LOGIC AROUND THE WORLD

On the Occasion of 5th Annual Conference of the Iranian Association for Logic

Edited by:

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In memory of Avicenna, the great Iranian medieval polymath who made a major contribution to logic.

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PREFACE

The present volume has been provided by the Amirkabir Logic Group on the occasion of 5th Annual Conference of the Iranian Association for Logic (IAL), a unique national event with occasional international contributors which brings many Iranian mathematical and philosophical logicians together. In December 2017, this event took place at the Amirkabir University of Technology, which hosts Amirkabir Logic Group. Currently, the group consists of several faculty members, postdoctoral researchers and graduate students who conduct research in different branches of mathematical logic, including model theory, recursion theory, set theory and modal logic.

The following collection includes several articles in various disciplines of logic and its applications. It is intended to give an introductory view of some particular topics in mathematical and philosophical logic to the general audience in the Iranian logic society in the hope of preparing the ground for possible collaborations between Iranian researchers and their colleagues in other countries along these lines. Also, as a source of inspiration for the researchers and managers of the Iranian logic community, the development history of some successful logic schools around the world, as well as some biographies of internationally renowned logicians, have been added into the contents. Furthermore, the collection contains some brief reviews of logic websites and books of interdisciplinary nature in favor of motivating the readers to follow the related content in future.

Many people have contributed either directly or indirectly to this collection. First and foremost, the editors thank all logician and philosopher fellows of various nationalities who kindly accepted our invitation for contributing to the present book. We would also like to thank William Flesch for the re-publication permission of his submitted article and Heike Mildenberger for her supportive note which has been sent to the editors after her visit of the Institute for Research in Fundamental Sciences (IPM) in April 2017. We also express a special gratitude to Robert Solovay, Tomek Bartoszynski, John Corcoran, Parisa Jahangiri, Anand Jayprakash Vaidya and Keyvan Yahya who assisted the editors during communications with some of our collaborators at the University of California, Berkeley, the University of New South Wales, City University of New York, Dalhousie University, Victoria University of Wellington and University of Bristol respectively. Further thanks to Sylvain Poirier for advertising the Iranian logic groups as well as the present book on his comprehensive logic portal, settheory.net. Also, we thank Jaysankar Lal Shaw, who kindly invited the second editor (as the representative of the Amirkabir Logic Group) to send a message about the present volume addressing the members of the Society for Philosophy and Culture in New Zealand.

Additionally, we would like to extend our appreciation to Vadim Kulikov for his effort to provide an article for us based on an already organized interview with Hugh Woodin. Unfortunately, the interview got canceled due to Prof. Woodin's busy schedule. In the same way, we appreciate Samuel Gomez da Silva's help with organizing a team of Brazilian logicians to provide a paper on the history of Brazilian logic for the present collection. However, this project also got canceled due to some co-authors' lack of time. We also thank Sergei Akbarov and Merve Secgin for their effort in the direction of preparing some articles (with the help of their colleagues) on the history of the Russian school of mathematical logic and the logic activities at Nesin's Mathematics Village in Turkey respectively. Although, both projects failed to get prepared in time due to various reasons. While the editors regret this, we are consoled that these topics, at least, will be documented elsewhere in the future.

Last but not least, we would like to thank Mohammad Golshani for reviewing some of the papers in this collection, Mina Mohammadian for helpful communications with the officials at the Iranian Association for Logic concerning this project, and Atefeh Rohani for reformatting part of the text which enabled us to prepare the earlier draft for publication more quickly. Also, we are grateful to Seyyed Ahmad Mirsanei, the manager of Andisheh & Farhang-e Javidan Publishing, for his close collaboration during the publication process.

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PART 1

TOPICS IN LOGIC AND PHILOSOPHY OF MATHEMATICS

THE ROLE OF DEFINABILITY IN TREE PROPERTY

Ali Sadegh Daghighi

1. Introduction

The present article is a survey of four related works by Leshem [Le], Daghighi and Pourmahdian [DP], Golshani [Go] and Enayat and Hamkins [EH]. It is intended to tackle a specific aspect of the famous tree property project in set theory, namely the role of definability in the long standing open question concerning the consistency of holding tree property at various collections of regular cardinals.

In section 2, we briefly review the literature surrounding the classical tree property problem. Then, in section 3, we explain the new definability approach suggested by Leshem which is actually the basis for further investigations along these lines by the others.

In sections 4, a generalization of Leshem's result by Daghighi and Pourmahdian concerning the case of successor of regular cardinals is explained. In section 5, we discuss Daghighi and Pourmahdian's proof of the case of successor of a singular cardinal and then explain a solution to the general definable tree property conjecture by Golshani.

In section 6, we point out to the work of Enayat and Hamkins who proved the failure of a different version of definable tree property for Ord-trees which itself provides an answer to a question of the author on Mathoverflow [Da] about the limit of potential large cardinal strength of the proper class of all ordinals, Ord.

Finally, a conclusion is added in section 7 comparing the case of definable tree property with the definable version of some other open problems in set theory such as the role of the axiom of choice in the Kunen inconsistency theorem.

2. The classical tree property problem

Tree is a fundamental mathematical concept with numerous applications in various branches of mathematics, science and technology. Most trees that appear in daily mathematics and computer science have a finite number of vertices. These form the set theoretically *small* trees. The theory of small trees is extensively investigated by graph theorists and other mathematicians of various disciplines. But the theory of *large* trees (i.e. those of size $\geq \aleph_0$) turned out to be quite different. It needs essentially different mathematical tools to deal with such large trees. Such tools are mainly developed in set theory and are powerful enough to provide many unexpected discoveries including independence results.

First, let us add a brief historical background of the main tree property problem. It goes back to 1927 when Konig [Ko] proved the following theorem about infinite locally finite connected graphs.

Theorem 2.1. (*Konig's Infinity Lemma*) Every infinite, locally finite (i.e. each vertex has a finite degree) connected undirected graph includes an infinite simple path.

Later in 1955, Beth [Be] independently proved a seemingly weaker but in fact equivalent form of the above theorem.

Theorem 2.2. (*Beth's Tree Theorem*) Every infinite, locally finite (undirected) tree with a root has an infinite simple path.

The property of locally finite infinite trees in Konig's infinity lemma and Beth's tree theorem became isolated as what is called *tree property* in set theoretic literature. Tree property is the property of an infinite regular cardinal κ. Roughly speaking it states that the same phenomenon that happens for locally finite trees of size $\aleph_0$ should hold for locally small (with respect to κ) trees of size κ (aka κ – trees).

Here are the definitions for the sake of completeness:

Definition 2.3. Assume κ is a regular cardinal.

(1) A tree is a partially ordered set $(T, <_T)$ such that for any $t \in T$ the set $\{s \in T \mid s <_T \ t\}$ of T - predecessors of t is well-ordered under $<_T$ and there is a root $r \in T$ such that for any $t \neq r,\ r <_T t$.

(2) For an ordinal α the α th level of T denoted by T_α, is the set of elements of T whose set of T – predecessors has order type α.

(3) Height of T is defined as $ht(T) := \sup\{\alpha \mid T_\alpha \neq \emptyset\}$.

(4) A κ - tree is a *locally small large tree* with respect to cardinal κ, namely a tree $(T, <_T)$ whose underlying set is κ with $ht(T) = \kappa$ and $\forall \alpha \in Ord \quad |T_\alpha| < \kappa$.

(5) A branch b is a $<_T$ - linearly ordered subset of T which is downward closed. i.e. $\forall s, t \in T \ \left(s <_T \ t \in b \rightarrow s \in b \right)$. A cofinal branch is a branch which meets every non-empty level of T.

(6) A κ – Aronszajn tree is a κ – tree with no cofinal branch.

(7) We say κ has the tree property (denoted by $TP(\kappa)$) if there is no κ – Aronszajn tree, equivalently if every κ – tree has a cofinal branch.

The question which immediately arises is whether every regular cardinal has tree property? Soon afterwards it turned out to be a false assumption. Using the Axiom of Choice, Aronszajn [Je] managed to construct an $\aleph_1$-tree with a cofinal branch (also known as an $\aleph_1$ – Aronszajn tree) proving that $\aleph_1$ doesn't have the tree property. This phenomenon contradicts the case of tree property on $\aleph_0$ which follows by Konig's infinity lemma.

Theorem 2.4. (*Konig*) $\aleph_0$ has the tree property.

Theorem 2.5. (*Aronszaijn*) $\aleph_1$ does NOT have the tree property.

Question 2.6. What about $\aleph_2$? Does it have tree property?

First one should note that due to the following results of Specker [Sp] and Baumgartner [Ba], the situation of tree property on $\aleph_2$ may vary depending on the combinatorial properties of the universe.

Theorem 2.7. (*Specker*) For every regular cardinal κ with the property $\kappa^{<\kappa} = \kappa$, there is a κ^+ - Aronszajn tree. Thus $CH \to \neg TP(\aleph_2)$.

Theorem 2.8. (*Baumgartner*) If Proper Forcing Axiom holds then there is NO $\aleph_2$ - Aronszajn tree (i.e. $PFA \to TP(\aleph_2)$).

However, Silver and Mitchell's [Mi] significant result (which has been obtained through separated forcing and inner model techniques) surprisingly revealed a deep relationship between tree property and large cardinal axioms.

Theorem 2.9. (*Mitchell - Silver*) The followings are equiconsistent:

(1) $\aleph_2$ has the tree property.

(2) A weakly compact cardinal exists.

Soon after Mitchell and Silver's result, the investigation into the problem of having tree property on other uncountable regular cardinals brought up much more complexity. For example, Abraham and Magidor [Ab] observed that the consistency strength of having tree property on multiple regular cardinals simultaneously could be greater than the combined large cardinal strength of their separated cases.

Theorem 2.10. (*Abraham*) Assuming the consistency of a supercompact cardinal and a weakly compact above it, it is consistent that both $\aleph_2$ and $\aleph_3$ have tree property.

Theorem 2.11. *(Magidor)* The consistency of having tree property on both $\aleph_2$ and $\aleph_3$ implies the consistency of the large cardinal axiom "$0^{\#}$ exists".

Corollary 2.12. The large cardinal strength of $\mathrm{TP}(\aleph_2) + \mathrm{TP}(\aleph_3)$ is bounded between "$0^{\#}$ exists" (that is stronger than the existence of two weakly compacts) and the existence of a supercompact cardinal and a weakly compact above it.

Continuing this way, Cummings and Foremann [CF] obtained the following result:

Theorem 2.13. (*Cummings - Foremann*) Assuming the consistency of the existence of ω-many supercompact cardinals, it is consistent to have tree property on all $\aleph_n$s for $2 \leq n < \omega$ simultaneously.

Also it turned out that getting tree property on certain regular cardinals such as (double) successor of singular cardinals is generally much harder than the other cases and needs a more sophisticated analysis while keeping an eye on the potential PCF-related issues about singular cardinals. For example, see the following result of Magidor and Shelah [MS].

Theorem 2.14. (*Magidor - Shelah*) If λ is the singular limit of λ^+ - supercompact cardinals then λ^+ has the tree property. Consequently assuming a "very strong large cardinal axiom", it is consistent to have tree property on $\aleph_{\omega+1}$.

Magidor and Shelah's result is actually the first example of obtaining tree property at the successor of a singular cardinal. Later, Sinapova [Si3] reduced the very strong large cardinal assumption in the above theorem to the existence of ω–many supercompact cardinals. Later Neeman [Ne] managed to combine Magidor and Shelah's result with Cummings and Foremann's theorem in order to obtain a uniform consistency result as follows:

Theorem 2.15. (*Neeman*) Starting from ω – many supercompact cardinals it is consistent to have tree property at all successor cardinals in the interval $[\aleph_2, \aleph_{\omega+2}]$.

Another type of important results in this direction is those like the below theorem of Friedman and Halilovic [FH] which deals with *double* successor of singular cardinals rather than single successors. It is actually an extended version of a similar result of Cummings and Foremann [CF].

Theorem 2.16. (*Friedman - Halilovic*) Assuming a weak compact hypermeasurable cardinal, one can get the consistency of having tree property at $\aleph_{\omega+2}$ with $\aleph_\omega$ strong limit.

More along these lines is the following result of Golshani and Hayut [GH] which provides a large cardinal strength upper bound for the consistency of tree property at a countably infinite segment of singular cardinals.

Theorem 2.17. (*Golshani - Hayut*) Assuming the existence of κ^+-many supercompact cardinals where κ is a supercompact cardinal itself, for every countable ordinal η, it is consistent with ZFC+GCH to have tree property on all successors of singular cardinals of the form $\aleph_{\omega.\alpha+1}$ for $0 < \alpha < \eta$.

Inspired by the technique that has been used in the proof of the above theorem, Hayut [Ha] proved a more general result as follows. However, the corresponding paper is still under review.

Theorem 2.18. (*Hayut*) Starting from a stationary set of supercompact cardinals, one can find a generic extension in which the tree property holds at every regular cardinal between $\aleph_2$ and $\aleph_{\omega^2}$.

Recall that in spite of all these theorems, it is NOT consistent that all regular cardinals satisfy tree property (e.g. $\aleph_1$) but the above results suggest that the tree property could be forced in all other cases if strong enough large cardinal axioms are assumed. This observation led Magidor to the famous tree property conjecture which is still open and technically out of reach.

Conjecture 2.19. (*Tree Property Conjecture*) Starting from appropriate large cardinal axioms, it is consistent to have tree property on all regular cardinals except $\aleph_1$.

So much effort has been put into solving this main problem in set theory so far. Many advanced forcing techniques have been developed in order to provide partial solutions for this general conjecture.

The inhomogeneity of the existing techniques for different cases also suggests that a complete solution for the main problem might be of high complexity if not impossible.

Generally, Mitchell's forcing works for the double successor of regular cardinals. For double successor of singular strong limit cardinals, a combination of Mitchell's argument and Prikry or Magidor forcings may work depending on the required countable or uncountable cofinality of the singular cardinals in the ultimate model.

For successor of singulars, there are three different arguments by Magidor and Shelah [MS], Sinapova [Si3] (using Prikry-type forcings) and Neeman [Ne] (via iterated Levy collapses).

In the case of successor of singulars (with possible failures of SCH which is based on an important question of Woodin [Fo]), successive cardinals, even cardinals and odd cardinals, a combination of the above techniques and recent tools such as Gitik – Sharon's diagonal supercompact Prikry forcing [GS] and Sinapova's diagonal supercompact Magidor forcing [Si1, Si2] (for uncountable cofinalities) works.

In the upcoming sections, we are going to investigate a new approach towards tree property conjecture which reveals the role of definability in this problem while demonstrating how a definable version of this long standing open question could be solved with less strong large cardinal assumptions.

3. Definable version of tree property: the case of $\aleph_1$

In 2000, Amir Leshem [Le] realized that the use of the Axiom of Choice in Aronszajn's original construction of an $\aleph_1$ – Aronszajn tree (i.e. an $\aleph_1$ – tree with no uncountable branch) is essential and this fact makes such a tree undefinable.

Then he brought up the idea of making a distinction between definable and undefinable κ – Aronszajn trees and considering definable tree property, a weak version of the usual tree property, as follows:

Definition 3.1. (*Definable tree property*) Let κ be a regular cardinal,

(1) $T = (\kappa, <_T)$ is a definable κ – tree if it is a κ – tree and there exists a natural number n such that the order relation $<_T$ is definable in the structure $(H_\kappa, \in)$ using some Σ_n - formula with parameters from H_κ.

(2) We say that κ has the definable tree property, denoted by $DTP(\kappa)$, if every definable κ – tree has a cofinal branch. Equivalently, if there is no definable κ – Aronszajn tree.

Remark 3.2. If κ satisfies usual tree property, then it has definable tree property as well.

Moreover, Leshem proved that assuming the existence of a Π_1^1– reflecting cardinal κ, which is a very weak large cardinal axiom and a definable analogue of Π_1^1 - indescribable cardinals (aka weakly compact cardinals) one can prove the consistency of definable tree property on $\aleph_1$ through a Levy collapse of κ into $\aleph_1$. Also he showed that $DTP(\aleph_1)$ implies that $\aleph_1$ is a Π_1^1– reflecting cardinal in L and so these two statements are equiconsistent.

Definition 3.3. (*Π_n^m– indescribable cardinal*) An inaccessible cardinal κ is Π_n^m-indescribable, if and only if for every $A \subseteq V_\kappa$ and for every Π_n^m-sentence Φ, if $(V_\kappa, \in, A) \vDash \Phi$ then there is an $\alpha < \kappa$ such that $(V_\alpha, \in, A \cap V_\alpha) \vDash \Phi$.

Definition 3.4. (*Π_n^m– reflecting cardinal*) Let κ be a cardinal. We say that κ is a Π_n^m – reflecting cardinal, if κ is inaccessible and for every $A \subseteq V_\kappa$ *definable* over V_κ (with parameters) and for every Π_n^m - sentence Φ, such that $(V_\kappa, \in, A) \mid = \Phi$ there is an $\alpha < \kappa$ such that $(V_\alpha, \in, A \cap V_\alpha) \mid = \Phi$.

The following folklore theorem of Hanf and Scott provides a bridge between indescribable large cardinals and tree property. It is a source of inspiration for similar results on definable tree property and reflecting cardinals.

Theorem 3.5. (*Hanf-Scott*) The following statements are equivalent:

(1) κ is weakly compact.

(2) κ is Π_1^1 - indescribable.

(3) κ is inaccessible and has tree property.

Next is the main result of this section:

Theorem 3.6. (*Leshem*) The following statements are equiconsistent:

(1) $\aleph_1$ has the definable tree property.

(2) There exists a Π_1^1 - reflecting cardinal.

Leshem's result indicates that even those regular cardinals like $\aleph_1$ that don't satisfy tree property might have definable tree property. So, one can be hopeful to obtain definable tree property at all regular cardinals. In this direction, a definable version of the main tree property conjecture has been proposed in [DP].

Conjecture 3.7. (*Definable tree property conjecture*) Starting from appropriate large cardinal axioms, it is consistent to have definable tree property on all regular cardinals.

In the next sections we show how this conjecture could be solved affirmatively in several partial steps.

4. Definable tree property conjecture: the regular case

The first step in the direction of generalizing Leshem's result has appeared in Daghighi and Pourmahdian's work [DP] where the following generalization of Leshem's result is proved.

Theorem 4.1. *(Daghighi - Pourmahdian)* The following statements are equiconsistent:

(1) For every regular cardinal κ, κ^+ has definable tree property.

(2) There are proper class-many Π^1_1 - reflecting cardinals.

The theorem pinpoints the consistency strength of definable tree property conjecture for successors of regular cardinals. It significantly reduces the expected large cardinal strength of the analogue of this theorem for the usual tree property.

As a sketch of the proof, note that (1) to (2) part could be obtained through an inner model argument in L where the fact that the successor of each regular cardinal has tree property in V indicates that this cardinal is Π^1_1 – reflecting in L which itself implies that the number of Π^1_1 - reflecting cardinals should be unbounded in L.

The (2) to (1) part is also provable through iterated forcing with Easton support of Levy collapses of Π^1_1 - reflecting cardinals in proper class length. The point that makes this forcing work is the preservation of homogeneity of Levy collapse forcing by Easton reverse iterations which allows us to bring down definable objects derived from our definable trees into lower levels of our iteration.

The homogeneity-definability idea in the proof of the above main theorem is actually a recurring theme in most forcing arguments of this type which deal with definable tree property, particularly those which appear in the next section.

Next part deals with the case of definable tree property at the successor of singular cardinals which opens the road to a complete solution for the consistency of the definable tree property conjecture.

5. Definable tree property conjecture: the singular case

The natural step after proving the consistency of definable tree property for the successor of all regular cardinals is to deal with the case of the successor of singular cardinals. Due to the interconnected nature of singular cardinals (revealed by Silver's singular cardinal theorem and PCF-theory), this case is usually more sophisticated in comparison with the case of regular cardinals and needs more advanced techniques.

The first result in this direction that is worth mentioning is the definable analogy of Magidor - Shelah's theorem about $\aleph_{\omega+1}$.

Theorem 5.1. *(Daghighi - Pourmahdian)* Assume GCH holds in V and there is a supercompact cardinal κ with supercompactness elementary embedding j. Let λ be a measurable cardinal above it. Then there is a generic extension of the V like $V[G]$ in which:

(1) κ is a strongly limit singular cardinal of cofinality ω.

(2) $\lambda = \kappa^+$ and the definable tree property holds at λ.

(3) No bounded subsets of κ are added. So GCH holds below κ.

(4) $2^\kappa = |j(\lambda)|$, in particular if $|j(\lambda)| > \lambda^+$, then SCH fails at κ.

The last two parts of the theorem about holding GCH below κ and failing SCH at κ together with having definable tree property at κ^+, provides an affirmative answer to a definable version of an old open question of Woodin and Neeman who after producing a model for failure of SCH at a singular cardinal κ and tree property at κ^+observed that GCH fails cofinally often below κ, and then asked whether it is possible to have GCH below κ in a similar model or not.

The theorem reduces the large cardinal assumption which is needed for proving the consistency of the definable tree property at successor of a singular cardinal. According to Shelah and Magidor's proof and Sinapova's modification of their theorem, definable tree property at the successor of a singular cardinal could be obtained from a large cardinal assumption as strong as the existence of ω – many supercompact cardinals.

The proof is based on the supercompact extender based Prikry forcing introduced by Merimovich [Me] in 2011. Continuing along these lines and using a generalized version of supercompact extender based Prikry forcing, namely supercompact extender based Radin forcing, Golshani [Go] proved the following result:

Theorem 5.2. *(Golshani)* Assume κ is a supercompact cardinal and $\lambda > \kappa$ is measurable. Then there is a generic extension W of the universe in which the following hold:

(a) κ remains inaccessible.

(b) Definable tree property holds at all uncountable regular cardinals less than κ. In particular the rank initial segment W_κ of W is a model of ZFC in which definable tree property holds at all uncountable regular cardinals.

Remark 5.3. In the above theorem the large cardinal strength of λ could be reduced to a Π^1_1-reflecting cardinal.

The corresponding paper is still under review and unpublished yet. If correct, it provides an affirmative answer to the definable tree property conjecture stated in the section 3.

6. Definable tree property for class trees

On one hand, the ordinal numbers are generalization of natural numbers. In this sense the proper class of all ordinals, Ord, is very similar to the infinite set of all natural numbers ω. On the other hand, many large cardinal axioms are actually generalizations of the properties of ω in uncountable realm. Thus ω could be considered a large cardinal in many ways. For instance, ω is a strongly compact cardinal because $L_{\omega,\omega}$ is a compact logic. This fact shows that the nature of ω is very adequate for fulfilling large cardinal properties.

Inspired by these facts the author brought up the following natural question on Mathoverflow in 2013 [Da]. It simply asks whether Ord shows a similar adequacy for accepting large cardinal properties just like its set-sized counterpart, ω, or not.

Question 6.1. How strong can the large cardinal properties of Ord be? Is there any large cardinal property that Ord provably fails to satisfy?

In order to approach this question, one first should note that Ord provably behaves like small large cardinals. For instance, using power set axiom and the axiom of choice it could be shown that Ord is closed under exponentiation and so is strongly limit. Due to the replacement axiom Ord also satisfies a definable analogue of regularity and so in this sense is definably strongly inaccessible in ZFC.

In 2016, Enayat and Hamkins [EH] investigated the large cardinal properties of Ord more extensively. They came across a rather strange behavior of Ord in connection with tree property and weakly compactness.

Their research led to the discovery of an upper limit on the large cardinal strength of Ord by establishing the fact that within ZFC, the proper class of all ordinals never satisfies a certain variant of the definable tree property (different from Leshem's version) and so never can behave like a weakly compact cardinal. The result provided a surprising affirmative answer to the above question which contradicts our intuition about the similarity between proper class Ord and the infinite set ω. The corresponding definition and main theorem are as follows:

Definition 6.2. Suppose $\mathcal{M} |= ZFC$.

(a) Suppose $\tau = (T, <_T)$ is a tree ordering, where both T and $<_T$ are $\mathcal{M}$ - definable. τ is an Ord-tree in $\mathcal{M}$ iff $\mathcal{M}$ satisfies "τ is a well-founded tree of height Ord and for all $\alpha \in Ord$, the collection T_α of elements of T at level α of τ form a set". Such a tree τ is said to be a definably Ord-Aronszajn tree in $\mathcal{M}$ iff no cofinal branch of τ is $\mathcal{M}$ - definable.

(b) The definable tree property for Ord fails in $\mathcal{M}$ iff there exists a definably Ord-Aronszajn tree in $\mathcal{M}$.

Theorem 6.3. *(Enayat – Hamkins)* Let $\mathcal{M}$ be any model of ZFC.

(1) The definable tree property fails in $\mathcal{M}$: There is an $\mathcal{M}$-definable Ord-tree with no $\mathcal{M}$-definable cofinal branch.

(2) The definable partition property fails in $\mathcal{M}$: There is an $\mathcal{M}$-definable 2-coloring $f : [X]^2 \to 2$ for some $\mathcal{M}$ -definable proper class X such that no $\mathcal{M}$ -definable proper class is monochromatic for f.

(3) The definable compactness property for $L_{\infty,\omega}$ fails in $\mathcal{M}$: There is a definable theory Γ in the logic $L_{\infty,\omega}$ (in the sense of $\mathcal{M}$) of size Ord

such that every set-sized sub-theory of Γ is satisfiable in $\mathcal{M}$, but there is no $\mathcal{M}$ -definable model of Γ.

The proof of the last theorem uses model theoretic constructions based on some tools that Enayat has already developed in a few papers. Particularly, the proof of a special case of this theorem has appeared in [En].

7. The conclusion

In nutshell, the results demonstrated in this survey indicate how drastically the nature of the tree property problem changes when one considers its definable version. This is indeed not a new phenomenon in set theory. The same happens about some other open problems of set theory such as the question regarding the essential use of the axiom of choice in the Kunen inconsistency theorem, the statement that there is no non-trivial self-elementary embedding of the universe.

It is an important long standing open question in large cardinal theory to understand whether Kunen's result still holds if one removes AC from the foundations. In fact by a result of Suzuki [Su] it turned out that the definable version of this problem has a simple solution.

Theorem 7.1. (*Suzuki*) Within *ZF* it is provable that there is no non-trivial self-elementary embedding of the universe which is definable from parameters. So the definable version of the Kunen inconsistency theorem doesn't make an essential use of the axiom of choice.

Thus, it is always interesting to consider the definable case of a complicated problem because definability in essence provides a *simple* description of the involved objects which itself provides some possible shortcuts to an eye-opening partial solution. Such partial solutions may shed some light on the main problem by giving an idea of the way things work in a simpler case. They also give a better view of the complexity of the general case with all undefinable objects involved.

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References

[Ab] U. Abraham, *Aronszajn trees on $\aleph_2$ and $\aleph_3$*, Annals of Pure and Applied Logic, vol. 24, pp. 213 - 230 (1983).

[Ba] J. E. Baumgartner, *Applications of the Proper Forcing Axiom*. In "Handbook of Set- Theoretic Topology" (K. Kunen and J. E. Vaughan, eds.). North- Holland Publishing Co., Amsterdam-New York, 913–959 (1984).

[Be] E. W. Beth, *Semantic entailment and formal derivability*, Mededelingen der Kon. Ned. Akad. v. Wet., new series 18, **13**, 309–42 (1955).

[CF] J. Cummings and M. Foremann, *The tree property*, Advances in Mathematics 133, no. 1, pp. 1 - 32 (1998).

[Da] A. S. Daghighi, *How strong are large cardinal properties of Ord?*, https://mathoverflow.net/questions/141119/how-strong-are-large-cardinal-properties-of-ord, Mathoverflow, (2013).

[DP] A. S. Daghighi and M. Pourmahdian, *The definable tree property for successors of cardinals*. Arch. Math. Logic 55, no. 5–6, 785-798 (2016).

[En] A. Enayat, *Power-like models of set theory*, J. Sym. Log. vol. 66, pp.1766-1782, (2001).

[EH] A. Enayat and J. D. Hamkins, *ZFC proves that the class of ordinals is not weakly compact for definable classes*, The Journal of Symbolic Logic, Accepted (2017).

[Fo] M. Foreman, *Some problems in singular cardinals combinatorics*, Notre Dame J. Formal Logic, vol. 46, no. 3, pp. 309–322 (2005).

[FH] S. D. Friedman, A. Halilovic, *The tree property at $\aleph_{\omega+2}$*. J. Symbolic Logic 76, no. 2, pp. 477-490, (2011).

[GS] M. Gitik and A. Sharon. *On SCH and the approachability property*. Proc. Amer. Math. Soc., 136(1):311–320, (2008).

[Go] M. Golshani, *Definable tree property can hold at all uncountable regular cardinals*, Submitted (2017).

[GH] M . Golshani and Y. Hayut, *The tree property on a countable segment of successors of singular cardinals*, Fund. Math. 240, no 2, 199-204, (2018).

[Ha] Y. Hayut, The tree property up to $\aleph_{\omega^2}$, Preprint (2017).

[Je] T. Jech, *Set theory: The third millennium edition. revised and expanded*, Springer Monographs in Mathematics, Springer-Verlag, Berlin, (2003).

[Ko] D. Konig, *Uber eine Schlussweise aus dem Endlichen ins Unendliche*, Acta Litt. Ac. Sci. Hung. Fran. Josep 3, pp 121 – 130 (1927).

[Le] A. Leshem, *On the consistency of the definable tree property on* $\aleph_1$. J. Symb. Logic **65**(3), 1204–1214 (2000).

[MS] M .Magidor and S. Shelah, *The tree property at successors of singular cardinals*, Arch. Math. Logic 35, no. 5-6, pp. 385 - 404 (1996).

[Me] C. Merimovich, *Supercompact extender based Prikry forcing*. Arch. Math. Logic 50 (5–6), 591–602, (2011).

[Mi] W. J. Mitchell, *Aronszajn trees and the independence of the transfer property*. *Ann. Math. Logic* **5**, 21–46. (1972/73)

[Ne] I. Neeman, *The tree property up to* $\aleph_{\omega+1}$, J. Symbolic Logic, vol. 79, pp. 429–459, (2014).

[Si1] D. Sinapova. *The tree property and the failure of SCH at uncountable cofinality*. Arch. Math. Logic, 51(5-6):553–562, (2012).

[Si2] D. Sinapova. *The tree property and the failure of the singular cardinal hypothesis at* $\aleph_{\omega^2}$. J. Symbolic Logic, 77(3):934–946, (2012).

[Si3] D. Sinapova, $The\ tree\ property\ at\ \aleph_{\omega+1}$. J. Symb. Logic **77**(1), 279–290 (2012).

[Sp] E. Specker, *Sur un probleme de Sikorski*. *Colloq. Math.* **2**, 9–12. (1949).

[Su] A. Suzuki. *No elementary embedding from V into V is definable from parameters*. J. Symbolic Logic, 64(4):1591–1594, (1999).

SKOLEM SATISFIED: ON £ AND ₽

Frode Alfson Bjørdal

I have been kindly asked to explain some of the central features and motivations of my work in the foundations of mathematics. I will first relate the alternative *librationist* set theory and theory of truth £ ("libra") which I have come to work out over many years, and I also relate some on the much weaker *volutionary* foundational approach ₽ ("rouble") which I recently discovered and which is also geared to reinterpret arithmetical *incompleteability* and more generally *unsolvability* as genuinely paradoxical phenomena.

Let me apologize somewhat for perhaps making too many references to my own work. This is in part due to accidental time pressure, but it also ended up as a determined choice to give many references to myself on account of considerations that the purpose is not to guide the reader to classical contributions though rather to convey the upshots or gists of the work of the author. I even include more of my works in the bibliographical section than explicitly referred to in the main text as titles of the works are most usually informative, and some also give hyperlinks which may be useful to some.

I parenthetically remark that I started using the pound sign £ only in 2014, so this is not found in earlier texts. My exploration of what I now call *volutionism,* or also *volutionary* arithmetic, started at the end of 2015 and I have come to use the Russian Ruble sign ₽ as a name of the volutionary stance on foundational issues. Moreover, if ⊤ is the particular arithmetical theory being voluted then ⊥ (pronounced "eet") is the name of the volutionary image of ⊤.

A primary motivation for my approach to foundational matters has been the conviction that the standard resolutions of the semantical and set theoretical paradoxes are highly unsatisfactory, and I have always wanted that the Liar's paradox and set theoretical paradoxes à la that of Russell should be treated in a unified way; Bjørdal (2014A) explains best how we can resolve the Liar's paradoxes within the librationist set theory £ and so obtain a unified treatment of the set theoretic and semantical paradoxical quandaries.

Another main desideratum that always was a driving force for me since I began struggling with these matters is that classical logic must be accepted and that it is not to be contradicted even in the presence of the comprehension we want: Let formula ψ be an *antithesis* of a system T if its negation $\neg\psi$ is a thesis of T. Consider theory T^* an extension of theory T if all theses of T are theses of T^*, and consider T^* a *sedation* of T if no thesis of T^* is an antithesis of T. T^* is considered a *sedate extension* of T if T^* is a sedation of T and T^* is an extension of T. £ is a sedate extension of classical logic, as are the much weaker volutionary approaches guided by the volutionary philosophy ₽.

It has therefore throughout these developments always been important to me to distinguish the librationist approach £ – or precedent approaches – and now also the volutionary philosophy ₽ and volutionist systems from the variety of paraconsistent approaches that have been proposed to resolve paradoxes in recent literature.

Moreover, a central motive for the librationist philosophy is that **all** conditions on sets give rise to a well-defined set, and so full self-referentiality is allowed. It is very much on account of this self-referentiality that mathematical strength and usefulness is achieved by £.

A further advantage with the librationist philosophy - which should not be underestimated - is that it allows us to talk about everything, and so £ has universal sets.

It is largely for such reasons as invoked in the preceding paragraphs connected with the desire to get out from the implausibly never ending cumulative hierarchy or kindred caves that the author invoked the word “closures” in the title of Bjørdal (2012).

When we note that a set s is *paradoxical in* £, this means that for some set t both $t \in s$ and $t \notin s$ are theses of £. As an example, if r is Russell's set $\{x|x \notin x\}$ then both $r \in r$ and $r \notin r$ are theses of £. Similarly, a sentence φ is paradoxical in £ if both ϕ and $\neg\varphi$ are theses of £. A sentence ψ is a *maxim* of £ if ψ is a thesis of £ and also an antithesis of £.

£ does not have the adjunction (“conjunction” in the majority of idiolects) $\varphi \wedge \neg\varphi$ as a thesis for any formula φ. Thence the inferential principles of £ are novel, and the reader is sent to Bjørdal (2012) to study these; here we just remark that *modus ponens* is not a generally valid inference principle for £.

I adapt some from page 18 onwards of the current 8th version of Bjørdal (2014A) in order to convey some on how I have attempted to think about these odd and confusing things just noted as concern the failure of adjunctivity in £:

“A sentence φ is taken as a *contrapresentive* thesis of a theory iff φ is both a thesis and an antithesis of the theory. Let us agree that *a theory is contrapresentive* iff it has contrapresentive theses. A theory is trivial iff all sentences of its language are theses. Trivial systems and inconsistent theories with simplification or adjunction elimination are contrapresentive. £ is contrapresentive, but neither trivial nor inconsistent. *Contrapresentationism* is the view that a contrapresentive theory, such as £, is true.

We say that two formulas φ and ψ of a theory T are *connected* iff φ and ψ are theses of T only if also the adjunction $\varphi \wedge \psi$ is a thesis of T. Two sentences disconnect with each other iff they do not connect with each other. A sentence is disconnected iff it disconnects with some sentence. A set b is disconnected iff for some set a the sentence $a \in b$ is disconnected. Paradoxical theses of £ are disconnected theses of £, and vice versa. It is straightforward that if φ is not a thesis of £ then φ connects with all sentences, and further that all maxims connect with all sentences. All sentences are self-connected and the relation *connects with* is also symmetric, but not transitive. A theory is disconnected iff it has disconnected theses. A topic is disconnected iff a true theory about it is disconnected and *disconnectionism* is the view that there are sound disconnected theories. Some paraconsistent logics, such as the ones following the approach by Jaskowski, are *non-adjunctive*. But such logics do not in and of themselves have disconnected theses, though extensions of such logics with suitable comprehension principles or semantic principles may be disconnected if not trivial."

We say that a theory is *prosistent* iff it is not the case that there is a sentence ψ which is a thesis as well as an antithesis of the theory, and else the theory is *contrasistent*. A theory is inconsistent iff some antilogy (negation of a tautology) is a thesis of the system, and else it is consistent. Typically, a theory will be prosistent iff it is consistent; but £ is contrasistent and consistent, as is the volutionary approach we relate below.

In my opinion the Zermelo-Fraenkel axioms in classical set theory is a nearabout haphazard collection which was isolated in perhaps an ad hoc manner, but one might consider that the original Zermelo axioms were chosen by Zermelo on the basis of their usage in some important mathematical constructions accessible to him. To me it has always seemed that the prevailing use of ZF-like axioms of set theory at best could have a preliminary justification as it may account for structures as mathematicians came to believe in on account of Cantor's work and that of others and in rare cases on account of their own mathematical practices.

It is true that a semantical sort of justification came to be attempted for even ZFC with the postulation of a so-called *cumulative hierarchy* by Zermelo in 1930, i.e. 22 years after the publication of his original axioms; this gave birth to the purported *iterative conception* of sets. Clearly informal semantical ideas akin to the iterative conception of sets were also at play in the independent work of Skolem and Fraenkel to suggest the replacement axiom schema, and as well in the postulation of the axiom of regularity or foundation by von Neumann in 1925.

From my point of view, there ***is*** no intended model for ZFC or even Zermelo set theory or even Second-order arithmetic. This is because the power set operation in £ is *always* paradoxical in the sense that $\{x|x \subset u\}$ is paradoxical in £ for any set u of £; for this see section 7 of Bjørdal (2012). Librationism radicalizes the predicativist doubts concerning power sets, and to my mind gives a more precise rationale for why we should not let our theories be over-powered by the assumption that some sets have non-paradoxical power sets.

However, I do not believe that ZF is inconsistent; rather, I think that at most just countable Henkin style models of ZF exist. I have more precisely come to think that the most reasonable model of a formal logical or mathematical theory simply **is** a real number, and I then use the term "real number" as in set theoretic *parlance* for "set of natural numbers". The justification for this way of thinking in terms of what I consider ***real models*** is that we by means of appropriate coding techniques may identify the formulas of the formal language of the system to be modeled with natural numbers from the external point if view. In Bjørdal (2014A) these ideas are made more precise.

By means of a Herzberger style revision semantics for £ we at its closure ordinal Ϙ (Koppa) have a correlated real number ⊧ (Ϙ) so that a natural number associated by the external coding with a formula is in the real number ⊧ (Ϙ) iff it is to hold by the (minimalistic) librationist semantical set up.

In our account in Bjørdal (2014) of the closure ordinal we call Ϙ it is assumed to be Σ_3-admissible for expositional reasons that connect with elegance of proofs, but less is needed according to results by Welch (2011). Welch and others have also related to me in private communications that the Infinite Time Turing Machines and the Herzberger style semantics can simulate each other, but at the moment I do not have a reference for this.

It bears mentioning that £ is fully impredicative and has a *proof theoretic style* ordinal above the ordinal Takeuti gave for $\Pi_1^1 - comprehension,$as Bjørdal (2012) shows how £ accounts for PA + transfinitely iterated inductive definitions + virtually all of Bar Induction in manners not infested by paradoxicality, and thus surpasses the Big Five of the Reverse Mathematics Program in strength.

Bjørdal (2014A) shows that £ + *The Skolem Cannon* + *The Fraenkel Postulate* gives an interpretation of ZFC **if** ZFC is consistent through extending an interpretation of ZF by Friedman (1973) in a system S which is ZF minus extensionality with collection and *weak* power.

In £ the novel operation librationist *capture* is instrumental in appropriate restricted contexts where it entails *collection*, *specification* and *choice*. The novel impredicative operation *domination* is based upon a utilization of the librationist truth predicate, and domination supplants the paradoxical power set operation in £. The domination operation invokes the impredicative fixed point operation we call *manifestation point* that was articulated without that name for precedent type free systems in Cantini (1996) and with roots in Visser (1989) and earlier work by Kleene and Gödel.

We show in Bjørdal (2014) how we may combine the use of librationist capture and domination to isolate precisely the definable real numbers in £, and the domination operation ensures that the definable real numbers are Dedekind complete. Importantly, an isolation such as we provide of definable real numbers in £ is not possible in classical set theories by results of Hamkins et al. (2013). Future work will in part concentrate upon exploring topological consequences and repercussions from the fact that the domination set of the set of definable real numbers is a σ-algebra in order to generate measures with desirable features. The hope is that these and related matters will help shape *definable analysis*.

Moreover, if v is a non-paradoxical empty set of £ then already $\{x|x \subset v\}$ has infinitely many non-paradoxical members. Notice that by this there are infinitely many distinct non-paradoxical empty sets. So extensionality fails essentially in £, and for all extensions. On these matters, consult primarily Bjørdal (2012).

It follows almost as a corollary from the above that Cantor's arguments do not provide for uncountable sets according to £. I emphasize that Cantor's arguments are completely valid reductio arguments, but they both smuggle in paradoxical assumptions so that according to £ the conclusion that there are uncountable sets is not justified. However, £ agrees that there are sets which are not *listable*. Bjørdal (2012) so far gives the best librationist account on these anti-Cantorian or para-Cantorian facets of librationism.

Nevertheless, I purport to have shown in Bjørdal (2014A) that £ *believes* that it has a standard model of ZFC **if** ZFC is consistent. A kindred construction going up to Mahlo cardinals is related in *Elements of Librationism*, so that £ *believes* that it has a standard model of ZFC plus the Mahlo-axiom in question **if** the latter is consistent. Nevertheless these conditional interpretations £ has of strong theories are countable from the perspectives outside the restricted quantifiers, and so there is nothing *Skolem-untoward* with there being such intepretations in £ if the theories mentioned are consistent.

In the 1990s I already had some inklings on how to go about in a novel and unified way concerning the paradoxes, but this was still imprecise and programmatic such as in Bjørdal (1998) as I had the disadvantage and advantage of being trained foremostly as a philosopher and not as a mathematician. I already in that decade came to think that Cantor's proofs of the existence of uncountable sets are really genuine paradoxicalities dressed up in sheeps' clothing as something else, but it was not until Bjørdal (2005) that I published a clear statement that related a precedent of £ in justification of a *denumerabilist* philosohy rejecting Cantor's conclusion without rejecting the validity of his arguments.

As we know, Cantor's entirely valid arguments have exercised a profound and lasting influence upon mathematics up until today, but I believe and hope that the non-absoluteness and other implausible features of classical set theory and increased interest in computational issues will diminish the interest in the Cantorian points of view.

I finally write some on the volutionary philosophy Ᵽ and related volutions of particular arithmetics, and I do this by adapting from the abstract of my recent talk Bjørdal (2017) and other recent works that just were submitted.

At Trends in Logic XVI in Campinas and after in seminaries at *Universitetet i Oslo* and at *Universidade Federal do Rio Grande do Norte* I talked about *volutionary* arithmetics which shifts attention to the set $\perp$ of sentences whose negation are not theses of the formal arithmetic $\top$ as traditionally conceived; we presuppose that $\top$ is axiomatized so only sentences are derivable and only modus ponens is a primitive inference rule. It is straightforward to give such a *sententialist* axiomatization given the insight that we only have to presuppose *modus ponens* as a primitive inference rule to axiomatize first order logic; the latter idea can be traced back to Tarski and found exposed e.g. in Hunter (1971) and Enderton (1972).

Volutionism suggests that we alter how to think about fundamental matters e.g. in that the standard Gödel sentence of $\top$ in $\perp$ is taken as a textbook liar sentence, and so the volutionary turn gives occasion to reinterpret issues concerning decidability and computability as other sentences independent of $\top$ are treated similarly; this is connected with predicative limits, as hinted towards below.

As we said above volutionary systems cannot be subsumed under so-called paraconsistent approaches. Nevertheless, if γ is the standard Gödel sentence for $\top$ both γ and $\neg\gamma$ are theses of $\perp$; so *modus ponens* does not, but exotic induced inferential principles *modus maximum, modus conditionalis* and *modus antecessor* hold for $\perp$.

The volutionary resolution of paradoxes has some similarities with that of the £ as related above. A deviant volutionary truth predicate $\boldsymbol{Tr}_{\perp}[\varphi]$ defined as $\neg\boldsymbol{Pr}_{\top}[\neg\varphi\,]$ is introduced, and it is straightforward to show that φ is a thesis of $\perp$ iff $\boldsymbol{Tr}_{\perp}[\varphi]$ is a thesis of $\perp$. Moreover, we have minimal justifications (see next paragraph) for the *Reflection-schema* $\boldsymbol{Tr}_{\perp}[\varphi] \to \varphi$ and what we dub the *Flection-schema* $\varphi \to \boldsymbol{Tr}_{\perp}[\varphi]$ being theses of $\perp$.

Let formula ψ with just the free variable x be Δ_1*-limited in*$\top$ precisely if it is a Σ_1-formula and $\top$ proves $\forall n(\varphi(n/x) \leftrightarrow \psi(n/x))$ for some Π_1 formula ψ with just x free. Recall predicate sub in Smorynsky (1977) p. 837 which functions so that $sub([\varphi(v_i)], i, [t]) = [\varphi(t)]$. Let $SUB(x, y, z) = sub(x, i, z)$ if $y = [v_i]$, else 0. Let PAIR be Cantor's pairing function and the projections of a natural number $m = PAIR(m_0, m_1)$. $\{x|\varphi\}$ is short for $PAIR([\varphi], [x])$. $s \in t$ is an abbreviation of $\boldsymbol{Tr}_{\perp}(SUB(t_0, t_1, [s])$. A thesis of $\perp$ is *maximally* justified if also a thesis of $\top$, and else just *minimally* justified. We have shown that there is a minimal volutionary justification for $(\forall x)(x \in \{y|\varphi\} \leftrightarrow \varphi(x))$ if $\varphi(x)$ is Δ_1-limited in $\top$, and thence of a volutionary style recursive comprehension $\exists\, y\forall x(x \in y \leftrightarrow \varphi(x))$.

The previous paragraph point to matters that may be of interest combined with the weak completeness theorem of RCA_0 in Simpson (1999) p. 93 and other weak assumptions to secure some sort of volutionary aritmetical models for stronger theories of interest. An ingredient of such an approach would be to have $\top$ already include a transfinite progression of consistency statements upon a base arithmetic as with Feferman (1962) and earlier Turing (1939).

The highly predicativist and iconoclastic volutionist philosophy ₽ may be justifiable, and it has an interest in the way it reinterprets foundational phenomena. But these are matters which must be considered and weighed carefully, and it may on the other hand be that impredicative systems such as £ are justified by their own impredicative philosophies.

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References

F. A. Bjørdal: *Towards a Foundation for Type-Free Reasoning*, in T. Childers (ed.) *The LOGICA Yearbook 1997*, FILOSOFIA , Prague, 1998, pp. 259-273

F. A. Bjørdal: *There are only Countably Many Objects*, in M. Bilkova and L. Behounek (eds.) The Logica Yearbook 2004, FILOSOFIA, Prague, 2005, pp. 47-58.

F. A. Bjørdal: *Considerations Contra Cantorianism*, in M. Pelis and V. Puncochar (eds.) *The LOGICA Yearbook 2010*, College Publications, London 2011, pp. 43-52.

F. A. Bjørdal: *Librationist Closures of the Paradoxes*, in *Logic and Logical Philosophy*, Vol. 21 No. 4 2012, pp. 323-361.

F. A. Bjørdal: *A Sober Librationist Interpretation of ZF*, a lecture for the Non-Classical Mathematics workshop at the 4th World Congress and School on Universal Logic in Rio de Janeiro, Brasil, March 29 - April 7, 2013

F. A. Bjørdal: *Elements of Librationism*, technical preprint at Arxiv.org, http://arxiv.org/abs/1407.3877. 1st version 2014; to be updated more before submission.

F. A. Bjørdal: *Definable Real Numbers with Librationist Capture and Domination*, a lecture for the *Encontro Brasileiro de Lógica* in Petrópolis, Brazil, April 8th 2014.
https://www.dropbox.com/s/bmxo591ls867yok/Definablenumers.pdf?dl=0

F. A. Bjørdal: *Capture, Collecton, Choice and Domination*, a lecture for the *Seminary in Logic* of the *Russian Academy of Sciences* at the *Steklov Institute of Mathematics* in Moscow, September 4th 2014. B
https://www.dropbox.com/s/rmkukdupuvvsror/Steklovbeamer.pdf?dl=0

F. A. Bjørdal: *On the Type Free Paracoherent Foundation of Mathematics with the Sedate Extension of Classical Logic by the Librationist Set Theory*

£, and specifically on why £ is neither Inconsistent nor Contradictory nor Paraconsistent, in J.-Y.Beziau and M.Chakraborty and S.Dutta (eds.) *New Directions in Paraconsistent Logic*, Springer 2015, pp. 509-515. A

F. A. Bjørdal: *The Librationist Domination of Second Order Arithmetic*, a lecture for the *Congress on Logic, Methodology, and Philosophy of Science* at the University of Helsinki, August 2015. B

F.A. Bjørdal: *Volutionary Foundational Ruminations*, a lecture for *Encontro Brasileiro de Lógica XVIII* in Pirenópolis, Brazil, May 2017.

A. Cantini: *Logical Frameworks for Truth and Abstraction*, Studies in Logic and the Foundation of Mathematics vol. 135, Elsevier, 1996.

H. B. Enderton: *A Mathematical Introduction to Logic*, Harcourt/Academic Press, 1972.

S. Feferman: *Transfinite Recursive Progressions of Axiomatic Theories*, in *The Journal of Symbolic Logic* 27(3), 1962, no. 3, pp. 259-316.

H. Friedman: *The Consistency of Classical Set Theory Relative to a Set Theory with Intuitionistic Logic*, in *The Journal of Symbolic Logic* 38(2), pp. 315-319, 1973.

J. D. Hamkins, D. Linetsky, and J. Reitz: *Pointwise definable models of set theory*, in The *Journal of Symbolic Logic* 78(1), pp. 139-156, 2013.

G. Hunter: *Metalogic: An Introduction to the Metatheory of Standard First Order Logic*, Berkeley, University of California Press, 1971.

S. G. Simpson: *Subsystems of Second-Order Arithmetic*, Springer 1999.
C. Smorynsky: *The Incompleteness Theorems*, in Jon Barwise (ed.) *Handbook of Mathematical Logic*, North Holland 1977, pp. 821-865.

A. M. Turing: *Systems of Logic Based on Ordinals*, in Proceedings of the London Mathematical Society, ser. 2, vol. 45, pp. 161-228, 1939.

A. Visser: *Semantics and the Liar Paradox*, in D. Gabbay and F. Guenthner (eds.) *Handbook of Philosophical Logic* vol. 4, D. Reidel 1989, pp. 617-706.

P. D. Welch: *Weak systems of determinacy and arithmetical quasi-inductive definitions*, in *The Journal of Symbolic Logic* 76 (2):418-436.

SET THEORETIC PLURALISM

Toby Meadows

Over the last decade or so, pluralistic interpretations of set theory have reappeared in discussion of the philosophy of set theory and set theory's role in the foundations of mathematics. My goal in this article is to discuss the underlying motivations for such views and to examine how different programmatic agendas lead to different approaches to pluralism in this context. To give the article some shape, I will take three set theorists and their philosophies as case studies: Saharon Shelah, Joel David Hamkins and John Steel. Each of these set theorists espouse versions of pluralism which reflect quite distinct motivations and aspirations.

However, I want to stress up front that the goal of this paper is to draw out philosophical positions rather than undertake painstaking exegesis. Each of these set theorists draws on a deep background of set theoretic research from which they have developed their philosophical attitudes. The results are rich and subtle. As such, I will be forced to ignore some aspects of their views by smoothing off some of the more baroque (and fascinating) details. As a result, there may be a certain amount of straw and caricature in my depictions of these positions. On the positive side, I hope that this simplistic approach will make clearer the intense interaction between philosophical attitude and set theoretic outlook.

1. Why pluralism? ... The folk story

In the beginning there was Cantor's theorem. It told us that there could be no surjection from the natural numbers onto the real numbers. Or more picturesquely, there are more real numbers than natural numbers. A staggering result: there are different sizes of infinity! But as soon as one has taken in this revelatory result, another (very natural) question should leap to mind:

Question 1. Is there some size of infinity strictly between that of the naturals and the real numbers?

This, of course, is the question underlying the infamous Continuum Hypothesis (CH). If the answer is no, then CH is false. At first blush, it's the obvious question to ask. We've just opened the door into the realm of the transfinite; we've seen that the naturals and the reals have different sizes; is there something in between? Moreover, the question appears to be an innocent and interesting combinatorial question outside the self-referential tricks bag wielded by logicians. It seems like a good mathematical question deserving of a good mathematical answer.

Unfortunately, the question has proven extremely difficult, if not, utterly intractable. First, we learned that our foundational theory, ZFC has nothing to say on the matter.

Theorem 2. Suppose ZFC is consistent. Then:

(i) $ZFC \nvdash \neg CH$ (Godel); and

(ii) $ZFC \vdash CH$ (Cohen).

The first result was established using the constructible hierarchy, while Cohen's result was proven using the enigmatic technique of forcing. It would be fair to say that Cohen's result was surprising to the mathematical community at the time. Indeed, a period of anxiety ensued in which this newly discovered weakness of the ZFC axioms was thought to imply that our interpretations of set theoretic vocabulary are forced to bifurcate. One might interpret this as an early form of the pluralism discussed in this article. However, this line of thought quickly retreated - with Cohen - from a metaphysical form of pluralism back to the curious comforts of formalism.

Despite this, hope was not lost. Just because ZFC cannot solve CH, perhaps some extension of ZFC can. So began the quest for new axioms. Arguably most prominent in this campaign has been the

Large Cardinal Programme. The extension of ZFC with such cardinals has yielded a surprising array of results with noticeable impacts in areas of ordinary mathematics. Here is an example of a more recent addition to that suite:

Theorem 3. If there are infinitely many Woodin Cardinals, then every set of reals definable in the language of analysis is Lebesgue measurable.

With results like this and others to hand, one might have been tempted to hope that it was just a matter of time before CH too succumbed. Alas, Cohen's forcing technique slams the door on this project too. For example,

Theorem 4. Suppose ZFC + "there is a measurable cardinal" is consistent. Then

(i) ZFC + "there is a measurable cardinal" $\nvdash \neg CH$;

(ii) ZFC + "there is a measurable cardinal" $\vdash CH$.

At heart, this results from the fact that measurable cardinals are impervious to the effects of small forcing. Moreover, this result generalizes to all other known large cardinals! This limitative result raises a thematic question for this article:

Question 5. What do these results tell us about the nature of set theoretic research and the interpretation of its language?

1.1. Into the multiverse.

The kinds of pluralism with which we are concerned emerge out of this and related problems. But first, it will be useful for the ensuing discussion to make a couple of contrasting definitions:

Definition 6.

(i) A universe interpretation of set theory asserts that there is one correct way in which the 2 relation can be interpreted.

(ii) A multiverse interpretation of set theory asserts that are many ways in which the 2 relation can be correctly interpreted.

The universe interpretation is arguable the de facto standard view regarding how the language of set theory should be interpreted. And there is a lot to commend in the universe view: it's heuristically simple and elegant; it lines up with naive intuitions; and it appears to line up with mathematical practice.

By contrast multiverse interpretations are frequently difficult to get one's head around. How could it be possible that the seemingly straightforward concept of membership has multiple meanings? Why do people entertain such views? In response, we propose the following argument template:

(1) Problems like CH appear to deserve answers but are intractable.

(2) This intractability is evidence that our understanding of sets is incorrect.

(3) We should see that our notion of set is under-determined and as such, acknowledge a multiplicity of different interpretations for the membership relation.

We should pause here a moment to note the philosophical linchpin in this argument. We are arguing that the epistemic difficulties of set theory are so great that they warrant a revision of out *metaphysical* outlook on the field. More bluntly, it appears that we cannot know these things, so perhaps there is nothing to know!

Another thing to note is that the argument template - as it stands - is very weak. Just because we haven't found an answer to some question is hardly a good reason - in general - to think that there is no answer. As such the move from (2) to (3) needs some bolstering.

For the remainder of this article, I am going to explore three ways of doing this, through:

(1) the limitations of justification;

(2) a deferral to practice; and

(3) an alternative perspective on the large cardinal programme.

2. Intrinsic Limitations: Shelah

My case study for this section is Saharon Shelah's the Future of Set Theory [Shelah, 1991].

In this is an enigmatic and fascinating paper we are given an insight into a perspective on set theory from one of the world's most gifted logicians. I am particularly concerned with two remarks made in this paper and I am going to use them to lever out an argument for a certain kind of multiverse interpretation of set theory.

Shelah outlines five major methodologies of set theoretic research:

(1) ZFC;
(2) forcing;
(3) inner models;
(4) large cardinals;
(5) ZFC + DC+some form of determinacy.

He then remarks:

> *From the point of view of adherents of ZFC (1 above) (and I tend to agree to a large extent) proving a theorem means proving it in ZFC, and the other attitudes are supplementary; forcing (2) is necessary to tell us when we cannot prove a theorem, large*

> *cardinals (4) are needed in some consistency proofs and - by a happy coincidence - they are ordered on a linear scale. Finally, inner models (3) are used to show that large cardinals are necessary and, even better, to get equiconsistency results. [Shelah, 1991]*

For our purposes, the key point here is that real theorems are those proven in ZFC and the rest of these methodologies should be understood as ancillary. By way of explanation, he remarks:

> *My feeling is that ZFC exhausts our intuition except for things like consistencystatements, so a proof means a proof in ZFC. [Shelah, 1991]*

One way to unpack this is to consider Gödel’s distinction between intrinsic and extrinsic justification. Typically intrinsic justification is delivered via self-evidence or some kind of conceptual analysis of the meaning of the terms in our language. One might say that the Axiom of Extensionality is intrinsically justified in that it is just part of the meaning of “set” that if two putative sets have the same members, then they are - in fact – the same set. Extrinsic justification, on the other hand, is wrought from the fruitfulness of the consequences of the principle involved. For example, one might argue that the Axiom of Choice is extrinsically justified on the basis of its value in set theory and mathematics at large. In general, if intrinsic justification is working well, it should be much stronger than extrinsic justification, which is obviously and intentionally defeasible.

It seems clear that intrinsic justification is the most pertinent to Shelah's remark above. He appears to be saying that we are able to intrinsically justify ZFC but no more (with the possible exception of statements like Con(ZFC) or perhaps the existence of small, large cardinals like inaccessibles). This move is not without precedent if we widen our gaze. With regard to arithmetic, Dan Isaacson has argued that PA represents something like the intrinsic limit of the arithmetic sentences [Isaacson, 1992]. Thus, Rosser sentences are to be understood as true, but not really arithmetic: some extra-arithmetic content is at play in these cases. Analogously in the case of set theory, we see Shelah arguing that statements like CH or the existence of $0^\#$ are beyond the range of our intrinsically justified principles.

If we return to our argument template, we see that Shelah has bolstered the move from (2) to (3) by arguing that there isn't any intrinsic justification that could settle a question like CH and as such, these questions don't really deserve answers after all. Implicit in this argument, is the idea that the defeasibility of extrinsic justification renders it unsuitable to the project of selecting suitable axioms for a foundation of mathematics.

How then does this lead us to a multiverse position? It should be noted that nothing of the kind is suggested in Shelah's paper. But if we follow through on the argument template, it's not difficult to see where we might end up. In accepting ZFC as the limit of proper justification we open ourselves to the acceptance of multiple interpretations of the 2 relation. We might wonder which interpretation should be understood as acceptable for such a position, but that interesting question is outside the scope of this paper. We, thus, have a motive and argument for a certain kind of multiverse position.

2.1. A bump in the carpet.

Despite saying that I will not be particularly concerned with exegesis in this article, it would be remiss of me not to discuss the following remark that Shelah makes with regard to where his comments leads him. He asks rhetorically:

> *Does this mean you are a formalist in spite of earlier indications that you are a Platonist? [Shelah, 1991]*

and responds

> *I am in my heart a card-carrying Platonist seeing before my eyes the universe of sets, but I cannot discard the independence phenomena. [Shelah, 1991]*

This seems like a clear declaration of a deep-seated commitment to a universe interpretation of set theory. What is going on here?

A distinction in philosophical logic is helpful here. In the literature on indeterminacy and vagueness, there are two mainstream approaches to the solution of the Sorites paradox: supervalution and epistemicism. The supervaluationist deals with indeterminacy in the meaning of some term by considering all of the different ways in which the meaning of that term could be filled out. In our context, this is the standard form for the multiverse interpretation of set theory. We have indeterminacy - as witnessed by the intractability of questions like CH - and we deal with this indeterminacy by admitting multiple interpretations of the membership relation. The epistemicist, on the other hand, looks at indeterminacy - not as a limitation of meaning - but rather as a limitation of knowledge.

So the epistemicist admits that for all they know any one of a multiplicity of different interpretations of set theory could be the right one. And that's the salient distinction: there is a right one; we just can't know which one it is.

I think the right way to understand Shelah is that he is an epistemicist with regard to how set theory should be interpreted. He believes that ZFC is the upper bound on what can be properly justified; and as such, this is the point at which knowledge rightfully ends. But he also believes that there is one correct interpretation of set theory.

Thus, we see that even though Shelah is in a position to make an argument for a multiverse interpretation, he does not carry it out. Rather than going on and revising metaphysics on the basis of intractable epistemology we hold on to the universe view. Clearly, this leads us to a divergent, non-pluralistic view, but I don't think we should take this as any reason to reject the argument sketched in the previous section. Rather, we are seeing a further division in the logical space of solutions to our motivating problems.

3. Almost Naturalism: Hamkins

Our next case study emerges with the work of Joel David Hamkins who has perhaps been the most vocal proponent of multiverse interpretations in recent years. An excellent overview of his perspective can be found in his paper the Set-Theoretic Multiverse [Hamkins, 2012]. In it, one finds: a number of arguments for his view; examples exploring relationships within and without set theory; and counterarguments to possible objections. Continuing in this spirit of this article, I am going to focus on one particular aspect of Hamkins' view. In this, I _nd the most convincing arguments for Hamkins' views. Moreover, I think there is room for some variations in this area. I'll start with a quote:

> *While group theorists study groups, ring theorists study rings and topologists study topological spaces, set theorists study the models of set theory. [Hamkins, 2012]*

The idea here is that set theorists study models of set theory in the same way various other mathematicians conduct their research. They are not so much concerned with the inner workings of particular groups, rings or topologies as they are with the relationships between such structures. He goes on,

> *Set theory appears to have discovered an entire cosmos of set-theoretic universes, revealing a category-theoretic nature for the subject, in which universes are connected by the forcing relation or by large cardinal embeddings in complex commutative diagrams, like constellations filling the night sky. [Hamkins, 2012]*

Surely anyone familiar with contemporary set theory has some affinity with the rather pleasing picture Hamkins paints. How does this lead us into the multiverse? There are two parts to the argument: first, we need some methodology for understanding the ontology of a particular discipline; second, we need to apply that methodology.

As to the first question, Hamkins asks us to defer to the experts on the topic at hand: set theorists. Just as we defer to physicists with regard to which subatomic particles exists; or biologists on which collections of organisms constitute a species; we should defer to the set theorist on the topic of set theoretic ontology. This kind of move is familiar in philosophical circles and often goes under the name naturalism. The idea is that (via Quine) we regard ontological questions as being on a par with questions of natural science.

As such, if our best scientific theories tell us that some entity exists then we should accept that such an entity exists.

But some care is required here. In general, mathematics' - an in particular set theory's - position in the pantheon of natural sciences is a little vexed in the philosophical literature.

In particular, Quine was notably reluctant to admit set theory and its excesses. As such, we must admit that the kind of naturalism presented here is somewhat unorthodox, although by no means to be ruled out of court.

This leads us to the second part of the argument: applying the methodology. Hamkins' claim is that if we defer to the practice of set theorists, then we shall see that models of set theory are an integral part of the metaphysical furniture. And as such, they should be admitted into the ontology of this field. The question of whether set theoretic practice is like this is, of course, a difficult one. Perhaps the model theory of set theory is just that: a part (albeit an important one) of set theory, but not so large a part that it should override its original ontological outlook. On the other hand, it is difficult to find much space in set theory untouched by model theoretic techniques. I don't propose to settle this issue here.

Our argument for a multiverse interpretation of set theory is motivated by a naturalistic deferral to set theoretic practice. The move from (2) to (3) in our argument template is bolstered by claiming that set theoretic practice has already moved beyond seeking answers to questions like CH and is already embedded in the multiverse view. In a sense, the argument is that the battle over the multiverse has already been fought and won (or lost, depending on one's perspective).

4. The End of the Road: Steel

Our final case study comes from John Steel's paper, Gödel's Program, in which an axiomatic multiverse theory is presented [Steel, 2014]. The motivation for this multiverse perspective comes from two observations that emerged from research into the large cardinal programme:

Observation 1: All known natural strengthenings of ZFC can be calibrated by consistency strength against some large cardinal.

Observation 2: Increasing large cardinal strength increases the agreement between natural theories.

Regarding Observation 1, the following results are apropos.

Theorem 7.

(i) $0^{\#}$ exists $\leftrightarrow$ Π^1_1 sets are determined.

(ii) Con(ZFC+there is a Woodin cardinal) $\leftrightarrow$ Con(ZFC+ Δ^1_2 sets are determined).

And of course a positive answer the following infamous open question would add significant support for this observation:

Conjecture 8. Con(ZFC+ $\exists$ a supercompact) $\leftrightarrow$ Con(ZFC + PFA).

The underlying thought is that the large cardinal hierarchy gives us a linearly ordered measure of consistency strength. The restriction to natural theories is not mathematically precise. The basic idea is to restrict the strengthenings of ZFC to questions of combinatorial interest and avoid logical statements involving consistency or self-reference. With regard to the second observation, the following results are pertinent.

Phenomenon: Let $T \subseteq_{\Gamma} U$ mean $\{\varphi \in \Gamma \mid T \vdash \varphi\} \subseteq U$. Then

	ZFC		$T \subseteq_{\Sigma^1_2} U_{\Sigma^1_2}$		$U \subseteq_{\Sigma^1_2} T$
	ZFC + a measurable cardinal		$T \subseteq_{\Sigma^1_3} U$		$U \subseteq_{\Sigma^1_3} T$
For any two *natural* theories, T, U extending...	ZFC + infinite Woodin cardinals	we have…	$T \subseteq_{\Sigma^1_\omega} U$	or	$U \subseteq_{\Sigma^1_\omega} T$
	ZFC + infinite Woodins & Measurable Above		$T \subseteq_{Th(L(\mathbb{R}))} U$		$U \subseteq_{Th(L(\mathbb{R}))} T$

This is not a theorem as the notion of a natural theory is not precise. That said, some reasonable assumptions can be used to tighten these statements into theorems if required. The essential idea for the proofs is that we force in one direction and use inner model theory in the other. The philosophical upshot is that more mathematics is being fixed in place as we strengthen via the large cardinals. For example, we see that with infinitely many Woodin cardinals, it is not possible for natural theories to disagree on any statement articulated in the theory of analysis.

Putting these observations together, we see that: there is only one road up in terms of strength; and following that road leads to increased agreement between all theories. So why not follow that road?

From our discussion in Section 1, we know that there is an upper bound to how much agreement can be obtained. The continuum hypothesis vacillates under the effects of forcing and so the proof techniques used to establish the phenomenon above will not work.

In Section 1, we took this kind of result as evidence for pessimism about the ultimate value of the large cardinal programme for deciding questions like CH. Steel's motivation turns this pessimism on its head. Rather than seeing these limitations as a failure on the part of the large cardinal programme, we are invited to see them as symptomatic of some failure on the part of the questions being asked. If we accept that Observations 1 and 2 are on the right track, then we should take the large cardinal programme more seriously. We should take it that large cardinals provide the right way to track strength. And - this is the important part for multiverse considerations - we should take it that the agreement wrought through large cardinal addition demarcates the space of reasonable mathematical questions. In a nutshell, good mathematical questions are those questions that all natural theories agree on.

But how do we get a multiverse interpretation out of this? This comes from observing that the questions that are agreed upon by natural theories are questions which are impervious to forcing while indeterminate questions, like CH, are vulnerable to its effects. So to account for this distinction we consider a plurality of interpretations of set theory which is closed under generic extension and refinement: forcing and its inverse. Interpretations of this kind go under the title *generic multiverse*.

If we return to our argument template, the move from (2) to (3) is bolstered by taking the large cardinal programme more seriously than arbitrary questions posed in the language of set theory (like CH). We justify our revision of the metaphysics of set theory on the basis that the large cardinal programme and its measure of theoretical strength (not our intuitive theory of collections) delimits the range of reasonable questions in mathematics. The key point here is that we are not so much throwing our arms up in despair as discovering a new way of understanding many of the more profound results in set theory over the last three decades.

5. Concluding Remarks

This article has considered three very different ways of motivating multiverse understandings of set theory. I doubt this list is exhaustive, but it provides some evidence that the diversity involved in set theoretic pluralism isn't merely at the level of the interpretations of the language of itself. At bottom, each of these positions is driven by the deep and arguably disappointing incompleteness that appears to be intrinsic to our set theoretic foundations. They each respond by revising our metaphysical outlook on the basis of this epistemic difficulty.

Our simple version of Shelah revises because they envisage no possibility of properly justifying principles that go beyond ZFC. Our simplification of Hamkins revises because they believe that set theoretic practice has already revised in the face of these problems and pluralism is a more honest representation of this state of affairs. Our quick overview of Steel suggests revision because recent results suggest that our understanding of the purpose of set theoretic foundations was misguided all along.

References

Joel David Hamkins. *The set-theoretic multiverse*. The Review of Symbolic Logic, 5:416{449, 2012.

Daniel Isaacson. *Some considerations on arithmetical truth and the co-rule*. In Michael Detlefsen, editor, Proof, Logic, and Formalization, page 94. Routledge, 1992.

Saharon Shelah. *The future of set theory*. Israel Mathematical Conference Proceedings, 6, 1991.

John R. Steel. *Gödel's program*. In Juliette Kennedy, editor, Interpreting Gödel: Critical Essays. Cambridge University Press, 2014.

THE DEVELOPMENT OF AORISTIC MODAL LOGIC

Christopher Gifford

Saul Kripke (1973) distinguished three pairs of distinctions; that of the analytic and synthetic, that of the *a priori* and *a posteriori*, and that of the possible and the necessary. The analytic and synthetic distinction was semantic and pertained to the meaning of words. Such a distinction was a categorisation of truth; ‘analytic’ indicated truth in virtue of the meaning of the concatenation or juxtaposition of items of vocabulary and ‘synthetic’ indicated truth in virtue of a property in addition to the meaning of the concatenation or juxtaposition of words.

The *a priori* and *a posteriori* distinction was associated with epistemology – more specifically, with knowledge states of agents in the world – *a priori* indicated knowledge that did not depend on experience, and *a posteriori* indicated knowledge that did depend on experience. The third distinction – a distinction that pertained to *modality* – was also a method of categorisation of truth and was presented as a metaphysical distinction; more specifically, a distinction that pertained to that of which is true in possible worlds (ways in which the world can be true).

Kripke’s three distinctions categorised true sentences and categorised false sentences. Applied to problems within the discipline of philosophy, the three distinctions facilitated a method to analyse problems and to establish responses to the problems. The establishment of a modal distinction was one that emphasised the salience of possible worlds, their importance in the formation of distinctions, and how distinctions inter-relate. By comparison of an example, the *de re*/*de dicto* distinction was a method that disentangled different meanings in ambiguous sentences expressed in natural language. The *de re*/*de dicto* distinction pertained to metaphysical necessity and metaphysical possibility understood in terms of possible worlds; for example, 'The number of planets is necessarily nine.' (*de re*) and 'Necessarily, the number of planets is nine.' (*de dicto*). Truth-value(s) of the former and of the latter are not determinately identical (since there are either determinately distinct or not determinately not identical).

Kripke also presented an argument against contingent identity[1]. The argument against contingent identity was one against the existence of contingently identical objects. Contingency was a variety of possibility – defined in modal logic as truth of a contingent proposition in a possible world and truth of the negation of that contingent proposition in another possible world. (One definition of possibility in a possible world semantics was truth in a possible world, and one definition of necessity was truth in all possible worlds.)

One interpretation of Kripke's argument is that the argument against contingent identity proposes that the property of 'is contingently identical to a thing' is an ontological entity that is sufficient to distinguish an object that has that property from an object that is identical to itself. Such an interpretation is an argument of metaphysics in the sense that Kripke's argument appealed to identity, objects, possible worlds, properties, the substitutivity of identity, and Leibniz's Law. Putative examples of contingent identity include the use of names or different descriptions for the same putative object such as 'Goliath' (the statue) and 'Lumpl, the clay', both of which putatively refer, denote, or designate the same putative object.

Modal logic is a powerful tool that systematises distinctions in philosophy which would have otherwise remained obscure if left unsystematised and without a formalisation in natural language. The models in the logistic of modal theory are part of a useful methodology to account for different semantic assignments of meanings to items of vocabulary by the assignation of meaning to names and predicates. Following the introduction of modal logic and possible world semantics to express possibility, contingency, and

1. Kripke (2011): 17-18.

necessity, modal logic has also been used as a method to shed light on vagueness and indeterminacy by the introduction of aoristic operators that express determinacy and indeterminacy.

A traditional problem associated with vagueness is that of the sorites paradox – sometimes referred to as little-by-little arguments. An example: let us start with the case of a hirsute person – a non-vaguely bald person – who has thousands of strands hair on their head. The removal of one hair from their scalp makes no purported difference between a hirsute person and a bald person (that was an induction step that you can render as an instance of mathematical induction[1]). So, if an individual were to remove one hair from the person's scalp then the result would still be a hirsute person. Yet if the individual were to repeat the induction inference thousands of times and continue to remove one hair at a time then the result would be a man with two hairs in his scalp – a bald man – yet, one who is hirsute in accordance with the line of reasoning that the removal of one strand of hair does not make the difference (a sharp cut-off) between a hirsute man and a bald man.

The traditional problem was attributed to Eubulides of Miletus[2] and called a 'sorites' paradox because its introduction was traditionally presented subject to a heap of stand – '*soros*' – coming from the ancient Greek. Popular approaches to resolve the problem have been the introduction of a formal modal framework to express vagueness – sometimes conceptualised as indefiniteness, indeterminacy, or unclarity – as a modal operator. On such a formal approach, the way in which to reason about indeterminacy and the means by what is meant by 'indeterminacy' or 'vagueness' is made more formal, rigorous, and precise. For example, borderline objects which occupy a concrete space and time (or, potentially abstract objects in non-space and non-time) in between two extremals of a sorities series – for example, in between the hirsute man and the bald

1. See Dummett (1975).
2. See Williamson (1994): 9.

man – are classified as that which is indeterminately hirsute and that which is indeterminately bald.

Although the modal system space for aoristic modal logic is in a stage of development (in stark contrast to the existence of a Hasse diagram of modal systems for, for example, *K, D, M, 4, B, 5,* and other systems for alethic modal logic, epistemic logic, information logic, doxastic logic, provability logic[1]), there has been more of a consensus that the appropriate operator for indeterminacy has a logistic and semantic structure to that of contingency. Such a logistic structure is explicable by the use of the item of vocabulary ‘whether’ to express indeterminacy in a modal logic and of the item of vocabulary ‘whether’ to express contingency in a modal logic.

The close logistic structural similarity between contingency and indeterminacy is demonstrated in a comparison between Kripke’s argument against contingent identity and Gareth Evans’s argument against vague objects[2]. Evans asked us: do indeterminately identical objects exist? One interpretation of Evans’s argument is that, for an object *a* that is indeterminately identical to another object *b* that is determinately identical to itself, *a* can be determinately distinguished from *b*. One justification for such a determinate distinction is the existence of the property of being indeterminately identical to an object. The dialectic present in the previous two sentences is analogous to Kripke’s and Evans’s arguments, whereby contingency and indeterminacy are both expressed in the same syntactic position as an (aoristic) modal operator in the lambda calculus in order to express a property. Evans’s argument was responsible for ‘spilling much ink’ in the literature on vagueness and Joseph Moore introduced a proposed new argument against vague objects[3]. I have argued that Moore’s argument was a disguised variant of Evans’s argument against vague objects[4]. At the end of that paper I argued for the concatenation of aoristic and alethic modal operators as a formal

1. See Boolos (1993).
2. Evans (1978).
3. Moore (2008).
4. Gifford (2013).

notation to express properties in the lambda calculus. One prominent example that results from such a concatenation is that of indeterminate contingency; for example, it is indeterminate whether there exists an object that is identical to another object in a possible world and indeterminate whether that object is distinct from another object in another possible world[1].

One result of the acceptance of indeterminate contingency is that it is indeterminate whether a possible world is identical to another possible world (in accordance with the Strong and Weak Kleene truth tables). Such an outcome poses a challenge to those who want to revise classical standard possible world semantics; the syntax and semantics of classical possible world semantics do not permit such indeterminacy if only because the meta-theory is classical. One alternative to Kripke frame semantics that incorporates vagueness or indeterminacy is Barnes and Williams's theory[2] in which there exist halos of indeterminacy that represent non-determinately incorrect possible worlds in accordance with each possibility and there exist modal selection functions which select a multiverse. Other approaches exist; see Taylor's (2017): 63 and Warren's (2017): 112.

An alternative approach is the introduction of another symbol in the meta-theory. Usually a turnstile is used to express truth-in-a-world or satisfaction – here '⊨' is used. Another turnstile – for indeterminate satisfaction – that expresses 'indeterminately satisfies' can be introduced (e.g. '‡') in a world. Let '•' be a modal aoristic operator for indeterminacy that expresses 'It is indeterminate that'. Thence the conditions on frames can be relaxed from:

$W \vDash \neg A$ if and only if in w: $w \neg \vDash A$
$W \vDash$ if A then B if and only if in w: $w \neg \vDash A$ or in $w \vDash B$
$W \vDash$ It is necessary that: A if and only if $w \vDash A$ for all w such that wRw'

1. For a further discussion on vague existence see Hawley (2002).
2. Barnes and Williams (2008). For a response see Ecklund (2011).

(where 'W' is a constant for the set of all possible worlds, '$\neg$' is a negation, and 'A' and 'B' are propositions, 'R' is an accessibility relation, and 'w' and 'u' are variables for possible worlds)
to:

W ‡ $\neg A$ if and only if in w: w ‡ A
W ‡ A if and only if in w and w' • ($w = w'$) and • $\neg$ ($w = w'$)
W ‡ $\neg A$ if and only if in w and w' • ($w = w'$) and • $\neg$ ($w = w'$)
$W \neg \vDash$ if A then B if and only if in w: $\neg B$
W ‡ if A then B if and only if in w: ‡ B.

The guiding idea here is that an indeterminate proposition results in the indeterminate identification of worlds, which in turn, results in indeterminate satisfaction. One interpretation of the conditional above is that it expresses determinate preservation of truth. Both Weak and Strong Kleene truth tables include entries with indeterminacy for a logical equivalence if there is indeterminacy on one side of a logical equivalence. Accordingly, the above conditions can be taken in a Weak/Strong Kleene spirit.

A penultimate point.

One available interpretation of the variety of indeterminacy expressed in the above possible world semantics is that it manifests as a variety of commensurability. Understand commensurability as the possibility of the existence of a common measure between a thing and a thing. Possible worlds are a measure of possibility and necessity; the existence of one and only one possible world suffices as a method to establish whether a proposition is possible; a comparison between all worlds is required as a method to establish whether a proposition is necessary. For indeterminacy, if it is indeterminate whether A then the identification of A across possible worlds is indeterminate (either because of the indeterminacy of the identity of possible worlds or *vice versa*). Hence, the result of indeterminate commensurability since it is indeterminate whether there is a common measure between a possible world and a(nother) possible world.

A final point.

Indeterminacy in logic and philosophy is standardly understood as absence of fact[1] and one appropriate doxastic treatment of the indeterminate is that humans are uncertain of that which is indeterminate; that is, on such an eventuality, human agents have a lack of knowledge of the probability of an outcome. In Quantitative Finance, mathematical models are implemented which incorporate the concept of randomness as a method to model contracts; one of these is the stochastic differential equation: $dS = \mu S\, dt + \sigma S\, dX$ where X is a Wiener process). The incorporation of randomness in this manner to model finance is an acknowledgement of the non-deterministic behaviour of assets [2].

One question elicited from the above presented possible world formal semantics is prescriptive: 'how ought a logician consolidate a maximal amount of information from a method of reasoning to and from indeterminacy?' The manner of representing randomness in Quantitative Finance provides a prescriptive method of reasoning with indeterminacy. Namely, express indeterminacy as randomness in the semantic and syntactic form of modal operators in a multi-modal logic. Then the above presented possible world formal semantics can be altered so that '‡' and '•' are read as 'randomly satisfies' and 'it is random whether' respectively.

A remaining question: what of the semantic and syntactic interaction between an aoristic and random operator in a multi-modal logic? Then, since randomness of indeterminacy elicits random determinacy, the challenge is to provide an account of how 'indeterminate A' and 'determinate B' are inter-substitutable *salva-veritate* within the scope of a randomness operator. I leave the solution to the reader.

1. See Field (2003).
2. See Wilmott (2006), 55-70.

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This paper is dedicated to the memory of Kenneth Arrow.

References

Barnes, E. and Williams, J. R. G., (2011). *A Theory of Metaphysical Indeterminacy*, Oxford Studies in Metaphysics VI: 103-48, ed. Karen Bennett and Dean W. Zimmerman.

Boolos, G. (1993). *The Logic of Provability*, Cambridge: Cambridge University Press.

Dummet, M. (1975). *Wang's Paradox*, *Synthese* **30**(3): 301-24.

Ecklund, M. (2011). *Being Metaphysically Unsettled: Barnes and Williams on Metaphysical Indeterminacy and Vagueness*, in Karen Bennett & Dean Zimmerman eds., Oxford Studies in Metaphysics, Oxford University Press, pp. 6-149.

Evans, G. (1978). '*Can There Be Vague Objects?*', *Analysis* **38**(4): 208.

Field, H. (2003). *Truth and the Absence of Fact*, Clarendon Street: Oxford University Press.

Fine, K. (1975). '*Vagueness, Truth, and Logic*', *Synthese* 30 3/4: 265-300.

Gifford, C. S. (2012). *All Bar One: The Problem of the Many* (Unpublished doctoral thesis). University of Bristol, Bristol, U.K.

Gifford, C.S. (2013). '*Against The Modal Argument*', *Erkenntnis*, **78**(3): 627-646.

Hawley, K. (2002). '*Vagueness and Existence'*, Proceedings of the Aristotelian Society' **102**(1): 125-140.

Kripke, S. (2011). '*Identity and Necessity*' in Philosophical Troubles Volume 1, Saul Kripke, pp. …: Oxford: Oxford University Press.

Moore, J. G., (2008). '*A Modal Argument against Vague Objects*', *Philosophers' Imprint* **8**(12): 1-17.

Warren, J. (2017). '*Quantifier Variance and Indefinite Extensibility*', *Philosophical Review*, **126**(1): 112.

Williamson, T. (1994). *Vagueness*, London: Routledge.

Taylor, D. E. (2017). '*Deflationism and Referential Indeterminacy*', *Philosophical Review*: **126**(1): 63.

Warren, J. (2017). '*Quantifier Variance and Indefinite Extensibility*', *Philosophical Review*, **126**(1): 112.

Wilmott, P. (2006). *Paul Wilmott on Quantitative Finance (Second Edition)*, West Sussex, England: John Wiley & Sons Ltd.

AN INTRODUCTION TO RAMSEY ALGEBRAS

Zu Yao Teoh

1. Ramsey Algebra

Ramsey algebra can be said to be a Ramsey-type combinatorics for algebras. We will come to the history, but first we explain what a Ramsey algebra is. We define an algebra to be any structure modeling a purely functional first-order language. Suppose that $\mathcal{L}$ is a purely first-order functional language. A term t of $\mathcal{L}$ is said to be $orderly$ if (1) no variable appears more than once in t and (2) the indices of the variables appearing in t is increasing going from left to right. For instance, if $\mathcal{L} = \{\circ\}$ denotes the language of groups, then $\circ\circ\, v_0 v_2 v_3$ is an orderly term of $\{\circ\}$. If $\mathcal{L} = \{+,\times\}$ is the language of rings, then $\times\, v_0 + v_6 v_{10} v_{12}$ is an orderly term of $\mathcal{L} = \{+,\times\}$. Denote the set of orderly terms of $\mathcal{L}$ by OT($\mathcal{L}$). If $t_1, t_2 \in$ OT($\mathcal{L}$) and the index of the variable occurring last in t_1 is strictly less than the index of the variable occurring first in t_2 then we write $t_1 \prec t_2$. We call an infinite sequence $t_0, t_1, \ldots$ of orderly terms an $admissible$ sequence if $i < j$ implies $t_i \prec t_j$ for all i, j.

Suppose that $\mathcal{A}$ = ($\mathcal{A}$, $\mathcal{F}$) is an algebra and let $\mathcal{L}_{\mathcal{F}}$ denote the language $\mathcal{A}$ models. Let ${}^{\omega}A$ denote the set of infinite sequences of A. If $\vec{a}, \vec{b} \in {}^{\omega}A$, then we say that $\vec{a}$ is a *reduction* of $\vec{b}$ if the following holds: there exist an admissible sequence $\langle t_0, t_1, \ldots \rangle$ of orderly terms of $\mathcal{L}_{\mathcal{F}}$ such that, for each $i \in \omega$, $\vec{a}(i) = t_i^{\mathcal{A}}[\vec{b}]$, where $t^{\mathcal{A}}[\vec{b}]$ means the interpretation of the term t in the model $\mathcal{A}$ under the assignment $\mu\,(vi) = \vec{b}$ for each term t and $i \in \omega$. Thus, for instance, if $\vec{b} = \langle 1,1, \ldots \rangle$ and $t_0 = v_0 v_1$, $t_1 = + + v_2 v_3 v_4$, $t_3 = + + + v_5 v_6 v_7 v_8$, ..., then the sequence $\vec{a} = \langle 2,3,4, \ldots \rangle$ is a reduction of $\vec{b}$. Define

$$FR_{\mathcal{F}}(\vec{b}) = \left\{ t^{\mathcal{A}}\,[\vec{b}] : t \in OT(\mathcal{L}_{\mathcal{F}}) \right\} \quad ; \qquad (1)$$

We also say that a sequence $\vec{a} \in {}^{\omega}A$ is *homogeneous* for $X \subseteq A$ if $FR_{\mathcal{F}}(\vec{a})$ is contained in X or disjoint from X.

Definition 1.1 (Ramsey Algebra). *Suppose that $\mathcal{A} = (\mathcal{A}, \mathcal{F})$ is an algebra. Then is said to be a Ramsey algebra if, for each $\vec{b} \in {}^{\omega}A$ and each $X \subseteq A$, there exist a reduction $\vec{a} \in {}^{\omega}A$ of $\vec{b}$ that is homogeneous for X.*

A classic theorem of infinitary combinatorics reads:

Theorem 1.1 (Hindman). *Every semi-group is a Ramsey algebra.*

2. History

The history of Ramsey algebras can be traced back to Carlson's work on topological Ramsey spaces in his 1986 manuscript [1]. Inspired by the Ellentuck topology, Carlson's result on the topological Ramsey space of variable words has profound consequence in infinitary combinatorics because of its unifying power. Hindman's theorem and the dual Ellentuck theorem have straightforward derivations from Carlson's work. In turn, the dual Ellentuck theorem generalizes a long list of earlier results such as Ramsey's theorem, the Galvin-Rothschild theorem on *n*-parameter sets, etc. (What is called a topological Ramsey space is called a Ramsey space in [1]; later work by Todorcevic [5] generalized the latter notion and the former notion is commonly referred to as a topological Ramsey space in the modern literature.)

Ramsey algebras enters the picture when a topological Ramsey space is generated by algebras. An abstract version of Ellentuck's theorem is key to relating Ramsey algebra to topological Ramsey spaces. In Carlson's own words: "The first step [in obtaining the main theorem of his manuscript] is to reduce the topological question of whether a certain structure is a [topological] Ramsey space to a more combinatorial question. This is accomplished by an abstract version of Ellentuck 's theorem…" For a space $\mathfrak{R}$(A, $\mathcal{F}$) generated by an algebra

(A, $\mathcal{F}$) with a finite family $\mathcal{F}$, the combinatorial problem is phrased in the definition of a Ramsey algebra:

Theorem 2.1. *Let $\mathcal{F}$ be a finite family of operations on A. then $\mathfrak{R}$(A, $\mathcal{F}$) is a topological Ramsey space if and only if (A, $\mathcal{F}$) is a Ramsey algebra.*

3. Questions & Results

Our work has revolved around the following three main questions:

1. Extend the notion of a Ramsey algebra to heterogeneous algebras-structures that model many-sorted, purely functional first-order logic.

2. What is a necessary and sufficient condition for an algebra to be Ramsey?

3. How do we construct Ramsey algebras from known Ramsey algebras?

3.1 Heterogeneous Ramsey Algebra

Groups and rings are examples of algebras that model one-sorted languages. However, there are other naturally occurring algebras that are of a heterogeneous nature. Vector spaces for example, fall in this category. A vector space interprets a two-sorted functional language $\mathcal{L} = \{+, .\}$-scalar multiplication and vector addition. The domain of a vector space thus consists of two *phyla*, the phylum of scalars (fields) and the phylum of vectors. How then should we extend the notion of a Ramsey algebra to heterogeneous algebras? In fact, Carlson's original work on the space of variable words involved elements from different phyla.

Suppose $\left((A_\xi)_{\xi\in I}, \mathcal{F}\right)$ is a heterogeneous algebra whose phyla are indexed by the set I and $\mathcal{F}$ is a family of functions, each having as domain a Cartesian product of some members of $(A_\xi)_{\xi \in I}$ and codomain some member A_η.

To formulate heterogeneous Ramsey algebra, the notions of an orderly term, an admissible sequence, and a reduction introduced earlier remain unchanged, expect that they are now defined in the context of many-sorted logic. One additional notion is needed though, namely the notion of a *sort*, which is an $\vec{e} \in {}^{\omega}I$. In order that Carlson's abstract Ellentuck theorem may apply, we define the analogue of Eq. 1 as follows:

Definition 3.1. If $\vec{b}$ is an $\vec{e}$-sorted sequence, define:

$$FR_{\mathcal{F}}^{\vec{e}}\vec{b} = \{\vec{a}(0) \colon \vec{a} \leq_{\mathcal{F}} \vec{b} \; and \; \vec{a} \; is \; \vec{e}\text{-}sorted\}. \quad (2)$$

A sequence $\vec{a}$ is said to be homogeneous for X (with respect to $\mathcal{F}$) if $FR_{\mathcal{F}}^{\vec{e}}(\vec{a})$ is either contained in or disjoint from X.

For each sort $\vec{e}$, we then have the notion of an $\vec{e}$-Ramsey algebra:

Definition 3.2. *($\vec{e}$-Ramsey Algebra). An algebra $\left((A_\xi)_{\xi \in I}, \mathcal{F}\right)$ is said to be an $\vec{e}$-Ramsey algebra if, for each $\vec{e}\text{-}sorted$ sequence $\vec{b}$ and $X \subseteq A_{\vec{e}(0)}$, there exists an $\vec{e}\text{-}sorted$ reduction $\vec{a}$ such that $\vec{a}$ is homogeneous for X .*

Definition 1.1 is, of course, a special case of Definition 3.2.

Our work on the Ramsey algebraic property of vector spaces culminates in the following results:

a. If the underlying field of a vector space is finite, then the vector space is an $\vec{e}$-Ramsey algebra for all sorts of $\vec{e}$.

b. If the underlying field is infinite, then the vector space is an $\vec{e}$-Ramsey algebra if and only if $\vec{e}$ is constant with value 1 or is otherwise non-constant but eventually constant with any value.

We also included the study of various algebras involving different combinations of matrix operations as well as generalizations of such algebras. The result can be found in [4].

3.2. Characterization of Ramsey Algebra

We return to algebras with a single phylum. Arguably the most well-known result of the subject is Hindman's theorem. Apart from that, not many commonly encountered algebras are known to be Ramsey algebras. For example, infinite fields are not Ramsey algebras. This follows from a more general result that infinite integral domains are not Ramsey algebras. Of course, the Pigeonhole principle implies that any empty algebra, i.e. an algebra equipped with an empty family $\mathcal{F}$ of operations, is a Ramsey algebra.

We know exactly when a finite algebra is Ramsey (an algebra is finite if its domain is finite). Indeed, the following is known to Carlson: a finite algebra $(A, \mathcal{F})$ is Ramsey if and only if every constant sequence of elements of A can be reduced to a constant sequence of elements *idempotent* with respect to $\mathcal{F}$. An element a of A is said to be idempotent with respect to the family $\mathcal{F}$ of operations on A if $f(a, \ldots, a) = a$ for all $f \in \mathcal{F}$. If the domain of an algebra $(A, \mathcal{F})$ is infinite and $\mathcal{F}$ consists only of unary operations, then to decide if the algebra is Ramsey amount to the question of whether a reduction to some sequence of points fixed by all operations in $\mathcal{F}$ is possible; the precise statement can be found in [2]. The question as to when exactly is an infinite algebra Ramsey? is still open.

A localized version called Ramsey Orderly Algebra has been proposed as a line for attacking the characterization problem. Ramsey orderly algebra shifts the focus from algebras to sequences, studying the algebra that can be generated from the sequence. The precise formulation can be found in [3], in which we derive some basic theorems concerning orderly algebras as well as presenting a case study in support of this formulation.

We also note that it is interesting enough to be able to characterize exactly when a groupoid, i.e. an algebra $(A, \mathcal{F})$ consisting of one binary operation, f is a Ramsey algebra.

3.3. Constructing New Ramsey Algebras

Can we construct new Ramsey algebras by taking Cartesian products of Ramsey algebras? A Cartesian product of semi-groups is a semi-group, hence a Ramsey algebra, but what about the general case? Namely, given a finite collection $\mathcal{A}_i$ of Ramsey algebras, must $\prod_i \mathcal{A}_i$ also be Ramsey?

This is one of the main open problems we hope to know an answer to although we believe that the answer is "no". Nevertheless, we have a minor result, namely $\prod_i \mathcal{A}_i$ is Ramsey if at most one Cartesian component $\mathcal{A}_i$ is infinite. This result can be easily deduced using the characterization of finite algebras based on the existence of idempotent reductions.

What are other ways of obtaining new Ramsey algebras from old? Ultra-products based on non-principal ultra-filters? Elementarily extending a Ramsey algebra $(A, \mathcal{F})$ in its natural language, $\mathcal{L}_{\mathcal{F}}$?

We are just about to embark on a study of these constructions and we cordially invite collaborators to join us in our quest.

4. Final Remark

The research described above is part of the author's doctoral work done under the supervision of Wen Chean at University Sains Malaysia. There are still many elementary open problems in the study of Ramsey algebras and we believe that the Iranian logic community will find it worthwhile to work on some of them. The author would like to invite the interested reader to participate in a collaborative work in attacking the problems mentioned as well as suggesting valuable questions to pursue.

References

[1] Carlson, T.J.: *Some unifying principle in Ramsey theory*. Discrete Mathematics 68(2), 117-169 (1988)

[2] Teh, W.C.: *Ramsey algebras*. J. Math. Log.16(2) (2016), 16 pages.

[3] Teh, W.C., Toeh, Z.Y.: *Ramsey Orderly Algebras as a New Approach to Ramsey Algebras*, arXiv:1511.06048

[4] Toeh, Z.Y., Teh, W.C.: *Heterogeneous Ramsey Algebras and Classification of Ramsey Vector Spaces*, submitted to *Bull. Malays. Math. Sci. Soc*

[5] Todorcevic, S.: *Introduction to Ramsey spaces*, Annals of Mathematics Studies, vol. 174. Princeton University Press, Princeton, NJ (2010).

ADDITIONAL SET THEORETIC ASSUMPTIONS AND TWISTED SUMS OF BANACH SPACES

Claudia Correa

In this paper, we discuss the role played by additional set-theoretic assumptions in the investigation of the existence of non-trivial twisted sums of c_0 and spaces of continuous functions on nonmetrizable compact Hausdorff spaces.

1. History and Background of the Problem

In these notes we analyze the role played by some additional set-theoretic assumptions in the study of a classical problem in Banach space theory. This problem is related to the existence of nontrivial twisted sums of Banach spaces. We recall some basic definitions and facts.

Definition 1.1. Let X and Y be Banach spaces. A *twisted sum* of Y and X is an exact sequence in the category of Banach spaces, i.e. it is an exact sequence of the form:

$$0 \to Y \to Z \to X \to 0,$$

Where Z is a Banach space and the maps, are bounded linear operators. This twisted sum is said to be *trivial* if the sequence splits, i.e. if the image of the map $Y \to Z$ is complemented in Z.

For a nice discussion on exact sequences of Banach spaces, see [8, Chapter I]. Given Banach spaces X and Y, note that the direct sum of Y and X provides a twisted sum of Y and X. More precisely:

$$0 \to Y \xrightarrow{i_1} Y \oplus X \xrightarrow{\pi_2} X \to 0$$

Is a twisted sum, where the direct sum is endowed with a product norm, the map i_1 is the first inclusion and π_2 is the second projection. It is clear that this twisted sum is trivial. At this point one might wonder about the existence of nontrivial twisted sums of Banach spaces. Unlike the category of vector spaces, in which every twisted sum is trivial, in the category of Banach spaces there are examples of nontrivial twisted sums. A classical example is provided by an old result of Phillips [19] which states that the space c_0 of real sequences converging to zero is not complemented in ℓ_∞, the space of bounded real sequences. Therefore, the following twisted sum is nontrivial:

$$0 \to c_0 \to \ell_\infty \to \ell_\infty / c_0 \to 0,$$

Where the arrows are the inclusion and the quotient map. However there are pairs of Banach spaces that admit only trivial twisted sums. Interesting examples of this phenomenon are consequences of the classical theorem of Sobcyk [20], that states that c_0 is complemented in every separable super-space; more precisely, every isomorphic copy of c_0 inside a separable Banach space is complemented. Since separability is a three space property [8], it follows that if X is a separable Banach space, then every twisted sum of c_0 and X is trivial.

In these notes we are interested in the converse of this last implication. In other words: If X is a Banach space such that every twisted sum of c_0 and X is trivial, then X must be separable? This question is easily answered negatively since there are non-separable projective Banach spaces; namely, the space $\ell_1(\Gamma)$ for any uncountable set Γ [8, Lemma 1.4.a]. However this question becomes interesting when we restrict ourselves to the class of $C(K)$ spaces. Here $C(K)$ denotes the Banach space of continuous real-valued functions defined on a compact Hausdorff space K, endowed with the supremum norm. Recall that $C(K)$ is separable if and only if K is metrizable. Therefore Sobczyk's Theorem implies that if K is a compact metrizable space, then every twisted sum of c_0 and $C(K)$ is trivial. In this context, the converse we are discussing can be phrased as the following question.

Question 1. *Is there a compact Hausdorff nonmetrizable space K such that every twisted sum of c_0 and $C(K)$ is trivial?*

This question was first stated in the papers [3, 4]. Until last year, there were few results related to this problem. They were summarized in [7, proposition 2], namely we have the following proposition.

Proposition 1.2. *Let K be a compact Hausdorff space. There exists a nontrivial twisted sum of c_0 and $C(K)$ under any of the following assumptions:*

(1) K is a nonmetrizable Eberlein compact space;

(2) K is a Valdivia compact space which does not satisfy the countable chain condition (ccc);

(3) The weight of K, denoted by $w(K)$, is equal to w_1 and the dual space of $C(K)$ is not weak-separable;

(4) K has the extension property ([10]) and it does not have ccc;

(5) $C(K)$ contains an isomorphic copy of l_∞ ; in particular, it is the case if K is infinite and extremally disconnected.

In a recent series of papers [7, 11], this problem was extensively studied and great progress was extensively studied and great progress was achieved towards its solution. In these works the importance of additional set-theoretic assumptions in the understanding of Question 1 became clear. Finally, in [18] the first consistent examples of compact Hausdorff nonmetrizable spaces K such that c_0 cannot be nontrivially twisted with $C(K)$ were given. As we will discuss in section 3, thanks to the result of [7] and [11] the existence of nontrivial twisted sums of c_0 and $C(K)$, for some of the spaces K given by Marciszewski and Plebanek in [18], are independent of the axioms of ZFC. The additional set-theoretic assumptions used in those works are the Continuum hypothesis and Martin's axiom. We observe that the existence, in ZFC, of a compact Hausdorff nonmetrizable space K such that c_0 cannot be nontrivially twisted with $C(K)$ is still an open problem. In section 3, we discuss the results obtained in [7], [11], and [18], and we give more details about those obtained by myself and D. V. Tausk in [11]. In the final section, we describe the ongoing investigation of some open problems related to Question 1 and the progress achieved towards their solutions in [12], where we worked assuming the Diamond axiom. In section 2, we present a brief introduction to the additional set-theoretic assumptions used in the works mentioned above.

2. Continuum Hypothesis, Diamond Principle and Martin's Axiom

The Continuum hypothesis (CH) was born in the early history of modern set theory; actually CH stimulated the development of this theory. It all began with G. Cantor's investigation of different kinds of infinities [5]. Cantor showed that there is no one-to-one correspondence between the set of natural numbers and the set of real numbers. In other words, using the idea that the existence of a one-to-one correspondence between two sets expresses the notion of having the same number of elements, Cantor showed that there are strictly more real than natural numbers. In this context, he started investigating the existence of a set with (strictly) more elements than the natural numbers and (strictly) less elements than the real numbers. This investigation led to the modern formulation of CH. We denote by ω the first infinite ordinal, by ω_1 the first uncountable ordinal and by $\mathfrak{c}$ the cardinality of the real numbers, also known as the cardinality of the continuum. The continuum hypothesis is the following statement: there is no cardinal number strictly between ω and $\mathfrak{c}$, i.e. $\mathfrak{c} = \omega_1$. Cantor was never able to prove or disprove CH. It was only with the works of K. Gödel [13] and P. Cohen [9] that we were able to understand the status of CH regarding the axiomatic set-theory we use to do modern mathematics. Those mathematicians showed that CH is independent of ZFC, i.e. both CH and its negation are consistent with ZFC. Therefore, Cantor would never be able to prove or disprove CH, using the axioms of ZFC.

Now we turn our attentions to a classical problem in set theory that motivated the next axioms we want to discuss here: Diamond axiom and Martin's axiom. In this paper [5], Cantor characterized the canonical order of the real numbers; namely, he proved that every nonempty totally ordered set with no endpoints that is connected and separable, when endowed with the order topology, is order-isomorphic to the real line. The *solution problem* asks if we can replace separable with ccc in the above statement. More precisely, the Souslin problem is the following question: If X is a nonempty totally ordered set without endpoints which is connected and satisfies ccc, when endowed with the order topology, then X is order-isomorphic to the real line?

The *Souslin hypothesis* (SH) is the statement that the Souslin problem has positive answer. The first appearance of Souslin problem was in an early volume of Fundamenta Mathematicae in 1920 as part of a list of open problems and it was attributed to Mikhail Souslin. The first great breakthrough on the investigation of SH happened when S. Tennenbaum [22] showed the relative consistency of the negation of SH with ZFC. Later, R. Jensen [14] gave another proof of the consistency of the negation of SH. Jensen isolated an interesting combinatorial principle (consistent with ZFC) that implies the negation of SH, this principle is called the *Diamond Axiom* (◊). Finally, in 1971, R. Solovay and Tennenbaum established the consistency of SH [21]. Martin's axiom (MA) was then isolated by T. Martin from this work of Solovay and Tennenbaum.

In what follows we discuss briefly the statement of ◊ and of MA. We start with ◊. In order to understand this combinatorial principle, we need to recall some basic concepts. This axiom is related to, in some sense, big subsets of ω_1 is said to be a *club* if it is closed and unbounded in ω_1, where ω_1 is endowed with the canonical order topology; we say that is *stationary* if it intersects every club.

Definition 2.1. Let I be a set.

(1) An $\alpha - filtration$ of I, where α is an ordinal number, is an increasing family $\left(I_\beta\right)_{\beta\in\alpha}$ of subsets of I such that $= \bigcup_{\beta\in\alpha} I_\alpha$. This filtration of I is said to be *continuous* if, for every limit ordinal $\beta \in \alpha$, we have $I_\beta = \bigcup_{\lambda\in\beta} I_\lambda$.

(2) Given an ω_1-filtration of I, a diamond family for this filtration is a family $(I_\alpha^\lozenge)_{\alpha\in\omega_1}$ with each $I_\alpha^\lozenge$ a subset of I_α and such that given any subset J of I, the set $\{\alpha \in \omega_1 : J \cap I_\alpha = I_\alpha^\lozenge\}$ is stationary.

The axiom ◊ is the statement that there exists a diamond family for every continuous ω_1-filtration $(I_\alpha)_{\alpha\in\omega_1}$ of a set I such that each I_α is countable. It is easy to see that ◊ is equivalent to the statement that there exists a diamond family for the canonical ω_1-filtration of the set ω_1 , i.e. with $I_\alpha = \alpha$, for every $\alpha \in \omega_1$. Moreover, since there exist ω_1 disjoint stationary subsets of ω_1 [17, Chapter II, Corollary 6.12], we have the version of ◊ which requires the existence of a diamond family only when the set I is countable is equivalent to CH.

We finish this section by discussing a bit about MA. In order to state Martin's axiom, we need to introduce some concepts regarding partial orders. Let $(\mathbb{P}, \leq)$ be a partial order. A subset D of $\mathbb{P}$ is called *dense* in $\mathbb{P}$ if given $p \in \mathbb{P}$ there exists $d \in D$ with $d \leq p$. Two elements p and q of $\mathbb{P}$ are said *compatible* if there exists $r \in \mathbb{P}$ with $r \leq p$ and $r \leq q$.

If p and q are not compatible, we say that they are *incompatible*. An *antichain* of $\mathbb{P}$ is a subset of $\mathbb{P}$ with the property that any two distinct elements are incompatible; a partial order $(\mathbb{P}, \leq)$ is said to satisfy the *countable chain condition* (ccc) if every antichain of $\mathbb{P}$ is countable. Since MA assure the existence of some filters on partial orders satisfying ccc, lets define those important objects.

Definition 2.2. A nonempty subset G of $\mathbb{P}$ is said to be a *filter* if it satisfies the following properties:

- If p and q belong to G, then there exists $r \in G$ with $r \leq p$ and $r \leq q$, i.e. each pair of elements of G are compatible and this fact is testified by some element of G;

- If $p \in G$ and $q \in P$ satisfy $p \leq q$, then $q \in G$.

Now we can state MA. For each infinite cardinal κ, $MA(\kappa)$ is the following statements: Let $(\mathbb{P}, \leq)$ be a nonempty partial order and $\mathfrak{D}$ be a family of dense subsets of $\mathbb{P}$. If $\mathbb{P}$ satisfies ccc and the cardinality of $\mathfrak{D}$ is at most κ, then there exists a filter in $\mathbb{P}$ that intersects every element of $\mathfrak{D}$. Finally, MA is the statement that $MA(\kappa)$ holds for $\leq \kappa \leq \mathfrak{c}$. Since $MA(\omega)$ is a theorem of ZFC and $MA(\mathfrak{c})$ is false [17, Lemma 2.6], we have that MA is interesting only if we assume the negation of CH.

3. Consistency and Independence Results

The class of nonmetrizable compact Hausdorff spaces contains dramatically distinct subclasses of spaces: we have classes that are "close" to the class of metrizable spaces, in the sense that their elements have many properties implied by metrizability and we have classes that are "far" from the class of metrizable spaces, i.e. their elements have little in common with the metrizable ones. For instance, with respect to the sequential properties, the class of Corson compact is close to the metrizable ones. In fact, every Corson compact space is a Frechét-Urysohn space [15, Lemma 1.6 (ii)]. On the other hand, the extremally disconnected compact spaces are completely different from the metrizable ones: they have no nontrivial convergent sequences [2, Theorem 18]. In light of Sobczyk's Theorem, it is reasonable to believe that if there exists a nonmetrizable compact Hausdorff space K such that c_0 can not be nontrivially twisted with $C(K)$, then it will belong to classes close to the class of metrizable compact spaces. Having those considerations in mind, D.V. Tausk and myself decided to investigate the twisted sums of c_0 and $C(K)$, for K belonging to the class of Corson compact spaces and more generally, its superclass of Valdivia compacta. To discuss our results, we start by recalling some standard definitions and well-known facts. Given an index set I, we write $\Sigma(I) = \{x \in \mathbb{R}^I : supp\ x\ is\ countable\}$, where the support sup x of x is defined by $supp\ x = \{i \in I : x_i \neq 0\}$.

Definition 3.1. Given a compact Hausdorff space K, we call A a Σ-*subset* of K if there exists an index set I and a continuous injection $\varphi: K \to \mathbb{R}^I$ such that $A = \varphi^{-1}[\Sigma(I)]$. The space K is called a *Valdivia compactum* if it admits a dense Σ-subset and it is called a *Corson compactum* if K is a Σ-subset itself.

It is clear that every compact metric space is Corson and that every Corson space is Valdivia (for an amazing survey on Corson and Valdivia compacta, see [15]). In [11], we developed a technique for constructing nontrivial twisted sums of c_0 and certain nonseparable Banach spaces, using the existence of interesting biorthogonal systems. Using these techniques, we were able to solve the problem for Corson compacta assuming CH.

Theorem 3.2. *If K is a Corson compact space with weight greater or equal to $\mathfrak{c}$, then there exists a nontrivial twisted sum of c_0 and $C(K)$. In particular, under CH, there exists a nontrivial twisted sum of c_0 and $C(K)$, for every nonmetrizable Corson compact space K.*

Proof. See [11, Theorem 3.1]. □

It is known that, under MA and the negation of CH, every ccc Corson compactum is metrizable [1]. Having in mind proposition 1.2 item (2), we have that, under MA and the negation of CH, there exists a nontrivial twisted sum of c_0 and $C(K)$, for every nonmetrizable Corson compact space K. Therefore it follows from theorem 3.2 that, under MA, there exists a nontrivial twisted sum of c_0 and $C(K)$, for every nonmetrizable Corson compactum K. In this context, the following question remains open.

Question 2. *Does it hold, in ZFC, that there exists a nontrivial twisted sum of c_0 and $C(K)$, for every nonmetrizable Corson compact K?*

The general Valdivia case, under CH, remains open, but many results were obtained in [11]. They are summarized in the next theorems. Recall that given a point x of a topological space $\mathcal{X}$, we define the *weight* of x in $\mathcal{X}$ by:

$\omega(x, \mathcal{X}) = \min \{\omega(V): V\ neighbourhood\ of\ x\ in\ \mathcal{X}\}$.

Theorem 3.3. *Let K be a Valdivia compact space admitting a G_δ point x with $(x, K) \geq \mathfrak{c}$. Then there exists a nontrivial twisted sum of c_0 and $C(K)$. In particular, under CH, if K is a valdivia compact space admitting a G_δ point with no second countable neighborhoods, then there exists a nontrivial twisted sum of c_0 and $C(K)$.*

Theorem 3.4. *Assume CH. Let K be a Valdivia compact space admitting a dense Σ-subset A, such that some points of $K \backslash A$ is the limit of a nontrivial sequence in K. Then there exists a nontrivial twisted sum of c_0 and $C(K)$.*

Regarding twisted sums of c_0 and spaces of continuous functions, a particular family of Valdivia compact spaces was recently shown to be special; namely the spaces 2^κ. In [11], Tausk and myself presented the following result.

Theorem 3.5. *If κ is a cardinal number with $\kappa \geq \mathfrak{c}$, then there exists a nontrivial sum of c_0 and $C(2^k)$.*

Proof. See [11, Corollary 2.10].

Surprisingly, Marciszewski and Plebanek showed that, given a cardinal number $\kappa < \mathfrak{c}$, $if\ MA(\kappa)$ holds, then every twisted sum of c_0 and $C(2^\kappa)$ is trivial [18, Corollary 5.2]. In particular, under MA and the negation of CH, if κ is a cardinal number satisfying $\kappa < \mathfrak{c}$, then every twisted sum of c_0 and $C(2^\kappa)$ is trivial. This answers consistently Question 1. Note that, under CH, theorem 3.5 states that there exists a nontrivial twisted sum of c_0 and $C(2^\kappa)$, for every uncountable cardinal κ. Therefore the existence of nontrivial twisted sums of c_0 and $C(2^{\omega_1})$ is independent of the axioms of ZFC. The problem of determining if Question 1 can be answered in ZFC remains open.

Question 3. *Is there, in ZFC, a compact Hausdorff nonmetrizable space K such that every twisted sum of c_0 and $C(K)$ is trivial?*

To finish this section, we would like to tell the reader about the results of [7]. In this work, Castillo showed that, assuming CH, if K is a nonmetrizable compact hausdorff space with finite Cantor-Bendixson height, then there exists a nontrivial twisted sum of c_0 and $C(K)$ [7, Theorem 1].

It is worth commenting that, unlike the results in [11], where the nontrivial twisted sums were constructed, Castillo did not construct his nontrivial twisted sums; their existence is established by counting arguments (see [7, Lemma 2]). Interestingly, Plebanek and Marciszewski showed that, under $MA(\kappa)$, if K is a separable scattered space of height 3 and weight κ, then every twisted sum of c_0 and $C(K)$ is trivial [18, Theorem 9.7].

4. Towards the answer to question 3

It follows from the discussion in Section 3 that Question 3 can be rephrased as follows.

Question 4. *Is there an additional consistent set-theoretic assumption that assure the existence of nontrivial twisted sums of c_0 and $C(K)$, for every nonmetrizable compact Hausdorff space K?*

Since the first examples of nonmetrizable compact Hausdorff spaces such that c_0 cannot be nontrivially twisted with their spaces of continuous functions were given assuming MA and the negation of CH, one might wonder if under CH, there exists a nontrivial twisted sum of c_0 and $C(K)$, for every nonmetrizable compact Hausdorff space *K*. Having this consideration in mind and continuing the work of

[11], D. Tausk and myself are currently investigating the following question.

Question 5. *Is it true that, under CH, there exists a nontrivial twisted sum of c_0 and $C(K)$, for every nonmetrizable Valdivia compactum?*

The techniques developed in [11] provided nontrivial twisted sums of c_0 and $C(K)$, for a huge class of Valdivia compact spaces K. Note that Theorems 3.3 and 3.4 do not solve the problem under CH, if K is a nonempty Valdivia compactum satisfying all the following properties:

(1) K satisfies ccc;

(2) K does not admit a G_δ point;

(3) K does not admit a nontrivial convergent sequence in the complement of a dense Σ-subset.

Note that the case when K does not satisfy ccc is handled by Proposition 1.2(2). Finding examples of nonempty Valdivia compact spaces with no G_δ points and no nontrivial convergent sequences in the complement of a dense Σ-subset is not a trivial task, since the absence of G_δ points tends to make the complement of dense Σ-subsets "large" (see, for instance [15,Theorem 3.3] for a more precise statement).

In [11, Proposition 4.7], it was shown that the path space of a certain tree T, endowed with the product topology of 2^T, provides such an example. However, using this topology it is not possible to have a nonempty path space with no G_δ points and ccc. In [12], D. Tausk and myself constructed an example of a nonempty Valdivia compact space satisfying Properties (1), (2) and (3) described above. This construction is done under $\lozenge$. This space, given in [12, Theorem 4.1], is the path space of a tree, endowed with an intricate compact Hausdorff topology. In what follows, we describe briefly the tools used in [12].

Recall that a *tree* is a partially ordered set $(T, \leq)$ such that, for all $t \in T$, the set $(\cdot, t) = \{s \in T : s < t\}$ is well-ordered. A subset X of T is called an *initial part of* T if $(\cdot, t) \subset X$, for all $t \in X$; a *chain* if it is totally ordered; an *anti-chain* if any two distinct elements of X are incomparable; a *path* if it is both a chain and an initial part of T; the *path space* of a tree is the set of its paths. We say that T satisfies the *countable chain condition* (ccc) if every anti-chain in T is countable. At this point, the reader must be wondering: what is the relationship between trees and Valdivia compact spaces? It is a good question that is answered by the following facts:

- Kubiś and Michalewski established in [16] a correspondence between Valdivia compact spaces with weight at most ω_1 and certain inverse limits of compact metric spaces [12, Theorem 2.9];

- We established a correspondence between those inverse limits and certain inverse limits of path spaces of trees [12, Proposition 3.3].

Therefore, combining those two correspondence, we obtain a characterization of Valdivia compact spaces with weight at most ω_1 in terms of trees with some additional structures and suitable topologies on their path spaces [12, Theorem 3.4].

This characterization allows one to fine-tune the structure of a Valdivia compactum by manipulating the properties of the corresponding tree. We observe that axiom $\lozenge$ is used in the construction of our tree in a similar way that is used to construct a Soulsin tree. Recall that a Soulsin tree is a tree with height ω_1, satisfying ccc and admitting only countable paths (see [17, Chapter II, 4] to understand the relationship between Souslin trees and the Souslin hypothesis).

References

[1] S. A. Argyros, S. Mercourakis, and S. Negrepontis, *Analytic properties of Corson compact spaces*, Proc. Fifth Prague Topological Symposium, Heldermann Verlag, Berlin, 1982, 12-23.

[2] A. V. Arhangelskii (editor), *General topology II*, Encyclopedia of mathematical science, vol. 50, Springer, 1995.

[3] F. Cabello, J. M. F. Castillo, N. J. Kalton, and D. T. Yost, *twisted sums with C(K) spaces*, Trans. Amer. Math. Soc. 355 (11) (2003) 4523-4541.

[4] F. Cabello, J. M. F. Castillo, and D. Yost, *Sobczyk's Theorem from A to B*, Extracta Math. 15 (2) (2000) 391-420.

[5] G. Cantor, *Beiträge zur Begründung der transfiniten Mengenlehre I*, Mathematische Annalen 46 (1895) 481-512; translated in [6] below.

[6] G. Cantor, *Contributions to the founding of the theory of transfinite numbers*, with introduction and notes by Philip E; B; Jourdian, Open Court, Chicago, reprinted by Dover, New York 1965.

[7] J. M. F. Castillo, *Nonseparable C(K)-spaces can be twisted when K is a finite height compact*, Topology Appl. 198 (2016) 107-116.

[8] J. M. F. Castillo and M. González, *Three-space problems in Banach space theory, Lecture Notes in mathematics*, vol. 1667, Springer, Berlin, 1997.

[9] P. J. Cohen, *The independence of Continnum Hypothesis*, Proc. National Acad. Sci. USA 50 (6) (1963) 1143-1148.

[10] C. Correa and D. V. Tausk, *Extension property and complementation of isometric copies of continuous function spaces*, Results Math. 67 (2015) 445-455.

[11] C. Correa and D. V. Tausk, *Nontrivial Twisted sums of c_0 and $C(K)$*, J. Funct. Anal. 270 (2016) 842-853.

[12] C. Correa and D. V. Tausk, *Small Valdivia compacta and trees*, Studia Math. 235 (2016) 117-135.

[13] K. Gödel, *The consistency of the Continnum Hypothesis*, Ann. Math. Study 3 (1940), Princeton.

[14] R. B. Jensen, *The fine structure of the constructible hierarchy*, Ann. Math. Logic 4 (1972) 229-308.

[15] O. Kalenda, *Valdivia compact spaces in topology and Banach space theory*, Extracta Math. 15 (1) (2000) 1-85.

[16] W. Kubiś and H. Michalewski, *Small Valdivia compact spaces*, Topology Appl. 153 (2006) 2560-2573.

[17] K. Kunen, *Set Theory: An Introduction to Independence Proofs*, Studies in Logic and the Found of Math. 102, North-Holland Publishing Company, Amsterdam, 1980.

[18] W. Marciszewski and G. Plebanek, *Extension operators and twisted sums of* c_0 *and* $C(K)$ *spaces*, preprint available at http://arxiv.org/abs/1703.02139 .

[19] R. S. Phillips, *On linear transformations*, Trans. Amer. Math. Soc. 48 (1940) 516-541.

[20] A. Sobczyk, *Projection of the space (m) on its subspace* (c_0), Bull. Amer. Math. Soc. 47 (1941) 938-947.

[21] R. M. Solovay and S. Tennenbaum, *Iterated Cohen extensions and Souslin's problem*, Annals of Math. 94 (2) (1971) 201-245.

[22] S. Tennenbaum, *Souslin's Problem*, Proc. National Acad. Sci. USA 59 (1968) 60-63.

ŁUKASIEWICZ, JASKOWSKI AND NATURAL DEDUCTION: CURRY-HOWARD FOR CL[1]

Adrian Rezus

§0. Introduction

The present notes consist of a thorough revision of a draft dated Nijmegen, October 9, 2015, based on previous work.

The bulk of the *historical* information on the *early Polish logic school* (Łukasiewicz, Jaśkowski, Tarski et al.) is based on research done as a graduate student at the University of Bucharest (1972-1977), while busy with a very different subject, abandoned since. My specific interest in 'Polish logic' has been first caused and stimulated by several Romanian mathematicians and philosophers, including some of my teachers and friends.

On the other hand, most of the *technical* details - on λ-calculus - appearing, mainly, in §7, go back to an equally old research project bearing the title *Subsystems* of *type-free* and *typed λ-calculus*, submitted to the Chair of Dirk van Dalen [*Logic and Foundations of Mathematics*] at the University of Utrecht, in 1978. The original project, actually supervised by Henk Barendregt, was focused on the *strict λ-calculi* of Alonzo Church (especially Church's 'ordinal logic', appearing in his Princeton Lectures of 1935-1936), as well as on the proof theory of *relevance logics* - Anderson, Belnap et al. (1975, 1992) - , and concerned only incidentally the proof theory of classical logic, as such. Cf. Rezuş (1981, 1982).

The subject has been revisited on several occasions since: first in the Chair of Nicolaas G. de Bruijn, at the University of Technology of Eindhoven (1982) - where I was involved in research on AUTOMATH -, and next, after 1983, at the University of Nijmegen,

1. Classical Logic

with support from NWO [formerly ZWO], the Dutch National Science Foundation.

The earliest *public* records (seminars, lectures, conferences, etc.), concerning *explicitly* what I was brought to call 'the witness theory of classical logic', are dated - as far as I can remember - *after the mid-eighties* (Canberra ACT 1986, Paris 1987, Nijmegen 1988, Karlsruhe 1990, etc.). See also Rezuş (1983, 1990, 1991, 1993).

As to the very last revisions (2016-2017), I am grateful to J. Roger Hindley (Swansea University, Wales, UK) for stylistic remarks and useful suggestions concerning previous drafts of these notes.

In view of my title, I have adopted, throughout in what follows, the Łukasiewicz notational habits.

§1. Sometime during 1926, while still an undergraduate mathematics student at the Warsaw University, Stanisław Jaśkowski (1906-1965) presented, in the local (i.e. Warsaw) Logic Seminar of his teacher, Jan Łukasiewicz (1878-1956), a 'natural deduction' formulation of classical (two-valued) propositional (including propositional quantifiers), first and second-order logic.

Let us pause, first, on *historico-bibliographical* details. Apparently, the work was done at the instigation of Jan Łukasiewicz [1]. As to terminology, the phrase 'natural deduction' (German: 'natürliche Schliessen'), still in common use in logic today, appeared in print first in Gentzen (1934-1935), although the idea was already clear in the motivation of Jaśkowski's research: he meant, on the authority of his teacher, to design a logic of rules (a 'Regellogick', so to speak), close to the 'natural' mathematical reasoning, as opposed to the 'Satzlogik' of Łukasiewicz himself, i.e. an axiomatic presentation – as in the lectures of Łukasiewicz (1929) – in the footsteps of Ferege (1879),

1. Stanisław Jaśkowski graduated from high-school at eighteen, in 1924, so he was about twenty, by then.

Peirce (1885), Russell (1906), and Whitehead & Russell's 'Principia Mathematica' (1910-1913)[1].

The phrase, 'deduction theory ' [Polish: 'teorja dedukcji'] was initially, Łukasiewicz's own term for 'propositional logic', (including possibly, propositional and / or first- and second-order quantifiers). Cf. the introductory lines and other occasional side-remarks appearing in Jaśkowski (1934)[2].

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1. The contrast 'Satz-' vs 'Regellogik' - roughly: 'sentence / proposition logic' [sic] vs 'rule logic' -, current in the German logico-philosophical literature, mainly after Gentzen, goes back to Frege (1893) and is meant to stress a difference of approach: *pace Frege*, the pioneers were mainly concerned with the formal study of propositions and /or propositional schemes, as expressed by formulas, and the properties thereof (like, e.g., provability in a given 'logistic' system ['Satzsystem'], etc.), while Frege and, subsequently, Gentzen paid also attention to the rules of inference and to their properties (like, e.g., derivability and / or admissibility in a given system [of rules]). With a suggestive term, we may refer to the former approach - and to its defenders / practitioners - as Formularian (with implicit allusion to Peano's various editions of his 'Formulaires', mere collections of [formalized] formulas). Roughly speaking, for a Formularian, a logic is a set of provable formulas, and a provable formula [`thesis' or, even, 'theorem'] is, at best, the codification of a [bunch of] rule[s] of inference. The Formularian approach has been effective in the early development of 'algebraic logic' and, later, in model theory, but is, conceptually speaking, rather inadvertent, since two distinct 'logics' may share exactly the same set of 'tautologies' [provable formulas], while still differing as to the corresponding derivable rules. The alternative rule-oriented approach, suggested by Łukasiewicz in his Warsaw Seminar (1926), was motivated in terms of 'naturalism', by reference to the actual mathematical reasoning and this was, apparently, also the case for Gentzen, a bit later. Technically speaking, the distinction between rule-admissibility [closure of a set of propositions / formulas under a given rule of inference] and explicit [rule-] derivability, already implicit in Gentzen (1934-1935), comes rather late to the attention of the logical theorists; to my knowledge, it is due to Paul Lorenzen (1915-1994) - cf. Lorenzen (1955) - and to Haskell B. Curry (1900-1982), slightly later. The first *explicit* counterexample to the (Formularian) tenet that a logic = *a set of provable formulas*, is due to Henry Hiż (circa 1957-1958), who described a [three-valued] 'logic', **H3** say, containing all classical, two valued tautologies as provable formulas, where not all classically valid rules of inference are derivable. See Hiż (1957, 1958, 1959) and, possibly, Nowak (1992), for a model-theoretical account of **H3**.

2. For bio-bibliographical and historical details on Jaśkowski and his system(s) based on 'supposition rules' [Polish: 'oparta na dyrektywach załozeniowych'], i.e., →

As to publication matters, Jaśkowski's results were promptly announced as Jaśkowski (1927), at the First Congress of the Polish Mathematicians, held on Lvov, September 7-10, 1927. See e.g. the Congress [PPZM] proceeding (1929) and specifically, the references of Lindenbaum (1927). Due to circumstances unknown to me (as well as to other, better informed people, apparently), the final paper appeared actually in print only eight years later (in a projected logic collection edited by Jan Łukasiewicz himself, later to become an international logic journal), as Jaśkowski (1934), more or less simultaneously with Gerhard Gentzen's Göttingen *Inauguraldissertation*, Gentzen (1934-1935).

Before going into the proper details of the subject announced in the title, a few more technical and historical remarks on the material available in print – or otherwise – to Jaśkowski, around 1926, are in order.

Modern logic – also called 'mathematical' or 'symbolic', was (re) born by the end of the XIX-th century (around 1879, in print with an entertaining sequel, in 1893), in two instalments, authored by Gottlobe Frege, viz. Frege (1897) [BS] and Frege (1893) [GGA:1]. There was an intermediate episode, due to Charles S. Peirce (1885) – that Frege ignored – equally worth noting which, although sketchy, was in some respects, conceptually superior to Frege's BS-account. Both Frege and Peirce had a venerable predecessor more than twenty one centuries before they were born, in the work of Chrysippus of Sol[o]i (this was

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← proper / general rules of inference, using 'assumptions' in their premises, viz., entailments, see, e.g.,Dubikajtis (1967, 1975) - a reliable informant on the logical whereabouts of Stanis law Jaśkowski, Lech Dubikajtis (1927-2014) was a former PhD student and a research assistant of Jaśkowski at the Warsaw National Institute for Mathematical Sciences (currently, the Institute of Mathematics of the Polish Academy of Sciences) -, as well as Kotas & Pieczkowski (1967), Or łowska (1975), Indrzejczak (1998, 2016), Piętka (2008), and, possibly, the textbook Borkowski & S łupecki (1963). On the subsequent history of 'natural deduction' - a comedy of conceptual errors, indeed -, see, e.g., Pelletier (1999, 2000), and Hazen & Pelletier (2012, 2014). General information on the 'Lvov-Warsaw school' can be found in Woleński (1985, 2015), Jadacki (2006), Wybraniec-Skardowska (2009), etc.

an obscure place in *Cylicia Campestris,* nowadays in modern Turkey), a Phoenician emigrant to Athenes, the father of Stoic logic and the grand-father of [classical] logic *tout court.* The latter (historical) fact was first noted by Jan Łukasiewicz, sometime during the early 1920[1].

A less known (historical) fact is that Frege came out with two 'logics', not with a single one: a *Satzlogik* (2879), and a *Regellogik* (1893). Both were 'axiomatic' by modern standards. The latter one meant to be closer to actual 'mathematical thinking' (a kind of formal counterpart of 'natural deduction', as occurring in mathematical texts), and was vastly anticipating among other things, Gentzen (1934-1935), for instance.

The other relevant (historical) fact is that neither Gentzen nor any other *Göttinger* – David Hilbert (1862-1943) or Paul Hertz (1880-1940), for that matter[2] – had ever read Frege's GGA (1893) [sic].

In this matter, I cannot, however, speak for the Lvov-Warsaw Poles - those active in logic before cca 1935 - because we lack the right kind of (historical) documentation. Both Łukasiewicz (1929) and Jaśkowski (1934) mention only Frege's axiomatic system of BS, while, curiously enough, the young Alfred Tajtelbaum [aka Tarski] (1901-1983) does not refer to Frege's 'logics' at all[3].

On the other hand, the main trouble with Frege (1879, 1893) was in the fact he did not recognize the *general* concept of a 'rule of inference'. Specifically, with [material] implication, [classical] negation and the [classical] universal quantifier as primitives, he only

1. Circa 1923. See the final outcome in Łukasiewicz (1934) and, possibly, Rezuş (2007, rev. 2016), for the main claim.

2. On the authority of Paul Bernays (1888-1977), Gentzen borrowed his 'structural' rules from Hertz, a former physics student of Hilbert in Göttingen. Cf. Rezuş (2009, rev. 2016).

3. See, e.g. the *Bibliography* and *Index of Names and Persons* of the collection Tarski (1956).

acknowledged 'flat' rules (more or less like the algebraic operations), of the form:

$$(\flat) \vdash \alpha_1, \ldots \vdash \alpha_n \Rightarrow \vdash \beta,$$

Rejecting, implicitly, the (old-fashioned) idea of entailment (= finite sequence of propositions, with exactly one being tagged *qua* 'conclusion'):

$$(\vdash) \alpha_1, \ldots \alpha_n \vdash \beta,$$

As a legitimate - and otherwise essential - logic concept[1].

The young Bertrand Russell (1872-1970) - the only (more or less competent) person who did actually read Frege (1893) in the epoch[2] - was less interested in such absconse distinctions[3], so he missed the point, as well, and stuck to axiomatic, in the shadow of Frege (1879) and Peirce (1885)[4].

As another aside, I was, so far, unable to date exactly the event as such, in the moderns, viz. the identification of the concept of a *general rule of inference*[5]. The earliest date I am able to quote is 1921, when Alfred Tarski (Leśniewski's only PhD student) noticed the so-called

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1. In this respect, Frege was about two millennia behind Chrysippus (and, even, Aristotle, in a way). On this, see, e.g., Rezuş (2007, 2009, rev. 2016).

2. As I could gather from the newest Russell expertise, this happened sometime around 1902-1903.

3. As noticed, in passing, by Kurt Gödel, Russell was even later quite confused about the general concept of a *logical rule of inference*.

4. Although he did not acknowledge the latter source. Cf. Russell (1906).

5. One should perhaps read, once more, carefully, the rather vast output of Stanisław Leśniewski, on this. Cf. the collection Leśniewski 1992.

'Deduction Theorem' (DT)[1], i.e., the implication-introduction rule of Gentzen (1934-1935), an obvious case of 'non-flat' inferential rule, with an entailment (including 'assumptions') as a premise:

$$(DT)\alpha \vdash \beta \Rightarrow \vdash C\alpha\beta,$$

Certainly, Łukasiewicz was aware of such details, if not in 1921, at least sometime before 1926, when he assigned his [very] young student (Jaśkowski) the home-work leading to Jaśkowski (1927, 1934). Anyway, the implication int-elim rules (the 'Deduction Theorem' and the famous *modus ponens* / detachment rule) appear explicitly in Jaśkowski's early home-work, and so, a *fortiori*, in Łukasiewicz's Warsaw Seminar, sometime during 1926[2].

Whence a question: 'Why hasn't Jan Łukasiewicz solved the problem [the one assigned to young Jaśkowski] himself - sometime before 1926 – and presented the outcome is his famous lectures Łukasiewicz (1929)?' Because he had at hand the (conceptual and technical) means to do it, anyway. Which is *what I mean to show next*. In order to do this, in proper terms, I need a conceptual revision of the received views on proving (in logic) and some appropriate notation and terminology.

§2. *Rules of inference as witness-operators***.** A typical case of the 'flat' rule (♭) above is the *modus ponens* or the *detachment rule*, in logics with implication [here, C], either primitive or defined:

$$(\rhd) \vdash C\alpha\beta, \vdash a \Rightarrow \vdash \beta,$$

1. See Axiom 8* in Tarski (1930), and the footnote on page 32, in the collection Tarski (1956), for references. Some authors used to credit Herbrand with the discovery. However bright, Jacques Herbrand (1908-1931) was a teenager, just 13 years old, in 1921, so it is unlikely he spotted errors in Frege's [German] texts nobody used to read by then, even in Germany!

2. At a quantifier-free level, Jaśkowski had a third rule - of the same kind as (DT), actually -, yet a less inspired choice I will go into later on.

Now, in axiomatic presentations of a given logic (classical, two-valued logic, for instance), the premises α_i $(0 < i < n + 1)$ of a 'flat' rule of the form (♭) are taken to hold 'unconditionally', without further assumptions, they are provable formulas (expressing propositions / propositional schemes), 'theorems' or 'theses' (in the jargon of the early Polish 'school'); alternatively, they are, semantically, *true* (or else two-valued '*tautologies*', in the classical case).

In fact, any particular axiomatic amounts to an *inductive definition* of the predicate 'provable' (expressed by notation ⊢) applying to formulas (expressing propositions or propositional schemes): the axioms are paradigmatically provable (the basis of the induction), while any (primitive) 'flat' rule of inference carries this property - *provability* - from premises to conclusion (inductive step).

As long as we have only primitive rules of the form (♭) around, 'proving axiomatically' amounts to a piece of *algebraic notation*: a 'flat' primitive *rule of inference* with *n* premises (*n* > 0) looks like a usual algebraic nary operation[1], whereupon a derivable rule of inference is just an *explicit* definition of an operation in terms of 'primitive' operations (here, axioms and primitive rules of inference).

'Operations on what?' one might wonder. A first - approximate – answer could be: 'On formulas.'[2] A slightly better one would amount

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1. In the limit case (n = 0), the axioms may be thought of as null-ary operations, if we want full generality.

2. This was actually the case, historically speaking: the idea came first - exactly in these terms - to a later (Irish) student of Łukasiewicz, in the '(logical) Polish quarter' of Dublin, during the early fifties. [After the WWII, 'being unwilling to return to [...] Poland [...], Łukasiewicz looked for a post elsewhere. In February 1946 he received an offer to go to Ireland. On 4 March 1946 the Łukasiewiczes arrived in Dublin, where they were received by the Foreign Secretary and the Taoiseach Eamon de Valera. In autumn 1946 Łukasiewicz was appointed Professor of Mathematical Logic at the Royal Irish Academy (RIA), where he gave lectures at first once and then twice a week. Simons (2014)] See the references to the $\mathfrak{D}$-operator, the 'condensed detachment' operator, of Carew A. Meredith, below, and, possibly the →

to an additional piece of formalism, to be justified, intuitively, as follows:

Proving something - a proposition expressed by formula α, say – amounts to *providing a reason* - or '*grounds*' - for α, or else, like in court, to displaying a *witness* a for α. Formal notation: $\vdash a : \alpha$.

With this minimal formal equipment, in axiomatic presentations, the axioms are to be witnessed by primitive constants (possibly parametric, in the case of axioms schemes), whence 'witnessing' a 'flat' rule of the form ($\flat$) would amount to providing an operation (operator) [and a piece of explicit formal notation $\flat(a_1, \ldots, a_n)$, such that

$$[\ \flat\ ] \vdash a_1 : \alpha_1, \ldots, a_n : \alpha_n \Rightarrow\ \vdash\ \flat(a_1, \ldots, a_n) :\ \beta;$$

So, in particular, 'witnessing' a 'flat' rule like *modus ponens*, for instance, would consists of using a binary operation $\rhd$, say, to the effect that

$$[\rhd] \vdash f : C\alpha\beta, \vdash a : \alpha \Rightarrow \vdash (f \rhd a) : \beta.$$

Summing up, a 'flat' rule of inference is just an *algebraic operation*, in this view. Note, however, that, as long as we do not *define explicitly* the 'operations' $\flat$, we have only a *witness notation*, at most. In other words, in order to have a *witness theory* - as a formal counterpart of (axiomatic) proving - we must be able to *characterize the witness operations* first. Usually, we can do this, like in algebra, by equational conditions, expressing witnessor *proof-isomorphisms*.

The general case is obtained from the 'flat' case by 'parametrisation' so to speak, where the parameters are finite (possibly empty) sequences of formulas (expressing propositions, resp.

← notes Meredith (1977) - by David Meredith, the American cousin of Carew, also a logician - for further historical details.

propositional schemes) $\Gamma_i := [\beta_{i,1}, \dots, \beta_{i,m_i}]$ $(0 < i < n+1, m_i \geq 0), resp. \Gamma := [\beta_1, \dots, \beta_m]$ $(m \geq 0)$, called 'assumption contexts' (alternatively: *witness-contexts* or *proof-contexts*). Every premise of a general rule is thus an entailment of the form $\Gamma_i \vdash \alpha_i$ $(0 < i < n+1)$, while the rule has a conclusion of the form $\Gamma \vdash \beta$, i.e., one has

$$(\flat \vdash)\ \Gamma_1 \vdash \alpha_1, \dots, \Gamma_n \vdash \alpha_n \Rightarrow \Gamma \vdash \beta,$$

With 'witnessed' counterpart of the form

$$[\flat \vdash]\ \hat{\Gamma}_1 \vdash a_1 : \alpha_1, \dots, \hat{\Gamma}_n \vdash a_n : \alpha_n \Rightarrow \hat{\Gamma} \vdash b : \beta,$$

Where $\hat{\Gamma}_i \coloneqq [x_{i,1} : \beta_{i,1}, \dots, x_{i,m} : \beta_{i,m_i}]$, the 'witnessed' counterpart of Γ_i (and analogously for $\hat{\Gamma}$ and Γ) contains 'decorated - or typed - witness-variables', allowing us to manipulate the witness-contexts[1].

In particular, in the limit case, a null-premiss rule of inference is just an entailment (considered valid). Examples in point:

$$[id]\ \alpha \vdash \alpha,$$

Or more generally,

$$[prj]\ \Gamma \vdash \alpha_i, \text{for } \Gamma \equiv [\alpha_1, \dots, \alpha_n], (0 < i < n+1),$$

$[\rhd]$ $C\alpha\beta, \alpha \vdash \beta$ $(modus\ ponens, viewed\ as\ a\ valid\ entailment)$, etc., and analogously for the witnessed variants:

$$[id \vdash]\ x : \alpha \vdash x : \alpha,$$

Resp.

$$[prj \vdash]\ \hat{\Gamma} \vdash x_i : \alpha, for\ \hat{\Gamma} \equiv [x_1 : \alpha_1, \dots, x_n : \alpha_n], (0 < i < n+1),$$

1. Besides, one must have additional rules, called 'structural rules', in the current proof theoretic terminology borrowed from Gentzen (1934-1935), that are rather trivial, and remain un-expressed, formally, in usual presentations of 'natural deduction' .

$$[\rhd\vdash]\ z : C\alpha\beta, x\colon \alpha\ \vdash (z \rhd x) : \beta.$$

Here, in [♭ ⊢], the witness b, appearing in the conclusion, must be of the form $\flat\,(\natural_1(a_1), \dots, \natural_n(a_n))$,where the prefixes $\natural_i$ $(0 < i < n + 1)$ are either empty (*nil*) or specific variable-binding operations, called 'abstraction operators', acting on finite sequences $\vec{x}_i \equiv [x_{i,1}, \dots, x_{i,m_i}], (m_i > 0)$, of *pairwise distinct witness-variables*) and the associated 'body' a_i. In each case, a witness variable is decorated (or 'typed') by an associated formula thereby witnessed 'hypothetically'.

Of course, if every ♮-prefix is empty, we have a 'flat' rule, the 'degenerated' case. E.g., in particular, the most general forms of *modus ponens*, viewed as a rule of inference, should be

$$[\rhd\vdash \otimes]\hat{\Gamma} \vdash f : C\alpha\beta, \hat{\Gamma} \vdash a :\ \alpha \Rightarrow \hat{\Gamma} \vdash (f \rhd a) : \beta$$
['Parametric']

or

$$[\rhd\vdash \oplus]\hat{\Gamma}_1 \vdash f\colon\ C\alpha\beta, \hat{\Gamma}_2 \vdash a : \alpha \Rightarrow \hat{\Gamma}_1, \hat{\Gamma}_2 \vdash (f \rhd a) : \beta$$
['Cumulative'].

In general, however, a rule of inference can be arbitrarily complex, so that the identification

(General) rule of inference = witness **operator**

Goes beyond the conventional views on 'algebraic operators'[1]. In order to Accommodate, formally, the terminology - and the notation - , one

1. The so-called 'abstraction-operators' - and, in general, the variable-binding mechanisms - are not welcome in (abstract) algebra, indeed. This on historical reasons, likely. Nicolaas G. de Bruijn observed once, in conversation, that abstraction-operators do not occur in pre-XIX-century mathematics. This explains, in a way, the initial lack of interest in such phenomena among algebraists. In recent times, when confronted incidentally with such cases - first-order quantifiers, for instance -, they made appeal to elaborated local solutions in order to cope with the problem. Paul Halmos and Alfred Tarski invented specific constructions - *polyadic algebras*, resp. *cylindric algebras* - in order to algebraize first-order logic with →

can use the idea of a *generic arity* (gen-arity, for short), viewed as a finite sequence of non-negative integers, to be associated to an arbitrary operator, taken in the new sense.

In this setting, the algebraic [null-ary] constants would get gen-arity nil [= the empty sequence], the usual n-ary algebraic operations would get gen-arity [0, ... , 0] (n times 0, n > 0), the n-adic abstractorswould get gen-arity [n] (n > 0), so that the monadic λ-abstractor, as well as the usual quantifiers, for that matter, must have gen-arity [1], the dyadic abstractor *split* [∫], mentioned incidentally below, has gen-arity [2], and so on. In particular, the 'mixed' operators (gen-arity $[k_1, \ldots, k_n],\ k_i > 0$) can be handled as 'flat' (algebraic) n-ary operations acting on k_i-adic abstractors ($0 < i < n + 1$).

In practice, however, we rarely, if ever, encounter complex rules corresponding to 'mixed' operators; we are normally confronted with 'flat' (ordinary algebraic) operators or with n-adic abstractors with $n := 1{,}2$ (operators of gen-arity [1] or [2]), at most, so that, in the end, the talk about gen-arities amounts to a piece of empty generality[1].

← quantifiers, resp. quantifiers and equality. In more general situations, modelling abstraction operations in mathematical terms requires specific category theoretic methods and constructs that go far beyond the traditional algebraic way of thinking about 'operations' and 'operators'. Moreover, there are genuine phenomena, occurring frequently in computer science - like, e.g. the *non-local control* (typically, jumps), the *side effects*, or the so-called *continuations* - that can be easily described in terms of abstraction-operations, but whose behavior resist algebraisation, as understood in traditional terms. The (general) logical rules of inference fall within the same category.

1. Like in the usual algebraic case, in fact, as we do not encounter 13-ary or 17-ary operations in current mathematical practice, either. In logic, an exception can be encountered in the usual presentation of intuitionistic propositional logic, where the so called or-elimination rule (case-analysis) is a witness operator of gen-arity [0,1,1], as well as in the case of Jaśkowski's rule _ (a witness-operator of gen-arity [1,1]) to be discussed below.

§3. Essentially the Łukasiewicz Warsaw Lectures of 1928-1929, Łukasiewicz (1929), contain a very detailed axiomatic presentation of

(1) [Classical] propositional logic, based on the signature [N,C] (classical negation and material implication, in Łukasiewicz notation)[1], and

(2) A mild - yet very clean - version of the 'extended propositional [classical] logic', i.e., the [classical] propositional logic with propositional quantifiers, à la Peirce (1885), Russell (1906) and Tarski (doctoral diss., Warsaw 1923, under Leśniewski), or else Leśniewski's 'protothetic', for that matter[2].

Now, except for a minor detail, the latter one is not more than the former, because we can define explicitly the [classical] propositional quantifiers in terms of [classical] connectives K [and], A [*or*] (in Łukasiewicz notation), and propositional constants **v** [*verum*] and **f** [*falsum*], anyway (just 'truth value quantifiers', as Nuel Benap Jr would have had them[3]). The 'minor detail' refers, here, to the fact that the primitive [N,C]- signature is not *functionally complete*: we cannot obtain the propositional constants **v** and **f** from [N,C] alone. This does not affect our discussion of (2) below, as the signature [N,C,Π], with Π for the *universal propositional quantifier*, is functionally complete and even redundant, since one can define **f** and **N** à la Peirce (1885) by f := Πp.p and Nα := Cα**f**, resp.[4] In an axiomatic quantifier-free setting - as in Łukasiewicz (1929), Chapter II – the absence of the

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1. Completeness is shown in Łukasiewicz (1929), Chapter III, §**22**. Cf. also Łukasiewicz (1931).

2. Specifically, Chapter IV of Łukasiewicz (1929) is based 'in great part' [see the Preface of the first edition] on Tarski's previous work. Cf. also Łukasiewicz & Tarski (1930), §**5**.
For Leśniewski, see now Leśniewski (1992).

3. Cf., e.g., *mutatis mutandis*, Anderson & Belnap (1992) [**2**], §**33.4.**

4. As actually done in Łukasiewicz (1929), Chapter IV, x**24**.

propositional constants might, however, affect the translation off the axioms in terms of rules of inference (and conversely). The point is that we need a primitive **f** [*falsum*] in order to express something as simple as the 'law of (non-) contradiction', for instance, in inferential (entailment-like) terms.[1] By adjoining a *falsum*-constant f to [N,C], the Łukasiewicz original axiom system is, however, incomplete as it stands. We cannot even prove, from the Łukasiewicz axioms, the 'thesis' ⊢ $\boldsymbol{V}$ [$\equiv N\boldsymbol{f}$], for instance.[2]

Recall that the Łukasiewicz (1929) quantifier-free axioms, with *modus ponens* and substitution as only 'rules of inference', are (in Łukasiewicz – 'Polish' - spelling):

$$\vdash \boldsymbol{CB}[p,q,r] \coloneqq 1 : CCpqCCqrCpr$$

[transitivity of implication: 'suffxing'] - axiom in Peirce (1885)

$$\vdash E[p] \coloneqq 2 : CCNpp$$

[the *consequentia mirabilis* of Gerolamo Cardano (1570) or the *Law of Clavius*, viewed as a 'thesis']

$$\vdash \boldsymbol{O}[p,q] \coloneqq 3 : CpCNpq$$

[ex *contradictione quodlibet*, 'explosion'].

To this team, we add, for reasons discussed above:

$$\vdash \boldsymbol{\Omega} : \boldsymbol{V}.$$

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1. The 'algebraic' alternative - which consists of [1] defining first a 'relative' *falsum* by $\boldsymbol{f}[\alpha] := NC\alpha\alpha$, say, and [2] proving next $E\boldsymbol{f}[\alpha_1]\boldsymbol{f}[\alpha_2]$, for any two fomulas α_1, α_2 etc. - induces unnecessary formal complications. A similar remark applies to Jaśkowski's 'natural deduction' quantifier-free system, based originally on [N,C] alone.

2. It turns out that all we need is just a single new axiom ⊢ **v**, for this purpose. For completeness, see, for instance, Wajsberg (1937), I, §**5**, resp. 1939, II, §**2**, and the remark (below) that *ex falso quodlibet* can be obtained from the Łukasiewicz axiom ⊢ **O**[p,q] : CpCNpq and a 'paradigmatic' proof of **v**, like ⊢**Ω** : **v**.

§3.1. *Proof-combinators***.** Taking **CB**, **E** and **O** - with the appropriate propositional parameters (here: [p,q,r,] [p], and [p,q], resp.) -, as well as **Ω**, in guise of primitive 'witnesses' for the corresponding axioms, detachment / *modus ponens* can be viewed as a binary (algebraic) operation $\mathfrak{D}$, from 'detachment' (to be defined properly - i.e., equationally - later on) acting on witnesses, to the effect that:

($\rhd$= *modus ponens*) if **f** is a witness for C$\alpha\beta$ and a is a witness for α, then $\mathfrak{D}$fa is a witness for β.

We write, for convenience, f(a) = (f $\rhd$ a) := $\mathfrak{D}$fa. This is to be understood modulo arbitrary uniform substitutions, with the proviso that one must take most general substitutions into account (here, substitutions are endomorphisms of the corresponding [free] algebra).[1]

For the record, the formal grammar (for formulas, resp. *witness terms* [*proof-terms* or *w-terms*, for short]) is:

Propositional variables :: p, q, r, ...

Formulas :: α, β := p | Nα | C$\alpha\beta$

W-variables :: x, y, z, ...

W-terms :: a,b,c,d,e,f := x | **Ω** | **CB** | **E** | **O** | c$\rhd$a.

Where α is a formula and a is a w-term, we write, as ever, $\vdash$ a : α, for the fact that a is a witness (actually, a w-term) for α.

So, we have, in particular, *derived* rules (here, *definable witness-operators*):

1. Technically speaking, the $\mathfrak{D}$-operator is the 'condensed detachment' operator of Carew A. Meredith (1904-1976). Notably, the Irishman attended Łukasiewicz's lectures in Dublin, during the early 1950. See, e.g., David Meredith's bio-bibliographical note, Meredith (1977), and, possibly, Rezuş (1982, 2010), Kalman (1983), and Hindley & Meredith (1990), for details.

$\vdash g \circ f \coloneqq \boldsymbol{CB}[p,q,r](f)(g) : Cpr, if \vdash f : Cpq \; and \; \vdash g : Cqr,$

$\vdash \boldsymbol{E}[p](f) : p, if \vdash f : CNpp,$

$\vdash \boldsymbol{O}[p](a)(c) : q, if \vdash a : p \; and \; \vdash b : Np.$

Examples, ignoring propositional parameters on witnesses, as well as explicit substitutions[1]:

$\vdash 4 \coloneqq \boldsymbol{CB} \rhd \boldsymbol{CB} : CCCCqrCprsCCpqs$

$\vdash 5 \coloneqq 4 \rhd 4 = (\boldsymbol{CB} \rhd \boldsymbol{CB}) \rhd (\boldsymbol{CB} \rhd \boldsymbol{CB}) : CCpCqrCCsqCpCsr$

$\vdash 6 \coloneqq 4 \rhd 1 = (\boldsymbol{CB} \rhd \boldsymbol{CB}) \rhd \boldsymbol{CB} : [exercise]$

$\vdash 7 \coloneqq 5 \rhd 6 : [exercise]$

$\vdash 8 \coloneqq 7 \rhd 1 : [exercise]$

$\vdash 9 \coloneqq 1 \rhd 3 = \boldsymbol{CB} \rhd O : [exercise]$

…

$\vdash \boldsymbol{I} \coloneqq 16 = 9 \rhd 2 = (\boldsymbol{CB} \rhd \boldsymbol{O}) \rhd \boldsymbol{E} : Cpp - axiom \; in \; Peirce$ (1885)

and so on. Further, Łukasiewicz meticulously obtained

$\vdash \boldsymbol{K} \coloneqq 18 : CqCqp$ $['the \; law \; of \; simplification']$ –
axiom in Frege BS (1856)

$\vdash \boldsymbol{CI} \coloneqq 20 : CpCCpqq$
['assertion' or internalised *modus ponens*]

---

1. The latter can be uniquely restored (modulo alphabetic variants) by the Robinson *unification algorithm*. Cf. Rezuş (1982).

$\vdash \boldsymbol{C} := 21 : CCpCqrCCqCpr$
['the law of commutation'] - axiom in Frege BS, as well as in Peirce (1885) [Note by Łukasiewicz (cca 1925): superfluous in Frege BS, it can be already obtained from **K** and **S**.]

$\vdash \boldsymbol{B} := 22 = \boldsymbol{C} \rhd \boldsymbol{CB} : CCqrCCpqCpr$
[transitivity of implication: 'prefixing']

$\vdash \boldsymbol{P} := 24 : CCCpqpp$
['the Law of Peirce'] - axiom in Peirce (1885)

$\vdash \boldsymbol{W} := 30 : CCpCpqCpq$
['Hilbert' or 'contraction']

$\vdash \boldsymbol{S} := 35 : CCpCqrCCpqCpr$
['Frege' or 'selfdistribution on the major'] - axiom in Frege BS

$\vdash \Delta := 39 : CNNpp$
['law of double negation' (elim)] - axiom in Frege BS

$\vdash \nabla := 40 : CpNNp$
['law of double negation' (intro)] - axiom in Frege BS

…
[46-49: 'the laws of transposition' (or 'contraposition')]

$\vdash 46 : CCpqCNqNp$

$\vdash 47 : CCpNqCqCNp - axiom\ in\ Frege\ BS$

$\vdash 48 : CCNpqCNqp$

$\vdash 49 : CCNpNqCqp$

etc.

This amounts to a 'typed' (stratified, decorated) combinatory logic notation, where one manipulates formulas in guise of so-called principal type schemes.[1]

Now, as Tarski should have known (in 1921), in presence of *modus ponens*, the Deduction Theorem (DT) - or implication-introduction - can be obtained from **K** and **S** alone. This yields the λ-calculus counterpart of the same story.

§3.2. *(DT) and the λ-abstraction-algorithm***.** I have argued at length elsewhere that Tarski must have been, likely, familiar with some form of (typed) λ-calculus - or (typed) combinatory logic or both - during the early 1920, knowledge that enabled him to prove some tricky axiomatizability results around 1925.[2]

Indeed, there is, essentially, a single way of proving (DT): the proof amounts to a simple *inductive argument*.

The reasoning can be repeated in any (propositional) logic - with substitution and modus ponens, as only primitive rules of inference - that contains the (witnessed) 'theses':

$(\boldsymbol{K})\ \vdash\ \boldsymbol{K}[p.q] : CpCqp,$ and

$(\boldsymbol{S})\ \vdash\ \boldsymbol{S}[p,q,r] : CCpCqrCCpqCpr.$

Note first that, in such cases, one has, as derived rules:

1. Cf. Hindley (1969, 1997), Hindley & Seldin (1986), and Hindley & Meredith (1990). As a matter of fact, here, one has a 'rigid' typing, à la Church and de Bruijn, instead. For the difference, see Hindley (1997), Barendregt et al. (2013) and the review Rezuş (2015). We could have had a (typed) combinator theory - a 'combinatory logic' -, as well, but, since the equational constraints on the primitive combinators are rather non-transparent, I prefer to skip the details. Otherwise, they can be recovered from remarks following below.

2. See, e.g, Rezuş (1982) and the discussion appearing by the end of Rezuş (2010).

$[\boldsymbol{K}]\ \hat{\Gamma} \vdash f : p \Rightarrow \hat{\Gamma} \vdash \boldsymbol{K}[p,q](f) : Cqp$, and

$[\boldsymbol{S}]\ \hat{\Gamma} \vdash f : CpCqr, \hat{\Gamma} \vdash g : Cpq \Rightarrow \hat{\Gamma} \vdash f\Box g : Cpr,$

Where $f\Box g := \boldsymbol{S}[p,q,r](f)(g)$, as well as the (witnessed) 'thesis':

$(\boldsymbol{I}) \vdash \boldsymbol{I}[p] :\ Cpp$

[ignoring propositional parameters, the latter is available as **S**(**K**)(**K**)].

Suppose that we have obtained a proof b[x] of β from the assumption that we have a proof x of α (so that b[x] depends possibly on [x:]). Then (DT) states that we must have a proof $\lambda x{:}\alpha.b[x] := \lambda([x{:}\alpha](b[x]))$ of C, that does not depend on the proof [x:], ceteris paribus.[1] That is to say, formally,

$$(\lambda)\ \hat{\Gamma} \vdash \lambda x{:}\alpha.b[x] : C\alpha\beta, if\ \hat{\Gamma}, [x{:}\alpha] \vdash b[x] :\ \beta,$$

For an appropriate assumption-context $\hat{\Gamma}$, as a parameter in the argument.

The induction pays attention to the form ('structure') of b[x]. To save repetitions, set $e \equiv \lambda x{:}\alpha.b[x]$. There are only three cases to examine:

(1) $b[x] \equiv [x :\ \alpha];\ so\ \alpha \equiv \beta;\ set\ e := \boldsymbol{I}[\alpha] :\ C\alpha\alpha;$

(2) $b[x] :\ \beta\ does\ not\ actually\ depend\ on\ [x :\ \alpha];\ set\ e := \boldsymbol{K}[\beta,\alpha](b) :\ C\alpha\beta;$

(3) $b[x] \equiv (f \rhd g){:}\beta;\ where\ f :$

1. The ceteris paribus clause refers to the fact that the argument can be taken relative to a parameter $\hat{\Gamma} \equiv [x_1{:}\alpha_1 \ldots x_n{:}\alpha_n]$ $(n > 0)$.

$C\alpha'\beta$ *and* $a: \alpha'$; *then the* (IH) *guarantees* $\hat{f} := (\lambda x: \alpha. f) : C\alpha C\alpha'\beta$ *and* $\hat{a} := (\lambda x: \alpha. a) : C\alpha\alpha'$; *set* $e := \hat{f} \square \hat{a} : C\alpha\beta$.[1]

§3.3. *An extended λ-calculus***.** As in (decorated / 'typed') λ-calculus, we can thus write (ignoring everywhere proof-context parametrisations):

$$(\lambda) \vdash \lambda x{:}p.b[x] : Cpq, if\ [x{:}p] \vdash b[x] : q,$$

On a par with the usual 'cut'-condition [*modus ponens*]:

$$(\rhd) \vdash f \rhd a : q, if \vdash f : Cpq\ and\ a : p.$$

As one might already guess, this makes up the first step in a would be attempt meant to replace the Łukasiewicz axioms - i.e., the primitive combinator team {**CB**, **E**, **O**} - together with **Ω**, on the signature [**f**, N,C], by appropriate witness operators (rules of inference).
Set now

$$(\epsilon) \vdash \epsilon x{:}Np.a[x] := \boldsymbol{E}(\lambda x{:}Np.a[x]) : p, if\ [x{:}Np] \vdash a[x] : p.$$

The latter derived rule (definable witness-operator) is the *consequentia mirabilis* of Gerolamo Cardano (1501-1576) or the *Rule of Clavius*, viewed as a single-premiss rule of inference[2].

1. This is the so-called 'bracket abstraction algorithm' obtained first in terms of combinators - and, rather late, in this form -, by Haskell B. Curry (in 1948-1949) and, independently, by Paul C. Rosenbloom (1950, 2005), that is about thirty years after Tarski. See also Rosser (1942, 1953) and Curry & Feys (1958), 6S.1, etc. One can improve on the last clause (3), by processing first the subcase a ≡ x : $\alpha \equiv \alpha'$, while setting e:= f : C$\alpha'\beta \equiv$ C$\alpha\beta$.

2. Cf. Cardano (1570), Lib. **V**, Prop. 201, resp. Cardano (1663) **4**, p. 579. For pater Clavius [Christophorus Clavius, aka Christoph Klau, SJ (1537-1612)], cf. Clavius (1611) 1.1, pp. 364-365 [comments ad *Euclid Elementa* **IX.12**], as well as **1.2**, p. 11 [comments *ad* Theodosius *Sphaerica* **I.12**]. See, also Rezuş (1991, rev. 1993), pp. 4, 23, 46, and Bellissima & Pagli (1996) *passim*, for details. Notably, Łukasiewicz was familiar with the references above, as well as with the medieval anticipations of →

As announced before, we adjoin the propositional constant **f** (*falsum*), with **v** := N**f** (*verum*), and a single additional (witness) axiom:

$$(\boldsymbol{\Omega}) \vdash \boldsymbol{\Omega} : \boldsymbol{V}$$

and set[1]

$$(\varpi) \ \vdash \ \varpi[p](e) := \boldsymbol{O}[\boldsymbol{f}, p](e)(\boldsymbol{\Omega}) : \ p, if \ \vdash \ e : \ \boldsymbol{f},$$

with, finally

$$(\star) \vdash c \ \star \ a := \boldsymbol{O}[p, \boldsymbol{f}](a)(c) : \ \boldsymbol{f}, if \vdash a : \ p, and \vdash c : \ Np$$
['inner cut' or the 'rule / law of (non-) contradiction'].

Conversely, **E** and **O** can be obtained as

$$(\boldsymbol{E}) \ \vdash \ \boldsymbol{E}[p] := \ \lambda f{:}\, CNpp.\epsilon x{:}\, Np.(f \star x) : \ CCNppp,$$

$$(\boldsymbol{O}) \ \vdash \ \boldsymbol{O}[p,q] := \lambda x{:}\, p.\lambda y{:}\, Np.\varpi[q](y \star x) : \ CCpCNpq.$$

As is well-known, the rules (λ), (⊳), (ϵ), (ϖ), and ($\star$), with the additional axiom (**Ω**), suffice to yield full classical [propositional] logic.[2]

On the other hand, if ($\star$) is present, the rules (ϵ) and (ϖ) of the [**f**,N,C]- signature, taken together, are equivalent, in this context, to *reductio ad absurdum*, (∂), viewed as a single- premise rule

$$(\partial) \ \vdash \ \partial x{:}\, Np.e[x] : \ p, if \ [x{:}\, Np] \ \vdash \ e[x] : \ \boldsymbol{f}.$$

← his O-axiom (the `Law of Duns Scotus'). Cf. e.g., Łukasiewicz (1929) and Łukasiewicz (1930), Chapter II, §**8**.

1. This is the only place where we actually need **Ω** in derivations.

2. If the basis consists only of rules, as here, the axiom (**Ω**) is redundant. See below.

Indeed, one has

$$(\partial) \vdash \partial x{:}Np.e[x] := \epsilon x{:}Np.\varpi[p](e[x]) : p, if\ [x{:}Np] \vdash e[x] : \boldsymbol{f},$$

and, conversely,

$$(\epsilon) \vdash \epsilon x{:}Np.a[x] := \partial x{:}Np.(x \star a[x]) : p, if\ [x{:}Np] \vdash a[x] : p, and$$

$$(\varpi) \vdash \varpi[p](e) := \partial x{:}Np.e, if\ [x{:}Np] \vdash e : \boldsymbol{f}\ (x\ not\ free\ in\ e),$$ [1]

So that, finally, classical [propositional] logic can be based on

(1) The axiom (**Ω**), and the four rules:

(2) (λ) [the 'Deduction Theorem', implication-introduction],

(3) (⊳)[*modus ponens*, implication-elimination],

(4) (∂) [*reductio ad absurdum*], and

(5) (⋆) ['the law of (non-) contradiction'].

The axiom is, in fact, redundant, since, in this case, one can define explicitly:

$$(df\boldsymbol{\Omega}) \vdash \boldsymbol{\Omega} := \partial x{:}Nv.\partial y{:}v.(x \star y) : v.$$

We shall keep, however, **Ω** around for a while, mainly for the sake of comparison with the Jaśkowski (1934) version of 'natural deduction'.

The 'natural deduction' system above is easily seen to be equivalent to the axiomatics of Łukasiewicz (1929), modified as above such as to fit the primitive [**f**,N,C]-signature. As the rules have been already seen to be derivable from the axioms, this amounts to writing down the explicit definitions of the witnesses (here,

1. No need for **Ω**, here. Cf., e.g., Rezuş (1990, 1991).

combinators) **CB**, **E**, and **O** in terms of the proof-operators contained in the 'basis' {λ, ⊳, ∂, ⋆}.

The corresponding (extended) λ-calculus is discussed next. It turns out that - if we forget about the constant - one can even formulate it in a decoration-free ('type-free') setting. This allows us establishing (its Post-) *consistency* in a straightforward way, using only some very basic λ-calculus facts.

§4. On the primitive [**f**,N,C]-signature, the minimal setting above – consisting of (**Ω**) [otherwise redundant], (λ) [implication-introduction], (⊳) [implication-elimination], (∂) [*reductio ad absurdum*], and (⋆) ['the law of (non-)contradiction'] - can be viewed as an extension of the basic ('simple') typed λ-calculus λ[C], obtained by 'replicating' its pure (λ)-(⊳)-part.

Formally, the decoration-free (`type-free') syntax of the resulting λ∂-calculus - λ(**Ω**), say - is given by:

Witness-variables :: x, y, z, ...

Witness terms :: a,b,c,d,e,f := x | λx.b | f⊳a | ∂x.e | c⋆a.

In the resulting equational system, one has the usual $\beta\eta$-conditions for (λ) and (⊳) [decoration-free spelling]:

$$(\beta\lambda) \vdash (\lambda x. b[x]) \,.\, a \;=\; b[x := a],$$

$$(\eta\lambda) \vdash \lambda x. (c. x) \;=\; c \; (x\; not\; free\; in\; c),$$

As well as the analogous $\beta\eta$-conditions for (∂) and (⋆):

$$(\beta\partial) \vdash c \star (\partial z. e[z]) \;=\; e[z := c],$$

$$(\eta\partial) \vdash \partial z. (z \star a) \;=\; a \; (z\; not\; free\; in\; a),$$

Together with the expected rules of monotony (compatibility of equality - here, *conversion* - with the operations).

This extension of pure λ can be easily seen to be consistent by interpreting it in [type-free] $\lambda\pi$-calculus, $\lambda\pi$, for instance.[1] Alternatively, one can choose to equip the resulting calculus with an appropriate *notion of reduction* and establish *confluence* [via a Church-Rosser theorem] first.

The *intended* decoration (typing) is given by the conditions (λ), (⊳), (∂) and ($\star$). In view of the above, if considered as a (decorated / 'typed') λ-theory, the outcome - the $\lambda\partial$-calculus λ[**f**,N,C] - is a witness *theory for classical logic*.

This yields the simplest Curry-Howard correspondence for (propositional) classical logic I know of.[2] (Cf. §**7.4**, below.)

§5. In his (1927, 1934), Jaśkowski chose to hide the applications of the 'inner cut' ($\star$) - which, as noted above, would have required the additional propositional atom **f** (*falsum*) - and expressed *reductio ad absurdum* in the form of a more complex rule, viz. by the Medieval ex *contradictione quodlibet* ['explosion'] principle, viewed as a rule of inference:

$$(\chi)[z{:}Np] \vdash c[z]{:}Nq, [z{:}Np] \vdash a[z]{:}q \Rightarrow \vdash \chi z{:}Np.(c,a) : p.$$

Upon adjoining the atom **f** and the 'hidden' rule ($\star$), the complex Jaśkowski rule (χ) can be obtained as:

$$(\chi) \vdash \chi z{:}Np.(c,a) := \partial z{:}Np.(c \star a) : p,$$

$$if\ [z{:}Np] \vdash c : Nq, and\ [z{:}Np] \vdash a : q,$$

1. This is possible since, unlike the pure λ-calculus λ, the extensional $\lambda\pi$-calculus $\lambda\pi$ contains infinitely many nontrivial copies of itself.

2. See also Rezuş (1990, 1991, 1993) and Sørensen & Urzycyn (2006).

While, conversely, one can have:

$$(\partial) \vdash \partial z{:}\, Np.\, e[z] := \chi z.\, (\boldsymbol{\Omega}, e[z]) : p, if\ [z{:}\, Np] \vdash e[z] : \boldsymbol{f},$$

in terms of (χ) and (**Ω**).

The 'hidden' rule (⋆), however, can be obtained explicitly from Jaśkowski's (χ) only by an ad hoc contextual artifice, setting, e.g.,

$$(\star) \vdash c \star a := \chi z{:}\, v.\, (c, a) : f, if \vdash c : Np, and \vdash a : p$$
[z fresh for c, a].[1]

As an aside, on ultimate formal grounds, I should have rather written down the Jaśkowski rule (χ), as:
$$[x{:}\, Np] \vdash c[x]{:}\, Nq;\ [y{:}\, Np] \vdash a[y]{:}\, q \Rightarrow \vdash \chi(x, y{:}\, Np).\, (c[x], a[y]) : p,$$

1. In retrospect, it is hard to say why Jaśkowski did prefer the complex (χ)-rule (a kind of 'mixed' abstractor, in witness-theoretic terms, like the rather complex case-construct [orelimination] in intuitionism), as a primitive rule of inference, in place of the 'elementary' *reductio ad absurdum* (∂) [here, a monadic abstractor, like (λ)] and the 'hidden' rule / operator expressing the 'law of [non-] contradiction' (⋆). Prima facie, I would suspect the choice was a matter of economy. Although there was an even more drastic economy in sight, that both Łukasiewicz and Jaśkowski were, apparently, well aware of, viz. by adopting the 'inferential' definition of negation, à la Peirce (1885), $\neg p := Cp\boldsymbol{f}$, in which case the primitive rule (χ) could have been replaced by an 'inferential' variant of *reductio ad absurdum* (γ,a monadic abstractor, with $\vdash \gamma z{:}\, \neg p.\, e[z] : p, for\ [z{:}\, \neg p] \vdash e[z] : \boldsymbol{f}$, in decorated / 'typed' version, etc.). Cf. Rezuş (1990, 1991, 1993). As a matter of fact, in the latter case, the witness-theoretic properties (as regards proof-conversion resp. proof-reduction [= detour elimination]) of the γ-operator are more involved that those presupposed by the 'natural' [(∂)-(⋆)]-pair, but Łukasiewicz and Jaśkowski did not think in such terms, anyway. Even Gentzen (1934-1935) was slightly confused as to the would-be proof-detours that could - and should - be associated to a genuine classical negation. It took us some thirty years, at least, until we were able to reach a clean conceptual insight on the matter. See, e.g., Prawitz (1965) for a solution, applying to the 'inferential' case and the combinator resp. λ-calculus variants, described in Rezuş (1990, 1991) [*λγ-calculi*]. Besides, it took us about other twenty years, in order to get something as simple as the λ∂-calculus sketched under §4 above (Rezuş, cca 1987), corresponding to what the pioneers - Frege, Peirce, Russell, Łukasiewicz, Leśniewski, Tarski etc. - might actually have had in mind.

But, as the two premises of (χ) are independent, I am probably allowed to use (a subtle form of meta-) α-conversion in this context.[1]

From this, the reader can easily reconstruct by herself the witness theory corresponding to Jaśkowski's natural deduction system for classical logic, i.e., a would be Jaśkowski $\lambda\chi\Omega$*-calculus* - **λ**[**f**,N,C], say - (equationally) equivalent to $\boldsymbol{\lambda\partial\Omega}$[**f**,N,C] above.

One might also note the fact that the original system of Jaśkowski (1927, 1934) - without **f** and (**Ω**), **λ**[N,C], say - was just a *notational device* (no proof-conversion, resp. proof-reduction rules). Moreover, it was constructed on a functionally incomplete propositional signature (as noted before, we cannot retrieve the constants **f**, **v**, definitionally, from N and C alone), whence the attempt to associate appropriate conversion-conditions to the Jaśkowski λ-primitive could only yield a *proper subsystem* of **λ**(**Ω**)[**f**,N,C].

§5.1. Worth mentioning is also the fact that Jaśkowki proposed a perspicuous *graphical representation* of his proof-primitives in the original paper of 1927 - a kind of block-structure, meant to isolate intuitively *sub-proofs* of a given proof (actually, sub-terms in the corresponding λ-calculus description)- , that was perfected by Frederic Brenton Fitch (1908-1987) *et alii*, later on.[2]

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1. Viewed abstractly, the witnessed entailments are, actually, a kind of meta-combinators, or closed *meta-terms*, in the end. Incidentally, with the terminology mentioned earlier, the Jaśkowski witness-operator χ should have gen-arity [1,1], not gen-arity [2] (sic), whence the alternative spelling above.

2. Cf. Fitch (1952) and Anderson & Belnap (1975, 1992), for applications to intensional logics. Notably, a similar representation was invented and used, later - independently -, by Hans Freudenthal (1905-1990), in didactic presentations of classical logic, as well as by Nicolaas G. de Bruijn (1918-2012), in his work on AUTOMATH [automated mathematics] and on the so-called 'Mathematical Vernacular' [WOT = *Wiskundige Omgangstaal, in Dutch*]. On this, see, mainly, de Bruijn's lectures on *Taal en structuur van de wiskunde* [The language and structure of mathematics], given at the Eindhoven Institute of Technology, Department of Mathematics and Computing Science, during the Spring Semester 1978, and summarised subsequently [in Dutch], in *Euclides* 44 (1979-1980), as well as Rezuş (1983, 1990, 1991), for further references.

Otherwise, the tedious and rather non-transparent formal description of the 'supposition rules' in Jaśkowski (1934) can be easily re-shaped, equivalently, in terms of assumption contexts and (witnessed) entailments as already suggested in the above. It is relatively easy to see that the usual 'structural' rules of Gentzen are implicit in Jaskowśki's description. Actually, Gentzen's L-system for classical logic is just a disguised form - namely, a special case - of 'natural deduction'.[1]

§5.2. Equally worth recording here is the (redundant) extension on the same primitive propositional signature [**f**,N,C], mentioned by the end of Rezuş (2009, rev. 2016), which consists of adding the double-negation (DN) rules:

$(\nabla) \vdash \nabla[p](a) : NNp, if \ \vdash \ a : p$ [double-negation introduction],

$(\Delta) \vdash \Delta[p](c) : p, if \ \vdash \ c : NNp$ [double-negation elimination].

In the latter case, the (DN) witness-operators [rules of inference] (∇) and (Δ) are supposed to obey inversion principles of the form

$$(\beta\Delta) \vdash \nabla(\Delta(c)) \ = \ c : NNp,$$

$$(\eta\Delta) \vdash \Delta(\nabla(a)) \ = \ a : p.$$

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1. A Gentzen L-sequent 'multiple on the right', $\alpha_1, \dots, \alpha_m \vdash \beta_1, \dots, \beta_n$ $(m, n \geq 0)$, is a specific entailment of the form $\alpha_1, \dots, \alpha_m,\ \bar{\beta}_1, \dots, \bar{\beta}_n \vdash \boldsymbol{f}$ - where $\bar{\beta}_i$ is a kind of 'surface negation' of β_i $(for\ 0 < i < n\ +\ 1)$, a rather confusing idea based on an ad hoc piece of ideography -, also known as rejection or refutation (*elenchos*, in the Greek of Aristotle and Chrysippus), about two millenia before both Jaśkowski and Gentzen were born. As a matter of fact, mutatis mutandis, Chrysippus' conceptual setting was cleaner. Cf. Rezuş (2007, rev. 2016) for details.

As earlier, the resulting extension (decorated / 'typed' λ-calculus, **λ∂**Δ, say) can be shown to be consistent by interpreting its 'type-free' variant in the (undecorated) $\lambda\pi$-calculus.[1]

It is easy to see that, in the formulation without primitive (DN)-rules, at least one of the $(\beta/\eta\Delta)$-conditions would normally fail, whence the idea of taking ∇ and Δ as primitive proof-operators (rules of inference).

§6. The extensions to quantifiers (either propositional or first- resp. second order) are straightforward.[2]

Illustrated next is the extension to propositional quantifiers on the (otherwise redundant) signature [**f**,N,C,Π], with Π standing for the universal quantifier, like in Łukasiewicz (1929) and Jaśkowski (1934). As above, α, β, ..., possibly with sub- and / or superscripts are used as metavariables ranging over formulas. If the propositional variable p occurs free (even fictitiously so) in a formula α, we write $\alpha[p]$ in order to make this visible. Substitutions are mentioned accordingly: $a[p:=\alpha]$, and $\beta[p \coloneqq \alpha]$ resp. (read 'p becomes α in a, resp. in β').

For the extended witness-syntax there are required two more proof-operators (rules of inference), corresponding to *Generalization* (Λ) and *Instantiation* (▸) resp. The new pair [(Λ),(▸)] is analogous to the [(λ),(▻)]-pair above.

We present here a version close to Jaśkowski (1934), leaving to the reader the task of showing equivalence with the corresponding

1. So, once more, consistency can be established already at undecorated / 'type-free' level. In fact, **λ∂**Δ is redundant: if the [(Δ), (∇)]-pair is present, we can leave out either the [(λ), (▻)]-pair or the [(∂), (⋆)]-pair, viz. **λ∂**Δ is, ultimately, equivalent [in a 'type-free' setting] with each of its 'halves', **λ**Δ, resp. **∂**Δ. See, *mutatis mutandis*, §**7.5.**

2. See, e.g., Rezuş (1990, 1991, 1993) for first-order quantifiers, and, mutatis mutandis, Rezuş (1986) for the 'extended propositional calculus' case (i.e., classical logic with propositional quantifiers), as well as for the second-order case.

formulation of Łukasiewicz (1929). As above, the construction admits of a decoration-free description.

The decoration-free ['type-free'] syntax of the resulting system (**λ∂Λ**) is:

witness-variables :: x, y, z, ...
witness terms :: a,b,c,d,e,f := x | λx.b | f⊳a | ∂x.e | c⋆a | Λp.a | f ▸α.
The additional conversion rules are (in 'type-free' spelling):

$$(\beta\Lambda) \vdash (\Lambda p.\, a[p]) \blacktriangleright \alpha = a[p := \alpha],$$

$$(\eta\Lambda) \vdash \Lambda p.\, (c \blacktriangleright p) = c, if\ p\ is\ not\ free\ in\ c^{1},$$

Together with the corresponding monotony conditions for (Λ) and (▸), meant to make equality (conversion) compatible with the operations.[2]

The decoration ['typing'] is, as expected, relative to an arbitrary assumption context (i.e., a finite list $\hat{\Gamma}$ of decorated witness variables, omitted below). We have (λ), (⊳), (∂), and (⋆), like before, as well as the new rules (for α, β arbitrary formulas):

$$(\Lambda) \vdash (\Lambda p.\, a[p]) : \Pi p.\, \alpha[p], if\ [p] \vdash a[p] : \alpha[p],$$

$$(\blacktriangleright) \vdash (\mathrm{f} \blacktriangleright \alpha) : \beta[p := \alpha], if\ \vdash f : \Pi p.\, \beta[p].$$

The first- (resp. second-) order case is completely analogous. In each case, the corresponding λ-calculi can be shown to be consistent by simple translation arguments.

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1. Exactly as in Girard's 'System F' (PhD Diss., Paris 7, 1971). Of course, the latter λ-calculus is the-[(∂),(⋆)]-free fragment of **λ∂Λ**, i.e., **λΛ**, by present notational standards. Cf. Rezuş (1986) for details on the Girard-Reynolds λ-calculus.

2. Since I have omitted, everywhere in the above, any reference to proof-contexts, the usual provisoes on p-variables are also tacitly assumed.

§7. The careful reader might have noticed a *general principle of construction* behind the *witness-theory* (proof-system) **λ∂Λ** above, viz. the fact that the primitive witness- / proof-operators come in pairs [(**abs**),(**cut**)], where (**abs**) is a (monadic) *abstraction operator* and (**cut**) is a *'cut'-operator*, i.e., an operation meant to 'eliminate' its associated abstractor (**abs**). Moreover, each such a pair is supposed to *characterize* the associated rules of inference as *operators*, by *equational stipulations* (here, $\beta\eta$-conditions), i.e., more or less, algebraically, by indicating their 'characteristic behaviour'. One could thus notice a *uniform introduction-elimination* pattern (of construction), provided one *thinks in terms of witnesses* (here: proofs), *not in terms of bare formulas* (expressing propositions / propositional schemes).

Technically, it is also possible to describe a proper *extension* of the (minimal) *witness-theory* (proof-system) **λ∂Λ**, based on an idea that goes back to the founder of classical logic, the Stoic philosopher Chrysippus of Sol[o]i, twenty-two centuries ago. The extension is a λ-theory, i.e., a consistent λ-calculus, as well (both 'type-free', and decorated / 'typed' as above). Of course, I will not credit the famous Phoenician with the details, but the reader should be certainly able to recognize the *Chrysippean spirit* behind the construction.[1]

Writing down things in 'Polish' - i.e., in Łukasiewicz notation, as everywhere here -, I will use the same propositional signature as before, viz. [**f**,N,C,Π][2], but choose a slightly different team of

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1. Cf. Rezuş (2007, rev 2016), for technical - and historical - evidence supporting the claim. The extension works for the system with (DN)-primitives, too.

2. We may want to abbreviate, for convenience, $\bar{C} := NC$ and $\bar{\Pi} \coloneqq N\Pi$ (so that $\bar{C}$ is marked as the 'polar [opposite]' of C, and $\bar{\Pi}$ as the 'polar [opposite]' of Π, resp.), but the Łukasiewicz notation makes this superfluous. One might also note the fact that, by the standards of Rezuş (2007, rev. 2016), $\bar{C}$ and $\bar{\Pi}$ would have counted as Chrysippean connectives. Specifically, $\bar{C}$ corresponds to the *Chrysippean connector* (binary connective) more, i.e., māllon... ē..., a kind of *rather... than...*, in English, while the quantifier-free part of the extended calculus [with (DN)-primitives] - to →

primitive witness-operators, while leaving ∂, $\star$ and λ unchanged, add two kinds of 'pairs', namely π(...,...) and ↓(...,...), as well as a (mixed) dyadic abstraction-operator Σ, with term forming rules ≺a,f≻, $\downarrow_{\alpha}$(a) [writing, conveniently, ≺a,f≻ ≡ π(a,f) and $\downarrow_{\alpha}$(a) ≡ ↓(α,a)], and Σ(p,x).c[p,x], resp., for proof- / witness-terms a, c[p,x], f and formulas α.

Whence the expected formal grammar [at a decoration-free / 'type-free' level], with p, q, r, ..., as (meta-variables for) propositional variables, as ever:

formulas :: α, β := p | f | Nα | C$\alpha\beta$ | Πp.α

w-variables :: x, y, z, ...

w-terms :: a,b,c,d,e,f := x | ∂x.e | c⋆a | λx.b | ≺a,f≻ | Σ(p,x).a | $\downarrow_{\alpha}$(a).
The (decoration-free / 'type-free') equational theory - called $\boldsymbol{\partial\lambda^{*}\Sigma}$, for convenience - consists of

$$(\beta\partial) \vdash c \star (\partial z. e[z]) = e[z := c],$$

$$(\eta\partial) \vdash \partial z. (z \star a) = a \ (z \ not \ free \ in \ a),$$

As before, in **λ(*Λ*)**, and the following 'polar' conditions:

$$(\beta\lambda^{*}) \vdash \prec a, f \succ \star (\lambda x. b[x]) = f \star (b[x := a]),$$

$$(\beta\lambda^{*}) \vdash \lambda x. \partial y. (\prec x, y \succ \star c) = c \ (x, y \ not \ free \ in \ c),$$

as well as

$$(\beta\Sigma) \vdash \downarrow_{\alpha} (a) \star (\Sigma(p, x). c[p, x]) = c[p := \alpha, x := a]),$$

$$(\eta\Sigma) \vdash \Sigma(p, x). (\downarrow_{p} (x) \star c) = c \ (p, x \ not \ free \ in \ c),$$

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be ← described next – corresponds exactly to the *semantic (C-$\bar{C}$)-fibration* of `Chrysippean logic' **Ch**.

Together with the expected monotony constraints on the primitive witness operators.
The intended decoration ('typing') is given by

$$(\partial) \vdash \partial x{:}N\alpha.e[x] : \alpha, if\ [x{:}N\alpha] \vdash e[x] : \boldsymbol{f},$$

$$(\star) \vdash c \star a : \boldsymbol{f}, if\ \vdash c : N\alpha\ and \vdash a : \alpha,$$

$$(\lambda) \vdash \lambda x{:}\alpha.b[x] : C\alpha\beta, if\ [x{:}\alpha] \vdash b[x] : \beta,$$

Like in the case of **λ(*Λ*)**, with moreover,

$$(\pi) \vdash \prec a, f \succ : NC\alpha\beta, if \vdash a : \alpha, and \vdash \boldsymbol{f} : N\beta, and$$

$$(\Sigma) \vdash \Sigma(p, x{:}N\alpha).c[p, x] : \Pi p.\alpha, if\ [p]\ [x{:}N\alpha] \vdash c[p, x] : \boldsymbol{f},$$

$$(\downarrow) \vdash \downarrow_\alpha (c) : N\Pi p.\alpha, if \vdash c : N\alpha$$

- With the expected restriction on (Σ) -, so that the classical 'polarities' become obvious.[1] Note that there is no primitive modus ponens (⊳), in $\boldsymbol{\partial\lambda^*\Sigma}$. (See, however, §**7.2**.)

A few more (technical) comments are in order.

§7.1. Setting, in $\boldsymbol{\partial\lambda^*\Sigma}$,

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1. Formally, ∂ looks, in the end, like a kind of degenerated Σ (sic). The informed reader has already realised the fact that the (Σ-↓)-rules are just (undecorated / 'typefree') analogues of the usual *intuitionistic* ∃-rules. Cf. Rezuş (1986, 1991), etc. On the historical side, if I am not very mistaken, I remember having encountered something similar to the 'polar' pair [(λ)-(π)] in work of Dag Prawitz, going back to the late nineteen-sixties and the early seventies (although with no reference to the Stoic lore and / or to would-be [classical] proof-isomorphisms, i.e., to proper proof-conversion rules). Whence, ultimately, the basic idea behind the construction of ∂λ*(Σ) should be, very likely, accounted for as a piece of (historical) data-retrieval, rather than as a genuine finding, due to the present author. In retrospect, virtually any *mindful reader* of Prawitz, already familiar with the basics of λ-calculus, could have come out with a *similar proof-formalism*, even ignoring the Chrysippean antecedents.

$$(df\ \Lambda)\ \Lambda p.a[p] := \Sigma(p,z).(z \star a[p]), z\ not\ free\ in\ a[p],$$

We get conditions analogous to $(\beta\lambda^*)$ and $(\eta\lambda^*)$, viz.

$$(\beta\Lambda^*)\ \vdash \downarrow_\alpha (f) \star (\Lambda u.a[x]) = f \star (a[p := \alpha]),$$

$$(\eta\Lambda^*)\ \vdash \Lambda p.\partial z.(\downarrow_p (z) \star c) = c\ (p, z\ not\ free\ in\ c),$$

As well as monotony for the defined operator.

Note that the defined operator *inherits* the *intended* decoration ('typing') from the primitive decoration of the *definientia*.

Conversely, let us replace the primitive dyadic abstractor Σ, of $\boldsymbol{\partial\lambda^*\Sigma}$, with a primitive monadic abstraction operator Λ, subjected to the conditions $(\beta\Lambda^*)$ and $(\eta\Lambda^*)$ above, including monotony for Λ, and call the resulting system $\boldsymbol{\partial\lambda^*\Lambda^*}$**.** Defining, in the latter system,

$$(df\ \Sigma)\ \Sigma(p,z).e[p,z] := \Lambda p.\partial z.e[u,z],$$

One has, by easy calculations, $(\beta\Sigma)$ and $(\eta\Sigma)$, so that, ultimately, $\boldsymbol{\partial\lambda^*\Sigma}$ and $\boldsymbol{\partial\lambda^*\Lambda^*}$ turn out to be equationally equivalent.

§7.2. It is easy to establish the fact that $\boldsymbol{\lambda\partial\Lambda}$ is a subsystem of $\boldsymbol{\partial\lambda^*\Sigma}$.[1] Indeed, define, in $\boldsymbol{\partial\lambda^*\Sigma}$,

$$(df\ \rhd)\ c \rhd a := \partial y.(\prec a, y \succ \star c), y\ not\ free\ in\ a\ and\ c.$$

This yields $(\beta\lambda)$ and $(\eta\lambda)$, and, of course, monotony for the defined $(\rhd)$-operator.

Set now, as before,

$$(df\ \Lambda)\ \Lambda p.a[p] := \Sigma(p,z).(z \star a[p]), if\ z\ is\ not\ free\ in\ a[p], and$$

$$(\mathrm{df} \blacktriangleright)\ \mathrm{c}\blacktriangleright\alpha := \partial z.(\downarrow_\alpha (z) \star c), if\ z\ is\ not\ free\ in\ c.$$

1. To show that $\boldsymbol{\lambda\partial\Lambda}$ is a proper subsystem of $\boldsymbol{\partial\lambda^*\Sigma}$ requires a more involved argument. I'd rather defer the details (too far from the subject of the present notes, anyway).

This yields ($\beta\Lambda$) and ($\eta\Lambda$), as well as the expected monotony conditions for the defined [(Λ)-(▶)]-pair of operators.
The fact that the defined operators *inherit* the *intended* decoration ('typing') from the primitive decoration of the *definientia* is obvious.

§7.3. Incidentally, the (Σ-↓)-free fragment of ['type-free'] $\boldsymbol{\partial\lambda^*\Sigma}$ - call it $\boldsymbol{\partial\lambda^*}$, for convenience - admits of an alternative, more general formulation [at a decoration-free level].

Indeed, setting

$(df\int)\ \int(x,y).\,c[x,y] := \lambda x.\,\partial y.\,c[e,y]\ (for\ the\ split\ operator),$
We get, in $\boldsymbol{\partial\lambda^*}$,

$$(\beta\int) \vdash \prec a,b \succ \star (\int(x,y).\,c[x,y]) = c[x:=a, y:=b],$$

$$(\eta\int) \vdash \int(x,y).(\prec x,y \succ \star\ c) = c\ (x,y\ not\ free\ in\ c),$$

Together with the expected monotony condition for ∫, and it is obvious that we can trade ∫ for λ in this context (at a decoration- / 'type-free' level), i.e., that one could have had, in the background, a calculus $\partial\int$, say, instead of $\boldsymbol{\partial\lambda^*}$, in the above.

To see this, define, as above,

$(df\ \int)\ \lambda x.\,b[x] := (x,y).\,(y \star b[x]), y\ not\ free\ in\ b[x], as\ well\ as$
$(df\ \rhd)\ c \rhd a := \partial y.(\prec a,y \succ\star c), y\ not\ free\ in\ a\ and\ c,$

In $\partial\int$. This yields the expected conditions ($\beta\lambda^*$) and ($\eta\lambda^*$), as well as ($\beta\lambda$) and ($\eta\lambda$), so that $\partial\int$ and $\boldsymbol{\partial\lambda^*}$ are equationally equivalent, too.

As a bonus, for $\partial\int$ (Post-) consistency is straightforward. The latter is a (proper) subsystem of $\lambda\pi$: define ∫, in $\lambda\pi$-calculus, by

$$\int[x,y].\,c[x,y] := \partial z.\,c[x:=1(z), y:=2(z)],$$

Where j(c), j := 1, 2, are the usual $\lambda\pi$-projections, and $\partial \equiv \lambda$, for convenience.[1]

Note that the latter definitional pattern of the (λ,$\rhd$)-pair, in $\partial\int$ - actually, in $\lambda\pi$ - can be iterated, in the obvious way, in order to yield an infinite sequence of distinct (λ,$\rhd$)-pairs (for an alternative, see §**7.5**, below).

For $\partial\int$, the intended *art deco* would have been different, however. Actually, equational equivalence holds only in a decoration-free setting. In $\partial\int$, one should change the primitive (propositional) signature, by replacing the primitive C [implication] with D [the Sheffer-functor' *incompatibility*, or *nand*, i.e. semantically, negated classical conjunction], whereupon N [classical negation] becomes redundant, by setting Np := Dpp. The resulting ['typed'] calculus $\partial\int$[**f**,D], say - based on the witness primitives ∂, $\star$, $\int$ and the pair-construct π, as well as on the associated $\beta\eta$-conversion conditions ($\beta\partial$), ($\eta\partial$), and ($\beta\int$), ($\eta\int$), resp. - is, actually, an extension of $\boldsymbol{\partial\lambda}$***** [**f**,N,C], with Cpq := DpNq, in $\partial\int$[**f**,D].[2]

§7.4. [*A few more extensions*.] Where T_1, T_2 are equational theories, let us write, for convenience, $T_1 \preceq T_2$, resp. $T_1 \simeq T_2$, for the fact that T_1 is a subsystem of T_2, resp. that T_1 and T_2 are equationally equivalent.

1. The $\lambda\pi$-calculus is known to be consistent by a well-known lattice-theoretical (actually topological) construction due to Dana Scott (1969), as well as by constructive ('syntactical') means, as shown recently by Kristian Støvring (November 2005, rev. 2006). Notably, Støvring's method of proof applies to $\boldsymbol{\lambda\pi\Lambda}$, as well, i.e., to the decoration-free ('type-free') extension of $\boldsymbol{\lambda\pi}$ with a [(Λ),(▸)]-pair satisfying, *mutatis mutandis*, ($\beta\lambda$) and ($\eta\lambda$).

2. In his Warsaw lectures, Łukasiewicz alluded actually to the alternative - cf., e.g., Łukasiewicz (1929), Chapter II §**17** -, but he was, apparently, distracted by provability details on Henry M. Sheffer (1913) and Jean Nicod (1917), so that the idea was diluted, later on. It is only in (very) recent times that the Peirce-Sheffer *nand* and *nor* connectives deserved a proper treatment in 'natural deduction' terms.

From the above, we know already that

$$\lambda \preceq \lambda\partial \preceq \partial\lambda^* \simeq \partial\int \preceq \lambda\pi.$$

The reader can establish easily the fact that, in $\boldsymbol{\partial\lambda^*}$, one can replace ($\beta\lambda^*$) by the condition:

$$(\beta p) \vdash \prec a, c \succ \star f \;=\; c \star (f \rhd a).$$

Let now [in the syntax of $\boldsymbol{\partial\lambda^*}$], $\boldsymbol{\lambda\partial p_\beta} \coloneqq \boldsymbol{\lambda\partial} + (\beta \boldsymbol{p})$, and consider the (equational) extensions $\boldsymbol{\lambda\partial p} \coloneqq \boldsymbol{\lambda\partial_\beta} + (\eta\boldsymbol{p})$, $\boldsymbol{\partial\lambda^* p} \coloneqq \boldsymbol{\partial\lambda^*} + (\eta\boldsymbol{p})$ [in the syntax of $\boldsymbol{\partial\lambda^*}$], and $\boldsymbol{\partial\int p} \coloneqq \boldsymbol{\partial\int} + (\partial\int)$ [in the syntax of $\boldsymbol{\partial\int}$], with the additional conditions:

$$(\eta p) \vdash \partial z.e[z] \;=\; \lambda x.\partial y.e[z{:=}\prec x, y \succ],$$

$$(\partial\textstyle\int) \vdash z.e[z] \;= \int (x,y).e[z{:=}\prec x, y \succ],$$

resp. So, in particular, $\boldsymbol{\lambda\partial p} \coloneqq \boldsymbol{\lambda\partial} + (\beta\boldsymbol{p}) + (\eta\boldsymbol{p})$.[1] From the above, one can establish the fact that

$$\boldsymbol{\lambda} \preceq \boldsymbol{\lambda\partial} \preceq \boldsymbol{\lambda\partial p_\beta} \preceq \boldsymbol{\lambda\partial p} \simeq \boldsymbol{\partial\lambda^* p} \simeq \boldsymbol{\partial\int p} \preceq \boldsymbol{\lambda\pi}.$$ [2]

The reader may also want to contemplate, in particular, the *separation properties* of the $\boldsymbol{\lambda\partial}$**p**-axiomatics, which consists of [1] pure β-η-λ-conditions, (βλ), (ηλ), [2] pure β-η-∂ conditions, ($\beta\partial$), ($\eta\partial$), as well as (mixed) [3] β-η-π-conditions, (βp), (ηp), on the primitive 'pairs' (π).

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1. One can also spot some obvious redundancy in $\boldsymbol{\partial\int}$**p**. See §**7.7**.

2. For an explicit way of inserting the ∂-segment - i.e., the [(∂)-($\star$)]-pair - in $\boldsymbol{\lambda\pi}$, at decoration-free ['type-free'] level, see §**7.5**. All inclusions are *strict*. As for $\boldsymbol{\lambda\pi}$, 'projections' - relative to a primitive π, where available - are definable in **p**-subsystems, but the resulting pairing is not 'surjective', i.e. ($\eta\pi$) ['surjectivity of pairing'] fails.

Summing up, the *intended* decoration ('typing') for **λ∂p**[**f**,N,C], based on the bare 'propositional' (quantifier-free) syntax:
Formulas :: α, β := p | **f** | Nα | C$\alpha\beta$

w-variables :: x, y, z, ...

w-terms :: a,b,c,d,e,f := x | λx:α.b | f⊳a | ∂x:Nα.e | c⋆a | ≺a,f≻,

Is given by:

$(\lambda) \vdash \lambda x{:}\,\alpha.\, b[x] : C\alpha\beta, if\ [x{:}\,\alpha] \vdash b[x] : \beta,$
$(\rhd) \vdash f \rhd a : \beta, if \vdash f : C\alpha\beta\ and \vdash a : \alpha,$

$(\partial) \vdash \partial x{:}\, N\alpha.\, e[x] : \alpha, if\ [x{:}\, N\alpha] \vdash e[x] : \boldsymbol{f},$

$(\star) \vdash c \star a : \boldsymbol{f}, if \vdash c : N\alpha\ and \vdash a : \alpha,$

$(\pi) \vdash \prec a, f \succ : NC\alpha\beta, if \vdash a : \alpha, and \vdash f : N\beta,$

i.e., in this version, the primitive rules of classical ['propositional'] logic, based on the primitive [**f**,N,C]-signature, are the 'Deduction Theorem', *modus ponens*, *reductio ad absurdum*, the 'law of (non-) contradiction', and the rule (π), a rule of 'NC-introduction', so to speak.

§7.5. On a different route, one can define explicitly, in **λ*π***, a (∂,⋆)-pair satisfying ($\beta\partial$) and ($\eta\partial$), as well.

With a usual primitive pairing (i.e., pairs ≺a,b≻ and projections **j**(c), j:=1,2), as ever, let [(λ),(⊳)] be an arbitrary (**abs**,**cut**)-pair satisfying (βλ) and (ηλ), and define successively:

$(df\ \Delta)\ \Delta(c) := \lambda z.(c\, .\, \mathbf{1}(z)\, .\, \mathbf{2}(z)),$

$(df\ \nabla)\ \nabla(f) := \lambda x. \lambda y. (f \rhd \prec x, y \succ),$

$(df\ \partial_0)\ \partial z.\, e[z] \equiv \partial_0 z.\, e[z] := \Delta(\lambda z.\, e[z]),$

$(df\ \star_0)\ c \star a \equiv c \star_0 a := \nabla(a) \vartriangleright c,$
$(df\ \lambda_0)\ \rho x.e]x] \equiv \lambda_0 x.e[x] := \partial x.e[x := \Delta(x)],$

$(df\ \vartriangleright_0)\ c \vartriangleright_0 a := \nabla(a) \star c,$

Iterating, next, for any natural number n,

$(df\ \lambda_{n+1}) \lambda_{n+1} x.e[x] := \lambda_n x.e[x := \Delta(x)] \equiv \Delta(\lambda_n x.e[x := \Delta(x)]), with$

$(df\ \partial_n)\ \partial_n z.e[z] := \Delta(\lambda_n z.e[z]), and$

$(df\ \vartriangleright_{n+1})\ c \vartriangleright_{n+1} a := \nabla(a) \star_n c \equiv \nabla(c) \vartriangleright_n \nabla(a), with$

$(df\ \star_n)\ c \star_n a := \nabla(a) \vartriangleright_n c.$

From this, we get the 'inversion':

$(\beta\Delta) \vdash \nabla(\Delta(c)) = c,$

$(\eta\Delta) \vdash \Delta(\nabla(a)) = a,$

And, for any natural number n, (β-η)-conditions:

$(\beta\lambda_n) \vdash (\lambda_n x.b[x]) \vartriangleright_n a = b[x := a],$

$(\eta\lambda_n) \vdash \lambda_n x.(c \vartriangleright_n x) = c\ (x\ not\ free\ in\ c),$

$(\beta\partial_n) \vdash c \star_n (\partial_n z.e[z]) = e[z := c],$

$(\eta\partial_n) \vdash \partial_n z.(z \star_n a) = a\ (z\ not\ free\ in\ a),$

As well as the expected monotony-rules for the operations so defined.

In other words, unlike pure λ-calculus, **λ**, the λπ-calculus, $\boldsymbol{\lambda\pi}$, contains infinitely many distinct (non-trivial) copies of itself, so to speak. In particular, the fragments **λΔ**, ***ρ*Δ**, and **λ*ρ***, defined in the obvious way, by the corresponding (β-η)-conditions, are easily seen to be equationally equivalent.

The availability of the 'inversion' [($\beta\Delta$),($\eta\Delta$)] insures consistency for calculi containing, as a primitive, a [(Δ),(∇)]-pair satisfying ($\beta\Delta$) and ($\eta\Delta$).

§7.6. In the end, $\boldsymbol{\partial\lambda^{*}\Sigma}$ - as well as $\boldsymbol{\lambda\partial \mathbf{p}\Sigma}$ (i.e., the extension of $\boldsymbol{\lambda\partial\mathbf{p}}$ with (Σ-↓)-primitives) - is (Post) *consistent*, and so are the corresponding (DN)-extensions. For the (Σ-↓)-free part of the proof, the result is already contained in the above. The genuine (Σ-↓)-part consists of a trivial translation argument, collapsing the full system on its (Σ-↓)-free fragment.

§7.7. One might also notice that the analogous calculi $\boldsymbol{\partial\int(\mathbf{p})\Sigma}$[**f**,D,Π], based on {($\partial$), (⋆), (∫), ($\pi$), ($\Sigma$), (↓)}, are 'polar' (Chrysippean) constructions, as well. The same remark applies to the corresponding (DN)-extensions. The basic equational conditions are ($\beta\partial$), ($\eta\partial$), ($\beta\int$), ($\eta\int$), while the **p**-extension has also ($\partial\int$), whereupon either one of ($\eta\partial$) or ($\eta\int$) are redundant.

§7.8. As a final remark, all *consistency proofs* mentioned in this paper amount to an easy - even though oft slightly involved - exercise of (explicit) definability in the (*type-free*) λπ-calculus (**λ*π***). Algebraically speaking, we are *dealing* with (a rather specific class of) *monoids*. Since (the intuitionistically decorated) λπ is also known as 'the internal language of CCCs' [cartesian closed categories] among category theorists, most of the facts relevant here should also amount to *category theoretic folklore*.[1]

1. The specific subject - falling under the label *cartesian closed monoids* [CCMs, for short] - has been invented by Dana Scott and Joachim Lambek sometime during the 1970's and has been vastly explored since, mainly in research on *categorical models of λ-calculus*. On the equational theory of CCM's, also known as C-*monoids* – equivalent with the ['type-free'] λπ-calculus -, cf. Koymans (1982, 1984), Lambek & Scott (1986).

§8. *Coda*. I hope my discussion above has made more or less clear what Jan Łukasiewicz - and his (very) young student Stanisław Jaśkowski, as well as his (equally) young colleague Alfred Tajtelbaum [Tarski][1] - did *actually know* and / or *could have known*, as regards 'natural deduction', during the mid- and late twenties. Why they did not invent something like [decorated / 'typed'] λ-calculus, in order to make things *conceptually clean*, evades me completely. It's up to my better informed - and more gifted - readers to speculate upon.

References

[1] Alan Ross Anderson, Nuel Belnap *Jr et alii* (1975, 1992) Entailment**.** *The Logic of Relevance and Necessity* **1, 2,** Princeton University Press, Princeton NJ 1:1975 (second printing, with corrections 1:19902), **2**:1992.

[2] Fabio Bellissima, and Paolo Pagli (1996) Consequentia mirabilis. *Una regola tra la scienza eflilosofia*, Leo S. Olschki, Florence 1996 [*Biblioteca distoria della scienza 38*].

[3] Ludwik Borkowski, and Jerzy Słupecki (1963) Elementy logiki matematycznej i teorii mnogości [Elements of Mathematical Logic and Set Theory], PWN, Warszawa 1963, 19662, 19693. English translation: Elements of Mathematical Logic and Set Theory, Pergamon Press, Oxford 1967 [*International Series of Monographs in Pure and Applied Mathematics 96*].

[4] Gerolamo Cardano (1570) *Opus novum* De proportionibus [*Ex officina Henricpetrina, Basileae*], Henri Petri [Henricus Petrus], Basle 1570. (Available online at the *Archimedes Project* [*http://archimedes.mpiwgberlin. mpg.de*]. Reprinted in Gerolamo Cardano Opera omnia [*Hyeronymi Cardani Mediolanensis*], edited by Karl Spon, 10 vols. in f, Lugduni [i.e., Lyons, France], 1663, vol. 4. The full collection is also available as a reprographic reprint: Fromann Verlag, Stuttgart/Bad Cannstatt and Johnson Reprint Corporation New York/London 1966-1967.)

[5] Alonzo Church (1935-1936) Mathematical Logic, Lecture Notes [by F. A. Ficken, H. G. Landau, H. Ruja, R. R. Singleton, N. E. Steenrod, J. H.

1. See Rezuş (1982, 1990) for relevant complements of information.

Sweer, and F. J. Weyl], Princeton University, October 1935 - January 1936 [typescript], Princeton NJ 1936, 113 pp.

[6] Christophorus Clavius [= Christoph Klau] SJ (1611) [*Christophori Clavii Bambergensis e Societate Jesu*] Opera mathematica [*V tomis distribute ab auctore nunc denuo correcta et plurimis locis aucta*...], 5 vols., in f, edited by Reinhard Eltz and published by Anton Hierat, Moguntiae [i.e., Mainz] 1611-[1612]. (Micro_lm-edition, issued by the Pius XII Memorial Library, St. Louis University. Vol. 1: COMMENTARIA *In Euclidis Elementa geometrica* [et] In Sphaerica Theodosii…, in two parts: 1.1, pp. 1-638, and 1.2, pp. 1-248, with separate page-numbering.)

[7] Haskell B. Curry (1948-1949) *A simplification of the theory of combinators,* Synthese 7, 1948-1949, pp. 391-399.

[8] Haskell B. Curry, and Robert Feys (1959) Combinatory Logic 1, North Holland, Amsterdam 1958, 1968R, 1974R.

[9] Lech Dubikajtis (1967) Stanisław Jaśkowski, Wiadomości Matematyczne 10, 1967, pp. 15-28.

[10] Lech Dubikajtis (1967a) Stanisław Jaśkowski (1906-1965), Ruch Filozoficzny 25 (3-4), 1967, pp. 187-198.

[11] Lech Dubikajtis (1975) The life and works of Stanisław Jaśkowski, Studia Logica 34, (2), 1975, pp. 109-116.

[12] Frederic Brenton Fitch (1952) Symbolic Logic, *An Introduction*, Ronald Press, New York NY 1952.

[13] Gottlob Frege (1879) Begriffsschrift, *Eine der arithmetischen nachgebildete Formelsprache des reinen Denkens*, Verlag Louis Nebert, Halle a/S 1879 [BG]. (English translation: Conceptual Notation and Related Articles (Translated and edited with a Biography and Introduction by Terrell Ward Bynum), Clarendon Press, Oxford 1972, pp. 101-203.)

[14] Gottlob Frege (1893, 1904) Grundgesetze der Arithmetik, *begri_sschriftlich abgeleitet*, I. Band, Jena, Verlag von Hermann Pohle 1893; II. Band, 1903 [GGA:1], [GGA:2]. (German original reprinted by Olms Verlag, Hildeshiem 1962. For a complete English translation, see

Basic Laws of Arithmetic. *Derived using concept-script*, Volumes I & II (in one), Translated and edited by Philip A. Ebert & Marcus Rossberg, with CrispinWright, Oxford University Press, Oxford 2013.)

[15] Gerhard Gentzen (1934-1935*) Untersuchungen über das logische Schliessen*, Mathematische Zeitschrift 39, 1934-1935, pp. 176-210, and 405-431.

[16] Allen P. Hazen, and Francis Jeffry Pelletier (2012) *Natural deduction*, in: Dov M. Gabbay, Francis Pelletier, and John Woods (eds.) Handbook of the History of Logic 11 (*A History of Its Central Concepts*), Elsevier / North Holland, Amsterdam etc. 2012, pp. 341-414.

[17] Allen P. Hazen, and Francis Jeffry Pelletier (2014) Gentzen and Jaśkowski natural deduction: *fundamentally similar but importantly different*, Studia Logica 102 (6), 2014, pp. 1103-1142.

[18] J. Roger Hindley (1969) *The principal type scheme of an object in combinatory logic*, Transactions of the American Mathematical Society 146, 1969, pp. 29-60.

[19] J. Roger Hindley (1997) Basic Simple Type Theory, Cambridge University Press, Cambridge UK 1997 [*Cambridge Tracts in Theoretical Computer Science 42*].

[20] J. Roger Hindley, and David Meredith (1990) *Principal type-schemes and condensed detachment*, The Journal of Symbolic Logic **55** (1), 1990, pp. 90-105.

[21] J. Roger Hindley, and Jonathan P. Seldin (1986) Introduction to Combinators and λ-Calculus, Cambridge University Press, Cambridge UK 1986 [*London Mathematical Society Student Texts 1*].

[22] J. Roger Hindley, and Jonathan P. Seldin (2008) Lambda-Calculus and Combinators. *An Introduction, Cambridge University Press*, Cambridge UK 2008 (Revised from the first edition Hindley & Seldin (1986).)

[23] Henry Hiż 1957 *Complete sentential calculus admitting extensions* (abstract), Summaries of Talks Presented at the Summer Institute for Symbolic Logic, Cornell University, Ithaca NY, 1957, pp. 260-262.

[24] Henry Hiż 1958 *Extendible sentential calculus* (abstract), Notices of the American Mathematical Society 5, 1958, p. 34.

[25] Henry Hiż 1959 *Extendible sentential calculus*, The Journal of Symbolic Logic **24** (3), 1959, pp. 193-202.

[26] Andrzej Indrzejczak (1998) *Jaśkowski and Gentzen approaches to natural deduction and related systems*, in: Katarzyna Kijania-Placek & Jan Woleński (eds.) The Lvov-Warsaw School and Contemporary Philosophy**,** Kluwer Academic Publishers, Dordrecht 1998, pp. 253-264.

[27] Andrzej Indrzejczak (2016) *Natural deduction*, in: Jean-Yves Béziau (ed.) Internet Encyclopedia of Philosophy. Online @ *www.iep.utm.edu/natded/.*

[28] Jacek Juliusz Jadacki (2006) *The Lvov-Warsaw school and its inuence on Polish philosophy of the second half of 20th century*, in: Jacek [Juliusz] Jadacki, and Jacek Pa_sniczek (eds.) The Lvov-Warsaw School. *The New Generation*, Rodopi, Amsterdam & New York NY 2006 [*Poznań Studies in the Philosophy of the Sciences and the Humanities 89*], pp. 41-83.
[29] Stanisław Jaśkowski (1927) *Teorja dedukcji oparta na dyrektywach za lozeniowych* [Deduction theory based on supposition rules], Paper presented at the First Congress of the Polish Mathematicians, Lvov, September 7-10, 1927. Cf. the Congress Proceedings: [PPZM] Proceedings (1929). Also referred to explicitly, by title, by Alfred Lindenbaum - another student of Jan Łukasiewicz - in his communication, Lindenbaum (1927), to the same Congress, as well as in Jaśkowski (1934).

[30] Stanisław Jaśkowski (1934) *On the rules of suppositions in formal logic*, in: Studia Logica. Wydawnictwo Poświ_econe Logice i jej Historii [Studia Logica. Publications on Logic and Its History], edited by Jan Łukasiewicz, vol. 1, 1934, pp. 5-32. [Published by the Philosophical Seminar of the Faculty of Mathematics and Natural Sciences, Warsaw University, Warsaw 1934.] (Reprinted in: Storrs McCall (ed.) Polish Logic 1920-1939, Clarendon Press, Oxford 1967, pp. 232-258.)

[31] John A. Kalman (1983) *Condensed detachment as a rule of inference*, Studia Logica 42 (4), 1983, pp. 443-451.

[32] Jerzy Kotas, and August Pieczkowski (1967) *Scientific works of Stanisław Jaśkowski*, Studia Logica 21 (1), 1967, pp. 7-15.

[33] Karst [Christian Peter Jozef] Koymans (1982) *Models of the lambda calculus*, Information and Control [currently Information and Computation] 52 (3), 1982, pp. 306-332.

[34] Karst Koymans (1984) Models of the Lambda Calculus, CWI, Amsterdam 1984 [*CWI Tract 9*]. (Originally PhD Dissertation, University of Utrecht 1984. See also The Journal of Symbolic Logic **52** (1), 1987, pp. 284-285 [review by Giuseppe Longo].)

[35] Joachim Lambek, and Phil J. Scott (1986) Introduction to Higher Order Categorical Logic, Cambridge University Press, Cambridge UK, etc. 1986 [*Cambridge Studies in Advanced Mathematics 7*].

[36] Stanisław Leśniewski (1992) CollectedWorks (edited by S. J. Surma, J. T. Srzednicki, D. I. Barnett, and V. F. Rickey) [in two volumes, with continuous page numbering], Kluwer, Dordrecht & Państwowe Wydawnictwo Naukowe [PWN], Warsaw 1992.

[37] Alfred Lindenbaum (1927) *Méthodes mathématiques dans les recherches sur le système de la théorie de déduction* (abstract), Paper presented at the First Congress of the Polish Mathematicians, Lwów, September 7-10, 1927. Cf. the Congress [PPZM] Proceedings (1929).

[38] Paul Lorenzen (1955) Einführung in die operative Logik und Mathematik, Springer Verlag, Berlin 1955, 19692 [*Grundlehren der mathematischen Wissenschaften* 78].

[39] Jan Łukasiewicz (1929) Elementy logiki matematycznej [Elements of mathematical logic], Skrypt autoryzowany opracowa M. Presburger. Z częśsciowej subwencji Senatu Akademickiego Uniw. Warsz. Nakadem Komisji Wydawniczej Ko la Matematyczno-Fizycznego S luchaczów Uniwersytetu Warszawskiego, Warszawa 1929. (Wydawnictwo Ko la tom XVIII). [Publications Series of the Association of Students of Mathematics and Physics of the Warsaw University. Volume XVIII. Authorized typo-script (of lecture notes) prepared by Mojzesz Presburger. Subsidized in part by the Senate of the Warsaw University, Financed by the Publishing Committee of the Association of Students of Mathematics and Physics of the

Warsaw University, Warsaw 1929.] (Second edition: Państwowe Wydawnictwo Naukowe [PWN: Polish Scientific Publishers], Warsaw 1958. English translation [of the second edition] by Olgierd Adrian Wojtasiewicz, as Łukasiewicz (1963).)

[40] Jan Łukasiewicz (1930*) Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls,* Sprawozdania z posiedzeń Towarzystwa Naukowego Warszawskiego (Wydział III), XXIII, 1930 [Comptes Rendus des séances de la Société des Sciences et des Lettres de Varsovie (Classe III), vol. XXIII, 1930], pp. 51-77. (Translated by H.Weber, [*as Philosophical remarks on many-valued systems of propositional logic], in: McCall* (1967), pp. 40-65.)
[41] Jan Łukasiewicz (1931*) Dowód zupeności dwuwartościowego rachunku zdań* [Ein Vollständigkeitsbeweis des zweiwertigen Aussagenkalküls], Sprawozdania z posiedze_n Towarzystwa Naukowego Warszawskiego (Wydzia l III), XXIV, 1931 [Comptes Rendus des s_eances de la Sociètè des Sciences et des Lettres de Varsovie (Classe III), vol. XXIV, 1931], Warszawa 1932.

[42] Jan Łukasiewicz (1934) *Z historii logiki zdań* [From the history of the logic of propositions], Przegląd Filozoficzny **37**, 1934, pp. 417-437. (Reprinted in Łukasiewicz (1961). German translation by the author [*Zur Geschichte der Aussagenlogik*], in: Erkenntnis **5**, 1935, pp. 111-131. [Anonymous] Romanian translation [*Din istoria logicii propozit_iilor*], from the Polish original, in: Logică şi filozofie. Orientări în logica modernă şi fundamentele matematicii [Logic and Philosophy: Trends in Modern Logic and the Foundations of Mathematics], Editura politică [Political Publishing House], Bucure_sti 1966, pp. 119-143. English translation [*On the history of the logic of propositions*], from the German version, by Storrs McCall, in: McCall (1967), pp. 66-87. English translation [*On the history of the logic of propositions*] from Polish, by Olgierd Adrian Wojtasiewicz, in: Łukasiewicz (1970), pp. 197-217.)

[43] Jan Łukasiewicz (1961) Z zagadnień logiki i filozofi. Pisma wybrane [Problems of Logic and Philosophy: Selected Writings] (Edited by Jerzy S lupecki), Państwowe Wydawnictwo Naukowe [PWN: Polish Scientific Publishers], Warsaw 1961.

[44] Jan Łukasiewicz (1963) Elements of Mathematical Logic (Translated by Olgierd Adrian Wojtasiewicz), Pergamon Press, Oxford etc. & Państwowe

Wydawnictwo Naukowe [PWN: Polish Scientific Publishers], Warsaw 1963. (Translation of Łukasiewicz (1929), based on the second Polish edition of 1958.)

[45] Jan Łukasiewicz (1970) Selected Writings (Edited by Ludwik Borkowski), North-Holland, Amsterdam & Państwowe Wydawnictwo Naukowe [PWN: Polish Scientific Publishers], Warsaw 1970.

[46] Jan Łukasiewicz, and Alfred Tarski (1930) *Badania nad rachunkiem zdań* [Untersuchungen über den Aussagenkalkül], Sprawozdania z posiedzeń Towarzystwa Naukowego Warszawskiego (Wydzia l III), XXIII, 1930 [Comptes Rendus des séances de la Société des Sciences et des Lettres de Varsovie (Classe III), vol. XXIII, 1930], pp. 30{50, Warszawa 1930. (English translation [*Investigations into the sentential calculus*] in: Tarski (1956), pp. 38-59. Reprinted in Łukasiewicz (1970), pp. 131-152.)

[47] Storrs McCall (ed.) (1967) Polish Logic, 1920-1939, Clarendon Press, Oxford 1967.

[48] David Meredith (1977) *In Memoriam: Carew Arthur Meredith* (1904-1976), Notre Dame Journal of Formal Logic 18, 1977, pp. 513-516.

[49] Jean Nicod (1917) *A reduction in the number of primitive propositions of logic*, Proceedings of the Cambridge Philosophical Society **19**, 1917, pp. 32-41.

[50] Marek Nowak (1992) *On two relatives of classical logic*, Bulletin of the Section of Logic [Łódź, Polish Academy of Sciences] 21 (3), 1992, pp. 97-102.

[51] Ewa Or łowska (1975), *On the Jaśkowski method of suppositions*, **Studia Logica 34** (2), 1975, pp. 187-200.

[52] Charles Sanders Peirce (1885) *On the algebra of logic. A contribution to the philosophy of notation,* American Journal of Mathematics **(1),** 7 (2), 1885, pp. 180-202. (Presented at the National Academy of Sciences, Newport RI, 14-17 October 1884.)

[53] Francis Jeffry Pelletier 1999 *A brief history of natural deduction*, History and Philosophy of Logic 20 **(1),** 1999, pp. 1-31.

[54] Francis Jeffry Pelletier 2001 *A history of natural deduction and elementary logic textbooks*, in: John Woods, and Bryson Brown (eds.) Logical Consequence. Rival Approaches **1,** Hermes Science Publications, Oxford 2001, pp. 105-138.

[55] Dariusz Piętka (2008) *Stanisław Jaśkowski's logical investigations*, Organon **37** (40), 2008, pp. 39-69.

[56] Dag Prawitz 1965 Natural Deduction, *A Proof-Theoretical Study*, Almqvist & Wiksell, Uppsala [Stockholm-Gothenburg-Uppsala] 1965 [*Acta Universitatis Stockholmiensis: Stockholm Studies in Philosophy 3*]. Reprint [with corrections]: Dover Publications, Mineola NY 2006 [*Dover Books in Mathematics*]. (Originally PhD Dissertation, University of Stockholm 1965.)

[57] Adrian Rezuş (1981) Lambda-conversion and Logic, Elinkwijk, Utrecht 1981 (PhD Dissertation, University of Utrecht 1981.)

[58] Adrian Rezuş (1982) *On a theorem of Tarski*, Libertas Mathematica [Arlington TX] 2, 1982, pp. 62-95. (Work of 1979-1980.)

[59] Adrian Rezuş (1983) Abstract AUTOMATH, Mathematisch Centrum [CWI], Amsterdam 1983 [*Mathematical Centre Tracts 160*]. (The Amsterdam MC Tract is revised from a previous draft, issued as a Technical Report by the Eindhoven University of Technology, Department of Mathematics and Computing Science, in September 1982.)

[60] Adrian Rezuş (1986) Impredicative Type Theories, Report TR-KUN-WN-85-1986, University of Nijmegen, Department of Computer Science, Nijmegen 1986.

[61] Adrian Rezuş (1990) Classical Proofs. *λ-calculus Methods in Elementary Proof Theory*, [Draft of a monograph, based on previous lecture notes] Nijmegen [August 5] 1990. (Available online @ www.equivalences.org.)

[62] Adrian Rezuş (1991) Beyond BHK, Nijmegen, December 1, 1991. (A slightly revised version [July 20, 1993, updated bibliographically in 2000] appears online @ www.equivalences.org. Introduction and extended abstract printed separately as Rezuş (1993).)

[63] Adrian Rezuş (1993) Beyond BHK (extended abstract), in: Henk Barendregt, Marc Bezem, and Jan Willem Klop (eds.), Dirk van Dalen Festschrift, University of Utrecht, Utrecht 1993, pp. 114-120 [*Quaestiones In_nitae 5. Publications of the Department of Philosophy, University of Utrecht*].

[64] Adrian Rezuş (2007) *Im Buchstabenparadies*. Gottlob Frege and his Regellogik, Nijmegen 2007, rev. March 16, 2016. (Preprint available online @ www.equivalences.org.)

[65] Adrian Rezuş (2009) *An ancient logic*, Nijmegen 2009, rev. March 23, 2016. (A revised version of this paper has been published as: An Ancient Logic (*Chrysippus and His Modern Readers I*), LAP - Lambert Academic Publishing, Saarbrücken [Germany] 2017 [isbn: 978-3-330-01661-3].)

[66] Adrian Rezuş (2010) *Tarski's claim*: thirty years later, Nijmegen, October 1, 2010. (Preprint available online @ www.equivalences.org.)

[67] Adrian Rezuş (2015) Review of Henk Barendregt, Wil Dekkers, Richard Statman et alii, Lambda Calculus with Types, 2013, Studia Logica 103 (6), 2015, pp. 1319-1326.

[68] Paul C. Rosenbloom (1950) Elements of Mathematical Logic, Dover, New York NY 1950, 2005R.

[69] J. Barkley Rosser (1942) *New sets of postulates for combinatory logics*, The Journal of Symbolic Logic **7** (1), 1942, pp. 18-27.

[70] J. Barkley Rosser (1953) *Logique combinatoire et λ-conversion, in*: Deux esquisses de logique, Gauthier-Villars, Paris & E. Nauwelaerts, Louvain 1953, pp. 3-31.

[71] Bertrand Russell (1906) *The theory of implication*, American Journal of Mathematics **28** (2), 1906, pp. 159-202.

[72] Henry M. Sheffer (1913) *A set of five postulates for Boolean algebras, withapplication to logical constants*, Transactions of the American Mathematical Society 14, 1913, pp. 481-488.

[73] Henry M. Sheffer (1913a) *A set of postulates for Boolean algebra (abstract)*, Bulletin of the American Mathematical Society 19, 1913, p. 283.

[74] Peter Simons (2014) *Jan Łukasiewicz*, in: Edward N. Zalta (ed.), The Stanford Encyclopedia of Philosophy (rev. June 6, 2014). *Online @ plato.stanford.edu/archives/sum2014/entries/lukasiewicz*.

[75] Morten Heine Sørensen, and Paweł Urzyczyn (2006) Lectures on the Curry-Howard Isomorphism, Elsevier Science, Amsterdam etc. 2006 [*Studies in Logic and the Foundations of Mathematics 149*]. (Originally, Lecture Notes for a one-semenster graduate / PhD course held at DIKU [University of Copenhagen, Department of Computer Science], Copenhagen, 1998-1999.)

[76] Kristian Støvring (2006) *Extending the extensional lambda calculus with surjective pairing is conservative*, Logical Methods in Computer Science 2 (1), 2006, pp. 1-14. (Also: Research Report BRICS RS-05-35, DAIMI, Department of Computer Science, University of Aarhus, Aarhus [Denmark], November 2005.)

[77] Alfred Tarski (1930) *O niektórych podstawowych pojęciach metamatematyki* [Über einige fundamentalen Begri_e der Metamathematik], Sprawozdania z posiedzcen Towarzystwa Naukowego Warszawskiego (Wydzia l III), XXIII, 1930 [Comptes Rendus des séances de la Société des Sciences et des Lettres de Varsovie (Classe III), XXIII, 1930], Warszawa 1930, pp. 22-29. (English translation [*On some fundamental concepts of metamathematics*], in Tarski (1956), pp. 30-37.)

[78] Alfred Tarski (1956) Logic, Semantics, Metamathematics, *Papers from 1923 to 1938* (Translated by J. H. Woodger), Clarendon Press / Oxford University Press, Oxford UK 1956.

[79] Kazimierz Trzęsicki (2007) *Polish logicians' contribution to the world's informatics*, Studies in Logic, Grammar and Rhetorics **11** (24), 2007, pp. 15-34.

[80] Mordchaj Wajsberg (1937-1939) *Metalogische Beiträge I, II*, Wiadomości Matematyczne **43** 1937, pp. 1-38, and 47, 1939, 119-139. (English translation in McCall (ed.) (1967), pp. 285-334. Also in: Mordchaj

Wasjberg Logical Works (edited by Stanis law J. Surma), Ossolineum, Wroc law 1977.)

[81] Alfred N. Whitehead, and Bertrand Russell (1910-1913) Principia Mathematica 1-3, Cambridge University Press, Cambridge UK 1:1910, 2:1912, 3:1913; 1:19252, 2-3:19272.

[82] Jan Woleński (1985) Filozoficzna Szkoła Lwowsko-Warszawska [The Lvov-Warsaw Philosophical School], PWN,Warszawa 1985. (English version: Logic and Philosophy in the Lvov-Warsaw School, Kluwer Academic Publishers, Dordrecht 1989.)

[83] Jan Woleński (2015) *Lvov-Warsaw School*, in: The Standford Encyclopedia of Philosophy (rev. Oct. 18, 2015). Online @ *http://plato.stanford.edu/entries/lvov-warsaw/*

[84] Urszula Wybraniec-Skardowska (2009) *Polish logic - some lines from a personal perspective*, Publications of the Institute for Logic, Language and Computation, Amsterdam [July] 2009.

[85] Congress [PPZM] Proceedings (1929) Księga Pamiątkowa Pierwszego Polskiego Zjazdu Matematycznego [Commemorative Book of the First Polish Mathematical Congress], Lwów, 7-10.IX.1927 [Dotatek do 'Annales de la Sociètè Polonaise de Mathèmatique'], Kraków [Czonkami Drukarni Universytetu Jagiellonskiego] 1929. (Online @ *www.ptm.org.pl/zawartosc/i-zjazd-ptm-1927-e-book.)*

PART 2

ON THE LIFE AND WORK OF LOGICIANS

IN MEMORY OF RAYMOND SMULLYAN

Melvin Fitting

Raymond Smullyan passed away February 6, 2017, at the age of 97. He had a long and distinguished career as a logician, and an equally distinguished career as a writer of popular puzzle books based on logic. He had many other interests as well, and I will touch on these later on. Prof. Smullyan was born May 25, 1919 in Far Rockaway, a part of New York City very far from the city's centers. He showed independence early on, attended several colleges, and finally received an undergraduate degree from the University of Chicago, and then a PhD from Princeton, where Alonzo Church was his mentor. This was in 1959, when he was already 40, a late start for an academic career.

Prof. Smullyan's first published paper preceded his degree and remained one of his most cited academic works, "Languages in which self reference is possible", in *Journal of Symbolic Logic*, 1957. In it he reduced Gödel's machinery for proving incompleteness to a minimum, establishing that such results held for formal systems substantially weaker than Peano arithmetic. In a sense he extracted the essential core of Gödel's argument.

His 1959 dissertation gave rise to several published papers. The dissertation itself was published in 1961 by Princeton University Press as Smullyan's first book, *Theory of Formal Systems*. It introduced a remarkable range of influential material. His work from "Languages in which self-reference is possible" was developed as part of a general approach. A new atomata theoretic class called *rudimentary* was introduced. Fundamental results in recursion theory were proved, in particular a double recursion theorem. And what was of most interest to me personally, an extremely simple formalization of recursion theory itself was created, *Elementary Formal Systems*. This reduced the machinery needed to define recursive enumerability and the recursive functions to an intuitively attractive minimum. And this is something I want to say a few things about.

In the 1960's there was much work concerning the notion of computation—in particular, what might computation mean over arbitrary structures. Many different approaches were introduced and equivalences were established. Much depended, for instance, on whether the structure admitted a recursive pairing function. But in two short abstracts in 1956 in the *Bulletin of the American Mathematical Society*, Smullyan had already sketched elementary formal systems for arbitrary structures, and noted that on a structure of arithmetic it yielded ordinary recursion theory; on a structure of words over a finite alphabet it yielded Turing computability on words, and so on. Unfortunately this generality was not discussed in *Theory of Formal Systems*, and the extent of applicability was not generally known until years later.

For a number of years logic programming was an important topic of research in computer science. A semantics was developed for it (without negation as failure) by van Emden and Kowlski in 1976. It wasn't realized until later that Smullyan's elementary formal systems, on the structure of formal terms, essentially coincided with Prolog, and the van Emden/Kowalski approach had already appeared in one of Smullyan's 1956 abstracts. It should give some idea of the fruitfulness of Smullyan's work from this period, that important developments could lay there unrecognized because there was so much else to think about.

Prof. Smullyan's second book was *First-Order Logic* , in 1968. The idea, in part, was to simplify Beth's semantic tableau machinery and use it as the basis for first-order logic. Along the way the *Model Existence Theorem* was introduced (with a different name), *uniform notation* was presented, a constructive proof of cut elimination was given in an abstract setting that yielded it simultaneously for both tableaus and the sequent calculus. The book has influenced several generations of logicians, and has been reprinted in the Dover book series, 2010. This work has also had an unanticipated life in computer science, in the field of automated theorem proving. The Tableaux conference is devoted to the use of tableau methods applied to a wide range of logics, and most of the papers at these meetings trace back to Smullyan's book in one way or another.

Subsequently Prof. Smullyan wrote several other books giving his mature thoughts about the topics that always occupied him, beginning with *Theory of Formal Systems*. These are *Gödel's Incompleteness Theorems*, 1992, *Recursion Theory for Metamathematics*, 1994, and *Diagonalization and Self-Reference*, 1994. In addition, jointly with Melvin Fitting, there is a comprehensive treatment of axiomatic set theory, Gödel's constructible sets, and Cohen's forcing, in *Set Theory and the Continuum Problem*, 1996.

At some point in the 1970's, while continuing his mathematical logic researches, Prof. Smullyan developed an interest in puzzles that are based on logic. Martin Gardner devoted a column in the magazine *Scientific American* to some of these, and they turned out to be quite popular. This led to *What is the Name of This Book*, 1978. It was quite successful, and was followed by other books: *This Book Needs No Title*, 1980, *Alice in Puzzle-Land*, 1982, and many, many more. There have been a large number of translations into many languages, and non-academics may know Prof. Smullyan entirely through these works.

Prof. Smullyan realized that puzzles could be used to get across some of the fundamental discoveries of modern logic. This led to his book *The Lady or the Tiger?* in 1983, which took readers through the basic ideas of Gödel's incompleteness theorem, via a series of puzzles.

This was followed by *To Mock a Mockingbird*, 1985, which explored the Lambda calculus via puzzles about birds. The Lambda calculus is a system of formal logic with applications to the design and semantics of computer languages. Prof. Smullyan's book was of such interest that the automated theorem-proving group at Argonne National Laboratories once developed a program specifically for solving his so-called bird puzzles. Another work along these lines is *Forever Undecided*, 1987, which discusses Gödel's theorem in greater detail that before, and also modal logic and provability logics.

Smullyan's book, *Satan, Cantor, and Infinity*, 1992, discusses issues of probability, infinity, time, and change, largely through his familiar medium of puzzles. And I should mention *The Magic Garden of George B and Other Logic Puzzles*, 2007, which uses puzzles to present Boolean algebra and the Stone representation theorem.

For his last works Prof. Smullyan turned back to a more conventional format and wrote two textbooks which he hoped would be useful in the teaching of first-order logic and its metatheory. These are *A Beginner's Guide to Mathematical Logic*, 2014, and *A Beginner's Further Guide to Mathematical Logic*, his last book, published in 2016.

Prof. Smullyan also had an interest in what are called retrograde analysis chess puzzles. A simple example of one of his puzzles might show a board, you are told a piece is missing, and asked whose move it is. (It may not be possible to figure out which piece is missing, by the way. It depends on the puzzle.) Prof. Smullyan incorporated his puzzles into witty and entertaining stories, and these were published as *The Chess Mysteries of Sherlock Holmes*, 1979, and *The Chess Mysteries of the Arabian Knights*, 1981.

Prof. Smullyan had numerous interests outside logic. He was a first-rate pianist, and once considered a career as such. But he developed problems with his tendons and decided to concentrate on mathematical logic instead. Several of his performances can be found on YouTube, and one CD is available for purchase.

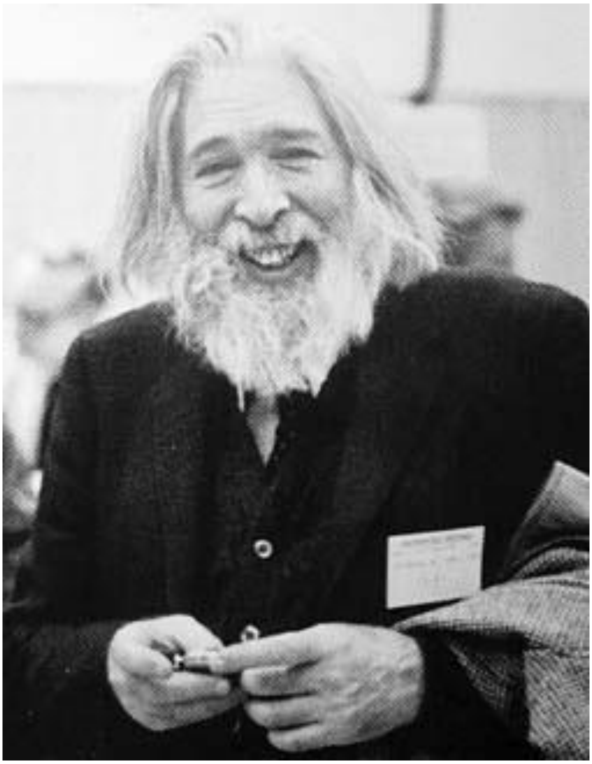
In his younger days Prof. Smullyan supported himself as a professional magician, doing slight of hand at tables in a nightclub. Almost to the end of his life, when he visited a restaurant he would bring a pack of cards and would go from table to table entertaining diners. I've seen this happen often, and always a good time was had by all.

There were lesser interests that would have been major for anyone else. He had a lifelong enthusiasm for astronomy and, when I first knew him he was making his own (quite large) telescopes, grinding his own lenses. Much later he became interested in sound reproduction, and put together some very formidable speaker systems with somewhere around 40 speakers, each of which could be individually controlled, to experiment with sound balance and related aspects of music reproduction. Prof. Smullyan also experimented with three-dimensional photography for many years. He developed a way to build viewers that were better and cheaper than those commercially available, and I believe even published an article about it.

Although Prof. Smullyan claimed to not know if he was religious or not, he had a deep interest eastern religions. Probably his best-known writing in this area is *The Tao is Silent* from 1977.

Prof. Smullyan received his undergraduate degree from the University of Chicago in 1955 and his PhD from Princeton in 1959 with Alonzo Church. He taught at Belfer Graduate School of Yeshiva University from 1961 to 1968, at City University of New York from 1968 to 1982, and at Indiana University at Bloomington from 1981 to 1989, when he retired from teaching. He was married briefly when he was young, and subsequently to Blanche de Grab, also a pianist, who ran a well-regarded music school in Manhattan for many years. This was a long and happy marriage. Blanche passed away in 2006 at the age of 100, having been cared for by Raymond and assistants during the last year of her life.

Prof. Smullyan was a remarkable man, humorous and kind. He was intense, concentrating on his own interests sometimes to the exclusion of all else, but his interests were, in fact, of wide interest. I was honored to know him. He left a legacy for us all.



Raymond Smullyan

(May 25, 1919 – February 6, 2017)

ANDRAS HAJNAL, LIFE AND WORK[1]

Mirna Džamonja

On July 30, 2016, the international set theory community lost one of its greatest, long standing, contributors: Andras Hajnal. He was known for his many theorems, including the Hajnal Free Set Theorem, partition calculus, where together with Erdős and Rado he was a founding father, and the theory of set mappings. He is also, with Galvin, the author of a celebrated theorem in cardinal arithmetics which was a precursor to Shelah's pcf theory. Although mostly known for his work on combinatorial set theory, Hajnal contribued to the study of constructibility, in an early work that extended the work of Godel by introducing the idea of relative constructibility. He also made major contributions to finite combinatorics, including his theorem with Szemeredi on equitable coloring of graphs that proved a conjecture of Erdős. In this article we shall briefly speak about Hajnal's life and then review some of his greatest theorems.

András Hajnal's life

Andras Hajnal was born on May 13, 1931 in Budapest, the city where he spent many years, from which he moved to the United States, to which he eventually returned at the end of his life, and where he died. He lived difficult days in Budapest during the second world war and, after being liberated by the Russian soldiers in 1945, he determined that whatever would happen later, he had had the hardest part of his life and it should go differently later. We have certainly witnessed that, as far as mathematics is concerned, it certainly did, since his was one of the most successful careers in mathematics. A large part of it is closely connected to the Eotvos Lorand University in Budapest, where he received his university diploma in 1953 and where he was a faculty member from from 1956 to 1972. He studied for his Candidate of

1. The full text of the present article, which has been written at the request of the editors, is available on the official website of the European Set Theory Society too: https://ests.wordpress.com/2016/07/30/andras-hajnal-may-13-1931-july-30-2016/

Mathematical Sciences at the University of Szeged degree, under the supervision of Laszlo Kalmar and obtained it in 1957 and then he obtained his Doctor of Mathematical Science degree in 1962. (Both these degrees in Hungary at that time were given by an independent national agency, not by a single university). In 1972, Hajnal started to work at the Math. Inst. of Hungarian Academy of Sciences as head of the department. He was elected member of the Hungarian Academy of Sciences in 1976. Then, in 1994 he moved to Rutgers University (USA) to become the director of the centre DIMACS, and he remained there as a professor until his retirement in 2004.

Hajnal was an Honorary President of the European Set Theory Society and, since 1982, a member of the Hungarian Academy of Sciences, where he directed its mathematical institute from 1982 to 1992. He was the general secretary of the Janos Bolyai Mathematical Society from 1980 to 1990, and president of the society from 1990 to 1994. Since 1981, he has been an advisory editor of the journal Combinatorica. In 1992, Hajnal was awarded the Officer's Cross of the Order of the Republic of Hungary. Hajnal’s influence on mathematics and mathematicians is enormous. We shall discuss Hajnal’s mathematics shortly, as for mathematicians, some of his students were Miklos Ajtai, Richard Carr, Peter Hamburger, Istvan Juhasz, Peter Komjath, Gyorgy Petruska and Lajos Soukup. Some of his mathematical grandchildren are Marianna Csornyei and Miklos Laczkovich. Generations of mathematicians in Hungary learned their set theory from a book written by Hajnal and Mate, which is since 1999 available in English, in an updated version authored by Hajnal, Mate and Hamburger. My students in England and in France invariably get this book on their reading list and they love it.

The writer of these lines did not have the chance to co-author a paper with Hajnal, but we were good friends and colleagues. I saw him regularly at Rutgers, where I was a frequent visitor and I had the honour to speak at the 1999 MAMLS conference at Rugers which was devoted to Hajnal. We were sad at that conference since it was known that he had been diagnosed with lung cancer. But, in spite of the odds, he made it! He fully recovered and a happy '80th birthday of Hajnal' conference was held at Rutgers in 2011. In 2007 my mother was diagnosed with lung cancer and I asked Andras for advice and contacts. He generously shared all he knew, but unfortnately it did not work neither for my Mum, who died in 2009, nor for Hajnal's own wife Emilia who died of lung cancer in 2015. I wrote to him 'You had such a wonderful life together and were an example of a couple whose love lasted a lifetime.' I have heard that Andras was no longer the same after she died, and he left us too, suddenly, of a heart attack. The last correspondance we had dates from 1st of July 2016, just a few weeks before his death, when we discussed the compactness of the chromatic number of graphs. Although he claimed the he was slower than before, I found him totally up to it and I am sorry that we could not continue these discussions later.

Emilia and Andras had one son, Peter, who is a very successful scientist in his own right.

Hajnal's Mathematics

All together, Hajnal published 164 papers and four books. One of the books is a celebrated bible of the partition calculus, “Combinatorial set theory. Partition relations for cardinals”, co-authored with Erdős, Mate and Rado. Another one is the book “Set Theory” which we mentioned above (in Hungarian and in English) and, in addition, he wrote a school manual on graph theory for school children. He also edited 7 volumes of mathematical papers. Hajnal’s papers were written in three different languages: Hungarian, German and English. A list of Hajnal’s papers is available on his web page at: https://www.renyi.hu/~ahajnal/hajnalpu.pdf

We refer to this publication list for references, although it is unfortunately not complete.

The first association that Hajnal's name gives us is the combinatorial set theory, including his many papers with Paul Erdős. But his first work was on something entirely different: in his Ph.D. thesis in written in 1956, he introduced the models $L(A)$ and proved that if κ is a regular cardinal and A is a subset of κ^+ then ZFC and $2^\kappa = \kappa^+$ hold in $L(A)$. This can be applied to prove relative consistency results:

e.g., if $2^{\aleph_0} = \aleph_2$ is consistent then so is $2^{\aleph_0} = \aleph_2$ and $2^{\aleph_1} = \aleph_2$. This was before the invention of forcing which gave a tool for proving such consistency results. These results were published as a paper in 1961.

Moving on, we quickly arrive, in 1961 (paper 12 on his list of publications), to the celebrated Hajnal's free set theorem. Suppose that we have a set S of size κ, a cardinal $\lambda < \kappa$ and a function $f: S \mapsto [S]^{<\lambda}$. A subset S' of S is *free* if for every X, Y in S' we have that X does not belong to $f(Y)$ and vice versa. Ruziewicz had conjectured that in this situation there must be a free set of size κ. Continuing a line of partial results by eminent authors, Hajnal finally confirmed this conjecture in a "surprisingly simple and ingenious way", as said Paul Erdős.

Hajnal is famous for his work on partition relations for cardinals, much of it in collaboration with Paul Erdős. Indeed, together they published 56 papers, both in finite and in infinite combinatorics. They also largely influenced the international community by publishing papers containing a list of open questions. He was also a majr contributor in the partition calculus of ordinals, including his result with Baumgartner (1973) that for every partition of pairs of vertices of the complete graph on $\aleph_1$ vertices into finitely many subsets, at least one of the subsets contains a complete graph on α vertices for every countable α. This result contains a new idea in the method of proof, since it was first proved under MA and then converted into a ZFC result by absoluteness.

With Juhasz (himself now a member of the Hungarian Academy of Sciences), Hajnal worked on set-theoretic topology and they were the first ones (1968) to construct an S-space and an L-space. They published 32 joint papers.

In graph theory, Hajnal made contributions both in the finite and the infinite domains. A celebrated construction is his construction of two graphs of chromatic number $\aleph_1$ whose product is countably chromatic (1985). This shows that Hiedetniami conjecture is false for the infinite. In finite graph theory, probably his most well known result is The Hajnal–Szemeredi theorem (1970) on equitable coloring, proving a 1964 conjecture of Erdős: let Δ denote the maximum degree of a vertex in a finite graph G. Then G can be colored with $\Delta + 1$colors in such a way that the sizes of the color classes differ by at most one. Hajnal has several important papers in graph theory with his former student Komjath, now a member of Hungarian Academy in his own right.

In a different part of set theory, Hajnal proved together with Galvin (1975, Annals of Mathematics) a result that was very unexpected at the time: if $\aleph_{\omega_1}$ is a strong limit cardinal then $2^{\aleph_{\omega_1}} < \aleph_{(2^{\aleph_1})^+}$. This was the result that initiated Shelah's pcf theory.

Hajnal had many other great contributions and continued producing mathematics to the very end of his life.

What else to say? All great men die but behind some of them, their theorems remain. Hajnal was in this class.

Acknowledgements

We thank I. Juhasz and P. Komjath for the comments on an earlier version of this document.

András Hajnal

(May 13, 1931 – July 30, 2016)

MY MEMORIES OF PROFESSOR JACK SILVER

Aleksandar Ignjatovic

I was very saddened by the news that Professor Jack Silver[1], whom I had the privilege of having as my PhD thesis advisor, had passed away on December 22, 2016. During my doctoral studies at UC Berkeley from the fall of 1985 to the spring 1990 I used to meet with him almost every week and had developed quite a close relationship with him. He tended to be quite a private and reserved man, but, at the same time, I found him to be very kind and generous. He would take me for lunch from time to time and we had lively discussions on many topics.

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1. Jack Howard Silver was a set theorist and logician at the University of California, Berkeley. Born in Montana, he earned his Ph.D. in Mathematics at Berkeley in 1966 under Robert Vaught before taking a position at the same institution the following year. He held a Alfred P. Sloan Research Fellowship from 1970 to 1972. Silver made several contributions to set theory in the areas of large cardinals and the constructible universe L. n his 1975 paper "*On the Singular Cardinals Problem*", Silver proved that if a cardinal κ is singular with uncountable cofinality and $2^{\lambda} = \lambda^{+}$ for all infinite cardinals $\lambda < \kappa$, then $2^{\kappa} = \kappa^{+}$. Prior to Silver's proof, many mathematicians believed that a forcing argument would yield that the negation of the theorem is consistent with ZFC. He introduced the notion of a master condition, which became an important tool in forcing proofs involving large cardinals. Silver proved the consistency of Chang's conjecture using the Silver collapse (which is a variation of the Levy collapse). He proved that, assuming the consistency of a supercompact cardinal, it is possible to construct a model where $2^{\kappa} = \kappa^{++}$ holds for some measurable cardinal κ. With the introduction of the so-called Silver machines he was able to give a fine structure free proof of Jensen's covering lemma. He is also credited with discovering Silver indiscernibles and generalizing the notion of a Kurepa tree (called Silver's Principle). He discovered $0^{\#}$ ("zero sharp") in his 1966 Ph.D. thesis, discussed in the graduate textbook *Set Theory: An Introduction to Large Cardinals* by Frank R. Drake. Silver's original work involving large cardinals was perhaps motivated by the goal of showing the inconsistency of an uncountable measurable cardinal; instead he was led to discover indiscernibles in L assuming a measurable cardinal exists. Ref: https://en.wikipedia.org/wiki/Jack_Silver

Needless to say, he had a truly extraordinary mind. His memory had perfect recollection and his knowledge of European history was truly astonishing. Sometimes he would ask me about historic figures and events from the Serbian past about which I had never heard before, but which I would subsequently find in books at the main library of the UC Berkeley.

From time to time I would get stuck not being able to understand a proof from a paper I was reading. Jack would take a brief look at the paper and would then often come up with his original solution which would make the paper appear almost silly. Once, I could not help it and I asked him in astonishment: “How did you do this?” “Did what?” he asked back. “How did you come up with such a solution?” I clarified. He looked puzzled and replied “I do not know, I just saw it!” Jack was not just an extremely smart man, he was a true genius.

The desk in his office had a huge and ever growing pile of papers on top of it, several feet high. Once I decided to play a prank on him – I was taking his Proof Theory class for which I had submitted homework, so I asked him if he could give me back my homework because I wanted to keep it. To my utmost amazement, he stuck his arm into the middle of the pile on his desk and pulled out my homework!

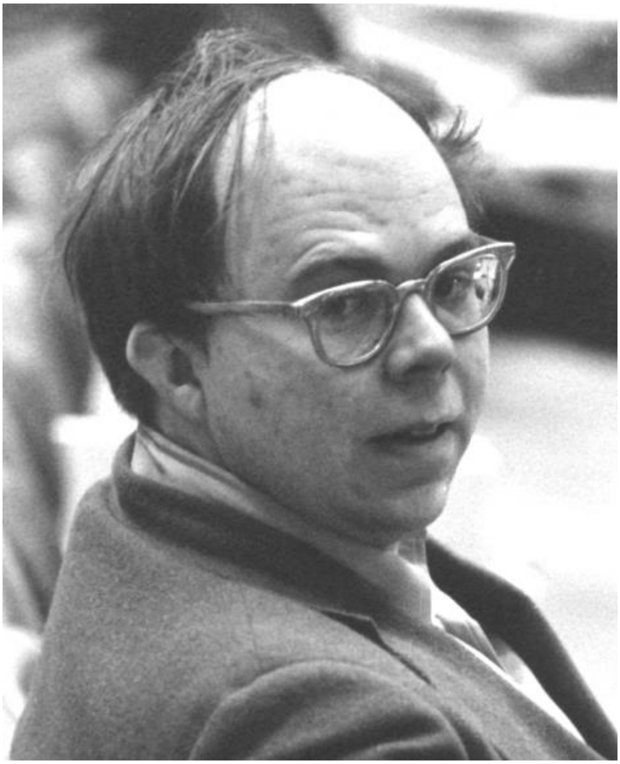
As a prototypical Professor, Jack always wore a jacket that was so old that he probably had bought it during his student years; in fact, on all photos I have seen of him, he always wears this very jacket.

However, he loved fancy cars and had a top of the line Volvo. Once he came late for our weekly meeting, noticeably upset. “You would not believe what has just happened to me!” he said. It turned out that he was having a problem unlocking his car and a nearby policeman approached him and grabbed his hand asking “What do you think you are you doing?!”. Apparently, the policeman could not believe that a person wearing such an old jacket could have such a fancy car, so Jack had to show him the registration.

As with everyone else at UC Berkeley, I had heard rumors that he believed that ZFC set theory was inconsistent, so once I asked him if this was true. “Well, that might very well be the case,” he said with a devilish smile, “but what is definitely true is that quite a few set theorists are seriously worried that I might just prove that ZFC is indeed inconsistent!”

He was a very sincere, genuine and entirely unpretentious man; to my utmost bewilderment, once he told me: “I never proved anything interesting or important, despite many attempts.” Knowing Jack all too well, I knew that he meant it seriously.

He will always occupy a special place in my memory and my heart.



Jack Silver

(23 April 1942 – 22 December 2016)

A LOGICIAN'S AUTOBIOGRAPHY

John Corcoran

1. Introduction

John Corcoran (born 1937, Baltimore, USA) is a logician, philosopher, mathematician, linguist, and historian of logic. His philosophical work stems from his desire to understand proof. This led to concern with the interrelations of objectual, operational, and propositional knowledge, the nature of logic, the nature of mathematical logic, information-theoretic foundations of logic, conceptual structures of metalogic, relationships of logic to epistemology and ontology, and roles of proof theory and model theory in logic. His interests and conclusions continue to evolve but many are foreshadowed in his earliest works, especially his 1973 paper "Gaps between logical theory and mathematical practice". See References below.

Corcoran's papers have been translated into Arabic, Greek, Persian, Portuguese, Spanish, and Russian. His 1989 signature essay "Argumentations and logic" has been translated into four languages. Fourteen of his papers have been reprinted; one was reprinted twice. He has been principal author on over 40 co-authored works. His 2015 article "Existential import today", coauthored with the Iranian logician Hassan Masoud, is currently first on its journal's most-read list with over 5000 readers.

His dedication and service to his colleagues and his constant interest in their contributions are reflected in his many published reviews, over 100 in Mathematical Reviews alone, the latest in 2017.

2. Education

Pre-doctoral studies: Baltimore Polytechnic Institute, Engineering 1956, Johns Hopkins University BES Engineering 1959, MA Philosophy 1962, PhD Philosophy 1963.
Dissertation: Generative Structure of Two-valued Logics; Supervisor: Robert McNaughton, a PhD student of Willard Van Orman Quine.

Post-doctoral studies: Yeshiva University Mathematics 1963–4, University of California Berkeley Mathematics 1964–5.

Corcoran's student years, the late 1950s and early 1960s, were wonderful times to be learning logic, its history, and its philosophy. His first logic teacher was Albert Hammond, who passed on from his own dissertation supervisor Arthur Lovejoy the tradition of the history of ideas—a tradition his university, Johns Hopkins University, was known for. Corcoran studied Plato and Aristotle with Ludwig Edelstein, the historian of Greek science and medicine who taught at the University and at the School of Medicine. His next two logic teachers were both accomplished and knowledgeable symbolic logicians: Joseph Ullian, a Quine PhD, and Richard Wiebe, a Benson Mates PhD who had studied with Carnap and Tarski.

Corcoran's dissertation supervisor, his "doctor father", was Robert McNaughton, who had already established himself in three fields: the metamathematics of number theory, the theory of formal languages, and the theory of automata. McNaughton encouraged Corcoran to do post-doctoral studies at Yeshiva University in New York City with Raymond Smullyan and Martin Davis, both doctoral students of Alonzo Church. McNaughton later encouraged Corcoran to go to UC Berkeley, the world center for logic and methodology, and he recommended Corcoran for a Visiting Lectureship at Berkeley. McNaughton was also instrumental in Corcoran's move to his first tenure-track position, in Linguistics at the University of Pennsylvania, where

McNaughton was a Professor of Computer and Information Science. In those early years Corcoran also attended semester-long courses and seminars by several other logicians, including John Addison, a Stephen Kleene PhD, Leon Henkin, another Church PhD, and John Myhill, another Quine PhD. Corcoran often mentions his teachers with great respect and warmth.

3. History of Logic

Corcoran's work in history of logic involves most of the discipline's productive periods. His approach to history has evolved but he still holds to the basic principles outlined in his 1974 article "Future research on ancient theories of communication and reasoning". He has discussed Aristotle, the Stoics, William of Ockham, Giovanni Girolamo Saccheri, George Boole, Charles Pierce, Richard Dedekind, Giuseppe Peano, Gottlob Frege , Bertrand Russell, the American Postulate Theorists, David Hilbert, C. I. Lewis, Jan Łukasiewicz , Stanisław Jaskowski, Alfred Tarski, Willard Van Orman Quine, Warren Goldfarb, and others.

4. Aristotle

His 1972 work on Aristotle's logic of the Prior Analytics, considered radical at the time, came to be regarded as faithful both to the Greek text and the historical context. It was adopted for the 1989 translation of the Prior Analytics by Robin Smith and for the 2009 translation of the Prior Analytics Book A by Gisela Striker. His 2009 article "Aristotle's demonstrative logic" presents to a broad audience an amended and refined version of the philosophical and historical consequences of the 1972 work without the mathematics. His interpretation of Aristotle's Prior Analytics, proposed independently by Timothy Smiley of Cambridge University at about the same time, has been instrumental in subsequent investigations by several scholars including George Boger, Newton da Costa, Catarina Dutilh, Kevin Flannery, John Martin, Mario Mignucci, Michael Scanlan, Robin Smith, and others.

5. Boole

His controversial 1980 critical reconstruction of Boole's original 1847 system revealed previously unnoticed gaps and errors in Boole's work and, moreover, it established the essentially Aristotelian basis of Boole's philosophy of logic thus undermining the groundless opinion that Boole sought to refute Aristotle. In a 2003 article he provided a systematic comparison and critical evaluation of Aristotelian logic and Boolean logic. A series of his abstracts and articles reveal the richness of Boole's fertile imagination and his previously unrecognized philosophical depth. For example, his 2005 article shows the connections of Boole's 1847 Principle of Wholistic Reference to doctrines later proposed by Frege and then, in a more modern setting, by Quine.

6. Tarski

In the late 1970s and early 1980s, he worked with Alfred Tarski on editing and correcting Tarski's classic 1956 collection Logic, Semantics, Metamathematics, which includes the famous truth-definition paper. The new edition appeared in 1983 with Corcoran's Editor's Introduction. In 1991 Mathematical Reviews invited him to review Alfred Tarski's Collected Papers, 4 vols. (1986). His collaboration with Tarski resulted in publications on Tarski's work including the 2007 article "Notes on the Founding of Logics and Metalogic: Aristotle, Boole, and Tarski" which traces Aristotelian and Boolean ideas in Tarski's work and which confirms Tarski's status as a founding figure in logic on a par with Aristotle and Boole.

7. Mathematical Logic

His mathematical logic treats propositional logics, modal logics, identity logics, syllogistic logics, standard first-order logics, the first-order logic of variable-binding term-operators, second-order logics, categoricity, definitional equivalence, model theory, and the theory of strings—a discipline first axiomatized in Tarski's 1933 truth-definition paper. The theory of strings, also known as concatenation theory and as abstract syntax, is foundational in all areas of logic. Corcoran's work in string theory dates to his earliest meetings as a graduate student with McNaughton who was then applying string theory to computer science and formal linguistics. String theory provides essential background for all of Corcoran's other mathematical work and it plays a seminal role in his philosophy.

8. Philosophy

In all of Corcoran's philosophy, especially his philosophy of mathematics, he has been guided by a nuanced and inclusionary Platonism which strives to do justice to all aspects of mathematical, logical, and linguistic experience including those aspects emphasized by competing philosophical perspectives such as logicism, constructivism, deductivism, and formalism. All of his work is grounded in the classical two-valued logic that was codified in the 1900s, often but not exclusively standard first-order logic. Non-standard logics have little relevance to his thinking.

Although several of his philosophical papers presuppose little history or mathematics, his historical papers often involve either original philosophy (e.g. his 2006 article "Schemata") or original mathematics (e.g. his 1980 article "Categoricity") and his mathematical papers sometimes contain original history or philosophy (e.g. the 1974 article "String theory"). He has referred to the mathematical dimension of his approach to history as mathematical archaeology.

His philosophical papers often involve original historical research. He has also been guided by the Aristotelian principle that the nature of modern thought is sometimes fruitfully understood in light of its historical development, a lesson that Corcoran attributes to Arthur Lovejoy's History of Ideas Program at Johns Hopkins University. Corcoran's attempt to integrate philosophy, mathematics, linguistics, logic, and history had been encouraged for many years by his Buffalo colleagues, especially the late American philosopher and historian Peter Hare.

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References

1972. *Completeness of an ancient logic*, Journal of Symbolic Logic 37, 696–702.

1973. *Gaps between logical theory and mathematical practice*. Methodological Unity of Science, Ed. Mario Bunge. Reidel. 23–50.

1974. *Future research on ancient theories of communication and reasoning*. Ancient Logic and Its Modern Interpretations, Ed. John Corcoran. Reidel. 185–187. MR0497848

1974. *String theory*. (Co-authors: William Frank, Michael Maloney). Journal of Symbolic Logic 39, 625–37.

1980. *Categoricity*. History and Philosophy of Logic 1, 187–207.

1980. *Boole's Criteria of validity and invalidity*. (Co-author: S. Wood). Notre Dame Journal of Formal Logic 21, 609–39.

1983. Editor's introduction to the revised edition. *Logic, Semantics, Metamathematics* by Alfred Tarski, translated by J. H. Woodger. Hackett. MR85e:01065.

1989. *Argumentations and logic*. Argumentation 3, 17–43.

1991. *Review: Alfred Tarski's Collected Papers*, 4 vols. (1986) edited by Steven Givant and Ralph McKenzie. Mathematical Reviews 91h:01101

2003. *Aristotle's Prior Analytics and Boole's Laws of Thought*. History and Philosophy of Logic 24, 261–288.

2005. *Wholistic reference, truth-values, universe of discourse, and formal ontology*: Manuscrito 28, 143–167.

2006. *Schemata: the concept of schema in the history of logic*. Bulletin of Symbolic Logic 12, 219–40.

2007. *Notes on the founding of logics and metalogic: Aristotle, Boole, and Tarski*. Eds. C. Martínez et al. Current Topics in Logic and Analytic Philosophy / Temas Actuales de Lógica y Filosofía Analítica. Imprenta Univeridade Santiago de Compostela (University of Santiago de Compostela Press).145–178.

2009. *Aristotle's demonstrative logic*. History and Philosophy of Logic 30, 1–20.

2015. *Existential import today: New metatheorems; historical, philosophical, and pedagogical misconceptions*. (Co-author: Hassan Masoud). History and Philosophy of Logic 36, 39–61.

2017. Review of Paseau, Alexander, "*Knowledge of mathematics without proof*". British J. Philos. Sci. 66 (2015), no. 4, 775–799. Mathematical Reviews. MR3427518.

John Corcoran

(Born 1937)

PART 3

LOGIC COMMUNITIES AROUND THE WORLD

LOGIC IN BOGOTA: SOME NOTES

Andres Villaveces

In November of 2015 I had the chance to visit the IPM in Tehran for a few days. In addition to a minicourse in the model theory of Abstract Elementary Classes, I had an intense schedule of mathematical interaction with various members of the Institute. I was invited recently by Ali Sadegh Daghighi[1] to offer for this special issue a personal perspective of the Bogotá Logic Group, and some connections and perceptions of my own visit to Tehran. These notes purport to do that.

1. The Bogotá Logic Group - Origins

1.1. Early Years

Academic interest in contemporary mathematical logic had been part of the background of mathematics and philosophy departments of the local universities since the mid-1960s[2] - there were some early reading seminars - but it wasn't until the late 1970s that actual research in mathematical logic started being done in a systematic way by members of the local universities.

The return to Colombia of model theorist Xavier Caicedo in 1977 from his doctoral studies at the University of Maryland triggered the formation of a genuine group of people doing research in

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1. I want to thank Ali Sadegh Daghighi for his invitation to write this memoir. I also want to thank the Tehran Logic Group for their very warm hospitality (and the great academic interactions) during my visit in November 2015. In particular, the work of Zaniar Ghadernezhad and Massoud Pourmahdian made possible that visit and the start of a very interesting academic interaction.

2. Only the departments at Universidad Nacional and Universidad de los Andes, in those early years.

mathematical logic. The beginnings were slow (there was no research institute as such[1], and research activities were barely organized in a systematic way back then) but sure-footed: after a few years, Caicedo had formed several Master's students who went on to continue their doctoral formation abroad, and there was an active logic seminar.

The role of the Latin American Mathematical Logic Symposia (known as SLALM by their initials in Spanish) was crucial in the consolidation of the group. Bogotá hosted (at Universidad de los Andes) the 5th SLALM in 1981 and in many ways this event may be regarded as marking the end of the beginning years and the opening a new phase.

It is worth mentioning, anyway, that this first period is marked by both a beginning in isolation and a strong pull against this isolation: although research conditions were scarce (not only in terms of money but first and foremost in terms of organization) and logicians like Caicedo back then had to work in many simultaneous fronts combining research, teaching and administration, the existence of a genuine network of Latin American logicians provided a very interesting support even in those early days[2].

On a more personal note, some very solid friendships of logicians formed back then, at a Latin American level. In this regard, the role of set theorist Carlos Di Prisco was crucial for the consolidation of the group in Bogotá[3]. He was roughly at the same time developing the

1. Even in 2017, there is no independent research institute doing Mathematics in Colombia. All research in mathematical logic is conducted at the universities, by members of the faculty who must combine teaching basic courses, advanced courses, and doing research.

2. The network has continued, as we shall see later.

3. Di Prisco visited Bogotá as a mathematical logician first in 1981. Since then he visited many other times the city, giving lectures, minicourses, and in general bringing his interest in Combinatorial Set Theory to Bogotá. In more recent years he has been living in Bogotá in a more or less permanent basis, still doing logic and being part of the Logic Group, at Universidad de los Andes.

logic group in Caracas, the capital of our neighbour Venezuela, and his personal closeness to Caicedo and in general to the Logic Group of Bogotá enabled the growth and blossoming of both groups. The history of both logic groups became intertwined, to this very day. The effects of specific economic and political situations of the two countries have become part of the history of the two groups and their interactions. Other such friendships with logicians in other countries have also triggered similar developments, but the Caracas case is particularly close as an example of two different groups growing in parallel, in a symbiotic way.

1.2. The group takes off and blossoms: 1980 to now.

During the 1980s the group starts really becoming something more similar to what we have now: Caicedo guides students toward their Master's programs[1], and later some of these students return to Colombia (after their own doctoral studies abroad) and join the faculty at local universities[2].

The main venues of mathematical logic for a long time were Universidad de los Andes and Universidad Nacional. Caicedo was for many years professor at both universities. He split his research between both places. Until recently, the seminar was joint between the two universities—the number of students was growing but still small for a long time.

Slowly, some of us returned to Bogotá and joined the faculties there, and later on started directing doctoral theses as well. This

1. It is worth mentioning that doctoral programs in Mathematics only started in Colombia in 1995, first at Universidad Nacional, and a few years later also at other universities including Universidad de los Andes. This is why all those early theses in Mathematical Logic were done in the context of Master's studies.

2. Among them, Sergio Fajardo finished his Master's Thesis around 1979 and then went to the University of Wisconsin, where he worked with Keisler toward his doctoral thesis; he almost immediately returned to Colombia and was involved in research[5] and teaching until the late 1990s, when he became a prominent politician, now of national relevance.

triggered the start of research in mathematical at other universities of the city: the "hegemony" of Andes and Nacional slowly started being replaced by logic being slightly more distributed in various universities. This also started some diversifying of the topics and even of the style of research.

The Bogotá Logic group slowly went from being very much a group of former students of Xavier Caicedo, and their own students and research associates (visitors from abroad as well) and started branching to other universities and other subjects. Today, the group has members at four or five universities, at least three research seminars each semester (many of them connected with the research being done by each one of us, often in connection with other parts of mathematics). A slight price tag of this new phase has been some atomization: we no longer have a centralized seminar on a regular basis. The reasons are compounded: geographic conditions, a high degree of research activity, combined with teaching duties and often

administrative tasks by many of us. Activity in research is high but perhaps less centralized than earlier in the Group.

2. Current Trends in the Bogota Logic Group

2.1. Topics and style of current research in logic in Bogotá.

The list of topics is simultaneously very "model-theory centered" (for the historic reasons explained above) and quite turned toward collaboration with other parts of mathematics (and more recently even with mathematical physics).

Here is a brief and probably incomplete list of topics of research:

- **Abstract Model Theory:** the early theses of the group had strong connection with abstract model theory questions (infinitary logic, interpolation properties, etc.). This was of course due to the interests of Caicedo back then.

- **Model Theory of Sheaves**: a bit later, during the 1980s, Caicedo developed his Model Theory of Sheaves[3]. Generalizing earlier results due to Macintyre and Comer, he developed a natural setting for his Generic Model Theorem for sheaves of first order structures over topological spaces[1].

- **Algebraic Logic:** a later development due to Caicedo and some of his students (Oostra) and collaborators.

- **Stability Theory:** this deserves a very special place in the description of the activities of the Bogotá Logic Group in the past 15 years or so. The work of Alf Onshuus, Alex Berenstein, John Goodrick in NIP theories, geometric theories, amalgamation of types, etc. has prompted the Bogotá Logic Group to a very interesting place in the worldwide map of stability theory. The work of Onshuus (for his thesis with Scanlon) on thorn-forking has had major impact in the area.

- **Continuous Model Theory:** Berenstein first brought the topic after his postdoctoral work in Illinois, where he co-wrote an important monograph on the subject[1]. He returned to Bogotá in 2005. One of his lines of research - involving also my own collaboration (later joint with Hyttinen)[2] and later our joint doctoral student Camilo Argoty (thesis on the model theory of representations of operator algebras, 2015), has been centered in continuous model theory. It is worth mentioning that Caicedo has combined Abstract Model Theory with Continuous Model Theory in his work with José Iovino (another early student of his, now a professor in Texas).

- **Non-elementary classes:** my own work since my return from my postdoc in Jerusalem has had a major center of work around the model theory of abstract elementary classes. Some results include the effect of categoricity upon stability (an early

1. This has been generalized by Caicedo himself and other people in Bogotá to various contexts. See [6].

theorem with Shelah under extra assumptions), the study of the problem of uniqueness of limit models (with Grossberg and VanDieren) and later the work with my former student Pedro Zambrano on metric abstract elementary classes. More recently, together with Ghadernezhad, Mariano and Zambrano, a study of the connections with category theory has produced some interesting connections.

- **Combinatorial Set Theory:** as mentioned above, Carlos Di Prisco originally from Caracas had a major role in the intertwining of the two groups. Since 2012 he lives in Bogotá and has brought research in combinatorial set theory to Universidad de los Andes. Additionally, my own work early after my return to Colombia and later Ramiro de la Vega's work have had been centered in combinatorial set theory.

- **Connections between model theory and set theory:** this was my own starting point and I have always kept some active research in the field. More recently this has been revived by new results in AECs and Large Cardinals.

- **Applications outside logic:** this has happened in various ways. For a while Luis Jaime Corredor (an early student of Caicedo) worked in the large project of Groups of Finite Morley Rank. Onshuus has also worked in applications to statistics and more recently to Berkovich spaces. My own recent work is geared toward applications of sheaves and AECs to Quantum Mechanics[7].

- **Philosophy and History of Mathematics/Logic:** the work of yet another member of the Logic Group, Fernando Zalamea, in the past two decades has been strongly geared toward the Philosophy of Mathematics. Zalamea started out in logic (as a student of Caicedo and then a doctoral thesis in categorical logic from U-Massachussets) but has steadily become one of the main references in the philosophy of mathematics (studies in Peirce, Grothendieck and mainly his Synthetic Philosophy of Contemporary Mathematics). Other members of the group (recently, myself) have also worked along these lines.

2.2. Some positive points.

The paragraphs above show some healthy variety of topics, as well as some interaction with other areas of mathematics. It is worth mentioning here that the Logic Group, although started essentially by Caicedo, has never become isolated unto itself. The Bogotá group has strong connections with logicians at (at least) Carnegie Mellon University, Berkeley, Wisconsin, UIC and CUNY in the United States; Paris, Lyon, Helsinki, Barcelona and Oxford in Europe; Jerusalem, Haifa, Istanbul and more recently the IPM in Tehran in the Middle East; Kobe in Japan; in addition to the more local connections in Caracas, Mexico City, Buenos Aires, Campinas and Sao Paulo. Students have benefited enormously from frequent visits to Bogotá by members of the logic groups of those cities, and faculty (and more recently also students) have often travelled to those cities, as part of joint research.

The main positive point is the fact that, in spite of being geographically far from many centers of research, the Bogotá Logic Group is definitely not isolated in terms of research.

2.3. Some problematic issues.

A reader may notice the entire lack of some subareas of logic. A high percentage of the logic in Bogotá is model theory (pure or applied in some way), some of it is set-theory related; there is algebraic logic but there is very little else inside logic. No proof theory, very little in terms of connections with computer science. This may be a natural development of the group but I believe it would be healthy to have a more balanced logic group.

Another point to notice is the high number of faculty (mentioned above) and relatively few students. Part of the reason is that our students tend to go for doctoral programs abroad. They seem to have a strong formation in parts of logic (and in general in mathematics) and many still prefer to travel to study abroad. The lack of consistent funding at a local level is partially responsible for this. Additionally, we have very few foreign students here (although this is changing, at least at Universidad Nacional and Universidad de los Andes). We have had relatively few postdocs (two research postdocs: Sonat Suer and Zaniar Ghadernezhad in Universidad Nacional, and then a few more teaching postdocs at Universidad de los Andes). But so far the lack of a real research institute and the relative weakness of funding has delayed further growth of our logic group.

3. Academic Connections with the IPM – Perspectives

These closing lines offer some perspective in connection to my visit to the IPM in November 2015; some perceptions, some perspectives. Needless to say, a visit of a few weeks, no matter how intense it may be, offers only a very dim and general view of the place! This “blurrying” effect is made much deeper when, as was my case at the IPM, language is a barrier. My direct interaction was in English—not my original language and probably not the original language of my hosts. Some students who had questions had to ask them in Farsi, and somebody translated the interaction of questions and answers from Farsi to English to Farsi.

So, with this caveat in mind, I now offer a very candid account of impressions during the visit in 2015.

The first impression is of course the marvel of seeing a real research institute, in a good building located in a good area of Tehran, and with housing for visitors. This is something we still lack in Bogotá, as mentioned above!

I understand that people I met combine faculty (researchers), postdoctoral fellows and students. I have no idea of the proportion of each, but I had the impression that the IPM in Tehran has the necessary budget for maintaining postdoctoral fellows and keeping up with the research.

A month before my visit, there had been a major conference, with many visitors from different parts of the world. Again, this reflects something crucial, and it is the strong pull against isolation.

I have no idea of whether it is possible for non-Iranian nationals to become postdoctoral fellows. I have no idea what kind of requirements are there in that sense. My (superficial) impression is that all the people I met were Iranians, at least everyone actually living in Tehran and connected with the IPM. Of course, I may be quite wrong.

The eagerness of students to ask questions, to engage in conversation, is something wonderful in Tehran, and I have very good memory of various academic interactions after my lectures.

What is more difficult these days, for geo-political reasons, is collaboration with other countries (travel, visas). We in Colombia for a long time were also partially affected by similar things; it is only very recently that those restrictions have been reduced. I was glad to see that people do travel a lot (to countries where that is possible) academically from Iran, and that they invite different people.

In the case of the Bogotá Logic Group, the collaboration with the IPM really started with Zaniar Ghadernezhad's postdoctoral visit to Bogotá (for almost a year), right after his doctoral studies in Münster (Germany). He had impact in our local logic life (a minicourse on Hrushovski constructions, work with several of us, including our paper[4].

After my own visit in 2015, two other members of the Bogotá Logic Group have had the chance of visiting the IPM in Tehran: Camilo Argoty (who worked with Berenstein and myself in his doctoral thesis) and Darío Alejandro García (a former student of Onshuus).

There is strong potential for a continuation of these efforts. The IPM's topics of research, as far as I can see, are strongly compatible with our own topics of research in Bogotá.

An agreement of academic collaboration was signed formally between the government of Iran and Universidad Nacional de Colombia just a few months ago. It may be possible to use this to further deepen our academic ties in Mathematical Logic.

References

[1] Itaï Ben Yaacov, Alexander Berenstein, C Ward Henson, and Alexander Usvyatsov. *Model theory for metric structures*. In the Lecture Notes series of the London Mathematical Society, 2007.

[2] Alexander Berenstein, Tapani Hyttinen, and Andrés Villaveces. *Hilbert spaces with generic predicates*. 2017.

[3] Xavier Caicedo. *Lógica de los haces de estructuras*. Rev. Acad. Colomb. Cienc, 19(74):569–586, 1995.

[4] Zaniar Ghadernezhad and Andrés Villaveces. *The small index property for homogeneous models in AECs*. Proceedings of IPM Conference (to appear), 2017.

[5] H. Jerome Keisler and Sergio Fajardo. *Model Theory of Stochastic Processes*, volume 14 of Lecture Notes in Logic. Association for Symbolic Logic, 2002.

[6] Maicol A. Ochoa and Andrés Villaveces. *Sheaves of metric structures*. In Jouko Väänänen, Åsa Hirvonen, and Ruy de Queiroz, editors, Logic, Language, Information, and Computation: 23rd International Workshop, WoLLIC 2016, Puebla, Mexico, August 16-19th, 2016. Proceedings, pages 297–315, Berlin, Heidelberg, 2016. Springer Berlin Heidelberg.

[7] Maicol A. Ochoa and Andrés Villaveces. *Quantum mechanics in a metric sheaf: a model theoretic approach*. arXiv preprint arXiv:1702.08642, 2017.

THE LOGIC GROUP AT NATIONAL UNIVERSITY OF SINGAPORE: A PERSONAL VIEW

Yang Yue

I have been working in the department of mathematics at the National University of Singapore (NUS) for more than 20 years.[1] Mr. Ali Sadegh Daghighi, the editor of the Amirkabir Logic Group's special issue for 5th Annual Conference of the Iranian Association for Logic (IAL), wanted me to "introduce the logic group at the National University of Singapore" to the members of IAL and asked me to "provide a survey of activities in the Logic Group at National University of Singapore".

This request gives me both pleasure and anxiety. On the one hand, I am very happy to take this opportunity to have more communications with fellow logicians in Iran; on the other hand, it would need a major effort to write any survey satisfying the minimum standard. Therefore, what I can do is to provide some facts that I know of. It is likely to be subjective and perhaps only reflects a tiny fragment of the complete picture.

The National University of Singapore was founded in 1905. Its latest phase of expansion was in 1980, when Singapore government merged the University of Singapore and Nanyang University to form the NUS. Since then the government has been putting tremendous amount of funding and effort to improve the quality of higher education in Singapore.

One cannot describe the development of the logic group without talking about the general growth of the mathematics department and the university. Even before the merger in 1980, the university had

1. Yang Yue joined the National University of Singapore (NUS) at the end of 1992. His research area is mathematical logic, in particular, recursion theory and reverse mathematics.

systematically sent its students overseas for graduate studies and then appointed them to the faculty upon completion of the PhD. Since the 1980s, the hiring in all fields has become more international and competitive.

When I joined NUS in 1992, the logic group at NUS was already well-established. The first Southeast Asian Conference in Mathematical Logic was held at NUS in 1981, which today is recognized to be the inaugural meeting of the Asian Logic Conference series.

The mathematics department in 1992 had two logicians (in the narrow sense), Chong Chitat and Feng Qi; and as far as I know, there were previously two other logicians with the department, both of them are recursion theorists: Rodney G. Downey from 1983 to 1985 and Joseph K. Mourad from 1988 to 1992. Rod Downey moved to the Victoria University of Wellington, New Zealand[1]and Joe Mourad returned to the United States.

Chong Chitat began his career in NUS as a Lecturer in 1974. He is definitely the most influential figure in the logic group. Not only did he have a vast amount of administrative experience at all levels (department, faculty and university), he also has a unified view of mathematical logic and of its role in mathematics. He is a recursion theorist, but his research topics cover all aspects of computation: From classical Turing degree theory to higher recursion theory, fragments of arithmetic, reverse mathematics, computation theory of the reals applied to complex dynamics, and higher randomness.

Feng Qi is a set theorist whose research areas include inner model theory and large cardinals. Together with Magidor and Woodin, he introduced the notion of universally Baire sets, which is fundamental in the study of large cardinals. In 1997, Feng Qi returned to China.

1. There will be workshops in both New Zealand and Singapore in honour of Rod Downey's 60th birthday. See the following websites for more details: http://sms.victoria.ac.nz/Events/CCS2017/WebHome
http://www2.ims.nus.edu.sg/Programs/017asp/index.php

From 2007 to 2013 he held a joint position at both Chinese Academy of Sciences and NUS. His influence and experience in both China and Singapore left a strong mark on the graduate study program of logic in NUS and the Summer School at the Institute for Mathematical Sciences (IMS), both of which I will say more in a moment.

Currently, NUS has four logicians in the mathematics department: Chong Chitat, Dilip Raghavan, Frank Stephan and Yang Yue. Frank Stephan joined NUS in 2004, he is holding a joint appointment in mathematics and computer science. His research interests include recursion theory and algorithmic randomness; learning theory, in particular inductive inference; computational complexity; and automata and formal languages, in particular automatic structures. His most cited result says that every PA-complete Martin-Löf random set already computes the halting problem, which revealed the connection between random sets and their computational power.

Dilip Raghavan joined NUS in 2012. He won the Sacks Prize of Association of Symbolic Logic in 2008, which is a prize awarded for the most outstanding doctoral dissertation in mathematical logic. His research area is set theory, in particular, forcing axioms, cardinal invariance, and infinitary combinatorics.

Besides logicians in the mathematics department, there are colleagues working in logic related areas in the School of Computing and in the department of philosophy.

I should mention that there is an active research group in logic at the Nanyang Technological University (NTU), even though my article is about NUS. NTU is a much younger university in Singapore, yet it has done extremely well and has become a serious competitor to NUS. There are two excellent recursion theorists in NTU, Guohua Wu and Keng Meng Ng. We often hold logic related events together.

The research group in NUS has international collaborations with logicians all over the world. Due to historical reasons, the connection with the Chinese logic community is particularly strong. An incomplete coauthor count would include logicians (besides China and Singapore) in Australia, Canada, Germany, India, Iran, Israel, Italy, Japan, Kazakhstan, New Zealand, Russia, United Kingdom and United States.

Now I turn to the education programs of mathematical logic in NUS. In the undergraduate curriculum, we have two logic courses. One is Set Theory which covers Zermelo-Fraenkal axioms, basic facts about cardinal and ordinal numbers, and the equivalent forms and applications of the Axiom of Choice. The other is Mathematical Logic which covers the basic syntax and semantics of first-order logic and Gödel's completeness theorem.

The department of mathematics used to offer two other courses on the theory of computation which covered regular and context-free languages, Turing machines and computational complexity. These courses are now listed under the School of Computing undergraduate programme, but students in mathematics can take them without any problem.

Although NUS awarded its first PhD degree in mathematics in 1964, it was not until the 1990s that the department of mathematics set up its graduate program. Unlike countries like Iran, Singapore is a very small country having very limited academic positions. For mathematics in general and for logic in particular, it is very hard to recruit graduate students and difficult for the degree holders to find a research related job in Singapore.

Luckily, NUS and the mathematics department realized that having a solid graduate program is vital to scientific research in the university, and is an indispensable part of the "academic eco-system" within the NUS. Hence the university has been actively recruiting graduate students from all over the world.[1]

1. People who are interested in graduate studies in NUS may find more information at our website http://ww1.math.nus.edu.sg/graduates.aspx; for applicants from many countries including Iran, Singapore government has a special fellowship SINGA, see: http://www.a-star.edu.sg/singa-award/Homepage.aspx.

A typical logic graduate student will spend 4 to 5 years to complete the PhD study. The first two years are devoted to course work. Besides the basic requirements for every Ph.D candidate, such as real and complex analysis and abstract algebra, a logic student will also take the following four logic courses: Logic and Foundation of Mathematics I and II, which focus on Gödel's incompleteness theorems and the independence of Cantor's continuum hypothesis (Gödel's L and Cohen's forcing method), respectively; Recursion Theory, which covers construction techniques such as forcing in arithmetic and priority methods; and Model Theory, which focuses on Morley's categoricity theorem. The first two courses are intended for graduate students in general, and often attract mathematics graduate students outside logic and students from computer science.

After taking the courses, the candidate must pass his/her qualifying exams (written exams on topics in Analysis and Algebra; oral exams on specialized topics in logic) within the first two years. The remaining two or three years will be devoted to writing a doctorate thesis. The department also requires graduate students to conduct tutorials or grade undergraduate homework exercises. Therefore a basic proficiency in English, which is the language of instruction in NUS, is required. So far the NUS logic program has produced about 10 doctorates. Most of them have either taken up research or teaching positions at universities in China, or postdoctoral positions in Europe and North America. Currently, there are 4 graduate students in logic.

Finally I would like to talk about the IMS and its summer schools in logic. IMS was established in 2000 as a university-level research institute. Its mission is "to foster mathematical research, to nurture the growth of mathematical expertise among research scientists, to train talent for research in the mathematical sciences, and to serve as a platform for research interaction between the scientific community in Singapore and the wider international community". The IMS Graduate Summer School in Logic was started in 2006, whose objective was to "bridge the gap between a general graduate education in mathematical logic and the specific preparation necessary to do research on problems of current interest in the subject".

While a majority of the participants will be graduate students, the summer school also attracts postdoctoral scholars and researchers. From the beginning, we were fortunate to receive the support of two eminent logicians, Theodore A. Slaman and W. Hugh Woodin, both of the University of California at Berkeley[1]. Over the past ten years, they have delivered excellent lectures on recursion theory and set theory respectively. Within a series of ten lectures, they bring the students from the standard textbook material all the way to the frontier of current research. They also helped to invite other experts in all areas of logic to give lectures. We hope that the IMS logic summer school can continue to train logicians of the next generation. Since 2013, we have had Iranian students attending every year. We expect to have Iranian students this year too.[2]

I hope that the logic communities in Iran and in Singapore have more collaborations in future. I wish the 5th Annual National Conference of IAL a success.

---

1. W.Hugh Woodin is now at Harvard University.

2. The 2017 summer school is from 19 June to 7 July. The speakers are (tentatively) Artem Chernikov, Theodore Slaman and W.Hugh Woodin. See http://www2.ims.nus.edu.sg/Programs/017logicss/index.php for more details.

WOMEN IN LOGIC: WHAT, HOW & WHY

Valeria De Paiva

1. Houston, we have a problem...

The Association of Symbolic Logic (ASL) clearly states in its webpage:

> *Logic benefits when it draws from the largest and most diverse possible pool of available talent.[...] Female students and young researchers may be concerned about entering logic, where few senior women occupy visible roles. The atmosphere in classes and seminars can feel unwelcoming, and many young women have practical questions about managing a career and personal interests.*

We, just as much as the ASL, would like to add our voice to the growing list of initiatives launched by organizations in the various science, technology, engineering, philosophy and mathematics fields aimed at correcting the gender imbalance in our field.

To help on this effort, we are holding the first workshop on "Women in Logic" (WiL) as a LiCS (Logic in Computer Science) associated workshop in 2017. The workshop intends to follow the pattern of meetings such as "Women in Machine Learning" or "Women in Engineering" or "Women in Computability" that have been taking place for quite a few years. Friends and collaborators mentioned how useful these meetings were.

2. Why Women in Logic?

Women are chronically underrepresented in academia, and specially in the LiCS community; consequently they sometimes feel both conspicuous and isolated, and hence there is a risk that the under-representation is self-perpetuating. (It is notable that many intellectually-neighbouring areas of both computer science and mathematics have more women in them, leading to suspicion that the phenomenon is indeed social.) The workshop will provide an opportunity for women in the field to increase awareness of one another and one another's work, to combat the feeling of isolation. It will also provide an environment where women can present to an audience comprising mostly women, replicating the experience that most men have at most LICS meetings, and lowering the stress of the occasion; we hope that this will be particularly attractive to early-career women.

Topics of interest of this workshop include but are not limited to the usual Logic in Computer Science topics, including automata theory, automated deduction, categorical models and logics, concurrency and distributed computation, constraint programming, constructive mathematics, database theory, decision procedures, description logics, domain theory, finite model theory, formal aspects of program analysis, formal methods, foundations of computability, programming language semantics, proof theory, real-time systems, reasoning about security and privacy, rewriting, type systems and type theory, and verification, amongst others.

3. Women in Logic: the workshop

The webpage for the workshop is at:
https://sites.google.com/site/firstwomeninlogicworkshop/home

Please submit your paper! Our goal is to enhance the experience of women in logic in computer science, and thereby increase the number of women in the field, help women in logic succeed professionally, and increase the impact of women in our community.

We also want to work to increase awareness and appreciation of the achievements of women in logic and in computer science in general.

We hope our workshop will help women build their technical confidence and that this publicity effort will help ensure that women in logic and computer science and their achievements are known in the community.

4. Our Initiative

We also have a Facebook group, where we share some experiences and news:
https://www.facebook.com/groups/WomenInLogic/

and a blog:
http://womeninlogic.blogspot.com/

where we try to discuss issues that need our attention. Everyone is welcome to suggest posts for the blog and to post relevant material in the group.

PART 4

ON INTERDISCIPLINARY APPLICATIONS OF LOGIC

SEMANTICS IN LOGIC AND COGNITION

Vadim Kulikov

1. Introduction

Philosophers have tried to understand the human mind for ages. One of the central problems is meaning and mental representation. What does it mean for symbols to have meaning? Do symbols need to have it? How can symbols refer to objects in the outside world, if the agent doesn't have access to the outside world and only has access to the symbols? How can symbols refer to concepts that are not grounded in sensory perception (e.g. abstract concepts such as Hilbert space)? How can an agent judge the truth of various symbolic statements? What are representations and how are they different from symbols? What is perception? Is perception inherently meaningful or contentful? What is the difference between representation, perception and sensation?

Logic originated as the study of reasoning, but nowadays it does not seem to play a crucial role in solving these philosophical problems. Logicians talk about semantics in their work, but it is not the semantics we are interested in here, see Section 3.1. In this essay I want to suggest that logic must stop being ambivalent to these issues. I believe that logic has the potential to provide a unifying framework for dealing with questions in philosophy of mind, especially those concerned with meaning and truth.

2. Motivation

2.1 The Problem of Meaning in Cognitive Agents

Consider a robot which, at the basic level, only processes two symbols: 0 and 1. We know that our computers process only those at the core. Can such a robot ever understand that there are other entities in the world other than patterns of 0's and 1's? Can it ever understand that these patterns can refer to some other things in the external world? Can it understand that the external world exists and that 0's and 1's are merely a method to communicate information about it? Will it get to a level where it even forgets about the individual 0's and 1's and starts operating at a higher level of consciousness? It appears that humans do all this despite that at the core our brains only operate with action potentials which are essentially digital information carriers, i.e. our own brains seem to suggest that it is indeed possible. But how?

In cognitive science this is known as the symbol grounding problem and one way to briey present its history is the following. In 1974 T. Nagel argued that the nature and quality of subjective meaning and phenomenology is hardly accessible by rigorous scientific methodology [19]. This can be interpreted as saying that the meaning of a symbol is private for the interpreter and the connection between the meaning and the symbol is (scientifically) unclear (e.g. meaning of the word "love"). Further J. Searle introduced his famous Chinese room argument to show that A.I. which is based solely on symbol manipulation cannot have intrinsic meaning. This argument was further developed by Harnad who compared it to the problem of "Chinese-Chinese dictionary" for someone who doesn't know any Chinese characters [9]. This is when Harnad formulated the symbol grounding problem. The many attempts to solve it include theories of sensory-motor grounding [13], Wittgensteinian language games [24] and statistical symbol covariations [17] (which are also criticized on the grounds of the Chinese room problem). It has been argued, however, that all the existing approaches make a semantic commitment; that is, the designer or programmer inputs some meaning into the agent from outside on which the agent builds up the "autonomous" meaning, which is, alas, not autonomous anymore, because it is based on this in-built semantic commitment [5].

There is an analogous problem in the foundations of mathematics. Formulas of set theory seem to account for all of mathematics. But, as argued in Section 2.2, formulas (including, say, axioms of ZFC) by themselves are not sufficient to serve as a foundation for mathematics. One needs to explain where the meaning of the formulas comes from and the answer is trickier than merely stating that it comes from the intuition we have about sets. Even if that was the answer, it is still not explained by set theory where such intuition comes from; an explanation of this requires cognitive science. See [16] for an attempt to understand this.

2.2 The Problem of Meaning in the Set Theoretic Foundations of Mathematics

ZFC and other set theoretic foundations are often motivated by saying that (A) the concepts of “set” and “inclusion” are very easy to explain and to back-up philosophically. They are semantically simple and it is possible to establish simple truths about them by which a recursive set of meaningful axioms can be motivated; and (B) almost all mathematics can be deduced from ZFC. Therefore (C) ZFC is a tractable and solid foundation. For example let us look at some quotes from set theory text books:

This quote from Kunen's book supports (B):

> *Working within ZFC, one develops: [...] All the mathematics found in basic texts on analysis, topology, algebra, etc. [15]*

An older book by Murray Eisenberg supporting (A):

> *[One of the primary aims] is to explain systematically what the most basic and general objects of mathematics really are and why they behave as they do. [7]*

Supporting (B):

> *Indeed, all mathematical entities can be represented as sets. [7]*

A book by one of the founders of forcing, Azriel Levy, supporting (B):

> *All branches of mathematics are developed, consciously or unconsciously, in set theory or in some part of it. .... We shall show how the concepts of ordered pair, relation and function, which are so basic in mathematics, can be developed within set theory.* [1]*[18].*

Jech's book which is also among the most popular text and reference books, exceptionally talks about ZFC as a foundation only for set theory, thus supporting not-(B):

> *The axioms of ZFC are generally accepted as a correct formalisation of those principles that mathematicians apply when dealing with sets.* [2]*[12]*

Supporting (B):

> *[about the Dedekind-construction of the reals:] The arithmetisation of analysis, brought about by Dedekind, Weierstrass, and others, succeeded in developing an algebraically self-contained notion of real number without any appeal to geometric intuition.* [3]*[10]*

1. This citation strikes me as quite arrogant. Just because you can interpret all mathematics in sets does not mean that everyone who is doing mathematics is “consciously or unconsciously” doing set theory. Note that the set theoretic universe can be represented as a graph, but nobody is claiming that you are unconsciously doing graph theory when you are doing analysis.

2. My emphasis

3. I claim this is false: if it were really done "without any appeal to geometric intuition", then we wouldn't "know" that these objects are real numbers.

Again supporting (B):

> *Among the many branches of modern mathematics set theory occupies a unique place: with a few rare exceptions the entities which are studied and analysed in mathematics may be regarded as certain particular sets or classes of objects.* [1]*[25]*

Thus we can see that (A) and (B) are indeed widespreadly believed among set theorists.

I am willing to accept (A): sets indeed are very easy to understand and are philosophically simple, or at least simpler than many other mathematical notions. There are some accounts on how the intuitions and semantics of sets might be grounded in human conceptual system [16]. As of (B), however, *I claim that the reasoning is flawed.* Even granted that sets and inclusions are semantically understood and granted that we can derive simple truths about them such as featured in the ZFC axioms, we still lack the semantic foundation for other mathematics. For example the semantics of the real number line remains a mystery. The reason is that there are no real numbers in the set theoretic universe. One might be asking "So what about the construction of the real line starting from natural numbers and ending with Dedekind cuts? Doesn't this prove the existence of the real number line in the set theoretic universe?" The main worry here is that the semantics do not transfer. If we use the classical construction of the reals to transfer the semantics from the primitive concepts (sets) to them, we get to the following, mildly speaking counter-intuitive situation:

> *A real number is an infinite set of infinite sets of pairs of infinite sets of pairs of natural numbers - whatever "they" are.*

---

1. I am curious what are the "rare exceptions" to the author. He doesn't seem to explain this in the book.

What we need to do after we construct real numbers from Dedekind cuts or equivalence classes of Cauchy sequences is that we attach semantics to the created set theoretic monster from outside. The semantics or the real line is not in-built or inherent in set theory. For a more thorough discussion of this topic see [14]. If the semantics of $\mathbb{R}$ cannot be deduced from its definition (or construction) by relying to the primitive notions used in it, then how else can we obtain the semantics for it? How do we understand what is the real number line? The only possibility is that the semantics comes from outside. This in turn means that it is not sufficient to understand what a set is and what inclusion is in order to understand what real numbers are.

3. Some Ways to Approach the Problem

3.1 A Classical Logical Attempt: Tarskian Semantics?

The Tarski definition of truth (TDT) specifies conditions under which a formula in a given formal language is true. I will argue that despite its usefulness within mathematics and mathematical logic, TDT does not provide us with sufficient tools to understand semantics in general.

TDT depends on the function called the assignment which in our context can be considered the function which attaches meaning to symbols in the language. For example in the theory of graphs, the language $L = \{R\}$ consists of one binary predicate and if $G = (V, E)$ is a graph (V is the set of vertices and $E \subset V \times V$ is the edge-relation), the assignment is the function $R \mapsto R^G$ and $R^G = E$. Now from the TDT we can read that for example the sentence $\forall x \exists y (R(x, y))$ is true if and only if for all $v \in V$ there exists $w \in V$ such that v is connected to w. Without knowing the meaning of R, i.e. the value of the assignment function, this evaluation of truth would be impossible.

Moreover given the assignment, we still cannot evaluate the truth if we lack su_cient knowledge about V and E. Also notice that in this example, as usual, the vocabulary of the language is the same as the "vocabulary" of the structure. When I write "vocabulary" in quotes I mean the building blocks of the structure which a priori do not have anything to do with a language, they are set theoretic objects like n-ary relations. In TDT the sentence $\forall x \exists y(R(x,y))$ is true if and only if it is true that for all v there is $w \in V$ such that $(v;w) \in E$ and it is not surprising that students find TDT circular for how can we define truth of a sentence in terms of truth of essentially the same sentence?

To wrap up, I have identified some things which the TDT relies on:

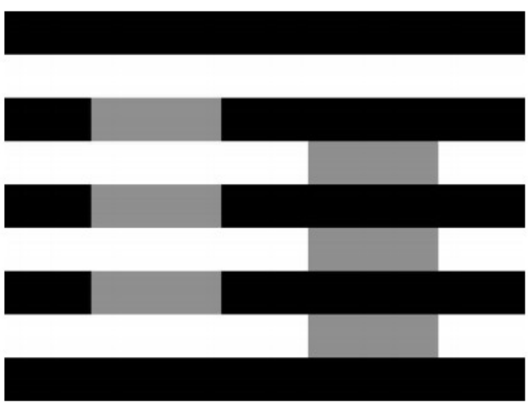
1. Semantics, realised as the assignment function,

2. The knowledge of structure of the model, realised usually as a set theoretic construction (like (V,E) where $\subset V \times V$),

3. The building blocks of the model correspond nicely to the building blocks of the language (vocabulary vs. "vocabulary").

Suppose one wanted to apply the TDT outside mathematics to metamathematics, natural language or cognition. For metamathematics an example would be motivated already by (2) above: to know whether a sentence holds in a model, you need to know something about the model. But how can you know anything about the model? The model is specified usually as a set theoretic construction, so it boils down to figuring out whether some sentence in the language of set theory holds. But if we refuse to be pure formalists and boil the latter down to syntactic provability from ZFC, we probably want to have some semantics for our language of set theory. We can now metaphorically apply the Tarski's definition of truth and say that the e.g. sentence φ = "$\exists x \forall y (y \notin x)$" is true in the language of set theory if and only if *there exists* an empty set. Now whetheror not empty set *really* exists is outside of the scope of ZFC. It is a philosophical question. One can prove φ from ZFC and use it in other proofs, but its semantics remains outside of the scope of set theory so to speak. In order to figure out its "true" truth according to this interpretation of TDT one must have knowledge about the universe of sets. By (1) we need to be able to attach "actual sets" to the symbols on the paper and also know something about them. What are these "actual sets"? The cleanest way to deal with this problem is to assume the Platonic universe of sets. The problem of (3) is now solved, if we assume that the Platonic universe of sets is based on the ∈-relation. However, the epistemic problem remains: how can we know anything about the true universe of sets? How do we even know that any sentence that we formulate in the formal language corresponds to something meaningful in the universe of sets? This is the problem posed by (1): where do we get the semantic assignment function from? It is usually assumed to be one of the defining properties of the Platonic universe, that we can meaningfully talk (prior to truth evaluation) about it with formulas of set theory, but I find it to be a separate philosophical issue here. In fact the problems with semantics in ZFC goes even deeper than this, see Section 2.2.

In natural language one could metaphorically adapt TDT and say that the sentence "snow is white" is true if snow is white. This poses several problems. It tells us what "snow is white" means, namely that snow is white, but it does not tell us what snow is white means, so ultimately it doesn't solve the problem of meaning. Is "white" even an objective property of physical objects? Can it be measured? There are reasons to claim that it cannot: it is easy to produce situations where the same wavelength produces different phenomenological colour experiences. Consider for example the White's illusion in which the grey rectangles on the left look brighter than the ones on the right [1]:



But even if we could measure is, for example if instead of colour we talked about temperature, like in the sentence "snow is less than 0 degrees Celsius", it is still not evident that we are talking about temperature in the world, because we may be just talking about the outcomes of our measurements. This addresses the problem of the domain of discourse, namely the TDT requires the well defined targets for the assignment mappings and it is not clear that we have this. But even if we have it, namely the well-defined set of references, it is still unclear how is the assignment mapping defined and how do we evaluate truths about the references. TDT only tells us how to define the truth of a sentence given that we have already defined conceptual truth of what is the case in the world.

This brings us to cognitive semantics. I can, at will, imagine that there is a tiger in my kitchen, or I can imagine my kitchen without a tiger. How is the truth value of these states, or representations, determined? The TDT approach suggests that the first is false, because there is no tiger in my kitchen and the second is true, because of the exact same reason. But this doesn't explain how do I know whether there is a tiger in the kitchen or not; how is the tiger in my imagination related to real tigers and what such a relationship means if there is no actual tiger in the vicinity; how did I form the mental representations in the _rst place and why do they seem meaningful even though the reference is lacking (there is no tiger)? Recall the thought experiment with the robot from Section 2.1. The TDT approach does not tell us how does the robot even know that there is anything other than patterns of 0's and 1's in the universe and even if it knows that, how does it attach those patterns to other things in the world, be it its own representations or actual external objects in the world.

There is another intriguing - at first glance - aspect of the TDT approach to truth. Namely the Tarski theorem about the non-definability of truth. It is sometimes taken to imply that in a cognitive system "truth" cannot be defined from within, but must be coming from outside, thereby killing the research objective of this paper. I claim that this argument, however, is missing the point. Suppose first that we were interested in defining the truth only for formulas of quantifier rank < 1000, or maybe even only for formulas with bounded quantifiers and maybe even bounded by some large natural number. Defining truth for these formulas would be, for our practical purposes, enough. The problem, however, is not whether we can define truth in principle or not. The problem is that even given a formula that "defines truth", how do we know that this is the truth? Note that the truth is "defined" as a subset of natural numbers (which requires semantics in the first place) and it requires an external observer to interpret the numbers as formulas and check that they indeed correspond to true formulas and we get back to the problem with semantics in metamathematics.

To conclude, the Tarski definition of truth is a useful mathematical tool to produce invariants and to mathematically talk about formal truth. But its philosophy hardly extends beyond mathematical practice.

3.2 Semantic and Non-semantic Information: is there a difference?

A theory of meaning is outlined by Eliasmith in [8]. In this theory, information is as understood in information theory through measures of covariation such as Shannon entropy. This theory proposes that any variable x in the nervous system is a representation of some variable y in the world if and only if x and y stand in a covariate relationship with each other which could be measured for example as the conditional Shannon entropy of y given x. The problems of understanding meaning and semantics through Shannon information are numerous. For example if I have a dog allergy, then itching in my nose maybe in a covariate relationship with presence of a dog and so it is, according to this theory, a representation of a dog. Itching of a nose, namely, is certainly accompanied with specific neurological activity as well, thus fitting the definition of neurosemantics. Another problem is that in order to know whether your variables are covarying with something in the world, you need to know what is in the world, which makes the problem similar to that of Tarskian semantics. It works only if you assume that you have access to some "objective" parameters in the world.

A completely different point of view is presented by the school of enactivists, especially Daniel Hutto and Erik Myin, [11]. They make a distinction between information that is merely carried as a pattern of covariation and information that has content. Content, according to Hutto, is constituted by conditions of satisfaction which may or may not be met. If I am imagining a tiger in my kitchen, this thought has content whether or not it stands in covariate relationship with the world, i.e. whether or not there is an actual tiger in my kitchen or not, because this thought constitutes conditions of covariation.

Many other philosophers have proposed that there is a difference between semantically contentful information and mere information as understood in information theory, see e.g. [6].

3.3 Grounding Meaning in Sensorimotor Contingencies

A common line of reasoning in the embodied cognition paradigm is that all or most cognition is grounded in the sensorimotor (SM) domain. Thus, Barsalou and colleagues argue [3, 2, 13, 4] that the meanings of concepts and words can be reduced to bodily sensations, actions and their combinations. Similarly Alva Noe and his colleagues argue [22, 20] that perception and mental representations are the result of SM contingencies (in fact they propose to replace the notion of representation by the one of SM contingencies).

There are various problems here, especially when we want to ground meaning in the SM-domain. Recall the robot thought experiment described in Section 2.1. The SM domain of the robot consists of patterns of 0's and 1's. If it were to ground more abstract meaning in the SM-domain, it would require that there is some meaning in the SM-domain. Otherwise how can you ground meaning in something that is inherently meaningless? It becomes the same problem as Harnad called the "Chinese-Chinese dictionary" problem (see Section 2.1). Or in this case, I would re-name it to "Chinese-Malaysian dictionary" problem, because the type of symbols is different, but there is still the problem of semantics.

Another problem is that this theory doesn't really explain abstract meaning. In the same way as the meaning of "real numbers" is not really grounded in the axioms of ZFC (even though you can construct an isomorphic copy within ZFC, see Section 2.2), the meaning of "real numbers", or lets say uncountable cardinals, is not grounded in the SM-domain even if there is a conceivable story on how (sometimes major) parts of intuition can be traced back to the SM-domain.

3.4 Grounding Meaning Through Metaphors

To attack the problem of abstract meaning, some cognitive scientists have resorted not to sensorimotor contingencies, but to the human mind's ability to treat metaphors and map content in metaphoric ways from one domain to another. In this view an infinite set, for instance, can be understood as the result of a potentially infinite process, where the "result of a process" is a applied in a metaphoric way from a domain where processes actually end [23, 16].

4. Questions for Logicians

These are not mathematical questions but rather open ended philosophical questions whose anticipated solution lies in proper formalisation of mathematical modelling.

> **1.** One of the benefits of the Tarski definition of truth is that it formalises the distinction between syntax and semantics. Can we find a formalisation of the distinction between information in the Shannon sense and semantic information as described in Section 3.2?
>
> **2.** Can one formalise the "sensorimotor grounding" as a mathematical model which would show how meaning in one domain can emerge from meaning (or even from meaningless pattern covariations) in another domain?
>
> **3.** Can one formalise various models proposed in [23, 16, 21]: creating new concepts through metaphor blending?

4. Can it be that the abstract or formal models that solve the above two questions are somehow equivalent? Can these two approaches be special cases of a unifying logical framework to understand meaning? This would give an elegant account of both concrete and abstract meaning under one unifying framework.

5. Apply a theory of meaning, either existing one or one that is inspired by the above questions, to create either a new foundation of mathematics, or complement the set theoretic foundations with the explanation of how does the semantics arise, see Section 2.2.

References:

[1] E. H. Adelson. *Lightness perception and lightness illusions*. In M. Gazzaniga, editor, The New Cognitive Neurosciences, chapter 24, pages 339-351. Cambridge, Massachusetts: MIT Press, 2000.

[2] L. W. Barsalou. *Perceptual symbol systems*. Behav Brain Sci., 22(4):577-609, 1999.

[3] L. W. Barsalou. *Grounded cognition*. Annu. Rev. Psychol., 59:617-645, 2008.

[4] Lawrence W Barsalou. *On staying grounded and avoiding quixotic dead ends*. Psychonomic bulletin & review, pages 1-21, 2016.

[5] R. Cubek, W. Ertel, and G. Palm. *A critical review on the symbol grounding problem as an issue of autonomous agents*. In KI 2015: Advances in Artificial Intelligence, pages 256-263. Springer International Publishing, 2015.

[6] F.I. Dretske. *Knowledge and the Flow of Information*. The David Hume Series. Cambridge University Press, 1999.

[7] M. Eisenberg. *Axiomatic theory of sets and classes*. Holt, Rinehart and Winston, 1971.

[8] Chris Eliasmith. *Neurosemantics and categories*. Handbook of categorization in cognitive science, 1035:1054, 2005.

[9] Stevan Harnad. *The symbol grounding problem*. Physica D: Nonlinear Phenomena, 42:335-346, 1990.

[10] W.S. Hatcher and M. Bunge. *The Logical Foundations of Mathematics: Foundations and Philosophy of Science and Technology Series*. Foundations & philosophy of science & technology. Elsevier Science, 2014 (first edition 1982).

[11] Daniel D Hutto and Erik Myin. *Radicalizing enactivism: Basic minds without content*. MIT Press, 2012.

[12] T. Jech. *Set Theory*. Springer-Verlag Berlin Heidelberg New York, 2003.

[13] M. Kiefer and L. W. Barsalou. *Grounding the human conceptual system in perception, action, and internal states*. In W. Prinz, M. Beisert, and A. Herwig, editors, Action science: Foundations of an emerging discipline, pages 381-407. Cambridge, Massachusetts: MIT Press., 2013.

[14] V. Kulikov. *The problem of semantics in set theoretic foundations of mathematics*. 2017. to appear.

[15] K. Kunen. *Set Theory: An Introduction to Independence Proofs*. North Holland Publishing Company, 1980.

[16] George Lakoff and Rafael E Nunez. *Where mathematics comes from: How the embodied mind brings mathematics into being*. Basic books, 2000.

[17] T. Landauer, P. Foltz, and D. Laham. *Introduction to latent semantic analysis. discourse processes*. Behav Brain Sci., 25:259-284, 1998.

[18] Azriel Levy. *Basic set theory*. Courier Corporation, 2012.

[19] T. Nagel. *What is it like to be a bat?* The Philosophical Review, 83(4):435 -450, 1974.

[20] A. Noe. *Action in Perception. A Bradford book*. Bradford book, 2004.

[21] Rafael E Nunez. *Creating mathematical infinities: Metaphor, blending, and the beauty of transfinite cardinals*. Journal of Pragmatics, 37(10):1717-1741, 2005.

[22] J Kevin O'Regan and Alva Noe. *A sensorimotor account of vision and visual consciousness*. Behavioral and brain sciences, 24(05):939-973, 2001.

[23] Markus Pantsar. *In search of $\aleph_0$: how infinity can be created*. Synthese, 192(8):2489-2511, 2015.

[24] L. Steels. *Grounding language through evolutionary language games*. In L. Steels and M. Hild, editors, Language Grounding in Robot, pages 1-22. New York: Springer, 2013.

[25] Patrick Suppes. *Axiomatic set theory*. Courier Corporation, 1960.

LOGICAL METHODS FOR THE VERIFICATION OF SOFTWARE MODELS

Magdalena Widl

The challenge of dealing with the increasing complexity of software systems has been anticipated as early as in Frederick Brooks' 1987 seminal paper [3]. Soon after, David Harel responded to this challenge by underlining the importance of visual programming formalisms to mitigate this complexity. Indeed, today, visualization of software is almost omnipresent. Software models are used for many purposes such as requirements engineering, specification, communication, documentation, and, most recently, to automatically derive executable code within the new software development paradigm of model-driven engineering (MDE) [5, 12].

Usually, multi-view models are employed. Such models comprise a set of diagrams where each diagram describes a different view on the system, altogether providing a holistic representation. The most popular and widely-used modeling language for this purpose is the Unified Modeling Language (UML) [2], a standard that was established in 1997 out of a myriad of different modeling languages that were in use back then. Since the main applications of the UML were mostly informal, not much effort was spent for its formalization and so its semantics is still mainly specified in natural English language [7].

However, the shift from code-centric development to MDE requires a modeling language based on a solid formal semantics - a necessity, that is now considered one of the major current challenges in MDE research and in future improvements of the UML [4, 6]. A way to establish a formal semantics is to express properties of the modeling language in a logical formalism. Using a well-known formalism for this task has the immediate advantage that it makes the modeling language accessible for a wide range of users without the requirement of learning a new formalism. We previously used propositional logic to describe the semantics of a modeling language [9, 13].

Further, the software models' new role as core development artifacts brings along stronger requirements on their consistency since errors introduced on the model level in the worst case result in faulty code. They are also central to the evolution of a software system, where they undergo continuous and often parallel modifications which are prone to cause inconsistencies between the views. Due to the size of the models, the consistency management is too cumbersome to be done manually [12]. Hence, automated methods are required [6]. Here, a second advantage of expressing the model semantics in a well-known logic formalism comes into play: it allows to apply off-the-shelf solvers to detect and resolve consistency problems in the models. Community efforts in improving solver performance directly impact the solving speed for the consistency problem.

We previously applied propositional logic solve consistency problems of software models [8, 10, 9, 13]. In particular, we dealt with state machines and sequence diagrams. State machines model the behavior of a system in a reactive way. They respond to events, for example to the receipt of a message, by changing their states, and they create events in order to trigger other state machines.

Sequence diagrams model a sequence of messages that are received and sent by state machines. A set of state machines may or may not implement such a sequence of messages. A set of sequence diagrams can be seen as a specification of a system and a set of state machines as the implementation of the system. The problems we dealt with were related to the question whether a set of state machines indeed implements the behavior modeled in a set of sequence diagrams, i.e., whether the two views are consistent. We showed all problems to be solvable in nondeterministic polynomial time (NP). Since we had already expressed the semantics of these models in propositional logic, we also encoded their consistency problems to the satisfiability problem of propositional logic (SAT) and used an off-the-shelf SAT solver to find solutions.

In a future project, we plan to deal with the question of how to ensure model consistency in a similar way. However, we will deal with extended modeling concepts and go beyond mere consistency checking by proposing approaches for model synthesis and model repair. In particular, a question highly relevant in the practice of MDE is how to synthesize a system from a set of specifications, i.e., a system that is correct by construction. Again, the specification can be represented as a set of sequence diagrams and the system as a set of state machines. Similarly, a relevant question is how to repair a system that is inconsistent with a specification.

These problems are likely to have a computational complexity that is above NP and hence, SAT-based approaches will not suffice due to the formula size of the encodings. A solution to this provide quantified Boolean formulas (QBFs), an extension of propositional formulas that allows to quantify propositional variables, or satisfiability modulo theories (SMT), an extension of propositional formulas by theories containing different fragments of first-order logic. Both approaches allow to compactly encode problems of complexity above NP. In particular, QBFs can be used for problems of different levels of the polynomial hierarchy (PH) [11] and SMT for problems of different complexites depending on the theory [1].

References

[1] C. Barrett, R. Sebastiani, S. A. Seshia, and C. Tinelli. *Handbook of Satisfiability, chapter Satisfiability Modulo Theories*. Oxford University Press, 2009.

[2] J. Bezivin. *On the Unification Power of Models*. Software & Systems Modeling, 4(2):171-188, 2005.

[3] F. P. Brooks Jr. No Silver Bullet: *Essence and Accidents of Software Engineering*. IEEE Computer, 20(4):10-19, 1987.

[4] B. R. Bryant, J. Gray, M. Mernik, P. J. Clarke, R. B. France, and G. Karsai. *Challenges and directions in formalizing the semantics of modeling languages*. Computer Science and Information Systems, 8(2):225-253, 2011.

[5] R. B. France, S. Ghosh, T. Dinh-Trong, and A. Solberg. *Model-driven development using UML 2.0: promises and pitfalls*. IEEE Computer, 39(2):59-66, 2006.

[6] R. B. France and B. Rumpe. *Model-driven development of complex software: A research roadmap*. In *Future of Software Engineering*, pages 37-54. IEEE Computer Society, 2007.

[7] O. M. Group. *Specification of the unified modeling language (OMG UML)*. http://www.omg.org/spec/UML/, August 2011. retrieved Aug 26, 2014.

[8] P. Kaufmann, M. Kronegger, A. Pfandler, M. Seidl, and M. Widl. *Global state checker: Towards SAT-based reachability analysis of communicating state machines*. In *10th Workshop on Model-Driven Engineering, Verification, and Validation*, number 1069 in CEUR Workshop Proceedings, pages 31-40, 2013.

[9] P. Kaufmann, M. Kronegger, A. Pfandler, M. Seidl, and M. Widl. *Intra- and interdiagram consistency checking of behavioral multiview models*. Computer Languages, Systems & Structures,44:72-88, 2015.

[10] P. Kaufmann, M. Kronegger, A. Pfandler, M. Seidl, and M. Widl. *A SAT-based debugging tool for state machines and sequence diagrams*. In Software Language Engineering, volume 8706 of *LNCS*, pages 21-40. Springer, 2015.

[11] C. H. Papadimitriou. *Computational complexity*. Addison-Wesley, 1994.

[12] B. Selic. *What will it take? a view on adoption of model-based methods in practice*. Software and System Modeling, 11(4):513-526, 2012.

[13] M. Widl. *Symbolic Methods for the Verification of Software Models*. PhD thesis, Vienna University of Technology, 2016. http://crespo-widl.at/magdalena/publications/dissertation.pdf.

SET THEORY FOR POETS, POETRY FOR SET THEORISTS[1]

William Flesch

One modern incarnation of the debate between nominalism and realism is to be found in philosophical arguments about sets. There are two ways of characterizing a set: *intensionally*, through description (e.g. the set of all inhabitants of London, to use an example of Russell's), and *extensionally*, which is just a list of the members of the set.

Quine, as nominalist as they come, objected to the "ontological excesses of set theory" when construed intensionally. Is there really such an *entity* as "all the inhabitants of London"? Yes, there are inhabitants, and we, or God, or Facebook could list them. Each is an entity him- or herself (let's stipulate, because who wouldn't?)

The problem with extensional sets is that the vast, the utterly overwhelming majority of them would be utterly random, by our lights, like the contents of almost any book in Borges's "Library of Babel." Those books are all (*à très peu d'exceptions près*) useless, and so too, more or less, would be thinking about things in sets. The problem with intensional sets is that they may not exist (what is a set and where do I find one?), and even if some do exist, others might turn out to be impossible, despite seemingly innocuous descriptive criteria for membership.

Nevertheless, set theory is not only obviously useful: it's obviously a way that people think about the world and make sense of it (or it's a formalization of how we think and make sense of the world). "Natural kinds" for example really do rely on a concept of nature not unlike the nature that we live in, that we evolved to survive in. And it seems too

1. The present article is available on the following website: http://arcade.stanford.edu/blogs/set-theory-poets-poetry-set-theorists

that we find pleasure in finding sets, or figuring out what intensionally-characterized (or -characterizable) sets seemingly random extensional lists belong to.

Just to reiterate: *intensional* is more or less synonymous with *interesting.* To characterize a set intensionally is to say that its members share some interesting property—interesting enough that you don't have to list them.

But here I want to focus on the converse idea as part of human literary or cultural play (as well as work): figuring out from a list what interesting set would embrace the items on that list. It's true, of course, that a vast number of different interesting sets might embrace them, so we might want some further criteria of economy (this is also how Freud thinks about mental economy) for what the really interesting set is. (That kind of economy is something like the criterion for a natural kind, and also for Wittgenstein's ideas about rule-following, which is for another post.) The criteria would not necessarily be pure efficiency, but a balance between *specificity* and pith. Pithy specificity is what we're looking for, and we'll know it when we see it.

Example: {raven, writing desk}.

Now we're not *really* asking about this set itself. We're asking about the set it's a subset of, but we're still looking for a pretty small set. So *items whose names in English start with the phoneme /r/* won't cut it. Nor, probably will *nouns with the letter n*, nor *objects smaller than an elephant*, nor *things that don't taste like rhubarb*. They both belong to those sets, yes, and to many others too, but still.

The two terms are, as every school child will remember, from a riddle by Lewis Carroll, which the Mad Hatter asks Alice. He gives no answer, but later Carroll was prevailed upon to solve it. He wrote:

> *Enquiries have been so often addressed to me, as to whether any answer to the Hatter's Riddle can be*

> *imagined, that I may as well put on record here what seems to me to be a fairly appropriate answer, viz: 'Because it can produce a few notes, tho they are very flat; and it is nevar put with the wrong end in front!'*

This, however, is merely an afterthought; the Riddle, as originally invented, had no answer at all. As originally invented, then, it was offered as pure extension.

Now other writers offered later answers. Martin Gardner and The Straight Dope[1] give some of the best, e.g., Poe wrote on both (Sam Loyd). (Cecil Adams of *The Straight Dope* also explains the misspelling *nevar*: it's a palindromic raven.)

So the pleasure of riddles, of this kind of riddle, is the sudden collapse of extension into intension. Sometimes that will require a reconceptualization of the elements in the extension: not "What's black and white and red all over?" no, but "What's black and white and *read all over?"* The extension turns out to be the following set of qualities, denotable by adjectives and adjectival phrases: {black, white, read all over}.

What does this have to do with poetry? Well, in English, anyhow, rhymes are to be distinguished from inflections. We don't (really) count *unity* and *disunity* as a rhyme; *motion* and *emotion* are too close to each other. As Wimsatt argues, the best rhymes will tend to be different parts of speech, and, as Empson points out, the fact that singular verbs but plural nouns end with -s means that we can't generally or easily rhyme subjects with predicates. So rhyming words tend to be arbitrarily connected.

Consider the set **R** = {Mahatma Gandhi, the Coliseum, the time of the Derby winner, the melody from a symphony by Strauss, a Shakespeare sonnet, Garbo's salary, cellophane, Mickey Mouse, the

1. See here: http://www.straightdope.com/columns/read/1173/why-is-a-raven-like-a-writing-desk

Nile,..., Camembert}. Extensionally there's nothing unusual about it, even if it is, as the kids say, "kind of random." Not *that* random though: these all belong to a somewhat larger set of words that can be formed into subsets consisting of rhymed pairs, e.g. {the melody of a symphony by Strauss, Mickey Mouse}. Rhyming with a member of some smaller set is the principle of inclusion in the somewhat larger set.

Or to put it another way, rhyming provides a principle of one-to-one correspondence between two sets of entities whose names have at least one rhyme. That's not how I'm defining those sets: that's how I'm characterizing one of many facts about their members. So the set **R** (whose membership I haven't fully listed) is the union of those two sets that are in one-to-one correspondence.

Now that principle, as we've seen, tends to be highly arbitrary in English. The rhyming dictionary is disconcertingly senseless. But what a poet does, like a riddler, is to find some intensional principle which defines a set given randomly and extensionally. In this case that principle is that each member of the set **R** is a member of the set {*things that are the top*} (I am simplifying the song a little bit to make my point).

Now this distinction between intension and extension is also a distinction between use and mention. The principle of membership of the two sets whose union forms **R** is *first of all*, that is to say, *as a matter of poetic craft*, a principle which mentions terms, i.e. selects them for the fact that they rhyme. (The rhyming dictionary mentions words: it doesn't use them.) But the job of the poet is to take these mentioned words and *use* them, which means to say something with them and therefore something about the things they signify or refer to.

The solution isn't just economical (as it is with a riddle), isn't just the sudden lifting of a burden through the sudden glory of an elegant summary of its components. We shunt back and forth between use and mention, intension and extension, admiring at every moment how they fit together: look it rhymes! look, it's the top!

Studies (e.g. by Ray Jackendoff) of the neural handling of music suggest that different parts of the brain have different access to memory. Some of the cerebral material we use to process music chunks and forgets immediately, so when a theme or motif is played again, it handles it as entirely new. But other parts of the brain remember that motif or theme, and therefore experience a different relation to the novelty that is still being felt and processed. That back and forth, that counterpoint, that complex and differently phased experience of music *is* the experience of music, or at least a large part of it.

I think the same is true about rhyming (and meter), especially since it appears that music actually recruits the cerebral material that processes sounds: vowels are much lower pitched than consonants, and we put words together from sounds much as we put musical experience together. So I think that we go back and forth, sometimes putting together the longer-term, more coherent intensional sense of the set of rhymes we're given and sometimes testing the always novel extension of the list, and that the delight in doing so is how the abstract distinctions to be found in set theory play out in the pleasures of poetry, and of math. (At least that's what struck me today.)

PART 5

ON LOGIC BOOKS AND WEBSITES

CLARIFYING THE FOUNDATIONS: AN INTRODUCTION TO THE SITE SETTHEORY.NET

Sylvain Poirier

1. Introduction[1]

I am a math PhD from France[2], and develop the site settheory.net to clarify the foundations of math and physics, by self-contained undergraduate level courses combining depth, generality, rigor and concision, involving many simplified but accurate presentations of concepts usually considered graduate level. I do it for free outside any institution.

The settheory.net aims to:

(1) Rebuild mathematics with (almost) no prerequisites.

(2) Introduce the fundamental theories of physics (Relativity and quantum physics)

It may be situated *in between undergraduate and graduate levels*: by its way of rebuilding everything from the start, it aims to fill the role of (a subset in progress of) an undergraduate curriculum (1st university year), but the difficulty level with the care for powerful methods and deep and complete explanations, is comparable to graduate level (3rd year). Also, it takes some of its inspiration and

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1. In February 2017, I wrote this long introduction to the contents of this site and relations between logic and physics, for the special issue of the Amirkabir Logic Group in celeberation of 5th Annual Conference of the Iranian Association for Logic at Amirkabir University of Technology.

2. I entered in thesis in September 97 to the Fourier Institute of Grenoble, with Christine Lescop. My defence took place on January 28, 2000. Its subject is "*The configuration space integral for links and tangles in* $\mathbb{R}^3$".

concepts from existing graduate-level mathematics (in a simplified form).

It is thus mainly targeted at**:**

(1) Clever undergraduate students with genuine interests in mathematics or physics (rather than focused on preparing for exams);

(2) Teachers of mathematics or physics looking for deeper understandings of their subject;

(3) Any people trying to learn science outside the academic system.

It does not aim (nor exclude) to bring new ideas, results or theories (with few exceptions: the set generation principle; the page "Time in set theory" - interpretations of quantum physics), but focuses on developing new optimized paths to already known fundamental theories of mathematics and physics: putting things in the right order, to make high concepts look as clear, simple and fast to learn as possible. It combines the following advantages:

(1) To be as rigorous as possible (everything is explained, all what is provable is proven from the beginning)

(2) To give powerful and general concepts and tools, avoiding any unnecessary lengths. Rigor will not dilute or obscure the meaning. Proofs are usually very short. Pages are very dense, with many ideas per page to explain everything. The right formalism will express the highest concentration of meaning.

(3) Original (cleaned up and restructured) approaches are given to most subjects.

(4) Emphasis is placed on intuition, deep and "philosophical" explanations, the main foundational concepts and paradoxes, the mathematical world.

The set theory formalism presented here differs from the traditional ZF system, but aims to more directly fit the common use and notations of mathematics (without artificial encoding and abbreviation systems), and be cleaner on foundation and meaning.

2. Leaving Academic "Research" Behind!

I was always interested in mathematics, particularly algebra and geometry (especially in my youth). More generally, I am interested in global, foundational issues in diverse fields and the search for perfect theoretical solutions. During my teenage, I was very unsatisfied with the official teaching, which was an intellectual desert for me. I managed to learn Special Relativity and then express General Relativity by my own research. My dream was to become a particle physicist, as I naturally guessed that the fundamental laws of physics were rich of some of the most wonderful mathematical theories. But things turned out very differently, for many reasons.

A first trouble was the low level of the teaching curriculum I went through, which I so direly needed to escape in my free time to explore theoretical physics far away from it, an exploration I thus did alone outside any academic guidance. But people around almost objected to this exploration, insisting that it wasn't time for me to explore the high skies of theoretical physics because I first needed to more fully assimilate the basic concepts on which higher theories can be built. In fact there is some truth in this idea, but in a very different way. It is not that I shouldn't have gone to theoretical physics at the beginning because I needed the help of schools (which I hated) to first teach me the right basis, but that I'd rather not continue to further theoretical physics at the end because schools (precisely: undergraduate curricula) actually don't provide the right basis for this, so that my help is needed to explain how to do it (but away from the system, which isn't welcoming any such proposition of progress).

As in high school I was isolated in lack of hints for going further (to quantum physics...), I spent time re-thinking and finding ways to progressively clarify the same theories (general relativity and electromagnetism). Still it wasn't enough, so I also thought about metaphysics (absurdity of the AI thesis which reduces the mind to a mathematical object), and economics (foundations of money and its stabilization problem; what problems occur in the world and which logical structures could ideally resolve them).

At one point of my studies (in ENS Ulm), someone warned me that particle physics was actually a terrible field to work in. Following that advice I didn't continue far enough to fully verify this claim by myself, however I see indirect reasons for this to be true[1]. One is the mess of divergences that the fundamental equations of quantum field theory are plagued of, making it unclear whether any possibility of a rigorous mathematical approach should reasonably be expected at all. Another is, that many of the brightest minds from around the world are already working on the problem; so, what could just one more bright mind add to that, unless he'd have both incredible lucks of being somehow "the brightest", and that Nature would have chosen the ultimate laws of physics (or something amazing in them) exactly appropriate to be only discovered by the brightest physicist but not by others without him ? Wouldn't it be both more polite and cheaper in efforts, in case an amazing new discovery was just ahead of us, to leave the honor of making it to someone else (who would need it, for some reason I don't fully grasp but...), and simply later learn about it if really interesting ? The golden age of physics research, quickly grabbing lots of low hanging opportunities of discoveries, which Nature had available for us, seems to be past. After just a few decades of rapid advances (that is the blink of an eye compared to the long history of life on Earth), wonderfully explaining most of the physical phenomena that could be experimented, why did this quest have to suddenly stall without being complete ? God knows ; anyway we must cope with it.

Generally, I was quite disappointed with the academic environment of mathematical research I experienced during my PhD (in algebraic topology : Vassiliev invariants constructed from the

1. http://settheory.net/crackpot-physics

perturbative expansion of Chern-Simons topological quantum field theory[1], (a topic I abandoned since then). The main group activities (what makes a life in official positions and labs differ from just staying home doing free personal research) were regular seminars where people reported their boring ideas to others who anyway don't care and cannot use in their own research (as they don't work in the same topic) but who anyway feel obliged to come and pretend listening to just for the sake of politeness (I was once scolded for not coming, since "being polite" in this way visibly mattered more than any meaningfulness of time spent). Indeed, why would the ideas of whoever happens to work in the same lab be worthier hearing than those of any other people working elsewhere on Earth ? that is something I couldn't figure out. I also several times heard of stuff like HOMFLY having multiple independent co-discoverers. Why bother researching and discovering something, just to end up being one of several independent discoverers of the same thing ? Even if institutions were okay to pay me for this, it didn't feel to me like the best way of giving sense to my life.

Other factors drove me away from academic research, which I left for good after one year teaching as assistant professor of mathematics at Reunion university. It would still take me quite a long further study to catch up existing works until I could produce valuable new results myself in great fields such as particle physics (I had the chance to manage my PhD by picking a topic with not so great value to me, but wouldn't see what more to do there), while I was already exhausted by the wasteful academic path I previously followed with disgust as it wasn't done the way I believed to be needed. I also felt that the kind of popular hard unsolved problems which scientists usually work on, aren't often the most valuable ones:

> (1) The popularity of a topic cannot define its real value: the correlation may rather be negative, as popularity indicates that its possible low hanging fruits have been already picked;

1. http://spoirier.lautre.net/thesis.html

(2) I once read a report of a result of complexity theory that "hard theorems are useless" but lost the reference; anyway the point is that things appeared this way to me in practice.

Generally, it seems too many people focus their works on tackling some of the hardest questions, a bit like Olympic champions training their strength for the purpose of demonstrating how strong they are, but are not so good at asking the right questions (and I do not see academic philosophers better at this either despite their claims). A typical example is Bitcoin[1]: a wastefully sophisticated answer to the wrong questions about how a good online currency should work. Hardly any of its proponents seems to have seriously wondered for instance how the value of a currency could be stabilized, or even understood the fundamental importance of this question.

Also, institutions have fundamental flaws such as the Peter principle (workers are tendentially raised to their "level of incompetence"). As I want to make an optimal use of my work for the world, I rather search for the right questions or tasks, where I can produce the most valuable works not because I'd be the most clever, but rather because these are crucially important questions on which, strangely, no other good thinker seems to be working yet. But such a radical form of originality, of choosing a research topic or other working direction whose potential value is ignored by the rest of the world, also makes unlikely to find any job open for this in any institution (since job openings are set by administrators more often looking for security than originality, while, logically, widely unexpected discoveries are hard to expect). I explained more aspects of my reasons for leaving the academic system and why it was a bad idea to join it in the first place, in my video “Why learn physics by yourself” [2].

3. From Physics to Mathematical Logic

I was initiated to mathematical logic with amazement during my graduate studies (Magistère, ENS Ulm). It took me some maturation

1. http://settheory.net/future/Bitcoin
2. http://www.settheory.net/settheory.net/learnphysics

time until I could be productive there. Like in physics, I focused on simpler aspects, for which I could be directly useful: clarifying the basics (so, the foundation of the foundation). Things which many students have to go through but in need of clarification (to be done once and for all), usually neglected by other researchers who focus on more complex, high level issues with their hard open problems. But, as long as researchers only care to do research for the sake of "doing research", their productions are likely to stay desperately hidden in their ivory tower, useless to anyone who isn't yet already another researcher in the same field, having wasted many years of their life following traditional curricula followed by the other lengthy wasteful initiation path to that particular field of specialization.

Among the people to whom an initiation to mathematical logic could benefit, but who currently cannot afford it because of the wasteful complexity of its teaching path, there are... physicists who have no time for this long path because they already wasted too many years of their life with their own messy teaching curriculum. Since, what made them waste so much time in their own curriculum, is... their weakness in mathematical logic. But what could be the use of mathematical logic in physics, you may ask ? It is of course that the laws of physics are deeply mathematical[1], as reported by many great minds. Between the first guesses by Plato and Pythagoras, and some recent reports such as Wigner's "unreasonable efficiency of mathematics" (confirmed by Hamming in 1980), a remarkable short formulation is the one by Galileo:

> *Philosophy [i.e. physics] is written in this grand book — I mean the universe — which stands continually open to our gaze, but it cannot be understood unless one first learns to comprehend the language and interpret the characters in which it is written. It is written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to*

1. http://www.settheory.net/fqxi

understand a single word of it; without these, one is wandering around in a dark labyrinth.

But, once confirmed that physics is indeed written in the language of mathematics, we still need to specify what this language of mathematics actually is, for better clarifying our understanding of physics. Precisely, I found 3 interesting connections between physics and mathematical logic. A first one is a lesson which logicians have ready to teach to physicists. The second is an interesting formalism of mathematical physics usually ignored by logicians but in need of logical clarification for teaching. A third (but debatable) one is a possible lesson from logic for philosophers of physics.

First is the simple issue of what a theory is[1]. Physics has its list of theories[2]; mathematical logic has precise general concepts of how a mathematical theory can be formed[3] (as can be defined by either first-order logic or second-order logic, but their differences do not matter for physics), but strangely, physics teachers never seemed to care whether any match between these should be made and how. And which interesting lesson can logic have to say to physics teachers about what a theory is? That *each theory comes with its own language*: the language in which it is expressed, made of a list of names of the structures[4] of the system which the theory describes. *Then*, we may introduce and study the automorphism group[5] of the system, made of all transformations which preserve these structures.

But physicists took completely different habits: instead of this, they just express any of their theories in always the same language (the only one they know, as if it was the only possible language of mathematics): the language of packs of real numbers and packs of operations. Then, among their packs of operations they choose some specific ones: some *transformation groups*. And only at the end of

1. http://settheory.net/foundations/metamathematics
2. http://settheory.net/physics-theories
3. http://settheory.net/foundations/theories
4. http://settheory.net/foundations/structures
5. http://settheory.net/automorphism

this, they investigate which of the many possible operations have a special property : the property of being *invariant* by the given transformations (which they may name by some more sophisticated adjectives such as "covariant" to make it look like an amazingly intelligent quality, no matter if students cannot decipher what it may really mean). But did physicists consider that *in logic*, these invariant or "covariant" structures are those which come first and are actually the *only* structures which exist in the language of the intended theory, in terms of which the theory naturally ought to be expressed? Did they ever consider that they might have just wasted a lot of time and obscured the understanding of the theory in the students minds, every time they expressed something in formulas formed of variables and operations which don't already belong to this short list of invariant stuff ? Do I need to explain why such a way of "mathematically expressing theories" can be a terrible one, making the physics teachings much messier and harder to follow than actually necessary ? (I was recently censored from physicsforums.com [1]just for the sin of defending this view, that there exists such a thing as a mathematical conceptualization, and that a explanation's quality of "being mathematical" cannot be reduced to how numerically accurate it is ! Namely, I was denied the right to reply to an accusation that my approach was "without using any math" just because it wasn't purely computational).

Second, comes the issue of how formal expressions are structured. Many logic specialists, in a desperate try to kill boredom and give themselves some jobs, spend a lot of time inventing and investigating their own alternative logical frameworks, eventually involving some new extravagant ways of putting symbols together to form formulas, regardless that nobody beyond them has any chance of ever making use of such formalisms anyway. But hardly any of them seems to have heard of the following facts, actually well-known by anyone who studied graduate physics, just because... most logic specialists never went to learn graduate physics themselves:

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1. http://www.settheory.net/physicsforums

(1) A large part of the concepts and formulas throughout theoretical physics (from relativistic mechanics, electromagnetism and General Relativity to basic quantum physics and quantum field theory), is actually expressed in a common mathematical formalism: that of tensors[1] which looks quite different from all the "normal" style of mathematical formulas (the only one known by undergraduate math students) described by classical logic.

(2) What is remarkable there is that each (monomial) tensorial expression forms a graph made of occurrences of symbols linked together in ways respecting their types (reflecting the spaces to which arguments respectively belong), similarly to how, in classical logic, symbols are normally linked together (each operation symbol to its arguments) to form expressions, but without any definite hierarchical order of sub-expressions from a root to branches.

(3) The strangeness of this formalism in which physicists are already expressing much of their works, still makes it a big trouble for them to properly explain what it all means. Their usual initiation courses to the topic remain so messy and hard to follow, that this "difficulty" forms one of the main obstacles against introducing it, and with it some "more serious" physics, into undergraduate physics curricula.

Third, is the philosophical understanding of the nature of time. General Relativity only involves time as a geometric dimension, thus without any philosophical feature of time such as its flow and its orientation. Then quantum physics and statistical physics do involve a time orientation and flow (with measurement and entropy creation), however these seem to come "from nowhere" and are not really accounted for by any theory of physics. Mathematical logic, on the other hand, explicitly provides some kinds of time orientation interestingly similar to intuitions of the flow of time and free will

1. http://settheory.net/tensors

(though usually not pointed out) : complexity theory, randomness (Chaitin), halting problem, truth undefinability and incompleteness, transfinite induction... I already introduced the main aspects of this flow of the time of mathematics in the philosophical pages [1]at the end of my Part 1 on mathematical foundations.

I think these similarities could be worth entering the philosophical debate, and I already formulated my propositions for this in my metaphysics text A mind/mathematics dualistic foundation of physical reality[2].

4. Mathematical Foundations

To sum up some of the main points of my contributions in the foundations of mathematics:

> (1) While I agree that ZF(C) is one of the best references for research works on set theory, especially for relative consistency results such as that of CH, I find it not the best choice of basis to start mathematics: neither for "practical mathematics" for undergraduates, nor even as properly self-explaining of why it is indeed a good reference for high-level consistency results.
>
> (2) For a better start of mathematics[3] I propose a new formalization of set theory, introduced in parallel with the rudiments of model theory. There I admit as basic objects not only sets but also pure elements and functions. Oriented pairs and other tuples[4] are more cleanly defined as functions.
>
> (3) Several axioms of sets existence come as particular cases of a single principle[5], a sort of "any class behaving like a set is

1. http://settheory.net/foundations/time-in-model-theory
2. http://settheory.net/mind-math_dualism.pdf
3. http://settheory.net/foundations/
4. http://settheory.net/sets/tuples
5. http://settheory.net/foundations/set-criterion

a set", namely : the classes in which a quantifier is equivalent to a formula with bounded quantifiers. In the philosophical pages I then justify this principle itself [1] from a philosophical understanding of the difference between sets and classes: not that of limitation of size, but using a concept of mathematical time flow (through an interplay between set theory and model theory) : "a class is not a set if it remains able to contain elements which do not exist yet" (which may happen for very small classes such as one just able to contain one future element). The remarkable fact that the powerset[2] does NOT comply to this condition, accounts for the possibility for different universes to interpret the powerset differently[3].

(4) I introduce categories quite early (even before a formal definition of natural numbers). I start with a variant of the concept of concrete category[4], without any mention of functors. (I used the word "functor" for a more general concept[5] which I need to name but did not see named by other authors : the structures of any theory are operators and predicates ; the unary operators are what I call functors). That is my way to give categories a good place in the picture, implicitly responding to arguments of some category theorists that "ZF is not good because it does speak about categories, so let us take categories as an alternative foundation of maths". No, categories are nice but I never saw a good way for category theory to replace set theory as a foundation of maths anyway, so here is what I see as a good balance instead.

(5) I give a clean abstract mathematical definition of the concept of algebraic term[6], which I did not see well done elsewhere, as a particular case of relational system. It is not a

1. http://www.settheory.net/foundations/classes2
2. http://settheory.net/sets/powerset
3. http://settheory.net/model-theory/completeness
4. http://settheory.net/model-theory/concrete-categories
5. http://settheory.net/foundations/structures
6. http://settheory.net/model-theory/terms

particular case of "strings of symbols", an approach seemingly common by other authors (influenced by computer science where everything is a file that is a string of symbols ?), but which I see complicating things uselessly by its need to define criteria of syntactic correctness and then interpretations of such strings, and by its implicit use of arithmetic in the concept of "string". My way does not assume arithmetic (except for arities of symbols) but, on the contrary, provides it as a particular case of term algebra.

(6) I give a short proof of the Completeness theorem of first-order logic[1] : less than one page, that is much shorter than how I usually saw it done by other authors. Why do they spend so much time with complications when a simple way is possible?

(7) I finally explain the right philosophical justification for the axiom schema of replacement[2], which is much more subtle and complex than naively assumed.

For now I just have a few drafts on the foundations of geometry, which I plan to improve and develop later (affine, projective, conformal, hyperbolic... first done in French long ago[3]). Despite the abstraction which may repel some, I found interesting to present some universal algebra (clones, polymorphisms[4],...) as a basis for linear algebra and duality, before developing it into the formalism of tensors. I generally optimized the expression of many topics, to take much fewer pages than usual courses for a similar amount of knowledge, by many small ideas which I cannot list here.

5. Foundations of Physics

1. http://settheory.net/model-theory/completeness
2. http://settheory.net/zf-consistency
3. http://spoirier.lautre.net/geometrie
4. http://www.settheory.net/polymorphisms

In the physics part of my site I provide two general overviews of physics, and a series of expositions of specific theories. I did not work on it as much as on the math yet, so it remains quite incomplete, and more of the pages are still drafts.

In the list of physical theories[1] I explain the logical articulations between the diverse more or less fundamental theories of physics, and the hierarchical orders between them (another map of theories is given in the metaphysics text[2]).

In the exploration of physics by dimensional analysis [3]I present a wide overview of many phenomena by simply expressing the orders of magnitude of their characteristic quantities, such as the size of atoms and the velocities of the sound, as determined by the fundamental constants (and some contingent parameters), so as to give hints of how things work according to diverse theories without entering their actual formulation.

The exposition of specific theories starts with Special Relativity[4], which I see as the logically second theory of physics after geometry: it just describes the geometry of our physical universe, that is space-time, more correctly than the mere 3D Euclidean space we usually imagine it to be.

I still have to develop the exposition of relativistic mechanics from the Least Action Principle[5] to the conservation laws (which would already need tensors for a clean and complete formulation). But I already explained the fundamental equation of General Relativity by a simple approach, already rather rigorous without the tensorial formalization (required for more general cases) based on the example of the universal expansion[6]. This may be continued, on the one hand up to

1. http://settheory.net/physics-theories
2. http://settheory.net/mind-math_dualism.pdf
3. http://settheory.net/dimensional-analysis
4. http://settheory.net/relativity
5. http://settheory.net/least-action
6. http://settheory.net/cosmology

electromagnetism (I only wrote a comment on its Lagrangian[1]), on the other hand up to the symplectic geometry of the phase space.

But I wrote what comes after this: the Liouville theorem (conservation of volume of the phase space) gives the foundation of thermodynamics[2]. I precisely explained the nature of entropy and its creation process, by an exposition which follows the main logical structure of how it is actually known to work on the basis of quantum mechanics, but without actually formulating this basis; instead of this, I relate things with the approximation of classical mechanics. While this exposition isn't actually rigorous (since the reasoning does not match the described "classical foundation" while its actual quantum justification isn't explicitly formulated at that step), it still provides a clarity making it look somehow simpler, more intuitive and coherent than usual expositions still nowadays trying (and failing) to provide a form of logical rigor on the conceptual basis of pure classical mechanics (which was the only available one when statistical physics was first formulated, until it was found to not be the best match to reality). In particular I take account of the quantum fact that the states of systems are really probabilistic ones as they are undetermined until they are measured. And despite some widespread prejudices, this actually simplifies the math.

Then comes an introduction to quantum physics[3], and a review of its main interpretations. I found a way to express some basic aspects of quantum physics in mathematically accurate but quite simpler ways than usual courses, actually explaining in a simple geometric language (affine and projective transformations) the mathematical coherence of its paradoxical predictions such as those of the double slit experiment[4]. Then I develop critical reviews of the main interpretations (Copenhagen[5], Bohm[6], many-worlds[1], spontaneous

1. http://settheory.net/electromagnetism-lagrangian
2. http://settheory.net/entropy
3. http://settheory.net/quantum-measurement
4. http://www.settheory.net/double-slit
5. http://www.settheory.net/Copenhagen-interpretation
6. http://www.settheory.net/Bohm

collapse[2]), and an exposition of the one I support with original details: the Von Neumann-Wigner interpretation[3], giving a fundamental role to consciousness (considered immaterial) in the wavefunction collapse. I develop this into an original ontological position, variant of idealism : mind/mathematics dualism[4] (admitting mind and mathematics as two distinct fundamental substances, separately time-ordered, while the physical is a combination of them). I also provide many links to references on the topic.

6. Other Pages of the Website

In the “World” section [5]of the website I developed a long list of links to teams (and more isolated researchers) on logic and mathematical foundations around the world, encompassing 3 kinds of orientations (and thus of faculties they may belong to) : mathematics, computer science and philosophy. I guess it may be about 80% exhaustive (it becomes harder to complete as the few missing ones are harder to find). I also listed some other kinds of resources in the field (journals, proving software, organizations, blogs, conferences, courses...).

It is ironical to see all researchers working in the institutions, so much focused on similar kinds of missions and the publication of their own work without caring as much about that of their peers, that none of them cared to invest as much efforts as I did into this task of listing their peers worldwide.

Another important part of my site is a series of texts commenting diverse aspects of the stakes of the world's future[6], such as the failure of communism, human intelligence, and environmental, economic, demographic, political and educational aspects. I wrote much of these

1. http://www.settheory.net/many-worlds
2. http://settheory.net/spontaneous-collapse
3. http://settheory.net/quantum-mind-collapse
4. http://settheory.net/mind-math_dualism.pdf
5. http://settheory.net/world
6. http://settheory.net/future/

in reply to many essays I reviewed from the 2014 fqxi essay contest "How Should Humanity Steer the Future?".

Finally, another big part of my research was to design a plan of new online social network which would resolve many current defects of the Web and other world's problems. In the page "Why I am upset" [1]I commented on the dire lack of people combining the qualities of care and intelligence (ready and able to deeply and properly think outside the beaten paths and then act over the conclusions); how the few such people have no good place in this world, and the big consequence I faced: that, no matter that I could usually convince all the people who took the time to understand parts of my plan were convinced of its high value and rather cheap feasability, it still has "no reputation" just because "reputation" in this world remains a matter of global rumor, something completely mindless and circular which no mindful person ever cares to correct, as there is no good way to share and structure non-trivial information on trust and reputation. Ironically, this problem is precisely among the main ones my plan would resolve, as it was the starting issue around which I designed it. To implement it, all I need is one or a few good web programmers (a category of people usually very hard to find available at an affordable price as they are so much demanded on the world market).

1. http://www.settheory.net/life

INDIAN LOGIC IN "THE COLLECTED WRITINGS OF JAYSANKAR LAL SHAW"

Jaysankar Lal Shaw

1. About the Author

Jaysankar Lal Shaw is a retired professor of philosophy at Victoria University of Wellington. His main area of research is comparative analytic philosophy.

My research in the field of Indian and Comparative Philosophy is considered to be pioneering. My aim is to suggest some new solutions which involve both scholarship and creativity. My publications include eleven books and some ninety papers, some of which are monograph-length. My papers have been the first of their kinds in many Western journals of philosophy and logic. The seminars I organised were also the first of their kinds

I have presented approximately 160 papers at different conferences or seminars in several countries. I have received more than 110 funded invitations from institutions or universities around the world.

My papers were greatly appreciated by my peers as I used the techniques of classical Indian philosophers for solving some of the philosophical problems of Western philosophy. Some of my papers are also related to the works of classical Indian scholars or Pandits.

I have directed orientation courses on comparative philosophy in several countries. I am also a founder of several Societies in different countries for the promotion of Indian and comparative philosophy.

Profiles of my academic achievements and lectures have been published more than 125 times in various leading newspapers and magazines in different languages in several countries. A poem has also been published in my honour in Bengali.

My teaching programme encompasses both Western and Indian philosophy, which is a rarity.

My unique contribution to the field of comparative philosophy has taken on the form of a global movement. My research in the field of comparative philosophy will promote better intercultural understanding among different communities and nations at large. To quote the Deputy Mayor of Wellington: “He is doing God’s work”

2. About " The Collected Writings of Jaysankar Lal Shaw: Indian Analytic and Anglophone Philosophy"

One of the first philosophers to relate Indian philosophical thought to Western analytic philosophy, Jaysankar Lal Shaw has been reflecting on analytic themes from Indian philosophy for over 40 years. This collection of his most important writings, introduces his work and presents new ways of using Indian classical thought to approach and understand Western philosophy.

By expanding, reinterpreting and reclassifying concepts and views of Indian philosophers, Shaw applies them to the main issues and theories discussed in contemporary philosophy of language and epistemology. Carefully constructed, this volume of his collected writings, shows the parallels Shaw draws between core topics in both traditions, such as proper names, definite descriptions, meaning of a sentence, knowledge, doubt, inference and testimony. It captures how Shaw uses the techniques and concepts of Indian philosophers, especially the followers of the Navya-Nyaya, to address global problems like false belief, higher order knowledge and extraordinary perception. Exploring timeless ideas from Indian thought alongside major issues in contemporary philosophy, Shaw reveals how the two traditions can interact and throw light on each other, providing better solutions to philosophical problems. He has also reflected on modern issues such as freedom, morality and harmony from the classical Indian thought.

Featuring a glossary and updates to his writings, *The Collected Writings of Jaysankar Lal Shaw: Indian Analytic and Anglophone Philosophy* also includes new work by Shaw on the relationship between Indian and analytic philosophy today.

3. The Content of the Book

The Collected Writings of Jaysankar Lal Shaw: Indian Analytic and Anglophone Philosophy contains the following topics. The Part III deals with Indian and Buddhist logic.

- PART I: Metaphysics

1. Causality
2. Buddhism on Suffering and Nirvana
3. The Referent of 'I': An Indian Perspective
4. The Nature of Nyaya Realism

- PART II: Epistemology

5. The Nyaya on Sources of Knowledge – Perception, Inference, Analogy, and Testimony: Some Contemporary Problems and their Solutions from the Nyaya Perspective
6. Knowledge, Belief and Doubt: Some Contemporary Problems and their Solutions from the Nyaya Perspective
7. A Note on Cognition of Cognition in Indian Philosophy
8. Subject and Predicate
9. Navya-Nyaya on Subject-Predicate and Related Pairs

24. Some Reflections on Kripke

- PART V: Morals and Values

25. Dharma and the Law of Karma in Indian Culture

26. The Nature of Human Beings: East and West

27. Freedom: East and West

28. Swami Vivekananda and Bertrand Russell on Conception and Development of Human Beings

29. Concepts of Harmony in Indian Philosophy

4. Some Reviews

The book has received the following reviews from other researchers in the same area of research:

> *There has never been anyone who has done more to focus Western philosophers on the significance of Indian analytical philosophy, in particular, philosophical logic and the philosophy of language, than Professor Jay Shaw. I'm delighted that finally there is a single volume that brings all of these exciting and creative papers together.*
> *– Ernie Lepore, Board of Governors Professor of Philosophy and Acting-Co-Director of Cognitive Science, University of Rutgers, USA,*
>
> *Jay Shaw is greatly accomplished at demonstrating deep knowledge of not only one, but multiple philosophical traditions, using one to illuminate issues raised in another. By juxtaposing linguistic aspects of Nyaya with the philosophy of language in analytic*

philosophy, for example, he provides new comparative insights which are likely to come as a surprise to many analytic philosophers.
– David Lumsden, Research Associate in Philosophy, University of Waikato, New Zealand,

Dr. Jaysankar Lal Shaw is one of the most significant stalwarts of Indian and comparative philosophy in the contemporary analytical context. Grappling with the interstices of Indian and contemporary (Western) analytical thinking in an inimitable way, each of Shaw's essays shows the 'gift of fruitful dialogue' and 'conversation' between (seemingly) disparate traditions of thought, demonstrating his enormous influence across various sub-fields of philosophy.
– Purushottama Bilimoria, editor-in-chief of 'Sophia, International Journal of Philosophy and Traditions',

5. The Message of Amirkabir Logic Group

On March 24, 2017, the editor of the present collection, Ali Sadegh Daghighi, was invited by the author of this article to send a message, as the representative of the Amirkabir Logic Group, addressing the Society for Philosophy and Culture in New Zealand which was hosting a book launch session[1] celebrating the recent publication of *The Collected Writings of Jaysankar Lal Shaw*, on March 31. 2017. The full text of this message is as follows:

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1. Read more about this event and the Society for Philosophy and Culture here: http://www.philosophyandculture.org/seminars/the-collected-writings-of-jaysankar-lal-shaw/

Society for Philosophy and Culture,
Dear Members,
Distinguished Guests,

As the representative of the Amirkabir Logic Group, it is my pleasure and honor to have the chance to address my remarks to the attendees of this event.

First of all let me sincerely thank the Society for Philosophy and Culture for hosting this gathering and also the distinguished professor, Jaysankar Lal Shaw, from Victoria University of Wellington, for his kind invitation.

I got introduced to Prof. Shaw through my logician friend and colleague, Anand Jayprakash Vaidya from San Jose State University, who was informed of our intention to strengthen the ties with those colleagues around the world who are willing to promote research on Indian and Buddhist logic among the Iranian researchers. It is actually part of our general plan for introducing less well-known areas of research to the Iranian mathematical and philosophical logicians.

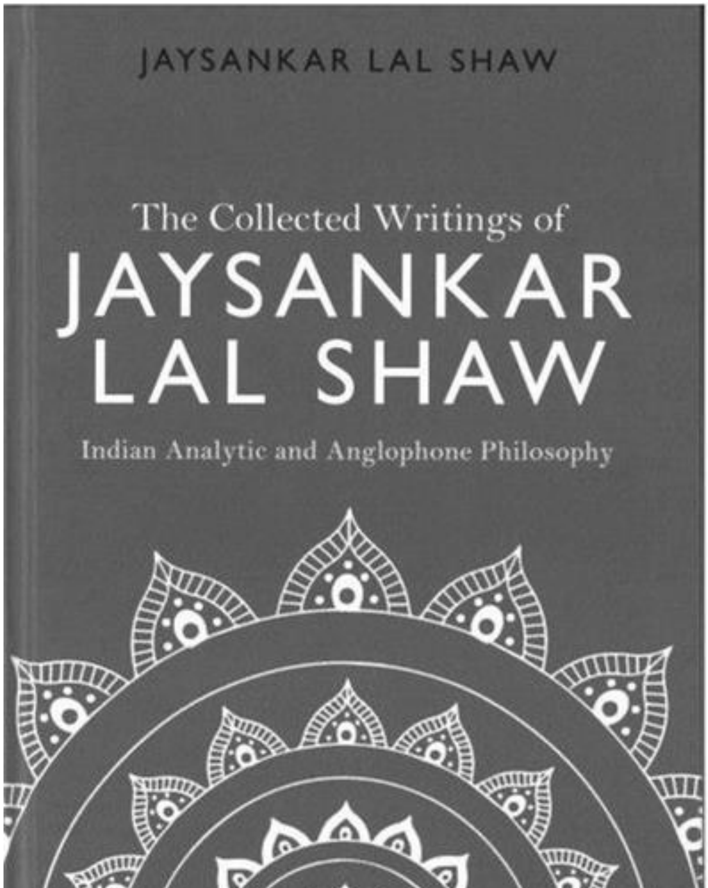
Having the privilege to be the editor of an under preparation special issue for the 5th Annual Conference of the Iranian Association for Logic (IAL) at the Amirkabir University of Technology and from the perspective of Amirkabir Logic Group in general, I think publishing a review of Prof. Shaw's “Collected Writings on the Indian Analytic and Anglophone Philosophy” in our book will be a perfect starting point for many joint collaborations in the future.

The review of Prof. Shaw's book, as a unique digest of his life-time research along the lines of Indian logic and comparative analytic philosophy, will appear in *our collection of papers and will be distributed among logicians of different research backgrounds in and out of Iran. We eagerly look forward to receiving our audience's feedback concerning Indian logic in general and Prof. Shaw's book in particular. We also sincerely welcome all those colleagues who might be interested in collaborating with the Iranian logic society on the joint projects of common interest.*

It is my hope, and indeed the hope of all members of the Amirkabir Logic Group, that a better environment for academic collaborations shall emerge out of such international interactions between Iranian logic society and its counterparts around the world – collaborations founded upon mutual understanding and collective efforts of the members of our communities.

Thank you for your time and attention, and I wish you a joyful and successful meeting.

Ali Sadegh Daghighi
Amirkabir University of Technology,
Tehran, Iran,
March 2017.


JAYSANKAR LAL SHAW
The Collected Writings of
JAYSANKAR
LAL SHAW
Indian Analytic and Anglophone Philosophy
BLOOMSBURY

A NOTE ON "THERE ARE TWO ERRORS IN THE TITLE OF THIS BOOK"

Robert Martin

I was born in 1942, and lived in New York until I graduated with a philosophy degree from Columbia University; then I attended the University of Michigan, where I got my PhD. My first job was at Dalhousie University in Halifax, Nova Scotia, Canada, and except for occasional visiting teaching elsewhere, I stayed there for forty years until I retired.

My first book, published in 1987, was about philosophy of language, and since then I have published books on deductive logic, on inductive logic, and on several other subjects. My aim always has been to write in a way that's friendly to readers – that makes things clear to them, and makes them interested in reading.

I appreciate informal, reader-friendly writing, and I try to produce it. I also think that humor has a place in even academic writing! I have just finished a book to appear later this year in a completely different area: how to write good English.

What led me to write the *Two Errors* [1]book was a list I had been keeping of philosophical thought experiments, puzzles and paradoxes. The list grew and grew, until it appeared to me that it might become a whole book if I continued to add to it.

I take philosophy to be concerned with providing theories for other sorts of fields. Thus for example there is philosophy of science, which attempts to discover the central principles of scientific procedure, and to justify them. A philosophical theory, like any sort of theory, can be of pure intellectual interest, and sometimes without much (or any) practical implication. This is not a bad thing! A great deal of theory-building is (at least for the moment) without practical implication, motivated just by people's curiosity—their desire to figure out how things work. People in any area usually get along quite well without a theory—a philosophy—of what they're doing. They just do it! But sometimes a a puzzle or paradox arises, or a thought experiment with

1. The book has been translated into Persian by Razieh Salim Zadeh and is published by Ghoghnous publications in Iran.

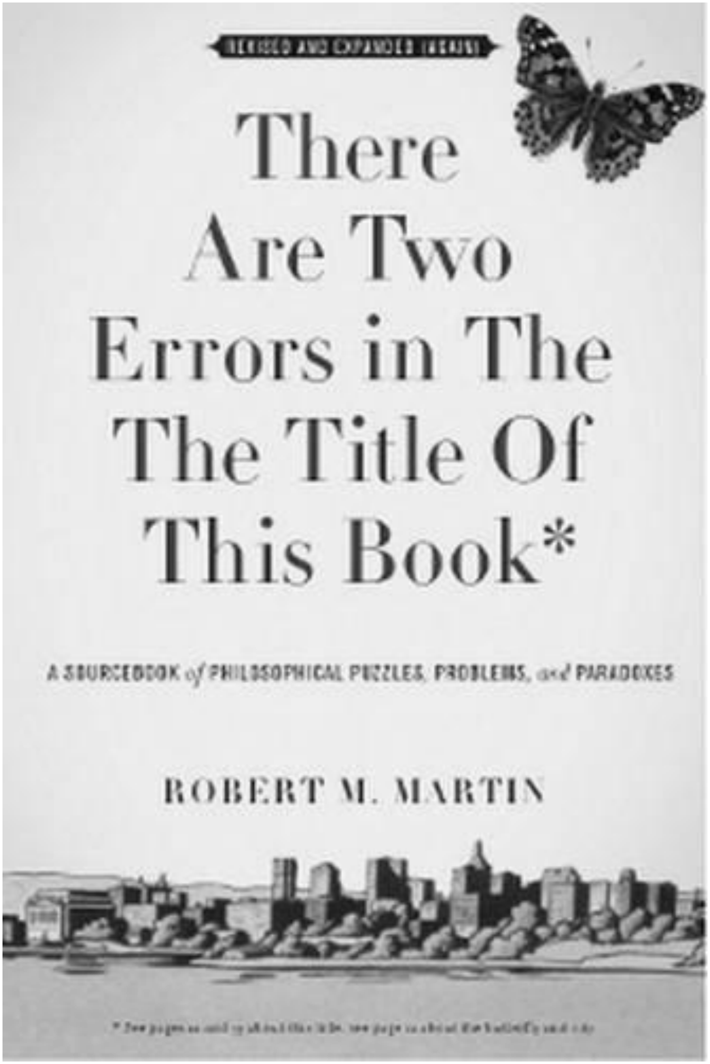
surprising results; this is a symptom of a contradiction or incompleteness in our unspoken—even unexamined—principles for doing something. This provides a special stimulus for thinking philosophically, and may be a source for its practical usefulness, if it can fix a contradiction or incompleteness.

Logic is the branch of philosophy which tries to provide theories of reasoning. As in the rest of philosophy, the motive for theorizing here may just be pure curiosity. I think that most of logic is really without practical use. The reason for this is that reasoning skills are not often helped by learning the theory of reasoning—in the same way as, for example, leaning the physics of moving objects is of no use in learning how to play football! Very often the theory of some kind of reasoning is more complicated, harder to understand and learn, than the reasoning it is the theory of. People who are having intellectual problems with reasoning itself might have even greater problems learning its logic.

But sometimes theory-building does have practical application, even importantly. In the case of logic, for example, it was necessary to figure out the theoretical structure of certain kinds of reasoning in order to build a machine that could to it—and that's why it took logicians to invent the computer.

I was asked about further readings. There are hundreds of good books in logic, and a hundred times as many in philosophy as a whole. But I'll mention just a very few that I liked very much. Smullyans' *What is the Name of This Book* is a good collection of problems, puzzles, and paradoxes. It is a bit less philosophically oriented than my *Two Errors* book, but there is, nevertheless, a good deal of overlap in areas talked about. (His title was obviously the inspiration for mine.)

Michael Clark's *Paradoxes from A to Z* is another such work. *What If: Collected Thought Experiments in Philosophy* is diverting and simple. A book dealing with philosophical perplexities in science and mathematics is John L. Bell's *Oppositions*. About philosophy in general, I highly recommend Chris Daly's rigorous, scholarly and complex *An Introduction to Philosophical Methods*; but for a much easier (and more entertaining, I think) book on the same subject, I should mention my recently published *For The Sake of Argument: How to Do Philosophy*.


(REVISED AND EXPANDED (AGAIN))
There
Are Two
Errors in The
The Title Of
This Book*
A SOURCEBOOK of PHILOSOPHICAL PUZZLES, PROBLEMS, and PARADOXES
ROBERT M. MARTIN

BRAIN'S ALGORITHM: ON VON NEUMANN'S "THE COMPUTER AND THE BRAIN"

Keyvan Yahya

There is certainly no discussion over the fact that neuroscientists will not ever be able to find a copy of Hamlet, Shakespeare's magnum opus, in the brain nor can they find a plugging to remove the contents of the brain to a PC or vice versa in the same way we are transferring our files stored on digital devices from one system to another. This statement is stemmed from a striking discovery of the brain science that blew the high hopes away by undermining a very popular view held by cognitive scientist, in that, the brain is no different to a digital computer working with bits , symbols and tokens most similar to that of the laptop by which this article is being typed. No such a thing as memory, cache and so forth do exist in the brain.

The historical root of this traditional trend in artificial intelligence is dating back in the 1940's when the invention of the modern computer was largely considered a breakthrough leading the experts including psychologists, linguists, and neuroscientists to assert that the human brain works pretty much like a computer. (Searle, 2004)

This seemingly promising view was later empowered by some especial blends from both functionalism and behaviorism and ended up fostering as an overgrowing school of taught called strong AI according to which every cognitive aspect of the brain is somehow associated with a series of functions which can thoroughly be explained in terms of algorithms and digital information, then again, just like computers.

It mustn't come to anybody's surprise that the past generations of scientists were enormously inclined to the tenet of such an appealing theory that appeared to help them disclose the most grotesque mystery of all times for which no single substantial leading que had ever come forward until after computer was invented, the stubborn problem of human cognition and consciousness, that was too equivocal and remote to fit in with the foundations of the natural sciences back then. Nevertheless, in spite of such a promising idea as brain as a computer plus a considerable progress we have so far made in the pursuit, the ultimate breakthrough is yet to come.

Taking a closer look at the brain of infants can show us how vacuous the whole idea is .Thanks to evolution, human neonates, like the newborns of all other mammalian species, enter the world prepared to interact with it effectively. A baby's vision is blurry, but it pays special attention to faces, and is quickly able to identify its mother's. It prefers the sound of voices to non-speech sounds, and can distinguish one basic speech sound from another. We are, without doubt, built to make social connections.What matters in the beginning is a bundle of senses, reflexes, learning mechanisms and nothing more. We would have never survived, had we not been equipped with these capabilities at birth. Meanwhile, a massive body of the literature implies that we are not born with: *information, data, rules, software, knowledge, lexicons, representations, algorithms, programs, models, memories, images, processors, subroutines, encoders, decoders, symbols, or buffers* – structural elements that enable digital computers to behave somewhat intelligently.

If we are to describe the quintessential job of digital computers, we should simply say information processing – numbers, letters, words, formulas, images. Digital computers encode the information into a certain type of format, i.e., patterns of ones and zeroes ('bits') organised into small chunks ('bytes'). Computers, quite literally, move these patterns from place to place in different physical storage areas etched into electronic components. They really operate on *symbolic representations* of the world and moreover, they *store* and *retrieve* this information. The human brain just doesn't do so.

The tenet of the view can easily be found in a seminal book by John von Newman (1958) in which he first clearly states that human brain bears a strong resemblance to digital computer and then he draws a detailed comparison between the neural pathways, neurons, neural networks and the component of digital computers of the day. This book, in effect, pioneered a new interdisciplinary field that aims to lead all those endeavors towards understanding the brain considering the brain is just another digital information processor. This view rose to fame during the 1970’s reflected through an impressive number of both technical and mainstream writings including (Ray Kurzweil, 2013) that all in all elaborate the algorithms the brain employs to receive, store retrieve and process information. Although the allure of the metaphor information processing (IP) fell out of favor in the recent years due to the significant studies that questioned the biological plausibility of these kind of theories, it seems that the IP metaphor has still enough deity to dominate the whole area of cognitive research so that we can hardly imagine the mainstream research programs could proceed without it.

As an example, the integrated information theory (IIT) suggested by the renowned Italian neuroscientists Guilio Tonini (2003) can be referred to as the incarnation of the same old discourse into a more biologically convincing body. As for the other theories born with the same inbred, IIT also takes the (IP) metaphor into account suggesting that consciousness is composed of a specific set of specific phenomenal distinctions given rise by the integration of information accumulating all across the thalamocortical pathway leading to a complex system with a threshold and once the brain passes the threshold, the cognitive qualities such as consciousness will emerge inside out.

Of course, it is noteworthy that the tenet of IIT in some ways differs to that of the existing theories of this category. First and foremost, IIT is not a functionalist theory of consciousness at all and also take a very dim view of another central metaphor of functionalism that considers mind to be just like software running on your brain.

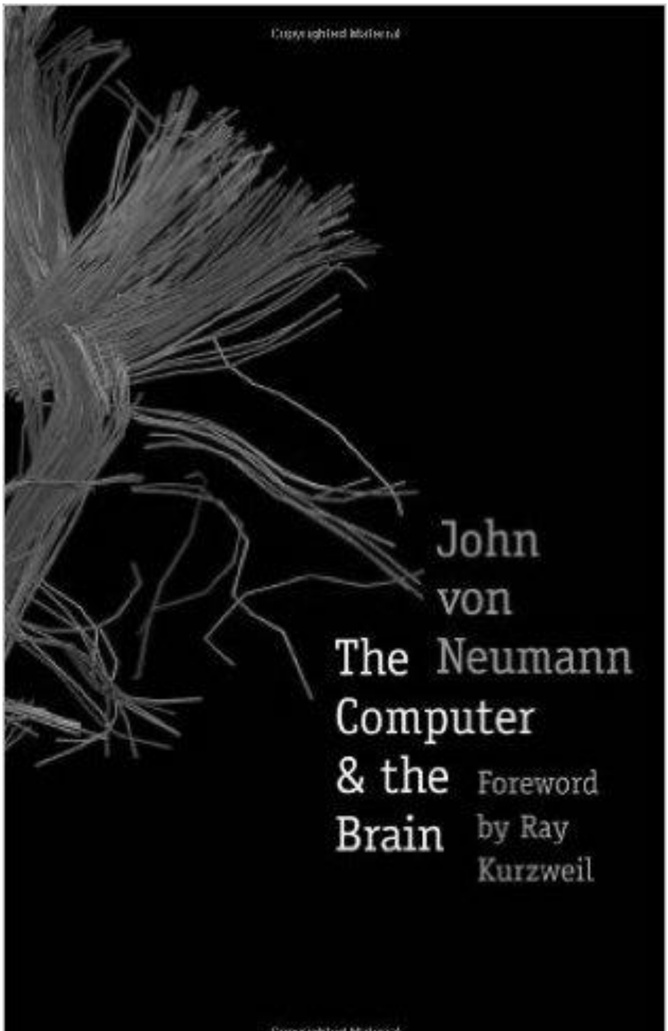
References

[1] Searle, J. R. (2004). *Mind: A brief introduction*. New York: Oxford University Press.

[2] Von Neumann, J. (1958) *The computer and the Brain*. Yale University Press.

[3] Kurzweil, R. (2012). *How to create a mind: The secret of human thought revealed*. New York: Viking.

[4] Tononi, G; Boly, M; Massimini, M; Koch, C. *Integrated information theory: from consciousness to its physical substrate*. Nature Reviews Neuroscience. **17** (7): 450–461.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