)$ all distinct for $n \in N$: $\tau\sigma$ is thus cyclic too), then the correctness criterion can be written as:

σ is a correct proof of A if $\sigma \perp \tau$ for all $\tau \in \mathfrak{S}(A)$, where $\mathfrak{S}(A)$ stands for the permutations arising from switchings of A.

(iv) By the way, cut-elimination ensures that negation matches orthogonality:

$$(\mathfrak{S}(A))^{\perp\perp} = (\mathfrak{S}(\sim A))^{\perp} \tag{1}$$

We have indeed reached the perfect example of contradictory foundations; the duality is between the stabilised sets $(\mathfrak{S}(A))^{\perp}$ and $(\mathfrak{S}(\sim A))^{\perp\perp}$ that can be seen as dual sets of « proofs ». This contradictory foundations are not inconsistent for the very reason that most of those permutations are only *deontic* (i.e., they do not prove A, they only *forbid* — $\neq$ refute — $\sim A$). Inconsistency is easily avoided by requiring that a « real » proof should be a symmetric permutation $(\sigma = \sigma^{-1})$ such that $\sigma(i) \neq i$ for all i: the product of two such permutations cannot be cyclic[13].

This contradictory foundations suggests an *existentialist* approach; instead of proceeding from the top (A with its frozen logical rules), we proceed from the bottom. We start with a sort of « id », an unstructured magma of permutations. These permutations are put in duality by:

$$\sigma \perp \tau \quad \Leftrightarrow \quad \sigma\tau \text{ cyclic} \tag{2}$$

A « superego », i.e., a formula A thus appears *a posteriori* as a set of permutations equal to its biorthogonal (with the previous notations, $A := \mathfrak{S}(A)^{\perp}$):

$$A = A^{\perp\perp} \tag{3}$$

[12]Modern Greek: *axiomatikos* means « officer », i.e., the guy whose orders, whatever stupid they may be, are beyond discussion.

[13]I am not claiming that this restriction actually caracterises proofs, what is wrong. I am just getting rid of an illiterate objection against Hegel.

This existentialist approach is the origin of *Geometry of Interaction*, an important milestone in transcendental syntax. But not the last word: existentialism corrects the essentialist arrogance, but bends the stick too far by neglecting the idea of *law*. Except in the limited case of multiplicatives, we shall never get any certainty, relative or not, in this way. The multiplicative case is special in the sense that the previous analysis yields an *absolute* certainty: *apodictic* is the right expression in that case.

Let us now express *normalisation* in this case; one knows (e.g., the Principle of the Tortoise, see *infra*) that cut can be reduced to *Modus Ponens*:
if $\sigma \in A, \tau \in \sim A \parr B$, assume that the literals of $\sim A \parr B$ are the elements of $M+N$, M corresponding to B, the segment $M+N \setminus M = \{M, \ldots, M+N-1\}$ corresponding to A, so that $\tau \in \mathfrak{S}(M+N), \sigma \in \mathfrak{S}(M+N \setminus M)$; we seek a permutation $\rho = [\tau]\sigma \in \mathfrak{S}(M)$, indeed an element of B. The definition is simple: starting from $i \in M$, we alternately apply τ and σ until we come back (using τ) to M, in other terms:

$$\rho := M(\tau \cup \tau\sigma\tau \cup \tau\sigma\tau\sigma\tau \cup \ldots)M \tag{4}$$

The two occurrences of M indicate that the function is restricted and co-restricted to M. If we move to linear algebra and to the Hilbert space $\mathbb{C}^{M+N}$, then σ, τ, M become *operators*, σ unitary, M projection, τ unitary on the subspace $\mathbb{C}^{M+N\setminus M}$: all are indeed all *partial isometries* (uu^* idempotent). The formula rewrites as:

$$\rho := M(\tau + \tau\sigma\tau + \tau\sigma\tau\sigma\tau + \ldots)M \tag{5}$$

which could become:

$$\rho := M\tau(1-\sigma\tau)^{-1}M = M(1-\tau\sigma)^{-1}\tau M \tag{6}$$

provided the series (5) converges. Since its terms are of discrete norms 0 or 1, the only possible way is that:

$$\sigma\tau \text{ is nilpotent} \tag{7}$$

Indeed, coming back to the correctness criterion, we see that τ is orthogonal to all elements of $A \otimes \sim B$; if $\theta \in \mathfrak{S}(B) \subset \sim B$ is a switching of B, then the product of σ and $\tau \cup \theta$ is cyclic, what forces the nilpotency of $\sigma\tau$. If we implement σ, τ on Hilbert space, then (6) expresses the solution of a *feedback equation*: given $x \in \mathbb{C}^M$, find $x' = \rho(x) \in \mathbb{C}^M$ and $y, y' \in \mathbb{C}^{M+N\setminus M}$ such that:

$$\begin{aligned} \tau(x+y) &= x' + y' \quad (8)\\ \sigma(y') &= y \quad (9) \end{aligned}$$

This equation expresses one of the deepest intuitions about implication and cut-elimination: implication is a *connexion* (typically, $A \multimap A$ is an extension cord) and cut is *plugging*; the feedback equation yields the outcome of the plugging, thus proposing an equivalent plugging-free connexion. Nilpotency expresses the absence of *short-circuit*.

2.3 Geometry of interaction

Geometry of Interaction (GoI) is the generalisation of this existentialist approach to full logic. I shall not present the latest version which requires some non-trivial functional analysis and content myself with the original version of 1988 [5]. GoI has to be revisited anyway in the light of transcendental syntax, hence I content myself with the easiest version. This original version makes a superficial use of functional analysis; as I shall present it here, we hardly need the notion of a C^*-algebra; also its duality, based on « $\sigma\tau$ nilpotent » is good enough to account for strong normalisation, but not fitted for a complete deontics; finally, this treatment is limited to the linear version of system **F**, thus excluding additives, which are indeed not vital to logic and whose accomodation would involve the sophisticated idea of *dialect* [7, 10].

GoI is *locative*; this means that formulas receive *locations*, usually disjoint (except in the case of a subformula, which naturally corresponds to a sublocation); this is made possible by the replacement of the pseudo-concept of « occurrence » by the manageable one of *delocation*. In the multiplicative case, a location is a subset of the set N of the literals of the formula under study; each subformula of A occupies a subset of N and these *locations* are pairwise disjoint or incomparable w.r.t. inclusion. The general case (especially exponentials) calls for a generalisation of these finite locations together with a possibility of changing their size. The simplest choice consists in a *unification algebra*. The one we shall present here is close to the original version, but not quite the same, the difference being of no real interest. It must however be stressed that unification algebras (there is basically one, like there is basically one free group) are not the panacea; logic is presumably sensitive to the choice of the underlying algebra. See for instance my excursion into LOGSPACE which makes use of a semi-direct product.

Typically, consider the terms (*locations*) generated from two constants `l`, `r`, variables $x, y, \ldots$ (up to renaming) and a binary function letter « $\cdot$ », without repetition of variables. Two such terms t, u intersect by *matching*; when matching is impossible, the intersection is empty. Typically $x \cap t = t$, $l \cdot x \cap x \cdot r = l \cdot r$, $l \cdot x \cap l = 0$ $(= \emptyset)$. This can be algebraised by considering finite sums of terms, 0 corresponding to $\emptyset$. The commutative structure thus obtained is well-suited to handle *locations*, what amounts at locating the « subformulas[14] » of a given A. For a while, the variables will not be up to renaming; when B is at depth n (box nesting), then it receives a location ℓ_B depending on the variables $x_0, \ldots, x_n$. Locations are defined recursively, starting from the conclusion $\vdash A$ of the proof; the upper indices stand for the depths of the formulas involved; except in the exponential rules, this depths remains constant.

(i) In the conclusion $\vdash A^0$, $\ell_A := x_0$.

(ii) If $(B \odot C)^n$ is introduced by a $\odot$-rule (multiplicative), then
$\ell_B := \ell_{B\odot C}[l \cdot x_n / x_n]$, $\quad \ell_C := \ell_{B\odot C}[r \cdot x_n / x_n]$[15].

(iii) If $(QXB)^n$ is introduced by a quantifier rule, then $\ell_{B[T/X]} := \ell_{QXB}$.

(iv) If $(!B)^n$ is introduced by promotion, then $\ell_B := \ell_{!B}[x_n \cdot x_{n+1} / x_n]$.

[14] Indeed the formulas occurring in a cut-free proof of A.

[15] Hence, contrarily to what I thought, $\sim(A \otimes B) = \sim A \parr \sim B$ and not $\sim B \parr \sim A$!

(v) If $(?B)^n$ is introduced by a combination dereliction/weakening/contraction from the $B_i^{n_i}$ $(n_i \geq n)$, select distinct closed terms for each index i, say $\mathtt{i}$ for i. Then $\ell_{B_i} := \ell_B[(\ldots((\mathtt{i} \cdot x_{n_i}) \cdot x_{n_i-1} \ldots) \cdot x_n / x_n]$.

The algebra so far constructed is commutative; it is indeed part of a bigger algebra whose basic elements (*transitions*) are of the form $t \rightarrow u$, where t, u are locations with the same variables. Transitions are composed by unification (if possible); finite sums of transitions are thus equipped with a non-commutative product. The non-commutative algebra thus obtained extends the algebra of locations (identified with the projections $t \rightarrow t$). This unification algebra has an adjunction $(t \rightarrow u)^* := u \rightarrow t$.

Each transition can be identified with a partial permutation of the set of *closed* locations: to $t \rightarrow u$ associate the partial map $\varphi_{t \rightarrow u}$ such that $(t \rightarrow u)(\theta \vdash \theta) = \varphi_{t \rightarrow u}(\theta) \vdash \theta$. The map is defined only when unification between u and θ is possible; since the same variables occur in t, u, the map is well-defined and injective. These partial bijections are easily transformed into partial isometries of a Hilbert space whose Hilbertian basis is the set of closed locations, composition and adjunction corresponding to the usual operations on operators. The unification algebra is thus imbeddable in a C^*-algebra, that one can make unbique by selecteing the *greatest* stellar norm[16]. Further developments would require a von Neumann algebra; however, since the unification algebra contains atomic projections: the closed locations) the vN algebra would be trivial in that case, i.e., of type $\mathbf{I}_\infty$. But, as I already said, I am not precisely guilding the lily in these lectures!

It is now time to interpret proofs; every formula occurring in the proof received a location. The proof is interpreted by the sum of all $t_B \rightarrow t_{\sim B}$ and their adjoints $t_{\sim B} \rightarrow t_B$, for all identity axioms (or axiom links; our construction works with sequent calculus or proof-nets with !-boxes as well) $\vdash B, \sim B$. By the way, a hermitian (self-adjoint) sum of transtions $t_i \rightarrow t_j$, with the t_i pairwise disjoint is called a *connexion*. The definition makes sense because the two formulas of an axiom link are at the same depth, hence have the same variables. This also why we so far chose variables *literally*; once the axiom links have been interpreted, the variables lose their individuality.

The main result is that (up to some minor details), a cut between connexions σ, τ of $\vdash \Gamma, A$ and $\vdash \sim A, \Delta$ normalises according to the GoI formula (given by (6) when $\Gamma = \emptyset$). In particular, $\sigma\tau$ is nilpotent. There is a small mismatch due to the connective « ? » (and, due to the absence of subformula property, to $\exists X$), namely that the GoI formula makes *mistakes* when handling structural rules. A typical mistake occurs in the case of weakening: In the case of a cut between proofs of $\vdash A, !B$ and $\vdash ?\sim B, C$, the latter one coming from a weakening:

$$\begin{bmatrix} \gamma_{11} & \gamma_{12} \\ \gamma_{21} & \gamma_{22} \end{bmatrix} \qquad \begin{bmatrix} 0 & 0 \\ 0 & \delta \end{bmatrix} \tag{10}$$

symbol-pushing would erase, while GoI would leave part of the structure untouched (and untouchable), like a useless *beard*, to quote a medieval philosopher, Guillaume d'Ockham:

$$\begin{bmatrix} \gamma_{11} & 0 \\ 0 & \delta \end{bmatrix} \quad \text{vs.} \quad \begin{bmatrix} 0 & 0 \\ 0 & \delta \end{bmatrix} \tag{11}$$

[16] Norm enjoying $\|uu^*\| = \|u\|^2$.

However, when the type of the output is free from ? and $\exists X$[17], then (6) makes no mistake.

In fact the « mistake » is presumably on the side of symbol-pushing, for (6) is Church-Rosser:

$$f(t)(u) \quad = \quad f(u)(t) \tag{12}$$

Remember that there is nothing weird here: due to locativity, t, u have disjoint types, hence the distinction between $f(t)(u)$ and $f(u)(t)$ is only a matter of the order in which the normalisation (6) is performed; the familiar distinction occurs when t, u are of the *same*, i.e., isomorphic, types, in which case, $f(t)(u) \neq f(u')(t')$, where t', u' have been replaced with isomorphic (delocated) copies of the other type[18]. By the way, Church-Rosser is the transcendental condition of category-theoretic associativity; the do-it-yourself Church-Rossers (e.g., leftmost reduction) are just another instance of the « publish or perish ».

Associativity shows that, although $f(t)$ may differ from its expected interpretation, GoI will agree with symbol-pushing, provided the output is free from ? and $\exists X$.

« Mistake » is a therefore inadequate, « mismatch » would be better, since the difference between GoI and symbol-pushing is inessential. And remember that transcendental syntax is not a polishing brush like Kripke models: syntax is globally correct but likely to make minor mistakes. Compared to (6), the list of reduction rules of sequent calculus (including nets with boxes) looks like a cookbook.

3 Existentialism vs. essentialism

3.1 Church vs. Curry

GoI defines a type as the set of its elements (its proofs). Starting with an orthogonality relation, a type is is a set of objects (connexions) equal to its biorthogonal, a *behaviour*:

$$A = A^{\perp\perp} \tag{13}$$

The original GoI has a non-satisfactory orthogonality:

$$\sigma \perp \tau \quad \Leftrightarrow \quad \sigma\tau \text{ nilpotent} \tag{14}$$

In order to recover the full power of « cyclic », GdI must be elaborated so as to work in an algebra with (positive) determinant, typically the *hyperfinite factor*, in which case:

$$\sigma \perp \tau \quad \Leftrightarrow \quad \det(1 - \sigma\tau) \neq 0, 1 \tag{15}$$

the value 0 corresponding to cycles [10, 11]. But this is another story, and we shall forget the (minor, in the context of these lectures) limitations of the first GoI.

The main result concerning GoI is that it makes sense without any reference to the original syntax, namely that one can define connectives as constructors

[17] $\exists X$ may hide a « ? ».

[18] The literal commutativity of the tensor does not force category-theoretic commutavity: shoudl there be non-commutative tensor $\oslash$, it should be written $A \oslash B = B \oslash B$.

of behaviours. Typically, $\forall X$ appears as an intersection:

$$\forall X A := \bigcap_B A[B/X] \tag{16}$$

The intersection ranges over all possible behaviours B; $\exists X$ must content itself with a dual definition:

$$\exists X A := \sim\forall X \sim A \tag{17}$$

$\parr$ is handled by means of a functional definition, in particular, the combination $A \Rightarrow B \ := \ !A \multimap B$:

$$f \in A \Rightarrow B \quad \Leftrightarrow \quad \forall a \in A \quad (f \cdot !a \text{ nilpotent and } f(!a) \in B) \tag{18}$$

This approach is very sound, and once more, the orthogonality can be fixed so as to recover familiar deontic requirements and weirder ones (typically the ones corresponding to light logics).

This approach is *existentialist*, in the sense that the typing is *a posteriori*: typically, the connexion $t \to u + u \to t$ (t, u disjoint), the « extension cord », can be given any type $A \parr \sim A$, hence the type $\forall X (X \parr \sim X)$. In particular, this approach allows polymorphism, subtyping and a satisfactory approach to *records*. In the context of λ-calculus, *a posteriori* typing is said to be « à la Curry ». GoI can be seen as a completely convincing development of this idea, free from any *a priori* concerning the types. Anything is a matter of tests, at least in the last versions of GoI.

However, something is wrong with this existentialist approach and with *existentialism* in general. In real life, existentialism would lead us to refuse any law, even those already tested, on the sole grounds that a new test could contradict the previous one: even the fact that the same test performed a second time yields the same answer belongs to « laws ». In other terms, although the idea is respectable, it concretley leads to anarchy and to the worse empirism! The same in logic: it surely yields a correct deontics (justification of the rules), but this justification remains ethereal, abstract, since we are never in a situation when we can *enforce* it. In order to apply the definitions *a posteriori*, we should be able to:

(i) Write objects and their tests in a finite way; this is not the case in operator algebras, but if we stick to a unification algebra — or any other effective structure giving eventually rise to an operator algebra, there is no problem.

(ii) Each test should be performed easily, i.e., in *low* complexity. I have not in mind any specific complexity: LOGSPACE is the best, but any complexity would suit me at this level of discussion, as long as it is given in advance! Unfortunately, the nilpotency of $\sigma\tau$ is only semi-decidable ($\exists n \ (\sigma\tau)^n = 0$), a complete Σ^0_1 property); by the way, this is why it succeeds in representing the monstrous functions involved in system **F**!

(iii) There should only be finitely many tests. This is not the case: not only there are infinitely many tests, but we are not even in a Popperian situation: at the deontic level, tests can be recused by counter-tests; this recusation being mutual, which one is eventually excluded cannot be decided on a finitary basis! The decision indeed corresponds to the phenomenon of logical complexity, known to be irreducible since incompleteness.

As to the interrogations about certainty, we surely managed to give a finitary meaning to proofs, to their deductive properties (including strong normalisation), but we failed at finding any form of *certainty.* Typically, how do we know that the extension cord $\theta := t{\to}u + u{\to}t$ belongs to $\forall X(X \parr \sim X)$? The book says: take any $\sigma \perp \tau$ located at t and *check* that $\theta \perp \sigma + \theta(\tau)$. Of course, it is easy to *prove* that all these tests succeed, but we are supposed to be beyond proofs, at the empirical level of tests and recusations: there are infinitely many tests to perform, in other terms, we cannot even prove that $A \multimap A$ from this standpoint. To sum up:

(i) The complexity of tests should be *tamed.*

(ii) One should be able to restrict to a *finite* battery of tests.

Anyway, even if I pushed the existentialist pattern to its end, I several time casted doubts about existentialism, seen as a simplisitic remedy to the essentialist arrogance. The question is « are galaxies made of stars or is it the other way around? ». Common (analytical) sense supports the first solution: formulas are made of proofs, but the history of modern logic shows that, starting with an essentialist approach to logic, i.e., the formulas and their ready-made semantics, proof-theory was able to reconstruct the underlying objects, i.e., from the galaxies, to reconstruct the stars. There are, of course, radical differences between formulas and proofs: proofs are effective, whatever approach we take, one can compute them, compute with them. Formulas are extremely complex, since their semantics of any layer, including their deontics, is of unlimited logical complexity. But proofs and formulas, albeit heterogeneous, manage to get in contact in a satisfactory way, whereas this should be impossible. How come?

Compared to the approach of Curry, typing « à la Church » seems rather essentialist. System **F** remains one of the major examples of a system in which the typing is *a priori.* An important detail is that the process of normalisation involves the typing while staying perfectly Church-Rosser, i.e., associative. In other terms, although the types are supposed to be *specifications*, i.e., logically complex monsters, one can compute with them. By the way, my first interpretation of system **F** by means of *coherent spaces* involved a category of « types », coherent spaces with embeddings, terms being sort of functors preserving direct limits and pull-backs. By the way, this does not make this interpretation quite category-theoretic, since nobody has so far been able to find which abstract nonsense second-order quantification is an instance of. Explicit typing can easily be transformed into implicit typing: this is the *forgetful functor* of system **F**: erase everything related to types, the type of variables, type abstractions and type extractions; this yields a pure λ-term. This erasure is *functorial* in the sense that the image of a rewriting $t \rightsquigarrow u$ of **F** is a rewriting $t^- \rightsquigarrow u^-$ of λ-calculus. Moreover, the types, now seen as the sets of erased terms, enjoy the existentialist definitions.

Typing « à la Church » is too essentialist to explain polymorphism, subtyping, locativity. But it yields *certainty*: we actually *know* that such or such object does what it is supposed to do, i.e., is of the right type. This certainty is not absolute; typically, a closed term f of type $\mathbf{nat} \Rightarrow \mathbf{nat}$ of **F** should always, when fed with a natural number $\overline{n}$ of type **nat**[19] yields a natural number $\overline{m}$,

[19] One should not write $\overline{n} \in \mathbf{nat}$, except as a shorthand: in **F**, a type is a not the set of the objects of that type.

the normal form of $f(\overline{n})$. But how do we know this? Because of the normalisation theorem for system **F**. But the subtle point is that this normalisation theorem [2, 11], whatever clever in its day, is hardly more than a paraphrasis of the typing, *modulo* the basic and quite elementary properties of *candidats de réductibilité*. In particular, type extraction relies on the existence of specific sets, i.e., upon comprehension axioms; the proof that a term is reducible (i.e., enjoys a sort of definition à la Curry) is therefore completely contained in the typing. Our certainty is therefore relative to our belief in the possibility of defining such types, which is a matter of experience (one century of set-theory), but can be disputed for various ideological reasons (e.g., by predicativists). Anyway, whether one accepts these types or not, we achieve certainty relatively to them.

3.2 BHK and the Saaty War

Independently due to Brouwer (with Heyting as penholder) and Kolmogorov, this is the first existentialist approach to proofs. Although not quite a semantics, it often referred to as *semantics of proofs*. Its formulation is rather vague, what, due to the balbutiating status of logic (in 1930, cut-elimination was unknown), should be seen as a quality. Here « functions » are neither graphs, nor morphisms, not to speak of λ-terms (another anachronism), but they can become any of those, including operators of functional analysis. It is not pure chance that these definitions bear some similarity with implicit typing. For instance:

(i) A proof of $A \Rightarrow B$ is a function π sending proofs of A to proofs of B.

(ii) A proof of $\forall n A$ is a function π sending natural number N to a proof of $A[\overline{N}/n]$.

The comparison of this approach to Tarski's « truth definitions » is devastating:

(i) $A \Rightarrow B$ is true when the truth of A implies the truth of B.

(ii) $\forall n A$ is true when for any natural number N $A[\overline{N}/n]$ is true.

Such a truism may sometimes be useful, but it is clearly meaningless: tarskian truth is a classified topic.

Logicians (e.g., Gödel) were aware of the want of certainty associated with BHK: how do we know that a proof is a proof? Thus BHK (indeed the two problematic connectives $\Rightarrow$ and $\forall n$) was currently amended as follows:

π is the pair (π_1, π_2) of a function π_1 sending... and π_2 is a proof of the previous fact.

I remember quite well discussing this with Takeuchi (quoting Gödel's opinion about this issue) in 1975. With the distance, one must admit that the « auxiliary proof » is pure baloney; this does not show too much in the case of implication, indeed, we could imagine a tower of auxiliary proofs, all the way down like japanese turtles. But consider a Π^0_1 statement, typically a quantified equality, $\forall n f(n) = g(n)$. Semantics of proofs rightly deserves the qualiication of *semantics* in the case of equality, since $t = u$ has a proof when t and u have the same denotation, this proof being whatever you like, e.g., 0. Therefore, a proof of $\forall n f(n) = g(n)$ is the function sending any natural number N to 0, if $f(n) = g(n)$ for all n. This function (which exists anyway) has not the slightest

relation to the question; we could as well say that a proof of $\forall n f(n) = g(n)$ is any function φ, provided the formula is true! We see that in this case (fortunately, this case only) BHK is just as bad as Tarski's *vérité de La Palice*. Moreover, the auxiliary proof which should say that φ is a function sending... is exactly a proof of $\forall n f(n) = g(n)$! In other terms, there is no hope of a tower (possibly transfinite) of proofs and meta-proofs: the thing is plainly circular.

This problem about BHK, which must be related to the want of certainty of typing « à la Curry », is a central question of transcendental syntax: how come that the most important class of formulas (which contains a good half of the most memorable mathematical results and conjectures) has been completely ignored by proof-theory? The classic expression « Π^0_1 statements are algorithmically empty » is a positive way to compensate for the emptiness of the logical approach in this case. It seems that this problem can be split in two parts:

(i) Remove the word « function » from BHK, including its subrogates (λ-term, morphism, operator), in order to stay finite with a bounded finitary way of ensuring certainty. However, this certainty must be relative under the penalty of contradicting incompleteness.

(ii) Something should be done for equality: the present state of a category-theoretically degenerated equality (together with the associated do-it-yourself proof-theory) is a stain on logic. Transcendental syntax yields no answer, it only provides a methodology: keep away from any kind of semantics if you *really* intend to solve the problem.

I never quite understood why Kreisel started the Saaty War; when I asked him about this (in 1980) his only defense was that he was making fun of these discussions. Apparently his followers, e.g., Troelstra had no sense of humour, since everybody not accepting the « Saaty amendment » was excommunicated... ask Per Martin-Löf for details. In 1965, a mathematician by the name of Saaty edited a handbook of mathematics and commissioned Kreisel to write the entry « Mathematical logic ». Kreisel presented there BHK with his own version of the amendment, i.e., of the « auxiliary proof » π_2: π_2 must be a formal proof in a system *given in advance*. Remembering our analysis of Π^0_1 statements, this means that a proof of a Π^0_1 statement is just a proof in your favourite formal system, period! If you realise that people were still quarelling around this corny[20] proposition twenty years later, you realise that the analytic *newspeak* succeeded in impeding any decent foundational discussion: the main ideas of BHK are interesting, but in want of a severe lifting. But how can you do this when the ultimate reference is semantics, not to speak of the technical bleakness of the definition? Indeed, the only sensible point in the Saaty amendment is that, by incompleteness, absolute (*apodictic*) certainty does not make sense, therefore certainty should be relative, hence the use of formal methods somewhere, explicitly or implicitly. But not in that straightforward way!

Indeed, I was wrong in denying BHK the status of a « semantics ». The resaon is due to the absence of certainty: BHK yields a « functional » *interpretation* of proofs (or rather its canvas), by no means an alternative approach to proofs: a proof, a finite construction conveying a form of certainty is something else. We can give functional interpretations for proofs in the spirit of BHK, but this

[20] Usually, Kreisel was better inspired.

is still a translation. The remark holds for implicit typing, including GoI, as well. One should not hastily conclude that there is no conditions of possibility for language, that it must always be the description, the red tape, of *something else*: after all, BHK and its avatars, from the rather awkward realisability to the more satisfactory GoI, succeeded in getting rid of most of semantic artifacts. The question is to *finish the job*, by finding the transcendental status of *formal systems*... almost the opposite of Kreisel's amendment! What he proposes is a blunt, essentialist, use of formal systems, as a sort of wallop ensuring a ready-made certainty. One should rather try to understand what certainty is made of; in the logical landcape, there is only one artifact[21] providing a non ready-made certainty, namely proof-nets: certainty lies in the correctness criterion, not in the docility w.r.t. logical rules... let us revisit them.

3.3 Proof-nets revisited

GoI has been built as a sort of infinite-dimensional theory of proof-nets, extending the multiplicative case, and this case only. This calls for a discussion:

(i) Multiplicative proof-nets, seen as proofs of universally quantified multiplicative formulas, e.g., $\forall X \forall Y \forall Z((X \otimes (Y \parr Z)) \multimap ((X \otimes Y) \parr Z))$ are *apodictic*, i.e., beyond reasonable doubt. Indeed, the *connexion* of such a proof-net should be tested against all switchings (in the usual multiplicative sense) and all possible choices for the literals X, Y, Z; instead of testing against all choices, we can content ourselves with bounded choices for X, Y, Z: each literal splits (using $l \cdot x, r \cdot x$ in the unification) into two atoms, e.g., X_l, X_r and we can test against $X = X_l \parr X_r$ (hence $\sim X = \sim X_l \otimes \sim X_r$) or $X = X_l \otimes X_r$, which makes $2^3 = 8$ possible tests. Observe that these tests ensure that occurrences are rightly cabled (X can only be linked to a $\sim X$, not to another X or to a Y).

(ii) GoI, when generalising multiplicative nets to infinite dimension, loses its apodicticity; this infinite generalisation is another instance of a semantic prejudice. Since all meaningful tests have been completed at a finite level, the other tests are therefore pure superfetation. We just discovered an important idea, absent from our previous discussion (and anything so far published), namely that, if tests are rightly homogeneous to proofs of the negation, they remain *virtual*; in the « switchings as proofs paradigm », where not all « proofs » of the negation are considered, witness the brutal splitting of the context in the case of $\sim(B \parr C)$ (*supra*).

(iii) In GoI, we are supposed to perform all tests; since we cannot do this, we are led to a sort of semantic (rather deontic) argument about tests. I thus propose is to perform only a finite battery of tests, chosen as representative of the full bunch. This does not quite amounts to the same, since nobody compells us to prove that this battery is representative, indeed:

- A proof of this fact would bring « nothing new », in the same sense that the normalisation proof of system **F** is a sort of paraphrasis of the typing.

[21] If we except Herbrand's theorem.

- The choice of a representative battery of tests is precisely the *formal* step which becomes absolutely necessary when stepping out of the apodicitic paradise and dealing with the incomplete universe. Instead of the worn out « formal », « axiomatic », I propose « epidictic ». This expression conveys the idea of arrogant certainty indeed rather frequent[22]. Epidictics being a sort of axiomatics (in the usual sense), its only justification may be consensual, based upon experience, etc. Logic should stop there: any logical analysis of epidictics being eventually circular, like the justification of the normalisation for system **F** (basically the proof-systems of second-order comprehension) by means of comprehension. The idea is thus to say: OK, we believe, for reasons that we cannot really justify, that this finite battery of tests is enough; notwithstanding the reasonable doubt concerning this assertion, everything else is absolutely certain. This transcendental approach to certainty has nothing to do with a brutal regression to « logic = axioms + rules » of the sort Kreisel was proposing in his notorious Saaty entry. It gives the right place for certainty and doubts. Should we discuss doubts, this would involve semantics (or better: deontics), but this discussion is definitely no longer part of logic proper.

In logic, we already know what is and what is not apodictic. Restricting to closed second-order formulas, the ones in which the second-order quantifiers are universal — taking the *signature* into account or using a language without implication or negation — are exactly the apodictic ones. Typical examples are given in arithmetic (using the Dedekind definition of natural numbers) by Σ^0_1 formulas). This apodicticity can be expressed in a vulgar way by the completeness theorem: if A is true[23], then A is provable, and in a refined way by the *subformula property.* As soon as one steps out of Π^1, e.g., consider Σ^1 (e.g., Π^0_1) formulas, then we lose certainty; this is known as *incompleteness*, with a vulgar reformulation due to Tarski « true but not provable » of the original Gödel staement *a formula that the system cannot decide*, and whose eventual justification lies in the loss of the subformula property: $\exists X A$ follows from $A[T/X]$, where the second-order T cannot be bounded in any possible sense (it depends upon the system).

The fact that there may be disputes as to such and such logical law (classical vs. intuitionistic or linear; usual or bounded exponentials) is not an obstacle to Π^1 apodicticity: the same Kreisel — this time better inspired — observed that there was no real conflict between classical and intuitionistic, just a matter of knowing what we are speaking of. As to alternative exponentials whose list seems to be rather open, for those which really make sense, one should reach apodicticity « relatively to what they are supposed to do ».

[22]« The son of our beloved President is the most gifted guy ever; his gift is to have gifts! »: no need read the *Pravda* for epidictic crap, just read *Le Figaro*!

[23]The fact that A has been universally quantified frees us from the parasitic « in all models ».

4 Connexions, gabarits and tectonics

4.1 Quantifiers

GoI handles (second-order) quantification $\forall X$ as an intersection, corresponding to the forgetful functor of system **F**. This choice, an orthodox extension of BHK, is satisfactory in the Π^1 case, i.e., in the absence of existential quantifiers, but problematic in general; typically it introduces *deontic monsters*. For instance, in presence of additives, a proof of:

$$\forall X(A[X] \oplus B[X]) \multimap \forall X A[X] \oplus \forall X B[X] \tag{19}$$

also valid for intuitionistic disjunction, a principle which is classically inconsistent: since we deal with an intersection, the choice of a bit l/r discriminating between A and B must be independent from X. In the beginning, I was extremely excited by these non-classical laws (there a similar, although less spectacular, one with « $\otimes$ » in the place of « $\oplus$ »), that I considered as a new form of incompleteness[24].

I would now consider them as monsters, resulting from the semantic prejudice of the « objective world ». This *objectivism*, which is at the very root of fregeism, impregnates all logicians, including myself when dealing with proof-nets and GoI! In the treatment of proof-nets, the tendency was to get rid of « meaningless » sequentialisations, i.e., to quotient, to « erase too much ». For instance, in the additive case, the problem was to put together a proof π of $\vdash \Gamma, A$ and a proof π' of $\vdash \Gamma, B$ (two « slices ») to get a proof $\pi \,\&\, \pi'$ of $\vdash \Gamma, A \,\&\, B$. A lot of energy was devoted (and possibly wasted) in trying to make this superposition context- (i.e., Γ-) independent. The idea of « objective » superposition is a *semantic monster* created by coherent spaces: semantically speaking, $\pi \,\&\, \pi' := \pi \cup \pi'$. But the idea is a strategic failure: we should indeed remember that a proof is a *subjective* artifact, that if some part of this subjectivity is red tape (typically everything leading to those sucking commutative conversions), some other part may be what conveys *certainty*. Typically, in the case of $\pi \,\&\, \pi'$, it might be the case that some sequentialisation was *intended*, since *needed* (as part of some explicit temporal dependency); we have not the right to erase it, just as we have not the right to erase the symbol « end of list » of data bases. This does not mean that we must regress to additive boxes; this only means that the proof may contain informations that would not make sense from a semantic (e.g., coherent space) standpoint.

Let us come back to quantified nets; they seem to achieve the ideal of objectivism: although the introduction rule of $\forall X A$ makes use of a context Γ free from X, it is possible to forget this Γ. This means that proof-nets do better than natural deduction: the introduction for $\forall$ being the same as the elimination for $\exists$, this opens the way for a natural deduction without commutative conversions for existence. The problem at stake is the handling of quantifier dependencies and again remember that the question was already treated by Herbrand, unfortunately in an inadequate format. The idea is that of a *jump*: in a $\forall$-link we can either move to the premise or move to any free occurrence of X in the proof. In this way, we find a cycle in the proof-structure corresponding

[24] Quite admissible, since (19) is not Π^1.

to $\exists x \forall y \sim p(x,y) \parr \exists y \forall x\, p(x,y)$:

$$\begin{tikzcd}
{\sim p(x,y} \arrow[rr, dash, bend left] \arrow[d, dash] & & {p(x,y} \arrow[d, dash] \\
{\forall y \sim p(x,y} \arrow[d, dash] & & {\forall x p(x,y} \arrow[d, dash] \\
{\exists x \forall y \sim p(x,y} & & {\exists y \forall x p(x,y}
\end{tikzcd}$$

Jump from $\forall x$ to $\forall y$ and from $\forall y$ to $\forall x$: we get a cycle. I chose the first-order formulation to compare with Herbrand's treatment: $\neg p(x', y) \vee p(x, y')$, with $x' := x, y' := y$ should resist to the substitution $f(x)/y$, $g(y)/x$; the failure of unification is the same as the short-circuit in the proof-net. By the way, the usual semantic refutation of $\forall x \exists y p(x,y) \Rightarrow \exists y \forall x p(x,y)$ is, as usual, a classified subject: in the premise, y depends upon x, in the conclusion, it does not depends upon x; OK if you need side wheels, but admit that this amounts at saying that opium puts you to sleep because of its *dormitive virtue* (Molière making fun of physicians, sort of fregeists *ante litteram*).

Let us, for a while, forget that second-order quantification $\exists X$ involves substitutions of complex formulas for X and let us restrict to substitutions of *literals* $Y, \sim Y$: this is enough for the dependency aspect involved in quantification. Then one is stricken with something: quantified proof-nets (in general, all sorts of proof-nets involving *jumps*) do not follow the orthogonality pattern of multiplicatives. The reason is simple: the jumps, which are part of the logical « control », depend upon the proof — contrarily to the multiplicative case, where the switchings depend upon the sole A. Therefore, we cannot see GoI as a generalisation of quantified proof-nets. Revisiting this old inadequation with transcendental eyes, i.e., searching for conditions of possibility and being unable to avoid jumps made me discover an egg of Columbus: jumps are problematic insofar they are part of the « control » (the switchings), not at all if they are part of the proof. Here we see how objectivism strikes: we are supposed to find the dependencies by an external (thus, metaphysical) look at the proof; why not listing them directly? Of course, this must be done with the right methods, i.e., logically.

As I said, transcendental syntax is not a polishing brush; in particular, it may (and must) correct syntax, at least on some minor points; the nice point is that the idea of condition of possibility is so demanding that it almost imposes a unique formulation! Here the constraints are:

(i) To make jumps appear as part of the proof proper.

(ii) ηxpansion, namely that the extension cord between $\exists X \sim A$ and $\forall X A$ cannot be distinguished from the proof of the same obtained using logical rules on both sides.

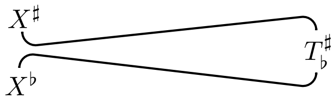
4.2 Gabarits

A new logical artifact, *gabarits* (meaning something like « format », « shape »), the transcendental condition for typing must be introduced. This indeed cor-

responds to a major change in our ontology: up to now, there was only one kind of object, « connexions ». With gabarits, we enter into a dualist world. This choice is the result of a long transcendental reflexion on proof-nets and the conditions of certainty. If you prefer, system **F** proper (à la Church) can only be interpreted by the use of gabarits.

The typical gabarits are the links $\otimes$ and $\parr$. In general, a gabarit is a link with n premises and one conclusion, together with a set of switches. The precise definition is still to be written[25]: basically, if we restrict to combinations of tensors and pars, we lose very little, at least at the level of intuitions. Gabarits are equipped with an involutive negation (typically by swapping tensors and pars). When I say a link with several premises, it implies that the premises are disjoint *sublocations* of the conclusion[26]. Every formula will eventually give rise to a gabarit; hence second-order quantification must be understood as dealing with general gabarits.

Syntactically speaking, a literal $X, \sim X$ (say X) stands for a gabarit; this means that it should be represented as a link X with premise $X^\sharp$ and conclusion $X^\flat$; $X^\sharp$ stands for the unknown premises of X, but, thanks to unification, we can ignore how many and where they are. Indeed, we shall sometimes be unable to write the link X, therefore instead of a direct link between $X^\flat$ and $X^\sharp$, an indirect connexion will be established; *deontic links*, sort of axiom links, will connect $X^\flat, X^\sharp$ to the premise and conclusions of some concrete gabarit T, typically one given by a formula:



This roughly amounts at saying that $X = T$, but not quite!

We first begin with $\exists X A$; the *witness* T becomes a gabarit. The premises of the $\exists$ link are A together with « occurrences »[27] of T and $\sim T$ corresponding to the X and $\sim X$ of A. Deontic links are drawn between T and X, $\sim T$ and $\sim X$ in such a way that, if we swich $T, \sim T$ according to the book, all of this amounts at the same thing as working in $A[T/X]$. Not quite, since a switching of the $\exists$-link rightly consists in independent switchings of the various T and $\sim T$, together with the choice of a premise to which the conclusion should be linked; the choice of A is standard, but the choice of one of the T and $\sim T$ induces a real novelty. ηxpansion helps us to tell the proof (the answer) from the control (the question). Indeed, the occurrences of T are part of the answer: if we remember that gabarits stand for types, we are beyond the Curry paradigm! But the deontic links between X and T cannot be part of the proof: indeed the ηxpanded identity between $\exists X \sim A$ and $\forall X A$ must be a sum of extension cords between their respective locations, what is not the case of the deontic link. One may object that T is not of this form: wrong in the case of ηxpansion, since T will be a variable, thus handled trough a deontic link with the $\forall X$-zone.

[25] However, it already exists, permutationwise in my paper on multiplicatives [4], graphwise by Danos & Regnier [1]. Permutationwise gabarits are more general, since they may accommodate non-commutativity

[26] However, in order to travel between premises and conclusion, a sublocation of the conclusion, disjoint from all premises, must be distinguished.

[27] « Occurrences » is the right word, but remember that this notion is only a convenience, a *façon de parler*, without transcendental status.

In the case of a $\forall X A$, the premises are now A, together with formal gabarits $X, \sim X$, for all all « occurrences » of X and $\sim X$ as literals of A or of some T (or $\sim T$) used as a premise of some $\exists$-link; the proof also uses formal gabarits for the literals $X, \sim X$. Deontic links between the premises $X, \sim X$ and their « origins » (in A and the various $T, \sim T$) are drawn; those not coming from A are part of the proof. The $\forall$-link is switched as follows:

(i) One must first fix a value, i.e., a gabarit G, for X; this gabarit is then used in all occurrences of the unknown gabarit X and its negation $\sim X$, i.e., as premises of the link and as literals of A. Observe that our deontic links will have the effect of propagating our choice of X in the various witnesses who would not know what to do about it otherwise. One can restrict to binary gabarits, so that the criterion remains finite.

(ii) All occurrences of $X, \sim X$ above the rule are switched according to G or $\sim G$; these switchings are, as usual, independent.

(iii) Finally, one of the premises must be selected with an edge to the conclusion. The selection of A is standard, but the selection of premise X induces, using the deontic link, a « jump » to the appropriate occurrence of $X^\flat$ in the appropriate occurrence of T. Now (this must be part of the basic properties of gabarits), there is a particular swiching linking $X^\flat$ to $T^\flat$; moreover the $\exists$-link may select T: in this way, a jump from the $\forall$ to the $\exists$ is created, so that we can handle quantifier dependencies.

4.3 General proof-nets

The immediate thing to do is to revisit all sorts of proof-nets in this spirit; the idea being not that much to erase as much « irrelevant data » as possible, but to provide a notion in which one can normalise without commutative conversions; by normalisation, I mean the use of the GoI formula (6), not a battery of rewritings from your favourite cookbook! The idea is to make part of the proof what is usually considered as control, typically jumps, with the help of this new artifact, gabarits. The test of ηxpansion should help us to avoid bad jokes.

Exponentials: the notion of box should be fixed so as to avoid commutative conversions (boxes nesting inside boxes). Let us remember that natural deduction deals with exponentials, at least in the combination $A \Rightarrow B := !A \multimap B$ without using boxes, hence something might be found there[28]. By the way, remember the issue of *polarisation*, absolutely needed to handle classical logic [6, 11]: there are several morphologies of formulas *linear* (default), *perennial* (!), *co-perennial* (?); these morphologies have not the slightest semantic meaning (typically, a *perennial* formula can be replaced with an equivalent *linear* one), they only deal with the sort of interaction the formula is likely to accept. The last GoI [10, 11] yields a dialectal explanation of polarisation thus suggesting that certain combinations (typically $!A \parr !B$) should be forbidden. There is nothing wrong with this: if language is the description of something else, we must consider everything, including combinations that are notoriously problematic,

[28]But beware: natural deduction cannot handle existence in an adequate way, hence don't expect any miracle!

with a paralysing effect on the theory. If we are looking for conditions of possibility, it is natural to consider that a combination, moreover bizarre, that makes things untractable, should be excluded; this is, by the way, the very sense of *constructivism.* Clearly, ? deserves a gabarit, *which should be seen as part of the proof*, a n-ary combination of dereliction, contraction and digging (inside nested boxes), weakening when $n = 0$; moreover, the case of $n = 1$ with a depth 1 digging makes it ηxpandable.

The problem is with $n = 0$, i.e., with weakening. Weakening is a problematic rule; Lazy Susan (the Chinese turntable), which cyclically proposes the same dishes to every one[29], once presented this problem as a question of « material implication »: the premise should « contribute » to the conclusion. But there is a problem with chopsticks: if you don't know how to use them, mind your socks! Exactly what happened with the semantic, i.e., *prejudiced*, treatment of « contribute »: « How can we toy with models so as to remove weakening while staying classical? », yielding thus *relevance* « logic » — a sort of castrated classical logic, good for nothing but the « publish or perish ». Lazy Susan presented the same dish to to me, first in 1973 (under the form of linearity; I wisely abstained, for want of the right chopsticks) and in 1984 (about the elimination rule of intuitionistic disjunction in coherent spaces): the second time, I used the chopsticks of mathematical culture and handled the question in relation to other structural rules, hence linearity. However, in order to avoid castration, weakening — still problematic — was reintroduced at the level of exponentials: however, the metaphysical expression « contribute » got its a transcendental meaning, that of a *physical* link in the proof-net.

I didn't make my mind on this issue; a solution could be to revert to intutionistic implication (thus coexisting with the linear one), that we cannot handle in the *natural deduction style* which consists in restricting the switchings of $?{\sim}A \parr B$ to the sole « R » and is therefore unable to cope with existential quantification. Indeed, if $?A$ is given together with a gabarit G — in that case, a list of occurrences of A, yielding the *locations* of the premises —, then we should switch by chosing either « R » or one of the premises listed in G, *if possible.* I am not completely convinced by this solution, but one thing makes no doubt to me: $!A \parr !B$ (dually $?A \otimes ?B$) is a semantic monster coming from this realistic illusion, the idea that language is about something else. Syntax is about what works in language, not about what does not work!

Additives: the idea will be to make jumps, weights, part of the proof, as we did for the quantifiers. No need to look for the « optimal » superposition, which, as I observed long ago, is not even preserved by normalisation!

Dependent types: this is the most important issue, since it is clearly the unique way to deal with first order. The official definition is a semantic monster: a predicate would be a *function* from proofs of A to formulas. Such a thing is impossible *a priori*, since it impedes any possibility of certainty. The typical example of the function associating to $t, u \in A$ a

[29] Another example, games. Considering the early time, Gentzen handled them correctly in his first consistency proof, judged inadequate by Bernays. . . But perhaps its was Bernays who was inadequate. On the other hand, the Lorenzen School spoiled their socks: for instance, Felscher was not even able to make his games associative!

trivial formula, true if $t = u$, false otherwise, shows that there can be no way of checking a proof of such a thing. Here we meet a blind spot of Martin-Löf's approach, already responsible for the paradox I found in 1971 in the very first version of the system: it is correct (while semantics should forbid it in the name of logical complexity) to consider expressions $P(t)$, a formula depending upon a proof of A; but the dependencies should be *immediate*, « compilationwise »: a test is not an evaluation, it is a prediction in which we may or not believe (for this use semantics or ask the Pope), a prediction that one can make in constant complexity, say LOGSPACE. The mistake in the original Type Theory was to treat these deontic dependencies as functional ones, whereas they are only formal, virtual, dependencies. In the same way, the *aporia* of Π^0_1 formulas comes from the fact that an inadequate treatment of deontic dependencies corners us into Popperism (infinitely many tests) i.e., to blunt truth.
By the way, there is no emergency in treating full type dependencies; it is enough to understand dependencies of the form $n \rightsquigarrow P(n) \neq Q(n)$, where P, Q are polynomials. The *dependent gabarit* involved should be handled as a finite proof-net. Of course, the solution is not yet at hand, but there is no major impossibility here; to put it in positive terms, the (relative) certainty provided by systems of arithmetics comes from something, that nobody ever analysed, but which must exist!

The process will be completed when it will be possible to deal with these proof-nets on the sole basis of their *locations*, i.e., by ignoring the language. Only when we reach this point shall we have understood the conditions of possibility of language, i.e., the way language works as itself and the not as the coding of something else, which cannot exist, or rather exists as unanalysed language.

4.4 Lewis Carroll and Lazy Susan

The programme of revisiting proof-nets is only sketched, and globally unproblematic. But it poses an important question: proof-nets convey certainty, absolute certainty: they are *apodictic*. However, transcendental syntax proposes proof-nets for full logic, including the non-apodictic Σ^1 (i.e., Π^0_1) formulas. Here the certainty becomes relative, only *epidictic*. This means that our certainty is stained with foundational doubt; how come?

Here we must invoke a strange chap, once professor of logic in Oxford, better remembered as a nonsense writer, a photographer, a ridiculous snob and a pedophile, Lewis Carroll. Lazy Susan presented him the dish of cut-free provability, with which — partly because of his usual shallowness, partly because the time was not ripe — he spoiled his ecclesiatic stockings: read his short story about Achilles and the Tortoise. Slightly simplified and put in modern terms, a cut on A can be replaced with an introduction of $A \Rightarrow A$ on the left; this is technically interesting, since one can replace a general cut with a *Modus Ponens* with a known premise $A \Rightarrow A$: this « Principle of the Tortoise » simplifies the structure at the price of a complication of the formulas, see [11]; a second use of the principle is of no interest, since it only complicates the formulas without further simplification of the structure. To finish with Lewis Carroll, he used the only chopsticks he could handle, that of *nonsense*: he iterated the process by introducing $(A \Rightarrow A) \Rightarrow (A \Rightarrow A)$ on the left... claiming an analogy with

Zeno's, what is preposterous.

A modern reading of this penomenon consists in saying that the cut-free world is a world in which A does not quite match is negation $\sim A$. In other terms, in which we can make extension cords, but not plug them. There are two series of semantic images, useful as side wheels:

Three-valued models: also called *valuations* by Schütte[30] [14, 3, 11]. There is a third (undefined) value $\mathbf{i}$ besides $\mathbf{t}, \mathbf{f}$. The standard (cut-free) interpretation is that A is not false, in particular, $A \vdash B$ reads « if A is true, then B is not false ». This accounts for extension cords (if A is true, it is not false), but not for cut, as soon as the cut formula assumes the value $\mathbf{i}$. Apart from some drawings and archaic cut-elimination results [15], this is good for nothing: truth values work only in the binary case; if you are not convinced, try a three-valued truth table: typically the formula $y \in \mathbf{nat}$ takes the value $\mathbf{i}$ in all *proper* three valued models. But this remains a good picture, A and $\neg A$ are not quite complementary, $A \cap \neg A \neq \emptyset$.

Hexagons: they naturally occur as a category-theoretic explanation of polymorphism. The problem is that, in the hexagon, the preconditions are stronger than the postconditions. From a strong form of A (category-theoretic version of « true ») morphisms lead to a weak version of B (sort of « not false »), hence do not compose [12, 11]. One should not try to fix hexagons, indeed no diagram-chasing can do it. The reason is that this want of compositionality corresponds to a categorical treatment anterior to logic formatting and to morphisms. Logic formatting involving arbitary logical complexity, no diagram chasing can deal with it once for all.

I have been using this idea (under the name *asymmetric interpretations*) in various contexts; typically when the same formula may receive two interpretations, a strong one and a weak one. For instance, in the ancestor of coherent spaces, quantitative domains, finite coefficients (weak) vs. finitely many coefficients, all finite (strong), yielding the result that a normalisable λ-terms has the weak (cut-free) property: all coefficients finite. But I never quite understood how this was fitting the general pattern.

If we come back to quantified proof-nets, we see that we manage to keep the existential quantifier « certain » by specifying a certain witness T (i.e., a gabarit); indeed (at least in the presence of exponentials), it would not be feasible to oppose to a proof of $\exists X A$ the infinitely many possible counterproofs of $\forall X {\sim} A$. This is why we use the witness, which is therefore part of the proof. Indeed, the proof contains several occurrences of T and its negation $\sim T$. The question is thus the following: how can we be sure that the T and the $\sim T$ match?

(i) By drawing an identity link between T and $\sim T$, and asking a local correctness criterion, we can ensure some correlation between them. This would involve additional switchings, but nothing that a proof-net *geek* cannot handle.

[30] He plainly understood that one should not use semantics, but he took the wrong solution: to use models and call them another name!

(ii) But this is only half of the problem; in this way, we get an asymmetrical interpretation: maybe both T and $\sim T$ are too liberal. One should check the analogue of $A \cap \neg A = \emptyset$, but this cannot be done, since too involved.

Here we are; proof-nets with gabarits should, when completed, yield a *tectonic* foundation for logic, a solid ground beyond any discussion. The reason is simple, tectonics being « hexagonal », « three-valued », is not compositional, not deductive. It is thus necessary to sort out the possible gabarits and this is not possible without an act of *authority*. As of to-day, I see the situation as follows:

(i) By regrouping the gabarit of the proof with the formula (e.g., by writing $B[T/X]$ instead of $\exists X B$), we find a battery of gabarits for A; fitting one of these gabarits is enough to ensure that something is a proof of A.

(ii) The battery of gabarits for A and the battery of gabarits for $\sim A$ match, in a duality of the style GoI; in other terms, there are infinitely many tests of unbounded complexity and, in case of a mutual exclusion, the situation is extremely involved.

(iii) However, *modulo* the list of gabarits for A, it is easy to reach certainty.

There we eventually reach *formal systems*: they provide us with appropriate gabarits. The good news is that we thus delimitate a deductive portion of tectonics; the bad (although expected) news is that we no longer reach *absolute* certainty. We can refuse certain choices for theological reasons; by the way, some of them may have been too hastily constructed and be antinomic, i.e., paradoxical. We can even perhaps construct batteries enjoying such and such property: this would be, for once a real semantic (rather deontic) achievement.

The programme is thus not to change the intrinsic limitations of logic such as incompleteness, but to move the furniture in such a way that the real questions, typically certainty, are no longer swept under the rug. It would be interesting to see whether or not there exists a tectonic version of system **F**, i.e., a non-deductive extension of the system containing all possible extensions of system **F**, i.e., all imaginable type extractions, which includes « impossible » types. In the same way, is there a tectonic version of Peano arithmetic, i.e., a non-formated, incorrect, inconsistent system, correponding to the original formulation, which involves induction on « all » properties. In other terms, can we imagine a « pure » typed system, in which the typing would be inconsistent, but that would contain « everything » of a certain kind, a bit like λ-calculus which contains all algorithms of a certain kind, including non-terminating ones. The objection to this idea is that typing is supposedly a *superego* forbidding the deviations of the untyped *id*. Instead of that, I propose to put existence and essence together, essence playing its usual epidictic role w.r.t. existence, while being subject to a dialectics of mutual recusations. Here should lie language proper, with no further external reference. Usual language should be seen as a « safe » part of this language, basically, *modulo* some convenient shorthands, the thing itself.

4.5 Addendum

By the way, there is another mystery in logic I never quite understood: when we deal with system F, we consider terms with variables, what enables one to avoid empty *candidats de réductibilité*, in other terms, we can use the contexts.

Contextual duality rests upon a cut between a proof $\pi \vdash \Gamma, A$ and a proof π' of $\vdash \sim A, \Delta$, yielding a proof $\ll \pi \mid \pi' \gg$ of $\vdash \Gamma, \Delta$; if the syntax is known in advance, this is a perfectly legitimate way of proving, say, normalisation: we require $\ll \pi \mid \pi' \gg$ to be strongly normalisable. But, in a purely existentialist approach, this hardly works: formulas are defined by this duality; $\ll \pi \mid \pi' \gg$ must be a proof, but a proof of what?

The new approach offers a way to fix this, thus an access to contextual proofs: say that the contexts of π, π' are equipped with their own gabarits G, G'. Then $\ll \pi \mid \pi' \gg$ must fit the gabarit $G \parr G'$.

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