The undergeneration of permutation invariance as a criterion for logicality

Catarina Dutilh Novaes (University of Amsterdam)

In his seminal paper 'On the concept of logical consequence' (1936/2002), Tarski presented his semantic notion of logical consequence, and recognized that "[a]t the foundation of our whole construction lies the division of all terms of a language into logical and extra-logical" (Tarski 1936/2002, 188). At that point, however, Tarski was skeptical concerning the possibility of establishing an absolute, principled division of the terms of a language into logical and extra-logical. Thirty years later, Tarski seemed to have overcome his original skepticism when he gave a lecture (1966/1986) on the matter of defining the scope of logic by means of the demarcation of a privileged class of entities, which he refers to as logical notions. Drawing on Klein's use of the technical concept of transformation to distinguish the notions used in different systems of geometry (Euclidean geometry, affine geometry and topology), he raises the following question: if we consider *all* possible one-one transformations of the universe onto itself, i.e. arbitrary permutations of objects, what are the notions that are still preserved? Tarski claims that once we consider *all* arbitrary permutations of objects, the notions that remain invariant are precisely the *logical* notions. Hence, permutation invariance could be used as a *criterion* for what is to count as logical. The criterion seemed to capture successfully the class of notions recognized as logical at the time: "Every notion defined in *Principia Mathematica*, and for that matter in any other familiar system of logic, is invariant under every one-one transformation of the 'world' or 'universe of discourse' onto itself.'" (p. 150)

Besides its apparent extensional success, the criterion also suggested something more controversial; that logic is essentially about counting, about numbers. In Tarski's words, "it turns out that our logic is even less than a logic of extension, it is a logic of number, of numerical relations" (p.151). The fact that the criterion 'overgenerates' in that it counts as logical some notions that are arguably not logical on account of being about a substantial characteristic of a given domain – its cardinality – has been extensively discussed by e.g. Etchemendy and Feferman. But what seems to be equally significant, and yet scarcely discussed, is the fact that the criterion also appears to undergenerate in light of the developments in logic of the last decades. At the time of Tarski's lecture, the logical systems being studied were still those which had been developed against the background of the logicist program. But as the purpose of logic according to the logicist program was to provide foundations to *arithmetic* (and then on to the whole of mathematics), it was to be expected that these logical systems would be particularly sensitive to matters concerning *cardinality, numbers and counting*. However, since then logic has developed in a variety of new directions, and its interface with computer science is particularly significant. Indeed, many of the notions and operators that are currently considered to be logical do not satisfy Tarski's permutation invariance criterion; this is the case for example of any modal operator interpreted on Kripke models which are not universal (or with an empty or an exclusively reflexive accessibility relation).

In my talk, I look into these cases of undergeneration of the permutation invariance criterion. I suggest that this is related to a shift of paradigm in logic from *mathematical* (and more specifically arithmetical) operations to *computational* operations. While in arithmetic one is indeed mostly concerned with numbers and quantities, in computer science one is essentially concerned with *transformations*, i.e. with passages from one informational state to another. Permutation invariance is neither sufficient nor necessary as a criterion for logicality. To reflect on why it fails with respect to many of the notions now recognized as logical sheds light on the matter of the true nature and scope of logic. It would seem that logic is not about numbers and counting after all, but rather that it is the systematic study of controlled, successive transformations of informational states.

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The Beginning of Model Theory in the Algebra of Logic

Volker Peckhaus (University of Paderborn)

The German mathematician Ernst Schröder (1841-1902) is one of the pioneers of the algebra of logic. His monumental "Vorlesungen über die Algebra der Logik" (1890-1905) seemed to provide some sort of sum of this field. Comming from Combinatorics and Combinatorial Analysis he developed his first ideas on logic completely independent from Boole and the British logicians. He was mainly influenced by Hermann Günther Grassmann's Theory of Forms opening Grassmann's "Ausdehnungslehre" (1844), and by the logic of Robert Grassmann. Schröder's conception of a formal, and in its last step of development absolute algebra, can be seen as an early precursor not only of Lattice Theory, but also of Universal Algebra and even Model Theory, in a broader sense understood as the study of the interpretation of any language, general or natural, by means of set-theoretic structures (Hodges).

Schröder first formulated his programme of Formal Algebra in his textbook for arithmetic and algebra (1873). There he defines mathematics as the "theory of numbers" (Schröder 1873, 2). What kind these number are is left open. Therefore the structure erected needs an interpretation. Later Absolute Algebra is founded on a generalization of the notion of number. Schröder proceeds from the existence of "unlimited manifolds [Mannigfaltigkeiten] of objects (of any kind)" (1874, 3) which he calls "domains of numbers" (Zahlengebiete). Examples for such "objects constituting a manifold" called "general numbers" are "proper names, concepts, judgments, algorithms, numbers [of pure mathematics], symbols for dimensions or operations, points, systems of points, or any geometrical object, quantities of substances, etc." (1874, 3). In his textbook he demanded that Formal Algebra should (1) compile all assumptions that can serve for defining connectives for numbers of a domain. (2) Formal Algebra should compile for every premise or combination of premises the complete set of inferences ("separation" of resulting formulas from the complete stock of combinatorically possible formulas). (3) Formal Algebra should investigate which particular domain of numbers can be constructed by the defined operations. (4) Formal Algebra has finally to decide "what geometrical, physical, or generally reasonable meaning these numbers and operations can have, which real substratum they can be given". After completion of the last step Absolute Algebra is reached. Schröder used, e.g., the language of his algebra and logic of relatives to transcribe and evaluate the definitions of finitness viz. infinity by Ch. S. Peirce and R. Dedekind.

Schröder discusses probabilities in the 2nd volume of his "Vorlesungen" in connection with modalities of traditional logic, in particular in connection with problematic judgments. For Schröder the traditional distinction between apodictic, assertoric and problematic judgments concerns epistemology, not formal logic. A problematic judgment can be understood as a judgment with a degree of credibility less than 1. It can be expressed by means of mathematical probabilities. Schröder claims that the tools of exact logic can be extended for dealing with probable inferences determining the probability of a proposition. However, he never elaborated such theory.

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Philosophical Foundations for Type-Theory
Graham Stevens, University of Manchester

In 1910's *Principia Mathematica*, Whitehead and Russell presented their demonstration of the thesis that mathematics is a branch of logic. In response to the paradoxes plaguing the foundations of their logic, the most famous of which was Russell's paradox concerning the class of all classes which are not members of themselves, they stratified the formal language (PM) employed in *Principia* in accordance with a theory of logical types.

The adoption of a theory of types appears to mark a radical departure from the philosophical position underpinning Russell's original statements of logicism in the 1903 *Principles of Mathematics*, which takes logic to be essentially type-free. Recent studies of Principia have attempted to interpret the work in a way which avoids the conflict with the earlier view by suggesting that type-theory in *Principia* does not apply to the non-linguistic realm, but only applies to a linguistic domain. Therefore it does not contradict Russell's earlier view that the *objects* of logic and mathematics are not typed. In this paper I offer a defence of this 'linguistic' interpretation of *Principia's* type-theory.

Russell took the multiple-relation theory of judgement to be a key component in the philosophical foundations of type-theory in *Principia*. This theory of judgement was abandoned in 1913 in response to objections from Wittgenstein. I argue that those objections raised a conflict for Russell between the theory of judgement and the theory of types it was designed to lend support and justification to. This claim is not new. However, unlike previous defences of this claim, I appeal to this interpretation in defence of the linguistic interpretation of types in *Principia*, arguing that Russell's dramatic response to Wittgenstein's criticisms is inexplicable unless the theory of types in *Principia* is interpreted linguistically. A non-linguistic theory of types would have provided Russell with a very simple and obvious reply to Wittgenstein's objection. But if the theory of types is a linguistic theory, Wittgenstein's criticisms are indeed devastating and show that type-theory and the multiple-relation theory of judgement are incompatible.

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Historical Remarks on Hans Reichenbach's Conception of Probability
Nikolay Milkov (University of Paderborn)

Our objective is to give historical orientation of Reichenbach's concept of probability. We claim that in the 1910s it was greatly influenced by the Göttingen neo-Friesians. The first influence on Reichenbach in this direction was exercised Kurt Grelling's 1910 paper "Philosophical Foundations of the Calculus of Probability" to which Reichenbach's teacher in Munich, Ernst von Aster, draw his attention in 1913. Grelling's paper defended three ideas that Reichenbach followed till the end of his days: (i) the objective ("ontological") interpretation of probability against Carl Stumpf and Johannes von Kries. (ii) The discrimination between mathematical and philosophical probability introduced by Jacob Friedrich Fries. (iii) The marriage of probability to induction. While in Göttingen between April and September 1914, Reichenbach had intensive discussions with two members of Leonard Nelson's group: Grelling and Walter Dubislav. However, he was not a pupil of Nelson himself (the latter in turn was a friend and protégé of the mathematician David Hilbert). In these months Reichenbach also wrote his Dissertation in probability. Its objective was to supplement Kant's epistemology: the laws of physics cannot be justified by the principle of causality alone; they are to be extended with the principle of probability.

After Reichenbach attended Einstein's lectures on theory of relativity in Berlin (1918/1919), he concentrated his efforts to exploring the concepts of space and time. Not for long, however. In 1924 he turned back to his old theme: probability, introducing at that another neo-Friesian idea—he employed a theory of action in epistemology: human actions are only possible because future events are indeterminate. Indeed, if the future would be exactly predictable, we could not act. Between 1926 and 1933, Reichenbach, Grelling and Dubislav gathered together again in what came to be known as "The Berlin Group". In his paper "Logistic Empiricism in Germany and the Present State of its Problems" (1936) Reichenbach remembered: instead of investigating the principle of verification, or the protocol-sentence debate, the members of the Berlin Group put stress on probability: "There were years of work in Berlin [on this subject], filled with fresh starts and tentative solutions, proposed in ardent discussions, before a definitive theory was reached." In Reichenbach's epistemology after 1932, "a statement about *a single event* concerning the future is not maintained as true or false; it is maintained as a wager", or posit. Posits are widely employed in science. In fact, the use of the notion of "posit" was the next point on which Reichenbach followed Fries. The latter introduced the concept of "hunch" (*Ahnung*) which denotes direct comprehension of the situation under review that prepares the propositional conceiving of reality.

Reichenbach's scientific method is a self-correcting procedure, which starts with blind posits and goes on to formulate appraised posits in a continuous interplay between *experience* and *prediction*. Reichenbach has called this method "the method of trial and error": it is the only existing instrument for safe predictions in science. Some interpreters (e.g. Alberto Coffa) have already noted that this method is related to Karl Popper's method of conjectures and refutations. This wouldn't be a surprise if we follow Malachi Hacohen's clue that Popper's philosophy of science too was profoundly influenced by Leonard Nelson (through Nelson's pupil Julius Kraft who was 1925–1927 in Vienna). Also the orientation of both Reichenbach and Popper to analyzing scientific practice, which contrasted the more logical examination of science in the Vienna Circle, has its roots on Nelson and his pupils.

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Johannes von Kries and the Philosophy of Probability Theory

Michael Heidelberger (University of Tubingen)

One of the first works on the philosophy of probability was written by the German physiologist and (neo-Kantian) philosopher Johannes von Kries (1853-1928): *Die Principien der Wahrscheinlichkeits-Rechnung: Eine logische Untersuchung*. Freiburg 1886 (repr. 1927). In this book, von Kries put forth a new interpretation of probability theory, the so-called *Spielraum*- or range theory. His aim is to clarify the sense of numerical probability statements. My talk is divided into five sections:

In the first part, a short account of von Kries's life and work is given. In the second part, I deal with von Kries's sharp critique of several philosophical interpretations of probability: the "psychological" (or subjective), the personalist, the classical interpretation of Laplace and the relative frequency interpretation.

In the third part, I explain von Kries's range-theory of probability – the only alternative interpretation that really makes sense for him. The probability of an event is the proportion of the range of the event to the whole range of outcomes. In order to be measurable, the ranges have to fulfill three criteria: They must be comparable, i.e. there must be a compelling, objective and non-arbitrary way to subdivide the *Spielraum* into alternatives. They must be indifferent, i.e. the different alternatives of a range must be equally possible and disjunctive. And they must be original, i.e. they are irreducible to more fundamental ranges so that the pre-history of the alternatives is irrelevant. The only cases that fulfill these criteria are "ideal games of chance", such as the roulette, which is conceived as consisting of infinitesimally small red and black stripes. The probability of an event is thus more precisely the proportion of those original states within the state space of a random experiment that leads to the event in question. Since the range-principle, like the causal law, can neither be proven nor refuted, it must be, according to von Kries, of an "unempirical" nature.

The fourth part deals with von Kries's distinction between "nomological" and "ontological" features of reality. A nomological feature is the result of a necessary, objective law of nature, whereas an ontological feature is the result of the contingent distribution of the original initial conditions. As a result we can explain the world nomologically by subsuming its changes under (mechanical) laws, but we can also explain certain structures of the outcome in an ontological way, e.g. the approximate balance of black and red results in the long run. In this way, von Kries is able to deal successfully with many controversial cases where the law of large numbers is involved, like thermodynamics, social mass phenomena or even legal responsibility.

In the fifth part, some of the historical contexts are given, in which von Kries's discussion is placed. There is first the controversy over the measurement of subjective states of mind in psychophysics. A second context is given with Adolphe Quetelet's "Social Physics" where statistical regularities are taken as determinist nomological features of reality. A third context derives from the neo-Kantian logic of Friedrich Albert Lange where disjunctive judgments are to be conceived as having a very close relation to probabilities and an *a priori* basis in spatial intuition (in the Kantian sense!). Still a forth context is given by the doctrine of Southwest German neo-Kantianism that there is a basic distinction between the "nomothetical" natural sciences and the "ideographical" historical ones that deal exclusively with individual features of reality.

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The Vienna Circle on Induction

Artur Koterski (University of Lublin)

In his monograph on the Vienna Circle (1950) Victor Kraft wrote that 'one of the earliest and most fundamental insights of the Vienna Circle' was 'that no deductive or logical justification of induction is at all possible'. In Logik der Forschung (1934), published at the peek of the Vienna Circle activities, Karl R. Popper developed his conception on the very engaged cri-tique of logical positivism and its alleged essential feature—inductivism. Although Kraft was right, Popper's opinion prevailed and finally dominated philosophical handbooks for decades. Nowadays, with many major misunderstandings cleared, Popper's carping about the Vienna Circle inductivism is slowly losing its impact. However, it must be admitted that the actual Vienna Circle attitude towards induction might have been misleading. Whilst the Schlick-Kreis members clearly recognized the impossibility of logical justification of induction, they also felt it was a part of scientific conduct and instead of denying its existence they tried to change its status.

The aim of this paper is, firstly, to demonstrate this evasive policy about induction—i.e. how to keep it rationally, nevertheless without justification; secondly, to illustrate that such 'ambiguousness' about inductive reasoning in science was inherited from the works of the most influential authors for the development of the Vienna Circle: French conventionalist (Poincaré, Duhem, Rey), Russell, Wittgenstein, as well as physicists who influenced neopositivism with regard to foundations, aims and methods of empirical science—Helmholtz, Boltzmann, Mach and Einstein. Thus, it is finally possible to argument that Popper's criticism of 30's, repeated for years by himself and his students, was already then an anachronism.

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Unifying Carnap's Three Concepts of Probability

Eckehart Köhler (Lauder Business School, Vienna)

Carnap (1945) originally distinguished two kinds of probability: *epistemic* (*logical*) and *frequentist*, claiming they were *both* valid and could be used within *a single* system. Later, Carnap (1962) brought the newly popular *subjective* concept within his purview, relating the subjective with the logical notion that he had meanwhile developed. Thus it seems that Carnap recognized three separate concepts of probability by the end of his career, which have in common merely the Kolmogorov axioms. I claim he had, not three concepts, but rather three theories, distinguished from each other on two different dimensions. Dimension 1 is the semantic (or ontological, if preferred) level of the theory involved: "object" or "meta-level"; dimension 2 is the degree of "normative standardization". This I explain as follows.

In Köhler (2001), I had argued that the *frequentist* and the *logical* concepts can be "unified" in the sense that they codify *equivalent information*, one on the object level, the other on the meta-level of theories. My prime examples today are two well-known versions of Quantum Mechanics (QM), the original one using frequentist probabilities, the later one using the meanwhile fashionable (epistemic) subjective probabilities. [In Köhler (2001), my examples were two concepts of information (negentropy) developed by Carnap (1977): thermodynamic and logical.] The two formulations of QM are empirically equivalent, whereby the frequentist probabilities are parameters of empirical ensembles *in* nature, whereas the subjective probabilities are measures of a (normatively standardized) subject *reflecting on* nature. (We know from Tarski's original idea of semantics that the sentences on the meta-level are *equivalent to* those on the object level: on the object level, states of nature are described, on the meta-level epistemic reflections of nature are described.)

In the subjective example, if we now were to substitute Carnap's (or *e.g.* Jeffreys's) *logical* probabilities for the *subjective* ones, what would that change? Not much, because it turns out that *logical* probabilities are simply special versions of *subjective* ones: they are merely more strictly normatively standardized. This can be seen from Carnap's (1962) procedure, where he has *subjective* probabilities become *equal to logical* ones, if they satisfy sufficiently rigorous rationality criteria — in particular symmetry conditions allowing fixing of prior probability distributions. What kept Carnap from assimilating the two notions was his preconception (certainly derived from Frege's Antipsychologism) that the logical concept is never subjective. This is easily refuted by pointing up the inherently mental character of logic. Logic's only distinctions from the usual (empirical!) psychology are a) that it is normative and b) it is “unboundedly rational” (H.A. Simon). But rendering psychology normative and unboundedly rational does not render it into anti-psychology.

The main barrier to assimilating concepts comes from a wide-spread misunderstanding of the objective / subjective divide. This is not a true dichotomy, because the divide is actually used in two different senses: a) to characterize the *material / mental* and b) the *reliable / unreliable* (or valid / invalid) distinctions. Some *mental* states can be *completely reliable* in one sense (hence valid or *objective* in the other sense), if we are sure that the mind’s processes (observations, inferences) are *correctly executed* — which the following of norms ensures. (Whether norms are really *correct* is in turn a meta-level problem concerning consistency and adequacy — epistemic auditing.)

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Some historical and philosophical aspects of non-classical (quantum) probability theory and its interpretation with special regard to von Neumann's views.

Miklos Redei (London School of Economics)

In non-classical (quantum) probability theory the Boolean algebra S in a Kolmogorovian probablity measure space (X,S,p) is replaced by the non-distributive, orthocomplemented lattice P(N) of projections of a general von Neumann algebra N and the classical probability measure p by a countably additive map s from P(N) into the unit interval [0,1]. The different types of von Neumann algebras in the Murray-von Neumann classification that was carried out in 1935 by von Neumann and Murray yield all the typical types of quantum probability spaces (N,p(N),s) that occurr in classical probability theory -- von Neumann and his work is what Kolmogorov and his work was for classical probability theory.

While there is thus a very tight conceptual-structural analogy between classical and quantum probability theories, the interpretation of quantum probability has remained very controversial. In particular, there is no straightforward frequency interpretation of quantum probability spaces in the sense of von Mises, for two reasons: (1) the projection lattice P(N) cannot be interpreted as the structure that represents random events because, as a consequence of Kochen-Specker Theorem for general von Neumann algebras, there exist no Boolean algebra homomorphism from P(N) into any Boolean algebra; in particular there exists no evaluation on P(N) that would specify for all elements in P(N) whether they occur or not. (2) General additivity of s on P(N) is a necessary condition for s(A) (A in P(N)$ to be interpretable as relative frequency in a fixed ensemble; however, an s on P(N) has the general additivity if and only if it is a trace, i.e. if it is insensitive for the non-commutativity (non-distributivity) of P(N) and in general there exists no tracial state on a von Neumann algebra. In view of the difficulties of the frequency interpretation of quantum probabilities, around 1936 von Neumann had given up the frequency interpretation of quantum probabilities in favor of a "logical" interpretation, which he did not regard as properly worked out and understood however. The talk reviews quantum probability theory and the interpretational difficulties along the lines mentioned above and finishes with a discussion of a more recent proposal about interpretation of quantum probability, the so called "Kolmogorovian Censorship Hypothesis", according to this hypothesis there are no genuine quantum probabilities: quantum probabilities can always be interpreted as classical conditional probabilities. The Kolmogorovian Censorship Hypothesis is instrumental in character and the talk also points out some technical difficulties that prohibit the Hypothesis to be as general as would be required.

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Vienna Indetermism. Some remarks on the emergence of a philosophical stance in the context of explorative experimentation

Michael Stöltzner (Columbia, SC and Bielefeld)

Almost two decades before the advent of quantum mechanics, the idea that the basic laws of nature could be indeterministic represented the prevailing view among Viennese physicists. Vienna Indeterminism – as I call this tradition that was first touted in Franz Serafin Exner's 1908 inaugural address as rector of the University of Vienna – can be summarized by three tenets. i) The highly improbable events admitted by Boltzmann's statistical derivation of the second law of thermodynamics exist. (ii) In an empiricist perspective, the burden of proof rests with the determinist who must provide a sufficiently specific theory of microphenomena before claiming victory over a merely statistical theory. (iii) The only way to arrive at an empirical notion of objective probability is by way of the limit of relative frequencies.

In this paper I want to emphasize the empiricist element in (ii) by focusing at the years before Exner's speech. This involves two of the three research fields then characteristic of the Exner group, to wit, atmospheric electricity and radioactivity (radium research). In contrast to Boltzmann's statistical derivation of the second law of thermodynamics and the debates about atomism, research in these two fields was dominated by a description of the phenomena and explorative experimentation with a shared instrument, the electroscope. It is important to note, however, that the Viennese combined this descriptive approach with a solid understanding of Boltzmann's statistical mechanics. This quite specific Viennese mélange of methodologies, so I argue, permitted them to consider fluctuations ("Schwankungen") as viable candidates for physical laws – and not as disturbations of hitherto unknown underlying deterministic laws, errors to be explained or explained away.

This mélange was instrumental for two breakthroughs that occurred around 1905, Egon von Schweidler's theory of radioactive fluctuations and Marian von Smoluchowski's theory of Brownian motion. Both were propounded as descriptions of genuinely random phenomena, but their reception would become markedly different. Brownian motion was quickly viewed as decisive evidence for atomism, not least because the continuing validity of classical mechanics on the atomic level –at the price of the ergodic hypothesis – made it palatable to those insisting on a deterministic foundation of natural law.

Schweidler's fluctuations, on the other hand, were commonly conceived as a vehicle to better describe a given radioactive substance than as a phenomenon in its own right, let alone as a proof of indeterminism. The Viennese, contrast, emphasized their genuine nature. After Exner's rectorial address provided them with a general indeterminist outlook on the laws of nature, they even argued that Schweidler had definitely proven the indeterministic nature of radioactive decay. Schrödinger's 1919 paper on "Probability theoretic investigations concerning Schweidler's fluctuations" represented definite and theoretically well-elaborated word from the Viennese on the matter. It played an important role in motivating the stance he took in his famous 1922 inaugural address "What is a law of nature?". Schrödinger there considered fluctuation phenomena an interdisciplinary phenomenon ranging from Brownian motion to atmospheric fluctuations, fluctuations of radioactivity, and the second law of thermodynamics. This shows that the tradition of Vienna Indeterminism was not merely a philosophical movement, but counted on a complex research tradition that was driven by an empiricism prevailing even before Exner's adoption of the relative frequency interpretation would ease the conceptual understanding of fluctuations.

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Statistics between Natural and Social Science. Some Historical Facts and Interpretations

Donata Romizi (University of Vienna)

My paper reconsiders some stages in the history of statistics in the 19th and early 20th centuries relating them to the debates on the issue of the unity or disunity of science.
In particular, the paper is an historical inquiry into two subjects:

(1) the relationship between social statistics and the origins of probabilism in physics;
(2) the presence of statistics *both* in social and natural sciences as an argument for the unity of science.

Being a (philosophically interested) historical reconstruction, the paper is based mainly on primary sources and proceeds in chronological order.

I start by analyzing in some details Quetelet (1796-1874)'s *Physique Sociale* and highlighting how Quetelet's work built up a 'crossroad' through which statistical thinking could easily 'commute' between natural and social sciences. It was mainly through the reference to probabilistic and statistical models that Quetelet could argue in favour of the scientific character of his "Social Physics" and thus in favour of a unitary conception of social and natural science. Taking into consideration some reactions to Quetelet's work (e.g. by Thomas Buckle and by some German-speaking statisticians) I show how different conceptions of statistics in the 19th century went hand in hand with different standpoints on the question of the unity of science. Furthermore, I reconsider T. Porter's thesis according to which "a close and significant relationship between social statistics and the origins of probabilism in physics is apparent" (Porter 1986). This thesis has been criticized by Shafer (1990) by arguing that Maxwell's and Boltzmann's references to social statistics in connection with statistical physics were only analogies or didactic devices. Still, I show how the analogy between social statistics and statistical mechanics had a significant resonance and was further developed within the Vienna Circle, in particular by Ph. Frank, E. Zilsel and O. Neurath. These Vienna Circle's members appealed, exactly like Quetelet, to statistics in order to argue for the unity of science. Nevertheless, after the development of statistical mechanics and quantum mechanics, this kind of argument has been interestingly turned upside-down: while Quetelet pointed to statistics to argue that social sciences do resemble natural sciences with respect to causality, lawfulness, prediction and – in sum – determinacy, the Vienna Circle's members pointed to statistics to show that natural sciences share with social sciences a certain degree of *in*determinacy (which however does not prevent the formulation of laws and predictions).

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Will someone say exactly what the H-theorem proves?

Jos Uffink (University of Utrecht)

The above question was famously raised in a letter to Nature by E. Culverwell (1894) after Boltzmann's visit to a meeting in Oxford where his H-theorem was much discussed. Culverwell's letter triggered a number of responses trying to pinpoint the exact ingredient in the derivation that is responsible for the time-asymmetry of the theorem, culminating in Boltzmann's own formulation of the Hypotheses of Molecular Disorder. We will look at the contributions of the key figures in this debate (Burbury, Boltzmann Bryan and Jeans) and argue that some of the confusion in their views is to some extent still present in modern presentations of the H-theorem.