Symposium

Karl Popper and the Philosophy of Mathematics

Proceedings of the Conference held in Klagenfurt, 5 – 7 April, 2018

Edited by
Reinhard Neck

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Contents

Preface

April 5–7, 2018 the Karl Popper Foundation Klagenfurt at the Alpen-Adria-Universität Klagenfurt held a symposium, lasting for 2½ days, on the topic "Karl Popper and the Philosophy of Mathematics." Karl Popper contributed intensively to the theory of probability, to mathematical logic, and to some other areas of mathematics, in addition to his main interests in the philosophy of science, political philosophy, and other areas of philosophy. After a Call for Papers sent to a large number of prospective contributors, we received around 20 proposals for presentations on a variety of topics in the philosophy of mathematics (in a wider sense) that related to the work of Karl Popper and to Critical Rationalism.

The scientific committee consisting of Max Albert (University of Giessen), David Miller (University of Warwick), Reinhard Neck (Alpen-Adria-Universität Klagenfurt) and Peter Schroeder-Heister (University of Tübingen) refereed all proposals and invited the authors of the best presentations to submit their papers for presentation. The present document contains the programme and the abstracts or preliminary versions and some presentations of the papers plus one paper that could not be presented due to the airline and railway strike in France. They are reproduced here in the preliminary form in which they were presented and in no case should be regarded as final publications; please don't quote them without asking authors for consent. It is not yet decided whether the final papers will be published together in a collective volume or a special issue of a journal.

Reinhard Neck, Klagenfurt, June 15, 2018

Participants at the end of the symposium in front of the Alpen-Adria-Universität

Programme

Thursday, 5 April 2018

Chair: Reinhard Neck

09.00 Peter Schroeder-Heister: *Popper on Deductive Logic and Logical Deduction*

10.30 David Binder: *A Critical Edition of Popper's Work on Logic*

11.30 Constantin Brîncuş and Iulian Toader: *Non-normal Interpretations of Positive Logic*

14.00 Guided Tour: Karl Popper Collection of the Main Library

Karl Popper Collection in the Main Library

Chair: Peter Schroeder-Heister

16.30 Daniel Pimbé: *Popper and the „Absolute Proofs"*

Friday, 6 April 2018

Chair: David Miller

09.00 Max Albert: *Critical Rationalism and Decision Theory*

10.30 Flavio Del Santo: *The Physical Motivations for a Propensity Interpretation of Probability and the Reactions of the Community of Quantum Physicists*

11.30 Oseni Taiwo Afisi: *Propensity Probability and Its Application of Knowledge in Ifa*

Chair: Max Albert

15.00 David Miller: *Independence (Probabilistic) and Independence (Logical)*

16.30 Bernard Burgoyne: *From Cosmic Paths to Psychic Chains*

Saturday, 7 April 2018

Chair: Reinhard Neck

09.30 Brian Boyd**:** *The Psychology of Reasoning, the Logic of Discovery, and Critical Rationalism*

11.30 General Discussion

Scientific Committee:
Max Albert, David Miller, Reinhard Neck, Peter Schroeder-Heister

Abstracts and Preliminary Papers

Popper on deductive logic and logical education

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Although not very well known (not even among Popperians), Popper provided substantial work on logic and logical deduction. Much of it was published in the late 1940s. A re-edition of this work together with the edition of a considerable amount of unpublished material from the Klagenfurt Karl Popper archive is almost finished and will be described by David Binder in his contribution to this conference. Poppers logical writings are very remarkable both from the logical aspect and from the point of view of Popperianism. Logically, they make very significant contributions to Gentzen-style deductive systems and to what today is called "proof-theoretic semantics". However, the ideas advocated there are not perfectly coherent with the views on deductive logic that Popper otherwise held in his well-known work on scientific method, at least not at first glance. In this talk I shall try to give an overall assessment of Popper's contribution to deductive logic and to provide some ideas of how it might fit into Popper's work in general.

A Critical Edition of Popper's Work on Logic

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Karl Raimund Popper's work on formal logic [1, 2, 3, 4, 5, 6, 7], written and published in the 1940s, is not as widely known as it deserves. Only very few detailed investigations into the philosophical and technical aspects of these articles have been published [8, 11, 12, 13, 14]. In view of the extensive literature on almost every other aspect of Popper's philosophy, this is in itself noteworthy, especially in view of the fact that decidedly logical arguments are at the core of Popper's philosophy of science.

There are various reasons which may explain this scholarly neglect. Popper himself disavowed this line of research, later classifying it as a poorly worked out rediscovery of natural deduction. The articles also contained technical errors that were pointed out by reviewers, something which discouraged Popper from continuing this work. But despite all this his articles contain ideas which merit a more detailed study. In order to make this part of Popper's legacy more available to other philosophers and logicians, we (Peter Schroeder-Heister, Thomas Piecha and David Binder) are currently preparing a critical edition of these articles together with a wealth of additional material from the Karl Popper-Sammlung of the University of Klagenfurt. This book will contain:

- Newly typeset versions of the published articles, together with errata, editorial history and introductions.
- All the original reviews written by Kleene, Curry, Beth, Ackermann, Hasenjaeger and McKinsey.
- Unpublished manuscripts written in preparation of the published articles and containing entirely new material relating to these articles.
- Correspondence. Popper discusses, clarifies and amends the content of these articles in correspondence with other logicians and mathematicians such as Carnap, Bernays, Quine, Forder and Brouwer.

Having studied thousands of unpublished pages from the collection of the Karl Popper-Sammlung in Klagenfurt, I will present an overview of the history of Popper's investigations into logic in the 1940s and how it can be reconstructed from the material we plan to include in the book. The following is a historical overview of Popper's preoccupation with formal logic.

VIENNA. Popper probably first got in contact with logic and the foundations of mathematics by enrolling in a course of Hans Hahn in 1922 in which Principia Mathematica was part of the curriculum. He soon got in contact with members of the Vienna circle, among them Gödel and Carnap, and in 1934 he met Tarski who had a profound influence on Popper's views on logic. We do know about the impression that Tarski's analysis of truth made on him, and in a letter written in 1943 he calls himself a “disciple of Tarski” and mentions that he helped Tarski in the translation of “Über den Begriff der logischen Folgerung” into German.[1] There is little written testimony about his views on formal logic during the Vienna years, due to the lack of publications on formal logic and the poor archival situation regarding the time before his departure to New Zealand.

CHRISTCHURCH. In 1937 Popper had to flee from Austria and found employment as a lecturer at the University of Canterbury in Christchurch, New Zealand. Part of his teaching duties was a course in logic for philosophers. He was not content with the available logic textbooks suitable for philosophers and planned to write a logic textbook in ~1937/38. The three people whom Popper discussed logical problems with during his time in Christchurch are, as far as we can see, John Findlay, Henry George Forder and Rudolf Carnap. The evidence for Findlay, who taught at the University of Otago at the time, is rather slim and rests on (1) handwritten remarks on a paper that is likely to be an early version of [9], and (2) the fact that Popper discussed that article with Paul Bernays in 1946. With Forder, a professor of mathematics at Auckland University College, on the other hand, the situation is clear since there is an extensive correspondence from February 1943 to July 1945 (23 letters in total). They discuss university politics but also problems in the philosophy of mathematics, logic and quantum physics. It is in these letters that Popper mentions for the first time his conception of logic as a “meta-propositional calculus”; a particular interpretation of the inequations of boolean algebra. The contact with Carnap is through exchange of letters, averaging about three letters per year. Every time Carnap finishes another book, Introduction to Semantics in 1942 and Formalization of Logic in 1943, he sends a copy to Popper who replies with questions and sometimes long sheets of comments. Carnap is certainly, together with Tarski, the one person who inspired most of the logical investigations Popper undertook during that time. Remarks in letters and published and unpublished articles show that it is through reading Carnap that he found the problems that he tried to solve. In 1943 Popper writes a series of articles on boolean algebra, at least one of which he intended to publish in the Journal of Symbolic Logic.[2] They are called “Extensionality in a Rudimentary Boolean Algebra”, “An Elementary Problem of Boolean Algebra”, “Completeness and Extensionality of a Rudimentary Boolean Algebra”, “Postulates for Boolean Algebra” and “Simply Independent Postulates for

[1] Letter from Popper to H. G. Forder, May 7th 1943. Karl-Popper-Sammlung (KPS) 296, 15.

[2] In the LATEX-version that we work with, these articles take up about 100 pages. They are from KPS 12,3; 12,4; 12,5; 16,13.

Boolean Algebra”. Forder supported Popper by proofreading his typoscripts and by lending him articles that were not available in Christchurch, most importantly Huntington's [10] on which much of the development in Popper's articles is based.

LONDON. In 1946 Popper gets a position at the London School of Economics and moves back to Europe. For reasons that are still not clear to us, he met with Bernays in Zürich in December 1946. During discussions, Bernays proposed to publish an article together with Popper who eagerly accepted and set himself to work in the first months of 1947. He finished the article, entitled “On Systems of Rules of Inference” by March 3rd and sent a copy of the manuscript to Bernays.[3] The reason why the article never got published is unclear, but it seems that Bernays was not in full agreement with Popper regarding some of the arguments of the article. The content of this article is already quite close to the content of [2] and [3], but contains significant material that was omitted in those later articles. Among other things, it contains an explicit comparison with Tarski's system [15] and a criterion for the “purity” of inference rules[4]. Popper wrote on the distinction between derivation and demonstration in three unpublished drafts, written some time between the completion of “On Systems of Rules of Inference” and the writing of [3]. One of them is untitled; the other two are called “Derivation and Demonstration in Propositional and Functional Logic” and “The Propositional and Functional Logic of Derivation and Demonstration”[5]. They contain material which would later be incorporated in section 8: “Derivation and Demonstration”, of [3]. He draws the distinction between demonstrational logic, exemplified by the systems of Russell-Whitehead, Hilbert-Ackermann and Hilbert-Bernays, and derivational logic, to which only Gentzen has come close with his system of natural deduction. In these drafts Popper formulates an idea much more radically than in his published articles: the logic of derivation should be primary and the logic of demonstration should be introduced via a definition of demonstrability as a second step. As indicated in the introduction, the reception of Popper's articles by the reviewers was rather negative. But not all reception was negative; William Kneale and Brouwer responded positively. Brouwer had presented three of Popper's articles to the Koninklijke Nederlandse Akademie van Wetenschappen and spoke very warmly about Popper's articles on logic. Even though Popper did not publish anything substantial on formal logic for the rest of his life, he continued to work on logical problems such as the

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[3] The article is in KPS 13,5; 14,8; 36,13. The title is proposed in the letter to Bernays from March 3rd, 1947.

[4] This definition of purity appears as de nition D8.1 in [3], where it is discussed rather elliptically.

[5] KPS 36,20; 36,21.

quantum logic of von Neumann, the relation between non-classical negations and modality and, especially around 1950, on the different concepts of implication.

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Non-normal Interpretations of Positive Logic

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This presentation analyses a problem raised by Popper in connection to Carnap's 1943 book, *Formalization of Logic*, in a private letter from that year, namely, whether the calculus of positive propositional logic allows for non-normal interpretations. Before considering this problem and in order to understand its relevance, we have to explain **a)** why Popper is interested in positive logic and **b)** why it is important to study the non-normal interpretations in connection to it.

a) Popper's interest in positive logic

[Popper 1947a: 290] emphasizes that the distinction between derivation and demonstration (or proof), with some exceptions, "has been often neglected by logicians". Among the exceptions, [Carnap 1937: 28-29, 1942: 167] takes a demonstration to be a special case of derivation, namely, that derivation in which the conclusion "is derivable from the null series of premises, and hence from any sentence whatsoever". In agreement with Carnap (see Popper 1947a, footnote 24), Popper defines a proof as that derivation which "asserts the truth of the conclusion absolutely – independently of the question whether any particular other statement is true". Thus, in a proof, "the conclusion can be validly derived from any premise whatsoever" [Popper 1947b: 231]. The main idea is that a proved statement is true independently of the truth of the premises from which it is derived, while in a regular derivation the conclusion is true provided that the premises are true.

The distinction between derivation and demonstration underlies two ways of constructing a system of logic: as a derivational logic or as a demonstrational logic. A system of logic "intended from the start to be a theory of inference in the sense that it allows us to derive from certain informative (non-logical) statements other informative statements" [Popper 1947b: 230] is *a derivational logic*, i.e., it contains rules of inference for drawing consequences from hypotheses. In contrast, "most systems of modern logic are not purely derivational, and some (for example in the case of Hilbert Ackerman) are not derivational at all." [ibid.] These systems are *demonstrational logics.* The derivations conducted in them *usually* start from logical axioms, definitions or theorems. However, this is not always so because, for instance, in an indirect proof (*reductio ad absurdum*) the premises are jointly contradictory. The main point is that even in an indirect proof, as in any proof, the truth of the conclusion holds independently of the truth of the premises.

[Popper 1970: 17-20] treats the two systems of logic in connection to two systems of logic in connection to two essential features of deduction, i.e., the transmission of truth and the retransmission of falsity, and talks about two uses of logic: *a demonstrative use*, in the mathematical sciences, for proofs, and *a derivational use*, in the empirical sciences, for criticism. In critical discussions in general, and in empirical sciences in particular, we should use the strongest logic, i.e., classical logic, because we want our criticism to be severe.[1] However, in the mathematical sciences, we should use a *minimum apparatus* instead of any strong logical means. Popper regards a proof of a known theorem that deploys weaker resources than the old proof as 'a real mathematical discovery'. This is so because what we aim at in 'sophisticated mathematics' is to know what is *necessary* for proving a theorem and not only what is sufficient. Therefore, in the demonstrative use, we should weaken the classical logic as much as possible, "and we can, for example, introduce intuitionist logic or some other weaker logic such as positive logic, and investigate how far we can get without using the whole battery." [Popper 1970: 19]. We see thus that for Popper intuitionist logic and in particular positive logic, which is a common sub-system of both classical and intuitionist logics, could serve as a firm foundation of mathematical proofs. However, is positive logic an *objective* instrument for carrying on mathematical proofs? In particular, do the statements of positive logic have a unique meaning or do they allow for non-normal interpretations of their logical constituents?

b) Non-normal interpretations and positive logic

In *Formalization of Logic,* Carnap proved that the standard formalizations of classical propositional and predicate logic allow for non-normal interpretations, i.e., interpretations for which the calculi remain sound and complete, but in which the logical constants have different meanings than the standard ones. For instance, there are non-normal interpretations in which a sentence and its negation are both true and non-normal interpretations in which a disjunction is true although both of its disjuncts are false. The existence of such interpretations shows that the standard calculi do not fully formalize all the logical properties of the logical terms and, thus, fail in uniquely determining their meaning.

Carnap's discovery of the non-normal interpretations is seen nowadays as a challenge to logical inferentialism[2] (i.e., the view that the formal rules of inference uniquely determine the meaning of the logical terms). It is surprising then that, although Popper knew of Carnap's results, he defined, some years later, the logical constants in inferential terms. In his review of Popper's article, "Logic

[1] [Popper 1970: 35] mentions that a weakening of classical logic, like that suggested by Birkhoff and von Neumann, or by Reichenbach, is not adequate in the empirical sciences, because it can render an empirical theory irrefutable.

[2] See Raatikainen 2008, Murzi & Hjortland 2009, Bonnay, D. and Westerståhl, D. 2016.

without Assumptions", J. McKinsey pointed out correctly, though without referring to Carnap's results, that Popper's inferential definition of disjunction is inadequate, since it may lead to the result that a sentence follows from a disjunction although it follows from neither of its disjuncts.

In the above-mentioned letter, from July 5th 1943, Popper wrote to Carnap that he also believed that the truth-tables are not fully formalized by the propositional calculus, but had no idea how this problem could be spelled out. Fascinated by Carnap's existence proof for the non-normal interpretations, Popper went further and asked whether a specific sub-system of propositional logic, namely, the positive propositional logic (i.e., propositional logic without negation, formulated by Hilbert and Bernays) allows for non-normal interpretations. More precisely, Popper wondered whether:

I) the axioms of positive logic allow non-normal interpretations in general, and for implication in particular;

II) by adding the axioms for conjunction and equivalence to the implicational axioms of positive logic, the new system allows non-normal interpretations for conjunction and implication, and

III) what happens if we add, separately, to the system of positive logic defined at (II), the axioms for disjunction and the axioms for negation.

In his response to Popper, from December 9th 1944, Carnap qualified Popper's remarks as "the best comments I have received on this book" and confessed to Popper that he had not given much study to the 'positive logic' and, thus, did not know whether there are non-normal interpretations for these systems. However, he encouraged Popper to investigate the problem and told him that "if you find any results, they should be published in the *Journal of Symbolic Logic*." Although Popper did not investigate this problem any further, his questions deserve attention, especially given the important role ascribed by him to positive logic in proofs.

In this presentation, we answer some of Popper's questions regarding the existence of non-normal interpretations for the systems described under I-III, and then discuss some of their consequences for his distinction between demonstration and derivation. We consider the relationship between the existence of non-normal interpretations of a logical system and thus its failure to determine uniquely the meaning of logical terms, on the one hand, and its construction as a derivational or a demonstrational logic, on the other hand. In particular, we discuss the question whether a system of logic that admits of non-normal interpretations could satisfy Popper's constraints on mathematical demonstration, i.e., his insistence that it ought to use a minimal logic apparatus.

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Popper and "absolute proofs"

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Few months ago, while I was reading Popper's book ***The World of Parmenides***[1], I discovered, much to my surprise, a passage in which Popper asserts that there are "absolute proofs" in mathematics. At first I believed, wrongly, that in using this expression "absolute proof" Popper intended to describe a kind of super-proof, a proof apt to establish an absolute certainty: then, something which seems incompatible with his philosophy. So, there was an apparent gap between what Popper said and what I expected him to say. This apparent gap was the first reason – a bad reason – of my interest for the question. I hope that I have found afterwards some better reasons for persisting in this interest.

As far as I know, there are only three texts on absolute proofs in Popper's works: two passages in ***The World of Parmenides***, and one paragraph in a conversation between Popper and the Polish philosopher Adam Chmielewski in the book ***Popper's Open Society After Fifty Years***[2]. These three texts date from the last years of Popper's life, and in the latter (the interview) he points out that his interest for absolute proofs is a recent one.

Here are more precise details about the three texts.

The first passage occurs in ***The World of Parmenides***, more precisely in Essay 4, "How the Moon might throw some of her light upon The Two Ways of Parmenides". This essay was written in March 1989. The expression "absolute proof" (more exactly "almost absolute proof") occurs on p. 86.

The second passage also occurs in ***The World of Parmenides***, but in an Appendix whose title is "Popper's late fragments on Greek philosophy". It belongs to a unfinished paper of Popper entitled "Aristotle's mathematics misunderstood". The expression "absolute proof" occurs three times on p. 297.

The third passage is more similar to the second one than to the first one. It occurs in the course of a conversation between Popper and Chmielewski, which took

[1] Karl POPPER, *The World of Parmenides, Essays on the Presocratic Enlightment*, edited by Arne F. Petersen and Jørgen Mejer, London, Routledge, 1998.

[2] *Popper's Open Society After Fifty Years, The Continuing Relevance of Karl Popper*, edited by I. C. Jarvie and Sandra Pralong, London, Routledge, 1999

place on 29 July 1994. The title of the conversation is "The future is open". This interview is now the section 2, Part 1, of the book ***Popper's Open Society After Fifty Years***. The expression "absolute proof" occurs twice on p. 28.

These three texts have much in common, but also some great differences. Now it is difficult to accommodate these differences with the common points. As we shall see, this is the main problem about Popper's absolute proofs.

Let us consider, first, their four common points: According to the three texts, absolute proofs

- are defined as proofs without assumptions
- are opposed to axiomatic method
- were proposed by pre-Euclidian mathematicians
- were forgotten and neglected after Euclid.

The first similarity between the three passages is that "absolute proof" is every time defined by Popper in the same manner: an absolute proof is a proof "without assumptions". The word "absolute" means precisely that the proof is not relative to any assumption. This definition is implicit in the first passage, explicit in the second and in the third, where we can read: "proofs that need no assumptions" (second passage), or "there are no assumptions there" (third passage).

Now, is it possible to conceive a proof which would be completely without assumptions? We must suppose, I think, that in Popper's mind an absolute proof is "without assumptions" in a peculiarly pertinent meaning, a meaning which may be regarded, for a good reason, as the only pertinent meaning, considering what the proof is supposed to prove. This is what I will describe as the first requirement.

But it is not enough. The alleged absolute proof must furthermore be "without assumptions" in an explicit way, openly. For the simple fact that no assumption has been explicitly stated is not sufficient to conclude that a proof is without assumption. Popper is required to convince us that his absolute proofs are not simply incomplete proofs whose assumptions are not yet made explicit. This is the second requirement.

Now, I proceed to the second common point: absolute proofs are opposed to axiomatic method.

When he asserts that there are proofs without assumptions, Popper obviously suggests that these proofs are not dependent on the "axiomatic method", the method of making a few assumptions and then deducing from them, by purely logical means, the huge edifice of theorems. Where this axiomatic method is adopted, there is no room for "absolute proofs": any supposed proof is relative to some propositions which cannot be proved.

Then, we may understand why Popper took great interest in absolute proofs. He was not interested in super-proofs, in absolute certainty. His interest was a methodological interest. The existence of absolute proofs points out that axiomatic method is not the only possible method in mathematics: there is an alternative.

What alternative? Mathematics is a deductive science, and a deductive science, apparently, is bound to start with axioms. Whenever he discusses axiomatic method, Popper never rejects it. He always emphasizes its importance, but criticizes the idea that the construction of an axiomatized deductive system should be the final task, the true end of science[3]. It is merely a means. The true end of deduction, he says repeatedly, is not to establish and guarantee the conclusions, but to test the premises. So, what Popper names "method by conjectures and refutations" seems to be the only alternative to axiomatic method. When mathematical assumptions are regarded as axioms, every proof is relative to these axioms. When they are regarded as conjectures, some absolute proofs are possible.

Third common point: absolute proofs were proposed by pre-Euclidian mathematicians. After Euclid, Popper asserts, everything is axiomatic in mathematics. Then, since we are looking for the use of an alternative method, we shall find it only before Euclid, more precisely during a period of three centuries from Pythagoras to Euclid.

Now, Popper teaches that history is the history of problem situations. So, what matters in any period of mathematics, for instance in that pre-Euclidian period, should be the mathematical problem situation in that period.

According to Popper, the problem situation in pre-Euclidian mathematics was not a purely mathematical problem situation. Rather it was a cosmological problem situation, namely the breakdown of the Pythagorean programme of deriving cosmology (and geometry) from the arithmetic of natural numbers. The discovery of the irrationality of the square root of two destroyed this programme. Popper often lays stress on this point: in ***The Open Society*** (note 9 to chapter 6), in ***Conjectures and Refutations*** (chap. 2), and again in ***The World of Parmenides***. Here is the necessary background of his absolute proofs.

Fourth common point: absolute proofs were forgotten and neglected.

After Euclid, a proof was regarded as a proof only if it was correctly and explicitly connected with the axioms. Then, pre-Euclidian proofs which were without assumptions appeared in retrospect as incomplete, pre-mathematical

[3] See for instance Karl POPPER, *Conjectures and Refutations, The Growth of Scientific Knowledge*, London, Routledge & Kegan Paul, 1963, p. 221.

proofs. They were not regarded as belonging to another "mathematical area" (as Popper says), with another method and another background.

So, Popper's first care is to point out that absolute proofs did not deserve to be forgotten, for this pre-Euclidian mathematical area was a genuine mathematical area. And Popper's second care is to point out that absolute proofs did not deserve to be neglected, for something has been lost when this mathematical area was superseded by the Euclidian mathematical area.

Besides these four common points, there are also two very important differences between the absolute proofs described by Popper in the first text (***The World of Parmenides***, Essay 4) and the absolute proofs described in the two other texts (both ***The World of Parmenides***, Appendix and the conversation with Chmielewski): clearly, Popper does not speak of the same absolute proofs in the two cases. Then, the problem will be: how to make these differences compatible with the previous common points?

Let us consider the first difference.

In the first text, Popper asserts that some pre-Euclidian mathematical proofs were without assumptions, then absolute, because they were proofs by *reductio ad absurdum*. But in the two other passages, he asserts that some pre-Euclidian mathematical proofs were without assumptions then absolute, because they were intuitive proofs. Then, he gives two very different reasons for the same description.

Now, proofs by *reductio ad absurdum* and intuitive proofs seem to be not only two different kinds of proofs, but even two opposed kinds of proofs. The principle of *reductio ad absurdum* is that pure logic is trustworthy and that seeing the truth is not needed. On the contrary, appealing to intuition means that blind logic is insufficient or irrelevant.

Despite this opposition, may we suppose that in pre-Euclidian times some proofs by *reductio ad absurdum* on one hand, some intuitive proofs on the other hand, did belong to the same category of absolute proofs? May we conjecture some unity or complementarity between these two opposite kinds of proofs belonging to the same "mathematical area"? That is what I assert, but Popper himself nowhere says so. As far as I know, there is no text in which he speaks of the two opposed reasons together, in order to accommodate them. Furthermore, when he presents one of the two reasons, he does so in such a manner that the other seems excluded.

Here is a problem, which may be solved, to some extent, in considering the second difference between Popper's first text on absolute proofs and the two other texts.

What is this second difference?

According to what Popper suggests in the first text, pre-Euclidian mathematicians did not use *reductio ad absurdum* because they were strictly mathematicians, but because they were cosmologists. For *reductio ad absurdum*, Popper asserts, was the method of proof used by all the Presocratic thinkers (Parmenides, Zeno, Gorgias and so on) which were interested in cosmology. By contrast, intuitive proof appears, in the two other texts, as a strictly mathematical and even strictly geometrical method of proof.

In order to understand the link between the two differences, let us remember Popper's view about the distinction between mathematics and empirical sciences. This distinction concerns the use of logic. In mathematics, Popper says, we use logic mainly in order to prove, that is in order to transmit truth from premises to conclusions. In empirical sciences, we use logic mainly in order to disprove or refute, that is in order to retransmit falsity from conclusions to premises. When we are intending to prove, Popper adds, we should avoid establishing more than is necessary: we have interest in proving with the help of the minimum apparatus, in using the weakest possible logic, as it is recommended in intuitionist methodology. By contrast, when we are intending to disprove or refute, we have not to be afraid of being over-critical and using too strong means: then the best logic is the strongest one, the classical two-valued logic[4].

Then, if pre-Euclidian mathematicians were both cosmologists and mathematicians, they had both to disprove, as cosmologists, and to prove, as mathematicians: either to disprove with strong logical means, or to prove with weaker logical means. Now, *reductio ad absurdum* is a way of refutation which requires a strong, two-valued logic. On the contrary, an intuitive proof is a way of proving which requires a minimum logical apparatus. Here is the link between the two differences: pre-Euclidian mathematicians used *reductio ad absurdum* as cosmologists, and intuitive proofs as strictly mathematicians, even as strictly geometers.

Our problem was to understand how Popper may include two opposed kinds of proofs in the same category of absolute proofs, or proofs without assumptions. This problem may now be solved, but only to some extent. As a cosmologist, an early Greek mathematician might have to refute his own assumptions by *reductio ad absurdum*. Such a refutation may be said a proof without assumptions, since it consists precisely in proving that its own assumptions are false and must therefore be rejected.

On the other hand, a pre-Euclidian mathematician might also, as a pure geometer, have to prove some geometrical propositions in using a very minimum logical apparatus, in appealing only to some intuitive acquaintance with geometrical structures, without any other argument. Such an intuitive proof may equally be

[4] On this point, see Karl POPPER, *Objective Knowledge, An Evolutionary Approach*, Oxford, Clarendon Press, 1972, pp. 139–140 and 304–307

said a proof without assumptions, since it consists precisely in proving that assumptions are needless.

Now, when axiomatic method rules over mathematics, we may not conceive that the assumptions reveal themselves to be false or needless: only true and useful assumptions are permitted. But this situation is not inconceivable when mathematics is ruled by the method of conjectures and refutations.

Yet this is clearly not enough to solve entirely the problem. We have not only to understand why *reduction ad absurdum* on the one hand, intuition on the other hand, may give rise to proofs without assumptions, then absolute proofs. We have to understand why these two ways of denying assumptions (either because they are false or because they are needless) were united as two complementary ways in the pre-Euclidian mathematical area. We have also to understand why pre-Euclidian mathematicians were bound, in view of their problem situation, to behave either as cosmologists, or as pure geometers.

In order to answer these questions, we must consider some examples of absolute proofs in Popper's sense.

Unfortunately, Popper does not give any example when he expounds his first reason, concerning the "absoluteness" of some pre-Euclidian proofs by *reductio ad absurdum*. But what he says on the question irresistibly suggests one example, one unique example in fact, a well-known example, on which we have some very plain texts elsewhere in Popper's work.

Let us consider the proof, partly reported by Aristotle[5] that the diagonal of the square is incommensurable with the side of this square. The proof is clearly a *reductio ad absurdum*: supposing that the diagonal is commensurable with the side, it is proved that in the alleged ratio a ⁄ b the denominator b has to be both odd and even: the supposition leads to an absurd conclusion.

The main point, in Popper's mind, is that this proof was in the beginning, in pre-Euclidian times, a negative proof, a disproof, a genuine refutation. It was not (yet) the indirect positive proof that the square root of two is an irrational number: here is precisely the retrospect misunderstanding of the proof, the symptom that it has been forgotten and neglected. Rather it was a cosmological refutation, the falsification of the Pythagorean conjecture that all things are, in their essence, numbers or ratios of numbers. For if that conjecture is true, all measurement must consist in counting a certain number of natural units. Then, if counting is impossible, the conjecture must be false: pre-Euclidian *reductio ad absurdum* retransmitted falsity from consequences to premises.

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[5] ARISTOTLE, *Analytica Priora*, 14 *a* 26.

On the contrary, Euclidian *reductio ad absurdum* indirectly transmits truth from axioms to consequences. In Euclid's ***Elements***, *reductio ad absurdum* is nothing but an indirect means to deduce the proposition which ought to be deduced from the axioms, when this proposition could not be deduced directly. Thus a false supposition is voluntarily and cunningly tried in order to prove, through the absurdity it entails, that the opposite is needed.

The other point on which Popper lays stress about our first example is the fact that the proof has been formulated for the first time in the Pythagorean School. This point is essential for the understanding of its "absoluteness". For the proof was a refutation of arithmetization by arithmetical means. Its only pertinent assumption was the arithmetic of natural numbers and ratios of numbers, and it destroyed this assumption, in the most explicit way. Thus, our two requirements are entirely satisfied: this proof was without assumptions, then an absolute proof.

Popper gives two examples of the second kind of absolute proofs, intuitive proofs. The first example is well known. It is the passage of the dialogue ***Meno***[6] in which Plato proves that the square constructed over the diagonal of any given square has an area of twice this given square. It was a diagrammatic proof, which consisted in drawing a square with on diagonal, in extending the drawing to the square over this diagonal, and then showing to a young boy, unlearned in geometry, that the latter square contains four isosceles rectangular triangles equal to the two isosceles rectangular triangles contained in the former square.

The intuitive character of this proof is obvious. But this intuitive character precisely made Plato's proof misunderstood after Euclid. From an axiomatic point of view, compared with Euclid's demonstrations on the same point, it is simply a very incomplete proof. For instance, Plato does not prove the equality of the isosceles rectangular triangles. In retrospect, it seems that his proof was "without assumptions" only because he did not take trouble in clarifying his assumptions.

Then, why was this proof an absolute proof according to Popper?

In order to regard Plato's proof from the good point of view, we have to consider its context. The proof itself is the second part of a story, the story of a young unlearned boy who is supposed able to discover himself, without any teaching, the answer to a mathematical question. The young boy fails in the first part of the story, and succeeds in the second part. Why does he fail in the first part? Because he is not yet without any teaching, that is without assumptions. For Socrates has put to him this question: "Supposed that each side of a square is two feet, so that its area is four feet, how long the side must be in a square which has an area twice the former, then eight feet?" It is clearly assumed that this arithmetical question "how long is the line?" must be answered by counting and even calculating. In consequence, the first part of the story is a hopeless research. The young boy

[6] PLATO, *Meno*, 84*d*–85*b*.

answers first that the side must be double, so four feet long, then understands his mistake and searches for a side longer than two and shorter than four, answers three feet, understands his mistake and that he ought to search for a side longer than two and shorter than three, but does not see, obviously, how to find it.

But why does the young boy succeed in the second part of the story? Because he is now really without any teaching that is without the arithmetical conjecture that measuring consists in counting. The previous proof has established that this conjecture is false, Plato proves that it is needless. The young boy can get rid of it to solve any problem of measurement. His intuitive acquaintance with some geometrical structures is sufficient. “Intuitive”, here, is not opposed to “discursive”, as in Kant or Schopenhauer: it is directed against the need of arithmetic. “Intuitive” means that nothing but the consideration of geometrical figures is needed for measuring geometrical figures. So, the young boy has no longer to answer the arithmetical question “how long is the line?” His good answer is a geometrical one, describing the structural type of the line: the “diagonal”.

Thus, in so far as Plato’s proof was a purely geometrical proof, it was without assumptions in a pertinent meaning: it rejected as needless the only assumptions which matter about a problem of measurement. And in so far as this proof was an intuitive proof, it was without assumptions in an explicit way. The two requirements are satisfied: Plato’s proof was an absolute proof.

Thus, we have two examples of absolute proofs, concerning two different kinds of proofs: one example of *reductio ad absurdum*, one example of intuitive proof. And we may now see clearly the unity of these two kinds of proofs, their complementarity in view of the problem situation in pre-Euclidian mathematics, namely the breakdown of Pythagoreanism. According to the first example, absolute proofs of the first kind established the impossibility of an arithmetical (Pythagorean) cosmology. According to the second example, absolute proofs of the second kind established that this impossibility did not prevent the foundation of a cosmology, since what was impossible was at the same time needless. A geometrical cosmology, a cosmology whose elements were the shapes or figures themselves, was therefore possible. According to Popper, this new programme of geometrization was the great idea of Plato, and the true meaning of the famous inscription: “Nobody untrained in geometry may enter my house”.

We may also understand why absolute proofs of the first kind appeared as cosmological refutations, while absolute proofs of the second kind appeared as purely geometrical demonstrations. In fact, the two kinds of proofs had a cosmological meaning. But a geometrical cosmology was possible only if geometry was “pure”, that is autonomous, freed from any arithmetical assumption of commensurability or rationality.

As I have said previously, a second example of alleged intuitive absolute proof is given by Popper, namely Aristotle’s proof, in ***Metaphysics***, that the angle in a semicircle is in all cases a right angle[7]. According to Popper, Aristotle's proof is absolute, like Plato's proof, and for the same reason. Yet, it seems to me that a study of this second example may be useful, owing to its peculiar features.

The first peculiar feature is Aristotle's vivid consciousness of the intuitive character of his proof: "The conclusion, he says, is evident at a glance". What are we supposed to see at a glance? We are supposed to see that the angle in the semicircle is half of another angle, namely the "stretched angle" (as Popper says) which forms the diameter of the circle. Only two conditions are needed for that glance. First condition: we have to draw the line from the middle of the diameter to the vertex of the angle in the semicircle: this line divides our two angles. Second condition: we must know that the sum of the angles in a triangle is equal to two right angles. That is enough for seeing at a glance (according to Aristotle and Popper) that the angle in the semicircle is composed by two angles which are halves of the two angles of which the stretched angle is composed. So, the angle in the semicircle is in all cases a right angle.

The second peculiar feature of this new example is that Aristotle proposes a theory of the intuitive character of his proof. His aim, in ***Metaphysics*** Book 9, chap. 9, is to discuss the connection between what he names *energeia*, (*actus*, actuality) and what he names *dynamis* (*potentia*, potentiality). His geometrical proof exemplifies one aspect of this discussion. Thanks to the activity of the geometer when he draws the line from the middle of the diameter to the vertex of the angle in the semicircle, the truth which is contained only potentially in that figure becomes intuitively accessible.

The third peculiar feature is the most important in Popper's mind: remember that he intended to write, on Aristotle's proof, a paper whose title was "Aristotle's mathematics misunderstood".

For like Plato's proof, Aristotle's proof was bound to be misunderstood after Euclid. But this misunderstanding, according to Popper, concerns the text itself, which has been distorted, and must therefore be restored. In order to realize the distortion, let us consider Aristotle's proof in the English translation of David Ross. We may read this:

"Why is the angle in a semicircle in all cases a right angle? If three lines are equal – the two which form the base [the diameter, the stretched angle], and the perpendicular from the center [and not: any line from the center to the vertex] – the conclusion is evident at a glance."

Now the expression "and the perpendicular from the center" wrongly suggests, Popper claims, that Aristotle's proof failed to fit the promise of proving that the angle is right "in all cases": it only proved that one very special angle is right, and

[7] ARISTOTLE, *Metaphysics*, Book 9, chapter 9, 1051 a 26–28

needed afterwards a new theorem (namely Euclid, II, 21) in order to establish that if one angle is a right angle, all the angles must be so. This is traditional misunderstanding (from Alexander of Aphrodisias to David Ross) is a symptom of the common conviction, in post-Euclidian times, that an intuitive proof, “at a glance”, may not prove what must be proved in mathematics, namely some necessary and universal truth.

So, in the last months of his life, Popper was interested, against a tradition of scholars, in the rehabilitation of a proof that he described as “brilliant”, “impressive”, “beautiful”, “exciting”.

Now, this admiration towards Aristotle’s intuitive proof raises a problem. For Popper often criticizes intuition, and he often criticizes Aristotle. He has repeatedly said, in his whole work, that intuition, though it is an important source of knowledge, is not a reliable source of knowledge, not a mark of truth: then, intuition should not be regarded as a basis for any proof. Furthermore, Popper’s criticism against intuition is especially directed against the Platonic and Aristotelian theory of an infallible “intuition of essences”, of a grasping of the true essences by a kind of vision, thus “at a glance”. It is therefore surprising that he approves a practice of geometry which applies this theory.

This difficulty may be removed, I think, in considering what Popper writes in section 13 of ***The Self and Its Brain***, whose title is “Grasping a World 3 Object”. Popper explains that the false theory of intellectual intuition, of the vision of essences, contains, like any false theory, something which is true. Plato and Aristotle were wrong when they believed that we attain intellectual objects through a passive vision, but they were not entirely wrong since vision is in fact an active process. So we can translate their false theory into a true one, namely the theory that grasping an intellectual object is an active process which consists in making, re-creating this object. The point is that we do not grasp only what we have made, but much more: our action produces consequences of which we have not thought so far, new objects we have to discover in exploring the autonomous world to which they belong. We have to draw, to construct a geometrical figure in order to grasp or discover in it what we have not drawn, not constructed, what was “potentially”, as Aristotle says, in the figure. For what is important, in Popper’s mind, is to understand that the objects we are grasping in intuitive proofs are “mathematical facts”[8] which have an objective existence, independent from our grasping. Thus, Popper may accept mathematical intuition in a Platonizing sense, but not in an intuitionist sense, since intuitionism conflates the proof with the assertion to be proved[9].

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[8] *Objektive Knowledge*, p. 133.

[9] *Ibid.*, pp. 128 and 139.

Let us sum up what we have learnt from these examples of absolute proofs:

1- They were “without assumptions” in two ways, either because they proved that these assumptions were false, or because they did not need them for proving.

2- False or needless, the assumptions were of the same kind: they were arithmetical assumptions.

3- Thus, the absolute proofs established the impossibility of an arithmetical cosmology and the possibility of a geometrical cosmology.

Now, according to Popper, it is precisely in view of the perfect achievement of this geometrical cosmology in Euclid’s ***Elements*** that absolute proofs became forgotten and neglected. The point sounds paradoxical and must be considered.

Popper repeatedly asserts that Euclid’s **Elements** was originally intended as an attempt to solve systematically the problems raised by Plato’s cosmological programme of geometrization. In other words, the aim of this treatise was the systematic realization of Plato’s and Aristotle’s (and others) absolute proofs. But this was done with such success that the problems, having been so well solved, disappeared and were forgotten. And when the problems were forgotten, the significance of absolute proofs was forgotten too.

This explanation by the success is not as paradoxical as it seems. For Euclid’s success inevitably consisted in working out a completely autonomous geometry, freed from any arithmetical assumption of commensurability or rationality, and thereby protected against incommensurability and irrationality. But this geometry was so completely autonomous that people might well forget in what respect it was autonomous, and consider it as only geometry. Solving the problems had removed the memory of these problems. Then, ***Elements*** no longer appeared as a cosmological treatise, but as a textbook of pure geometry.

Pre-Euclidian mathematical problem situation was the opposition between two cosmological theories: geometrization against arithmetization. When this problem situation is forgotten, a new mathematical area appears. This new mathematical area is no longer divided into conflicting theories on the world, but into several branches. Geometry becomes a simple branch of mathematics, side by side with an arithmetical branch in which irrationals may be accepted as a peculiar kind of numbers. Each branch of mathematics is concerned with its own assumptions: the fact that arithmetical assumptions are needless in geometry is no longer a pertinent fact. What is pertinent, now, is only the fact that every geometrical proof depends on geometrical assumptions: then, axiomatic method supersedes method of conjectures and refutations. In pre-Euclidian mathematical area, logic was used in two inverse ways, in order to disprove as in order to prove, and some proofs might be absolute proofs. In post-Euclidian mathematical area, logic is only used to prove, and no proof may be an absolute proof: every proof is relative to the axioms.

In that change, Popper says, pre-Euclidian mathematics has not only been forgotten, it has been neglected. This point concerns above all the old absolute proofs, in their two forms, proofs by *reductio ad absurdum* and intuitive proofs: their original meaning is misconceived. In pre-Euclidian mathematics, these two kinds of proofs were united by their complementarity: each of them was needed to reject the pertinent assumption in a specific way. In contrast, they become regarded after Euclid as two conflicting kinds of proofs. Any argument in favour of *reductio ad absurdum* must emphasize the necessity of blindly relying on logic, then the uselessness of intuition for proving. Any argument in favour of intuition must inversely emphasize the necessity of constructing in our mind what is proved, then the invalidity of *reductio ad absurdum*. So, if the new mathematical area is no longer divided in two conflicting theories on the world, it is divided in two conflicting theories on mathematics, two opposite conceptions of what mathematics has to do.

In order to escape this division, post-Euclidian mathematicians are bound to diminish the significance of the two kinds of absolute proofs: that is another manner to neglect them. For instance, in pre-Euclidian times, *reductio ad absurdum* was required to disprove, and because criticism requires strong logical means. In the new mathematical area, this kind of proof is used to positively prove, then as an indirect way of proving, for lack of a direct one: it is an obvious depreciation

A similar fact may be noted about intuitive proofs. In the old mathematical area, intuition was required to establish, positively, the autonomy of geometry, its own ability to answer without counting any question of measurement. In the new mathematical area, this autonomy of geometry has no longer to be established. Proving is only deducing from the axioms. Then, intuition is regarded as being just a help fordeduction: it is another obvious depreciation. Something has been lost, according to Popper, when pre-Euclidian mathematics was forgotten.

As a conclusion, I wish to remember what Bertrand Russell says, in his ***Autobiography***, about axiomatic method. He tells that he began Euclid at the age of eleven, with his brother as tutor. This was a great happiness, he says, though this happiness was spoiled by a regrettable fact: “I had been told that Euclid proved things, and was much disappointed that he started with axioms”. Then Russell adds: “At first I refused to accept them [the axioms] unless my brother could offer me some reason for doing so, but he said: ‘If you don’t accept them we cannot go on’, and as I wished to go on, I reluctantly admitted them *pro tem*.” And Russell concludes: “The doubt as to the premises of mathematics which I felt at that moment remained with me, and determined the course of my subsequent work.”

Thus, Russell’s early reluctance to axiomatic method was a kind of doubt as to the premises or assumptions of mathematics: perhaps a doubt concerning the truth of these assumptions, perhaps a doubt concerning their usefulness. But Russell has never thought, afterwards, that a mathematical assumption may be mathematically proved to be false, or needless, that is by a mathematical proof: what happened, according to Popper, in early Greek mathematics. In other words Russell has never thought that mathematical assumptions may be, not axioms, but conjectures. Then, his reluctance was somewhat different from Popper's reluctance.

However that may be, Popper explains Russell's early reluctance by a kind of "instinct" which Russell, unfortunately, did not follow. And I conclude in quoting what he intended to write about this point in his unfinished paper on Aristotle's mathematics: "Bertie's brother, he claims, was misinformed, and he misinformed Russell: there are geometrical (and other) proofs that need no assumptions: absolute proofs."[10]

[10] *The World of Parmenides*, p. 297.

Critical Rationalism and Decision Theory

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The two main areas of so-called “normative” decision theory are decision making under uncertainty, where Bayesianism (or subjective expected utility theory) is the most prominent approach, and decision making under risk, where the v. Neumann-Morgenstern (NM) theory is widely accepted. Normative decision theory is normative in the sense of a hypothetical imperative: it is, supposedly, in the interest of a decision maker to decide in line with the theory. This claim seems to be false in the case of Bayesianism. The NM theory, in contrast, is closely linked to the propensity interpretation of probability. It seems that, at least in simple decision problems, an adherent of the propensity theory would accept the claim that it is in his own interest to decide in line with the NM theory.

The physical motivations for a propensity interpretation of probability

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Karl R. Popper has been perhaps the first modern philosopher of science to realise that ‘quantum mechanics [(QM)] and probability theory share one peculiarity. Both have well established mathematical formalisms, yet both are subject to controversy about the meaning and interpretation of their basic concepts. Since probability plays a fundamental role in QM, the conceptual problems of one theory can affect the other.’ (Ballentine, 2016). A fact that only recently found some support in the physics community.

As a matter of fact, with the pivotal work of John von Neumann (1932), QM was provided with a consistent axiomatic formulation: a physical system is represented by a vector Ψ (actually a ray) in a complex Hilbert space, which encompasses all the physical variables. What is, however, the ontological status of Ψ is still to date object of heated debate.

Roughly in the same year, probability theory underwent a systematization (as an axiomatic measure theory on a Boolean algebra), mostly thanks to Kolmogorov (1933). Popper’s *Logik der Forschung* (1934) - although it owes its fame mainly to methodological issues and the novelty of falsification, therein proposed - was extensively devoted to probability (in particular to the frequency interpretation, see e.g. Miller 2016) and to some problems in quantum theory.

QM is indeed related to probability as follows: given a certain experiment with experimental settings x and a possible outcome a, quantum theory allows to compute the probability $p(a|x)$ of finding that outcome. Yet, there is to date no unique and satisfactory explanation of the mechanism that leads to the observation of a specific outcome in a certain experimental run (this has gone down in history as the quantum measurement problem, see e.g. Brukner 2017). On the other hand, despite the well-defined laws of formal calculus of probability, the symbols $p(a|x) = r$ (read: “the probability of a given x”; and where r is a real number between 0 and 1), as well as the arguments a and x are in general left open to interpretation (see e.g. Popper 1967, eight thesis).

For about 60 years, Popper has been one of the foremost critics of the “orthodox” Copenhagen Interpretation of Quantum Mechanics, the vastly accepted anti-realistic, subjectivist and instrumentalist viewpoint on how to interpret quantum formalism. Popper, indeed, strove for an objectivist, realistic interpretation of quantum theory, and only in his late years he gathered the support of illustrious physicists (see Del Santo 2017). At the same time, at least since 1934, Popper fought against subjectivist interpretations of probability (which interpret probability as a rational degree of belief of an event to occur, based on a subjective lack of knowledge). It is thus not a case that Popper's ideas on quantum mechanics and in probability co-evolved in a way that is impossible to disentangle. Despite the severe criticism levelled by Milne to Popper's own *propensity interpretation* of probability (PIP), he rightly encapsulated the importance of the relation that bonds quantum theory and the propensity interpretation in Popper's view: 'The support is mutual: propensity theory helps us to understand quantum mechanics, quantum mechanics provides evidence for, or naturally gives rise to, a propensity interpretation.' (Milne, 1985).

For many years, Popper had adhered to the (objective) frequency interpretation of probability (specifically as expounded by von Mises, and improved by Popper himself) that regards probability as the relative frequency of events *a* among the events *b*. Yet, from 1950 (see a letter from Popper to S. Körner on 21/04/1956. PA, 48/27) Popper started developing the PIP. Propensities were first presented in 1953, in the course of lectures (published in C. A. Mace, 1957), and eventually presented to the academic world, in 1957, at the Ninth Symposium of the Colston Research Society in Bristol (Popper 1957). There Popper started delineating the problems of the interpretations of probability, related to quantum theory. A major concern was how to treat the probability of a single event, something that frequency interpretation leaves uninterpreted. However, it ought to be stressed that the reasons that led Popper to abandon the frequency interpretation are not to be sought in an alleged untenability of the frequency interpretation. Indeed, Popper always maintained the frequency interpretation to be 'immune to usual objections' (Popper 1959); the actual motivation came, as anticipated, from physics. In fact, single-case probability 'is of importance in connection with quantum theory because the Ψ-function determines the probability of a *single electron* to take up a certain state, under certain conditions' (Popper 1957). In a second and more technical (not about physics though) paper on PIP, Popper explicitly stated: 'I gave up the frequency of probability in 1953 for two reasons.

(I) The first was connected with the problem of the of quantum theory.

(2) The second was that I found certain flaws in my treatment of the probability of single events (in contrast to sequences of events)

[…] the first point […] was the first in time and importance: it was only after I had developed […] the idea that probabilities are physical propensities, comparable to Newtonian forces, that I discovered the flaw' (Popper 1959).

It was in fact an attempt to interpret in a realistic and objective view the quantum double-slit experiment that led Popper to the conviction that 'probabilities must be "physically real" -they must be propensities, abstract relational properties of the physical situation […] and real not only in the sense that they could influence the experimental result, but also in the sense that they could, under certain circumstances (coherence), interfere, i.e. interact with one another.' (Popper 1959).

This revolutionary idea, despite having been presented for the first time in the course of what has been defined as 'the first major event after World War II" about foundations of quantum mechanics (Kožnjak, 2017), was completely ignored by physicists for at least a decade.

I will show that it is only with the publication of Quantum Mechanics without the Observer (Popper 1967) that Popper (i) for the first time fulfilled his original aim of applying propensity interpretation to quantum physics in a comprehensive way and, consequently, (ii) he received the first attention by the community of physicists concerned with quantum mechanics. I will try to offer an overview on the main reactions, both positive (D. Bohm, H. Bondi. B. van der Waerden, F. Selleri) and negative (J. Bub, P. Feyerabend, P. Milne). This reconstruction will be based on unpublished private correspondence between Popper and his major interlocutors among physicists, retrieved in Popper's Archive in Klagenfurt (Austria), as well as through a thorough analysis of the literature available.

Propensity interpretation has been expounded by Popper in a number of works, throughout about four decades, and went through many refinements. D. Miller (1991, 2016) has rightly highlighted that the PIP was not developed in its ultimate and most sophisticated formulation until the *Postscript* (Popper 1982), wherein propensities were eventually formulated as the first objective interpretation that could consistently deal with single case probabilities -and also frequency of events in the long run. According to this view, it is eventually the whole universe, everything that lies within the light cone of the considered event, that can possibly contribute to influence the probabilities (propensities). Although I fully agree with this view, I will focus on the local condition that determine physical experiments, namely the local experimental conditions that are directly controllable in scientific practice. Therefore, I maintain that for this purpose the PIP was already fully developed as it was expounded by Popper in his *Quantum Mechanics without the Observer* (Popper 1967).

I will then attempt a discussion of the main features that relate propensities to fundamental aspect of physics. In this regard, I will discuss, for instance, the role that determinism plays in quantum mechanics and support the fact that 'propensities […] can assume non-extreme values only in an indeterministic world.' (Miller 2016). Moreover, I will review the different definition of physical reality that Popper assumed in different period as the underlying assumption for a realistic interpretation of probability. In fact, it seems that in his first work on PIP (Popper 1957, 1959), Popper used (quantum) physics as a mere triggering motivation for a revision of the issue of single-case probability, but the ontology of the propensities was scarcely outlined. At first, Popper defined the propensities as 'abstract relational facts [that] can be "causes" and in that sense physically real' (Popper 1957). The ontology of propensities was however thoroughly discussed only later, when Popper maintained that it is not solely the one-direction causation to define what is physically real, but reality is attributed to an entity ‘if it can be kicked, and it can kick back’ (Popper, 1967, eight thesis).

Coming to quantum theory, the ontological problem in the microscopic world has puzzled physicists for over nine decades. Historically, the most prominent realistic class of interpretations of quantum theory was developed with the so-called *hidden variable* models. These all share the feature of ascribing the whole oddity of quantum theory (wave-particle duality, entanglement, etc.) to the existence of underlying hidden variables (HV), λ, not experimentally accessible (either in principle or provisionally). Albeit HV were firstly introduced to restore classical determinism (i.e. $p(a|x,\lambda) = 0$ or 1), and therefore supposed to account for the observed probabilistic behaviour of quantum mechanics, models of increasing complexity flourished throughout the 1960s-1970s, which did not rely on strict determinism, but still maintained a realistic description of the physical world (realism in this context means that physical objects have well defined values of all the physical variable at any instant in time). Following the idea of L. de Broglie of a pilot-wave guiding quantum particles, Bohm proposed the first fully developed model of QM in terms of deterministic HV. However, contrarily to what many physicists believe, Bohm himself was ready to abandon strict determinism. Popper and Bohm had a long and fruitful intellectual relationship (not free from tensions though), and their views on interpretations of quantum mechanics were convergent to a large extent, even more than Popper himself had possibly realised, according to Bohm (letter from Bohm to Popper on 13/07/1984. PA 278/2). I shall therefore draw a parallel between Popper’s physical propensities and hidden variables, showing that Popper’s physical probabilities (propensities) -even though, to my knowledge, never stated explicitly by Popper- are not only strongly related to the interpretation of quantum mechanics, as ceaselessly stated by Popper, but, given the fact that they are granted a status of physical reality, propensities are actually a form of hidden variables. Popper’s interpretation of quantum mechanics thus results composed of two elements: a corpuscular ontology of directly detectable particles, and physically real propensity fields (hidden variables) that can be indirectly manipulated by altering the experimental conditions. Admittedly, Bohm noticed that in his (non-deterministic) HV model one can regard ‘the stochastic movement of the particle as affected by a field of propensities’ (letter from Bohm to Popper on 13/07/1984. PA 278/2). In this light, propensities as hidden variables survive the fundamental limitation imposed by the Kochen-Specker theorem (Kochen and Specker, 1967), which states the incompatibility of non-contextual hidden variables (i.e. independent of the choice of the disposition of the measurement apparatus, called context) with quantum mechanics.

In conclusion, the aim of the present paper is to provide an historical account of the development of Popper’s propensity interpretation of probability, with a focus on the essential relation with (quantum) physics, as well as a brief reconstruction of the resonance of Popper’s proposal in the community of quantum physicists.

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Propensity Probability and Its Application of Knowledge in Ifa

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This work is to grow an appreciation for Karl Popper's idea of propensity probability. Two major tasks are to be accomplished here. First is to show the pragmatic evaluation of propensity probability in the practice of Ifa literary corpus. Second is to establish that Popper's idea of severe testing of hypothesis with his disposition to single-case probability is a derivative of Bayesian probability. The aim is to establish that Popper's propensity probability is inductive in nature, yet to rise with it to a higher plane of appreciation of Popper's critical rationalism. To accomplish this I accept that Popper's anti-justificationism is unstable, but I inject the least amount possible of justificationism into the needed rescue of critical rationalism.

I

Popper developed a bold form of propositional calculus he termed propensity probability. This differs from the purely frequency interpretation of probability that deals with statistics of sequence. Two main issues are at stake in Popper's propensity probability. First is his disposition to single-case probabilities. Second is how Popper's propensity theory of probability is made explicitly in relation to environmental factors.

Popper had rejected the frequency theory of probability for offering an account of probabilities with respect to statistical sequences. The frequentists, in Popper's view, failed to consider probability in terms of a single case, but only in terms of infinite sequence of events. Popper's propensity probability sees results of events not in terms of the sequence, rather in terms of the factors that conditions the result of such events. The crux of Popper's propensity theory of probability, therefore, is that the probability of the result of every kind of event is conditioned, dependent or determined by the factors of the physical environment at that point in time, and not by the result of the frequent sequence.

There is an implicit symmetry of science and metaphysics in Popper's propensity probability. On the one hand, the single-case propensity is a property of experimentation with which severe testing of hypothesis can be achieved. On the other, there are ontological properties evident in propensities of physical factors that determine the outcome of the probability of any happenings given certain initial conditions. Both are mutually accommodated in Popper's propensity probability. As David Miller rightly posits, Popper's propensity probability is significant for quantum theory and for a new metaphysics of nature.

There is an underlying practical relevance of Popper's propensity probability to a probabilistic cognition of the traditional Yoruba knowledge of *Ifa*, to which this study partly looks into. This is intended is to bring to fore an indigenous knowledge system of *Ifa*, which emphasizes on both the scientific and the metaphysical, as Popper's propensity probability does. *Ifa* is structured in a binary format in its organisation and application of knowledge, which can be tested in a single-case manual experiment. The outcome of every manual throw of the *ọ̀pẹ̀lẹ̀* in *Ifa* corpus is embedded in the mathematics of the binomial probability distribution, and it is determined on a number of physical/metaphysical/spiritual factors.

The *ọ̀pẹ̀lẹ̀* is *Ifa's* divining chain, organised in four binary pairs, placed on the divination tray named the *opon ifa.* The ọ̀pẹ̀lẹ̀is an 8 pieces of coins chained together. When the ọ̀pẹ̀lẹ̀is tossed on the divination tray *"opon ifa",* each piece has only two mutually exclusive outcomes and all eight have a total of 256 possible combinations. The complexity here can be made simple by an example of the outcome of tossing a coin. You get a "head" or a "tail", not both and not neither. With this, one has two mutually exclusive and exhaustive possibilities. Each possibility is of a p or q chance. This is different from obtaining ½. The two possibilities have a total chance of one (p + q = 1). But if one tosses the same coin 8 times, or one tosses 8 (equally weighted) coins once, you get 2x2x2x2x2x2x2x2 = 2 to the power 8 = 256 possible outcomes. But it is not the relative frequency of the sequence of events that is important in *ifa*, it is the result of divination, which is largely determined by factors external to the ọ̀pẹ̀lẹ̀.

In the Ifá system, a disposition to probability is used in order to remove any bias that may arise from the preconceptions of the priest who tosses the ọ̀pẹ̀lẹ̀. Ordinarily when the ọ̀pẹ̀lẹ̀is tossed the priest uses the appropriate poems and aphorism called the *odu-ifa* to achieve a divination for a given situation. As earlier mentioned, the ọ̀pẹ̀lẹ̀ divination chain consists of eight disc-like seed, attached together by a string, each having two distinctly differentiable sides. The divination chain is cast by gently swinging and then throwing it onto a flat surface. The manual swinging randomizes the arrangement of the discs, so that when the chain is thrown to the ground, a random pattern manifests. Due to the fact that the ọ̀pẹ̀lẹ̀ has eight disks that can manifest dichotomously, it works as an eight bit random number generator with the capacity of generating 256 distinct patterns. Each pattern corresponds to an *odù-ifa*, each of which describes key social circumstances and various prescriptions relevant to each circumstance.

The underlying process and methods of *Ifá* divination share many similarities with the processes in Popper's propensity probability. Essentially, both processes represent real life situations that understand that the outcome of any events is not determined by the relative frequency of the number of sequence, which sees the world as a physically closed society, but are categorised by genuine freedom and creativity. Unlike the relative frequency probability approach to mathematical modeling which is deterministic in its assumptions and results, propensity probability of Popper captures the essential stochastic nature of real life and is therefore capable of modeling real life without any unfounded assumptions of determinism. In a similar way, the *Ifá* oracular system models the random nature of various life events by introducing a probability distribution defined by the divination chain.

II

In spite of the application of Popper's propensity probability to practical life situations, this second section of the paper examines the technical details of the account of Popper's theory of severe testing, in an effort to bring out its relationship to Bayesianism. In Popper, let the expression 'prob(E, T)' stand for *the probability of the evidence "E" given theory "T"*. Popper defined the severity of a test by comparing the likelihoods of the evidence "E" given both the new and the older background theories, vis., "T", and "B": That "E" is a severe test of "T" with respect to background theory "T", or thus that S(E, T, B) holds, demands by definition that prob(E, T) is much greater than prob(E, B).

The Bayesian explanation for this would be as follows. Let us take the case where from "T" (with any auxiliaries) we can deduce "E"; then prob(E, T) = 1, or is very high.

Now considering the background theory B and E, there are two cases:

(1) If on the one hand "E" is highly likely given "B", i.e., either prob(E, B) = 1 or at least prob(E, B) is very high, then the Bayesian probabilification of T given E is low. In Popperian terms, either S(E, T, B) = 0 or at least S(E, T, B) is very low. In this case, "E" gives roughly the same degree of support to "T" as to "B" and is not well able to decide between them.

(2) If on the other hand "E" is quite unlikely given "B", i.e. either prob(E, B) = 0 or at least prob(E, B) is very low, then the Bayesian probabilification of T given E is high. In Popperian terms, either S(E, T, B) = 1 or at least S(E, T, B) is very high. In this case, "E" gives quite different degrees of support to "T" and "B" and is well worthy of consideration to help decide between them.

This paper steps away from Popper to the extent of employing Bayesian principles. However, this is intended to clearly re-establish Popper's idea of a severe test. The Bayesian principles, in one way, suggest that Popper's anti-inductivism needs to be relaxed. It is by making probable the improbable-seeming that a theory manages to be susceptible to a severe test; and it is when the theory passes such tests that it can be looked upon with favour.

Another way to consider this is an instability in Popper's own anti-justificationism. Popper sought not any-old kind of favouring of a theory but rather a justified kind of favouring of a theory. Favouring a theory because it has passed many tests none of which is severe would be a mistaken kind of favouring in Popper's own view. Only favouring a theory because it has passed a good number of severe tests (and has not yet failed any tests) is justified, according to Popper. So Popper must to some extent have been oriented to justification. The account of Popper that we need is one that minimises the needed justificationism.

Independence (Probabilistic) and Independence (Logical)

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This paper is concerned with the problem of defining the relation of *probabilistic independence* in a way that reveals how, if at all, it is a generalization of one of the various relations of *logical independence* that are familiar to logicians. It is well known that in this regard not all is well within the classical theory of probability, in which probability is intrinsically a singulary function, or within the modern variant of that theory that was given currency in the axiom system of Kolmogorov (1933). More incisiveness might have been hoped for in those subtler axiomatizations in which a relative (binary) probability function is adopted as primitive, especially the axiomatic theory analysed in appendix *iv, and presented systematically in appendix *v, of *The Logic of Scientific Discovery* (Popper 1959). But it turns out that the classical definition is even less satisfactory here than it is in its classical setting. Near the end of his life Popper became aware that something was amiss, and in the final appendix to the 10th edition of *Logik der Forschung* (Popper 1994) he proposed an alternative definition of probabilistic independence. This appendix, on which Dorn (2002), §2.4, reports, has, predictably enough, been neglected, anyway in English-language publications (by me as much as by anyone). A recent paper (Fitelson & Hájek 2017) devoted to the same topic does not notice it. It is the primary purpose of the present paper to show to what extent Popper's definition is an advance on the definitions commended by Fitelson & Hájek. But I shall also address once more the question of what logical significance the relation of probabilistic independence en joys, and shall broach the idea that there is a genuinely attractive alternative.

Probabilistic independence

In the classical theory, where absolute probability $\mathsf{p}(a)$ is primitive, the *probabilistic independence* of elements a, b in the domain of the p is defined by

DU $\quad \cup(a, b) \quad \leftrightarrow \quad \mathbf{p}(ab) = \mathbf{p}(a)\mathbf{p}(b).$

This is a symmetric relation. An immediate and untoward consequence of this definition is that every element b is independent of every element a for which $\mathsf{p}(a)$ = 0; in particular every b is probabilistically independent of the contradictory (or zero) element s, even though b, being deducible from s, is logically dependent on s. It is rather less immediate that every element a for which $\mathbf{p}(a) = 1$ is independent of every element b. But if $\mathbf{p}(a) = 1$, then $\mathbf{p}(a \vee b) \leq \mathbf{p}(a)$, whence by the addition and monotony laws $\mathsf{p}(b) \leq \mathsf{p}(ab) \leq \mathsf{p}(b)$, and therefore $\mathsf{p}(ab)$ =

p(*a*)**p**(*b*). This implies that the tautological (or unit) element *t* is probabilistically independent of every element *b*, even though *t*, being deducible from *b*, is logically dependent on *b*. These anomalies in the classical theory are perfectly well known, and some effort has been made to surmount them. There is, in particular, a stronger asymmetric sense of the *probabilistic independence* of *a* from *b* that uses the relative (often called *conditional*) probability **p**(*a, b*), which, provided that **p**(*b*) ≠ 0, is defined by **p**(*a, b*) = **p**(*ab*)/**p**(*b*). This stronger definiens is

DV   V(a, b)   ↔   **p**(a, b) = **p**(a).

Since the first term on the right here is not defined if **p**(*b*) = 0, the conclusion that every element *a* is probabilistically (but not logically) independent of *s* is thwarted. It remains true, nonetheless, that the element *s* is independent of any element *b* that is not identical with *s* itself, and that the element *t* and any element *b* for which **p**(*b*) ≠ 0 are mutually independent.

In appendix **XX* to *Logik der Forschung* Popper acknowledged these shortcomings of the classical definitions: *s* and *t* ought not to be probabilistically independent of other elements. But he thought that it must be possible for some other contingent statements with zero or unit probability - his examples of the latter were ‘There exists a white raven’ and ‘There exists a golden mountain’ - to be counted as probabilistically independent of each other. His solution was to introduce two new relations of *weak independence* W(*a, b*) and *independence* I(*a, b*),

DW   W*(a, b)*   ↔   **p**(*a, b*) = **p**(*a, b´*)
DI   I(a,b)   ↔   W(a, b) & W(a´, b) & W(b, a) & W(b´, a)

(Popper called these definitions D_0 and D_1 respectively). The main theorem (*Hauptheorem*) of appendix **XX* demonstrates that neither *t* nor *s* bears the (symmetric) relation I to any element. Fitelson & Hájek also dismiss DU because it requires that ‘anything with extreme probability has the peculiar property of being probabilistically independent of *itself*’; this is a state of affairs that, they judge, may perhaps be acceptable for contingent events with unit probability (§6), but it is intolerable for contingent events with zero probability (§8). They propose, as successors to U(*a, b*), two other definientia of the probabilistic independence of *a* from *b*: the weaker one is just V(*a, b*) released from the restriction that **p**(*b*) ≠ 0, while the stronger one is just W(*a, b*) itself. Popper had shown that, among other things, W(*a, b*) implies V(*a, b*), and hence that **p**(*a, b´*) = **p**(*a*); and, near the end of the appendix, that I(*a, b*) implies U(*a, b*), so that independence, newly defined by DI, ‘implies classical independence’. It is easily checked that none of U(*a, b*), V(*a, b*), W(*a, b*), I(*a, b*) is equivalent to any of the others, and hence that they provide stronger and stronger definitions of independence. It is evident too that I, like U, is a symmetric relation, whereas V and W are asymmetric. Fitelson & Hájek appear to regard this as a discovery, rejoicing that ‘on a Popperian account of independence’, as they audaciously label their approach, ‘we must specify a *direction* of independence’ (§8).

Popper did not give an explicit construction to show that the relation $\mathtt{I}(a, b)$ can obtain between two contingent elements *a, b* with probability 1, such as the existential statements lately mentioned, but it is easily done. There are some other nice results that he did not assert (let alone prove). One, which he would surely have been pleased about is that, according to DI, no element *a* is independent of itself, or of its negation *a´*. He would perhaps have been less pleased to learn that probabilistic independence $\mathtt{I}(a, b)$ may obtain even when the element *b* logically implies the element *a*; that is, when they are, in the usual sense, *logically dependent*.

Logical Independence

If we are to decide between the relations $\mathsf{V}(a, b)$ and $\mathsf{W}(a, b)$ commended by Fitelson & Hájek, which may be too weak, and the relation $\mathtt{I}(a, b)$ commended by Popper, which may be too strong, we shall have be clearer about the job that probabilistic independence is being asked to do. At one point Fitelson & Hájek note that 'We may well want inductive logic, understood as probability theory, to be continuous with deductive logic' (§6). Popper too, has in several places (for example 1957, point 3) been inclined to 'identify logical independence with probabilistic independence', and the vague idea that logically independent elements are probabilistically independent, or approximately so, lurks unacknowledged behind many judgements of probabilistic independence. But there are several different kinds of logical independence too, and some of them turn out to be more appropriate than others.

Simple (logical) *independence* is simply non-deducibility. It is what is asserted when it is said that the axiom of parallels is independent of the other Euclidean axioms, and that the axiom of choice AC is independent of ZF set theory. More generally, a set K of statements is *simply independent* if and only if no *a* in K is deducible from the other elements. There are two well known extensions of this idea. The set K is *completely independent* (Moore 1910) if and only if for every subset *A* of K, all the elements of *A* can be true while all the other elements are false. It is immediate that a completely independent set K is both consistent and simply independent. It is to be noted that Popper did not attempt to show that the four conjuncts in the definition DI of the relation $\mathtt{I}(a, b)$ are consistent, which of course they are, or simply independent, which they are, or completely independent, which they are not: it is impossible that exactly one of the four conjuncts $\mathsf{W}(a, b)$, $\mathsf{W}(a´, b)$, $\mathsf{W}(b, a)$, and $\mathsf{W}(b´, a)$ is true. Although this fact deftly intimates that the relation $\mathsf{W}(a, b)$ is needlessly weak, the virtues of complete independence are really far from obvious. It is known that, in classical logic, there are infinite sets that are not equivalent to any completely independent set (Kent 1975). What is decisive, however, is that, unless all probabilities equal 0 or 1, there exist completely independent sets whose elements in pairs are not probabilistically independent, even according to DU (Popper & Miller 1987, note 2). A brave attempt to solve this problem has been made by Mura (2006).

A set K is *maximally independent* (Sheffer 1926) if and only if it is simply independent and no (non-tautological) consequence of any element *a* in K is derivable from the other elements. The elements of a maximally independent K have no content in common, and are what was traditionally called *subcontraries*. Tarski (1930, Theorem 17) showed that, in classical logic, every set K is equivalent to a maximally independent set. Maximally independent elements are not in general probabilistically independent (Popper & Miller *ibidem*, Theorem 1). Nonetheless, it is attention to maximal logical independence that looks like the way forward.

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From cosmic paths to psychic chains

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Although I had been aware of Popper's work for well over 50 years, it is surprising – to me – that I had never thought of trying to reconstruct his relation to mathematics, and even more so, to its foundations. So I started by looking at what may have been his orientation to mathematical problems around 1920. Popper writes of this time: "At the University … I soon gave up going to lectures, with the exception of those in mathematics and theoretical physics." … "Only the Department of Mathematics offered really fascinating lectures. The professors of the time were Wirtinger, Furtwängler, and Hans Hahn. All three were creative mathematicians of world reputation … All these men … were demigods"[1]. [Freud had recorded a similar homage: if not directly towards mathematics, certainly towards what he took to be the methods of the sciences. The man that Freud had described – while he too was in in his twenties – as his "household God" was Hermann von Helmholtz.]

That's a good start, but what was Popper looking for? "I studied mathematics because … I thought that in mathematics I would learn something about standards of truth". "Standards of truth" rather than "standards of proof" already indicates a programme set within a somewhat wider domain than simply that of mathematics. [The text of his intellectual autobiography was written a half century after the experiences being described; but the text clearly has a high regard for historical accuracy]. In any case, at least we can say that a serious concern for the nature of mathematics was, in Popper's case, longstanding.

Already at this early period Popper had developed his distinction between critical thinking and dogmatic thinking, dogmatic theorizing representing "a stage that was needed if critical thinking was to be possible. Critical thinking must have before it something to criticize"[2]. If one were now to locate some of the earliest "dogmatic thinking" at - and before - the time of the Ionian Greeks, it might not be too hasty to attempt to reformulate this distinction as that between metaphysics and the progressive critical articulations of science[3].

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[1] KRP: *Autobiography* [UQ pp39–40].

[2] [UQ p41].

[3] A reformulation that would also raise questions concerning a critical articulation of mathematics.

Popper regularly comes back to give explanations – at a descriptive level – of his concern with “standards of truth”. “Learn (such standards) from the way in which scientists and mathematicians proceed”, giving special attention to ”the mathematical background of physics”: progress in science “constantly create(s) new theoretical and mathematical means for more nearly approaching” the truth[4]. As he describes his notion of metaphysical research programme, Popper makes the following comment: there have been “changes down the ages in our ideas of what a satisfactory explanation should be”[5]. This applies with special pertinence to questions of what can be called the metaphysics, or foundations, of mathematics. A central question here is whether or not formulations of mathematical problems constitute the leading ideas in the development of the philosophy of mathematics. In sketching the nature of this question, I will look at the proposals put forward by Arpad Szabó and by Popper as regards the relations of early Greek [Ionian and Eleatic] cosmology and mathematics.

Popper’s relation to Szabó (as well as to the work of Imre Lakatos) itself needs some outlining. Popper’s early distinction between deductive methodology in mathematics and deductive methodology in science needs much revision in the light of Lakatos’s work on the centrality of a dialectic of refutation in the logic of mathematical discovery – on the importance, that is, of counter-examples to proof-claims in the progressive development of mathematics. I do not know when Popper first became aware of Szabó’s work[6] – but this would raise a new perspective on the development of critical – or “hard hitting” - arguments in Eleatic philosophy, including problems in the philosophy of mathematics. These issues could be pursued piecemeal, but many of these questions can be put into a single perspective by introducing what Popper called his theory of transference.

Popper proposes a theory of transference – one that is central to his theory of science, and to his account of mathematics. In fact he proposes at least four such theories, and they stand in relation to each other in the form of a critical progression. This is an important method for Popper: “one of my principle methods of approach”[7]. He describes the method he proposes as follows: (1) “in logical problems” to “translate *all* (my italics) the subjective or psychological terms … into objective terms”. This gives a means of transformation from Psyche to Logic. It should be noted that this involves a one-way relation. (2) Popper then extends this initial parallelism to a relation of transference between problems of scientific method and problems of logic. (3) He then augments this by a further

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4 [UQ pp 89 and 131, and N205].

5 [UQ pp150-151].

6 Szabó had published a series of papers starting in the 1950s; KRP would have been aware of IL’s work by 1959.

7 KRP: *Objective Knowledge*: [OK pp6-8].

extension to a transference between the history of science and logic[8]. But already there are some difficulties: they arise from the phrase “whenever logical problems are at stake”, or rather from a combination of two phrases: “whenever logical problems are at stake … what is true in logic is true in scientific method and in the history of science”. The problem is that the “logic” of the first phrase, operating on the left hand side of the relation, ruins the transference claim[9].

Actually, Popper drops the first clause as he gives his version of his own *principle of transference*: “what is true in logic is true in psychology”. And clearly this more succinct form – as well as avoiding the difficulty – requires a generalisation of his initial claim. He moves to such a generalisation: and it is a – further generalised – principle of transference. He calls it a “heuristic conjecture” – “quite generally, what holds in logic also holds … in psychology”; the omitted section here contains a new qualification: “provided it is properly transferred” – and after this conjecture, it is straightforward to propose the further transferences of form (2) and (3)[10]. Popper in fact extends these generalisations even further. This can be best seen in his [somewhat earlier] response to Szabó’s presentation at the London Colloquium on the Philosophy of Science on 14th July 1965. Popper found Szabó’s claim of the existence of transference relations between Euclidean geometry and Eleatic logic “very interesting”. Szabó had been looking particularly at the question of the origins of axiomatisation, but again Popper generalises this problem-situation to that of the existence of transference relations between Euclid’s geometry and Ionian and Eleatic cosmology. To assert the existence of a series of transference relations of this kind is a statement of the philosophy of mathematics: a statement within a programme – within a metaphysical programme – of mathematics[11]. So we have here a series of

[8] I have elsewhere described an identical form of transference between psychoanalysis and mathematics. The elements on the analytical side that require translation into mathematical structures are associative pathways (or logical thread, or signifying chains).

[9] At best the “all” subjective states becomes “some”, and this by virtue of some – unspecified – logic. KRP was aware that there existed a wide class of logics, and that establishing a priority amongst them invoked a metaphysical series of claims about the problems of mathematics. He may not have been totally aware of the great multiplicity of contemporary logics, or of the variety of mathematical theories used in constructing very many of them.

[10] In proposing this version, KRP takes it that he is proposing a formulation that avoids “unconscious expectation” and “irrational” content. See [OK pp26 and 80].

[11] There is a need for the availability of certain functions to be able to be able to carry out such a programme. Its development would involve the introduction of a metaphysical programme for the theory of sets – including some assumptions as to the question of “strong” or “weak” logics [extending even into the theory of large cardinals].

problems involving the philosophy of mathematics, and the relations between logic and the structure of the mind[12].

Any solution proposed to problems of the relation of "truth and proof" in this domain would hopefully provide an initial account of the autonomy of mathematics, allowing problems of the philosophy of mathematics to find their roots in mathematics, rather than proposing that a prior philosophy direct the orientation of the mathematics[13].

Popper has set out a transference between the psyche and logic, and given the nature of modern logic, this is effectively a transference relation between the psyche and mathematics. His proposal of course is for a one-way transference relation. The work of the Hungarian psychoanalyst Imre Hermann has extended this notion to a two-way transference relation between psychoanalytical structures and mathematics. I have elsewhere given accounts of the mathematics that would be involved in this, using in part the work of the Irish mathematician William Rowan Hamilton.

{Some of the themes involved here are: Transference in Dugald Stewart from 1811; in Freud from 1891, in Hermann from the 1920s onwards. This theme of transference – in its original philosophical and then psychoanalytical and then mathematical aspects – is at the centre of the problem-situation of the formalisation – that is, the mathematisation – of psychoanalysis. On these accounts, the problems of psychoanalysis can be solved by translating them into a corresponding mathematics, solving – where possible – the corresponding mathematical problems, and translating back [and vice-versa]. Any particular theory of psychoanalysis-mathematics transference can itself be tested by the ascertaining of clinical results. If such a transference is two-way then it is also at the centre of a reconstruction of the problem-situation of set theory from Zermelo onwards – of course with a pre-history starting in the initial years of the nineteenth century with Herbart}.

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[12] Clearly a central aim of these transference theses is to avoid any form of psychologism. I will sketch an account of how this is done using partial order relations – as developed for instance by Sierpinski and Garrett Birkhoff (whose texts Popper worked on over decades).

[13] Szabó at times seems to deny this autonomy, finding directive principles for mathematics in the dialectic of Eleatic political philosophy. A contrary view is put forward by Kanamori [in his Appendix to *The Higher Infinite*], where he may well be referring to Szabó as he distinguishes the structure of mathematics from "the dialectical to and fro of philosophy". In terms of these relations, KRP – in his reply to Szabó – seems to give an autonomy to cosmology: of course, he is here referring to a cosmology that since the time of Thales and Anaximander has had mathematics embedded into it – or, to use KRP's terms, transferred into it.

As for the title of this piece: it asserts a transference between the cosmological problems of the early Greeks, and problems formed by the pathways and chains in the mind. This presupposes that a clear structure is available for the spaces that constitute the psyche – and this has been generated by the mathematical philosophy of Herbart, which is the starting point of a programme that goes from Herbart to Riemann to Dedekind to Zermelo. *En route* it discovers the modern theory of topology, and builds set theory [the notion of chain – a technical notion of chain – is at the centre of Dedekind's set theory, as well as that of Zermelo].

On Situational Logic as a Method in a World of Propensities

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> Human knowledge, like animal knowledge, is uncertain. Scientific knowledge is very much hypothetical. Still, people love certainty. And so, it seems, do some mathematicians. (For example, they like proofs.)
>
> POPPER , about 1990.

Not being a mathematician, it is, indeed, a relief for me to have passed the gate of admission for participating in this Symposium. Despite the announced handicap, and far from being certain, I trust, however, that the following shall be of interest to the meeting, as it draws heavily on Popper's *Postscript to The Logic of Scientific Discovery* (1982–83).

In what follows I shall try to show that there is a link between the *idea of propensity* and the *idea of situational logic* which may be summarized this way: situational logic is a method by means of which we may discover causal laws of phenomena, or other types of lawful interactions, between phenomena brought about by propensities at work in the observational or experimental situations under investigation.

1. On Propensity Equal to Probability as a Property of Generating Conditions

Like the physical notion of 'field of forces', the idea of *propensity* draws attention to the existence of unobservable dispositional properties of the world: that matter has a tendency or disposition to realize itself depending on its own composition and the surrounding fields. 'Propensities may be explained', Popper (1959, p. 30) writes, 'as possibilities (or as measures or "weights" of possibilities) which are endowed with tendencies or dispositions to realize themselves, and which may be taken to be responsible for the statistical frequencies with which they will in fact realize themselves in long sequences of repetitions of an experiment.'

The propensity theory holds that probability is *a property of the set of generating conditions* that constitutes or defines the sequence of the repeated events in question. Such 'generating conditions', which may be said to characterize virtual or actual sequences of events, Popper (*op. cit.*, p. 34) views as 'a set of conditions whose repeated realisation produces the elements of the sequence'; or, as he would later say: ‘propensity distributions are properties of the state of the world, its physical realities’ (Miller, 2018).

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Let me remind you of Popper’s (1990, pp. 9–10) opening example and his explanation of these points: ‘The classical theory of probability erected a powerful system upon the following definition: “The probability of an event is the number of the favourable *possibilities* divided by the number of all the equal *possibilities*.” Thus, the classical theory was about mere *possibilities*; and the probability of the event “tails turning up” would be 1 divided by 2 because there are altogether two equal possibilities and only one is “favourable” to the event “tails”. The other possibility is *not* favourable to “*tails*”. Similarly, the possibility of throwing an even number smaller than 6 with a perfect die is 2 divided by 6. … For there are 6 sides and therefore 6 equal possibilities and only two of these possibilities, that is the sides marked 2 and 4, are favourable to the event “an even number smaller than 6 turning up”.

But what happens if the die is loaded or if the penny is biased? Then, according to the classical theory… we can no longer say that the six possibilities of the die, or the two possibilities of the coin, are *equal possibilities*. Accordingly, since these *are* no equal possibilities in such cases, we simply cannot speak here of probabilities in the classical numerical sense. … There are still the six possibilities; but they are now not *equal* possibilities but *loaded* or *weighted* possibilities; possibilities that may be unequal and whose inequality or different weight may be assessed…’[1]

The idea of weighted possibilities, Popper (*ibid*.) argues, is fundamental for a more general theory of probability. ‘It is clear’, he says, ‘that cases of *equal* possibilities could and should be treated as special cases of weighted possibilities: obviously, equal possibilities can be regarded as weighted possibilities whose weights happen to be equal.’[4] As cases of loaded dies and human life tables show, it is, Popper (1988a, p. 12–13) says, necessary to develop a probability theory to account for the different ways in which both inanimate and animate matter appear in realizing themselves according to their weighted possibilities or propensities. A general theory of probability suited for working with weighted possibilities would therefore be of great use in all the sciences, in physics, biology, economics, history, psychology and so on.

The step from weighted possibilities to the propensity theory of statistical events can now be read out from the following fours points, stressed by Popper (*op.cit*., pp. 11–12):

(1) 'If we can measure the weight of the possibility of "*two turning up*" in throwing a certain loaded die, and find it to be only *0.15* instead of *0.16666 = 1/6* – then there must be inherent in the structure of throws with this die... *a tendency or propensity* to realize the event "*two turning up*" that is smaller than the tendency shown by a fair die. Thus, the first point is that a tendency or propensity to realize an event is, in general, *inherent in every possibility* and in every single throw, and that we can estimate the measure of this tendency or propensity by appealing to the relative frequency of the actual realization in a large number of throws; in other words, by finding out how often the event in question actually occurs.

(2) So, instead of speaking of the *possibility* of an event occurring, we might speak, more precisely, of an inherent *propensity* to produce, upon repetition, a certain statistical average.

(3) Now this implies that, upon further repetition – upon repetition of the repetitions – that the statistics, in their turn, do show a tendency towards *stability*, provided all relevant conditions remain stable.

(4) Just as we can explain the tendency or propensity of a magnetic needle to turn… towards the north by (a) its inner structure, (b) the invisible field of forces carried with it by our planet, and (c) friction, etc. – in short, by the invariant aspects of the physical *situation*; so we explain the tendency or propensity of a sequence of throws with a die to produce… stable statistical frequencies by (a) the inner structure of the die, (b) the invisible field of forces carried with it by our planet, and (c) frictions etc. – in short, by the invariant aspects of the physical *situation*: that, is, the field of propensities that influences every single throw.'

The tendency of statistical averages to remain stable if the conditions remain stable, Popper considers to one of the most remarkable characteristics of our universe, and he concludes that this characteristic can only be explained by the propensity theory (*ibid*em); by the theory that there exist weighted possibilities *which are more than possibilities*, and, in fact, are *tendencies or propensities to become real*. Paraphrasing Niels Bohr, we may say: 'If God threw dice when creating the world, be sure they were loaded.'

As an example, we may consider a table by Peter Medawar (1986, p. 196), which shows how our life expectancy may be considered an outcome of propensities at work during the lifetime of human individuals, and how in principles such propensities may be assessed with the help of the sciences involved. *Table 1* gives an overview of the life

Period		*Mean Expectation at Age*		
		0	60	80
1755-76	Male	33.20	12.24	4.27
	Female	35.70	13.08	4.47
1856-60	Male	40.48	13.12	3.12
	Female	44.15	14.04	4.91
1936-40	Male	64.30	16.35	5.25
	Female	66.90	17.19	5.49
1971-75	Male	72.07	17.65	6.08
	Female	77.65	21.29	7.28

Table 1. Mean Expectation of Life of Males and Females in Sweden over Two Centuries. For Medawar these cumulated results are sufficient to show that life expectancy depends on genetic factors, or propensities, that always let women live a bit longer than men, and on factors, or propensities, of the milieu, as described in the text, even though it is difficult to arrive at precise estimations of life expectancy in a von Mises distribution or a Weibull survival-distribution over individuals plotted in according to their own age. (For a criticism of this interpretation, see Miller, 2016, p. 236.) Expectancy of men and women living in Sweden over the last 200 years. It appears that life expectancy for different age-groups (0, 60 and 80) has increased markedly, bringing the expectancy for men at 80 up to 6.08 years in the 1970's and the expectancy for women at 80 up to 7.28 years in the same decade. While the continuous differences between life expectancy for men and women, where women always seem to live a bit longer than man, reflect certain genetic dispositions, the overall increase in life expectancy over the last two centuries will be due to a number of 'exosomatic' factors, as Medawar would call them, such as new forms of medical care, nutritional factors, improvements in the care of children and elderly people, etc.

Now, in considering this case of life's urge to realize itself, we have also started to answer an important question of method, namely: does there exist a method – or an instrument like a pair of scales – that can help to find out the actual weight of the weighted possibilities? Or, does there exist a method that allows us to attribute numerical values to possibilities that are unequal?

As can be gauged from what has been said so far, the answer is: *yes*, a statistical method – provided we can repeat the situation that produces the probabilistic events in question and provided that the propensities remain constant; or, provided the events in question repeat themselves, without our interference.

2. Situational Logic as a Method of Analysis

To this, I now wish to add that we also have to know which of the repeatable factors (parameters) to look for and pick out in order to find the regularities of the phenomena we are interested in.

It is here, in my opinion, that situational logic comes in: that is to say, *we need analyses of the situation in which the propensities are at work in order to be able to specify the initial conditions and factors of importance for our experimental set-up or for the way in which we carry out the observations we wish to repeat.*

The methodological view behind Popper's *idea of situational logic* is the thesis that even events taking place in, say, complex social settings may be explained by *a rational reconstruction* of the situation, *i.e.* by finding out which are the contributing factors responsible for the events we have observed or recorded – or more generally: what is *implicit* in the situation (Popper, 1967; 1963; 1994). According to this view, interactions between people will, by and large, be explicable by genetic dispositions to act and by in situational factors such as space-time parameters of the *physical* environment *plus* customs, traditions, and institutions of the *social* environment *plus* actualized intentions, aims and goals of the *individual* agents of the situation. Thus Popper does not exclude the *bio-psychological* situation of the actor in question, his needs and dispositions to act.[2] What Popper (1966, Ch. 14) tries to play down, however, is the eventual contribution to the situation of the agent's *subjective world of experiences*. In order to predict the course of a man crossing a busy street, he argues, it is more important to know the relative movements of cars, trams, lorries, etc. in the street than to know what the subjective experience of this man is, while he is crossing the street.

The kind of explanation offered by situational logic will be of the form of a *generalized historical account.* 'This means that the situation [in which an agent finds himself] is supposed to be *typical* rather than *unique*. [And] thus it may be possible to construct at times a simplified *model* of the situation.' (Popper, 1972, p. 270.) 'Model' is here used in the sense of a representation of typical initial conditions and of typical relations between these conditions. As in physics, models have to be animated by some 'driving force' in order to simulate real events and predict singular events. Such singular events can then be explained, even if the animating principle or driving force may only in part be explained.

Among possible animating principles for *models of social interactions between people,* Popper has proposed the aforementioned *rationality principle* which, in its weakest zero formulation, states that 'Agents always act in a manner appropriate to the situation in which they find themselves.' (Popper, 1967, p. 361.) Admittedly, this formulation does not tell us very much, and it does not assert that human beings always act *rationally*. It is rather to be considered as a consequence of the methodological view that we should try to explain human action in terms of the objective features of the situation in which they act – including the objective aspects of the agents' understanding of the situation and relative to the agents' expectations and aims. The rationality principle is not, then, a testable hypothesis about reality; for being only an approximation, it cannot be universally valid, and so must therefore be false. Often, however, it may be sufficiently near the truth to provide a first explanation. Defending Popper's choice of keeping the rationality principle despite of its dubious status, Lagueux (2006, p. 203) has argued that the principle cannot be dismissed and replaced by some principle that admits irrationality without making our understanding of social phenomena impossible.

Models of social events, which are animated like this, may be seen as a general substitute for universal laws, which the social sciences have had such difficulty in finding. Such models are necessarily rough approximations to the truth as they are usually schematic simplifications of real life situations. For this reason – and also because the rationality principle is only an approximation – Popper warns that tests of social and behavioural models are neither clear-cut nor easy to obtain.

The following theoretical considerations, and the examples from behavioural science in *Section 5* will further indicate why this might be so.

3. More on the Special Case of Propensities as Life's Urge to Realize Itself.

The idea of propensity draws attention to the existence of dispositional properties of physical and organic entities, or, as mentioned above, that matter has a tendency 'to urge' depending on its composition and the surrounding fields. Propensities are thus *relational properties* since they are determined by the total set of generating conditions pertaining to the entire system under consideration, not only to dispositional properties inherent in the individual entities. (Life phenomena may be compared to chemical affinity, crystal formation, osmotic pressure, and resonance that may be said also 'to urge' in one way or another.[3])

Popper's modification of the frequency interpretation of probability has now allowed the conjecture that probabilities are dispositional properties of the generating condit- ions of the situation in question. The modification further permits an interpretation of the probability of *singular events* as properties of the very events themselves, and where the probability is to be measured by a conjectured potential or virtual statistical frequency rather then by an actual or observed frequency – as Popper (1983, p. 356) explains: '… we can say that the singular event *a* possesses a probability *p(a, b)* owing to the fact that it is an event produced, or selected, in accordance with the generating conditions *b*, rather than owing to the fact that it is a member of a sequence *b*. In this way, a singular event may have a probability even though it may occur only once; for its probability is a property of its generating conditions: it is generated by them.' The conditions in which the singular event occurs – whether we think of the event as a member of potential or virtual sequences of repeatable events – have to be visualized as endowed with a tendency, or disposition, or propensity, to produce sequences with frequencies equal to probabilities, which we may be able to calculate.

It is at this point that the importance of the propensity theory for the life sciences becomes particularly obvious in that the different strains of organisms are seen to form sequences of repeatable entities while, at the same time, each singular organism can be shown to possess distinctly individual properties.

As a case of special interest Popper (1982, p. 209) considers the problem of explaining the organization of living matter in discrete individual: 'The individual organisms … with their strange character of far-reaching autarky have often been compared to crystals; and indeed, they could be compared with physical systems that are endowed with strong propensities to retain their character as relatively autarkic systems – we might call these peculiar propensities towards autarky, with their surprising independence of environmental conditions, "*inherent propensities*" of the system. They are relational, as all propensities are, of course; and yet, they do resemble Aristotle's inherent potentialities of a thing more than other physical or biological propensities. (This is no accident: Aristotle was a biologist.)'

Following the propensity theory it seems possible tentatively to ascribe, say the capability of migrating animals to move in certain directions, to a kind of 'magnetic effect' which, as it were, replaces the game of chance on the lower kinetic level by an 'inherent propensity' which, superimposing a systematic bias upon these more chance-like physical propensities, thereby leading the animals to their chosen habitat. So, when we move from the inanimate to the animate world, there seems to be an increase in complexity of the propensities: 'What we can now … see is that this kind of … superposition of 'inherent propensities' … already plays a role, in a rudimentary way, in classical physics (load- ed dies, osmotic pressure, resonance); and we can therefore form an intuitive idea of how it may fit into our physical world, and yet transcend it, by superimposing upon it a hierarchy of purposes – a hierarchy of systematic and increasingly purposeful biases.' (*Op. cit.*, p. 210.)

Examples of organic processes conducted by propensities, or system of propensities, are numerous. They may be found in what organic chemistry (Fairlay & Kilgour, 1966, pp. 268-71) describes as *enzyme repression* and *enzyme induction*, where the production of a particular enzyme is regulated in accordance with either a surplus or a lack of a specific compound in the vicinity. More complex still are the propensities guiding the different *chemical cycles* that are active in the metabolism of an organism. In case there is a disturbance of the normal functioning of such a cycle, the system of propensities may adjust itself in order to re-establish the normal functioning of the cycle. We may say that the propensities reveal themselves by the 'effort' made to re-establish the normal functioning of the cycle.[4]

Biological structures are not deterministic structures as they change according to developmental pre-programming of the individual organism and with the dynamics of the environment. Even in identical twins, we see that a *genetically* identical system of propensities comes to manifest itself in a *biologically* different way during ontogeny as a result, for instance, of a developmental difference with respect to the resistance to various diseases. The ‘effort’ made by the organisms of the twins to re-establish health after periods of disease may cause a diversity of immunity towards a number of diseases in the two individuals. Furthermore, it is known from the literature that, as early as 8 years of age, one identical twin had become a diabetic and the other began to develop obesity (Williams, 1956, pp. 11f.)

Thus propensity refers to relative probability with regard to both individual proper ties and that of the life situation of the individual. On the human level both types of propensities may be subject to chance caused by man-made interventions. For this reason the death risk of people with a certain illness may be greatly reduced (or if unlucky: greatly increased) by the invention of new medical aids and changes in the environment. As before, we can speak of relative probabilities summarizing the outcome of all the forces active during the encounter between the individual and his life conditions. Although greatly simplified this type of calculation can be read out directly from *life tables* similar to *Table 1*, above, which are used by insurance companies to fix the life insurance fees of people according to their state of health. The contents of such tables change with the invention of new medical treatments for a given illness. Life expectancy is thus a propensity class on which statistics can be based, and this, in principle, is the case for all biological (including behavioural) characteristics.

What counts in the ‘life tables’ of insurance and everyday life is therefore individual survival, whereas in biology in general stress is put on the survival of the species. In both cases, however, life manifests an urge, a propensity, to realize itself and survive as long as possible – or as Popper (1992, p. vii) later put it: ‘all things living are in search of a better world.’ And this is precisely the way in which the propensity theory may clarify the age-old assertion that organisms struggle to survive in worlds with different selection pressure.

4. Steps from Chemical Propensities to Organic Preferences.

Variations of propensity and form in the mineral kingdom, and their subsequent limitation, seem to have been necessary conditions for the origin of life. Not surprisingly, Popper considers the really important question about Life’s origin to be a question of reproduction. Logically, therefore, the question ‘Where does life start?’ has to be answered: ‘Life starts with reproduction.’

The argument is that as long as natural selection is considered to be the main factor responsible for the evolution of life – right from the beginning – a more or less similar *internal selection*[5] seems to be logically prior to natural selection itself. In other words, if such a selection did not exist in some crude form even on the chemical level, leaving a variety of more or less viable ‘offspring’, there would not have been any material for natural selection to work on, and consequently no improvement of life, in the sense of adaptive changes, could have taken place.

So, at first, natural selection has to improve the closeness of reproduction, partly in order to enforce its own effects: ‘If reproduction is not fairly close’, Popper (1988b) argues, ‘natural selection cannot expect much effect, since any organic property which at one point in time had proved unfit could crop up generations later in spite of being eliminated by natural selection at the first instance. When, however, as it happened, the results of adaptation by natural selection are retained, it may be said that reproduction within the strings of life considered is fairly close.’ Similarity of reproduction can therefore be seen as the accidental starting point of life, whereas assimilation and growth are to be considered as mere parts of a solution to the problem of reproduction – as a means of establishing some sort of reproductive unit or self-reproducing device.

From this logical reconstruction Popper conjectures that the first reproductive unit was a kind of very small chemical machine, something like RNA, the reason being that RNA develops chemical activity and that it seems to come before DNA. Still, such proto- types of chemical machines are likely to have been more primitive than RNA. Following Schrödinger’s (1944; 1967, pp. 5–6) idea of crystals as possible ancestors to living organisms and Cairns-Smith’s (1971) more recent idea about clay pores as non-living geological formations, which give prototype RNA an opportunity of reproduce itself, Popper (1988b) embarked on the following idea about the origin of life, *i.e.* for pre-cellular living structures not necessarily equipped with membranes: ‘According to what I have called “the logic of the origin of life” it may be conjectured that the first organisms on Earth were chemical cycles, some short strings of, say, amino-acid equipped with two main propensities, or tendencies, which amount to more or less the same: (1) a tendency for the organism to double itself in length, and (2) a tendency for the organism to double itself in such a way that it can split. These two tendencies for making a similar reproduction – which could be the logically first tailors of natural selection – may have been particularly easy to realize in an ecology of clay layers in water, where these chemical strings could spread over the porous surfaces, perhaps utilizing the porous clay both as an equivalent of a membrane and as a means of limiting the variability of the reproduced organisms. It is conceivable, though only a guess, that this early form of life made use of a precursor of the copying method found in organisms today.’[6] It goes without saying that many more processes had to be added before this small chemical machine could become autonomous: enzymes had to be invented, enzyme-producing devices and codes had to be brought in to stabilize growth and defence; membranes were needed to envelop the whole organism and make external drifting around part of new ways of life for, at first, the bacteria and the protozoa, and later, in the metazoans to further internal division of labour between separated parts of the organism.

Although any idea or theory about the origin of life is bound to remain non-testable in the strict sense, even though we may happen to be successful experimentally in showing that it could *not have happened* in the way sketched above – and that we have to think a- new to hit upon a better hypothesis – analyses of the logic of the origin of life may, however, yield useful working hypotheses about not only life's origin but also about the biology of organisms in general. In fact, such applied situational analyses, or explanation in principle, are important parts of the propensity theory applied to organic processes.

Another consideration emerges from what has been said so far, namely a hypothesis about the evolutionary significance of the *preferences of organisms.* As mentioned in note 4, complex systems of preferences are usually thought to have evolved only in higher vertebrates; however, it may be argued that something corresponding to preferences comes in already with the first organisms-like creatures – not, of course, as functionally as the behavioural preferences behind, say, Mayr's (1963, pp. 89f.) 'ethological isolation mechanisms', but something like a *de facto* preference for a certain kind of habitat can be said to exist when the reproductive tendencies of our first chemical machines turn out to be successful on only one type of geological formation and not on another. As maintained by Campbell (1974, p. 181–82), the main point of preferences is that they influence the activity of the organisms, and that the effects of similar preferences in all (or practically all) members of a species can be measured also in the evolution of the species. That behavioural preferences can have a downward-causation effect upon the genetic level has not been realized, let alone recognized, by all biologists and theoretical evolutionists.

The title of this section may be understood to mean that the propensity theory may explain everything from chemical bonds to dispositions of organisms, but this does not appear to be easily done. For although propensities and dispositions are alike, because they can both be specified as 'possibilities endowed with tendencies to realize themselves (in long sequences of repeated events)', propensities have got no *aims* whereas dispo- sitions are always aim- or goal-directed. Propensities and dispositions, such as preferences, are therefore not homological metaphors.[7] How and when aims came into the world is not yet understood: 'The nearest to an aim we may come in the inorganic world is the tendency for, say, a gas to arrive in a state of equilibrium. But here there are at least two decisive differences in relation to the aims of organisms: (1) The tendency for a gas to arrive in a state of equilibrium is *not* an activity; and (2) an equilibrium state in a gas does *not* correspond to an organic state like homeostasis, since the equilibrium state is characterized by a maximum level of entropy, while any kind of living state of organisms are not so characterized.' (Popper, 1987.) What is more: aims of organisms are only rarely attained, so the equilibrium state of a gas cannot correspond to an organic state like homeostasis. The activity of organisms may thus be described only partially by physico-chemical principles. Organic activity is problem solving, and the activity of inanimate matter is not of this nature.

5. A Preliminary Analysis of Life Situations, With Illustrations From Behavioural Science and Anthropology

Situational logic in the life sciences is concerned mainly with questions of how organ- isms solve the problem of adapting themselves to varying life-conditions. Indeed, one of Popper's (1974a, p. 134; 1976, p. 168) thrilling insights is that natural selection in the Darwinian sense only works as a powerful explanatory system because it is, in itself, *a case of applied situational logic*.[8]

By applying situational analysis it is possible to refine our search for factors that have been or are of importance for such adaptation, as Günter Wächtershäuser's (1987) biochemical theory of the origin of the first light-sensitive cells shows so magnificently. Multiple forms of interaction take place on different levels of selection when organisms adapt themselves to the environment. The question now must really be: what are the important factors on a given level of interaction, and how has the resulting adaptation come about?

Although the situational aspect of the propensity theory is most important, situations have been much less dealt with in situational analysis than the logic of it. In trying to make up for this, the following preliminary analysis of problem situations, based on ethological methodology,[9] describes *life situations* of animal species as being of two main categories:

(i) Repeated species-typical ('a priori') situations;
(ii) Singular or repeated ('a posteriori') situations of learning.

To these biological problem situations we have to add a third, and very different category of situation, namely that of

(iii) Singular or repeated exosomatically constrained situations.

ad i. In such species-typical situations, repeated over generations as part of their life-conditions, the genetic endowment of members of extant species is supposed to have evolved through natural selection as solutions to problem situations belonging to the history of the species. This will hold for species-typical organs and behaviour patterns of most species, *Homo sapiens* included. In other words, the genetic endowments are the result of *long-time problem solving* in, or adaptation to, species-typical life situations, *i.e*. to situations that each and every individual of the species had to face some time or other during its lifetime. This may be how both the resulting organs and problem-solving activity, which may be observed in present-day individuals of the species under consideration, come to follow something that resembles universal laws. It is such law-like behaviour typical of the species, together with the typical set of organs of each individual, which make up the initial conditions for any new attempt at solving problems for these and later species-members. In this sense, the phylogenetic results of problem solving in the past can be called 'a priori' initial conditions since, for each generation of individuals, they influence organic growth and behavioural functioning to a high degree. The counterpart of the initial conditions was provided by the changing ecosystem of each generation of the species.

ad ii. Singular or repeated learning situations make up a class of ‘everyday’ situations which an individual may encounter in the typical eco- and social system of the species, and also of situations that may be encountered less frequently, perhaps only once in a lifetime. They are problem situations in the sense that the individual comes to know them and their character only through encounters and, as a consequence, may remember them positively if the problem was solved or, if not, in a negative vein or with fear. The term ‘a posteriori’ is here used to designate experience or learning obtained during such encounters. It will be situations of varying degrees of freedom in that both the individual and the situation will be constrained either by limiting dispositions to act or by limiting conditions of the situation. This is seen, for instance, when an individual is faced with a given problem for the first time and then spontaneously performs, say, a pre-programmed or previously learnt behaviour that turn out to be either partially ‘to the point’ or not at all. It will then depend on the *short-time* learning capacity of the individual, whether it shall manage to make another, more appropriate, trial. Another constraint may occur if the individual adheres too strongly to something already learnt or imitated from others.

ad iii. Singular or repeated exosomatically constrained situations are such ‘everyday’ situations which, for example, due to environmental pollution or other man-made inventions, have been disturbed to such an extent that some pre-programmed or learnt solution to problems do not work any longer. The kind of situations encountered by animals in most laboratory experiments, and those in which many industrial workers spend part of their lives, are known to provoke stress and stereotyped behaviour while only casually or gradually rendering gratification to the participants. In such constrained situations of individual or joint problem solving, the participants are often seen to rush for the ‘first and best’ solution, which makes them miss the real causes of the misery. Nevertheless, it is remarkable that living beings are able to solve problems in such non-biological and artificial situations. As Medawar (1957, pp. 96f.) pointed out, not all exosomatically evolved systems and tools are evils as such – perhaps only their misuse and other *unintended consequences* are – whose genetic and organic side-effects we may, however, only come to know hundreds of generations from now.

In the remaining part of this talk I intend to recall three examples from behavioural science and anthropology to illustrate how human adults and children may *behave appropriately to certain situations without knowing why*, since during phylogeny natural selection seems to have provided the necessary species-typical (perhaps universal) problem-solving behaviour. The examples also show how situational analysis – as in the case of Darwin, mentioned above – may be employed without the researchers being aware that they are using situational logic in their research.

(1) The first example is taken from a well-known cross-cultural study by Professor Irenäus Eibl-Eibesfeldt (1972 or 1983) of greeting behaviour in aboriginal societies and in the Western world. This study revealed that humans use a quick and conspicuous eyebrow-lift when they greet foreigners, when they emphasize something during a verbal discourse, speak about something astonishing, or show a positive (affirmative) interest or a negative (arrogant or hostile) attitude towards others. All 28 ethnic groups investigated displayed this kind of nonverbal communication during interpersonal encounters in the specified situations, and it seems that we have here an almost universal way of showing spontaneous interest in others, especially in critical situations such as greeting. Despite its great social importance, the eyebrow flash is a mimic pattern which primates and *Homo sapiens* displays without being conscious of doing so. It is a pre-programmed behaviour on which evolution seems to have put a high premium in order to make these primates better survive intra-species and inter-individual conflicts. This assumption constitutes one part of the animation principle necessary for our model to explain this presumed universal behaviour; another part is made up by a schematic representation of possible phylogenetical relationships between various nonverbal expressions and their accompanying affective states (*Figure 1*). An interesting level comes in with human culture where the same nonverbal signs have been interpreted differently by different ethnic groups; as the figure shows, the eyebrow flash is equivalent to a factual 'no' in Greek culture and to a factual 'yes' in Polynesian culture. Lastly, it is important for the present context to note that the behaviour in question is released in the same type of situation (*Situation category i*) that, basically, is a situation of surprise or greeting. Although eyebrow flashes also appears in other situations their propensity for appearing in greeting situations may be considered very high, approaching the probability 1 for occurring.

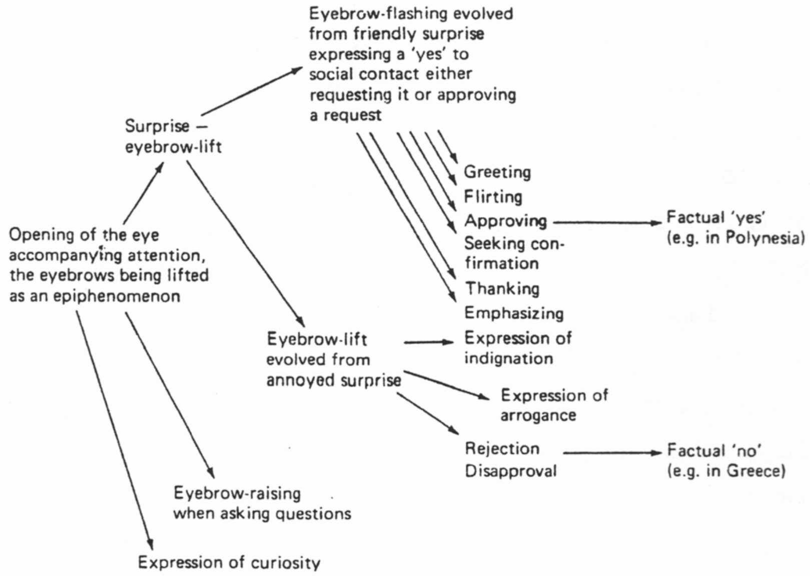

Figure 1: Hypothesis for the evolution of eyebrow movements into communicative signals in man. (From Eibl-Eibesfeldt, 1972.)

(2) In the 1970s another ethologist, Professor Hubert Montagner (1978), showed that 3-year-old children who played in a room with toys of the same kind, but less in number than the number of children, or with a similarly limited number of objects available, like low tables posed on each other with the upper table turned upside-down, would have a high tendency to start fighting over a toy or a table leg. This finding, interpreted with minor modifications in terms of the ethological theory of aggression was, for a time, considered almost a universal law. In the early 1980s Jacqueline Nadel (1986, pp. 109–87) and Pierre-Marie Boudonnière (1988, pp. 77–92), former students and colleagues of Montagner, showed that 3-year-old children, who had been given the possibility of playing with different toys, but always toys corresponding in number to the number of children in the group, would spend most of the time imitating one another by picking up the same kind of toy, but very rarely aggressing each other. This finding was explained by referring to traditional learning theory, which claims that imitation is among the first principles of learning. Comparing the two kinds of study it becomes obvious that the difference in behaviour between the two groups of children did not follow any of the animating principles put forward by the authors but rather varied in a predictable manner as a function of the situation – *i.e.* the number of toys available to the children during their time of play. What the animating principle should be is, at present, not clear but it is likely that we have to do with spontaneous behavioural tendencies that are released in the individual child, much like the eyebrow flash, according to the nature of the situation. (*Situation category i.*) However, much will depend on how evolutionary, developmental and

psychobiological theories manage to clarify such antagonistic and imitative human behaviour.

(3) A tribute to Popper's situational logic should not be without a reference to music, his preferred pastime activity. I shall therefore end by summarizing a most exiting story, brought home to us from the Upper Orinoco by the French anthropologist Alain Gheerbrandt, about what happened when, on several occasions, he played a Mozart symphony on his transportable record player to the Indians he encountered. (The presence of a record player did not change the situation to *Category iii.)* As Gheerbrandt (1992, p. 191) noted in his expedition diary, Mozart's music did not only win over the Indians' mistrust and thereby saved the life of the expedition, but music suddenly became the main objective of the whole enterprise since one of the members of the Yanomami tribe, who had been deeply moved by Mozart's music, spontaneously urged Gheerbrandt and his group to start off to visit the Yanomamis' 'brothers-in-law, up there', as they called the Makiritares of the mountains of Upper Orinoco, apparently with the aim of comparing their powerful music with that of Mozart's.

On their way searching for the Makiritares they encountered another fierce tribe of the Yano-mamis, and in trying to resolve the ensuing dangerous conflict, Mozart had to be called in to redirect the conflict in a peaceful direction; this appears to happen thanks to an exosomatic tool – the record player – and some inherited preferences for tonal harmonies. For when *Symphony No. 26 (Köchel 184)* sounded out again in the age old tropical forest, Gheerbrandt noticed how all the Yanomami men, except two chiefs who remained on their guard, came along and settled down close to him and the record player, lying on the ground or looking hypnotized at the turning disc: 'This recording has a softening effect on them and us', Gheerbrandt (*op. cit*., p. 333) writes, 'it relaxes the body and seems to make the soul breathe. It is oxygen at the same time as it is the most gentle consolation. It drives away all fear, melancholy, the weariness of isolation, the exhaustion of the journey and the tough life…[This music] opens up the most secret locks of our being, it relaxes and calms us down, it makes us feel like smiling and talking gently, it makes everything come out with a thousand voices, thousand colours, thousand forgotten shapes.' The music also attracted the women and children from their huts, and even the young girls came out to take in Mozart's music.

This description seems to me of the same nature as many ethnomusicological findings made this century in different parts of the world. The possibility of the existence of a universal musical semantics preferred by all people – except, perhaps, composers and performers of modern Western music and rhythmical sounds and noise – is not only in keeping with other human ethological phenomena, but also an important comment to the atonal fashions of the so-called second 'Viennese School of Music' – of which Popper (1976, pp. 54 and 71-72) has been particularly critical. Could it be that during Man's adaptation to his virgin habitats on our planet (*Situation category i*), he evolved aesthetic preferences which, in music, were spelled out in natural scales, rhythms, and harmonics like those still animating folk music and attuning the musical sensitivity and communicative musicality of people in unspoiled parts of the world?[10] It was at any rate thanks to Mozart's timeless music that Gheerbrandt (*op. cit.*, p. 334) for some precious moments felt at one with the Yanomamis and had a glimpse of how natural harmonies and rhythms may prepare and adapt the human mind to new and perhaps dangerous change: 'I do not know if music is really the universal language, as one says, but I shall never forget that we owe to a symphony of Mozart those rare moments where the abyss was filled up which centuries and development have eroded between us, the civilized of the twentieth century, and *them*, the civilized or barbarians of the stone age.

The examples used here illustrate that life situations, or scenarios, are indeed capable of releasing reactions typical of humans at different ages, and they may also serve to indicate the existence of basic motives for human behaviour – or, what amounts to the same: the animating principles for our models of human behaviour. The greater part of these motives are largely unknown or not recognized, although we experience daily their effects on our own conduct. Their way of functioning resembles that of chemical affinity and chemical cycles – an analogy that becomes particularly tempting in cases of archaic, repetitious reactions that imply *approaches to* or *withdrawals from* other organisms, certain objects and situations.

Such human activities are typically released and executed in *Situation* c*ategory i*, where no learning is required of the agent, who in most cases is quite unaware that *his own behaviour just 'happens to him'* and *happens to solve a problem he may not be aware of either*. We may thus class *behavioural* phenomena as activities that are *genetically pre-programmed* (so considered by the ethologists, and as *inherent propensities* by Popper) and exempt of learning to a great extent.

Human activities found in *Situation category ii and iii*, of which there are none among the above three examples, require *consciously planned actions* that most often will have been *learnt* by the individual agent over a certain time. Unlike behaviour, *actions* are not only *planned* by the agent (a case of downward causation), but are typically also highly *conscious* to the actor when carried out, unless they have become automatic like speech movements with *intention* and consciousness now focussing on the messages to be conveyed. Not surprisingly it is in *this world of actions* that we also find the major part of human knowledge, acquired by trial and error-elimination, and where the individual actor may try to come to grips with situational analysis and its logic.

6. In Conclusion.

Explaining his schematic representation of 'upward causation' in nature,[11] Popper, in a private conversation around 1970, pointed out to me how great the importance of weighted probabilities must have been, and still is, between each of the so far specified stages of cosmic evolution – from sub-sub elementary particles and elementary particles cooked in the stars and resulting in atoms, to molecules, and further to liquids and solids condensed in space, to crystals and the first living creatures on earth, virus and bacteria, and further on to so-called lower and higher animals with their gradually emerging conscious life. From one stage to the next there appear new higher-level entities which, seen in the rear-view mirror, must have been most improbable since so many other possibilities could have been realized – possibilities that would perhaps not have led to either molecules, or liquids or solids, let alone to planets with living organisms. ‘We live in a creative universe’, he said, ‘on a wonderful planet with conditions for life that were highly improbable before they arose.’

As we have seen, this view of the world which Popper (1990) specified, as *a world of propensities,* implies, among other things, that the so far emerged phenomena exert ‘downward-causation’ effects upon the conditions in which new phenomena take shape and appear. So there is always something new under the sun, thanks to downward causation.[12] The universe therefore has a history and an evolution, admittedly an evolution that is hard to follow and foresee, due to our lack of knowledge – although a surprising number of laws have been found by scientists using methods appropriate to the logic of situations in attempts at reducing explanations of higher-level phenomena to explanations of lower-level phenomena.

By these reduction-methods science have taught us much about the world, but as Popper (1974b, pp. 279-81) argues, this does not mean that any given phenomenon shall be completely reduced à la Democritus – since all attempts at reductions can be shown to be only partial. For this reason we cannot be *philosophical* reductionists but only *methodological* reductionists.

According to the propensity theory, one of the driving forces behind cosmic evolution is the enduring tendency of matter to realize its many potentials under conditions that change gradually or abruptly due to outcome of this very same urge of matter to realize itself.

Notes

[1] Among Mr. David Miller’s valuable comments, for which I am very grateful, there is one about my simplified way of considering loaded dies: ‘a die may be loaded in such a way that, by sheer geometric analysis, we can show that odd-numbered sides should be “twice as possible” as even-numbered sides. In that case, the probabilities would be 1/9 for 2, 4, 6, and 2/9 for 1, 3, 5.’

[2] David Miller (2006, p. 159) has elaborated Popper’s view of situational logic in arguing that in most situations a given agent acts ‘in a state of imperfect knowledge…He is bewildered, not just deluded. There is accordingly always some looseness in what his own situational analysis enjoins.’ Although the agent may know a little he is always to some extent ignorant. It is in this sense that trial and error-elimination, conjectures and refutations, is a kind of situational logic. This means that ‘the growth of knowledge is a more general phenomenon than is biological adaptation.’ The theory of evolution is therefore not restricted to biological phenomena.’ (*Op.cit*., p. 156.)

[3] This conception of ‘urge’ is somewhat enlarged compared to the notion Peter T. Mora’s (1963) uses in his interesting article.

[4] This also holds for the life cycles of developing organisms, and the effect of propensities will here reveal themselves in the controlling effects of genes. In his lightly disguised autobiography, Bonner (1993, p. 93) has this comment without, however, mentioning propensities: 'genes serve as the control elements for the [superstructure of] chemical and physical processes of development; without the genes calling the shots there would be chaos. The genes see to it that the development of a nematode is different from that of a fruit fly, or that of an oak tree, and … is consistent from one life cycle to the next for each of these organisms.'

[5] With the evolution of multi-cellular organisms, systems of anticipation or feed-forward will have emerged leading to *behavioural preferences* with their downward-causation effects upon the reproductive unit, and thereby on the subsequent composition of the population. In other words, behavioural preferences contributed to the overall selection pressure on the organism in much the same way as 'internal selection' exerted a pressure on the organism due to its internal 'architecture'. (For 'internal selection' and 'external selection', see Popper's 'Intellectual Autobiography', 1974, vol. I, pp. 138f., or Popper, 1976, pp. 173f.; 'downward causation' is expounded in Popper, 1977, pp. 14-21.)

[6] 1988 also saw the publication of Günter Wächtershäuser's great paper, 'Before Enzymes and Templates', where the material support for the first primitive organisms is conjectured to have been pyrite, not clay.

[7] Whereas propensities and dispositions are not *homological* metaphors, they are nevertheless *analogical* metaphors, as signalled by the title of this Section, and one of the aims of the sciences implied is to find *where* in the continuum between propensities and dispositions they obtain a homological character of similarity.

[8] To the disappointment of many readers, thrillers often turn out to be rather trivial, and Darwinism considered as situational logic is apparently no exception, as Miller acutely has it (2006, p. 159): 'Since Darwinism is the application of conjectures and refutations at the genetic level, and perhaps at higher levels, it too partakes the triviality of situational logic.'

[9] Ethological methodology is characterized by its focus on the animal's life-space. The approach is ecological and the ethologists study animals in their *natural habitats* as far as possible. The *unit of study* is the organism in its life-space or species-typical life-situation. What is of interest to the ethologist is not the animal's reactions to stimuli, independent of the circumstances (as in behaviourism), but rather the animal's species-typical behaviour in relation to significant objects in the situation, and its business with other living beings in its life-space. That is, behaviour which is largely 'genetically pre-programmed' and independent of learning. In applying this approach it is possible to identify basic situations in a given species' way of life, *Homo sapiens* included.

[10] From the point of view of folk music, the uprooted and artificial nature of modern atonal music becomes clear from the words of Béla Bartók (1932; 1976, p. 345): '…folk melodies are always tonal. Folk music of atonality is completely inconceivable. Consequently, music on twelve tones cannot be based on folk music.'

[11] Later included in Popper (1977), as Table 2, 'Biological Systems and their Parts', p. 17.

[12] David Miller (2018) agues against this by saying: 'Of course propensities change with time, but the question is whether propensities at later times are functions of propensities at earlier times, updated by conditionalization. I suspect not, and that genuine chance plays a role, in which case we are not living in a world of propensities alone. – In the present (non-subjectivist) context, what is meant is that when the event *e* occurs, the propensity of the event *h* changes from p(h) to p(h| e). This is a terrible oversimpli- fication, since it is rare that the passage of time is associated with just one event, or even an easily speci- fied event. The view that I disagree with is better put by saying

that the propensity of an event at a later moment is determined by the class of all events at preceding moments.'(See further Miller, 2016, §2.4.)

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Psychology of Reasoning, the Logic of Discovery, and Critical Rationalism

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Hugo Mercier and Dan Sperber's recent *The Enigma of Reason* (2017) offers a radically original but already much acclaimed account of the psychology of reasoning. Its hypotheses derive from the impetus of work that Karl Popper unintentionally initiated. Mercier and Sperber explain reason in a way that, equally unintentionally, echoes Popper's logic of scientific discovery and indeed his whole critical rationalism. Without realizing it, they show in new ways why Popper's breakthrough idea and his wider philosophy cut so deep and offer so much.

To anticipate: Mercier and Sperber propose that reason is not a faculty to lead individual minds toward right decisions and true conclusions but a socially evolved capacity to solve problems of cooperation and communication in the hypersocial species we have been evolving into. Reason works in lazy and biased ways on our own individual reasons but becomes much more alert in critiquing the reasons others propose. Popper too rejects reason as a faculty of individual minds, and proposes instead rationality in social terms, a readiness to listen to the criticism of others: "reason, like language, can be said to be a product of social life… we owe our reason, like our language, to intercourse with" others (OS II 1966: 225).

In 1966 the psychologist Peter Wason, at University College London, introduced the four-card selection task now known as the Wason selection task. He thereby initiated the modern psychology of reasoning, which "has to a large extent become the psychology of the Wason task" (M&S 39). Wason was inspired by Popper, teaching nearby at the London School of Economics, and especially by Popper's account of the power of falsification in discovery. Wason sought to find whether people were naturally inclined to seek falsifications. No, they were not: on average a mere 10–15% of subjects, when invited to pick which cards would test a simple "rule" in the card system, correctly picked only the cards that would logically falsify the rule. Wasons's work gave a new vigor to the psychology of reasoning, whose results have been consistent and insistent in the half-century since: people reason very badly, in lazy and biased ways.

Nevertheless, the recent psychology of reasoning has been much poorer at explaining than at simply uncovering reason's weaknesses. Twelve highly non-convergent explanations of human syllogistic reasoning have not shown why reason, if its function is to improve individual conclusions and decisions, should function so badly. Mercier and Sperber by contrast try to explain the results, and do so in a way that seems a late echo of Popper's evolutionary epistemology – not that they make that connection. They accept all the evidence that individuals reason so badly, but propose a different evolutionary function of reason, according to which reason serves its function well, if far from perfectly.

They propose that reason has evolved not to lead to better individual ideas and decisions, but to improve social exchange. In our hypersocial species, where we benefit enormously from cooperation, cooperation always faces a problem: how do others trust us, how do we trust others? Not only do our actions themselves get judged by others, directly and, through gossip, indirectly, but we can also offer reasons to explain our actions and ideas, to indicate that we are competent, norm-abiding, and trustworthy. We seek to justify our actions or conclusions to others, and therefore seek only reasons in support of what we do and say.

These reasons are much less the motivations or causes of our actions and conclusions than swift after-the-fact justifications to offer others (or if before the fact, in anticipation of a need for after-the-fact justifications). Our actions and conclusions are prompted by rapid, mostly unconscious intuitive inferences. Only after inferences have led us to a conclusion or a decision do we seek to offer justifications, if called on to do so. Reason, Mercier and Sperber argue, "does not objectively assess the situation in order to guide the reasoner toward sounder decisions" but "just finds reasons for whatever intuition happens to be a little bit stronger than the others." (MS 253)

Our justifications are biased – we usually seek only to support the position we have leaped to – and lazy – we do not seek hard for stronger reasons. Our reasons are not likely to be good reasons: they are arrived at not by some common mental logic, inductive or even inferential, but by the interaction of specialized, opaque, largely unconscious and opportunistic inferential subsystems; and we grab onto would-be justifications only after we have leaped to intuitive conclusions.

But there is another side to reason: "reason is more efficient in evaluating good arguments" – especially those of others – "than in producing them" (MS 11). As hypersocial animals we can benefit from the information others can share with us, but we need to sift what they offer so as not to be easily misled or manipulated. We therefore need to assess others' reasons for their conclusions, and in this, as the experimental evidence shows, we are much less biased and much more demanding: we tend to sift others' reasons much more stringently than our own, and to accept conclusions only when the reasons proffered seem strong. Mercier and Sperber therefore reject the term "confirmation bias," since we do not look primarily for confirming evidence of propositions and proposals that *others* advance. They suggest instead that we should see reason's bias as "myside bias": seeking to justify the position arrived at by me or my side.

Reason, Mercier and Sperber argue, is not a broad faculty of the mind, as it is for Plato or Cartesian rationalism (or as Popper calls it, intellectualism). Rather, it is a specialized metarepresentational module, a particular kind of intuitive inference that focuses only on our own or others' reasons for what we do or think. Proposing our own reasons, in justification, we are slack; shifting others' reasons, in argument, we are both more demanding and less biased. And when people have diverse opinions and are given the chance to discuss reasons in pursuit of a common goal, whether better understanding or better decisions, the performance rises steeply: among hunter-gatherers, children, the unschooled, the educated, juries and mock-juries, communities using deliberative democracy, expert forecasters, medical students and doctors, and scientists. Under conditions of open discussion, for example, performance on the Wason test rises to 80% answering correctly, far beyond what has been achieved in any other condition, even among highly educated subjects.

Reason operates poorly at an individual level, leading not only to sloppy thinking, but also to belief perseverance and belief polarization, but it operates well at a social level where there is open discussion, and therefore best of all in science. There, open discussion works all the way from lab meetings to publications read be well-informed colleagues with the time and motivation to counter-argue, gather counter-evidence or devise counter-experiments. These open exchanges of ideas and criticism make it likely that, although scientists as individuals and as researchers are as prone to myside bias as any, only the better ideas tend to survive, at least provisionally.

Wason tested what Popper was the logic of scientific discovery as if it could explain the psychology of discovery. But Popper had long rejected the psychology of discovery, partly following Frege, partly because he thought more progress could be made in the logic of discovery: as indeed it was, when in the early 1930s he recognized the impossibility of verifying a universal claim but the possibility of falsifying it. He recognized simultaneously the importance of the sociology of discovery, to explain not hypothesis forming, but hypothesis testing: the readiness of other scientists to test and seek to falsify scientific proposals before or in the course of trying to advance better hypotheses.

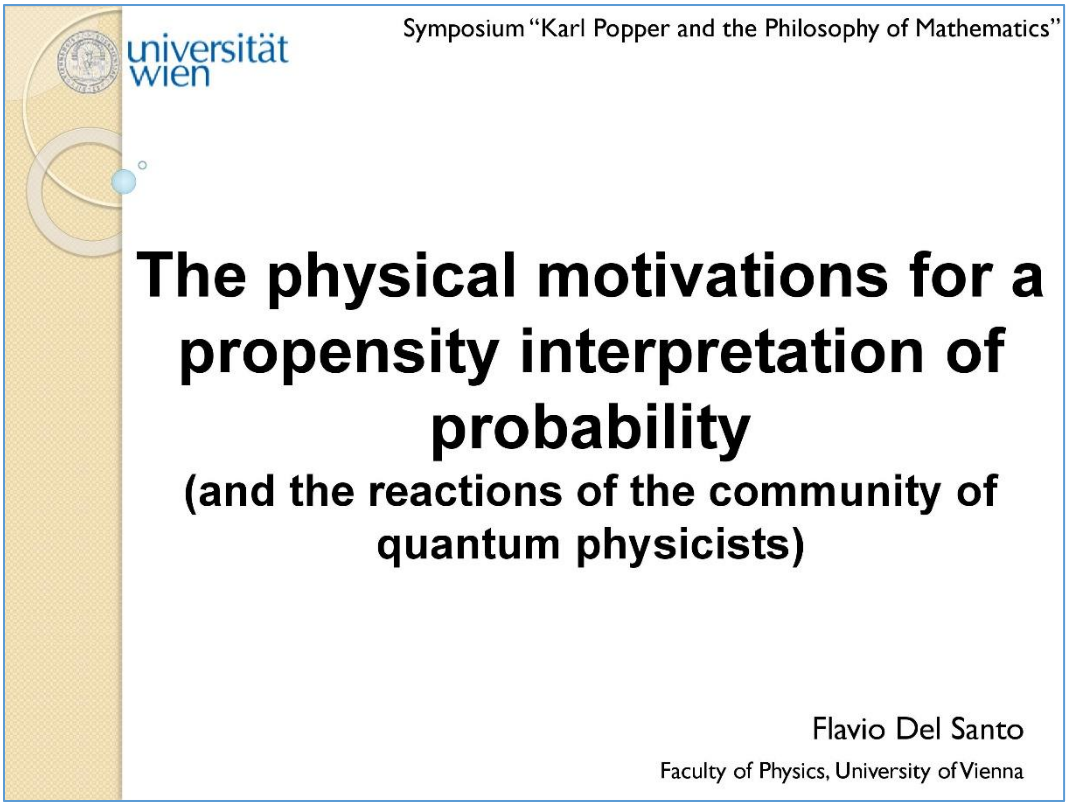
But almost a century later, after a half-century of empirical results that remained unexplained, the psychology of reasoning at last seems to have made real progress in explaining the role of reason and to cast new light on the logic of discovery. If, as Mercier and Sperber propose, our systems of intuitive inference are specialized, unconscious and therefore opaque to inspection, and opportunistic, they provide even less ground than many had assumed to suppose that we can induce from known examples to reliable generalizations. If our intuitive inferences about reasons for the conclusions we have reached are also after the fact (and tend to be lazy and biased) then we have even more reason for scepticism about our conclusions. The psychology of reasoning undercuts what confidence we may have had in our intuitions, in their apparent self-evidence, and in the adequacy of the reasons we find in support of them. It therefore places still more weight on the centrality of critical discussion and offers still more arguments for the intellectual modesty and the openness to discussion that Popper promotes.

Popper’s antijustificationist logic finds an echo in Mercier and Sperber’s psychology, their focus on the strength of our eagerness to justify, and the frequent logical weakness of the result. His logical critique of empiricism and of induction as a supposed means for reaching secure generalization by unbiased observation finds a psychological echo in their emphasis on the role that a network of diverse, opaque, swift but fallible inferential subroutines play in perception, memory, and the production of argument. Popper’s reorientation of reason as not a faculty of the mind but an acceptance of the power and value of critical discussion, as a social rather than a purely individual process of inquiry, is almost exactly echoed in theirs. His critique of the authoritarianism of those who trust in their own supposedly superior reason (OS II 1966: 240: “we owe it to other men to treat them and ourselves as rational”) also finds an echo in theirs (MS 172: “how rational is it to think that only you and the people who agree with you are rational?”)

Popper rejected an “intellectualist” theory of knowledge, whether in the Platonic or the Cartesian sense, and proposed an “interactionist” one: interaction, that is, between world 1, the physical world, world 2, the psychological worlds of individuals, and world 3, the world of objective knowledge, of problems, arguments, discussions and other products of many minds. Similarly, although with obvious differences, Mercier and Sperber explicitly reject the standard accounts of reason, which they call “intellectualist” (because such accounts assume reason’s function is to lead individual minds to better conclusions and decisions), and they propose instead what they call their “interactionist” account, one in which reason evolved as an adaptation in social discussion, where it works “reasonably” well, not lone thought, where it often leads astray.

Popper was right to reject the psychology of discovery and to focus instead on the logic of discovery: he had reached bold and rich results by 1934. But the psychology of reasoning may now have caught up with and provide new evidence for his conclusions in the logic of discovery and in stressing the social role of a rationalism alert to the power of criticism rather than based on the supposed power of individual reason.

Presentations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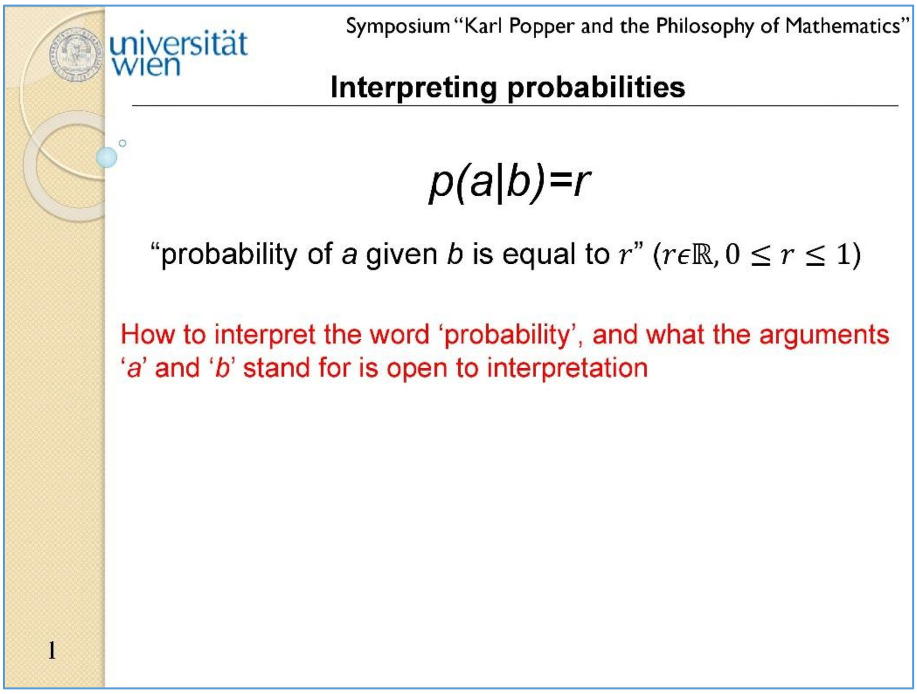
wien
Symposium "Karl Popper and the Philosophy of Mathematics"
The physical motivations for a propensity interpretation of probability
(and the reactions of the community of quantum physicists)
Flavio Del Santo
Faculty of Physics, University of Vienna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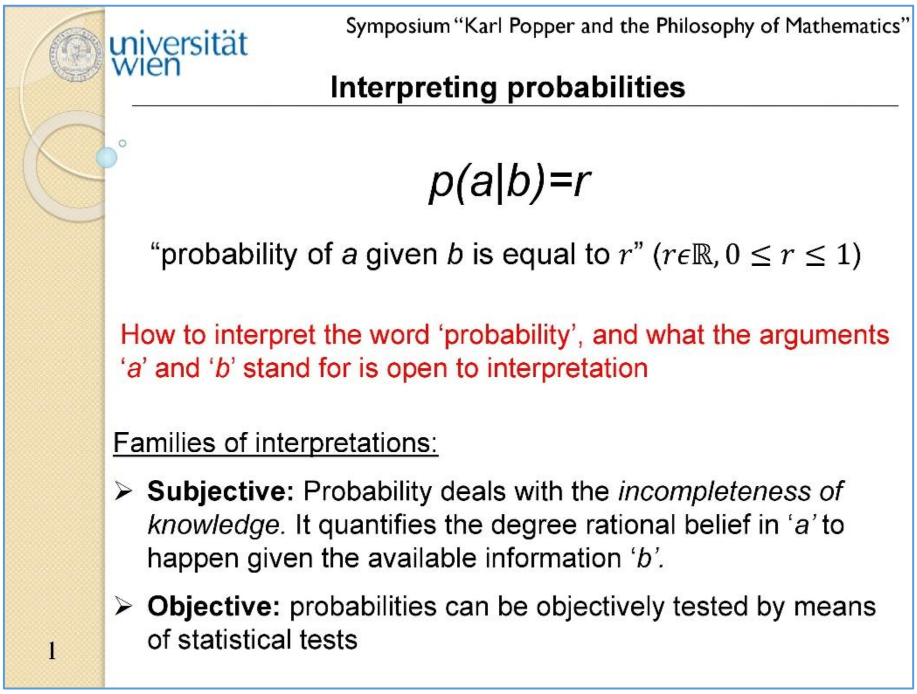
Symposium "Karl Popper and the Philosophy of Mathematics"
Interpreting probabilities
$p(a|b)=r$
"probability of *a* given *b* is equal to r" ($r\epsilon\mathbb{R}, 0 \leq r \leq 1$)
How to interpret the word 'probability', and what the arguments '*a*' and '*b*' stand for is open to interpretation
1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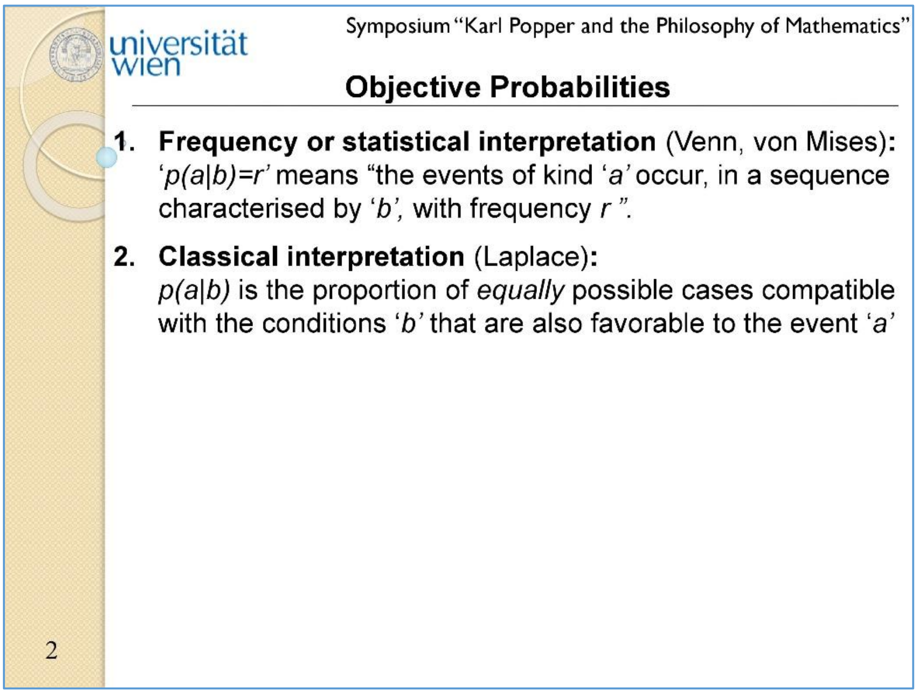
How to interpret the word 'probability', and what the arguments '*a*' and '*b*' stand for is open to interpretation
Families of interpretations:
➢ **Subjective:** Probability deals with the *incompleteness of knowledge.* It quantifies the degree rational belief in '*a'* to happen given the available information '*b'.*
➢ **Objective:** probabilities can be objectively tested by means of statistical tests
1


Symposium "Karl Popper and the Philosophy of Mathematics"
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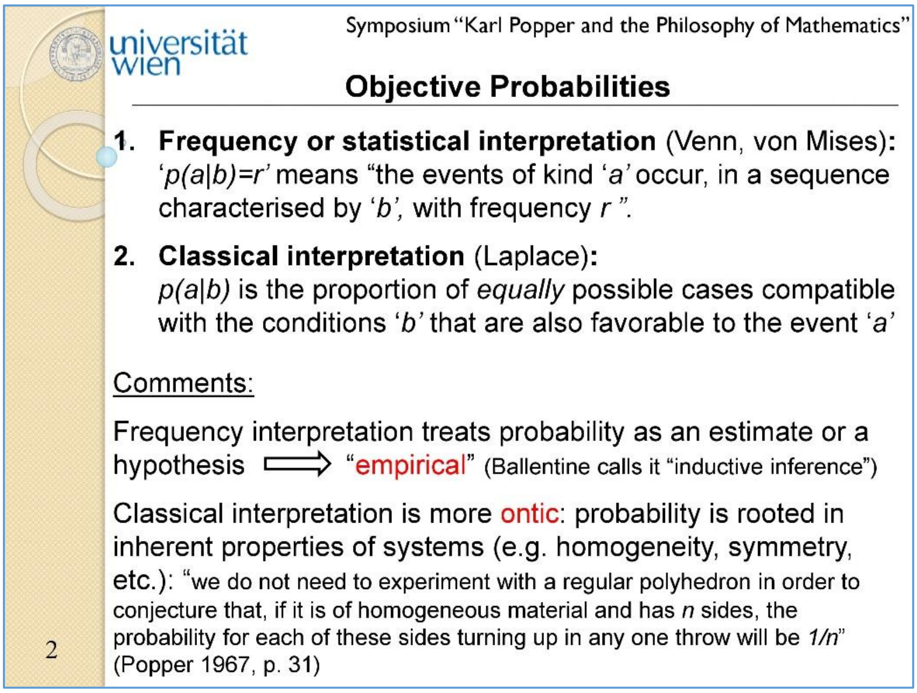
Objective Probabilities
1. Frequency or statistical interpretation (Venn, von Mises): 'p(a|b)=r' means "the events of kind 'a' occur, in a sequence characterised by 'b', with frequency r ".
2. Classical interpretation (Laplace): p(a|b) is the proportion of equally possible cases compatible with the conditions 'b' that are also favorable to the event 'a'
2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2. Classical interpretation (Laplace): p(a|b) is the proportion of equally possible cases compatible with the conditions 'b' that are also favorable to the event 'a'
Comments:
Frequency interpretation treats probability as an estimate or a hypothesis ⟹ "empirical" (Ballentine calls it "inductive inference")
Classical interpretation is more ontic: probability is rooted in inherent properties of systems (e.g. homogeneity, symmetry, etc.): "we do not need to experiment with a regular polyhedron in order to conjecture that, if it is of homogeneous material and has n sides, the probability for each of these sides turning up in any one throw will be 1/n" (Popper 1967, p. 31)
2

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Symposium "Karl Popper and the Philosophy of Mathematics"

Physics and probability

Besides the formal calculus of probability, interpretations seem to be rooted in 'real-world' cases $\Longleftrightarrow$ interpretations of theories

3

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Symposium "Karl Popper and the Philosophy of Mathematics"

Physics and probability

Besides the formal calculus of probability, interpretations seem to be rooted in 'real-world' cases $\Longleftrightarrow$ interpretations of theories

Quantum Mechanics (QM): irreducible probabilities (?)

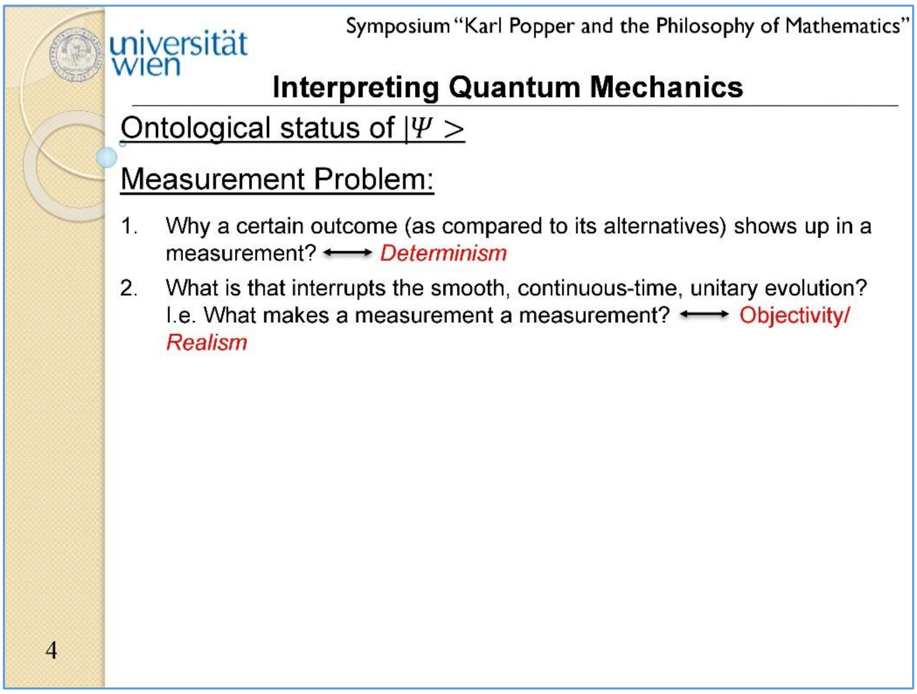
Axioms:

- The physical state of a system is described by a vector $|\Psi>$ in a complex Hilbert space.
- The state evolve in time according to the Schrödinger equation: $i\hbar\frac{\partial}{\partial t}|\Psi(t)>= H|\Psi(t)>$, where H is the Hamiltonian operator
- An *observable* is described by a Hermitian operator $A = A^{\dagger}$, with eigenstates $A|a_i>= a_i|a_i>$
- The *probability* of finding the outcome a_i upon ***measurement*** is (Born rule):

$$p(a_i|\Psi) = |\langle a_i|\Psi\rangle|^2$$

- After the *measurement*, the system is projected into the eigenstate $|a_i>$ (i.e. the state changes in general)

3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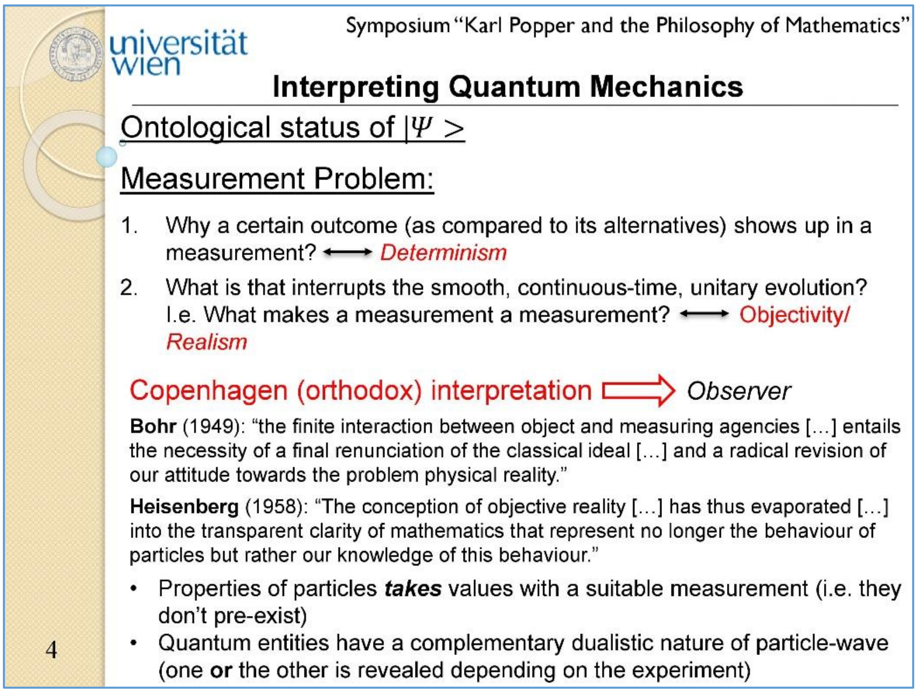
Symposium "Karl Popper and the Philosophy of Mathematics"
Interpreting Quantum Mechanics
Ontological status of $|\Psi >$
Measurement Problem:
1. Why a certain outcome (as compared to its alternatives) shows up in a measurement? ⟷ *Determinism*
2. What is that interrupts the smooth, continuous-time, unitary evolution? I.e. What makes a measurement a measurement? ⟷ Objectivity/ *Realism*
4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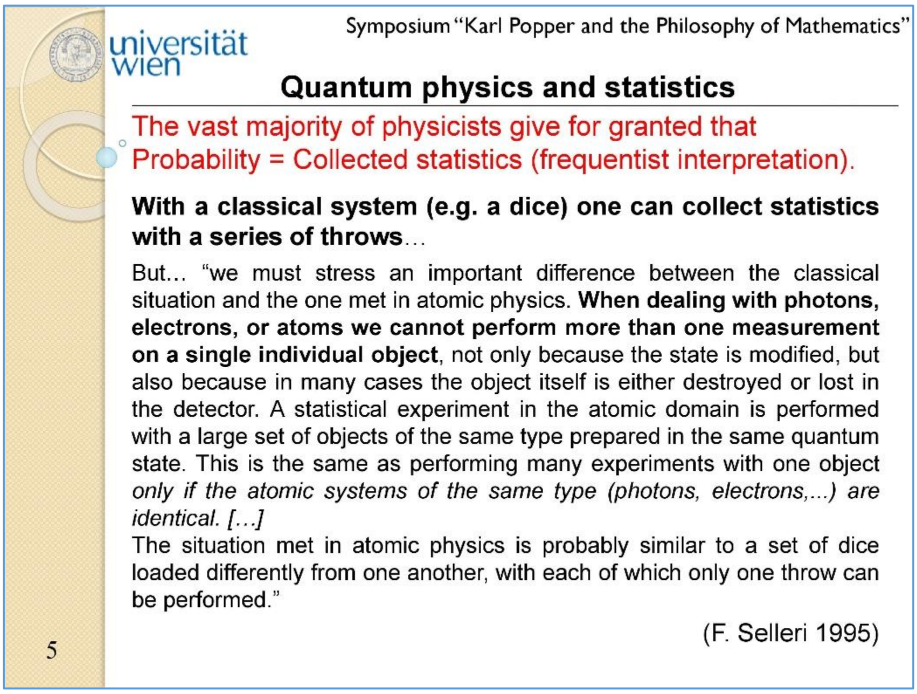
2. What is that interrupts the smooth, continuous-time, unitary evolution? I.e. What makes a measurement a measurement? ⟷ Objectivity/ *Realism*
Copenhagen (orthodox) interpretation ⟹ *Observer*
Bohr (1949): "the finite interaction between object and measuring agencies […] entails the necessity of a final renunciation of the classical ideal […] and a radical revision of our attitude towards the problem physical reality."
Heisenberg (1958): "The conception of objective reality […] has thus evaporated […] into the transparent clarity of mathematics that represent no longer the behaviour of particles but rather our knowledge of this behaviour."
• Properties of particles ***takes*** values with a suitable measurement (i.e. they don't pre-exist)
• Quantum entities have a complementary dualistic nature of particle-wave (one **or** the other is revealed depending on the experiment)
4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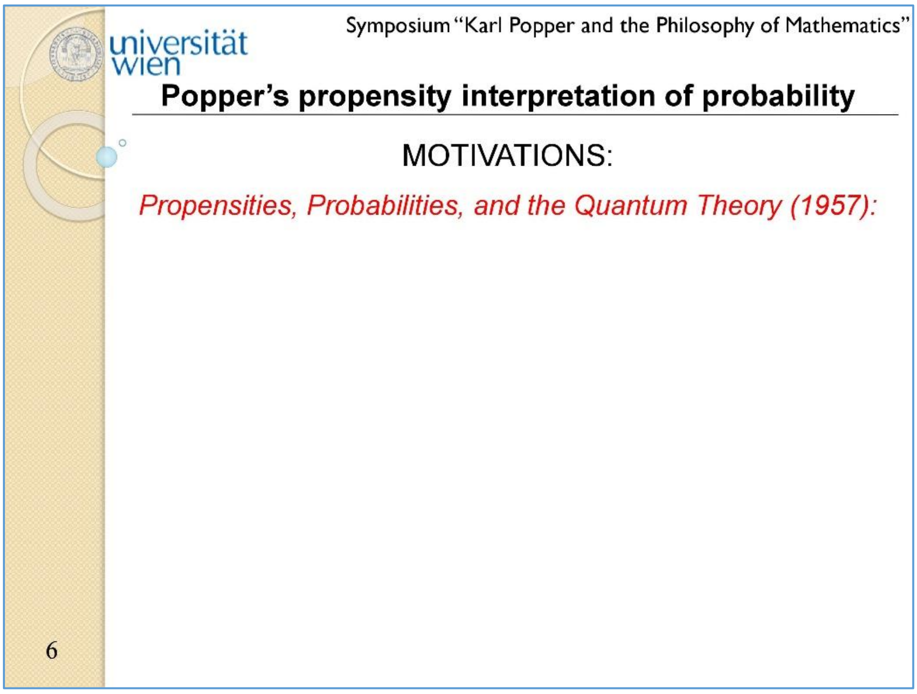
Quantum physics and statistics
The vast majority of physicists give for granted that Probability = Collected statistics (frequentist interpretation).
With a classical system (e.g. a dice) one can collect statistics with a series of throws…
But… "we must stress an important difference between the classical situation and the one met in atomic physics. When dealing with photons, electrons, or atoms we cannot perform more than one measurement on a single individual object, not only because the state is modified, but also because in many cases the object itself is either destroyed or lost in the detector. A statistical experiment in the atomic domain is performed with a large set of objects of the same type prepared in the same quantum state. This is the same as performing many experiments with one object only if the atomic systems of the same type (photons, electrons,...) are identical. […]
The situation met in atomic physics is probably similar to a set of dice loaded differently from one another, with each of which only one throw can be performed."
(F. Selleri 1995)
5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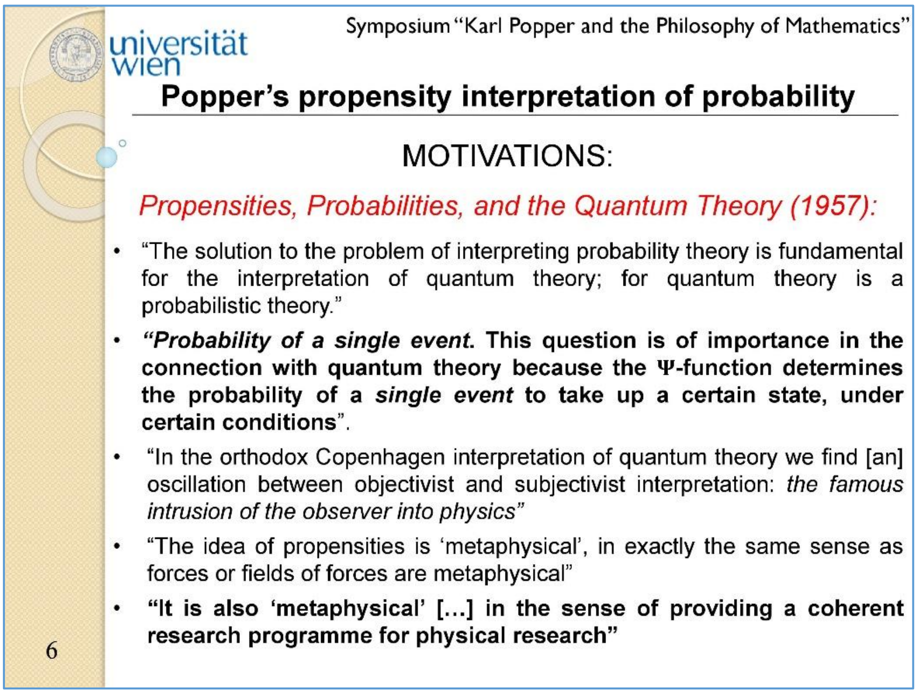
Popper's propensity interpretation of probability
MOTIVATIONS:
Propensities, Probabilities, and the Quantum Theory (1957):
6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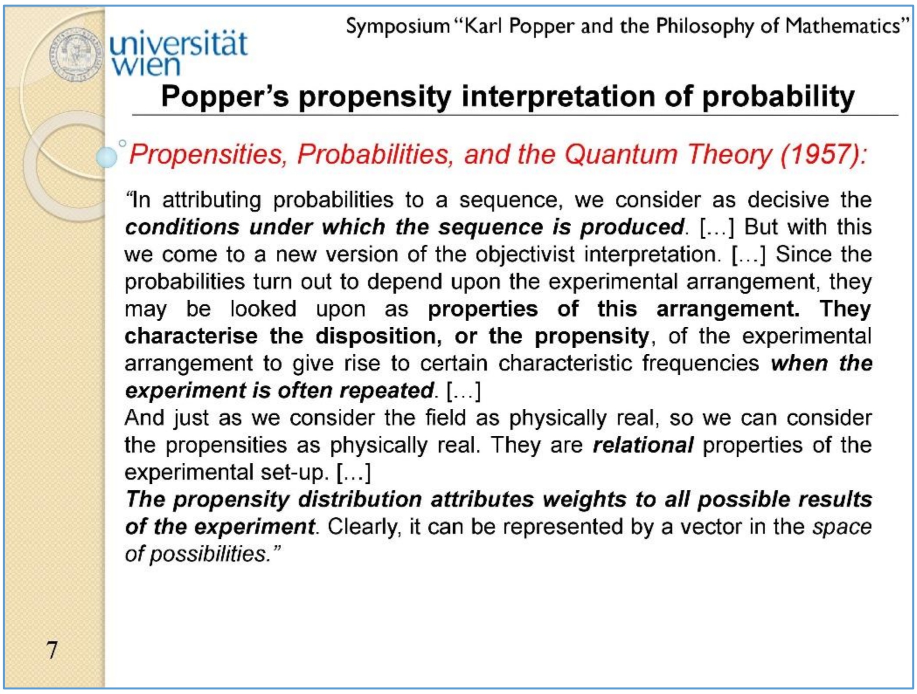
MOTIVATIONS:
Propensities, Probabilities, and the Quantum Theory (1957):
• "The solution to the problem of interpreting probability theory is fundamental for the interpretation of quantum theory; for quantum theory is a probabilistic theory."
• "Probability of a single event. This question is of importance in the connection with quantum theory because the Ψ-function determines the probability of a single event to take up a certain state, under certain conditions".
• "In the orthodox Copenhagen interpretation of quantum theory we find [an] oscillation between objectivist and subjectivist interpretation: the famous intrusion of the observer into physics"
• "The idea of propensities is 'metaphysical', in exactly the same sense as forces or fields of forces are metaphysical"
• "It is also 'metaphysical' [...] in the sense of providing a coherent research programme for physical research"
6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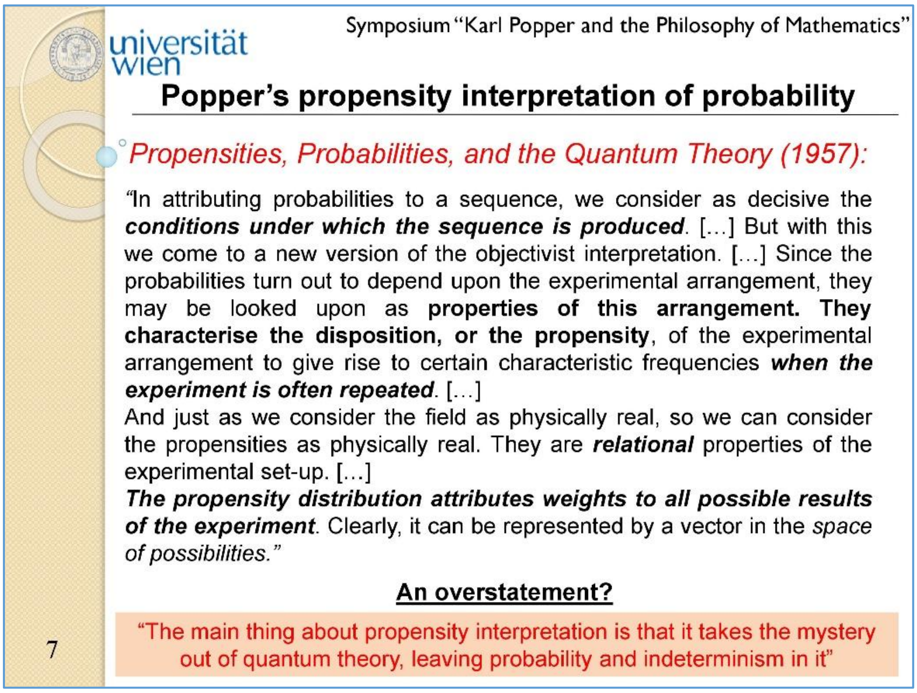
Popper's propensity interpretation of probability
Propensities, Probabilities, and the Quantum Theory (1957):
"In attributing probabilities to a sequence, we consider as decisive the **conditions under which the sequence is produced**. […] But with this we come to a new version of the objectivist interpretation. […] Since the probabilities turn out to depend upon the experimental arrangement, they may be looked upon as **properties of this arrangement. They characterise the disposition, or the propensity**, of the experimental arrangement to give rise to certain characteristic frequencies ***when the experiment is often repeated***. […]
And just as we consider the field as physically real, so we can consider the propensities as physically real. They are ***relational*** properties of the experimental set-up. […]
The propensity distribution attributes weights to all possible results of the experiment. Clearly, it can be represented by a vector in the *space of possibilities."*
7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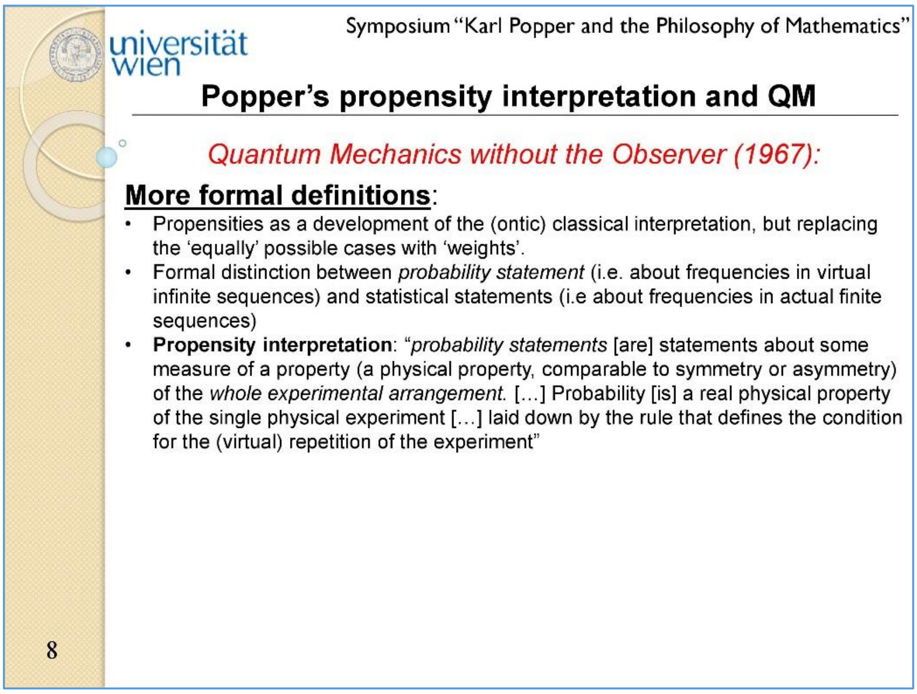
The propensity distribution attributes weights to all possible results of the experiment. Clearly, it can be represented by a vector in the *space of possibilities."*
An overstatement?
"The main thing about propensity interpretation is that it takes the mystery out of quantum theory, leaving probability and indeterminism in it"
7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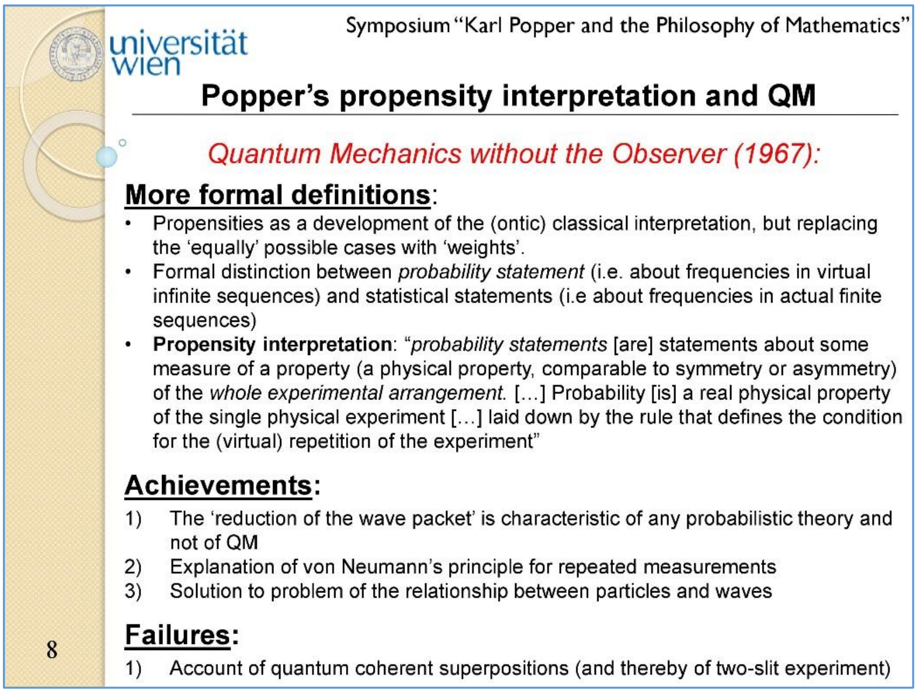
Popper's propensity interpretation and QM
Quantum Mechanics without the Observer (1967):
More formal definitions:
• Propensities as a development of the (ontic) classical interpretation, but replacing the 'equally' possible cases with 'weights'.
• Formal distinction between probability statement (i.e. about frequencies in virtual infinite sequences) and statistical statements (i.e about frequencies in actual finite sequences)
• Propensity interpretation: "probability statements [are] statements about some measure of a property (a physical property, comparable to symmetry or asymmetry) of the whole experimental arrangement. […] Probability [is] a real physical property of the single physical experiment […] laid down by the rule that defines the condition for the (virtual) repetition of the experiment"
8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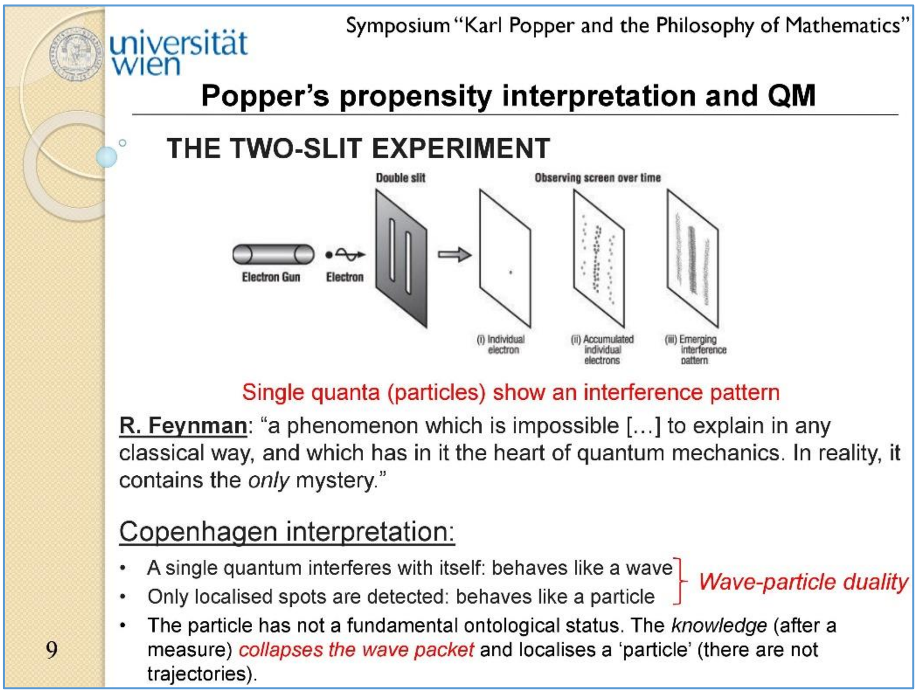
Achievements:
1) The 'reduction of the wave packet' is characteristic of any probabilistic theory and not of QM
2) Explanation of von Neumann's principle for repeated measurements
3) Solution to problem of the relationship between particles and waves
Failures:
1) Account of quantum coherent superpositions (and thereby of two-slit experiment)
8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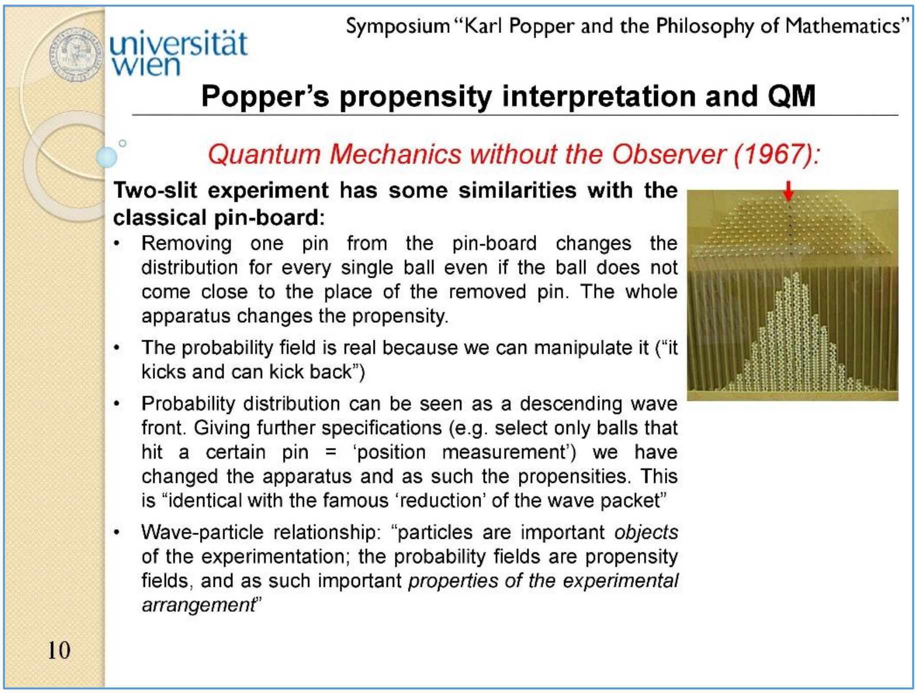
Popper's propensity interpretation and QM
THE TWO-SLIT EXPERIMENT
Double slit
Observing screen over time
Electron Gun
Electron
(i) Individual electron
(ii) Accumulated individual electrons
(iii) Emerging interference pattern
Single quanta (particles) show an interference pattern
R. Feynman: "a phenomenon which is impossible […] to explain in any classical way, and which has in it the heart of quantum mechanics. In reality, it contains the only mystery."
Copenhagen interpretation:
A single quantum interferes with itself: behaves like a wave
Only localised spots are detected: behaves like a particle
Wave-particle duality
The particle has not a fundamental ontological status. The knowledge (after a measure) collapses the wave packet and localises a 'particle' (there are not trajectories).
9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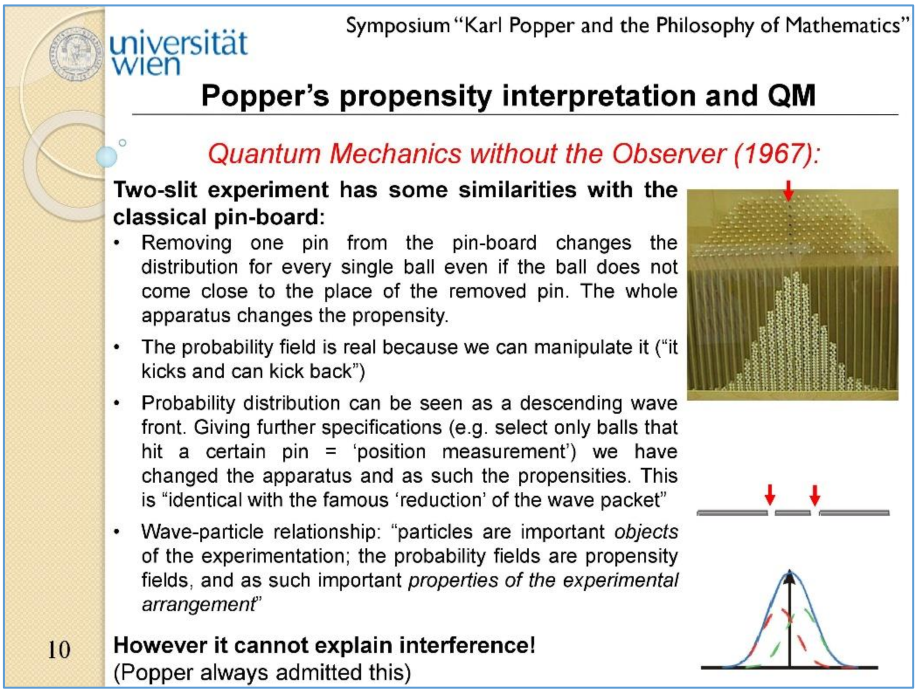
Popper's propensity interpretation and QM
Quantum Mechanics without the Observer (1967):
Two-slit experiment has some similarities with the classical pin-board:
• Removing one pin from the pin-board changes the distribution for every single ball even if the ball does not come close to the place of the removed pin. The whole apparatus changes the propensity.
• The probability field is real because we can manipulate it ("it kicks and can kick back")
• Probability distribution can be seen as a descending wave front. Giving further specifications (e.g. select only balls that hit a certain pin = 'position measurement') we have changed the apparatus and as such the propensities. This is "identical with the famous 'reduction' of the wave packet"
• Wave-particle relationship: "particles are important objects of the experimentation; the probability fields are propensity fields, and as such important properties of the experimental arrangement"
10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• Wave-particle relationship: "particles are important objects of the experimentation; the probability fields are propensity fields, and as such important properties of the experimental arrangement"
However it cannot explain interference!
(Popper always admitted this)
10

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Symposium “Karl Popper and the Philosophy of Mathematics”

First reaction of quantum physicists

Positive reactions from Popper's supporters:

- **03/03/1967 David Bohm:** “I feel that what you have to say about propensities makes a genuine contribution to clarifying the issues that you discuss”
- **05/09/1967 Alfred Landé**
- **11/09/1967 Hermann Bondi**

And the circle enlarges…

- **19/10/1968 Bartel Leendert van der Waerden**: “I fully agree with your 13 theses, and I feel it was very good you expounded them so clearly. I also agree with your propensity interpretation of probability. […] I feel my ideas are in perfect accordance with your theses. I discussed them with Heisenberg, [Carl Friedrich] von Weizsäcker and [Friedrich] Bopp after a lecture I gave about this subject in München, and we all agreed”
- **04/03/1969 Louis de Broglie:** “I noticed with great pleasure that your ideas are very close to mine.”

But also rebuttals

- **Paul Feyerabend**
- **Jeffrey Bub**

11

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Symposium “Karl Popper and the Philosophy of Mathematics”

The controversy with Feyerabend

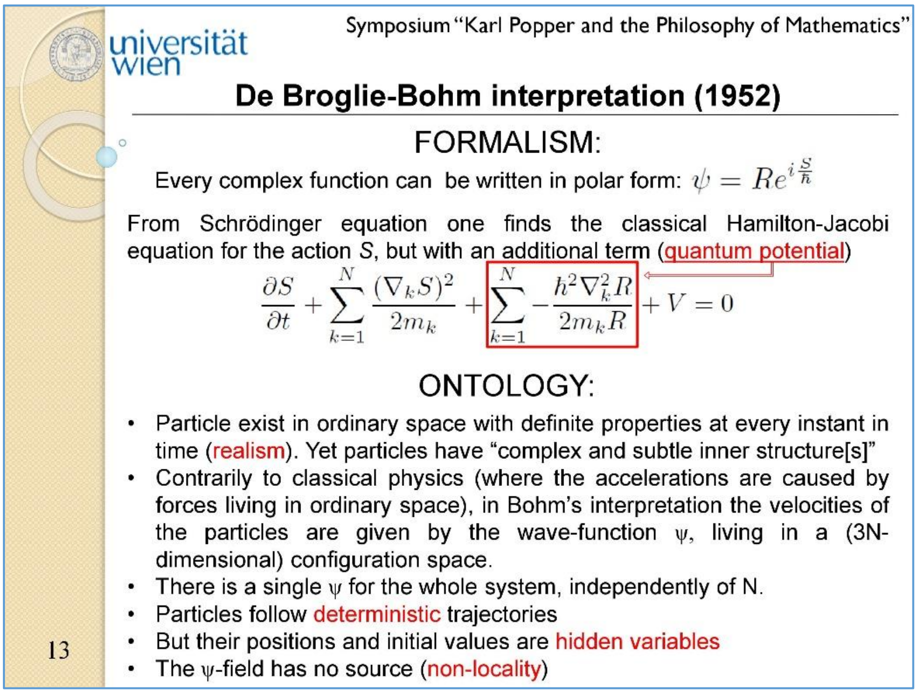
The controversy with Feyerabend:

- 1956-1961 **Feyerabend opposed Copenhagen interpretation** (being strongly influenced by Bohm and his mentor Popper)
- December 1968: **Feyerabend published “On a Recent Critique of Complementarity”: extremely severe critique of (Popper 1967), in particular of his pin-board example.**
- 1969 **Margenau and Landé defend Popper**

But Popper never intended to explain interference with the pin-board argument (and he could not in fact explain it with propensities)

Feyerabend's motivation also in personal resentment: “I was mad at Popper [because] his paper did not pay any attention to my criticism of 1962. Maybe he had not read my paper (which I sent him); maybe he did not like it.” (Feyerabend to J. Watkins, 17/12/1967)

12


Symposium "Karl Popper and the Philosophy of Mathematics"
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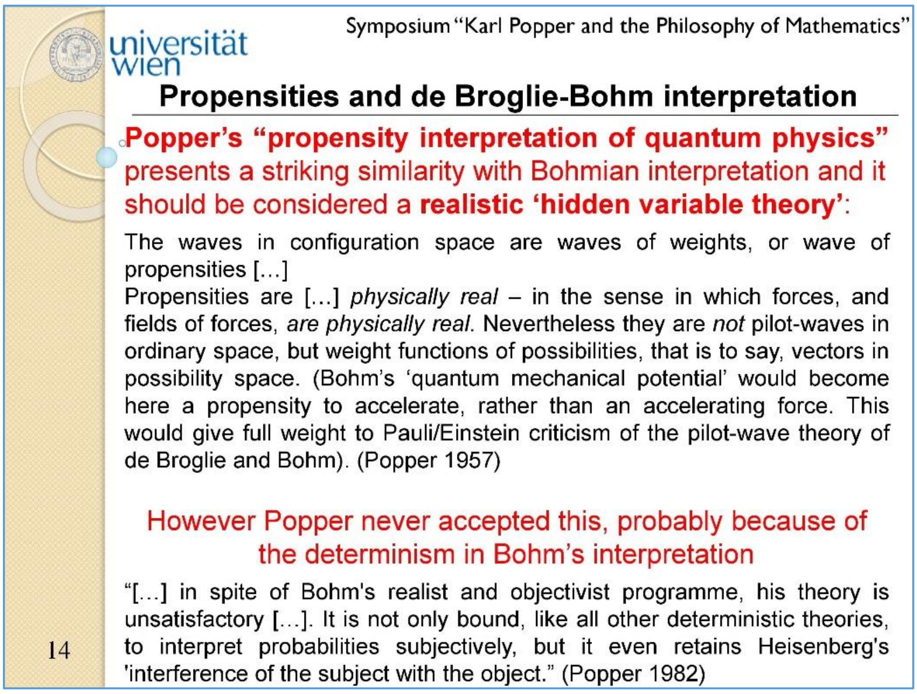
De Broglie-Bohm interpretation (1952)
FORMALISM:
Every complex function can be written in polar form: $\psi = Re^{i\frac{S}{\hbar}}$
From Schrödinger equation one finds the classical Hamilton-Jacobi equation for the action S, but with an additional term (quantum potential)
$$\frac{\partial S}{\partial t} + \sum_{k=1}^{N} \frac{(\nabla_k S)^2}{2m_k} + \boxed{\sum_{k=1}^{N} -\frac{\hbar^2 \nabla_k^2 R}{2m_k R}} + V = 0$$
ONTOLOGY:
• Particle exist in ordinary space with definite properties at every instant in time (realism). Yet particles have "complex and subtle inner structure[s]"
• Contrarily to classical physics (where the accelerations are caused by forces living in ordinary space), in Bohm's interpretation the velocities of the particles are given by the wave-function ψ, living in a (3N-dimensional) configuration space.
• There is a single ψ for the whole system, independently of N.
• Particles follow deterministic trajectories
• But their positions and initial values are hidden variables
• The ψ-field has no source (non-locality)
13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Propensities and de Broglie-Bohm interpretation
Popper's "propensity interpretation of quantum physics" presents a striking similarity with Bohmian interpretation and it should be considered a realistic 'hidden variable theory':
The waves in configuration space are waves of weights, or wave of propensities […]
Propensities are […] physically real – in the sense in which forces, and fields of forces, are physically real. Nevertheless they are not pilot-waves in ordinary space, but weight functions of possibilities, that is to say, vectors in possibility space. (Bohm's 'quantum mechanical potential' would become here a propensity to accelerate, rather than an accelerating force. This would give full weight to Pauli/Einstein criticism of the pilot-wave theory of de Broglie and Bohm). (Popper 1957)
However Popper never accepted this, probably because of the determinism in Bohm's interpretation
"[…] in spite of Bohm's realist and objectivist programme, his theory is unsatisfactory […]. It is not only bound, like all other deterministic theories, to interpret probabilities subjectively, but it even retains Heisenberg's 'interference of the subject with the object." (Popper 1982)
14

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Propensities as a 'hidden variable interpretation'

- Popper's criticism seems to be directed towards de Broglie's original idea of a wave living in ordinary space associated with each particle.
- Bohm's model posits a single (non-local) wave that acts in the configuration space and guides the particles in ordinary space.
- Propensities may be also seen as waves of possibilities in a configuration space.
- Popper's propensities can be seen as a form of realist 'hidden-variable' (i.e. not directly detectable, but only through an indirect manipulation of the apparatus and therefore really existing)
- Propensities as hidden variables are weakly non-local (a localised change of the apparatus influence the whole distribution but only within the light cone) and contextual (different experimental settings are never equivalent)
  ⇨ Propensities survive the fundamental limitation imposed by Kochen-Specker theorem but not sure about Bell's theorems.

The main disagreement between Popper's and Bohm's view on QM is then determinism, however…

16

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An unpublished letter from Bohm to Popper

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DB/DG

13th July, 1984

Prof. Sir Karl Popper,
Fallow Field,
Manor Road,
Penn,
Bucks.

Dear Sir Karl,

I have just finished re-reading your book "Quantum Theory and the Schism in Physics". I feel that much of what you say in this book is very acceptable. I certainly think that a realistic interpretation of physics is essential. I think also that I understand your propensity interpretation of probability, and have no objections against it. So, on these fundamentals we are in close agreement.

However, I feel that you have not properly understood my own point of view, which is much less different from yours than is implied in your book. Firstly, I am not wedded to determinism. It is true that I first used a deterministic version of the coaxial interpretation of the quantum theory. But later, with Vigier[1], a paper was written, in which we assumed that the movement of the particle was a stochastic process. Clearly, this is not determinism. Indeed, we can regard the stochastic movement of the particle as affected by a field of propensities, in accordance with your ideas on probability.

The key question at issue is therefore not that of determinism vs. indeterminism. I personally do not feel addicted to determinism, but I am ready to consider deterministic proposals, if they seem relevant in the context of interest, and if they offer some useful insight. I think that this is what the causal interpretation in its original form is doing.

The basic question is, in my view, that of realism. What is to be meant by realism? It is not enough to say that something is real, if it kicks back at you. Indeed, a dream entity can give one a very 'realistic' impression of a kick. We must also add, for discussion on a philosophical level, that what is real has a being independent of the consciousness of the observer. John Bell has used the term "beable" to describe such an independent reality.

From the point of view of realism, the main criticism of the orthodox interpretation of the quantum theory is that it has no room in it for "beables". It only mentions what can be observed.

1. Phys. Rev. 96, 208 (1954).

- 2 -

Even if we use the propensity interpretation of probability, these are mainly propensities for producing certain observable results (and not, for the independent existence of, for example, certain entities in certain states of being).

I introduced the notion that the "beables" of the quantum theory are the particles and the wavefunction (which contains information about the propensities). Along with Vigier, I can say that the "beables" are themselves conditioned by such propensities. What are called the observables of quantum theory are then potentialities of the "beables", realized according to a context, which in current physics, is determined by the experimental arrangement (though in nature, similar contexts will still exist without the intervention of human beings).

The entire process is thus objective. But without "beables" of some kind, this objectivity has no clear meaning. I feel that your analysis does not pay enough attention to the question of the need for "beables". My proposal has been that the "beables" are particles (moving stochastically), along with the wave function I can see many reasons why these are not very attractive to most physicists. Indeed, I myself do not find them very "beautiful" or "elegant". But they do have the merit that they provide the only fully consistent realistic interpretation of the quantum theory that is now available (in this regard, I have several major criticisms of Lande's approach, and regard his proposals as far from consistent and adequate).

I hope that this will clarify my point of view, and am looking forward to hearing from you what you think about my proposals.

Yours sincerely,

David Bohm

D. Bohm

P.S. I enclose a paper that illustrates my ideas in more detail.

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Thank you!

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Acknowledgments

I am indebted to the *Karl Popper Sammlung* of the AAU in Klagenfurt (Austria) for granting me the access to Popper's original correspondence and unpublished preprints on which this presentation is based.

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Independence (Probabilistic) and Independence (Logical)

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Popper and the Philosophy of Mathematics

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CONTENTS

0 Summary: the problem

The first task of this lecture is to present a well known problem concerning probabilistic independence that arises whenever elements with extreme probabilities (probabilities of 0 and 1) are of serious interest, to criticize briefly a solution published in 2017 by two leading writers in this area, and to compare it with the solution offered by Karl Popper in 1994 in appendix $*XX$ of Logik der Forschung.

The question inevitably arises of what job the relation of probabilistic independence should be asked to perform, in particular how it is related to logical independence. Relevant here is an extensive 1997 discussion by Georg Dorn.

0 Summary: a solution

It turns out that probabilistic independence, however defined, and logical independence, as usually understood, are not smoothly related. This may be of scant concern to physical interpretations of probability, but it is an unwelcome result for the logical interpretation, whose main aim is to give a metrical generalization of logical relations.

The second task of the lecture is to resolve the problem by understanding logical independence in a different (but not scandalously different) way, and by replacing logical probability by a different measure, contraprobability (which in Miller & Popper 1986 was called deductive dependence).

1 The classical definition of independence

In the classical theory, codified in the axioms of Kolmogorov, in which absolute probability $\mathfrak{p}(a)$ is primitive, elements a, b within the domain of the function $\mathfrak{p}$ are defined in this way to be probabilistically independent:

DU $\qquad \mathcal{U}(a, b) \leftrightarrow \mathfrak{p}(ab) = \mathfrak{p}(a)\mathfrak{p}(b).$

The relation $\mathcal{U}$ is symmetric. An immediate and untoward consequence is the probabilistic independence of every element a from every element b for which $\mathfrak{p}(b) = 0$; in particular every element a is probabilistically independent of the contradictory (or zero) element s, even though a, being deducible from s, is logically dependent on s.

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1 Elements with probability 1

It is rather less obvious that every element b with probability 1 stands in the relation $\mathcal{U}$ to every element a. For if $\mathfrak{p}(b) = 1$, then $\mathfrak{p}(a \vee b) \leq \mathfrak{p}(b)$, whence by the addition and monotony laws $\mathfrak{p}(a) \leq \mathfrak{p}(ab) \leq \mathfrak{p}(a)$, from which it follows that $\mathfrak{p}(ab) = \mathfrak{p}(a)\mathfrak{p}(b)$. As a consequence, the tautological (or unit) element t is probabilistically independent of every element a, even though t, being deducible from a, is logically dependent on a.

In response to these oddities, the classical theory admits a stronger (asymmetric) sense of independence in terms of the relative (often called conditional) probability $\mathfrak{p}(a, b)$.

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1 Alternative classical definition of independence

The **relative** (or **conditional**) probability $\mathfrak{p}(a, b)$ is defined, whenever $\mathfrak{p}(b) \neq 0$, by $\mathfrak{p}(a, b) = \mathfrak{p}(ab)/\mathfrak{p}(b)$.

The stronger classical definition of independence is this:

DV $\qquad \mathcal{V}(a, b) \leftrightarrow \mathfrak{p}(a, b) = \mathfrak{p}(a)$.

Since $\mathfrak{p}(a, b)\mathfrak{p}(b) = \mathfrak{p}(ab)$ for any b, DV implies DU. Moreover, $\mathcal{V}(a, b)$ holds, just as $\mathcal{U}(a, b)$ does, when $\mathfrak{p}(b) = 1$. It holds too when $\mathfrak{p}(a) = 0$ and $\mathfrak{p}(b) \neq 0$. But the conclusion that any element a is independent of s is thwarted, since the term $\mathcal{V}(a, s)$ is ill formed. Nonetheless, $\mathcal{V}(t, b)$ and $\mathcal{V}(s, b)$ both hold when $b \neq s$.

1 Popper's axiomatization of probability

In the theory presented in appendices $*$iv and $*$v of The Logic of Scientific Discovery the term $\mathfrak{p}(a, b)$ is well defined for all a, b. Elements a, b have a **conjunction** ab, and each a has a **negation** a'. The **disjunction** $a \vee b$, which is defined via De Morgan's laws, obeys the general addition law $\mathfrak{p}(a, c) + \mathfrak{p}(b, c) = \mathfrak{p}(ab, c) + \mathfrak{p}(a \vee b, c)$.

The self-contradictory and tautological elements s and t are defined in the usual way. The value of $\mathfrak{p}(a, s)$ is equal to 1 for every a, and so the special addition law $\mathfrak{p}(a, b) + \mathfrak{p}(a', b) = 1$ holds if & only if b is distinct from s. The **absolute probability** $\mathfrak{p}(a)$ of a is identified with $\mathfrak{p}(a, t)$.

2 Popper's 1994 definition of independence

Made aware by Dorn of the problems, noted above, in the classical definition of independence when applied to s and t (and to other elements with extreme probabilities), but also wanting some contingent statements with probability 1, such as 'There exists a white raven' and 'There exists a golden mountain', to count as independent of each other, Popper defined two new relations of weak independence $\mathcal{W}(a, b)$ and independence $\mathcal{I}(a, b)$:

DW $\qquad \mathcal{W}(a, b) \leftrightarrow \mathfrak{p}(a, b) = \mathfrak{p}(a, b'),$

DI $\qquad \mathcal{I}(a, b) \leftrightarrow$
$$\mathcal{W}(a, b) \;\&\; \mathcal{W}(a', b) \;\&\; \mathcal{W}(b, a) \;\&\; \mathcal{W}(b', a).$$

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2 Simple properties of the relations $\mathcal{W}$ and $\mathcal{I}$

Popper showed several simple results about $\mathcal{W}$ and $\mathcal{I}$:

If $\mathcal{W}(a, b)$, then $\mathfrak{p}(a, b) = \mathfrak{p}(a)$; that is to say, $\mathcal{V}(a, b)$.

$\mathcal{I}$ is symmetric; that is, $\mathcal{I}(a, b)$ is equivalent to $\mathcal{I}(b, a)$.

$\mathcal{I}(a, b)$ is equivalent to $\mathcal{I}(a, b')$; and also to $\mathcal{I}(a', b)$ (and, he could have added, equivalent to $\mathcal{I}(a', b')$ too).

Near the end of appendix $*XX$, he showed directly that $\mathcal{I}(a, b)$ implies $\mathcal{U}(a, b)$, so that probabilistic independence, newly defined, 'implies classical independence'. Indeed, $\mathcal{U}$, $\mathcal{V}$, $\mathcal{W}$, and $\mathcal{I}$ are increasingly strict relations.

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2 The contribution of Fitelson & Hájek

A recent paper by Fitelson & Hájek, published in 2017, but on line since 2014, which adopts the practice of taking relative probability as primitive, also dismisses DU for implying that 'anything with extreme probability has the peculiar property of being probabilistically independent of itself'; this is a state of affairs that, they judge, may perhaps be acceptable for contingent events with probability 1 (§ 6), but is intolerable for those with probability 0 (§ 8).

They accordingly propose, as successors to DU, two definitions of probabilistic independence: one is DV released from the restriction that $\mathfrak{p}(b) \neq 0$, the other is DW.

2 The symmetry of the relation $\mathcal{I}$

It is to be regretted that Fitelson & Hájek have paid no attention to $*XX$ of Logik der Forschung. As we shall see, the definition DI is in several ways better than either DV or DW. No element a is counted by DI as independent of itself or of its negation: $\neg\mathcal{I}(a, a)$ and $\neg\mathcal{I}(a, a')$.

Like $\mathcal{U}$, the relation $\mathcal{I}$ is symmetric. The two independence relations $\mathcal{V}$ and $\mathcal{W}$ that Fitelson & Hájek favour (but do not choose between) are asymmetric. They seem to regard it as a discovery that 'on a Popperian account of independence', as they audaciously refer to their proposals, 'we must specify a direction of independence' (§ 8).

2 Popper's Haupttheorem

The main theorem (Haupttheorem) of Popper's appendix $*XX$ states that neither s nor t bears the relation $\mathcal{I}$ to any element (itself included). The proof reduces to this: by DW, $\mathcal{W}(\mathrm{a}, \mathrm{t})$ is equivalent to $\mathfrak{p}(\mathrm{a}) = 1$; whence, by the addition law, at least one of $\mathcal{W}(\mathrm{a}, \mathrm{t})$ and $\mathcal{W}(\mathrm{a}', \mathrm{t})$ is false. But each follows from $\mathcal{I}(\mathrm{a}, \mathrm{t})$, so $\mathcal{I}(\mathrm{a}, \mathrm{t})$ is false. Since $\mathcal{I}(\mathrm{a}, \mathrm{b})$ and $\mathcal{I}(\mathrm{a}, \mathrm{b}')$ are equivalent, $\mathcal{I}(\mathrm{a}, \mathrm{s})$ is also false. By symmetry, $\mathcal{I}(\mathrm{t}, \mathrm{a})$ and $\mathcal{I}(\mathrm{s}, \mathrm{a})$ are false.

Popper did not assert it (let alone prove it), but he would undoubtedly have been pleased that, according to DI, no element a is independent of itself, or of its negation a'.

 2-4

2 No element is independent of itself or its negation

The falsity of $\mathcal{I}(\mathrm{a}, \mathrm{a})$ is trivial when $0 < \mathfrak{p}(\mathrm{a}) < 1$, since $\mathcal{I}(\mathrm{a}, \mathrm{b})$ implies $\mathcal{U}(\mathrm{a}, \mathrm{b})$, but it may be proved for all elements a. Let me offer a proof in Popper's memory.

First of all, $\mathcal{I}(\mathrm{a}, \mathrm{a})$ reduces to the conjunction of $\mathcal{W}(\mathrm{a}, \mathrm{a})$ and $\mathcal{W}(\mathrm{a}', \mathrm{a})$; that is, to the conjunction of $\mathfrak{p}(\mathrm{a}, \mathrm{a}) = \mathfrak{p}(\mathrm{a}, \mathrm{a}')$ and $\mathfrak{p}(\mathrm{a}', \mathrm{a}) = \mathfrak{p}(\mathrm{a}', \mathrm{a}')$. Since $\mathfrak{p}(\mathrm{b}, \mathrm{b}) = 1$ for any b, these equations imply that $\mathfrak{p}(\mathrm{a}, \mathrm{a}')$ and $\mathfrak{p}(\mathrm{a}', \mathrm{a})$ both equal 1, so that the multiplication law, used twice, yields $\mathfrak{p}(\mathrm{a}) = \mathfrak{p}(\mathrm{a}') = \mathfrak{p}(\mathrm{a}\mathrm{a}') = 0$. The addition law precludes this, and so $\mathcal{I}(\mathrm{a}, \mathrm{a})$ is false. Since $\mathcal{I}(\mathrm{a}, \mathrm{b})$ and $\mathcal{I}(\mathrm{a}, \mathrm{b}')$ are equivalent for all b, $\mathcal{I}(\mathrm{a}, \mathrm{a}')$ too is false.

 2-5

2 Independence of elements with unit probability

Another result that Popper envisaged, but did not prove, is the possibility that two elements with unit probability, such as two logically independent existential statements, can be probabilistically independent. To show this, let $\mathfrak{t}$ be the closed interval $[0, 1]$, and $\mathfrak{a}$ and $\mathfrak{b}$ be the half-open intervals $(0, 1]$ and $[0, 1)$. Under the uniform measure:

	$\mathfrak{a}$	$\mathfrak{a}'$	$\mathfrak{b}$	$\mathfrak{b}'$			
$\mathfrak{a}$	1	0	1	1	|	$\mathfrak{p}(\mathfrak{a}, \mathfrak{b}) = 1 = \mathfrak{p}(\mathfrak{a}, \mathfrak{b}')$	$\mathcal{W}(\mathfrak{a}, \mathfrak{b})$
$\mathfrak{a}'$	0	1	0	0	|	$\mathfrak{p}(\mathfrak{a}', \mathfrak{b}) = 0 = \mathfrak{p}(\mathfrak{a}', \mathfrak{b}')$	$\mathcal{W}(\mathfrak{a}', \mathfrak{b})$
$\mathfrak{b}$	1	1	1	0	|	$\mathfrak{p}(\mathfrak{b}, \mathfrak{a}) = 1 = \mathfrak{p}(\mathfrak{b}, \mathfrak{a}')$	$\mathcal{W}(\mathfrak{b}, \mathfrak{a})$
$\mathfrak{b}'$	0	0	0	1	|	$\mathfrak{p}(\mathfrak{b}', \mathfrak{a}) = 0 = \mathfrak{p}(\mathfrak{b}', \mathfrak{a}')$	$\mathcal{W}(\mathfrak{b}', \mathfrak{a})$

2 Logical independence of the conjuncts in $\mathcal{I}(\mathfrak{a}\ \mathfrak{b})$

The definition DW suggested by Popper for the weak independence $\mathcal{W}(\mathfrak{a}, \mathfrak{b})$ of elements $\mathfrak{a}$ and $\mathfrak{b}$ is given by the equation $\mathfrak{p}(\mathfrak{a}, \mathfrak{b}) = \mathfrak{p}(\mathfrak{a}, \mathfrak{b}')$, while his definition DI of independence simpliciter consists of the conjunction of the four formulas $\mathcal{W}(\mathfrak{a}, \mathfrak{b}), \mathcal{W}(\mathfrak{a}', \mathfrak{b}), \mathcal{W}(\mathfrak{b}, \mathfrak{a}), \mathcal{W}(\mathfrak{b}', \mathfrak{a})$.

Since DW, the less pedestrian of Fitelson & Hájek's preferred definitions of independence, is limited to the first of these formulas, it is natural to ask what work the other three conjuncts do in the definiens of $\mathcal{I}(a, b)$. Are they all needed? Is each of them logically independent of the others? Are there any logical connections among them?

2 Complete logical independence

Complete independence was introduced, and exploited, by E. H. Moore (1910). It is well known to philosophers as the kind of independence enjoyed by atomic propositions in Wittgenstein's Tractatus; that is, a set of statements is completely independent if all the members of any subset can be true while all the remainder are false. A completely independent set is consistent — its members can all be true together — and (simply) independent — one member can be false while all the others are true.

It turns out that the set of conjuncts in $\mathcal{I}(a, b)$ is consistent and independent, but not completely independent.

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2 What is easily shown, and what can be shown

We know that if neither $\mathfrak{p}(a)$ nor $\mathfrak{p}(b)$ equals either 0 or 1, then $\mathcal{W}(a, b)$, $\mathcal{W}(a', b)$, $\mathcal{W}(b, a)$, and $\mathcal{W}(b', a)$ are logically equivalent ways of stating the probabilistic independence of a and b (according to DU). This means that they are all true when a and b are, like reports of throws of dice, independent, and all false when they are not.

It will be shown that exactly one of $\mathcal{W}(a, b)$, $\mathcal{W}(a', b)$, $\mathcal{W}(b, a)$, and $\mathcal{W}(b', a)$ can be false, and that exactly two of them can be false. But it is not possible for exactly three of them to be false. By symmetry it will be enough to investigate just one singleton, one pair, and one triple.

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2 When two out of three conjuncts in $\mathcal{I}(a, b)$ are false

Suppose that $\mathcal{W}(a, b)$, the first conjunct of $\mathcal{I}(a, b)$, is true and that $\mathcal{W}(a', b)$, its second conjunct, is false. This supposition is equivalent, by DW, to the conjunction $\mathfrak{p}(a, b) = \mathfrak{p}(a, b')$ & $\mathfrak{p}(a', b) \neq \mathfrak{p}(a', b')$, which complies with the addition law if & only if b is identical with one or other of s and t. In either case $\mathfrak{p}(a)$ equals 1.

Now suppose that $\mathcal{W}(b, a)$, the third conjunct of $\mathcal{I}(a, b)$ is false. Then by DW, $\mathfrak{p}(b, a) \neq \mathfrak{p}(b, a')$, which is impossible if $b = t$. It follows that $b = s$ and therefore that $b' = t$. But then $\mathcal{W}(b', a)$, the fourth conjunct of $\mathcal{I}(a, b)$, which says that $\mathfrak{p}(b', a) = \mathfrak{p}(b', a')$, is true.

 2-10

2 Exactly two conjuncts in $\mathcal{I}(a, b)$ may be false

Given this analysis, it is straightforward to find elements a and b, and values for the function $\mathfrak{p}$ such that, of the four conjuncts that make up $\mathcal{I}(a, b)$, (i) only $\mathcal{W}(a', b)$ and $\mathcal{W}(b, a)$, the second and third conjuncts, are false, and (ii) only $\mathcal{W}(a', b)$, the second conjunct, is false.

We obtain an example where (i) two of the conjuncts in $\mathcal{I}(a, b)$ are true, and two are false, by identifying b with s and a with t. $\mathcal{W}(a, b)$ and $\mathcal{W}(b', a)$ then reduce (slightly differently) to $\mathfrak{p}(t, s) = \mathfrak{p}(t, t)$; that is, to the truth $1 = 1$, while $\mathcal{W}(a', b)$ and $\mathcal{W}(b, a)$ both reduce to $\mathfrak{p}(s, t) = \mathfrak{p}(s, s)$; that is, to the falsehood $0 = 1$.

 2-11

2 Exactly one conjunct in $\mathcal{I}(a, b)$ may be false

An example of (ii) is obtained by identifying b with t, and a with a distinct (that is, contingent) element with unit probability. Then $\mathfrak{p}(a, b) = 1 = \mathfrak{p}(a, b')$ and $\mathfrak{p}(b', a) = 0 = \mathfrak{p}(b', a')$; but $\mathfrak{p}(a', b) = 0 \neq 1 = \mathfrak{p}(a', b')$ and $\mathfrak{p}(b', a) = 0 = \mathfrak{p}(b', a')$. In short, $\mathcal{W}(a', b)$ is false, but the other three conjuncts in $\mathcal{I}(a, b)$ are all true. This example shows also that Popper's Haupttheorem does not follow from $\mathcal{W}(a, b)$, $\mathcal{W}(b, a)$, and $\mathcal{W}(b', a)$ together.

By an appeal to the invariance of $\mathcal{I}(a, b)$ if a and b are interchanged, or if either is replaced by its negation, we may extend these results to any choice of conjuncts.

2 The unnatural weakness of the definition DW

That the conjuncts of $\mathcal{I}(a, b)$ are not completely independent is an unexpected result, but does it have much significance? In advising us that whenever $\mathcal{W}(a, b)$ is true at least one of the other conjuncts in $\mathcal{I}(a, b)$ is true, it suggests that even if the definition DW commended by Fitelson & Hájek is not relinquished in favour of Popper's DI, it ought to be enriched with at least one further conjunct. This can be accomplished in seven different ways.

At several places the prospect of a range of relations of probabilistic independence (as there exists for logical independence) is equably entertained by Fitelson & Hájek.

3 Probabilistic dependence and confirmation

Not before time we must face the question of what problems a definition of probabilistic independence is designed to illuminate. I am thinking here not of physical interpretations of probability (frequency, propensity), where hypotheses of independence play a crucial role, but of what is called logical or epistemic or judgemental probability.

In various places Fitelson & Hájek adduce a link between probabilistic dependence and evidential or confirmational relevance. They ask, for example, apropos the dependence of any proposition on itself (§ 6): 'What better support, or evidence, for X could there be than X itself?'

3 Logical and probabilistic independence

A different idea, however, is visible when they write a few lines later that '[w]e may well want inductive logic, understood as probability theory, to be continuous with deductive logic'. This is the suggestion that probabilistic dependence and independence may be generalizations of logical dependence and independence. The viability of this suggestion is the topic of the rest of this lecture.

The matter is not entirely simple or entirely satisfactory. But, setting aside some subtleties for the moment, we may say that complete logical independence does not imply, and is not implied by, probabilistic independence.

3 Dorn's strong harmony requirement

Let us begin with the proposal that the logical dependence of $\mathfrak{a}$ and $\mathfrak{b}$ should ensure that they are also probabilistically dependent; in other words, if any one of the four relations $\mathfrak{a} \vdash \mathfrak{b}$, $\mathfrak{a} \vdash \mathfrak{b}'$, $\mathfrak{a}' \vdash \mathfrak{b}$, and $\mathfrak{b} \vdash \mathfrak{a}$, holds, then there is no probability measure $\mathfrak{p}$ under which $\mathfrak{a}$ and $\mathfrak{b}$ turn out to be probabilistically independent, $\mathcal{I}(\mathfrak{a}, \mathfrak{b})$.

This is a simplified form of the strong harmony requirement of Dorn (1997), § 11.6.1. If we exclude those probability measures under which some contingent elements have extreme probabilities, it is a truism. Nonetheless it is infringed, as Dorn was aware, by Popper's definition DI.

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3 A counterexample

Here is a simple example. Let $\mathfrak{t}$ be the closed interval $[0, 1]$, $\mathfrak{a}$ the half-open interval $(0, 1]$, and $\mathfrak{b}$ the singleton $\{1\}$. The uniform measure yields the relative probabilities given in the table below, from which the probabilistic independence $\mathcal{I}(\mathfrak{a}, \mathfrak{b})$ of $\mathfrak{a}$ and $\mathfrak{b}$ follows. Yet $\mathfrak{b}$ implies $\mathfrak{a}$. Likewise $\mathcal{I}(\mathfrak{a}', \mathfrak{b})$, yet $\mathfrak{b}$ contradicts $\mathfrak{a}'$. And so on.

	$\mathfrak{a}$	$\mathfrak{a}'$	$\mathfrak{b}$	$\mathfrak{b}'$			
$\mathfrak{a}$	1	0	1	1	|	$\mathfrak{p}(\mathfrak{a}, \mathfrak{b}) = 1 = \mathfrak{p}(\mathfrak{a}, \mathfrak{b}')$	$\mathcal{W}(\mathfrak{a}, \mathfrak{b})$
$\mathfrak{a}'$	0	1	0	0	|	$\mathfrak{p}(\mathfrak{a}', \mathfrak{b}) = 0 = \mathfrak{p}(\mathfrak{a}', \mathfrak{b}')$	$\mathcal{W}(\mathfrak{a}', \mathfrak{b})$
$\mathfrak{b}$	0	0	1	0	|	$\mathfrak{p}(\mathfrak{b}, \mathfrak{a}) = 0 = \mathfrak{p}(\mathfrak{b}, \mathfrak{a}')$	$\mathcal{W}(\mathfrak{b}, \mathfrak{a})$
$\mathfrak{b}'$	1	1	0	1	|	$\mathfrak{p}(\mathfrak{b}', \mathfrak{a}) = 1 = \mathfrak{p}(\mathfrak{b}', \mathfrak{a}')$	$\mathcal{W}(\mathfrak{b}', \mathfrak{a})$

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3 A simple-minded response

Dorn wrote (*loc.cit.*): 'in the light of the strong version of the harmony requirement, the semantics of probabilistic relations of dependence and independence is still in a mess'. I shall soon suggest a drastic way out of the mess, but first a straightforward expedient should be recorded.

This is to define $\mathfrak{a}$ and $\mathfrak{b}$ are probabilistically independent (with respect to $\mathfrak{p}$) as the conjunction of $\mathcal{I}(\mathfrak{a}, \mathfrak{b})$ and logical independence; and then to say that $\mathfrak{a}$ and $\mathfrak{b}$ are probabilistically independent simpliciter if they are so with respect to every function $\mathfrak{p}$. It follows at once that logically dependent elements are probabilistically dependent.

3 Is Dorn's requirement the right way round?

This proposal may seem to be rather contrived, but it does point to something odd about Dorn's strong harmony requirement. For in the normal run of things, it is not logical dependence that is regarded as a symptom of probabilistic dependence, but logical independence that is regarded as a symptom of probabilistic independence. (Often, it should be said, logical independence is required to be relative to a substantial background theory.)

Recall Popper's hope that 'There exists a white raven' should turn out to be probabilistically independent of the logically independent 'There exists a golden mountain'.

3 Admissible probability measures

Even in non-extreme cases, it is a decision, not a truism, to regard logically independent elements as probabilistically independent, and it incorporates no suggestion that the latter must hold for every probability measure $\mathfrak{p}$. The decision is a characterization of admissible probability measures, not a description of all possibilities. There are always measures under which the results of unrelated coin tosses, for example, are probabilistically dependent.

Unfortunately this way of connecting logical and probabilistic independence leads nowhere. It is firmly blocked if there exist more than two logically independent elements.

3 Complete logical independence leads to extremism

It was proved by Popper & Miller (1987) that if there exist three completely independent elements, and completely independent elements are asked to be probabilistically independent in the sense of DU (or DV, DW, or DI), then some of them have probability 0 or probability 1.

Proof. It is easily shown that if $\{\mathfrak{a}, \mathfrak{b}, \mathfrak{c}\}$ is a completely independent triple, then $\{\mathfrak{ac}, \mathfrak{b}\}$ and $\{\mathfrak{ab}, \mathfrak{cb}\}$ are completely independent pairs. It is implied by probabilistic independence that $\mathfrak{p}(\mathfrak{a})\mathfrak{p}(\mathfrak{b})\mathfrak{p}(\mathfrak{c}) = \mathfrak{p}(\mathfrak{ac})\mathfrak{p}(\mathfrak{b}) = \mathfrak{p}(\mathfrak{abc}) = \mathfrak{p}(\mathfrak{ab})\mathfrak{p}(\mathfrak{cb}) = \mathfrak{p}(\mathfrak{a})\mathfrak{p}(\mathfrak{b})^2\mathfrak{p}(\mathfrak{c})$, and therefore at least one of $\mathfrak{p}(\mathfrak{a})$, $\mathfrak{p}(\mathfrak{b})$, and $\mathfrak{p}(\mathfrak{c})$ is 0, or $\mathfrak{p}(\mathfrak{b}) = 1$.

4 Maximal logical independence

There is another familiar generalization of simple logical independence. The pair $\{a, b\}$ is said to be **maximally independent** if neither a nor b is a consequence of the other (simple independence) and, additionally, no (non-tautological) consequence of a or of b is a consequence of the other. Their common consequences are exactly the tautologies (which are consequences of all elements). In traditional terms a and b are independent **subcontraries**.

This definition can be generalized to all sets of elements. Tarski proved in 1930 that every set of elements is equivalent to a maximally independent set (it may be empty).

 4-0

4 Contrasting complete and maximal independence

In short, if a and b are completely independent then neither provides any information about the **truth value** of the other, while if they maximally independent neither provides any information about the **content** of the other.

In §5 of 'Carnap's Inductive Logic' (1967) Salmon asserted that statements a and b that are 'entirely about the past' and 'entirely about the future' respectively are independent in some sense, but not maximally independent. But then $a \vee b$ is entirely about both the past and the future, which implies that it is about neither, and is a tautology. This can hardly be what Salmon intended.

 4-1

4 Conflict with probabilistic independence

Theorem 1 of Popper & Miller (1987), mostly anticipated in their (1983) paper, states that, if a and b are maximally independent, then $p(a, b) < p(a)$, except when $p(a, b) = 1$ or $p(b) = 1$. That is, except in extreme cases, maximally independent elements are not probabilistically independent in any of the senses considered.

What is causing this impasse is not, I suggest, the unavailability of an appropriate sense of logical independence. What is missing is a way of measuring degrees of deducibility that is distinct from orthodox probability measures and in harmony with maximal independence.

4 Contraprobability

The function $q(a, b)$, here to be called the contraprobability of a given b, is defined as equal in value to $p(b', a')$. By the law of contraposition, a' logically implies b' if & only if b logically implies a, and in this case both $p(a, b)$ and $q(a, b)$ take the value 1. But whereas $p(a, b) = 0$ whenever a and b are contraries, $q(a, b) = 0$ whenever a and b are subcontraries; in particular, $q(a, b) = 0$ whenever the elements a and b are maximally independent.

In contrast to $q(a, b)$, $p(a, b) - p(a) = 0$ whenever a and b are probabilistically independent (by DV, DW, DI), but not, alas, whenever they are logically independent.

4 Harmony at last

Observe that the result just stated holds for all underlying probability measures $\mathfrak{p}$, so that something akin to Dorn's harmony requirement, albeit in the opposite direction, has been rehabilitated. Dorn hoped that logical dependence might imply probabilistic dependence for every probability measure, while what is demonstrable is that maximal independence implies contraprobabilistic independence [that is, $\mathfrak{q} = 0$] for every probability measure.

Yet there are measures under which $\mathfrak{q}(a, b) = 0$ when a and b are not maximally independent. For $\mathfrak{p}(a'b') = 0 \neq \mathfrak{p}(b')$ is quite possible when $a \vee b$ is not a tautology.

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Popper on Deductive Logic and Logical Deduction

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Overview

I am a veteran, as far as Popper's theory of deductive logic is concerned.

I hope I can say something new.

Plan:

- Popper's idea of inferential definitions of logical constants
- The logicality interpetation
- The structuralist interpetation
- The semantical interpretation

For more historical aspects and the edition we are preparing in Tübingen ⟶ David Binder's talk

My general background (then and now)

“Proof-theoretic semantics” (my term)

which is a special form of “inferentialism” (Brandom’s term)

which itself is a special form of “meaning as use” (Wittgenstein’s term)

“Proof-theoretic semantics is an alternative to truth-condition semantics. It is based on the fundamental assumption that the central notion in terms of which meanings are assigned to certain expressions of our language, in particular to logical constants, is that of *proof* rather than *truth*. In this sense proof-theoretic semantics is semantics *in terms of proof*.” (SEP entry)

“In terms of proof” means “in terms of rules of inference” – this is what Popper is claiming (after all, according to my actual interpretation, even though the meaning-theoretical background does not sound Popperian at all)

Klagenfurt 5.4.2018 – p. 3

Proof-theoretic semantics

Not just inferentialism — as opposed to denotationalism

More specific: “Gentzen semantics” in von Kutschera’s terminology.

Traditionally:

- Proof theory vs. model theory
- Syntax vs. semantics

Instead:

- *Semantics* should not be left to denotationalism
- *Proof* not merely syntactic

“General Proof Theory”

Klagenfurt 5.4.2018 – p. 4

The big names in the field

- Gentzen
- The Swedish school (Prawitz, Martin-Löf)
- German theoreticians (von Kutschera, Lorenzen)
- The French school of Girard
- Dummett on the philosophical side
- Brandom as a multiplicator for general philosophy

Quotations from Girard

"Everybody has seen a proof, maybe not a formal one, but at least something that can be transformed into a formal proof by a computer. But nobody has ever seen the tail of a model, models are ideal (usually infinite) objects."

"... commonsense should tell us that nobody has ever seen the class of all integers, not to speak of this 'Book of Truth and Falsehood' supposedly kept by Tarski. All these abstractions, truth, standard integers, are handled through proofs ..."

Quotations from Girard

"We are facing a *transcendental* explanation of logic '*The rules of logic have been given to us by Tarski, which in turn got them from Mr. Metatarski*', something like '*Physical particles act in this way because they must obey the laws of physics*'."

"The distinction between $\vee$ and a hypothetical meta-$\vee$ ist just a way to avoid the problem: you ask for real money but you are paid with meta-money."

Klagenfurt 5.4.2018 – p. 7

My special background in the early 1980s

- 1981 Thesis on the rule-based interpretation of logical constants (Hasenjaeger, Prawitz)
- Had been in contact with Popper about Kurt Grelling already in 1980
- Sent him a draft of my paper (appeared 1984) on the logicality interpretation of his logical writings on 1st July 1982
- Received an immediate reply dated 9th July 1982 with general remarks
- Received a further reply dated 10th July 1982 with detailed remarks based on Popper's reading of the manuscript
- Early 1983 rejection by BJPS, according to Popper (met at a conference in Leicester, together with David Miller) directed against him
- 1983 accepted and published (1984) by Grattan-Guinness in *History and Philosophy of logic*

Klagenfurt 5.4.2018 – p. 8

Popper's papers on deductive logic 1947-1949

Titles such as

- Logic without Assumptions
- New Foundations for Logic
- The Trivialization of Mathematical Logic

All in the spirit of Gentzen, strongly appealing to a proof-theoretic semanticist.

Klagenfurt 5.4.2018 – p. 9

Reactions by 1980

- Reviews by: Ackermann, Beth, Curry, Hasenjaeger, Kleene, McKinsey: Very critical (but overall fair)
- Remarks by: Kneale, Kneale & Kneale, Prior, Shoesmith & Smiley, Bartley and others
- Some remarks in footnotes
- Thesis work: B. Brooke-Wavell 1958 (Ph.D.), M. Dunn 1963 (Hon.)
- Only longer paper: Lejewski in Schilpp-volume

Reactions from the Popperians: Practically none

Popper had already given up the program shortly after its publication

Klagenfurt 5.4.2018 – p. 10

Incompatibility with Popper's "standard" views

- Anti-foundationalist and anti-justificationist
  Now: Foundations of logic are sought in a non-falsificationist way
  Now: Attempt at a justification of logical inferences
- Anti-epistemologist
  Now: Logic based on deduction and inference
- Anti-definitionalist
  Now: Logic extracted from definitions
- Truth primary notion
  Now: Proof theory is fundamental
- Classical logic fundamental
  Now: Constructive ("intuitionistic") logic considered a serious alternative

In later writings Popper returned to the "standard" view

Popper's procedure

Define a multi-ary *inference relation* "/", denoting that B can be deduced from $A_1, \ldots, A_n$:

$$A_1, \ldots, A_n / B$$

equipped with the principles of "reflexivity" and "generalized transitivity":

$$A_1, \ldots, A_n / A_i \quad (1 \leq i \leq n)$$

$$\frac{A_1, \ldots, A_n / B_1 \quad \ldots \quad A_1, \ldots, A_n / B_m \quad B_1, \ldots, B_m / C}{A_1, \ldots, A_n / C}$$

These are equivalent to Gentzen's structural rules.

They are called "absolutely valid" by Popper.

Side remark

Popper's "absolutely valid" rules are identical with the principles used by Paul Hertz (1881-1940) in one of his systems. Hertz was the immediate precursor of Gentzen, on whom Gentzen heavily built. He layed the foundations for structural reasoning and even paved the way for a proof-theoretical understanding of the resolution method.

It is unlikely that Popper knew Hertz's work in much detail. In a footnote, he refers to discussions with Bernays who drew his attention to the close relationship of his work with that of Tarski, Gentzen, and Hertz (Mind 1947, p. 204). This was probably after his work had already reached its final stage.

In any case it is remarkable that Popper realized the significance of logic-free inference principles as the basis of logical reasoning. Even 12 years after Gentzen's thesis, this was not a commonplace.

Inferential definitions

Define logical constants by giving equivalence conditions in terms of deducibility ("*inferential definitions*"):

$$C//A{\wedge}B \quad \leftrightarrow \quad (C/A \;\&\; C/B \;\&\; A,B/C)$$

$$C//A{\supset}B \quad \leftrightarrow \quad \forall D(D/C \;\leftrightarrow\; D,A/B)$$

$$C//A{\vee}B \quad \leftrightarrow \quad \forall D(C/D \;\leftrightarrow\; (A/D \;\&\; B/D))$$

$$C//\neg A \quad \leftrightarrow \quad \forall D\,\forall E(C,D/A \rightarrow (C,D/E \;\&\; D,E/A))$$

General form of an inferential definition:

$$C//S(A_1,\ldots,A_n) \;\leftrightarrow\; \mathfrak{A}(C,A_1,\ldots,A_n)$$

Right side turned into rules

$A, B/A \wedge B \quad A \wedge B/A \quad A \wedge B/B$

$$\frac{\Gamma, A/B}{\Gamma/A \supset B} \qquad \frac{\Gamma, A/D \quad \Gamma, B/D}{\Gamma, A \vee B/D}$$

$$\frac{\Gamma, \neg A/A}{\Gamma, \neg A/C} \qquad \frac{\Gamma, \neg A/A}{\Gamma/A}$$

This set is complete for classical propositional logic.

The rules make perfect sense for proof-theoretic semantics (with the usual problems for classical negation).

All right sides of inferential definitions considered by Popper can be turned into sets of rules, even though sometimes inconsistent.

Klagenfurt 5.4.2018 – p. 15

The left side of an inferential definition

$C//A * B \ \leftrightarrow \ \ldots$

This guarantees uniqueness.

In proof-theoretic semantics this is an absolutely crucial principle.

However, this is only weak uniqueness, which means that we cannot obtain an explicit definition via an analogue to Beth's definability theorem.

But weak uniqueness is what is considered sufficient in proof-theoretic semantics.

Popper aims at: implicit definition plus uniqueness.

Klagenfurt 5.4.2018 – p. 16

Popper's semantical claim

If we have a set of rules that is equivalent to the right side of an inferential definition, then these rules are called "fully characterizing rules".

Fully characterizing rules provide a semantics of the operators involved — the rules are meaning-determining.

In fact, practically all examples of inferential definitions given by Popper consist of the provision of rules.

This means that Popper is almost always concerned with meaning-determining rules.

The fact that the rules of logic follow from the (right side of) definitions "trivializes" logic.

We need no *assumptions* about logical constants, only their *definitions*.

Exception

Constants that are defined in terms of other constants sometimes don't have direct characterizing rules.

Example: The ternary constant $A \vee (B \rightarrow C)$

This can be given an inferential definition. Its right hand side essentially says the following:

Any D that is an implication between B and C, when conjoined with A disjunctively, has the force of the constant in question.

It can be proved that there is no 'direct' set of rules for $A \vee (B \rightarrow C)$.

The fundamental objection

It can be that the "defined" constant does not exist and cannot exist.

Much later this was Prior's argument against inferentialism, using his operator *tonk*, defined by

$$A / A \text{ tonk } B \qquad\qquad A \text{ tonk } B / B$$

or of Peano's addition operation, defined by

$$\frac{a}{b} \oplus \frac{c}{d} = \frac{a+c}{b+d}$$

which yields, e.g.,

$$\frac{2}{3} = \frac{1+1}{1+2} = \frac{1}{1} \oplus \frac{1}{2} = \frac{1}{1} \oplus \frac{2}{4} = \frac{1+2}{1+4} = \frac{3}{5}$$

Popper gave up

1. The real reason: Tarski didn't like his work on logic.
2. Another reason: No real success.

Tarski's reaction

"herzlichen Dank für Ihren Brief vom 1. Juli 1982 und für Ihr großes Interesse. Ich wurde damals dadurch entmutigt, daß Alfred Tarski, den ich sehr verehre, diese Arbeiten nicht ansehen wollte. Ich hatte sonst niemanden.

Kurz vor seinem Tod lernte ich in Brüssel McKinsey kennen. Wir freundeten uns an: er sprach zuerst zu mir und sagte mir, daß seine Kritik dieser Arbeiten nicht

Popper's reaction should have been: "so what"

Embarassment of failure

Philosophisch waren diese Untersuchungen (für deren teilweise Mißerfolg ich mich schämte) sehr wichtig für mich, da sie mich in meinem tentativen Antirelativismus einigermaßen (= tentativ) bestärkten; insbesondere der Nachweis, der mir wichtig schien, daß in einer Sprache, die die klassische Negation enthält (und das tut die intuitionistische Logik) '$\neg_{cl}\, a \,//\, \neg_{int}\, a$' gilt. Aber meines Wissens hat niemand außer Ihnen dieses Resultat gesehen. (Ich kenne die Literatur nicht, außer McKinsey.) Zu

Interesting is the emphasis on the collapsing result for negation.

Popper gave up

1. The real reason: Tarski didn't like his work on logic.
2. Another reason: No real success.

Ad 1. Popper gave too much credit to Tarski.
Tarski did not properly appreciate Proof Theory and in particularly not Gentzen's work.

Ad 2. Popper was much ahead of his time.
The philosophical appreciation of proof theory was not on its way yet. — It started only with Prawitz 1969, and even there from a perspective different from Popper's.

Popper did not even attempt to match the objections of the reviewers, in particular that of an implicit metalinguistic existence claim.

Klagenfurt 5.4.2018 – p. 23

Way out: The logicality interpretation

An inferential definition

$$C//S(A_1, \ldots, A_n) \leftrightarrow \mathfrak{A}(C, A_1, \ldots, A_n)$$

is a characterisation of logical constants in terms of deducibility.

$\mathfrak{A}(C, A_1, \ldots, A_n)$ only contains

- deducibility "/ "
- certain basic metalogical operators

Logicality with respect to the fundamental operations $\forall, \rightarrow, \&$.

The latter can even be given a 'transcendental' reading (cf. Lenk).

Klagenfurt 5.4.2018 – p. 24

Example: Disjunction

$C//A{\vee}B \;\leftrightarrow\; \forall D(C/D \;\leftrightarrow\; (A/D \;\&\; B/D))$

is interpreted as follows:

If there is a constant ∨ of disjunction in our language whose deductive behaviour can be described in exactly this way, then this constant ∨ is a logical constant
— because its characterisation has this particular form.

We distinguish between logicality and semantics. The existence assumption is left outside.

The has been made a standard principle in proof-theoretic semantics in Došen's work since 1980.

Popper's opinion of the logicality interpretation

This interpretation has considerable support in the texts (quotations in my paper).

It links Popper's intentions with that of Tarski.

However, he was not fully satisfied with it, even if he (perhaps) accepts that logicality and semantics are different items.

Laying foundations for logic remains Popper's goal

Lieber Herr Schroeder-Heister,
Eben habe ich Ihre Arbeit durchgelesen. Ich finde
sie ausgezeichnet. Sie haben ganz recht, mein erste
Ziel war die Charakterisierung der logischen Zeichen,
(formative signs). Aber dann schien es mir daß, falls
man damit Erfolg hat, man gleich die Aussagen
Logik begründen kann; und irgendwie geht das
auch, wenn es auch ein gesonderter Schritt ist.

Klagenfurt 5.4.2018 – p. 27

The structuralist interpretation

Second form of inferential definitions in Popper:
Instead of

$C//A{\wedge}B \;\leftrightarrow\; (C/A \;\&\; C/B \;\&\; A,B/C)$

Popper writes

C *is a conjunction of A and B* $\leftrightarrow\; (C/A \;\&\; C/B \;\&\; A,B/C)$

and instead of

$C//A{\vee}B \;\leftrightarrow\; \forall D(C/D \;\leftrightarrow\; (A/D \;\&\; B/D))$

he writes

C *is a disjunction of A and B* $\leftrightarrow\; \forall D(C/D \;\leftrightarrow\; (A/D \;\&\; B/D))$

We define inferentially (in terms of consequence "/"), what it means to be a conjunction, disjunction etc.

Again advantage: We do not presuppose that a corresponding operation is available in our language.

Klagenfurt 5.4.2018 – p. 28

The structuralist interpretation

More precisely:

Given a signature $\langle s_1, \ldots, s_m \rangle$, that is, an m-tuple of non-negative integers, then a logic structure is given as

$\langle \mathcal{D}, /, H_1, \ldots, H_m \rangle$

such that

- $\langle \mathcal{D}, / \rangle$ is a deducibility structure
- Each H_i is an s_i-place logical operation over $\langle \mathcal{D}, / \rangle$, that is, a function $\mathcal{D}^{s_i} \longrightarrow \mathcal{P}(\mathcal{D})$ such that $A//B$ for all $A, B \in H_i(A_1, \ldots, A_{s_i})$

An inferential definition of an n-place logical operation H is then a (metalinguistic) formula $\mathfrak{A}(A, A_1, \ldots, A_n)$ with at most $n + 1$ variables such that

$A \in H(A_1, \ldots, H_n) \;\leftrightarrow \mathfrak{A}(A, A_1, \ldots, A_n)$

Klagenfurt 5.4.2018 – p. 29

The fundamental objection in structuralist terms

Given a logic structure $\langle \mathcal{D}, /, H_1, \ldots, H_m \rangle$, where all H_i are inferentially characterized, then we can conservatively extend our language with operators $\circ_i(A_1, \ldots, A_{s_i})$ by just adding $\circ_i(A_1, \ldots, A_{s_i})$ to $H_i(A_1, \ldots, A_{s_i})$, provided the existence condition

$(\forall A_1, \ldots, A_n)(\exists A)\mathfrak{A}(A, A_1, \ldots, A_n)$

is satisfied for H.

However, this is not necessarily the case. We can construct deducibility structures, in which, for example disjunctions do not always exist (see Koslow).

Klagenfurt 5.4.2018 – p. 30

The weakness of the structuralist view

It is purely descriptive. But Popper wanted to be normative.

The structuralist view is inspired by Koslow. We describe in general terms a consequence structure with certain operators.

In Koslows framework, the right side of inferential definitions has a specific form that makes it suitable for semantic rules. We cannot just formulate arbitrary rules — there is no *tonk* in Koslow's framework (though formally allowed).

Popper has no explicit restrictions on the right hand side. Implicitly: The right hand side describes rules, which can be formulated in a sequent-style framework.

For example, there can occur no negation on the right hand side (against Lejewski).

Klagenfurt 5.4.2018 – p. 31

Partial meaning in proof-theoretic semantics

There is a branch of proof-theoretic semantics that deals with non-wellfounded definitions as occurring, for example, in paradoxes.

$$\frac{\Gamma/\neg\circ A}{\Gamma/\circ A}$$

It turns out that we can derive

$\Gamma/\circ A$ as well as $\circ A/C$

from which by cut (transitivity) any Γ/C follows

Argument: $\circ A$ is only partially defined in the sense that we do not have transitivity for $\circ$. (Or alternatively give up monotony.)

Klagenfurt 5.4.2018 – p. 32

The consequentialist reading of definitions

Not all definitions are fully well-behaved. Not all definitions are total.

However, whether they are well-behaved is a later discovery, not something to be read from the form of the definition itself.

Freedom for the definers!

We have this situation in recursive function theory.

This view is inspired by logic programming.

Usage: Definitional reflection and the evaluation of inductive definitions.

The existence of operations characterized comes in degrees (non-triviality, conservativeness, stability of structural rules, intensional aspects, ...)

The fundamental objection (‘*tonk*’) loses its power.

What speaks against ‘laying down’ rules rather than using symmetry or ‘harmony’ principles?

Laying down rules is often done in proof-theoretic semantics (for example in Martin-Löf).

Meaning explanations are often more complicated than the direct understanding of rules.

For example, Prawitz’s definition of validity, which is supposed to justify inference rules, is so difficult to explain that it is often easier to just write down the standard rules for the connectives and quantifiers.

‘Everybody will understand by themselves.’

Another example: Homotopy type theory and univalence.

(This holds for intuitionistic approaches in general.)

Acceptance does not come from the well-foundedness of the approach itself, but from its internal coherence in connection with its explanatory power.

The consequentialist reading of Popper

Every real Popperian will have sympathy for consequentialism.

We just accept tentatively any (finite) set of rules for connectives as a potential inferential definition and adjust consequences accordingly.

Even the uniqueness requirement, though perhaps important for logicality, is not necessarily needed.

Depending on the consequences, we can choose our logic. (Classical logic is preferable – no relativism w.r.t. logic, as demonstrated by the collapsing of negations)

This is in agreement with unpublished writings, and with Popper's general views.

This is not a global holism.

Popper's proof-theoretic semantics

Bernays-paper: On systems of rules of inference (early 1947)

Against dualist position. Consequence alone suffices. No truth-conditional semantics in addition.

Consequence relation replaces Tarski's consequence operator (Bernays).

Monistic rather than dualistic logic: Monism of rules — no axioms, no rules that can only be satisfied by logical propositions: Purified rules

This means essentially: Contexts in sequents crucial

Idea of "primitive rules" that generate classical logic

Uniqueness not mentioned.

Only separation of rules for logical constants explicitly required

Popper's system of classical propositional logic

$$\frac{\Gamma, \neg A / \neg B}{\Gamma, B / A}$$

$$\frac{\Gamma, A / B}{\Gamma / A \supset B}$$

$$\frac{\Gamma, B / C \quad \Gamma, C / B}{\Gamma / B \equiv C}$$

$$\frac{\Gamma, A, B / C}{\Gamma, A \wedge B / C}$$

$$\frac{\Gamma, A / C \quad \Gamma, B / C}{\Gamma, A \vee B / C}$$

Popper's system of propositional logic

This is a system based on double-line rules
(in the presence of conjunction, the contextual Γ need not always be available)

This is neither a reinvention of natural deduction nor a reinvention of the sequent calculus

It has a perfect symmetry.

Corresponding to Sambin's *Basic Logic*, a major approach in proof-theoretic semantics.

Popper and the two dogmas of standard semantics

The two dogmas:

- Priority of canonical proofs
- Priority of the categorical over the hypothetical

Popper instead:

- No proof steps distinguished
- The categorical is a limiting case of the hypothetical
  In the case of logical truth: *A* is logically true if it follows from any premiss.

This is not mainstream proof-theoretic semantics.

The priority of the hypothetical over the categorical

- Primacy of consequence over truth
  Truth-transmission is adequacy condition, but not defining condition of consequence
- Primacy of some form of sequent calculus (rather than natural deduction)
  Antecedent not just input, but can be addressed on its own
- Assumptions have a standing of their own, they are not just input-providing for forward-reasoning
  This fits very well with Popper's general view of hypotheses and his view of the orientation of reasoning
- Bidirectionality is the proper format of reasoning

Conclusion

- Popper provides an inferential framework for logic in a sequent calculus setting at a time when this was no commonplace
- Popper is very close to the idea of double line rules, which have a definitional character
- Popper has a clear view of the relationships between different negations and therefore different logical systems
- Popper strongly prefers the relational view, according to which consequence is basic, to the categorical view of logic
- Popper was much ahead of his time, antedating many developments from the 1970s on
- Overall, Popper was one of the first proof-theoretic semanticists, perhaps the first after Hertz and Gentzen (and definitely not a hardcore Tarskian, even though he might have understood himself as such).

Klagenfurt 5.4.2018 – p. 41

I hope I have understood Popper properly, and given as much credit to his logical work as possible

in $p(h, e \ldots e_n) - p(h) = st(h, e) = st(h \vee e, e) + st(h \leftarrow e, e)$

and so on:
~~All the~~ non-deductive support is counter-support.
This is just so: you do not seem to understand a very simple mathematical factum.

Klagenfurt 5.4.2018 – p. 42