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Introduction

Robert Rubens
Ghent University

The volume 31 of Sartoniana contains the laudatios and lectures of the Sarton Chair and medallists of the academic year 2017-2018. In the year marking the beginning of the third centenary of our university we still want to remember George Sarton, who devoted his life and scientific endeavour to the study of the history and philosophy of science. Following his main idea the volume 31 again contains contributions from every field of scientific research.

The lectureholder Adrian Forty is a historian of architecture. He very nicely develops the influence of metaphorical thinking in the buiding industry. Even the idea of circulation described by Harvey in the medical field had a profound influence upon architecture. In recent times functional architecture much more than "metaphoric" architecture became fashionable. However real "architectural" artists like Le Corbusier succeeded in reconciling both aspects.

The study of Ignazio Czegun about the colonial period of Germany is devoted to a mainly forgotten part of colonial history. Alongside England (later the United Kingdom), France, Holland, Spain, Portugal and Belgium also Germany had a colonial empire at the end of the 19th and beginning 20th century. Due to the fact that Germany has been stripped of this empire after world war I, it is mainly forgotten. The building of the administrative and judicial structure in those possessions is a frequently untold part of European colonial history. The structure and influences in those colonies of the German Reich form the content of this beautiful lecture.

The paper by F. Stadler enlightens the relation between the ideas of Mach and Sarton. The very detailed study explains on the basis of original research the connection between Mach and Sarton, and the Vienna Circle in exile. It should become obligatory reading for everybody interested in the birth of the history of sciences project in the beginning of the 20th century.

J. Leroy gives an outline of the original papers of Mendel. Furthermore the contribution of Tschermak von Eisenegg, a doctor honoris causae from our university, replicating and confirming the basic principles of genetics during his stay in Gent are nicely described.

The lecture by P. Allegaert about the main ideas in museology is a philosophical essay pinpointing towards the usefulness of this endeavour. He again highlights the need for a pluridisciplinary approach congruent to the basic mantra of Sarton.

The discussion in the nineteenth century about the age of the earth was a subject for a thorough discussion between physicists and geologists, even involving Lord Kelvin in Glasgow. The paper by Van den Haute provides an overview of the rational and scientific arguments which were used in that period.

The discussion about the origin of the clinical aspects of bacteriology or infectiology in human medicine in the nineteenth century is developed in the paper by Godeeris. Both protagonists, as well Pasteur as Koch, have been very important in this field. However they both would not have such an enormous prestige if they had not had very apt followers and collaborators. The latter ones have been sometimes nearly forgotten but have contributed enormously to the benefit of mankind

The synthesis of Marc Brysbaert about the history of psychology explains again the intrinsic difficulty of psychology as a scientific field bridging between humanistic and exact science. In a world where even language science becomes involved with classic experimental models the mind-body dilemma of psychology is paramount.

The contribution of Pedro Laims correctly details the development in the central and peripheral states of Europe. He tries to explain the differences in industrial evolution between the countries of the now European Union during the nineteenth and twentieth century. The start from a mainly agri-

cultural environment is certainly not the only reason for a very different evolution.

Kaat Wils, historian, gives an overview of the technique of hypnotism, as practised in the medical field during the nineteenth century. In that period hypnotism was considered a standard medical technique and performed mainly by neurologists. Hypnotism was not only a nice gimmick but was then also used for therapeutic purposes.

The essay on the power of names by John Peters is a sparkling ballet of the semantics of names going from the media, via linguistics towards a grand finale describing the euonyms.

We hope that again the panacea of philosophical and scientific essays in the history of science may interest readers from all scientific disciplines.

SARTON CHAIR LECTURE

Laudatio Adrian Forty

Maarten Delbeke

Adrian Forty is Professor Emeritus of the History of Architecture, University College London, and Principal Research Associate, at the Bartlett School of Architecture, UCL. He read history at Brasenose College, Oxford and obtained a Master's degree in the History of European Art at the Courtauld Institute in London in 1971. In the same year he became a Lecturer in the History of Art and Design at Bristol Polytechnic, before moving to the Bartlett in 1973, where he taught for nearly 40 years as a lecturer, senior lecturer and a professor until 2014. He is the recipient of multiple grants and honors: he was awarded the Misha Black award for innovation in Design Education in 2003. In 2011 he was elected Honorary Fellow of the Royal Institute of British Architects, and in 2015 he became Senior Fellow of the Royal College of Arts.

A central characteristic of Adrian Forty's work and activities as an architectural historian is a persistent concern with issues of media and mediation. His first book, *Objects of Desire: Design and Society 1750-1980*, first published in 1986, still in print and translated into several languages, set out to explore how "the design of manufactured goods is determined not by some integral genetic structure but by the people and the industries that make them and the relationship of these industries to the society in which the products are to be sold."[1] Design is not considered as the optimization of forms in response to technological requirements and popular taste, but as a process of negotiation between the different agents that generate, manufacture, distribute and eventually buy and use objects; a practice

[1] Adrian Forty, *Objects of Desire: Design and Society 1750*-1980, London: Thames and Hudson, 8.

imbued with, and expressive of, social values; and a strategy to lend technological and industrial developments acceptance in society.

The art of forgetting, edited together with Suzanne Kuechler in 1999, develops an integrated perspective on collective mechanisms of remembering and forgetting by confronting the materiality of monuments with the social practices and rituals that surround them. In so doing, it joins the attention of anthropologists and sociologists for the performative aspect of memory with the interest in material objects and their forms on the part of architects and architectural historians.

Adrian's second book, *Words and Buildings*, first published in 2000, examines how since the 18th century specific words, mostly borrowed from different domains and disciplines, have been used to explore, define, or advocate the meanings of visual forms and invisible qualities of architecture. The book shows how the language of architecture is a site of intense mediation, not just between architects and their multiple audiences, but also between architecture as a particular discipline and a changing constellation of discourses and practices that reflect the concerns of society.

Adrian Forty's most recent book, *Concrete and culture. A material history* (2012), intends to offer "not a history of a material in a way that a 'history' is normally understood – … but to think of concrete as a medium rather than as a material … to make sense of a medium that has a history, without being itself a history of the medium."[2] This approach implies an attention not just to architects, engineers and their realizations in concrete, but also to the ways in which concrete is used, understood and represented by "self-builders, sculptors, writers, politicians, entrepreneurs, photographers, or film-makers."[3] In order to do so, the book develops a truly global perspective on its subject.

It was Forty's tutor at Oxford, Theodore Zeldin, who encouraged his interest in what the ordinary person takes for granted – such as the seemingly banal material of concrete. This was not a self-evident path to forge in the emerging field of architectural history. Forty titled a keynote lecture given in 2015 about how it was to undertake architectural historical

2 Adrian Forty, *Concrete and Culture. A Material History*, London: Reaktion Books, 2012, 10.
3 Ibid., 9.

research in 1972 as: "adventures in an unformed discipline."[4] In the lecture, he acknowledged the historian and critic Reyner Banham as an early inspiration, remembering an article from 1970, "The Crisp at the Crossroads", a design critique of the potato chip. Banham's ability to examine even the most humble of things inspired an approach to architectural history divested from any exclusive attention to the architectural object and its formal qualities, but directed by an at once more inclusive and critical notion of design and building as activities deeply embedded in culture and society.

Banham was at the Bartlett between 1964 and 1976, and it is to some extent as his successor that Adrian Forty has become the preeminent architectural historian in the UK of the last forty years. It is telling that a volume compiled in his honour by colleagues at the Bartlett to celebrate Forty's career, presents, as the title indicates, *Forty Ways To Think About Architecture: Architectural History and Theory Today*, a clear acknowledgment of Adrian Forty's formative role in shaping the discipline of architectural history as it now exists.[5]

Adrian Forty's writing is also not confined to the format of academic publishing, but has critically engaged with contemporary debates, especially concerning London. London also loomed large in his legendary introductory course in architectural history at the Bartlett, as it incorporated numerous on site visits, a formative experience for many students. At the Bartlett, Adrian also directed the Master's program in Architectural History; established in 1981, it is the UK's longest established Masters course devoted to architectural history, theory and criticism.

His commitment to fostering the discipline of architectural history informed his work as the President of the European Architectural History Network (EAHN), a role he fulfilled from 2010 to 2014. Under Adrian's leadership the network transformed from an enthusiastic but fragile under-

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4 Adrian Forty, "Starting Research in 1972: Adventures in an Unformed Discipline," Keynote lecture at ReSkIN Spring 2015 Conference (London, 30 January 2015), as referenced in: Introduction by Barbara Penner; archival text by Reyner Banham, 'The Man Who Wrote Too Well', *Places Journal*, September 2015. Accessed 11 June 2018. https://doi.org/10.22269/150908.

5 Iain Borden, Murray Fraser and Barbara Penner (eds.), *Forty Ways To Think About Architecture: Architectural History and Theory Today*, London: Wiley, 2014.

taking into a still informal but solid structure that connects and supports the community of architectural historians in Europe and beyond.

As these activities attest, Adrian's work should be understood as a persistent engagement with the practice and writing of architectural history, so that it becomes available for a better understanding of the role and meaning of architecture in society. It explores how architects have struggled to become modern in a changing world, and how they enlisted ever new words, new materials and new justifications for their activities in order to do so. His writings provide incisions into the problem at hand, delivered with an exceptional precision and clarity, and speaking not just to the historian but to all who may have an interest in the built environment and the artifacts that shape it. Through these incisions, the dense matter of concrete, the ephemeral creations of design, and immaterial concepts and words become surprisingly comparable. They indicate the artifice involved in architecture and architectural history; not the artifice proper to creativity and design, but the one required to find a place for architects and architecture in a complex material, cultural, social and economic environment.

By laying bare this artifice and the efforts it requires, Adrian Forty challenges the determinism that used to characterize much 20th-century architectural discourse and historiography, prone to writing the history of architecture of the last 300 years as a linear progression towards modernity. His work also emphasizes that architecture never stands on its own, and often occupies a fragile position in wider network of forces and concerns. Finally, it takes language and other media seriously as the elements that negotiate and shape the relationship between a culture and its built environment. These concerns and values align closely with the approach to architecture, architectural design and architectural history of our Department, and we are therefore delighted that the Sarton Medal has been bestowed on Adrian Forty.

What Makes Architecture Tick?

Adrian Forty

Where do architectural ideas come from? I am not going to pretend that I can answer this question. As with any kind of artistic creation, if one could comprehensively explain how the ideas that went into the work came about, we would no longer have an 'art'. It is in the nature of art to evade explanation, and attempts to explain how creation comes about usually fall flat, or are so inadequate as to be absurd.

Nonetheless, looking at the history of architecture, we can see that there are certain routes by which ideas enter architecture, and that these routes have a certain recurrent pattern to them. The particular pattern that I am going to talk about this evening is via metaphors. Metaphors of all kinds have a long history in architecture – a great many architectural ideas have come about through people borrowing from other fields and discourses outside architecture, and then using these to sustain some particular theme within architecture. Metaphors may be visual. Frank Lloyd Wright's Guggenheim Museum in New York is a metaphor of a snail's shell. The spiral form is derived from Wright's fascination with shells, which he translated into an architectural form. More literal are the designs by the Flemish architect Renaat Braem for beach houses: the shell house, the seagull house, the seaweed house, the wave house. Or the metaphors may be verbal. According to Alberti (1), the fifteenth century scholar who wrote the first book since antiquity about architecture, 'a building is very like an animal'(p.301), and this simile was, as we shall see, fundamental to his whole theory of architecture. His book is full of metaphors, and ever since Alberti's time, architects have made heavy use of verbal metaphors. Or the metaphors can be both verbal and visual at the same time. Le Corbusier's sketch (2, p.125) of the human viscera, with the comment 'faire fonc-

tionner', doubles up a visual metaphor with a verbal metaphor, and in doing so lays out a whole way of thinking about architecture that was to be tremendously important in the twentieth century. We'll come back to Le Corbusier's sketch later, and look at it in more detail.

The metaphors that architects have been in the habit of employing come from a variety of fields. Among the most frequent are those from nature. We have already seen one of those in Alberti's 'a building is very like an animal'. Alberti used this theory to explain his theory of beauty, which we can summarise by saying that perfection in architecture occurs when nothing can be added, not taken away, without damaging the integrity of the whole. Every part must have a correspondence to every other part. From his animal metaphor, Alberti derives the rule that 'Beauty is a form of sympathy and consonance of the parts within a body' (1, p.303). Or let us take another celebrated 'nature' metaphor, that of the eighteenth century architectural critic and theorist, Marc-Antoine Laugier (3). Laugier proposed that the origins of architecture lay in 'nature' – in the way primitive man discovered that four uprights arranged in a square, with four more branches laid between them, and then two more at each end raised up to form a roof, constituted the basis for a satisfactory dwelling.

> 'Such is the course of simple nature; by imitating the natural process, art was born. All the splendours of architecture ever conceived have been modelled on the little rustic hut I have just described. It is by approaching the simplicity of this first model that fundamental mistakes are avoided and true perfection is achieved'. (p.12)

And obviously, over the course of time, as our ideas of what 'nature' is change, so 'nature' offes up wholly different metaphorical possibilities to architecture. We will come back to look at some of these later.

Nature is one source of metaphors, another is language. 'Vernacular architecture', to us the most familiar way of describing the ordinary building of a country or region, is a metaphor borrowed from language, but it was only available from the early nineteenth century once poets and linguists had developed this way of distinguishing between scholarly or court language and everyday spoken language. And to talk of the 'grammar' or 'syntax' of an architectural design, as many architects have been in the habit of doing, is similarly a borrowing from language.

Another big class of metaphors are those derived from other kinds of human artefacts apart from buildings. The most common of these borrowings are either from dress, or from machines, like cars or aeroplanes.

And then we come to what has without doubt been the single biggest metaphorical source of architectural ideas in the last century and a half – metaphors drawn from science. It is these that I want to concentrate on for the remainder of the lecture. The use of science metaphors may at first sight seem easy to explain, in that science has been the dominant discourse of our times, and in every aspect of life the language of science has taken over. Road transport has become 'logistics'. The Paris street cleaner is now a 'techniçien des surfaces'. A bar tender is a 'mixologist'. And so on. It is hardly surprising that architecture too has fallen for the vocabulary of science. But there is more to it than this.

The fashion for scientific metaphors in architecture could, you might suppose, be a way of saying that architecture *is* a science, in the way that the neologisms for street sweepers and bar tenders are meant to imply that there is something scientific about what they do. But if we examine what we know about the mechanisims of metaphors, this is not necessarily the case. Successful metaphors rely on the *unlikeness* of the things linked together, not upon their likeness. An effective metaphor borrows an image from one schema of ideas, and applies it to another, unrelated, schema of ideas. The less connected the two fields are, the more likely the metaphor is to stick. As the philosopher Nelson Goodman (4) puts it, in what is itself a wonderful metaphor, a metaphor is 'an expedition abroad' (p.73), from one realm of thought to another. Or, as he writes, a metaphor is a 'calculated category mistake' (p.73). Now if we follow this way of thinking, science metaphors only work in architecture because architecture is *not* a science. Were it to be a science, they would fail. So the lesson of the great flourishing of science metaphors in twentieth century architecture is to confirm that we are dealing with a practice that while it may approximate to a science in certain respects, such as when it deals with energy use or acoustics, is ultimately *not* a science.

With that *caveat* in mind, let us look at some of the scientific metaphors that have pervaded modern architecture. Let's start with 'circulation', the word commonly used to describe the movement of people within a building. The word, and the particular idea that it signifies, only entered the

architectural vocabulary in the mid-nineteenth century. And, of course, it is a metaphor – borrowed from physiology, from William Harvey's discovery in 1628 that only one type of blood flowed through the human body and not two, as had previously been thought. Harvey's term was quickly taken up by other fields, like economics, where by the late seventeenth century, it was being used to describe the flow of money. But architecture did not adopt it until two centuries later. For architecture, it offered a way to think of the movement of people within a building independently of the physical form of the building – it allowed a degree of abstraction. This kind of abstraction had not previously been a feature of architectural thinking. And in the early twentieth century, architects started to use this abstraction as a determining factor for the shape of buildings – as in Le Corbusier's 'outrageous fundamental proposition: *architecture is circulation*'. (2, p.47) Le Corbusier fully grasped the possibilities of this metaphor: think 'movement', and shape the building accordingly. And there are plenty of more recent examples of this way of approaching design, like the British architect James Stirling's 1975 design for an unbuilt museum at Düsseldorf.

Thinking of a building as a system of flows, whether of people, of goods, air, or anything else, can indeed be very productive. 'Circulation' has certainly done good service as a metaphor. But all metaphors, as well as opening up certain possibilities or connections, also close others down. In lots of fields, notably politics, metaphors can hide inconvenient things, as much as they may draw attention to new possibilities. Architecture is no exception here, and 'circulation' as a metaphor, while it allows for a particular aspect of buildings to be abstracted and treated independently, also encourages one to think of buildings as sealed systems. Seen in the broader perspective, one of the great shortcomings of modern architecture has been to treat each building as an independent, enclosed system of its own. But the reality is not like this: in buildings it is not always the same people going round and round, returning to the same point, but rather a matter of people coming in from outside, staying for a while and doing something in the building, and then going out again. 'Circulation' is not a good description for this kind of occupation. Buildings are part of larger systems, they are not discrete systems of their own. A better metaphor for human movement might be 'respiration' – but this has never caught on; it lacks the neatness of 'circulation', and would interfere with architects'

liking to see every building as a self-contained entity of its own – regarding each building as 'like a small city', in Alberti's metaphor (1, p.23).

From 'circulation', let us turn to look at another scientific metaphor, 'function'. This takes us back to Le Corbusier's sketch that I showed you earlier. Like 'circulation', 'function' had been around in the sciences for some time before being taken over by architecture. But 'function' is more complex, because it emerged in several different contexts. Architecture made use of all of these senses, not necessarily distinguishing between them. In the early twentieth century 'function' came to be integral to architectural thinking, an axis that held together a lot of disparate architectural ideas, and it occupied something of a hegemonic role within architecture. 'Function' had not been a word, or a concept, in architecture before the nineteenth century: we are looking at a wholly modern way of conceiving architecture. And, of course, it is a metaphor – and because it borrowed from such a variety of different sources, accumulating meanings all the time, by the mid-twentieth century had become very fuzzy indeed. 'Form Follows Function', probably the one architectural aphorism everyone has heard of, may sound precise – but it is not, it's a muddle of ideas. Fuzziness, it should be said, often turns out to be an asset in language; precision isn't necessarily a virtue as far as language is concerned. What are the sources for the metaphor? Well, first of all we have mathematics, with Liebniz's proposition that a function represents a compound of variables. Then we have biology, with Cuvier's comparative anatomy supplying the idea that the organs of plants and of animals are to be classified not by their appearance, but by their 'functions' – locomotion, respiration, reproduction, digestion, circulation. The significance of Cuvier's concept of 'function' was that it allowed the possibility of relationships between things that had nothing visibly in common. Applied to architecture, this was to be a concept of momentous importance, as we shall see. And then there was a third source for the metaphor, in machines, and engineering.

Let us look at how two of these metaphors unfolded within architecture. First of all, let us take the biological metaphor. Cuvier's contribution to biology was to shift attention away from the outward visible appearance of things, and on to their internal organisation. This development was, according to Michel Foucault (5), part of a paradigmatic shift in all aspects of thought and knowledge in the early nineteenth century. Its impact upon

architecture was to be seen, initially, not in the production of new architecture, but in the study of old architecture. The French architect and archeologist Eugène-Emmanuel Viollet-le-Duc set about analysing Gothic architecture in a way no one had done before, by seeking to understand its internal system, and interpreting changes in the form of buildings in relation to the evolution of that internal system. Viollet-le-Duc's drawings set out to demonstrate the internal functions of the various structural members of Gothic cathedrals. What is shown in them has no relation to anything that you could see; drawings of this sort were a novelty in architecture, and his technique was borrowed directly from anatomical and physiological illustration. Viollet approached the study of each element not purely in terms of its immediate function, but as having a structural relationship with every other part of the building. Like Cuvier, who boasted that from a single vertebra one could reconstruct the entire animal, Viollet maintained that from a single fragment it was possible to reconstruct the form of a whole building.

As I have said, the biological notion of 'function' was not at first used in relation to the design of new buildings, but only in the analysis of old ones. But if we jump forward sixty years or so, to the 1920s, we do find the biological metaphor being used in the creation of new designs. Le Corbusier's Villa Savoie, at Poissy outside Paris, is one of the iconic buildings of twentieth century architecture. It's a three story villa with the services – car parking, maid's room, laundry – on the ground floor, the main accomodation on the first floor, and a roof terrace on the second floor. The three levels are connected by not one but by two circulation systems, a spiral stair, and a continuous ramp that goes from the ground up to the roof terrace. Conveniently, Le Corbusier (2) provided a description of how he arrived at the design, and it goes like this. You have already seen part of this drawing, and now we can look at the whole sheet. Le Corbusier's description runs through a sequence of metaphors.

A little biology to begin with:
this skeleton *for carrying*
this muscular *filling for action*
these viscera *to feed and to operate*

A little automobile construction:
a frame

> a body
> a motor with its organs of feeding and evacuation
>
> Please note, in this last case, with what flexibility the electric cables, the gasoline pipes, the exhaust pipe go around the rigid organs – the motor, the frame, the body, etc.
> And in this sketch, in the upper corner, the rigidity of the elements of a masonry house, all slavishly superposed from floor to floor, and, next to them, the flexibility of the modern house with its independent structure, its free interior plan independent from floor to floor. (pp.123-4)

As we see, he moves from the body as a metaphor, to the car, until he finally reaches the house. The design aims 'to resolve numerous modern functions'. His procedure in developing the design is to classify the various organs of the house – heating, ventilation, daylighting, artificial lighting, vertical connections, and horizontal connections, or circulation. These are the 'functions', and they are just like Cuvier's anatomy, but applied to building. 'A cold blooded examination of these questions can give solutions that will make a revolution in the building industry' (p.126). The procedure is 'to plan a dwelling in accordance with the logic of reasonable functions' (p.127), 'thinking out well the functions by which our occupant will find pleasure in living in his house' (p.132). At the Villa Savoie, circulation is particularly privileged, with the vertical spiral stair – 'a pure vertical organ' – providing one system, while the other is the sequence of ramps. For the plan, Le Corbusier evolved alternative ways of fitting the organs in – either in the first example, you let the organs bulge out and give the house its outward shape; or you compress them together within a rectangular box, which has the advantage that the box can be given regular proportions; the Villa Savoie is a composite, both a box, but with the organs allowed to take on their own shapes inside the perimeter.

If this is one sort of 'function' metaphor, derived from biology, the other one that I want to talk about is derived from machines and engineering, and emerged first of all in Germany. Now, ever since the rise of the 'aesthetic' in the late eighteenth century, architecture had suffered from a particular disadvantage in relation to the other arts: Kant's definition of the aesthetic specifically excluded 'use', or 'utility'; beauty and utility were distinct, non-compatible qualities. Particularly in Germany, where Kant's philosophy had the greatest influence, this created a difficulty for architecture,

which has always, after all, relied upon its usefulness as part of its raison d'etre. But architects were prohibited from referring to the utility of their works as having any *aesthetic* value. By the late nineteenth century there was increasing pressure to make German industry commercially competitive in the world market, and people looked to the success of Britain and the United States, noticing that the superiority of British and American goods lay in large part in their *simplicity* and efficiency. The same argument started to be extended to architecture, with an expectation that the same principles of standardisation and efficiency that characterised American products particularly should be adopted within German architecture and design. But this would upset the rules imposed by Kantian aesthetics: there had to be some kind of conceptual shift, whereby people could dispense with the old system of thought. This is the kind of work that metaphors are often rather good at, and in Germany, from the 1890s, people started arguing that architecture should be seen according to the standards of automobiles, bicycles and so on. They should follow the same principles of matter-of-factness, straightforwardness, and objectivity – and they coined a new word to describe this quality – *sachlich*. Pictures of cars, bicycles, and engineering structures like grain silos and bridges started to pervade architectural publications, and were presented as if they were the outcome of an evolutionary process analogous to Lamarck's theory of biological evolution, in which needs, or functions, determined forms. By the 1920s, these mechanical forms are seen as precedents for architecture, and *sachlichkeit* becomes the new buzzword. Emboldened by this double, bio-mechanical, metaphor, some architects for the first time staked their reputations on utility, and got rid of the Kantian distinction between aesthetics and utility. As the German critic Adolf Behne (6) said in 1928, 'Function is the real saint in art history: it set people free' (p.61) – in other words it opened the border between utility and architecture.

We have seen one example of 'functional' architecture with Le Corbusier's Villa Savoie. Many others appeared in Germany, notably the housing schemes around Berlin, in Frankfurt and the 1927 housing exhibition in Stuttgart, the Weissenhofsiedlung. But what is surprising is that almost as soon as architects had discovered the potential of 'function', many of them turned against it: they drop it almost as quickly as they had taken it up. There were various reasons for this, not least that it was so easily ridiculed, like the cartoons that appeared at the time of the Stuttgart exhibition, or

other cartoons of 'functional' design. 'Function' seemed to turn people into prisoners of their buildings, or their furniture. Architecture, or design, that was so closely fitted to prescribed human needs ended up leaving no scope for human variability, human disorderliness. Architects who had been enthusiastic functionalists in the early twenties were distancing themselves from it by the early thirties. So, in 1933, Le Corbusier (7) says, 'let's empty the bag of *Sachlichkeit* completely. Its equivocal basis rests on the postulate that is as affirmative as it is doubtful: "that which is useful is beautiful"'. He continued with an anecdote from his office after someone had just repeated this cliché.

> 'At the same moment Alfred Roth […] kicked in the side of a wire mesh wastebasket which couldn't hold the quantity of old drawings he was trying to stuff in. Under Roth's energetic pressure, this wastebasket, which had a technically *sachlich* curvature (a direct expression of the wire netting), deformed and took on the appearance shown in the sketch. Everyone in the office roared. "It's awful", said Roth. "Ah, but this basket now contains much more", I replied: "it is more useful so we could say it is more beautiful! Be consistent with your principles'. […] I immediately reestablished equitable balance by adding: "the function beauty is independent of the function utility: they are two different things"'. (pp.603-4).

I have told you this because it is a classic case of a metaphor turning sour. What had, initially, been so liberating to architects, turned out to be a constriction, depriving them of freedom. Even Adolf Behne(8), who had been so enthusiastic about function, was to ask 'Is consistent functionalism not a dead-end street?'(p.123). In the later twentieth century, functionalism came to haunt architecture, and was often singled out as the cause of everything that was wrong with modern architecture. Its apparent prescriptiveness seemed to fix buildings according to the needs of a certain moment, and to take no account of changes in occupation over time; and dedicating a building to a particular pattern of use seemed to leave no scope for the users of buildings to invent their own forms of inhabitation and sociability. By the 1980s, the last thing that any architect wanted was for their work to be referred to as 'functional'. Yet, in a sense, 'function' has not gone away, it is still there: it is a little like eating the fruit of the tree of knowledge, once tasted it cannot be forgotten. 'Function' changed the conceptual schema through which architecture operates, and brought

utility, previously excluded from architecture, into its domain. You may choose to deny function, but it is still, nevertheless, there. 'New metaphors have the power to create new realities' say Lakoff and Johnson (9, p.145) in their book *Metaphors We Live By*, and this is what has happened with 'function'. Until some other, equally explosive, metaphor comes along, there seems very little chance that 'function' will be dislodged.

Finally, I would like to turn to one more scientific metaphor that has become something of a new paradigm in architecture, and that is 'research'. You might be surprised that I call this a metaphor – and I agree that there is a doubt when we call architecture 'research' whether this is a metaphor or not. Are we saying that architecture *is* research, in the way science *is* research; or are we saying that architecture is *like* research?The signs that what architects do is often now to be regarded as 'research' are plain enough to see. What used to be called studios are renamed 'labs', and what were once called design projects are now 'research projects'. Architectural practices, as well as presenting portfolios of designs, also publish 'research'; and some architects present themselves as 'researchers'. In all this, the distinction between what is 'design' and what is 'research' is – deliberately – left very hazy. Part of this can be explained by the existence of research funding programmes, heavily oriented towards scientific research, and the aspiration of architects to enjoy some of the largesse of these schemes.

Research in architecture is not new, it has been around for a long time. Indeed some of the 'functional' architectural projects in 1920s were beneficiaries of research funding. However, by and large, architectural research has been confined to those parts of architecture that overlap with other disciplines: so for example physics enters architecture in acoustics and energy, mathematics enters architecture in the study of building morphology, engineering in the study of materials and structures, the humanities in the study of architectural history. These fields all offer well-developed methods of research that have been applied to architecture. There is nothing new or controversial about research in any of these ancillary branches of architecture, in which there are long and respectable traditions of research. In the 1960s, some architects started to describe themselves as 'research architects'. One of the first to do so was Kevin Roche, the associate of Eero Saarinen. When Saarinen received the

commission to rebuild Dulles Airport in Washington, he and his assistants began by spending six months at the airport with stopwatches, timing everything that moved, planes taking off and landing, passengers entering and leaving, and so on. And when Roche was commissioned to design a school, the first drawing that he produced was a diagram, showing the ratio of teachers to pupils. But what Roche was doing was research into the *programme* for the buildings, or if you like into the 'functions', though he probably would not have used that word. This was not new. What is new today is to cast the core business of architecture as research – and by 'core business' I mean the imaginary projection of spaces and places that do not yet exist, and the capacity to describe in detail the techniques and procedures through which those imagined projections might be realised. It's all about imagining – and even if digital design narrows the distance between the imagined and the built, architecture still belongs in the work of imagination, of projection. Is this 'research'? Well, no, not in the conventional sense in which research has usually been understood, as proceeding from a question, or a hypothesis, through experimentation, to the discovery of new knowledge. In a sense, the architect has to know it all before they start, and only when it is finished can it be judged whether they knew enough, or had adequately configured the knowledge that they had. If anything, architectural practice is more like the reverse of research as it is usually understood: you start off knowing, or pretending to know, everything, and only at the end do you discover what you didn't know. For these reasons I am uneasy about accepting that architectural practice can be research – though I am quite prepared to accept that the techniques of the architect can be used to investigate things that may be resistant to investigation by other means. In other words, while architecture can certainly be put to use for purposes of research, I am doubtful that architectural practice itself can be research.

In all this discussion, though, there is an assumption that 'research' is the defining activity of science, and that what we are seeing is the attempt to apply a scientific paradigm to a practice that isn't entirely scientific. But I think that we have to consider the possibility that 'research' itself might not be quite as inherent to science as we sometimes think. 'Research' as a practice only became identified with science in the early nineteenth century. Previously science had got along quite well with 'observation' and 'enquiry' as its guiding precepts – and of course these procedures are not exclusive to 'science' in its modern sense, but embrace all fields of human

intellectual endeavour. 'Research' owes its origins to Wilhelm von Humboldt, elder brother of the better-known Alexander. While Alexander went dashing around the world discovering things, Wilhelm stayed at home in Prussia, and devoted himself to the reform of the educational system; his model for university education stressed the interrelation of teaching and research, and saw research, free and autonomous, as the means by which young minds would overcome dogma and received authority. Humboldt made 'research' an a priori principle of university education, in all disciplines, humanities and sciences alike. In this he was opposing the French *grandes écoles*, with their emphasis on received canons, and hierarchies. From Prussia, Humboldt's model was exported to, in particular, North America, where it became embedded in the university system. In art and architecture, Humboldt's system was an antidote to the long dominant French, Ecole des Beaux Arts system, which consisted of teaching fixed precepts; when Walter Gropius set up the reformed Bauhaus in 1924, he wasn't, as is usually said, so much creating a school of the avant garde, as applying a century old Prussian method, in which 'research' was at the top of the curriculum. Gropius subsequently went to the United States, where Humboldtian 'research' was already well established in higher education, and he had no difficulty in introducing the 'research' principle into architecture at Harvard.

But the recent adoption of 'research' in architecture is more than just an extension of the Humboldtian principle, or Gropius's interpretation of it. In recent years, we have seen the application of 'design thinking' to all sorts of other fields, like business studies and management, that have nothing to do with design practices. The presumption on which this is based is that the way designers work offers a less rigid, more lateral approach to problem solving to that normally followed in those fields – if managers, or scientists, could be more 'creative' in their way of thinking then they might do their jobs better. Architecture, because of its supposedly less restrictive way of working, has won the interest of other disciplines, and this favourable attention that design professionals find themselves receiving has also, I think, contributed to the desire to describe what they do as 'research', to give themselves equality with other, often better resourced, disciplines.

What we are seeing then is an apparent convergence between architecture and the scientific community, united through 'research'. Now, as I see it,

this isn't necessarily good for architecture. If one of the things that makes architecture tick, as I have been suggesting, is metaphors, then the closer architecture comes to science, the less effective metaphors drawn from science will be. Metaphors gain their force from their *unlikeliness*, from the lack of similarity between the fields involved. The more like science architecture pretends to be, it will, paradoxically, be less and less able to borrow from science – and this would be a loss. The best metaphors come as a surprise.

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SARTON MEDAL LECTURES

Laudatio Ignacio Czeguhn

Rik Opsommer

Ignacio Czeguhn may be considered one of the most multitalented legal historians in present-day Germany. Shortly after his birth in 1966 in San Sebastian, his family settled in Würzburg. From 1984 onwards, he studied law at his hometown university, the "Bayerische Julius Maximilians Universität", where he obtained his masters degree in law (the so-called German "Erstes Staatsexam") in 1993. During his studies for the second German "Staatsexam", he started working as a temporary assistant at the Institute for Legal History of the same university under the supervision of professor Jürgen Weitzel. Under Weitzel's influence Czeguhn took on medieval legal history. With joy I remember our one year common research on early medieval Germanic criminal justice. However, be it rather exceptional for a German researcher, Czeguhn quickly turned to (late medieval) Spain. Already in 1998 he published his first article in the famous "Zeitschrift der Savigny-Stiftung für Rechtsgeschichte. Germanistische Abteilung" on the role of the Spanish king as lord of the Basque Country, followed the next year by an article in the "Tijdschrift voor Rechtgeschiedenis" on the Estates of Castile during the 15th and 16th centuries. Anno 2001 these researches culminated in his German Ph. D. on the history of the superior courts and the supreme justice in Castile between 1250 and 1520. Only one year later this study was published in the prestigious series "Schriften zur Europäischen Rechts- und Verfassungsgeschichte". The work was also granted the prize of best Ph. D. of the Würzburg University. This led Czeguhn to becoming a well-known name in the circle of Spanish legal historians. Four of the six book reviews of his Ph. D. were actually in Spanish.

In 2011 Czeguhn, Weitzel and two Spanish colleagues initiated an edition on superior courts during the reign of the Holy Roman emperor Charles V ("Die Höchtsgerichtsbarkeit im Zeitalter Karls V."). This book can be considered as a masterpiece in comparative legal history. It certainly inspired Czeguhn to continue on that same track, since in 2014 together with Francesc Puértolas, another Spanish colleague, he edited an equally influential book on the role of the Spanish constitution of 1812 within the framework of other early 19th century European constitutions. With some Spanish pride, the editors consider this constitution as "el comienzo del constitucionalismo europeo".

For his habilitation, Czeguhn researched a German topic in which he demonstrated not to be afraid of taking on the difficult Nazi years. The "Reichserbhofgesetz" was an agricultural law from 1933 aimed at keeping huge estates in the hand of one German individual. This whole legislation was part of the Nazi "Blut-und-Boden" ideology. But in the end the "Reichserbhofgesetz" was not economically successful. The first printed results of this research topic will be published next year in the prestigious "Handbuch zur deutschen Rechtsgeschichte".

Since 2007, Czeguhn has taken an interest in all legal aspects of Germany's colonial ambitions in the late 19th century. He published on the role of the German parliament in colonial law-making as well as on the laws in effect in the German colonies themselves. The Sarton-speech, which he will deliver today, will look closer to the role of the German colonial companies, thus showing his interest in law and economics as well. I am convinced that this new approach on economics-colonies-law is still an enormously underestimated research topic in many European countries, Belgium included. And as could be expected, Czeguhn was quickly able to find an Iberian connection in this field of research as well. In 2013 he published an interesting article on the early Spanish and Portuguese slave-trade laws during the 16th century.

In 2014, Czeguhn was the driving force behind "Recht im Wandel, Wandel des Rechts" a voluminous book in honor of Jürgen Weitzel, in which he himself published a contribution on constitutional questions in 17th- and 18th-century France, showing Czeguhn's increasing interest in European legal history.

In 2012, he was one of the co-founders of the review "Rechtskultur" a new multilingual journal on European legal history. The journal is quite original since each issue is devoted to a single topic, providing the opportunity to get to know the various research methods in one specific field.

However, one should not forget that not all German professors have the luxury many other European colleagues have, since the latter can spend their lectures and research exclusively to legal history. As a German legal historian, Czeguhn also has to teach and perform research in other legal fields. At his Berlin University, he is for example also responsible for civil law. Hence, it is not a surprise that together with Claus Ahrens he published a highly estimated handbook on contract law. More than ten shorter articles on civil law topics were published in well-known German journals such as "Juristische Arbeitsblätter", "Juristische Schulung" or "Monatsschrift für Deutsches Recht".

Czeguhn not only is an excellent researcher, he also has excellent supplementary linguistic skills. At almost each international conference, he takes up the role of a translator, especially when it comes to the point of introducing Spanish-speaking legal historians and initiating them with the research-results of German and/or English native speakers. He continues to play that role outside the conference room, since his pan-European cultural knowledge is quite extraordinary. The fact that during his studies, he worked as an official Würzburg city-guide now clearly pays off. The communicative skills, which he obtained as a guide, nowadays are also an excellent asset when he organizes colloquia.

Legal historians often have peculiar hobbies and thus their name appears at unexpected places. Even in this field, however, Ignacio Czeguhn is able to combine his hobby with high publication standards, since this year he was the main author of one of the best publications on 19th-century German tin figures ("Zinnfiguren der Offizin Allgeyer").

With Czeguhn obtaining the Sarton medal, he follows in the footsteps of his late "Doktorvater" Jürgen Weitzel who received the medal in 2003. One can say that in the true spirit of Georges Sarton, the European (Spanish-Belgian-German) legal historical circle closes itself.

The German Colonial Companies 1887-1914

Ignacio Czeguhn

I. Introduction

Aside from the earlier attempts by the Great Elector in the African Gold Coast region, the concept of German colonialism was only proposed more intensely after the founding of the Reich in 1871 and, in 1882, the combined efforts of the industrial, trading and banking sectors led to the establishment of the 'German Colonial Companies'.[1] However, under chancellor Bismarck, the German role in colonial politics was relatively restrained when compared to the other European countries. Bismarck, who had still opposed an expansive colonial policy in 1881, did not want to disturb the delicate balance of the European powers, especially in relation to Great Britain, by claiming overseas territories; he furthermore deemed the costs of protection and administration of colonies to be higher for the Reich than the economical advantages. The German expanse in 1884 and 1885, conducted under Bismarck, was rooted more in domestic reasoning than foreign politics. The main goal of colonial territories was to provide a means for relaxing domestic issues stemming from industrialisation by offering emigration options and to hide troubles in domestic politics; furthermore, the chancellor was seeking to gain the votes of pro-colonial factions for the Reichstag election on October 28, 1884 and thereby harming the social democrats.[2] Economically, the goal was to improve

1 *van der Heyden*, Ulrich Rote Adler an Afrikas Küste. Die brandenburgisch-preußische Kolonie Großfriedrichsburg in Westafrika, Berlin 2001, p. 8 ff.

2 *Canis*, Konrad, Bismarcks Außenpolitik 1870-1890, Paderborn 2004, p. 85-108; Riehl, *Axel*, Der "Tanz um den Äquator". Bismarcks antienglische Bündnispolitik und die Erwartung des Thronwechsels in Deutschland 1883-1885, Berlin 1993, p. 22.

German foreign trade, which had been hampered by the custom protection territories of other colonial powers and, by providing new markets and material sources, to overcome the recession, which had dominated German economics from 1882 until 1886. It was under these circumstances that, on December 19, 1887, the 'German Colonial Club' and the 'Company for German Colonialism' merged into the 'German Colonial Company'. Now, barring a few exceptions, Germany had already gained most of its protectorates. Additionally, a further separation of the colonial areas among the industrialised nations did not appear to be very likely.

II. The 'German Colonial Company'

§ 2 of the German Colonial Company's statute stipulated the following principal goals:

> 'In order to serve the fatherland, making common knowledge of the German people the necessity of German colonies; the care and promotion of existing German colonies in matters organisational, economic and academic as well as the clarification and public representation of all further colonial and overseas interests of the German nation; without regard for any statements on party politics the German Colonial Company strives to convince all political parties for the German-colonial cause and to cause effect in that sense especially in times of important decisions.'

The German Colonial Company was organised in chapters, grouping together members of individual districts, as well as in regional groups, consisting of several chapters. The president, the committee, the board and the members' assembly were the bodies of the Company.[3]

Every year, the regular assembly of the members was held. The delegates of the chapters elected the board, who chose a president from their midst. From 1887, four vice presidents were elected in the same manner, and, from 1912 onwards, five vice-presidents and, should the president so wish, a managing vice-president for the duration of his term. The committee members were elected by the board and by presidential appointment.[4]

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3 *von Stengel*, Karl, Die deutschen Kolonialgesellschaften, ihre Verfassung und ihre rechtliche Stellung, in: Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich (Schmollers Jahrbuch) 1 (1888), p. 219 ff.

4 Ibid.

For the first years of the German Colonial Company, *Hermann Fürst zu Hohenlohe-Langenburg* held the office of the president. When he was appointed governor of the Reich-territory of Alsace-Lorraine, *Johann Albrecht Herzog zu Mecklenburg* took his place. He remained in office even after being called to reign over the Grand Duchy of Mecklenburg-Schwerin, following the death of the *Grand Duke Frederick Francis III.* While the president was not present in Berlin, the managing vice president conducted the business, acting as a deputy. Until the summer of 1900, this office was held by *Geheimrat* (Senior Advisor) Adolf Sachse. He was succeeded by Vice Admiral Victor Valois. In 1902, the board had grown to 150 members, including representatives of club branches that had over 50 members. The executive committee consisted of the president, the managing vice president and the four vice presidents.

To fulfil their substantial honorary duties, the governing and executive bodies relied on the central office in Berlin, paid officials as well as two departments, which included the special department for 'Central Point of Information for Emigration'. On November 16, 1896, Wilhelm II. granted the rights of a legal entity to the German Colonial Company, thereby providing it with more potential for action.[5]

In the year of foundation, the Company had 14,838 members, a number that had doubled 15 years later: in April 1902, the Company had 32,756 members. In 1913/14, the 43,000 members of the Company were strewn across 462 towns in the Reich, the German protectorates and the sections in foreign countries. Support for the local chapters, which were grouped into regional sections, came from the central office in Berlin.[6]

Members of the Company primarily came from the upper middle classes. However, a large number of industrialists from Rhineland-Westphalia, bankers, other industrial representatives, members of the high nobility and the nobility, as well as civil servants were part of the Company.[7]

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5 *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977, p. 20 ff; *von Stengel*, Karl, Die deutschen Kolonialgesellschaften, ihre Verfassung und ihre rechtliche Stellung, in: Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich (Schmollers Jahrbuch) 1 (1888), p. 219 ff.

6 *Demhardt*, Imre Josef, Deutsche Kolonialgesellschaft 1888-1918 Ein Beitrag zur Organisationsgeschichte der deutschen Kolonialbewegung, Wiesbaden 2002, p. 23.

7 *Ballhaus*, Jolanda, Wesen und Charakter der kolonialen Landgesellschaften Ende des 19. Jahrhunderts, in: Jahrbuch für Wirtschaftsgeschichte 13 (1972), p. 95 ff.

The activities of the Company were focused on colonial propaganda in Germany, as well as hands-on work in the colonies. The colonial cause was promoted through the publication of books, pamphlets and magazines. This included the weekly 'German Colonial Papers', which were provided free of charge to every member, as well as the 'Colonial Monthly', which appeared 12 times a year. Further measures included the colonial congresses, organisation of colonial exhibitions in Germany, as well as the exhibition of agricultural measures in the colonies.[8]

As a stakeholder in companies providing settlement and property in the colonies, the Company supported industrial companies in the colonies. In doing so, it aimed to influence the political decision of the parties and the government.[9]

The exploration of German protectorates, undertaken by many explorations under the guise of academic goals, often proved to be a front for the expansion and affirmation of German colonial territories. The Company provided a great deal of support for those looking to emigrate to the German colonies. The 'Central Point of Information for Emigration', founded in 1884, was in effect transformed into an office of information, whose main duties included marketing the possibility of buying property in the colonies for industrial workers. The Company inititated numerous projects and measures supporting emigration.[10] These included:

- the institution of a steam ship connection to German South West Africa in April 1891, transferring settlers to the colonies,
- the founding of the 'Syndicate for South West Africa Settling', 1890,
- Supporting settlements with financial aid and exploration of the possibilities of settling in German South West Africa,
- instituting the Committee for German East African Settlements 1905,
- preparation of settlements in German East Africa by exploring West Usambara an Uhehe, to name but a few.

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8 *Demhardt*, Imre Josef, Deutsche Kolonialgesellschaft 1888-1918 Ein Beitrag zur Organisationsgeschichte der deutschen Kolonialbewegung, Wiesbaden 2002; *Ballhaus*, Jolanda, Wesen und Charakter der kolonialen Landgesellschaften Ende des 19. Jahrhunderts, in: Jahrbuch für Wirtschaftsgeschichte 13 (1972), p. 95 ff.

9 *Fichtner*, Axel, Die völker- und staatsrechtliche Stellung der deutschen Kolonialgesellschaften des 19. Jahrhunderts, Frankfurt a.M. 2002, p. 58.

10 *Ballhaus*, Jolanda, Wesen und Charakter der kolonialen Landgesellschaften Ende des 19. Jahrhunderts, in: Jahrbuch für Wirtschaftsgeschichte 13 (1972), p. 95 ff; *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977.

Special consideration was also given to emigration to South America. The direction of settlers towards South America as a first step towards a future German colony was the calculated goal of the German Colonial Company, and was also aimed at undermining the Prussian ban on emigration to Brazil. Additionally, substantial colonisation projects had been established in Mexico by buying up land.[11]

The German Colonial Company especially promoted the policies regarding indigenous people in the German colonies by implementing the native law, as well as the formation of a Commission for Research and Combatting Slavery (1888).

The Women's Association of the German Colonial Company had a special role, with the mission to 'establish and uphold a fertile ground in our colonies for the German family spirit and German custom and way of life'. Following this principle, the Women's Association acted as a job broker, providing young women with placements as cooks, maids, governesses and teachers and, in doing so, also engaged in marriage brokering.[12]

As the umbrella organisation of the organised colonial movement, the German Colonial Company acted together with other German-national interest groups, such as the *Alldeutsche Verband*, the *Deutsche Flottenverein* or the *Verein für das Deutschtum im Ausland*, and was one of the first supporters of the build-up of a German navy.[13] On December 12, 1893, the Company called upon the Reichstag to provide the resources necessary for a quick naval armament. In the following years, calls for a fleet by the Company increased. The long-planned campaign for the acceptance of the Naval Bill of August 23, 1898 was met with support from the Company and resulted in the creation of a 'Committee for the Fund for Creation of a Navy'. Together with representatives from the Imperial Office for the Navy, the Company worked on establishing cooperation with the Naval

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11 *Emmes,* Manfred P. Interessenorientierungen und Konfliktlinien Großbritanniens, der Vereinigten Staaten von Amerika und Deutschland vom Ende des 19. Jahrhunderts bis zum Beginn des Ersten Weltkriegs, Berlin 2013, p. 86.

12 *Wildenthal*, Lora, German Women for Empire 1884-1945, Durham / London 2001, p. 191; *Luikenga,* Margret, Frauenbund der deutschen Kolonialgesellschaft, in: Den Frauen nach: Spaziergang am Landwehrkanal. Zur Berliner Frauengeschichte, Berlin 1993, p. 89.

13 *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977, p. 112.

Clubs located overseas from October 1897 onwards. In June 1898, the Association of German Overseas Naval Clubs was founded.[14]

Following the turn of the century, a temporary decline in influence of the Company can be registered. Lower numbers of membership substantiates this, among other things. The board of the Company reacted by taking measures aimed at strengthening the organisation and improving its mass influence. The Company successfully lobbied for universities to have more chairs teaching Geography and Ethnology. A further achievement was more geographical and ethnological lessons at schools in Germany.

From 1902, the Company increased membership numbers by propagandistic preparation and holding of the German Colonial Congresses and other public events.[15]

When the Reichstag denied money necessary to crack down on the uprising of the Herero and Nama on December 13, 1906, the relationship between Chancellor Bülow and the Centre Party broke down. When the conservative and liberal fractions emerged from the elections of January 1907 with a strengthened position, the position of the Company stabilised. This was followed by a uncommonly high increase in membership numbers.[16] On December 5, 1907, the Company followed calls to clearly and bindingly position itself in the Reich at an extraordinary member's assembly by amending the statute. It now read:

> 'The German Colonial Company aims to make the awareness regarding the necessity of German colonies common knowledge of the German people. The Company aims to uphold and promote the existing German colonial possession in organisational, economical and academic regards as well as establishing and promoting all other colonial and overseas interest of the German Nation. While rejecting any political statement along party lines, the Company seeks to convince all Parties in the German Reich for the

[14] *Demhardt*, Imre Josef, Deutsche Kolonialgesellschaft 1888-1918, Ein Beitrag zur Organisationsgeschichte der deutschen Kolonialbewegung, Wiesbaden 2002.

[15] Ibid.

[16] *Ballhaus*, Jolanda, Wesen und Charakter der kolonialen Landgesellschaften Ende des 19. Jahrhunderts, in: Jahrbuch für Wirtschaftsgeschichte 13 (1972), p. 95 ff.; *Demhardt*, Imre Josef, Deutsche Kolonialgesellschaft 1888-1918 Ein Beitrag zur Organisationsgeschichte der deutschen Kolonialbewegung, Wiesbaden 2002; *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977.

colonial cause and to act in that sense, especially in times of difficult decisions.'[17]

Nearly seven years later, the Company concluded that its field of action was too narrow and declared that colonial politics could not be viewed isolated from 'national tasks'. As such, an Overseas Commission was formed, tasked with improving the economic and cultural influence of the German Reich in the world, to strengthen the 'powerful economic and cultural position of Germany in the world economy'.

When the Great War erupted, propaganda regarding the goal of the war became the focus of the Company. The Committee agreed upon the following guidelines on June 16, 1916:

'1. Overseas and especially colonial activity is politically, economically and ethnically indispensible if the German Nation wishes to remain a leading nation […] of the world.

2. There surely is a need for expansion of the own areas of the German Reich and Nation in Europe as well as there is a need for a tight political, military and economic cooperation with friendly states in central Europe and towards the Orient; the possession of own colonies however does offer the necessary addition of European Germany, and due to which it will become an even more valuable friend to its partners. […]

7. The large demand in Germany […] for colonial resources, the necessity of securing market areas […] make the acquisition of colonial possession without timid anxiety an even greater necessity, as a comparably favourable situation may not come again in the near future.

8. As appealing as the thought to restrict oneself to an enclosed colonial empire might seem, […] a consideration of German interests shows […] that a colonial empire solely in Africa is not sufficient.'

These presumptuous war ambitions were null and void after the lost war. The Reich had to relinquish all colonial and overseas territories per the renunciation clause of the Treaty of Versailles, which led not to a margin-

[17] *Prager*, Erich, Die deutsche Kolonialgesellschaft 1882 – 1907. Im Auftrage des Ausschusses der Deutschen Kolonialgesellschaft dargestellt, Berlin 1908; *Reimer*, Albert, Die allgemeinen Rechtsverhältnisse der deutschen Kolonialgesellschaften. Unter Berücksichtigung der vom Reichskolonialamt ausgearbeiteten Mustersatzungen, Berlin 1910; *Sander*, Ludwig, Geschichte der deutschen Kolonial-Gesellschaft für Südwest-Afrika. Von ihrer Gründung bis zum Jahre 1910, Berlin 1912 (*Band 1: Geschichtliche Darstellung; Band 2: Urkunden).*

alisation of the colonial movement, but to a strengthening.[18] Again, the German Colonial Company was at the front of this movement, now with the aim of reinstating Germany into her colonial rights. This made a reorganisation of the German Colonial Company necessary. In preparation of the Committee meeting on April 4, 1919, a commission chaired by Dr. Friedrich von Lindequist presented a top-secret concept following this demand. It included the suggestion to hold onto the general member's assembly, in order to connect the members to the leading personalities of the Company. In addition, the assembly could also be extended to a congress on overseas interests every 3-5 years. According to the commission, board meetings were too expensive and complicated. It was proposed to abolish the board institutions, or to limit them to activities where a representation of the departments became necessary.[19]

The committee work was also deemed insufficient. Instead of a focus on the relevant matters, discussion would turn into endless matters without any moderation by a firm chair.

It was demanded that the president be a resident of Berlin, to focus fully and independently his activities on the Company. He was to be politically unsoiled, so that parties would accept him. The vice-president, however, would not necessarily have to be resident in the city. For him, the same standard was to be applied as for the president. Management was to be transferred to a general manager, who would be paid by the Company and would be responsible for the conduct of affairs under the supervision and according to the orders of the president. A small committee of three members would assist him.[20] Aside from this managing committee, the following committees would put the leadership of the Company together:

- Financial committee
- Propaganda committee
- Committee for emigration
- Colonial-economical committee

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18 *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977, p. 178; *Demhardt*, Imre Josef, Deutsche Kolonialgesellschaft 1888-1918 Ein Beitrag zur Organisationsgeschichte der deutschen Kolonialbewegung, Wiesbaden 2002, p. 98.

19 *Demhardt*, Imre Josef, Deutsche Kolonialgesellschaft 1888-1918 Ein Beitrag zur Organisationsgeschichte der deutschen Kolonialbewegung, Wiesbaden 2002, p. 94.

20 Ibid.

- Committee for the Women's Association of the German Colonial Company
- Committee for the welfare lottery
- Committee for activities in the colonies.

The proposition was that each committee would be chaired by a member of the board and the board as a whole would call the members of the committees. Every board member was entitled to participate in the sessions of every committee, albeit that the committees only had an advisory function.

The discussion on the proposed changes to the leadership and organisational structure of the Company by the Lindequist-commission began during the committee meeting on April 4, 1919. Issue was taken with the demand, to 'install in the charter a system that is as democratic as possible', which was founded on the fear that otherwise only members of the German National People's Party (DNVP) and the German People's Party (DVP) would remain as members of the Company. Only in Spring 1921 and following many disagreements did the charter committee reach the following proposal for § 1:

> 'In order to serve the country, the German Colonial Company intends to ensure the colonial and overseas interests of the German people. It aims to garner awareness that Germany cannot stand without a colonial activism adequate to her population, necessities and capabilities and must own colonies. Furthermore, it will support intellectually and economically the promotion and conservation of activities aimed at establishing an overseas German presence, will assist with words, and deeds the overseas emigration. Economically, it supports free seas and the open-door-principle. Without prejudice to any party politics the German Colonial Company strives to raise awareness for colonial and overseas issues and make this common knowledge of the German people as well as providing a rallying point on these issues.'[21]

The colonial movement, consisting of a great number of associations and organisations, required a unified direction in the first years following the

[21] *Hartmann*, Die neue Form von Überseegesellschaften, in: Verhandlungen des deutschen Kolonialkongresses 1924, p. 91 ff.; *Hupfeld*, Friedrich, Besondere Rechtsform für Überseegesellschaften, in: Koloniale Rundschau 1924, p. 22 ff.; *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977, p. 57.

war. For this reason, the Committee of the German Colonial Company decided on December 27, 1921, to support the Imperial Association of Colonial Germans and Colonial Supporters (RKK) in this direction and to intensify cooperation with other organisations. Under the Company's leadership and following the initiative of the military German Colonial Warriors Company, the Colonial Working Company (Korag) was founded in 1922 as a loose association of about 30 colonial organisations. On a local level, the chapters of the Company were instructed to establish relations with the presidents of the respective organisations, 'so that unified efforts can be undertaken and every fractured approach is avoided'.[22]

Surely, it was no coincidence that the Korag and other colonial associations established their head offices at the *Afrika-Haus,* which had been built for the Company in 1911. The aim was to have gained a leading position among the colonial organisations by the mid-20, and to enforce this using the German Colonial Company. Many incentives to have other Companies attached to the Company were initiated by the Korag.[23] Despite many attempts to unify the many colonial associations under the Company and to establish coordinated propaganda demanding the return of former German colonies, it failed to establish the necessary priority for the colonial issue during the Weimar Republic. In a 1936 report, the Company admitted that its propaganda had failed in raising enthusiasm among the 'broad masses of the German people' for the colonial issue. Already at the end of the 20s members of the colonial movement had attempted to establish contacts to the Nazi Party. The hope was for the far right to provide new impulses for colonial politics, despite the activities of the Nazi Party being marginal. Following the Nazi power grab in 1933, the colonial movement tried to connect to the power player politics of the period before 1914.[24] At the German Colonial Meeting, held in Frankfurt am Main from November 8 – 11, 1933, the Company merged with the German Colonial Club, and at the same time, joined the Reich Colonial League, which had emerged from the Korag in 1933. Officially, it was now known as the 'German Colonial

22 *Hartmann*, Die neue Form von Überseegesellschaften, in: Verhandlungen des deutschen Kolonialkongresses 1924, p. 91 ff.; *Hupfeld*, Friedrich, Besondere Rechtsform für Überseegesellschaften, in: Koloniale Rundschau 1924, p. 22 ff.

23 *Demhardt*, Imre Josef, Deutsche Kolonialgesellschaft 1888-1918 Ein Beitrag zur Organisationsgeschichte der deutschen Kolonialbewegung, Wiesbaden 2002, p. 78.

24 *Linne*, Karsten, Deutschland jenseits des Äquators? Die NS-Kolonialplanungen für Afrika, Berlin 2008, p. 21 ff.

Company. Leading member of the Reich Colonial League'. In the letter 17/33 to the members of the board and the chairs of the committees, President Dr Heinrich Schnee on June 20, 1933, confirmed the amendment of the charter to reflect new structures and organisations in the Company. It read:

> 'President's office: in future, following leader principle (*Führerprinzip*), the president's office will consist only of one president, who is to name a deputy. The current deputy presidents, insofar as they are not members of the new working committee or already honorary members, have been named honorary members following my nomination; as such, they are members of the board.
>
> Working committee: the working committee, nominated by me, has been confirmed as an organ of the Company. A new member in the working committee as a representative of the Steel Helmet, League of Front Soldiers, is retired Major von Wiese und Kaiserswaldau […].
>
> Board: The current board was drawn from the member's assembly and elected for three years, as well as chosen members of the committees. This double principle of composition of the board has been abolished in the new charter, allowing for a smaller board and the representatives of the chapters and divisions form a representative assembly. The board consists of the members of the working committee, the honorary members as well as 21 members elected by the representatives' assembly and 21 Company members named by the president […].
>
> Representative assembly: The new representative assembly will allow not only chapters, but also divisions to be represented according to their strength.
>
> General Meeting: The general meeting has followed my proposition, not require future general meetings in the charter. The representative assembly represents the interests of the general meeting.
>
> Honorary committee: the honorary committee, without functioning as an organ of the Company, will be called by me and include such personalities that are deemed to be of special value for the Company […].'[25]

The head office of the German Colonial Company was now split into ten divisions. Furthermore, there were other determinations:

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[25] *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977; *Speitkamp,* Winfried Deutsche Kolonialgeschichte, Stuttgart 2005, p. 160 ff.

- the president of the Reich Colonial League would simultaneously be the president of the Company;
- the committee of the Korag was transformed into the committee of the Company. The president was allowed to call for additional members;
- the merger of the local divisions and clubs of the associations under the name of the local Reich Colonial League division was made a goal.

The Reich Colonial League now had full control over the politics of the Company, which left little space for the Company independently to be politically active. The Company's attempt to hold onto leading positions of the League and to share power in the League with the Nazi Party ultimately failed. In mid-1934, Reich Organisation Leader Robert Ley decreed for the division leaders of the League to be made members of the Nazi party and be subordinated to the colonial experts of the Nazi Party. On May 5, 1933, the main committee resigned after allowing the president to further institute reforms for the Company. The president's function was adjusted according to the *Führerprinzip*. In 1936, all colonial Companies were dissolved into the Reich Colonial League, leading to the end of the German Colonial Company. On June 13, 1936, the representative assembly voted to dissolve the German Colonial Company.[26]

III. The Economic Colonial Committee

On June 18, 1896, following the initiative of Karl Supf, the Committee for the Import of Products from the German Colonies was founded in Berlin, Unter den Linden 43. A year later, at an extraordinary member's assembly on October 25, 1897, the committee adopted the title of the Economic Colonial Committee.[27] As a charitable organisation, the committee was one of the many colonial organisations that, as many other colonial organisations and associations, had attached itself to the German Colonial Company and, in cooperation with the Imperial Colonial Office, the Impe-

[26] *Linne*, Karsten, Deutschland jenseits des Äquators? NS-Kolonialplanungen für Afrika. Ch. Links, Berlin 2008, p. 40 ff.

[27] *Nollau*, Das Recht der aufgrund des Reichsgesetzes betreffend die Rechtsverhältnisse der deutschen Schutzgebiete errichteten Kolonialgesellschaften, in: Zeitschrift für Kolonialpolitik, Kolonialrecht und Kolonialwirtschaft 1904, p. 385 ff.; *von Stengel*, Karl, Die Konzessionen der deutschen Kolonialgesellschaften und die Landfrage in den deutschen Schutzgebieten, in: Zeitschrift für Kolonialrecht 6 (1904), p. 305 ff.

rial Office for the Interior and the Prussian Ministry for Trade and Commerce, supported the economic development in the protectorates. From a small beginning an organisation flourished, unifying German dukes, academic institutes, chambers of trade, commerce, crafts and agriculture, cities, banks, colonial, commercial and industrial organisations, workers unions and missions. As a coordination office for actors from industries, the state and other areas interested in colonial politics, the Committee commenced its activities at the same time as the economic upswing in August 1896. According to the charter, it consisted of the following organs:

1) the executive committee as board, consisting of the president and two deputies,
2) three auditors,
3) a member's assembly.

At the annual member's assembly, the members of the executive committee were elected for a three-year term, and the auditors were elected to serve for a single year. The president, following a petition by the executive committee, decided upon hiring and termination of the committee's employees. The renaming into Economic Colonial Committee in 1898, which served as an advisor on economic questions while maintaining legal independency as the 'Economic Committee of the German Colonial Company' since 1902, was emblematic for the broadened range of activities, which in 1903 included five points:

1. The promotion of the raw commodity industry in the colonies for German companies and the people's nourishment.
2. The improvement of German industrial sales, especially by establishing new mechanised industrial branches in the colonies.
3. Promoting transport infrastructure development, especially expansion of the rail networks, with and in the colonies.
4. Promotion of German settlement in the protectorates.
5. General activities in the interest of colonisation.[28]

The economic colonial expeditions of the first years undertaken by individual scientists in various tropical areas, the construction of state-run

[28] *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977.

testing stations in the colonies, the development of tropical agriculture and seeds and the professional advice provided by the magazine 'Tropical Gardener', published from 1897 onwards, intended to establish economically viable business models in the colonies. The Committee viewed itself not only as a trailblazer for private investment agencies, but rather intended to make use of the colonial economies for the needs of the home industries. This goal was supported by colonial exhibitions, touring or stationary, a publicly accessible credit agency, a job index, from 1897 the annual colonial address book and the colonial-economical archive, established in 1909.[29] The Committee viewed it as its main task to gain raw material sources to improve the Reich's position in the world market. The measures aimed at holding German capital in the colonies were often declared as welfare for the local indigenous population and included the construction of watering systems, exploration of water reservoirs, natural resources and possibilities of agricultural usage of the colonies, especially action to combat animal diseases, construction of road and rail access, academic research in various fields, as well as the introduction and expansion of cotton in the protectorates. The cotton programme, propagated in 1900 by Karl Supf, the chairman of the Committee, which was systematically realised in Togo, German East Africa and Cameroon until 1914, aimed at establishing the raw material needs of the German Reich. However, the colonies were to be developed not only as a means for delivering cotton to the home industries, but also as a customer for German industrial results. Apart from the development of the tropical landscape, railroad development received much attention. The construction of the Togo Inland Railway, the railway programme in German East Africa and Cameroon were the result of longstanding and focused campaigning by the Committee. The heavy industries were increasingly in favour of the construction works. Renown industrial actors such as the '*Friedrich Krupp AG, Gußstahlfabrik, Essen*', the '*Gelsenkirchener Bergwerks-AG*', the '*Rheinisch-Westfälische Kohlensyndikat, Essen*' and the 'League of German Iron and Steel Industrials' had gained membership in the Committee by 1909.[30] The increasing development of the Committee from

[29] *Jäckel*, Herbert, Die Landgesellschaften in den deutschen Schutzgebieten, Jena 1909; *Perels*, Kurt, Das Bergrechtsabkommen vom 17. Februar/2. April 1908 und die bergrechtliche Stellung der Deutschen-Kolonialgesellschaft für Südwest-Afrika, Berlin 1910; *von Stengel*, Karl, Die Konzessionen der deutschen Kolonialgesellschaften und die Landfrage in den deutschen Schutzgebieten, in: Zeitschrift für Kolonialrecht 6 (1904), p. 305 ff.

an institution focused on colonial trade and tropical agriculture into a colonial-economical political lobby group were illustrated by bodies established already in 1906, such as the Institute for Academic and Technical Research on Seeds, the branch offices in several colonies, the commercial management of the central offices in Berlin, as well as the implementation of the Standing Special Commissions. In these commissions, the members of the committee had a seat and a vote. They consisted of colonial researchers, industrialists, merchants and local colonial specialists. The chairman of the Committee held the chair of the commission. The deputy chair was elected from among the members of the commission. Until the beginning of the Great War, the following commissions had commenced their work: the Cotton Commission (est. 1906), the Colonial Technical Commission (est. 1910), the Rubber Commission (est. 1911), the Raw Oil Products Commission (est. 1913) and the Cotton Sheep Breeding Commission (est. 1914). The German Colonial Papers, published by the Reich Colonial Office, in 1911 described the tasks of the Colonial Technical Commission as follows:[31]

> '1. Information on the current state of railway, road, harbour and waterway construction, land improvement, on chemical-technical problems, on the availability of natural resources and the demand for certain industrial products; publication of colonial-technical papers; 2. encouragement and support for private technical expeditions in the colonies; 3. undertaking of technical pioneering work not bearable by private financing such as early work on agricultural cultures, harvest work and water-related matters, exhibition of machines and tools in the colonies and introduction of new mechanical industries, for example for the tropical agriculture in Germany; 4. use of technicians for work in the colonies.'

In special editions, the discussions of this commission were published. With the beginning of the First World War, the commission's goal of furthering the introduction of modern technologies in the colonies was impaired and finally made impossible. After the founder and organisational head of the Committee, Karl Supf, passed away in 1915, the committee was gradually reduced to an advisory function. In 1915, Dr. Ing. e. h. Dr. phil.

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30 *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977, p. 148 ff.

31 Deutsches Kolonialblatt, Amtsblatt für die Schutzgebiete des Deutschen Reichs, Berlin, 1911.

h. c. Wilhelm von Oechelhäuser took over the chairmanship of the Committee, but his professional workload forced him to step down from the office around the end of the War. Economic and Building privy councillor Friedrich Lenz became his successor in 1920. His focused approach was responsible for the continuation of the Committee even in times of dire finances.[32] Following the loss of the colonies at the end of the War, Lenz objected to all efforts aimed at disbanding the Committee in order to avoid the impression that the German colonial politics were lost beyond every hope of regaining the colonies. The broadening of the Committee's tasks was aimed at providing assistance and support to all German farmers and planters working abroad by information and reports, establishing new possibilities for sales of machines and industrial facilities by German companies, which were necessary for the extraction and processing of raw materials in Germany. The Committee relied on suitable propaganda for the colonial movement, especially by producing papers, viewing materials, lectures and exhibitions. The 'Tropical Gardener' magazine, edited from 1897 onwards by the board members Prof. Dr. Otto Warburg and Prof. Dr. Ferdinand Wohltmann, from 1922 onwards by Dr. Walter Busse and since April 1927 by Albrecht Zimmermann and Prof. Dr. Geo A. Schmidt, was the leading German magazine tropical agriculture.[33] On more than one occasion, circulation numbers had to be increased due to the high demand. The 'Supplements to Tropical Gardener', published in irregular intervals, included extensive treatises on special topics and travel reports. From 1898 onwards, the German Colonial Address Book was published, including reports on imports and exports with notation of the products and brand, agricultural Companies, companies registered in the colonies, importers and exporters, as well as sales offices. Apart from the more than 150 exhibitions in Germany and the Colonies, the Committee provided colonial materials and products for the World Exhibition in St. Louis in 1905, the agricultural exhibition in Zanzibar in 1905 and the International Rubber Exhibition in London in 1911. Committee delegates participated in the Cotton Congress in Zürich in 1904, Liverpool in 1905, Bremen in 1906, Vienna in 1907, Paris in 1908, Milan in 1909, the Congress for Colonial and Tropical Agriculture in Brussels in 1910, the Rubber Congress and the

[32] *Richter*, Lieselotte, Die Finanzierung von Kolonialgesellschaften, Dresden 1941.
[33] *Golf*, Arthur, Zu Ferdinand Woltmanns Gedächtnis, Leipzig 1919.

Race Congress in London in 1908 and 1911 respectively and participated in the wording of declarations. At the Colonial Congress from June 8 – 11, 1933 the German Colonial Company, like all other colonial organisations, joined the Reich Colonial League, which had resulted from the Korag. The German Colonial Company only had a limited amount of independency in the League. Following the *Gleichschaltung*, the League was transformed from a colonial association into the colonial-political organisation attached to the Nazi Party and the German Colonial Company was disbanded on June 13, 1936.[34]

IV. The German East Africa Company

Initially intended as a lower middle-class colonial Company aimed at collecting capital for colonial pursuits, the Company for German Colonialism was founded in Berlin by Dr. Carl Peters and Count Hugold von Behr-Bandelin.[35] The declared goal of the Company was to acquire and cultivate appropriate areas for German agriculture. This included the establishment of the necessary institutions, as well as the civilisation of the protectorate by settlement and trade, and to direct emigration to East Africa. The expedition dispatched in 1884, led by Dr. Peters and his friends served to conclude protection treaties with local independent chiefs. Due to skilful negotiation, Dr. Peters was able to conclude many treaties over a short period of time, in order to formally acquire the countries of Useguha, Nguru, Usagara and Ukami. These, along with further acquisitions, presented to the Foreign Office by Dr. Peters on February 27, 1885 – a day after the conclusion of the Congo Conference – were confirmed by an Imperial Letter of Protection. To establish a juridical form, a limited commercial partnership was founded, under the name of 'Carl Peters und Gen.'. Personally liable partners were Dr. Peters, Dr. Fr. Lange, Konsul Roghè and Carl Jühlke. Control was handed to a directorate consisting of 5 members, with management handled by Dr. Peters himself. The growing

[34] *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977.

[35] *Kurtze*, Bruno, Die Deutsch-Ostafrikanische Gesellschaft. Ein Beitrag zum Problem der Schutzbriefgesellschaften und zur Geschichte Deutsch-Ostafrikas, Jena 1913; *Ring*, Viktor, Deutsche Kolonialgesellschaften: Betrachtungen und Vorschläge nebst einem Anhang enthaltend die Statuten der Deutschen Kolonial-Gesellschaft für Südwest-Afrika, der Neu Guinea Compagnie und der Deutsch-Ostafrikanischen Gesellschaft, Berlin 1887.

tasks of administration and the expansion of the owned lands, as well as the resulting increase in material needs, soon exceeded the scope of a limited commercial partnership.[36]

On September 7, 1885, the directorate decided to transform the juridical form into a stock corporation, the German East Africa Company. Three months later, on December 14 1885, the general assembly confirmed this decision and elected a chairman of the directorate, two directors and a board of directors (with 21-27 members) to serve a five-year term.

The tasks of the first director included the hiring and release of the employees, as well as exercising the corporation's supervisory function. The Reich Chancellor, according to the charter, exercised overall supervision.[37]

After the necessary capital for the creation of a Reich corporation, an amount in excess of 3.5 million Reichsmark, had been collected at the beginning of 1887, the German East Africa was constituted, with Dr. Carl Peters serving as its president. In March 1887 the King of Prussia awarded the right of a corporation according to the *Allgemeine Landrecht* and in July 1889, the *Bundesrat* awarded the right of a Reich corporation.[38]

Berlin was chosen as the seat of the German East Africa Company, and the general office for East Africa was located in Zanzibar until November 1, 1905, and in Dar-es-Salam afterwards. It also had several trading offices and operated on a number of plantations.

Financially, the corporation held a stake in the German East African Bank, the Trading Bank for East Arica, the German Company for Tanganjika, the Rhineland Trade-Plantation-Company, the Central African Mining Company, the Ngomeni-Plantation Limited Company, the East Africa Company, the Lindi Trading and Plantation Company, the German Wood Company for East Afrika, the Usambara Magazine and others.

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36 *Fichtner*, Axel, Die völker- und staatsrechtliche Stellung der deutschen Kolonialgesellschaften des 19. Jahrhunderts, Frankfurt a.M. 2002; *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977.

37 *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977, p. 123.

38 *Ballhaus*, Jolanda, Wesen und Charakter der kolonialen Landgesellschaften Ende des 19. Jahrhunderts, in: Jahrbuch für Wirtschaftsgeschichte 13 (1972), p. 95 ff.; *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977.

According to the 1890 charter, the German East Africa Company consisted of the general assembly, an administrative board (with a standing committee), a board and an audit committee. The general assembly was called once a year in Berlin by administrative board or the board to discuss the end-of-year report and the report of the audit committee. The chairman of the administrative board or one of his deputies led the assembly.[39] The assembly had to vote, among other things, on the following points:

- the establishment of branch offices,
- taking out bonds,
- compensation for the members of the audit committee and the administrative board,
- charter amendment,
- issuance of preference shares,
- discontinuation of the corporation of merger with another.

The administrative board consisted of a minimum of 21 and a maximum of 35 members. The Reich Chancellor named three members, one member was sent by the royal general directorate of the Sea Trading Company. These members were not allowed to be involved in the corporation's activities. The remaining members were elected from the members of the corporation by members of the administrative board for a five-year term. At the first meeting after the ordinary assembly, the administrative board named its chairperson. He was in charge of supervising the entire administration in all branches of the Company.[40]

The board was composed of two or more members, who were elected by the administrative board and could be fired by it at any time. It represented the Company in all transactions and such cases in which the law required a legal mandate. The board was responsible for the hiring and firing of employees of the corporation, who were also subject to its supervision.

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[39] *Kurtze*, Bruno, Die Deutsch-Ostafrikanische Gesellschaft. Ein Beitrag zum Problem der Schutzbriefgesellschaften und zur Geschichte Deutsch-Ostafrikas, Jena 1913; *Ring*, Viktor, Deutsche Kolonialgesellschaften: Betrachtungen und Vorschläge nebst einem Anhang enthaltend die Statuten der Deutschen Kolonial-Gesellschaft für Südwest-Afrika, der Neu Guinea Compagnie und der Deutsch-Ostafrikanischen Gesellschaft, Berlin 1887.

[40] *Ballhaus*, Jolanda, Wesen und Charakter der kolonialen Landgesellschaften Ende des 19. Jahrhunderts, in: Jahrbuch für Wirtschaftsgeschichte 13 (1972), p. 95 ff.; *Fichtner*, Axel, Die völker- und staatsrechtliche Stellung der deutschen Kolonialgesellschaften des 19. Jahrhunderts, Frankfurt a.M. 2002.

Three members of the Company who were not allowed to be members of the board of the administrative board were elected to a three-year term as auditors. These supervised compliance with the charter and were allowed to demand access at any time to the correspondence and the accounting of the corporation.

The corporation itself was subject to supervision by a commissioner nominated by the Reich Chancellor, who was entitled to participate in meetings of the administrative board and the general assembly, as well as access to the documents of the Company.[41]

An agreement with the Sultan of Zanzibar on the expansion of the rights was thwarted by an uprising, starting on August 15, 1888 by the Arabs against the administration installed by the German East Africa Company. Only the efforts of the troops under Reich Commissioner Wissmann was able to defeat the uprising. Not least due to the Helgoland-Zanzibar-Agreement, the Company was forced to concede its sovereignty rights to the German Reich on January 1, 1891. After a loan of over 10 million Marks, making the Corporation dependent on big business money, as well as yearly payments of 600,000 Marks for the concession on customs, the development of a colony by construction of the Usambara Railway was made possible. In contrast, the corporation was obliged to pay the Sultan of Zanzibar an amount of 4 million Mark for granting the coastal areas to the German Reich. In return, the German East Africa Company was granted the monopoly rights for further exploitation of German East Africa.

As a commercial venture, the German East Africa Company survived both wars. Since the beginning of the 1970s, the corporation has not undertaken any commercial activities independently, functioning rather as a holding Company. In 1975, a rebranding of the former colonial Company into the *DOAG Warenhandels-AG* took place. In June 2001, the Company merged with the *Vereinigte Deutsche Nickel-Werke* (VDN AG), the *Hindrichs-Auffermann AG* and the *Langbein Pfanhauser Werke AG* into the 'Vereinigte Deutsche Nicke-Werke AG'.[42]

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41 *Ring*, Viktor, Deutsche Kolonialgesellschaften: Betrachtungen und Vorschläge nebst einem Anhang enthaltend die Statuten der Deutschen Kolonial-Gesellschaft für Südwest-Afrika, der Neu Guinea Compagnie und der Deutsch-Ostafrikanischen Gesellschaft, Berlin 1887.

V. The German New Guinea Company

The German New Guinea Company emerged from a consortium that had undertaken several expeditions to New Guinea in 1884-85 under the direction of economic privy councillor Adolph von Hansemann in Spring 1885 in Berlin. Its goal was to establish a state with its own sovereign rights in the Southern Pacific under the protection of the German Reich. The Company was led by renown members of big businesses and trading companies, such as Oppenheim, Hammacher, Guido, Heckel von Stolberg-Wernigerode, the Duke of Ujest, W. v. Siemens and Adolph Woermann. The more or less coercive annexation of the Hamburg Robertson + Hernsheim company to the Company gave it access to the protection letter dated May 17, 1885, and with that, the sovereign rights over New Guinea and the Bismarck Archipelago, as well as the newly won islands of the Solomon Islands. The area of the company's activities was spread over an area of roughly 240,000 square kilometres.[43]

Over time, it became clear that the company was not equipped for the duties and burdens entailed in administering sovereignty. For this reason, the directorate commenced negotiations regarding the administration of the protectorate by the Reich with the Reich government. In May 1889, the Reich temporarily took over the administration until September 1892, but the costs were still carried by the company. After investments had reached 7 million Mark in 1893 and the reports still only showed losses, administration of the Bismarck Archipelago – the only successful company venture – was transferred to the Reich in April 1895. On April 1, 1899, the Reich also took control of Kaiser-Wilhelmsland. For a compensation of 4 million Mark, payable in ten annual instalments of 400,000 Mark, and the grant of around 150,00 hectares of land, the company lost its sovereign rights, meaning that from then onwards, the Reich was responsible for the costs of the New Guinea Company. The company thus became a purely private acquisition company, which trans-

[42] *Köhler*, Dieter, Zur Situation der deutschen Kolonialgesellschaften, in: Das Wertpapier 1960, p. 782 ff.; *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977, p. 21 ff.

[43] Roman J. Foster, Komine and Tanga: A Note on Writing the History of German New Guinea, in: Journal of Pacific History, Band 22, Nr. 1 (1987), p. 56-64.

formed itself into a Reich Colonial Company on March 2, 1900, following approval by the *Bundesrat*.[44]

The directorate was replaced with a board and an administrative board. The Company's task was now to colonise Kaiser-Wilhelmsland and the Bismarck Archipelago, especially to support the local businesses in agriculture and plantation and the local trading companies.

The central trading office was established in Rabaul, where the main storage facility and the central management administration were also located. The head office in Herbertshöhe sent plantation workers to the Bismarck-Archipelago. Activities in Kaiser-Wihelmsland were supervised from Friedrich-Wilhelmshafen and Stephansort. The trading centre for Kaiser-Wilhelmsland was located in Friedrich-Wilhelmshafen, where the company managed a large shop.

To attract workers and ensure the smooth running of traffic, the German New Guinea Company managed its own shipping line.

After the possessions of the New Guinea Company were confiscated by the Australian authorities in 1920, it searched for new business opportunities in Venezuela and Cameroon.

VI. Postwar Politics

In post-war politics, the former German colonies played hardly any role. However, some West German politicians called for the assumption of late or post-colonial tasks, such as the fiduciary administration of Tanganyika and Togo. Even within the scope of the African freedom movement, single suggestions were made in the context of decolonization. At the end of 1952, representatives of Ewe proposed in a memorandum to the United Nations trust council that Germany should reunite the land administered by Great Britain and France and lead them to independence. The initiative was not taken up. Adolf Friedrich zu Mecklenburg, Togo's last German governor, took part in the celebration of independence in 1960 as honorary

[44] *Wackerbeck*, Lothar, Die deutschen Kolonialgesellschaften: Ihre Entstehung, Entwicklung und Sonderstellung im Gesellschaftsrecht, Münster 1977; *Hartmann*, Die neue Form von Überseegesellschaften, in: Verhandlungen des deutschen Kolonialkongresses 1924, p. 91 ff.; *von Bornhaupt*, Chr., Zum Artikel des Professors Freiherrn von Stengel: Die Konzession der deutschen Kolonialgesellschaften und die Landfrage in den deutschen Schutzgebieten, in: Zeitschrift für Kolonialpolitik, Kolonialrecht und Kolonialwirtschaft 1904, p. 559 ff.

guest. Efforts to revive the colonial warrior-federation after the Second World War led in 1955 in Hamburg to the establishment of the "association of former colonial troops", from which the still existing "traditional association of former protection and Überseetruppen" emerged. The last remnants of the "Schutzgebiets" legislation lasted until the legal expiry of the "colonial societies" in 1975 and tax adjustments in 1992.

Laudatio Friedrich Stadler

Erik Weber

Friedrich Stadler is full professor emeritus in history and philosophy of science at the University of Vienna, with a joint appointment at the Department of Philosophy and the Department of Contemporary History. He is president of the Austrian Ludwig Wittgenstein Society and Head of the Institute Vienna Circle (Institut Wiener Kreis) at his home university.

My professional contacts with Friedrich Stadler are partly connected to this latter position. Since 2001 the Institute Vienna Circle organises one of the most important summer schools in philosophy of science: the "Vienna International Summer University – Scientific World Conceptions" (VISU), from 2014 on known as the "University of Vienna Summer School – Scientific World Conceptions" (USS-SWC). Friedrich Stadler is the main organizer and scientific director of this summer school series. Over the last decade, I have sent him many letters of recommendation for doctoral students or postdoctoral researchers who wanted to participate in this event.

Friedrich Stadler was one of the founding members of the European Philosophy of Science Association (EPSA), which was established in Madrid in 2007. He served as president of this society from 2009 till 2013.

These positions show that Friedrich Stadler is very dedicated to the community of historians and philosophers of science, and that he is a very capable organiser. But that is not the main reason why we award him the Sarton Medal. He receives the medal primarily for his work on the history of philosophy of science, especially the Wiener Kreis and its predecessors.

In 1982 his first important monograph was published: *Vom Positivismus zur 'Wissenschaftlichen Weltauffassung'. Am Beispiel der Wirkungs-*

geschichte von Ernst Mach in Österreich von 1895 bis 1934 (Wien-München: Löcker). His second important monograph appeared in 1997. The original German title was *Studien zum Wiener Kreis. Ursprung, Entwicklung und Wirkung des Logischen Empirismus im Kontext* (Frankfurt/Main: Suhrkamp). It was translated into English and published as *The Vienna Circle – Studies in the Origins, Development, and Influence of Logical Empiricism* in 2001 (Wien-New York: Springer). Both versions of the book were published again in 2015 (Dordrecht: Springer).

Friedrich Stadler has been series (co-)editor of several important book series, which give historians of philosophy access to important primary sources:

- *Ernst Mach Studienausgabe* (Berlin: Xenomoi Verlag)
- *Moritz Schlick Gesamtausgabe* (Wien-New York: Springer)
- *Wiener Kreis – Schriften zum Logischen Empirismus* (Frankfurt/Main: Suhrkamp)
- *The Vienna Circle Institute Yearbook* (Dordrecht: Springer)
- *Veröffentlichungen des Instituts Wiener Kreis* (Dordrecht: Springer)

In sum, Friedrich Stadler has done important original research himself and has made large efforts to make primary sources available to other scholars. Because of this combination the Sarton Medal which he has received is very well deserved.

George Sarton, Ernst Mach, and the Unity of Science Movement

A Case Study in History and Philosophy of Science

Friedrich Stadler

Abstract

During his study years in Ghent the young scientist George Sarton read Ernst Mach's main books *Mechanics, Popular Scientific Lectures,* and *Knowledge and Error* with a strong appreciation, which continued after his emigration to the US in 1915 till the end of his life. Both scholars shared the historical-genetic perspective in the sciences and preferred the evolutionary approach in the emerging interdisciplinary field of history and philosophy of science, accompanied by a similar skepticism towards academic philosophy. A common theoretical frame can be identified with the complex concepts of "positivism", "conventionalism" and "pragmatism", aiming at a unity of the sciences as a regulative principle and research program.

This idea was revived and further developed in the 1930s by the Vienna Circle of Logical Empiricism, in exile from 1934 on. That enterprise was mainly organized by Otto Neurath, together with Rudolf Carnap, Philipp Frank, Charles Morris and others with six "International Congresses for the Unity of Science" (1935-41) and with the unfinished huge publication project *International Encyclopedia of Unified Science* since 1938 in the tradition of the French encyclopedists and European Enlightenment. Sarton was invited to contribute a monograph on the history of science, which did not work out because of his workload. But despite his commitments and own ambitious projects he supported the Unity of Science movement, both as a speaker at the 5th Congress in Harvard and as editor of *Isis*. In parallel, he met and corresponded with the main proponents of the former Vienna Circle.

The paper explores this hardly known communication between two related scientific movements and analyzes their common fate as mainly incomplete and unfinished projects in the Cold War period. Finally, some reasons for their relative decline in the postmodern age are sketched.

Introductory Remark

There are good reasons to focus on the local hero of Ghent, the pioneer and founder of history of science as an academic discipline, along with the still existing periodicals *Isis* and *Osiris*. The article draws on a story which is not so well known yet, namely the history and fate of two long-term projects in history and philosophy of science. While dealing with this fascinating topic I discovered the roots of my own research field and was deeply impressed by the achievements of the protagonists of this adventurous enterprise.

Luckily, for the first part of my paper we can also refer to the unique biography of Lewis Pyenson, *The Passion of George Sarton. A Modern Marriage and its Discipline* (2007), a fascinating story of George Sarton and his wife, the artist Mabel Elwes, which unfolds up to the 1920s. The second part is based mainly on my own studies on Mach, Logical Empiricism and the Vienna Circle (1982, 1988, 2015) as well as recent historiography on the Unity of Science movement in the context of the Institute Vienna Circle.

1. Mach and Sarton – An intellectual family resemblance between history and philosophy of science

In 1916, when the scientist and philosopher Ernst Mach (1838-1916)[1] died near Munich at the age of 78 years, George Sarton was a young 32-year old promising scholar already living in the US. At this time Mach was both famous and a contested scholar (in his own words "*Naturforscher*") in the scientific community. He was nominated for the Nobel Prize, and was highly appreciated by many physicists like Albert Einstein, politicians like Friedrich Adler, and writers like Robert Musil. After studying in Vienna he

[1] On the life and work of Mach: Heller 1964; Blackmore 1972; Wolters 1978; Stadler 1982; Haller/Stadler 1988; Hoffmann/Laitko 1991; Stadler 2018.

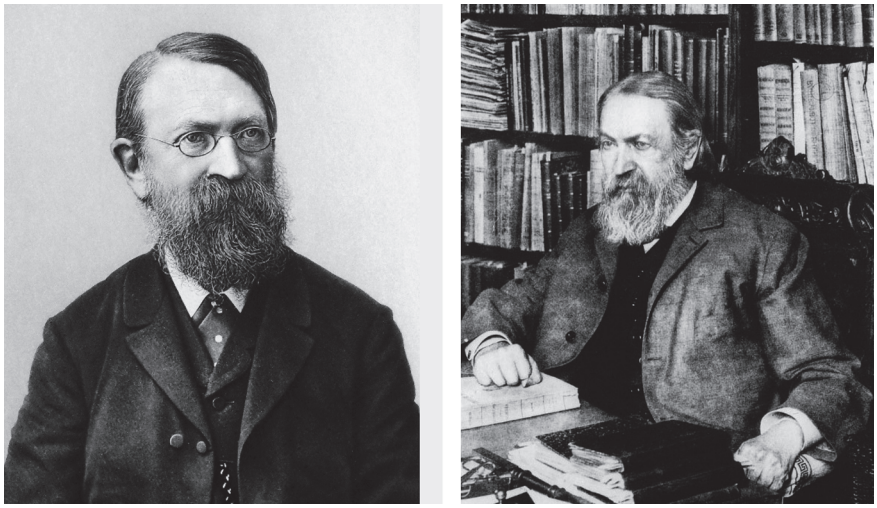


Fig. 1: Ernst Mach (1838-1916)
Source: Mach, *Knowledge and Error.* Dordrecht-Boston: Reidel 1976

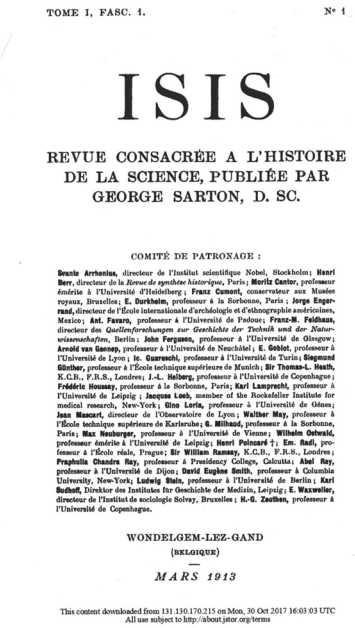
began his academic career as a professor of mathematics and physics in Graz. He went on with taking over the chair of experimental physics in Prague, and, in 1895 he became Professor of philosophy ("philosophy, esp. history and theory of the inductive sciences") at the University of Vienna. Although he suffered a stroke in 1897 which resulted in his early retirement in 1901, he paved the way for the subsequent Vienna Circle of Logical Empiricism, esp. for the so-called "first Vienna Circle" before WW I (around Hans Hahn, Philipp Frank, and Otto Neurath). During the First Austrian Republic the "Ernst Mach Society" (*"Verein Ernst Mach",* 1928-1934), the society for the popularization of scientific world conception of the Vienna Circle, founded by Moritz Schlick, was named after him. The society remained active till its forced dissolution following the Civil War in 1934. One of Mach's most important goals and achievements was his emphasis on the history of science for each discipline and the sciences in general as well as his skepticism towards any metaphysical "school philosophy" (a term coined by Philipp Frank) as an academic armchair doctrine. In this respect he can certainly be seen as a pioneer of the emergence of the present interdisciplinary field History and Philosophy of Science (HPS).[2]

[2] Stadler 2012, 2014

Mach underlined the historical embeddedness of all natural sciences and presented "Clio" as the role model for research: *"Let us not let go the guiding hand of history. History has made all; history can alter all. Let us expect from history all, …"*.[3]

Mach's book titles reflect his programmatic intention, and his lecture courses in Vienna nicely confirm his preference for a genetic methodology (*Historisch-kritische Methode*). This exemplary integration of history and philosophy in the sciences also became apparent in the wide interdisciplinary reception of his writings. But Mach was not a naïve "inductivist" or crude "positivist" (a concept which he did not use to characterize his theories): he combined a fallibilistic epistemology with a monistic method and tried to overcome the dualism of context of discovery and context of justification as he stated explicitly in his late book *Knowledge and Error*

GEORGE SARTON
(1884–1956)



TOME I, FASC. 1. N° 1

ISIS

REVUE CONSACRÉE A L'HISTOIRE DE LA SCIENCE, PUBLIÉE PAR GEORGE SARTON, D. SC.

COMITÉ DE PATRONAGE :

Svante Arrhenius, directeur de l'Institut scientifique Nobel, Stockholm; Henri Berr, directeur de la *Revue de synthèse historique*, Paris; Moritz Cantor, professeur émérite à l'Université d'Heidelberg; Franz Cumont, conservateur aux Musées royaux, Bruxelles; E. Durkheim, professeur à la Sorbonne, Paris; Jorge Engerrand, directeur de l'École internationale d'archéologie et d'ethnographie américaines, Mexico; Ant. Favaro, professeur à l'Université de Padoue; Franz-M. Feldhaus, directeur des *Quellenforschungen zur Geschichte der Technik und der Naturwissenschaften*, Berlin; John Ferguson, professeur à l'Université de Glasgow; Arnold van Gennep, professeur à l'Université de Neuchâtel; E. Gobiet, professeur à l'Université de Lyon; Io. Guareschi, professeur à l'Université de Turin; Siegmund Günther, professeur à l'École technique supérieure de Munich; Sir Thomas-L. Heath, K.C.B., F.R.S., Londres; J.-L. Heiberg, professeur à l'Université de Copenhague; Frédéric Houssay, professeur à la Sorbonne, Paris; Karl Lamprecht, professeur à l'Université de Leipzig; Jacques Loeb, member of the Rockefeller Institute for medical research, New-York; Gino Loria, professeur à l'Université de Gênes; Jean Mascart, directeur de l'Observatoire de Lyon; Walther May, professeur à l'École technique supérieure de Karlsruhe; G. Milhaud, professeur à la Sorbonne, Paris; Max Neuburger, professeur à l'Université de Vienne; Wilhelm Ostwald, professeur émérite à l'Université de Leipzig; Henri Pelnard †; Em. Radl, professeur à l'École réale, Prague; Sir William Ramsay, K.C.B., F.R.S., Londres; Prafhulla Chandra Ray, professeur à Presidency College, Calcutta; Abel Ray, professeur à l'Université de Dijon; David Eugène Smith, professeur à Columbia University, New-York; Ludwig Stein, professeur à l'Université de Berlin; Karl Sudhoff, Direktor des Institutes für Geschichte der Medizin, Leipzig; E. Waxweiler, directeur de l'Institut de sociologie Solvay, Bruxelles; H.-G. Zeuthen, professeur à l'Université de Copenhague.

WONDELGEM-LEZ-GAND
(BELGIQUE)

MARS 1913

Fig.2: George Sarton (1884-1956) and the first issue of *Isis*
Source: Sarton, *Das Studium der Geschichte der Naturwissenschaften*. Klostermann, Frankfurt am Main, 1965; Sarton, *A Guide to the History of Science*. Waltham, Mass.1952

[3] Mach 1872, pp. 3-4; Mach 1911, p. 18.

(1905): *"Abstraction and the activity of phantasy does the main work in the finding of new knowledge."*.[4] Consequently, for Mach the term "inductive sciences" is not representative for the natural sciences at all. Given this approach one may characterize Mach with the aim of bringing together history and philosophy with a strong preference for the history and thereby paving the way for a "historical turn" in the philosophy of science (currently represented by the professional organisations HOPOS and &HPS). At the same time Mach anticipated a prototype of contemporary "historical epistemology".

A few generations later George Sarton (1884-1956)[5] was to study philosophy, mathematics, and chemistry in Ghent before emigrating via England to the US in 1915. I don't think he ever personally met Ernst Mach, who for health reasons had moved away from Vienna to his son Ludwig near Munich in 1913, where he lived rather isolated from scholarly life and political events. Nevertheless, till the end of his life he remained a pacifist and a convinced supporter of Austrian Social Democracy. He tried to continue his research together with his son, with whom he could was able to publish his last small book entitled *Kultur und Mechanik* (Culture and Mechanics) in 1915.

It is remarkable that the young Sarton, in a letter written in May 2, 1912, invited Mach to join the editorial board of the journal *Isis*, which he founded one year later and of which he became the editor. Unfortunately, we did not find a response to this letter so far, as it is most likely that Mach did not reply because of his bad health condition and his move from Vienna to Munich in 1913. This handwritten letter (Sarton wrote to Mach in French), reads as follows – in English translation:[6]

Wondelgem, May 2, 1912

Sir,

I send you with the same letter my project and foundation of a new Journal about the history of science. I beseech you to read it with attention, and I

4 Mach 1905, p.236

5 On the life and work of Sarton: Pyenson 2007; Pyenson/Verbruggen 2009.

6 Sarton to Mach (May 2, 2012), Ernst Mach Archiv, Deutsches Museum München. Thanks to Deutsches Museum for the permission to quote and to Klaus Hentschel (Stuttgart) for providing the letter in advance.

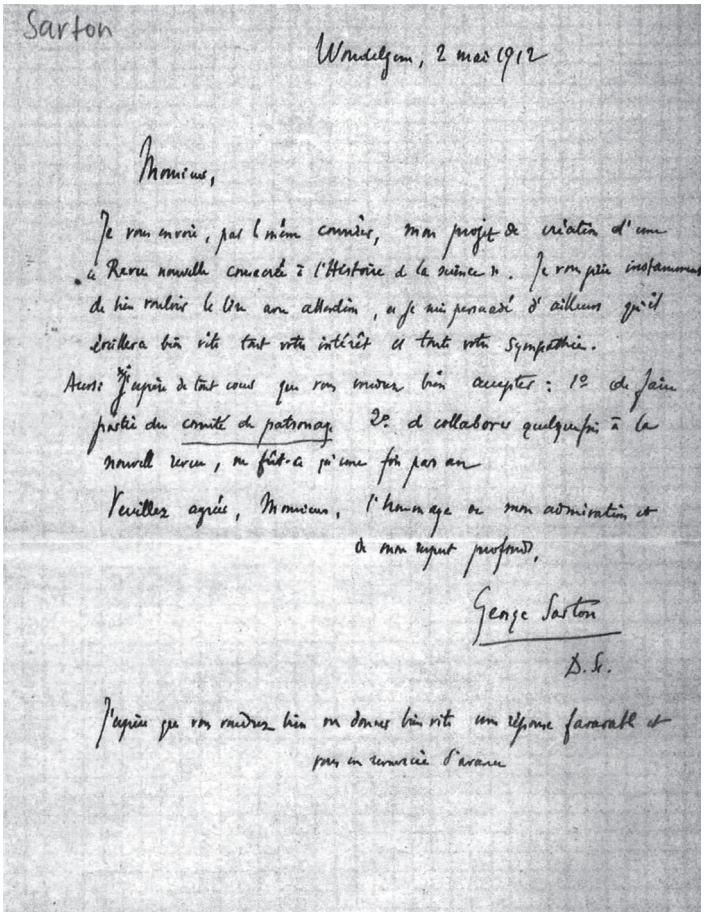
am convinced that it will rapidly raise your interest and your sympathies. Also, I hope cordially that you will accept: First, to be a member of the advisory committee; second, that you will collaborate occasionally with the new journal, may it be only once a year.

Dear Sir, please accept my admiration and my professional respect,

George Sarton

D. Sc.

I hope that you will send me rapidly a positive response and I thank you for it in advance.


Sarton

Wondelgem, 2 mai 1912

Monsieur,

Je vous envoie, par le même courrier, mon projet de création d'une « Revue nouvelle consacrée à l'Histoire de la Science ». Je vous prie instamment de bien vouloir le lire sans attendre, car je suis persuadé d'ailleurs qu'il éveillera bien vite tout votre intérêt et toute votre sympathie.

Aussi j'espère d'ores et déjà que vous voudrez bien accepter : 1° de faire partie du comité de patronage 2° de collaborer quelquefois à la nouvelle revue, ne fût-ce qu'une fois par an.

Veuillez agréer, Monsieur, l'hommage de mon admiration et de mon respect profond,

George Sarton

D. Sc.

J'espère que vous voudrez bien me donner bien vite une réponse favorable et vous en remercie d'avance.

Fig. 3: Letter of Sarton to Mach, May 2, 1912
Source: Mach Archives, Deutsches Museum

Although the project description is not attached in the correspondence, we can imagine that it was the mission statement of *Isis*. This can be drawn from the editorial of the first issue, where Sarton already refers to Mach's books *Mechanics* (*Le mécanique*, 1904), *Knowledge and Error* (*La connaissance et l'erreur*, 1908) and published his programmatic "L'Histoire de la science" as the first introductory article of his new periodical.[7] This early publication history is described in detail by Pyenson.[8]

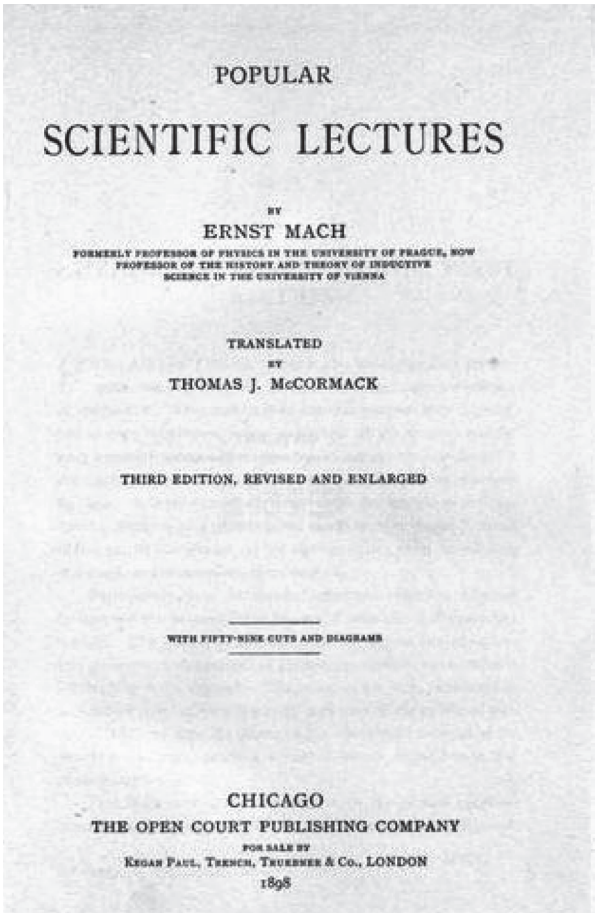
Three years later, the year of Mach's death (1916,) Sarton introduced himself to the US community after having emigrated there with an English version of his French *Isis* article. "The History of Science" appeared in the American journal *The Monist*. Here we already find a reference to Mach's method which inspired his own genetic-historical methodology.

This publication is another remarkable coincidence, because the journals *Open Court* and *The Monist* were founded and edited by German-born Paul Carus and Edward C. Hegeler, both of whom Sarton came into contact with, so that in these periodicals *Isis* was announced from the beginning, mediated by the mathematician and successor of Carus, P.E.B. Jourdain. According to Sarton *Isis* and the *Monist* had identical aims.[9]

In addition, both journals offered a forum for the early dissemination and popularization of Mach's ideas in North America.[10] His *Populär-Wissenschaftliche Vorlesungen* were published first in English as *Popular Scientific Lectures* (PSL) in 1895 with Open Court (Chicago), before they appeared in German in 1896 (Johann Ambrosius Barth).

This anthology was indicative of Mach's efforts to present his ideas since the beginning of his academic career, as evident in his inaugural lecture in Vienna 1895 ("Über den Einfluss zufälliger Umstände auf die Entwicklung von Erfindungen und Entdeckungen" / "On the Part Played by Accident in Invention and Discovery"). The first 12 articles were translated by Thomas J. McCormack (who also did the translation of Mach's *Mechanics*) at the invitation of Paul Carus (1852-1919), who had been in close personal contact with Mach since 1888.[11] Together with Edward C. Hegeler, Carus

7 *Isis. Revue Consacrée a l'histoire de la science. Publiée par George Sarton, D.Sc.*
8 Pyenson 2007, pp. 186-189.
9 Pyenson 2007, pp. 196 f.
10 Stadler 2015 and 2017, Holton 1992.
11 Thiele 1978, pp. 177-185.


POPULAR

SCIENTIFIC LECTURES

BY

ERNST MACH

FORMERLY PROFESSOR OF PHYSICS IN THE UNIVERSITY OF PRAGUE, NOW PROFESSOR OF THE HISTORY AND THEORY OF INDUCTIVE SCIENCE IN THE UNIVERSITY OF VIENNA

TRANSLATED

BY

THOMAS J. McCORMACK

THIRD EDITION, REVISED AND ENLARGED

WITH FIFTY-NINE CUTS AND DIAGRAMS

CHICAGO

THE OPEN COURT PUBLISHING COMPANY

FOR SALE BY

KEGAN PAUL, TRENCH, TRÜBNER & CO., LONDON

1898

Fig 4: Title Page of Mach's *Popular Scientific Lectures* (1895), 3rd edition
Source: Institute Vienna Circle

had founded the publishing house which edited the journals *The Open Court* and *The Monist*. Mach corresponded with both editors via 130 letters till the end of his life. Carus himself contributed an article on "Professor Mach's Philosophy" (1906) to his journal, where Mach had already been present as an author since the 1890s so that it is not surprising that his books and lectures had been translated into English since the end of the 19th century. Actually, the articles included in the *PSL* had been first published in the two above-mentioned journals, where Mach had euphorically declared:[12]

[12] Mach 1890, quoted after Holton 1993, p. 5.

> "*The time seems ripe for the overthrow of all metaphysical philosophies. I contribute this article to your magazine in the confidence that America is the place where new views will be most fully developed*."

In Mach's preface to the first English edition of his *PSL* in 1895, he refers to the poetry of research as well as to the connection between everyday life and science, which re-appears in his last publication *Kultur und Mechanik* (Culture and Mechanics, 1915, Reprint 2015). This reflects Mach's life-long commitment to the popularization of science, which he himself promoted with his university lectures for the public based on his conviction that scientific knowledge is crucial for the promoting a humanistic attitude in the spirit of the European Enlightenment. Even in the foreword to the fourth expanded edition of the *PSC* he wrote that "he had not lost the inclination to engage with the public on questions of general interest."[13] It is obvious that the young Sarton had been attracted by both the *PSL* (1895) and *Knowledge and Error* (1905) for several reasons. In his *Monist* article on the history of science he wrote in 2016:[14]

> "*The history of science is the study of the development of science – just as one studies the development of a plant or an animal – from its very birth. We try to see it grow and unfold itself under many diverse conditions. And it is not enough … to study separately the development of each science; one has to study the development of all sciences together. Besides, it is impossible to separate them satisfactorily one from the other; they grow together and mingle continually in innumerable ways*."[15]

Here we recognize a clear emphasis of the genetic unity of the sciences for historiography as a methodological presupposition and basic principle. And Sarton goes on to ask why the history of science, being such a general topic, had not been written so far. In his view the reluctance of laymen towards science can be overcome by adopting a specific historical position as an approximation, and this also holds also for the experts in the field. Dealing with the relation of *science* and *philosophy* Sarton continues to address the growing complexity of science with its division of labor as a danger for science itself but also for social life proper. In order to explain

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13 Mach 1895, p. xiv.
14 *The Monist*, XXVI, July, 1916, Nr.3, pp.321-365.
15 Ibid., p. 321 f.

the need for a balance between *analysis* and *synthesis* he refers to the relation of science and philosophy as a mutual interaction since ancient times and since the origin of modern science (e.g., Galilei, Kepler, Newton, Darwin on the one side and Descartes, Leibniz, Kant, on the other). Referring in particular to the history of the natural sciences since the end of the 19th century Sarton illustrates a strong exchange between science and philosophy, but he proposes a sort of "holistic" view on this development covering the "positivist schools" of Comte and Spencer which had influenced the sciences. This, Sarton argues, also prompted the emergence of the pragmatist movement (H. Bergson, W. James, F.C.S. Schiller) arguing for the independence of philosophy from the sciences via intuition.

Sarton proposes to bridge the gap between "neo-positivists and pragmatists" with the need for thinking *and* acting.[16] (This, by the way, converges nicely with the "hidden pragmatism" of Mach and the later convergence of Logical Empiricism and pragmatism). The common agenda of those two currents are therefore the study of the principles and the history of science with a focus on scientific "generalities" in the tradition of Comte:

"To secure the unity of knowledge it will be more and more necessary that some men make a deep study of the principles and of the historical and logical development of all the sciences",[17] which presupposes the collaboration of philosophers, historians, and scientists. And Sarton summarizes that the *"best instrument of synthesis, and the most natural hyphen between scientist and philosopher is the history of science."* (Sarton, ibid.).[18]

We can see that this explicit endorsement of a general history of science as a whole based on Auguste Comte and his follower, the historian of mathematics and science Paul Tannery (1843-1904) certainly resembles Mach, and at the same time the unity of science movement of the 1930s, in which Sarton himself was active at its periphery as we shall see later on. In addition, Sarton emphasizes the psycho-sociological point of view:[19]

> "*In short, the purpose of the history of science, as I understand it, is to establish the genesis and the development of scientific facts and ideas, taking into account all intellectual exchanges and all influences brought*

16 Sarton, ibid., p. 328
17 Ibid., p.330.
18 Ibid.
19 Ibid., p. 333.

into play by the very progress of civilization. It is indeed a history of human civilization, considered from its highest point of view. The center of interest is the evolution of science, but general history remains always in the background."

This programmatic paragraph nicely manifests once again the family resemblance with Mach, to whom Sarton refers several times in his article, which in the long run served as a guiding manual for his lifework. One reasonable classification of the history of science according to Sarton would be with time periods covering (1) the history of civilization, (2) the history of technology, (3) the history of religions, and (4) the history of fine arts and crafts.[20]

This list prompts another comparison with the philosopher and historian of science Edgar Zilsel, who worked in his US exile from 1939 ff. on the origins of modern science in Europe. Zilsel also personally met Sarton at the margins of the Unity of Science movement.

It should also be mentioned that Sarton at the same time had challenged the naïve conception of the history of science as a mere linear development when he acknowledged breaks, interruptions, discontinuities and hindrances for any scientific progress including the history of error, as featured in Mach's book title *Knowledge and Error*. Even if he argues that the history of truth is primarily to be illustrated by the history of medicine and technology, he anticipates the symmetry principle (balance of truth – falsity) as proposed in postmodern historiography. Consequently, all scientists should be committed to *both* the history of their own discipline *and* the general history of science. This is expressed by Sarton with a reference to Mach's *Science of Mechanics* (1902) in the following passage on the relation of scientists and historians of science:[21]

"But in my opinion, however important its heuristical value may be, there are still deeper reasons why the scientist should give his attention to the history of science. I am thinking of those which have been so splendidly illustrated by Ernst Mach in his Mechanics*. For one thing, it is obvious that 'they that know the entire course of the development of science will, as a matter of course, judge more freely and more correctly of the significance*

[20] Ibid., 334 f.
[21] Ibid., p.348 f.

> *of any present scientific movement than they who, limited in their views to the age in which their own lives have been spent, contemplate merely the momentary trend that the course of intellectual events takes at the present moment.' In other words, to understand and to appraise at its just value what one possesses, it is well to know what the people possessed who came before us; this is as true in the domain of science as it is in daily life. It is his historical knowledge that discloses to the scientist his precise attitude toward the problems with which he has to grapple, and that enables him to dominate them."*.

I think, this is passage clearly endorses the historicization of knowledge and science as a methodology, which could also be associated with German *Historismus* (historism) since the 19th century without the idea of a rational progress towards truth in the long run being lost. Additionally, this plea helps to avoid "the worst kind of metaphysics – scientific idolatry."[22]

At the end of his manifesto Sarton introduces some specific perspectives on science and its history: from a *pedagogical point of view* this implies history and philosophy of science being part of science teaching at schools and at universities. Again, he praises Mach for having published excellent textbooks in this field, with the latter having presented the genetic method of learning as opposed to mere abstract top-down procedures. *The psychologic(al) and sociologic(al) points of view* address the contested German historian Karl Lamprecht as the pioneer for a (law-like) universal history, the history of science being based on psycho-sociological investigations with psychological and material causes for the development of science. (This, by the way, alludes to the so-called *Methodenstreit* at the turn of the century 1900 with Lamprecht vs. the dominant German school of historism, which led up to the dualism of *Erklären* (explanation) und *Verstehen* (understanding/intuition) in historiography. But this is another story to be told in context of methodological debates up to the "science wars").

Sarton recommends the comparative method as the most appropriate one inferred from his approach. Last but not least, *the humanistic point of view* opens up a new "humanism", (strikingly anticipating some elements of Carnap's late claim for a "scientific humanism"):

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22 Ibid., p.350.

> "*As Mach has perfectly put it: 'Science has undertaken to replace wavering and unconscious adaptation, by a methodical adaptation, quicker and decidedly conscious.' It is the historian's duty to evidence all the scientific facts and ideas that make for peace and civilization; in this way he will better secure science's cultural function.*"

And Sarton argues here in favor of focusing on global science so as to overcome a dominant Euro-centric perspective. Only the collaboration of *scientists, philosophers, and historians* can guarantee a synthesis of this new humanism as a collective work in progress. And he concludes consequently:[23]

> "*The writer is convinced that the history of science – that is to say, the history of human thought and civilization in its broadest form – is the indispensable basis of any philosophy – History is but a method – not an aim.*"

(The Appendix dealing with the teaching of the history of science in the US seems to me to be the first account describing the universities in North America with some very few references to European institutions like the history of medicine in Leipzig and in Vienna, as well as the history of mathematics in Munich and Heidelberg).

These striking ideas will become relevant in the context of the science-teaching programs at Harvard University, to which both Sarton and Philipp Frank whould contribute some 25 years later.

Here, the early strong reception of Mach's oeuvre becomes obvious: Sarton's reading of *Knowledge and Errror*, of the *PSL*, and the common theoretical framing of French "positivism" and American "pragmatism" for the emerging field of HPS with its preference for *History* of Science over any normative and metaphysical *Philosophy* of Science. On both sides we note a sympathetic but also critical reflection on Duhem, Poincaré on the one side, and Peirce, James, and Dewey, on the other – which reappears with some variations again in the *International Encyclopedia of Unified Science* (IEUS) of the late 1930s.

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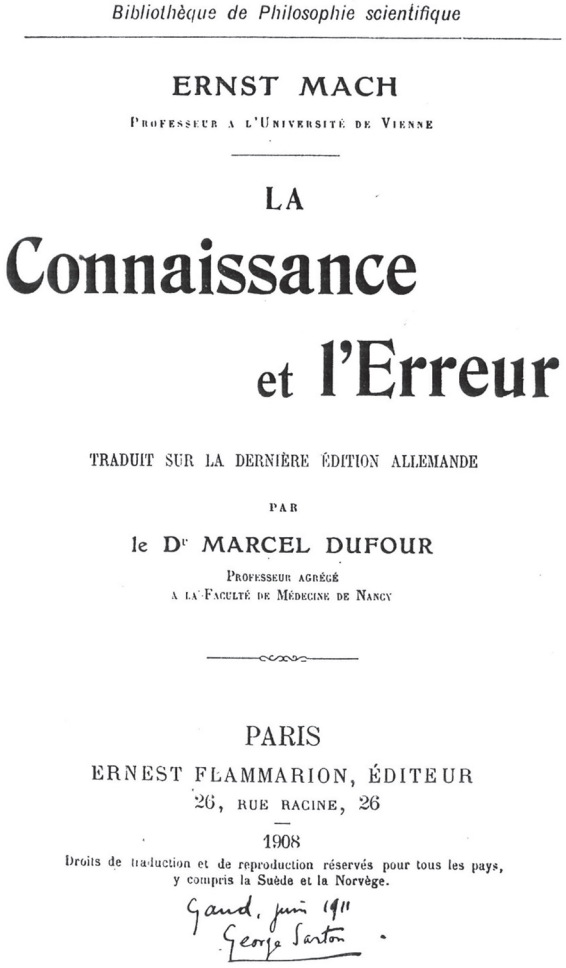
[23] Sarton, ibid., p.361



Bibliothèque de Philosophie scientifique

ERNST MACH

PROFESSEUR A L'UNIVERSITÉ DE VIENNE

LA

Connaissance

et l'Erreur

TRADUIT SUR LA DERNIÈRE ÉDITION ALLEMANDE

PAR

le Dr MARCEL DUFOUR

PROFESSEUR AGRÉGÉ
A LA FACULTÉ DE MÉDECINE DE NANCY

PARIS

ERNEST FLAMMARION, ÉDITEUR

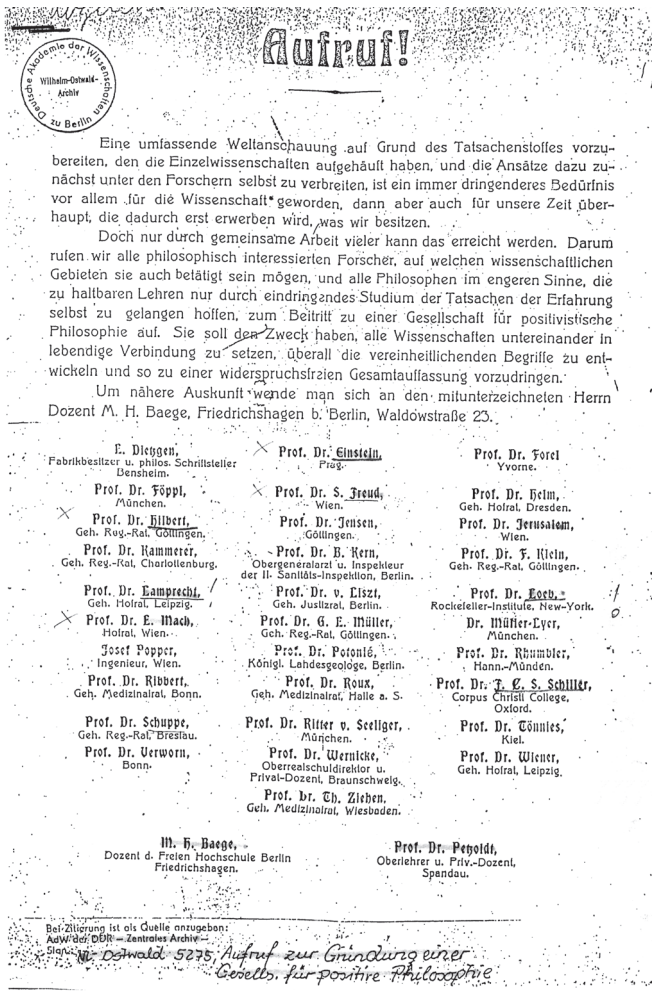
26, RUE RACINE, 26

1908

Gand, juin 1911
George Sarton

Fig. 5: French edition of Ernst Mach's *Erkenntnis und Irrtum / Knowledge and Error*
With the signature of George Sarton
Source: Mach, Knowledge and Error (Dordrecht-Boston: Reidel 1976)

Already in 1913 Sarton had reprinted the public call for the founding of a "Gesellschaft für positivistische Philosophie" (Society for Positivistic Philosophy) in Berlin in *Isis*, which was issued in 1911/1912. It had been signed by an impressive list of philosophers and scientists like Hilbert, Lamprecht, Mach, Einstein, Freud, Jerusalem, F.C.S. Schiller, and by the American biologist Jacques Loeb. It was published also in *The Journal of Philosophy, Psychology and Scientific Methods* (IX/15, 1912).



Aufruf!

Deutsche Akademie der Wissenschaften zu Berlin
Wilhelm-Ostwald-Archiv

Eine umfassende Weltanschauung auf Grund des Tatsachenstoffes vorzubereiten, den die Einzelwissenschaften aufgehäuft haben, und die Ansätze dazu zunächst unter den Forschern selbst zu verbreiten, ist ein immer dringenderes Bedürfnis vor allem für die Wissenschaft geworden, dann aber auch für unsere Zeit überhaupt, die dadurch erst erwerben wird, was wir besitzen.

Doch nur durch gemeinsame Arbeit vieler kann das erreicht werden. Darum rufen wir alle philosophisch interessierten Forscher, auf welchen wissenschaftlichen Gebieten sie auch betätigt sein mögen, und alle Philosophen im engeren Sinne, die zu haltbaren Lehren nur durch eindringendes Studium der Tatsachen der Erfahrung selbst zu gelangen hoffen, zum Beitritt zu einer Gesellschaft für positivistische Philosophie auf. Sie soll den Zweck haben, alle Wissenschaften untereinander in lebendige Verbindung zu setzen, überall die vereinheitlichenden Begriffe zu entwickeln und so zu einer widerspruchsfreien Gesamtauffassung vorzudringen.

Um nähere Auskunft wende man sich an den mitunterzeichneten Herrn Dozent M. H. Baege, Friedrichshagen b. Berlin, Waldowstraße 23.

E. Dietzgen, Fabrikbesitzer u. philos. Schriftsteller, Bensheim.
Prof. Dr. Einstein, Prag.
Prof. Dr. Forel, Yvorne.
Prof. Dr. Föppl, München.
Prof. Dr. S. Freud, Wien.
Prof. Dr. Helm, Geh. Hofrat, Dresden.
Prof. Dr. Hilbert, Geh. Reg.-Rat, Göttingen.
Prof. Dr. Jensen, Göttingen.
Prof. Dr. Jerusalem, Wien.
Prof. Dr. Kammerer, Geh. Reg.-Rat, Charlottenburg.
Prof. Dr. B. Kern, Obergeneralarzt u. Inspekteur der II. Sanitäts-Inspektion, Berlin.
Prof. Dr. F. Klein, Geh. Reg.-Rat, Göttingen.
Prof. Dr. Lamprecht, Geh. Hofrat, Leipzig.
Prof. Dr. v. Liszt, Geh. Justizrat, Berlin.
Prof. Dr. Loeb, Rockefeller-Institute, New-York.
Prof. Dr. E. Mach, Hofrat, Wien.
Prof. Dr. G. E. Müller, Geh. Reg.-Rat, Göttingen.
Dr. Müller-Lyer, München.
Josef Popper, Ingenieur, Wien.
Prof. Dr. Potonié, Königl. Landesgeologe, Berlin.
Prof. Dr. Rhumbler, Hann.-Münden.
Prof. Dr. Ribbert, Geh. Medizinalrat, Bonn.
Prof. Dr. Roux, Geh. Medizinalrat, Halle a. S.
Prof. Dr. F. C. S. Schiller, Corpus Christi College, Oxford.
Prof. Dr. Schuppe, Geh. Reg.-Rat, Breslau.
Prof. Dr. Ritter v. Seeliger, München.
Prof. Dr. Tönnies, Kiel.
Prof. Dr. Verworn, Bonn.
Prof. Dr. Wernicke, Oberrealschuldirektor u. Privat-Dozent, Braunschweig.
Prof. Dr. Wiener, Geh. Hofrat, Leipzig.
Prof. Dr. Th. Ziehen, Geh. Medizinalrat, Wiesbaden.

M. H. Baege, Dozent d. Freien Hochschule Berlin Friedrichshagen.
Prof. Dr. Petzoldt, Oberlehrer u. Priv.-Dozent, Spandau.

Bei Zitierung ist als Quelle anzugeben: Adw der DDR – Zentrales Archiv –
Sign.: Ostwald 5275; Aufruf zur Gründung einer Gesells. für positive Philosophie

Fig.6: Public Call for the Foundation of the "Gesellschaft für positivistische Philosophie"
Source: Ostwald Archiv, Berlin

This was mentioned already by Carl Hempel, Adolf Grünbaum, and Gerald Holton[24] and can be seen as a precursor of the later "Berlin Society for Empirical (Scientific) Philosophy" associated with Hans Reichenbach and as a proto-type of the famous Vienna Circle Manifesto *Wissenschaftliche Weltauffassung. Der Wiener Kreis* (Scientific World Conception. The

24 Holton 1993, p.52f.

Vienna Circle, 1929).[25] This first manifesto called for a synthesis of the individual sciences based on empirical research as opposed to any metaphysical philosophy and a focus on the unifying concepts that would lead to a unity of science.

It is one of several international attempts at *Pursuing the Unity of Science* in context, as the title of a book indicates from a broader and comparative perspective.[26] The cover page reproduces symbols from Otto Neurath's pictorial statistic movement "Isotype", which he had initiated in "Red Vienna", and continued after his emigration in the Netherlands and Britain as part of the IEUS. In addition to studies on Neurath's visual education in collaboration with the filmmaker Paul Rotha (Timothy Boon) and the fate of the Unity of Science movement in the US during the Cold War period (Peter Galison, George Reisch), this collection also includes an informative contribution entitled "Unifying science and human culture: the promotion of the history of science by George Sarton and Frans Verdoorn" (Bert Theunissen), which is relevant in connection with the *International Encyclopedia of Unified Science,* edited by Carnap, Morris, and Neurath, 1938 ff..[27] I only want to draw attention to the common fate of two ambitious projects inspired by a new scientific humanism in the Cold War period under the sway of an antagonistic *Zeitgeist* after WW II.

Given this intellectual history, it is no coincidence that Sarton had bought and commented on some of Mach's books which are now located in Harvard's Houghton Library. There we can see his signature on the title page of the French edition of Mach's book *Knowledge and Error* (*Erkenntnis und Irrtum* 1905 / *La Connaissance et l'Erreur* 1908) dated June 1911, when Sarton had just finished his dissertation on Newton's mechanics.

Here he added annotations which document his early interest in the author of this book. Erwin Hiebert, the editor of the English edition of Mach's *Knowledge and Error* (1976), does not comment on Sarton's hand-written remarks in his introduction. Only in the German translation of Sarton's small booklet *The Study of the History of Science* (1936) – unfortunately the only published translation into German as far as I know – the editors

[25] Reprint: Uebel/Stadler 2012
[26] Kamminga/Somsen 2016
[27] Carnap/Morris/Neurath 1938 ff.

mention Mach among Sarton's role models together with Auguste Comte and Paul Tannery.[28]

A first look in Sarton's library located in Harvard confirms this. Here we find annotations in the German edition of *Erkenntnis und Irrtum*, respectively in the first two chapters entitled "Philosophisches und naturwissenschaftliches Denken" (Philosophical and Scientific Thought), and "Eine psycho-physiologische Betrachtung" (A Psycho-physiological Consideration), in which Mach presents his criticism of metaphysical philosophy and his monistic mind-body conception.

And there is another significant common interest: both Mach and Sarton appreciated Pierre Duhem and Henri Poincaré as philosophers and historians of science in the tradition of French "positivism" – even if Mach himself never used this term as a self-description. Mach corresponded with Duhem (six letters, two cards) and wrote a favorable preface to the German edition of Duhem's book *La Théorie physique, son objet et sa structure* (1908), which was translated by his follower Friedrich Adler. Already in the preface of his *Knowledge and Error* Mach had expressed his "far-reaching agreement" with Duhem's anti-metaphysical interpretation of physical theories as a conception that complemented his own one, incl. a special reference to the "historical and genetic method of presenting physical theories".[29]

In the first issue of *Isis* Sarton published his obituary of Poincaré with a portrait of the philosopher-scientist he admired so much as another role model for his HPS project, whom he had also contacted regarding the foundation of *Isis* like Ostwald and Mach. Mach also acknowledged Poincaré and commented on his "conventions" in his *Wissenschaft und Hypothese* (Science and Hypothesis, 1904), which he saw as being not arbitrary but rather imposed.[30] And the latter referred to Mach's *Mechanics* in the book I just mentioned and in his *Der Wert der Wissenschaft* (The Value of Science, 1906). All three scientists, Mach, Duhem and Poincaré, were on the agenda of the "First Vienna Circle" before WW I as described by Philipp Frank[31]. The intention was to bring together Mach's empiricism

[28] Sarton 1965.
[29] Symposium on Mach and Duhem, organized by A. Brenner, in: Stadler 2018 (forthcoming)
[30] Mach to Dingler, 26/1/1912, in: Blackmore/Hentschel 1985, p.94f.
[31] Frank 1949, p.11 f.

with French conventionalism – and thereby also counter Lenin's attack on "empirico-criticism" (1909).

Recent research by Hayo Siemsen has uncovered the relevance of Mach's influence on Sarton regarding the genetic theory of learning, science teaching and public understanding of science, even if he criticizes Sarton because of a lack of epistemology and psychology. In his article "Ernst Mach and George Sarton: History of Science as Metapsychical Method" he refers to Sarton's evolutionary view on genesis and development as introduced in the first issue if *Isis* (1913).[32] The common positions cover the genetic epistemology and the historical-critical method ("historisch-kritische Methode") in the history and historiography of science. At the center of this discussion we can find Mach's seminal book *Die Mechanik – Historisch-kritisch dargestellt* (1883), published in English under the title *The Science of Mechanics – A Critical Account of its Development* in 1893. (We know that Sarton had read the *Mechanik* in 1911, the year in which he finished his unpublished dissertation on Newton ("Les Principes de la Mécanique de Newton", 1911).

But Siemsen also points to differences between Sarton and Mach, esp. regarding science teaching and the general method of scientific thinking. Sarton was skeptical of psychological methods as opposed to mere historical ones and missed specific empirical experiments. (Remember the subtitle of Mach's *Knowledge and Error*: "Sketches on the *Psychology* of Enquiry", Skizzen zur Psychologie der Forschung, by the way countered later on by Karl Popper's *Logik der Forschung* as a programmatic alternative). Mach, by contrast, did not favor separating the historical and psychological method for experiments, also covering thought experiments on the same epistemic level with an explicit genetic approach.[33] By the way, Karl Popper approached Sarton after WW II praising him for his writings on the history of civilization in the context of his books *Open Society and its Enemies* and *The Poverty of Historicism*.[34]

Mach integrated historical, psychological and epistemological perspectives with interdisciplinary methods (physics, physiology, psychology, mathe-

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32 Siemsen 2013b

33 Mach, "On thought experiments", in: Mach 1905/1976, pp. 134-147.

34 Popper to Sarton, March 16, 1942, December 30, 1952, July 28, 1954, October 11, 1957. Houghton Library, Harvard University.

matics including the concepts of sensation, "Gestalt", Richard Semon's "mneme", and "metapsychical" properties of each theory). In this context Siemsen mentions Sarton's influence on James Conant, who wrote the obituary on Sarton's lifework 1957 in *Isis*. A parallel story can be found in the Harvard science-teaching program elaborated by Philipp Frank from 1939 on, to be addressed later on.[35] According to Siemsen, the family resemblance between Mach and Sarton can be illustrated with the former's *Culture and Mechanics* and the latter's history of ancient science. When Sarton declared *genesis* and *development* as the main guiding principles of the journal *Isis*, we can detect an influence leading up to the history of science teaching and science education in Harvard (from James, to Conant, Kuhn, I.B. Cohen, Holton).[36]

In the *George Sarton Centennial* volume, published in 1984 in Ghent (co-edited by my deceased colleague and friend Werner Callebaut),[37] Marc de Mey convincingly compared Sarton's program with Leo Apostel's project for an interdisciplinary synthesis with the characteristic features of multi-disciplinarity, synthesis, genesis/development, and humanism.[38] All these elements can be found in *Isis*, in the Unity of Science movement from 1934 on up to Carnap's "scientific humanism", and I will address them in the second part of my paper. The fact that in 1947 (Vol. 37, No.1/2) the correspondence between Mach and Vienna born anthropologist Robert H. Lowie (Löwe) was published in *Isis,* nicely confirms Sarton's enduring appreciation of his *alter ego* in HPS with its evolutionary and interdisciplinary approach, and with its common-sense skepticism towards pure philosophy.

Let me conclude the first part of my paper with the Mach-Sarton comparison by taking a quick look on contemporary studies. I am referring to the evolution of knowledge from a global point of view. There are also explicit references to Mach as a pioneer of this expanding field along with the application of mass data and computers.[39] These are a further developments and elaborations of Mach's (and partly Sarton's) ideas based on a

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35 James Conant 1957. On Frank's important role see the related publications of Gerald Holton, recently in: The Life and Work of Philipp Frank. *Studies in East European Thought. Vol. 69*, Issue 3, Springer: Sept. 2017.

36 Siemsen, 2012, 2013a,b.

37 George Sarton Centennial 1984.

38 Marc de Mey 1984.

39 Renn (2012), Laubichler / Renn (2017)

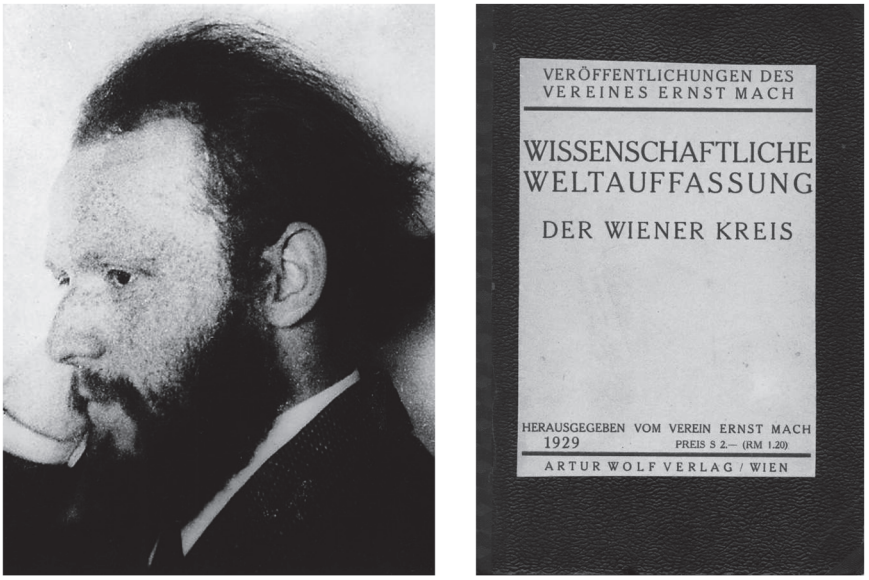


Fig. 7: Otto Neurath (1882-1945). The Vienna Circle Manifesto (1929), Cover Page
Source: Institute Vienna Circle

great number of sources thanks to the new information technology of the Internet – as digital humanities with open access policy. This research could also be seen as a kind of a revived version of the unity of science conception between the wars in the wake of the so-called "knowledge society" combining cultural evolution and development. We find it again on the agenda in one of the last issues of *Osiris* on "Data Histories" (32/ 1, 2017). I think, George Sarton would have been proud of this.

2. History of science and the unity of science movement

One of several attempts to achieve the unity of science in the interwar period emerged from the Vienna Circle's aspiration to bridge the gap between the sciences and humanities through scientific language and formal methods. This found expression in the famous manifesto of the Vienna Circle in 1929:[40]

[40] WWWK 1929, quoted after Uebel/Stadler, p. 144.

> *"With the proof of the possibility and the outline of the shape of the complete system of concepts, the relation of all statements to the given and with it the general structure of* unified science *becomes recognizable as well."*

And the manifesto, written by mainly Carnap, Hahn, and Neurath ends with the optimistic and programmatic diction, with reference to its diverse proponents:[41]

> *"However, their achievements take place in the historical development. We are witnessing how the spirit of the scientific world-conception penetrates in growing measure the forms of personal and public life, of education, of childrearing, of architecture, and how it helps shape economic and social life according to rational principles.* The scientific world-conception serves life, and life embraces it."

It was mainly Neurath and Carnap, who both initiated what was later to be called Unity of Science movement along with the huge publication project of an *International Encyclopedia of Unified Science* in the spirit of the French *Encyclopédie*. Together with the American pragmatist philosopher Charles Morris, they organized a preparatory conference in Prague in 1934 which was followed by five International "Congresses for the Unity of Science" in exile, organized in Paris (1935), Copenhagen (1936), Paris again (1937), Cambridge, UK (1938), Harvard (1939), and Chicago (1941).[42] The history of these congresses, documented in the journal *Erkenntnis* (*Journal for Unified Science*), exemplifies the joint efforts of Logical Empiricists (Vienna, Berlin, Prague, Warsaw, Paris, Cambridge/ Oxford) to cooperate with American scholars in the period of fascism and national socialism. For Neurath and his colleagues the unity of science as a regulative principle was already then "a historical fact in a sociological sense", one that was directed against any hierarchical system of sciences, which was for him a "great scientific lie".[43] Therefore, "the whole of science is basically always under discussion"[44] This amounted to a pragmatic and historical conception of science between methodological rela-

41 Ibid., p. 151
42 Stadler 2015, pp 161-194.
43 Neurath 1935, cited after Neurath 1983, p.115.
44 Ibid., p.118.

tivism and theoretical pluralism based on empirical statements without foundationalism. This position provided the basis for the development of Charles Morris's semiotics with the tripartite perspective of syntax, semantics, and pragmatics for all sciences. Whereas Neurath emphasized the empirical conception of all sciences (called "Ballungen" at the center) with the help of modern logic, Carnap and Morris focused more on formal methods. This led to a permanent tension between these pioneers and would later end up with a dramatic break between the good old friends Neurath and Carnap at the end of WW II.

From the beginning there was a stronger leaning towards the natural sciences, but the original plan of the IEUS according to its leading figures was to provide an overview of all sciences documenting their foundations and methodologies incl. sociology and history. This large-scale publication project was to include some 260 monographs complemented by 10 volumes of a visual Isotype "Thesaurus". Within the projected VIII volumes there was the plan to also include the social and humanistic sciences (Vol. VII) as well as the history of the scientific attitude (Vol. VIII). This ambitious enterprise was accompanied by the book series "Einheitswissenschaft" ed. by Neurath 1933-38 and a "Library of Unified Science" 1938-41 in Dutch exile. Three of the books were published before the invasion of German troops: Heinrich Gomperz, *Interpretation. Logical Analysis of a Method of Historical Research* (1939); Richard von Mises, *Kleines Lehrbuch des Positivismus. Einführung in die empiristische Wissenschaftsauffassung* (1939); Hans Kelsen, *Vergeltung und Kausalität* (1941).

For historical and personal reasons, but primarily because of the war (e.g., the unexpected death of Neurath in 1945 and the movement being dispersed thematically and geographically in US exile) with a number of scholars being forced to emigrate, this project remained incomplete with only 19 introductory monographs entitled *Foundations of the Unity of Science. Toward an International Encyclopedia of Unified Science* (ed. by Otto Neurath, Rudolf Carnap and Charles Morris with the University of Chicago Press 1938 ff., reprinted in 2 volumes in 1970/71). The editors had published a joint preview of "Encyclopedia and Unified Science" together with Niels Bohr, John Dewey, and Bertrand Russell.[45] In addi-

[45] Carnap/Neurath/Morris, pp. 1-76.

tion to the focus on the natural sciences, we also find "Procedures of Empirical Science" by Victor F. Lenzen, "The Conceptual Framework of Psychology" by Egon Brunswik, "Foundations of the Social Sciences" by Otto Neurath, "Theory of Valuation" by John Dewey, "The Development of Rationalism and Empiricism" by Giorgio Santillana and Edgar Zilsel. The most surprising contribution was certainly Thomas Kuhn's "The Structure of Scientific Revolutions" (1962). Carnap and Morris had invited Kuhn to submit this monograph and Carnap highly appreciated his contribution. This publication history was later on blurred by Kuhn himself and his followers, who praised him as the sole pioneer for the "historical turn" in contrast to Logical Empiricism and the Unity of Science. But given this remarkable episode it does not come as a surprise that Neurath and his colleagues had tried to include also the history and sociology of the sciences in their ambitious project, e.g., with (unsuccessful) invitations to the sociologist Louis Wirth in Chicago, but also the historian of science George Sarton.

Already in a letter, dated February 15, 1936, Otto Neurath wrote to Sarton in Harvard inviting him to 2nd International Congress for Unity of Science in Copenhagen on the main topic of causality, which did not work out for unknown reasons.[46] We have not been able to locate any response by Sarton to Neurath's letter. Nevertheless, Neurath wrote again to Sarton (July 14, 1938) inviting him, this time, to become a member of the Organizing Committee of the International Congresses for the Unity of Science, which listed some 40 prominent scholars from K. Ajdukiewicz to J.H. Woodger. At the beginning of October 1938 Sarton answered as the editor of *Isis* (letterhead):

> *"Your project interest me very much and if I were free I would gladly write the article which you suggest, but alas it is entirely (out of the) question. My time and energy are* completely *mortgaged, and I cannot undertake anything new of this size without loyalty to former undertakings. With kind regards, George Sarton"*

[46] Correspondence Neurath-Sarton, located at the Houghton Library, in Harvard University, and Vienna Circle Archives, Rijksarchief Nord-Holland, Haarlem (NL) and Institut Wiener Kreis, University of Vienna.

And he adds:

> "*Just arrived Mieli's "Science arabe" published in Holland. 80% at least is taken from my translation. Think of somebody writing a history of Greek science without knowing Greek!*"

The energetic organizer Neurath contacted Sarton once again (October 19, 1938): *"Thank you very much for your kind letter of July 26th. We are very pleased that you are interested in our Encyclopedia and in our attempts to further the unification of the sciences"* asking him again to become a member of the advisory committee of the IEUS and proposed mutual exchange of publications.

Shortly afterwards (November 2, 1938), Sarton wrote to Neurath in The Hague:

> *"Many thanks for your kind letter of October 19. Though my investigations are in the historical rather than in the philosophical field, I am deeply interested in your efforts, and will always be happy to do whatever I can in order to promote their success."*

And he agreed to become a member of the advisory committee of the IEUS if this did not require too much time. Moreover, they decided to exchange the journals *Isis* and *Erkenntnis* and related publications and to run for mutual "four half-page advertisements a year".

Neurath was pleased and wrote another invitation (Nov. 23, 1938) to Sarton as an author of an introductory monograph on "problems of the history of science" for the IEUS (after the Italian historian of science Federigo Enriques had declined). And he continues:

> *"Personally I regard the history of science as a very important factor in our analytical studies. It is not a mere accident, in my opinion, that Ernest (!) Mach, Duhem and others were so extremely interested in the history of science. I think ... comparison between different theories leads to logical analysis and therefore the history of science is a good preparation for the logic of science. The history of science also plays another part in our encyclopedical work; it is a discipline in itself and it is very useful to understand the evolution of the sciences as the product of the efforts of Mankind. I always was very impressed by your immense work, which enables us for the first time in history to see horizontal sections through the history down the*

ages. … Not many people have such an apparatus for the preparation of the history of science."

This monograph was intended – overly optimistically – to be published together with the 19 other ones on the occasion of the forthcoming 5th Harvard Congress 1939.

Sarton had apparently declined because Neurath hoped in his answer to him (July 5, 1939) for a later contribution in the forthcoming volumes, he acknowledged the ongoing cooperation and enclosed his booklet *Antike Wirtschaftsgeschichte* published in 1909 (History of Ancient Economics) as a present.

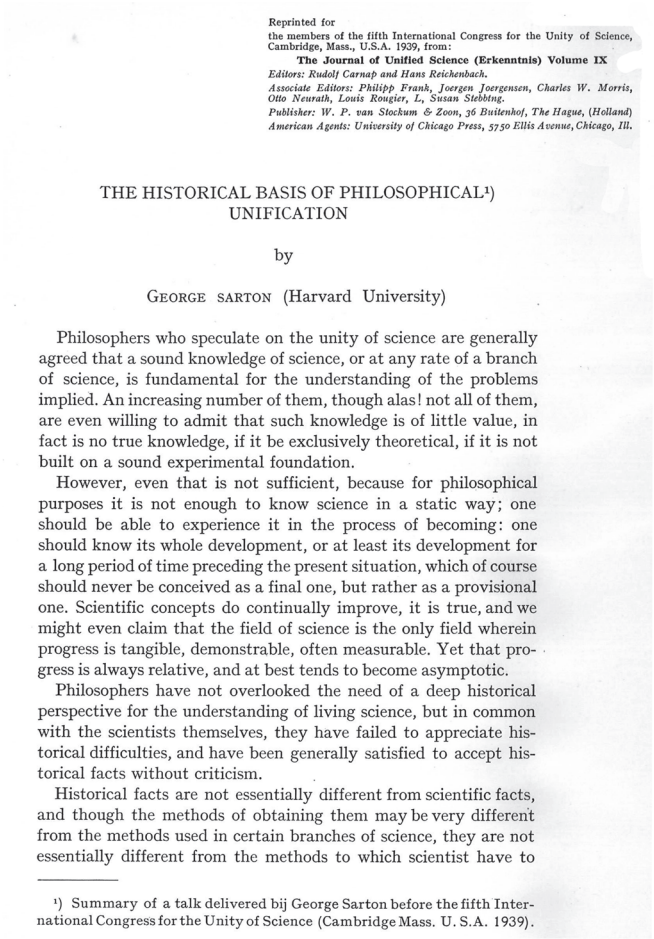
Actually, in August 1939 Sarton welcomed and met Neurath on the occasion of the 5th International of Unity of Science Congress in Harvard, to which Sarton had contributed with a paper on "The Historical Basis of Philosophical Unification". The huge program of the congress with 200 scholars from 9 countries participating coincided with the outbreak of WW II. (It was made possible by the Hague International Institute for the Unity of Science, supported by the AAAS, APA, PSA, History of Science Society, and the Association of Symbolic Logic). The Proceedings were supposed to appear in the 9th volume of *Erkenntnis / The Journal of Unified Science*, but because of the war this was not possible. Only 10 articles were to appear later on in the reprint of *Erkenntnis* (8, 386-437) – namely those of Heinrich Gomperz, Julius Kraft, Kurt Lewin/Karl Korsch, Hans Reichenbach, Alonzo Church, Jörgen Jörgensen, Kurt Goldstein, Hans Kelsen, Otto Neurath, F.S.C. Northrop. In 1994 the proofs of Carnap, Frank, Morris, and Zilsel were edited by the author and published, additionally.[47] In the schedule of the Harvard Congress three sessions were devoted to the history of science, one of them focused exclusively on an examination of Sarton's materials for the study of the history of science. This section announced eight papers, additionally.[48]

The inaugural lectures of the congress were delivered by Harvard President James B. Conant, P.W. Bridgman, Otto Neurath, and Charles Morris. The first section on "Aims and Methods for Unifying Science" (three sessions)

[47] Pauer-Studer 1994, pp. 289-308.

[48] Werner Jäger, Estelle de Lacy, Karl Dürr, George de Santillana, Talcott Parsons, Tenney L. Davis, Hans Kelsen, Philipp Frank.

was introduced by the already mentioned paper of Sarton, followed by talks delivered by P.W. Bridgman, H.M. Kallen, Susanne Langer, Herbert Feigl, Ernest Nagel, Joergen Joergensen, Richard von Mises, and Heinrich Gomperz. (The second section on "Scientific Method and the Language of Science" covered 4 sessions, the third one on "Methodology of the Special Sciences" four sessions, the fourth one on "Problems in Exact Logic" two sessions, the fifth one on "Science and Society" (two sessions), the latter including papers by Louis Wirth on sociology of science, and Edgar Zilsel on "The Social Roots of Science").



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THE HISTORICAL BASIS OF PHILOSOPHICAL[1])
UNIFICATION

by

GEORGE SARTON (Harvard University)

Philosophers who speculate on the unity of science are generally agreed that a sound knowledge of science, or at any rate of a branch of science, is fundamental for the understanding of the problems implied. An increasing number of them, though alas! not all of them, are even willing to admit that such knowledge is of little value, in fact is no true knowledge, if it be exclusively theoretical, if it is not built on a sound experimental foundation.

However, even that is not sufficient, because for philosophical purposes it is not enough to know science in a static way; one should be able to experience it in the process of becoming: one should know its whole development, or at least its development for a long period of time preceding the present situation, which of course should never be conceived as a final one, but rather as a provisional one. Scientific concepts do continually improve, it is true, and we might even claim that the field of science is the only field wherein progress is tangible, demonstrable, often measurable. Yet that progress is always relative, and at best tends to become asymptotic.

Philosophers have not overlooked the need of a deep historical perspective for the understanding of living science, but in common with the scientists themselves, they have failed to appreciate historical difficulties, and have been generally satisfied to accept historical facts without criticism.

Historical facts are not essentially different from scientific facts, and though the methods of obtaining them may be very different from the methods used in certain branches of science, they are not essentially different from the methods to which scientist have to

[1]) Summary of a talk delivered bij George Sarton before the fifth International Congress for the Unity of Science (Cambridge Mass. U. S.A. 1939).

Fig. 8: Offprint of Sarton's paper delivered at the Congress in Harvard 1939, p. 1
Source: Institute Vienna Circle

It seems that the paper of Sarton was planned as a sort of keynote talk. It merits closer interpretation.[49] The summary begins with a critical remark on philosophers:

> "*Philosophers who speculate on the unity of science are generally agreed that a sound knowledge of science, …, is fundamental for the understanding of the problems implied. An increasing number of them, though alas! Not all of them, are even willing to admit that such knowledge is of little value, in fact is not true knowledge, if it be exclusively theoretical, if it is not built on sound experimental foundation.*"

And Sarton continues by emphasizing:

> "*However, even that is not sufficient, because for philosophical purposes it is not enough to know science in a static way; one should be able to experience it in the process of becoming: one should know its whole development, or at least its development for a long period of time preceding the present situation … as a provisional one.*"

In keeping with his historical-genetic position he continues:

> *"Yet, that progress is always relative, and at best tends to become asymptotic. Philosophers have not overlooked the need of a deep historical perspective for the understanding of living science, but in common with scientists themselves, they have failed to appreciate historical difficulties, and have been general satisfied to accept historical difficulties without criticism. Historical facts are not essentially different from scientific facts, … they are not essentially different from the methods to which scientist have to resort when direct experiments … are out of the question."*

And Sarton introduces the criteria of truth and falsity as criteria of relevance with the correct application of methods. According to him they were neglected because of the diversity of approaches with the need of a complex scientific, historical, philological, and philosophical training. Therefore, there was the danger of the *"new discipline 'the history of science' being a sort of no-man's land at the intersection of science, history, philosophy, etc. offered great opportunities for historical shallow-*

[49] Sarton 1939

ness as well as for scientific dilettantism."[50] After this strong criticism of philosophical ignorance, Sarton switched surprisingly into the third person modus in order to present his own lifework as a paradigmatic research program:[51]

> "*At this point Dr. Sarton begged the audience to excuse him of his own experience ... Toward the end of long physical and mathematical studies at the University of Ghent, under the influence of Comte, Mach, Tannery, Poincaré, his thoughts were gradually detached from scientific technicalities and oriented more closely to the direction of scientific philosophy. At first his interest was predominantly philosophical, but as he realized the urgent need of a historical and humanistic preparation to complete his purely scientific one, he became more and more interested in the history of science, and more convinced of the necessity of studying that history as thoroughly as possible*."

Sarton then proceeds to remark that in 1912 he was determined "*to devote his life to a double project. 1.To prepare a survey of the scientific knowledge available in every branch of science at each period. As this survey was to be international, interracial, interreligious, it was not possible to divide the past to the accepted conventions"* Here Sarton refers to his extensive publication project spanning the periods up to the fourteenth century sponsored by the Carnegie Institution. *"2. To edit the journal* Isis *containing materials and bibliographies of the history of science with a registration and classification of all other publications to be criticized and discussed." Osiris* (1936 f.) also featured longer papers.

As the general purpose of these two publication projects Sarton formulated *"the diffusion of sound methods, the discouraging of dilettantism and the final establishment of the history of science as an independent discipline with a high standard of accuracy as any other scientific discipline."*[52]

Now, these statements were apparently of more principal and strategic character, because Sarton's talk was delivered in his own seminar room in the middle of Harvard Library, generously provided to the Carnegie Institution. Sarton had done this deliberately *"to show the large apparatus*

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50 Sarton 1939
51 Ibid.
52 Ibid.

which has thus far been collected and is very probably the richest of its kind in the world" … "in order that scientists, historians, and philosophers may be able to continue their own work on a sound foundation". And Sarton expressed his conviction that *"the field of the history of science is immense, for it concerns the history of every branch of science at every time and in every clime, as it was developed by people of every race, sect and nationality and written down in a great many languages. ... The amount of work remaining to be done will require the devotion of many scholars for many generations to come; in fact it will never be completed. The work should be done thoroughly, slowly, patiently, in the same spirit as similar work is done by naturalists and other scientists."*[53]

His strong plea ends with a remarkable commitment to a unified view on science: *"The unity of science may be proved by the convergence of modern methods, that is, by the efforts of modern scientists approaching definite subjects from many angles and in many ways yet obtaining results which tally and developing independent theories which harmonize. The unity of science is proved also by the consideration of its growth, similar to the growth of a tree the infinite ramifications of which do not destroy the singleness; that is, it is proved inductively thanks to the efforts of historians of science. These efforts need encouragement and purification, for the philosophy of science cannot be completely developed if its historical foundation is not soundly established as its scientific one."*

This is not only a manifesto for the history of science as a discipline proper but also an endorsement of the strong need of a history and philosophy of science, which is currently further developed as an integrated HPS ("&HPS").[54] Unfortunately, we do not know about the discussion of Sarton's talk and the session dedicated to his work later on in the 5th Congress, so I can only refer indirectly to his awareness and impact through some related summaries:

The most relevant paper in our context is that of the philosopher, sociologist and historian of science Edgar Zilsel, who had to emigrate from Vienna to the US in 1939 and tried to continue his studies on the origins of modern science under most the difficult circumstances. In his talk he presented his thesis ("Zilsel-thesis") on "The Social Roots of Science", which was elab-

[53] Ibid.
[54] Stadler 2017b

orated to "The Sociological Roots of Science" in the *American Journal of Sociology* (1942). Here he refers for the first time to Sarton's *Introduction to the History of Science* in the bibliographical notes, followed by a reference to Sarton on Stevin (1934). As a result of his first public presentation in English, Zilsel resumed his long-term investigations described here in the following:[55]

Fig. 9: Edgar Zilsel (1891-1944)
Source: Institute Vienna Circle

"*In the period from the end of the Middle Ages until 1600 the university scholars and the humanistic literati are rationally trained but they do not experiment as they despise manual labor. Many more or less plebeian craftsmen experiment and invent but lack methodical rational training. About 1600, with the progress of technology, the experimental method is adopted by rationally trained scholars of the educated upper class. So, the two components of scientific research are united at last: modern science is born. The whole process is embedded in the advance of early capitalistic economy which weakens collective-mindedness, magical thinking, traditions, and the belief in authority, which furthers mundane, rational, and causal thinking, individualism and rational organization.*"

[55] Zilsel 1939, p.220

This research project was subsequently refined and elaborated, in the hope of an intellectual exchange and administrative support, with and by George Sarton in Harvard. But Sarton himself had to fight for his own projects on the history of science as can be gleaned from the correspondence located in the Houghton Library in Harvard.[56] Zilsel had contacted Sarton on the occasion of the 5th Congress for the Unity of Science. They both met and their correspondence is a moving documentation of two pioneers in the history and sociology of science, still conceived of from a European point of view. Zilsel, who was facing ever greater economic hardships, immediately addressed Sarton asking him to help with the co-financing his project in N.Y.C., funded by the Carnegie Foundation and the Emergency Committee in Aid of Displaced Foreign Scholars. He attached praising reviews of his book on the concept of genius (*Die Entstehung des Geniebegriffs*, 1926) by prominent scholars and recommendation letters from his Viennese teacher Heinrich Gomperz and his Vienna Circle colleague Rudolf Carnap. Sarton apparently recommended the project to Carnegie before they met in person during the Harvard congress. One year later, Zilsel again asked Sarton for permission to list his name in an application to the Social Science Research Council and enclosed the reprint of his 1939 paper announcing another one on Copernicus and mechanics and on "William Gilbert's *De Magnete* and the origin of scientific method" for a possible publication in *Isis*. As additional references he named Philipp Frank and Sarton's assistant I.B. Cohen in Harvard. Zilsel then sent him another paper on "History and Biological Evolution". After that, Zilsel thanked Sarton for his promise to publish the Gilbert article in *Isis*, but lamented the late publication date after 1941, given his time pressure with all pending applications for further grants. He explained his precarious economic and private situation after his forced migration (e.g., his wife suffering from mental illness) and approached Sarton successfully for supporting him as an expert in the list of references and recommendations for his huge research project on the origins of modern science. The latter had also agreed to publish Zilsels' s paper on Gilbert in *Osiris,* which ultimately did not work out in the long run because of the invasion of German troops in Belgium, where Sarton's periodicals had been published until then. In the spring of 1941 Zilsel turned to Sarton and asked to be hosted

[56] For a detailed reconstruction of this communication: Raven / Krohn 2000; Fleck 2015.

by his Institute at the Widener Library in Harvard without any financial obligations, while he was a "free floating" research fellow at the International Institute of Social Research in New York conducted by Max Horkheimer. Once more, he describes his depressing family situation and economic difficulties, which his son Paul Zilsel reported most movingly decades later.[57] Obviously, Sarton had agreed to host his colleague in the case of his successful application for the two grants as a precondition for such a research stay, but Sarton himself should have applied on behalf of Zilsel for these grants. This was apparently a misunderstanding brought on by Zilsel in his letters. The application to the Rockefeller Foundation failed and Zilsel had to clear up the confusion about his relationship to the Horkheimer institute and a possible unintended competition with Sarton's collaborator in Harvard (most likely I.B. Cohen). Nevertheless, Sarton was willing to serve again as a supporter after several funding applications failed. The tragic correspondence ended when Zilsel had received a short teaching position at Hunter College in N.Y.C. Subsequently, he moved to California (Oakland), where, having been awarded a grant from the American Philosophical Society, he accepted a position as a teacher at Mills College in Oakland in order to secure his livelihood despite his efforts to finish his incomplete research project. Exhausted and disappointed by his demanding and unsuccessful efforts to continue his scholarly life in exile and reinforced by managing his family problems, Zilsel committed suicide on March 11, 1944 in Oakland. Only two months before his death he had expressed in a letter to Sarton his willingness to review Hans Kelsen's book *Vergeltung und Kausalität* (1940) for publication in *Isis*. This extensive study on the development of laws in nature and society since ancient times was first published as volume 2 in the book series "Library of Unified Science", ed. by Neurath with van Stockum & Zoon before Neurath fled to the UK, following Richard von Mises's *Kleines Lehrbuch des Positivismus* (1939). Due to the outbreak of WW II it was only available in 1946 but both books were later published in English (Kelsen 1941, Mises 1951). So, it's is not by accident that Kelsen, the founder of the pure theory of law and father of the Austrian Republican Constitution, had also contributed to the Sarton session in Harvard in 1939 just with his paper "Causality and Retri-

[57] P. Zilsel 1988.

bution" a summary of this book, with an extended published version in the *Journal of Unified Science* (8/1939).

Three years before his death Edgar Zilsel had contributed his article "Problems of Empiricism" to the IEUS. The whole monograph entitled *The Development of Rationalism and Empiricism* also included Giorgio de Santillana's contribution "Aspects of Scientific Rationalism in the Nineteenth Century" in 1941. Zilsel concluded his part as follows:[58]

> "*The breakdown of mechanistic physics could not fail to give a new empirical thinking. With failure of mechanistic physics, the assumption of a second world behind experience had lost its scientific support. Now the subject-object metaphysics, the pride of all philosophers, who looked down on the naïve layman, was badly shaken; its problems began to appear as pseudo-problems. Since causes and laws were employed in the new physics as functional connections and mere regularities, the unempirical components of those concepts, already criticized by Hume, became suspect for scientists as well. All these implications were consistently developed by Mach. On the other hand, physical hypotheses and models had suddenly turned out to be unsuitable, though having proved fruitful for three centuries. Necessarily, general methodological questions arose as a result from that fact, and, for the first time in the history of modern physics, the whole internal construction became problematic. .... Most of the problems deal rather with the deductive side of theoretical knowledge than with its empirical components. They were raised by Mach, by fictionalism and conventionalism of the late nineteenth century, and were more or less suggested by the physical revolution. Poincaré's conventionalism, however was influenced by modern mathematics as well as by the new physics. In the early twentieth century those mathematical and logical influences increased, united with the empiricist tradition, and resulted finally in logical empiricism – a subject which must be reserved for later treatment*."

By the way, in the first part Giorgio de Santillana, who also published in *Isis*, tried to reconstruct the story of rationalism as a decline since Descartes, Spinoza and Leibniz, with the quote *"What is conceivable can happen"* following Parmenides and Wittgenstein.[59] He sees the fate of

[58] Zilsel 1941, p. 93 f.
[59] De Santillana 1941, p.1.

scientific rationalism as being triggered by a new perspective*: "Under the relentless pressure of social change, with the growing operationalism of physical theory and the metaphysical devastations attendant on Darwinism, the myth of unity could no longer hold. It had to be replaced by unification. But with that the status of science is changed and also that of the scientist. The mirror of nature that reason had endeavored to build up through the ages is shattered, and we look for the first time straight out into an unknown world."*[60] Maybe this can be seen as another variation on "Vienna indeterminism?[61]

This joint historical contribution could not compensate for the missing monograph on the history of science, although within the Sarton session at the 5th Congress we see additional related contributions.

The first was coming from Heinrich Gomperz (1873-1942), who, a student and admirer of Mach at the periphery of the Vienna Circle, was one of Zilsel's (and Popper's) teachers in Vienna. He was a professor of the history of philosophy with a special expertise in Ancient philosophy like his father, the prominent philologist and philosopher Theodor Gomperz. After Austro-fascism had come to power he refused to sign a letter of obedience and emigrated with the help of F.C.S. Schiller to the US in 1935, where he got a position as a visiting professor at the University of Southern California. In exile in the US he remained active at the margins of the Unity of Science movement with publications and conference contributions. For example, his monograph *Interpretation: Logical Analysis of a Method of Historical Research* (Library of Unified Science. Monograph Series 8-9, The Hague 1939) represented the historical sciences and pragmatics within the Encyclopedia project and was complemented by his talk in Harvard on "Unified Science and Value" (1939). Here he concluded, *"that science cannot but take into account, or at least be aware of, values and value judgments in at least six respects"*.[62] He thus challenged the dominant position of a meta-ethical non-cognitivism (emotivism) dominant in Logical Empiricism and he ends up with a plea for cooperation, which finally leads to unification: *"The greatest possible degree of insight will certainly not be achieved as long as different workers work in separate fields, without communicating*

60 Ibid., p. 47.
61 Coen 2007.
62 Gomperz 1939, p.31.

with one another and unable to understand each other's language. In this sense Unified Science itself may justly be termed a value."[63] Besides John Dewey's monograph *Theory of Valuation* (1939) and later Abram Edel's *Science and Structure of Ethics* (1961), this paper partly compensated a desideratum of the Encyclopedia project on the role of values. Sarton was well aware of Gomperz' and his father's work because he addressed Heinrich Gomperz in a letter in May 1, 1939 with the proposal for an English edition of the projected 2 volume's biography of Theodor Gomperz, the 1st volume ed. by son Heinrich in 1936. Gomperz felt honored but declined, stating that the interest for his father's work was already limited in the German-speaking world, and also mentioning his own work load. So, Gomperz after having edited and introduced the first volume in 1936 (*Theodor Gomperz. Briefe und Auszeichnungen. Erster Band, 1832-1868)*, did not pursue this attractive as well as demanding publication project, which was realized many years later by the lawyer and historian Robert A. Kann. It was only in 1974 that a second volume appeared.[64]

Amongst the other speakers of the History of Science session we find the physicist and philosopher of science Philipp Frank (1884-1966), who was

Fig. 10: Philipp Frank (1884-1966) and Richard von Mises (1883-1953)
Source: Institute Vienna Circle

63 Ibid., p.36.
64 Kann 1974.

Einstein's successor in Prague as professor for theoretical physics and had to emigrate from there to the US in 1938. In Harvard he received a part-time position as lecturer and research fellow up to the end of his life, strongly supported by President James B. Conant in the context of his science-teaching program. He was also an active member of the Vienna Circle and the Unity of Science movement (with the contribution on the *Foundations of Physics*, 1946) and had edited together with Moritz Schlick the book series "Schriften zur wissenschaftlichen Weltauffassung". In Harvard he founded the "Institute for the Unity of Science" in 1948 as a forum for the preceding initiative of Neurath in Holland and the ongoing *Encyclopedia* project, which was dissolved and moved to the PSA after his death in 1966.[65]

With this biography it was to be expected that Frank and Sarton shared common interests and fostered an intellectual exchange from the beginning at Harvard Square. When Frank published his successful popular biography of Einstein (first edition *Einstein – His Life and Times*, 1947), Sarton immediately responded with enthusiastic appreciation of this book in a scientific and cultural context.[66] He only objected strongly to Frank's dealing with Max Brod's novel *The Redemption of Tycho Brahe*, where Brod compared the figures of Tycho and Kepler, the latter obviously being characterized with Einstein in Prague. No doubt, this was a fiction by Brod, who experienced Einstein personally. But Sarton expressed his distance to any historical novel and was especially critical of Kepler, even if a great astronomer, as an irrational thinker, who *"managed to write more nonsense than any other man of science."*

In his paper "The Position of Einstein's Theory of Relativity in the Evolution of Science" at the Harvard Congress in 1939, Frank refers to different philosophical interpretations of the relativity theory, more or less linked to the language of physical theory. According to Frank, these variations are due to the fact that *"the philosophical interpretation of a physical theory is connected with the means of representation used in the theory ..."* and *"Einstein's theory plays a double role in history. On the one hand it brought to an end the form of representation used by organismic, mediaeval physics and made possible a logical mechanistic mode of expression.*

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65 *Isis*, XXX, 2.
66 Sarton to Frank, April 14, 1947.

It then went on to show that this mechanistic representation would no longer cover the range of our presentday experiences, that it is possible to set up a theory which no longer fits into the frame of mechanistic physics, but which, within the frame of the logico-empirical conception, enables us to 'understand' the phenomena furnished by modern research."[67]

Several years prior to this exchange Sarton had promised Frank to publish a review of his book *Between Physics and Philosophy* in *Isis*, but he heavily criticized Frank for not having written his announced review of the book by his friend Richard von Mises, *Kleines Lehrbuch des Positivismus*[68] which later appeared in English in 1951 under the title *Positivism. A Study of Human Understanding*. This letter apparently impressed Frank very much. After a response, in which he expressed his regret in 1941,[69] we find one year later a highly appreciative review published in volume 33 (No.6, June 1942) of *Isis* entitled "Concerning an Interpretation of Positivism". Here Frank portrayed the intellectual development of Mises, who was strongly influenced by Mach and inspired by Rilke on his path from Vienna via Istanbul to Harvard:

"By advocating 'positivism', the author means answering 'positively' the question of whether science plays an essential part in all attempts to understand and predict the happenings in our world. In the 20th century the same question has been posed frequently, and frequently answered – in the negative."[70] The strict anti-metaphysical attitude of the Vienna Circle is not shared by him in favor of emqploying a sort of "connectibility" between science and non-science playing down the role of formal logic. This story is amazing because Richard von Mises himself had presented his book in 1939 precisely in the session which was opened by Sarton. His paper "Scientific Conception of World. On a New Textbook of Positivism" begins with the confession that *"the book I want to review is not a treatise on positivism, which discusses the pros and cons of the empiristic (!) view from a so-called higher standpoint. It is the positivist himself who speaks, who argues, who describes the world and, above all, within the world the intellectual efforts of men. The author is a devoted disciple of* Mach*, but imbued with a strongly critical attitude towards language; he is to some*

67 Frank 1939, p.299 f.
68 Stadler 1990.
69 Frank to Sarton, April 29, 1941.
70 Frank 1942, p.684.

extent connected with the Vienna School."[71] He then proceeds to describe the seven chapters of his book, which under the German subtitle is better characterized as "Introduction into the Empiricist Conception of Science". And he ends optimistically by expressing his hope *"that progressive expansion of human experience and of its systematization with regard to linguistical critics will furnish a more and more complete total of fully associable sentences which cover the whole field of human interest in physical as well as in biological or sociological matters."*[72]

Richard von Mises, the pioneer of applied mathematics in Berlin before he was forced to migration in 1933, continued to participate in the Unity of Science project on the periphery and, similar to Gomperz, showed a critical sympathy. Together with Reichenbach, he emigrated to Istanbul, where he published alongside his mathematical teaching and research, most notably probability theory, but also on general topics in the history and philosophy of science and helped to launch the *Encyclopedia* with the booklet on Mach.[73] Sarton's insistence on a review of his book *Kleines Lehrbuch des Positivismus* in *Isis* once more documents his agreement with this Harvard colleague as a mathematician and historian of science with an encyclopedic approach.

In any case, the collaboration between Sarton and Frank continued. In 1942, the astronomer Victor F. Lenzen (Berkeley), who contributed to the IEUS with *Procedures of Empirical Science* (1938) as an "appraisal of the prospects of unified science"[74], favorably reviewed Frank's book *Between Physics and Philosophy* in *Isis*[75] and delivered another review 10 years later on a volume entitled *Contributions to the Analysis and Synthesis of Knowledge*, ed. by Frank in 1951, which was geared primarily to specialists in logic.[76]

In the opening session of the 1939 congress we find the youngest proponent of Logical Empiricism in the US who already emigrated from Vienna in 1931 caused by the precarious political and scholarly perspectives for his academic career: Herbert Feigl (1902-1988), the gifted student of Schlick

71 Mises 1939, p.198.
72 Ibid., p.201 f.
73 Mises 1938
74 Lenzen 1938
75 Lenzen, *Isis* 1942, Vol. 34/2, p.180
76 Lenzen, *Isis*, 1952 Vol.43/1, pp. 87 f.

had introduced "Logical Positivism" in 1931 in North America and presented "The *Wiener Kreis* in America"[77]. In 1955 he founded the still existing "Minnesota Center for Philosophy of Science" at the University of Minneapolis, which became a vivid forum for the Vienna Circle in exile. His paper on "Unity of Science and Unitary Science" was intended to be *"an analysis of the mutual relations of empiricism, naturalism, and physicalism in the light of the convergence of theories as indicated by levels of scientific explanation"*[78]. He distinguished three meanings of the term "unity of science": first, as adopted by Carnap and Neurath as *unity of the language of science* as basic idea of the *Encyclopedia*; second, as the thesis of naturalism vs. traditional philosophy; and third, "*physicalism in the strict sense*, *postulating the potential derivability of all scientific laws from the laws of physics."*[79] Feigl recognizes a "convergence of theories towards a unitary scheme" united in the theories of relativity and quanta accompanied by the problem of a clear definition of "emergence". This is a sophisticated study advocating a principal interdisciplinary reductionism avoiding philosophical fallacies and overcoming "radical physicalism" as an underdetermined concept.

The network of intellectual co-operations and exchanges sketched so far is confirmed by the communication between Sarton and the two other editors of the IEUS besides Neurath, namely Rudolf Carnap and Charles Morris:

Rudolf Carnap expressed in his 1939 talk "Science and Analysis of Language" the need for applying all three branches of semiotics (pragmatics, semantics, syntax), and recommended that *"none of the components should be neglected, each should be acknowledged in its own right. The studies in these directions have only just begun; most of the work is yet to be done."*[80]. He was certainly the most systematic philosopher of science in the Vienna Circle. With the help of W.V.O. Quine and Morris he came from Prague to the US in 1936, where he was a visiting professor at Harvard before he was appointed to a chair in Chicago. He met Sarton during the Congress in 1939 and stayed in contact as one of his letters (May 19, 1942) show, where he asked Sarton for the option of publishing a longer manuscript on "The Development of Formal Method in Modern Mathe-

77 Feigl,/Blumberg 1931, Feigl 1968.
78 Feigl 1939, p.27
79 Ibid., p.28.
80 Carnap 1939, p.294.

Fig. 11: Rudolf Carnap (1891-1970)
Source: Institute Vienna Circle

matical Logic" by his student Milton B. Singer in the monograph series of *Osiris*. Singer, together with A. Kaplan and Quine, had served as assistant to the Congress in 1939. Independent of this query, it is remarkable that Carnap's main books were reviewed in *Isis* – despite of the unhistorical conception regarding syntax and semantics – e.g., the favorable review of *The Logical Syntax of Language* (1937) by Henry S. Leonard from Duke University[81]. Five years later Carnap's subsequent main work in English *Introduction to Semantics* was reviewed by Keith R. Symon (Harvard) in *Isis*, which is a remarkable critical appreciation of a book dealing with formal philosophy of science in a journal primarily dedicated to the history of science.[82] But, maybe there was greater convergence of Carnap and Sarton on a different level with regard to their shared commitment to humanism. Whereas Sarton had always endorsed a "new humanism", Carnap, after the publication of the Vienna Circle manifesto's "scientific world conception" in 1929, his personal view broadened to a so-called "scientific humanism" in the age of anti-science and irrational skepticism in the following way:[83]

81 *Isis*, Vol. 29, No.1, July 1938, pp.163-167.
82 Symon 1943, p.229.
83 Carnap 1963, p.83.

> "*The first is the view that man has no supernatural protectors or enemies and that therefore whatever can be done to improve life is the task of man himself. Second, we had the conviction that mankind is able to change the conditions of life in such a way that many of the sufferings of today may be avoided and that the external and the internal situation of life for the individual, the community, and finally for humanity will be essentially improved. The third is the view that all deliberate actions presupposes knowledge of the world, that the scientific method is the best method of acquiring knowledge and that therefore science must be regarded as one of the most valuable instruments for the improvement of life. In Vienna we had no names for these views; if we look for a brief designation in American terminology for the combination of these three convictions, the best would seem to be 'scientific humanism'*".

This invites a comparison with Sarton's "new humanism" as already presented by him in the 1920s, in "The Faith of a Humanist" (*Isis*, 3/1, 1920), "The New Humanism" (*Isis* 6/1, 1924), as an ideal to the defense of which *Isis* is dedicated"[84] based on three or four principles (I. Principles: 1. Human progress is essentially a function of the advance of positive knowledge, 2. The progress of each branch of science is a function of the progress of other sciences, 3.The progress of science is not due to the isolated efforts of a single people but to the combined efforts of all peoples, II. The unity of knowledge, III. The unity of mankind, IV. The history of science, V. The New Humanism), all aiming at unity with the history of science allowing for "a deeper understanding of science, nature, of life."[85]. And this history of science leads to the philosophy called "new humanism" by Sarton, which "derives its main inspiration from the past, yet it is turned towards the future."[86]

At the same time this anticipates the international controversy on the "Two Cultures", prompted by P.C. Snow's lecture in 1959 with his plea for a common approach of the humanities (*Geisteswissenschaften*) and the neglected or ignored natural sciences on the part of the literary intellectuals.[87] Accordingly, only *one* culture of science could bridge the gap

84 Sarton 1924, p.31 ff.
85 Ibid., p.31
86 Ibid., p.34.
87 C.P. Snow 1959.

between the disciplines and fields of knowledge by appreciating the sciences and arts simultaneously. And this dispute again re-appears with different actors and camps in the so-called "science wars" of the late 1990, where two leftist scientists accused postmodern philosophers (but also "relativist" philosophers of science like the Vienna Circle, and astonishingly even Popper) of having eroded the scientific and rational basis of the hard sciences by abandoning all scientific realism.[88] The compatibility of relativism and objectivism – as defended by Frank's *Relativity – A Richer Truth* (with a foreword of Einstein 1952) – appeared once again on the agenda.[89]

The third editor of the IEUS, the neo-pragmatist and Carnap's colleague in Chicago, Charles W. Morris (1901-1979) provided his conception of semiotics (with the triangle of syntax, semantics, and pragmatics) as a structural tool for the *Encyclopedia*, which was accepted and even applied by many

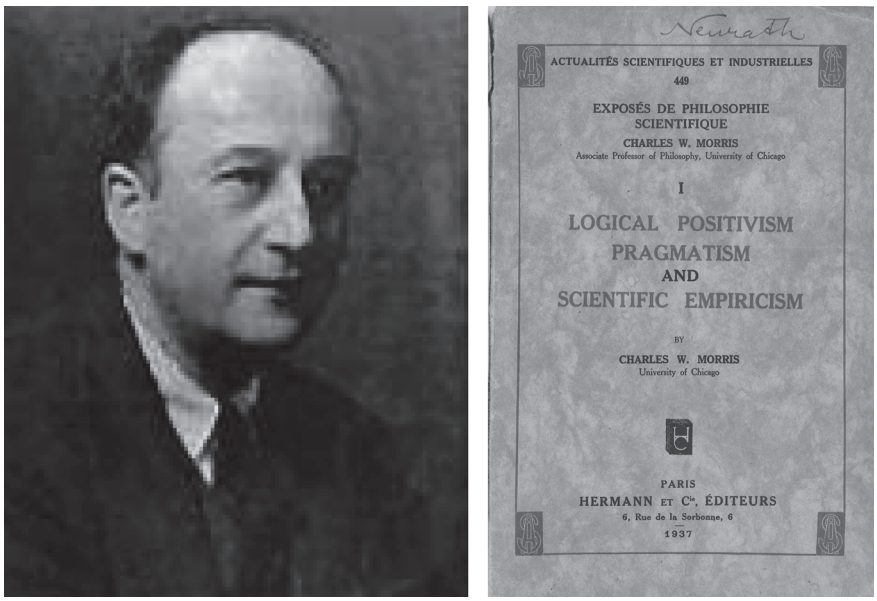

Fig. 12: Charles Morris (1901-1979), Title Page of his Monograph (1937)
Source: Institute Vienna Circle

88 Sokal / Bricmont 1998.
89 Frank 1959

of its contributors. He announced and reported on the Congresses of the Unity of Science and the formation of an International Institute for the Unity of Science (after the WW II founded by Ph. Frank) in *Isis* from 1936 on and continued to serve a co-editor with Carnap after Neurath's unexpected and premature death in 1945. This occurred alongside with Sarton's lifelong efforts to establish an "Institute for the History of Science and Civilization" since 1917, again in 1937 supported by R.A. Millikan, to be located in Harvard.[90] These efforts were argued with the need to humanize science and the aim of secular continuity, the fusion of science and learning (in the sense of Harvard President Conant), the combination of East and West, the inclusion of contemporary science with ethical implications, the unification of good will, the defense of the scientific spirit and method, the insertion of iconography together with museums (similar to Neurath's Isotype movement), to be housed in a large library as the most appropriate institutional location.[91]

Already in 1942, the Harvard historian of science and Sarton's successor as an editor of *Isis*, I.B. Cohen wrote an extensive review of the available monographs of the *Foundations of the Unity of Science* in *Isis*[92]. He later offered an alternative version to Kuhn's *Structure* with his *Revolution in Science* (1985),[93] published in 1942 a detailed review of the first 2 volumes of the IEUS in *Isis*[94], in which he gives a favorable account of Morris' theory of signs in connection with L. Bloomfield's linguistic aspects of science although expressing some doubts. He positively highlights Carnap's pragmatic "principle of tolerance" with respect to the choice of languages/logics, as well as Lenzen's procedures of empirical science exemplified by the development of physics. After addressing to the contribution of the biologist Joseph H. Woodger on theory construction with the rise of modern mathematics and logic since Boole, he deals more intensive with John Dewey's theory of valuation. Here he tries to bridge the gap between humanistic and non-humanistic subjects with the help of a science of valuation, quoting Dewey to the effect that *"in this integration not only*

90 *Isis*, Vol.28/1. 1938.
91 Sarton 1969, pp.169-175.
92 *Isis*, Vol.33, No.6, June 1942, pp.721-723.
93 I.B. Cohen 1985.
94 I.B. Cohen, *Isis,* Vol.33/6, June 1942, pp.721-723

science itself is a value … but it is the supreme means of the valid determination of all valuations in all aspects of social life."[95]

Finally, Cohen praises Ernest Nagel's article on the principles of the theory of probability, which were the subject of controversies within Logical Empiricism between Carnap, R. von Mises, Reichenbach, including their strongest anti-inductivist critic Karl Popper. Cohen ends his review with a correct prognosis that only after the waging war had been won by Western democracy was there a good chance for the realization of the vision of the *Encyclopedia.*[96]

Still in the middle of the war Sarton himself published a related "Defense of the History of Science"[97], based on a talk delivered at the Bicentennial Conference of the University of Pennsylvania, Philadelphia 1941. Here he stated that

> *"Mutual aid and struggle accompany and supplement each other. Love and hatred control our lives, and whatever progress or regress is experienced may be largely measured by the balance of these two forces. If hatred exceeds, we go down, while we go up in the proportion to the excess of love. To put it otherwise, there is possibility of civilization, of progress, or simply happiness, only if the constructive efforts overbalance the destructive ones, if love speaks better and is more active than hatred."*[98]

And Sarton once again recommends "history of science as a tale of increasing co-operation rather than the opposite."[99] This bears striking resemblance to the many references to co-operation and peaceful interaction within the "republic of scholars" expressed by Neurath as a prerequisite for any scholarly life, especially for the idea and vision of the *Encyclopedia* project. Consequently, we find the names of Mach, Bridgman, Duhem, Einstein, Frank, Lenzen, Neurath, Poncaré, Reichenbach, and Abel Rey, listed amongst the recommended authors in the section on "scientific methods and philosophy of science" in Sarton's *Guide to the History of Science* (1952).[100] Surprisingly, though, there is one name missing: Edgar Zilsel.

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95 Ibid., p.722.
96 Cohen, Ibid., p. 723
97 Sarton 1943
98 Ibid., p.465.
99 Ibid.
100 Sarton 1952, pp.86-93.

3. Conclusion: Mach, Sarton and The *International Encyclopedia of Unified Science*

3.1. George Sarton was a typical pre-WW I European intellectual and scholar, who cited amongst his role models the physicist and "Naturforscher" Ernst Mach. Although there was a huge difference in age between the two men they both shared pacifism, socialism and cosmopolitanism on the one hand, and the striving for an empiricist unity of science on the other. In particular, they both emphasized the priority of history over metaphysical philosophy, thereby paving the way for an integrated history and philosophy of science. It is not by accident that the so-called "First Vienna Circle" (1907-1911) associated with Hans Hahn, Otto Neurath, and Philipp Frank focused on a synthesis of empiricism and symbolic logic in order to update Mach's empiricism and pragmatism with French conventionalism including Pierre Duhem, Henri Poincaré, and Abel Rey.[101] Given all these developments it was not surprising that Sarton had approached Mach, asking him to join the committee for his publication project of the journal *Isis* already in 1912 – and that there was a direct exchange between Sarton, Neurath and Frank in exile in Harvard in the context of the Unity of Science movement.

3.2. Both Sarton and the Logical Empiricists shared a commitment to a unity of science and on different levels: Sarton, as part of his efforts to establish the discipline and institution of a history of science itself through *Isis*, *Osiris*, and the "History of Science Society", the Logical Empiricists with their six Congresses for the Unity of Science and the unfinished project of the IEUS from 1938 on in order to provide a new perspective on the sciences from a philosophical *and* historical point of view. Sarton was invited by Neurath to contribute to this international and interdisciplinary project in the spirit of the French *Encyclopédie*. He participated with a keynote talk at the 1939 Congress in Harvard, which has not been published so far. It is mainly because of the outbreak of WW II which endangered the existence of *Isis* and *Osiris* and his enormous work load with his book projects on the history of science, that there was no closer and permanent co-operation. (Instead, Thomas Kuhn contributed in 1962 his *Structure of Scientific Revolutions*, which was later mistakenly regarded as

[101] Frank 1949, pp.1-52.

an absolute alternative to the IEUS). Nevertheless, the correspondence and personal contacts between Sarton and Rudolf Carnap, Philipp Frank, Heinrich Gomperz, Richard von Mises, Otto Neurath, Edgar Zilsel and other members of the former Vienna Circle show the potential of such an interaction in "normal science". The convergence with American (neo-)pragmatists, especially with Ch. Morris, also informed by P.W. Bridgman, J. Dewey, E. Nagel, and W.V.O. Quine and others, was not strong enough for a closer fusion and sustainable establishment of both ambitious projects. The communication between Sarton and Zilsel, in particular, manifests the fate of a failed cooperation of two historians and philosophers of science in exile despite strong common efforts and visions. One is tempted to raise the counterfactual question "What would have occurred in the postwar period with history and philosophy of science?", if all these networks and collaborations would have succeeded in the long run? One wonders what the reasons for this break were for the realization of these two encyclopedic and unfinished projects in exile after WW I and WW II.

3.3. It seems that after the destructions of WW II the Cold War period changed and marginalized both projects coming from Europe as representative of "late Enlightenment". Both the focus on history of science and the claim for a unity of science came under personal, political and scholarly pressure as described by George Reisch in his splendid book on philosophy of science in the Cold War period.[102] These developments were investigated in a wider context in the book *Pursuing the Unity of Science* (2016).[103] Here we find contributions by Peter Galison, Bert Theunissen, and also George Reisch who all together provide an explanatory backdrop of the rise and fall of the unity of science "from the Great War to the Cold War" as the subtitle reads. It was mainly the skepticism towards leftist and Jewish European intellectuals and émigrés which led to political surveillance by the FBI and in academia to objections towards any "relativist" philosophy and science as one imputed reason for the rise of totalitarianism and NS. In addition, the cooperation of philosophers and scientists in the war period including the US Army led to a wholesale de-legitimation of the unity of science. A "post-positivistic" attitude was favored, followed by a postmodern conception of the sciences and humanities with the rejection of

[102] Reisch 2005.
[103] Kamminga /Somsen 2016.

pure empiricism, naturalism, and pragmatism as a theoretical frame. This tendency has been described convincingly by Gerald Holton who was Philipp Frank's assistant in Harvard, in his *Science and Anti-Science* (1993) and in his related articles covering the life and work of Mach, Einstein and the Vienna Circle in exile.[104] For instance, on "Mach in America" he mentions B.F. Skinner's close reading of Mach inspired by Sarton's lectures and describes the intellectual journey "From the Vienna Circle to Harvard Square" in exile.[105] He identifies an "ecological niche" for Logical Empiricism in the US as a convergence of pragmatism and the logical empiricists in the wake of their forced migration. And in his later account on unity and disunity (*Einheit* und *Vielheit*) in the sciences between absolutism and relativism in defense of a re-conceptualization of the former in our globalized world he again refers favorably to Frank's book on relativity (1950) and Sarton as an unknown ally of the Vienna Circle[106] – as appears already in Sarton's introductory article in *Isis* on the history of science (1913). By the way, the claim for synthesis appears in the name of the journal *Synthese* (1936-39), published in the Netherlands until WW II which included articles of Vienna Circle members like Neurath on "Einheitswissenschaft als empiristische Synthese" (1938, 18 f.) and reports on Otto Neurath's Institute for the Unity of Science, which was established in The Hague, prior to his adventurous escape to the UK in 1940. In the last issues of this journal a "Unity of Science Forum" was included with a report on the Unity of Science movement in the US (Nov. 1938). After the war it continued to be published in three languages, opening with the most likely last article of Neurath on the Unity of Science Movement, dated December 19th, 1945.[107] Subsequently, we find papers by L.E.J. Brouwer on the Dutch signific movement (*Signifiker* around G. Mannoury), by Morris on science and discourse, by Bridgman on the operational aspect of meaning as well as on philosophical aspects of science, by Quine on Carnap's logical truth, and still in 1960 by Morrris "On the History of the IEUS".[108] I agree with Holton that this was obviously in keeping with Sarton's programmatic claim for *Isis* to be a journal promoting synthesis in the spirit of the former "Berlin Society for Positivistic Philosophy".

[104] Holton 1992, 1993b. (Isis 83, 1992)
[105] Holton 1993a.
[106] Holton 2010.
[107] Neurath 1946, pp.77-82.
[108] Morris 1960, pp. 517-521.

Holton's successor Peter Galison published alternative studies on the unity of science in cultural context privileging *The Disunity of Science.*[109] This could be seen as a counter-movement after the period of the "received view" of scientific theories. And this is only one symptom for Sarton's disappearance in postmodern historiography of science in the wake of Kuhn: e.g., in Steven Shapin's *The Scientific Revolution* (1996)[110] he is not even mentioned within the "great tradition". Most likely, the lack of conceptual history and historical epistemology was certainly another reason for this break in addition to Sarton's preference for (rational) progress.

I think, all these developments are related to the changes brought on by the transfer and transformation of philosophy of science from Europe to America between 1930 and 1960, which I have reconstructed in detail in 2007, here only with a reference only to 2 results:[111]

- The already existing trans-Atlantic connections since the turn of the century 1900 enabled a later fusion of the history *and* philosophy of science including the syntax, semantics and pragmatics of all science. This process was reinforced by US neo-pragmatism and neo-behaviorism.
- There was an early anticipation of the "historical turn" which was hidden by the success story of Quine's "Two Dogmas of Empiricism" (1951), and Kuhn's *Structure* (1962) as a "paradigm change" from a normative analytic and formal philosophy of science to the emergence of historical and sociological conceptions in philosophy of science since the 1960s.

  Ernst Mach had been an ideal type scientist and role model for the young Sarton since his studies in Ghent: internationalism and cosmopolitanism with an interdisciplinary conception of the sciences aiming at unity with an "historical turn" remained relevant till the end of his life, which was shaped biographically and intellectually by his (pre)war experiences in Europe. Both scientists were inspired by, and were part of modernist circles in Vienna, Ghent and Brussels. Neurath, like Sarton, also communicated with Paul Otlet regarding his

[109] Galison / Stump 1996.
[110] Shapin 1996.
[111] Stadler 2007

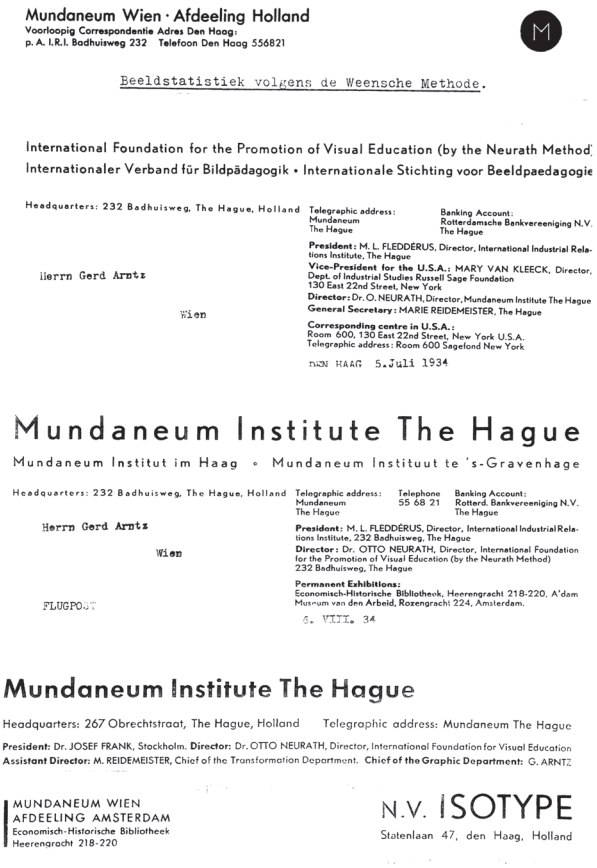

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Fig. 13: Leaflet of Otto Neurath's Mundaneum Institute in The Hague
Source: Institute Vienna Circle

efforts to document and structure knowledge as a precursor of our information age and as a proto-type of the Internet in the knowledge society.[112] Already in 1931 Neurath had founded the "Mundaneum Institute The Hague" inspired by Otlet's "Palais Mondial" for the international promotion and dissemination of his Isotype movement in Dutch and after 1940 in British exile, where he pioneered the application of Isotype to film productions with the renowned film maker Paul Rotha.[113] Prior to this, Neurath and Otlet had collaborated on a

112 Pyenson / Verbruggen 2009.
113 Boon 2016.

joint project to produce an international atlas of civilization. In 1933 Neurath established the "International Foundation for Education" anticipating his emigration to the Netherlands.[114] Otlet and Neurath can thus be seen as fathers of the "visual turn", which was to be proclaimed decades later in contemporary cultural studies.

- In the 1930s we may characterize the two pioneers in the history of sciences and philosophy of science with an asymptotic move towards HPS, namely Sarton and Neurath, as congenial intellectuals and scholars who had a similar life experience (war, emigration, social reform, organizers and programmatic thinkers, big international and interdisciplinary projects, which failed for similar reasons, etc.). The former as the proponent of history of science as an overarching discipline, the latter with the empiricist encyclopedia of unified science as a modernized version of the *Encyclopédie* covering all disciplines connected through empirical language (words and pictorial languages) and semiotics in the emerging knowledge society. We could speak of a "new humanism"[115] (Sarton 1924) on the one hand, and "new encyclopedia" (Neurath 1937)[116] on the other. Apart from this intellectual family resemblance, there are also striking biographical similarities (forced migrations, war experiences, science in exile, transatlantic co-operations etc.), which merit further study.
- This comparison applies in part also to that of Sarton and Zilsel, who only found posthumous recognition by prominent historians and sociologists of science, such as Joseph Needham (and Robert K. Merton). Zilsel's lifework was negatively impacted by his exile situation and even his studies on the origins of modern science were written under the most precarious circumstances in the US. In fact, he was not introduced to the scientific community in America and did not meet the leading figures Talcott Parsons or Robert K. Merton in Harvard. Sarton recognized the potential and brilliance of his colleague, who misjudged Sarton's academic status at Harvard while trying to find an institutional affiliation for his research project. Maybe Zilsel was an

114 Sandner 2014, p.234 f.
115 Sarton 1924
116 Neurath 1937

excellent scientist but a clumsy man[117], even if this can be also explained and understood by external factors. Only with the posthumous volume *The Social Origins of Modern Science* (2000), a collection of his dislocated papers and documents, was the importance of Zilsel's oeuvre confirmed by Joseph Needham's reference to his own studies on *Science and Civilizations in China*:[118]

> "*All we can be sure of, and this is where Zilsel's work is a veritable torch to light the darkness, is that we have to look for the 'social roots' as well as the purely intellectual ones, of science and technology, whether it be in the West or in the East. Fiat lux, we all cry, and Edgar Zilsel's life and work put him among the most notable taperers in the procession of those who seek to understand*".

- The exchange between Sarton and Frank was more felicitous for several reasons: both were located, and also had appointments in Harvard, being involved in the science teaching program. Both were relative successful in establishing two institutions in addition, even if not as Departments of Harvard University: the "History of Science Society", followed by the "Institute for History of Science", and the "Institute for the Unity of Science". The communication, exchange, and cooperation flourished more or less through the journal *Isis* before and after the 5th Congress in Harvard. Nevertheless, it did not lead up to a cooperation project, given that there were too many different commitments on both sides and different research interests in the field of philosophy, history and sociology of the sciences. One could characterize both men as friendly intellectual neighbors with a similar cultural background from good old Europe. Both acknowledged the societal roots of science vis á vis religion and the arts as a joint cultural phenomenon of mankind and both, similar to Dewey, were concerned about the democratic context and consequences of each discipline. The two scholars could have been able to anticipate the controversy about the "two cultures" debate (P.C. Snow) and Kuhn's challenge emerging in the 1960s. They would have also been partners in the subsequent "science wars" in defending the historical embed-

[117] Fleck 2015, pp. 251-294. Most recently on the Viennese roots of Zilsel's exile studies see Romizi 2018.
[118] Needham 2000, p.xiv.

dedness of the sciences, never questioning the basic search for objectivity and truth. The vision of their Harvard colleague Richard von Mises as a general approach for the "study in human understanding" was a welcome option for both Frank and Sarton, who had pressed the former so strongly to review it in his journal.

- Regarding the huge publication projects on both sides we may draw the conclusion that both were certainly cosmopolitan and unifying by virtue of their multi-lingual, multi-ethnic, international, and pluralist conception. Their fate was also a shared one: with the death of the founders and editors (Sarton and Neurath) a substantial break happened regarding the program, authors and planning. Even if the main journals (*Isis, Osiris, Erkenntnis*) still exist today, we can speak of a cognitive rupture and change due to new generations of science and historical-social circumstances: it is significantly *less philosophy* on the one side, and *less history*, on the other. Sarton as the driving individual organizer was unable to finish his ambitious *Introduction to the History of Science*, just as Neurath, the organizer of a collective, could not finish his IEUS. Both projects remind us of an ambitious and creative cooperation in really hard times but also of the contingency of history and science in general. And the reconstruction and evaluation of this story with all its breaks and continuities still remains the job of historians of philosophy *and* science – for the sake of our own present cognitive identity in an age of nationalisms and violent conflicts in the globalized world, where science and its history are also coming under pressure.

So, let's not forget the message and vision of Sarton, Neurath and their colleagues with the wise insight of Robert Merton that we all in the scientific community are still working "on the shoulders of giants".[119]

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[119] Merton 1965. With references to Sarton's related research.

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Appendix: Publications of Mach in German

[1] Mach, Ernst 1872. *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit.* Prag: J.G. Calve'sche k.k. Univ.-Buchhandlung.

[2] English translation: *History and Root of the Principle of the Conservation of Energy*. Transl. from the German and annotated by Philip E.B. Jourdain. Chicago: Open Court – London: Kegan Paul 1911.

[3] Mach's main books are republished with introductions and annotations in the running *Ernst Mach Studienausgabe* (Berlin: xenomoi), ed. by Friedrich Stadler, together with Michael Heidelberger, Dieter Hoffmann, Elisabeth Nemeth, Wolfgang Reiter, Jürgen Renn, and Gereon Wolters:

[4] *Die Analyse der Empfindungen und das Verhältnis des Physischen zum Psychischen* (1886). Hrsg. von Gereon Wolters 2008.

[5] *Erkenntnis und Irrtum. Skizzen zur Psychologie der Forschung* (1905). Hrsg. von Elisabeth Nemeth und Friedrich Stadler 2011. English translation: *Knowledge and Error.* Sketches on the Psychology of Enquiry. With an Introduction by Erwin N. Hiebert. Dordrecht-Boston: 1976.

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[8] *Die Prinzipien der Wärmelehre* (1896). Hrsg. von Michael Heidelberger und Wolfgang Reiter 2016.

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Laudatio Jules Leroy

Dirk Matthys & Johan Vande Walle

Jules Leroy was born in Deinze in 1934. He raised up in a traditionally Flemish and large family.

He is not only a driven academician, but he loves arts, classic music and litterature as well.

He is an excellent speaker, very ambitious and always wants to "win".

Jules Leroy realised to make bridges between the different University Paediatric Departments, which was not easy at that time in Belgium.

As a consequence, there came "peace" between the paediatric departments of the universities and "De Vlaamse Vereniging voor Kindergeneeskunde", the Flemish scientific and professional association.

That gave a "boost" to the development of paediatrics in Flanders.

Jules Leroy is very proud on his family and often speaks about his father, his mother who died at a very young age, his wife and this two children.

He studied medicine at Gent University.

During his professional career he always balanced between two different options:

- a classical training in paediatrics (Prof. C. Hooft) or fundamental research at the Heymans Institute (Nobel price winner);
- a "safe" training in Belgium or un "uncertain" future in het USA;
- when coming back from the USA to Belgium, again opting for safety in Ghent or starting up a new department in Antwerp.

Finally, he was nominated head of the department of Paediatrics at the University Hospital of Ghent, and full Professor at Gent University.

He was also head of the department of Genetics in Ghent.

The genetic department obtained international importance and the paediatric department became a well-known tertiary center.

Although paediatrics at that time were merely descriptive, Jules Leroy was innovative, scientific, with accents on genetics and metabolism. He founded diagnosis and treatment upon a thorough knowledge of the basic sciences. Decades later, it was called "translational" research.

He considered genetic counseling to be extremely important. Scientific knowledge and human empathy are here joined together.

If G. Mendel was the founding father of the genetics, Jules Leroy contributed in a large part to the development of modern genetics and paediatrics at Gent University.

Jules Leroy certainly deserves the Sarton medal.

Ghent rediscovers Mendel

Jules Leroy

As a former teacher of a comprehensive course in general Genetics to undergraduate students at the Antwerp University, the introductory lecture was on the experimental work with the garden pea by Gregor Mendel (1822-1884) an Austrian monk, who published the results and conclusions of his work in his native German language (Mendel JG1866). His paper was not understood even by his former teachers and the community of scientific botanists at the time and was largely forgotten for almost 35 years. Mendel's famous laws of single gene inheritance were nearly simultaneously and independently rediscovered and reported in 1900 by three botanists, De Vries of the Netherlands, Correns of Germany, and Tschermak von Seisenegg (1871-1962), a postdoctoral fellow of Vienna, Austria, who performed in 1898-99 part of his experimental work at the Ghent, Botany Institute Ghent, Belgium. As a teacher at the Antwerp University I was not aware of that last fact, although in my course the topics of the rediscovery and of the elucidation by Landststeiner of the ABO-human blood groups also published in 1900, were extensively discussed. The year1900 marks the start of the science, Human Genetics.

As this calendar year marks the bicentennial anniversary of the foundation by King William I of the Netherlands, of the University of Ghent, I thought it to be of interest to inform a larger audience of the contribution the UGent has made more than a century ago as the host to the Austrian postdoctoral student, the youngest co-rediscoverer of the Mendel laws of heredity.

Apologising to the readers with a biological background the essential aspects of the rediscovery cannot be presented unless Mendel's work and conclusions are fully explained. It is equally important to stress that

Mendel's insight in biology, the background and inspiration to his experiments differed favorably from that of the contemporary scientists and predecessors.

Before taking some steps into the past of the science of biology, it is important that the definition or concept of Heredity is understood as the phenomenon in nature that parent organisms beget offspring, which resemble them. "Like begets like". Any offspring is similar, but not identical to the parents. Likeness and difference, identity and variation are inseparable biological concepts, as is either side of any coin. Heredity of any feature in the kingdom of biology would not be discernable if all organisms in nature or all members of a single biological species would be identical. In such circumstance variant species would not even exist. Genetics is inconceivable without the observation of diversity and resemblance, without the well known variability in nature. Darwin's initial theory of the evolution of species driven by natural selection is based on variation, diversity within the various species in nature.

Johannes Gregor Mendel, born in July 1822 was the only son of three children in the family of humble farmers in the village of Heinzendorf (Hyncice, Czechia) located in the "Kuhländchen" of the part of Silesia, which remained Austrian following the Silesian wars in the 18th century. Mendel's family was very occupied with horticulture and attempts at biologic improvement of several species of plants with either nutritional or decorative value. From a young age, the boy must have learned from his father the involved experimental procedure of cross-fertilization in plants, almost all of which were known to use self-fertilization ("selfing") for procreation. Already in the local grade school Gregor's superior intelligence was noticed. He completed successfully the six years of gymnasium program (Latin-Greek section) in Troppau (Opava, Cz.). Natural history was not yet a formal topic of study. However his specific interest and background were kindled by several of his teachers and even by the parish priest, all of whom were somehow linked to the local industry of plant improvement. Shortly after his registration as an undergraduate student at the Philosophisches Institut of the university of Olmütz (Olomouc, Cz.), his father had a severe physical accident leaving him incapacitated and unable to continue his trade. It has even led to his untimely death. The family could no longer support Gregor's studying. Because of overexertion

by several remedial teaching jobs and other hard work, the young man became seriously ill and for this additional reason had to interrupt his study for the better part of an entire year. With the financial support of his younger sister Mendel could resume these studies and end the two year program succesfully. The results included the highest grades in mathematics and physics. Mendel could only dream of further higher education unless he accepted the opportunity offered to study for the priesthood with the Augustinian monks at Brno (at present Czechia, originally Brünn, Austria). Ordained a priest in 1847, he was still a student at the newly created Physics Institute, University of Vienna, where also botany and literature studies in Biology, Heredity and Evolution drew his full attention. For sure that is where the theoretical background for his future experimental work had been acquired. His acquaintance with the work of JG Kölreuter dates back to this period. Surprisingly, Mendel failed in several exams and did not graduate in Vienna. The reason was his paralyzing shyness, overexertion and insecurity in examination circumstances. When back at Brünn, it was clear to his superiors that he was an exceptional teacher but a poor fit for practical pastoral duties. He was assigned some undergraduate teaching and while particularly effective as an abstract and analytical designer of experimental work, given a major role in agriculturally maintaining and improving the large set of garden and nutritional plants in the monastery's garden. That is where Mendel's experiments were performed between 1853 and 1867. In the later part of his life, Gregor Mendel was elected abbot and encountered much administrative difficulties with the Austrian government. Throughout his adult life Gregor Mendel had a number of other active study interests: meteorology; apiculture; fruit growing; viniculture. He died in 1884.

Mendel's work must be viewed in its proper era of time. He was well aware of the relevant scientific achievements made in the preceding centuries. Corrective lenses were developed for telescope and microscope during the 17th century. Robert Hooke (UK) (1665) and Antoni van Leeuwenhoek (Nl)(1677) went on to construct the first microscopes and consequently discovered the existence of unicellular organisms, the protozoa. Human sperm cells were observed for the first time. Sexual reproduction in plants has been discovered by Rudolf Camerarius in 1694, following his microscopic description of pollen. In 1751 the famous Swedish biologist, Carl

Linnaeus had published his extensive systematic order of all known plants and his claim that hybridization only occasionally may lead to the appearance of new species.

Throughout the 17th and 18th centuries interest in "Speciation" in nature became prominent. Many hybridization experiments in plants and even in animals were reported within the framework of this type of thinking. This era was also the time that several rather philosophical texts on evolution of species and creationism were published, all of them with plenty of speculation but poor in or devoid of objective observation of nature.

Mendel had no difficulty accepting three conclusions accurately summarized by the German investigator Joseph G Kölreuter (1733-1806): 1. Hybrids generated following inter-species crossing usually have a phenotype intermediate to that of either parent; 2. The hybrid offspring is sterile precluding further crossing experiments; 3. Reciprocal crosses regarding sex of chosen parents yield approximately similar phenotypes in the hybrids. It must be stressed however that in past centuries discerning the biological concepts of "species" and "race" or "variant within a species" in nature was often challenging and still is in some instances today.

In his most important publication Mendel refers to the "Blending theory" of heredity, which had its origin also in some experimental work described by Kölreuter in 1765. He had studied microscopically the in vitro germination of pollen. In purely aqueous and hence hypotonic medium plasmolysis of the cells occurred promptly. For Kölreuter this meant that the fluid released was the likely "fertilizing matter" that would mix with a similar fluid produced by the ovum and thus achieve fertilization. This hypothesis has led to the "Blending of fluids theory" that claimed fertilization to be the consequence of irreversible mixing of fluids, theory that was still accepted as probable by some 19th century investigators.

It is time to compare in Table 1 the methods, subjects, experimental design and abstract ideas between the hybridization experiments by Mendel with the ones by most of his scientific forebears.

Table 1. Comparison of Hybridization Experiments between Mendel and "scientific" Forebears; Subjects; Conditions; Rationale; Methodology

Earlier & Contemporary BIOLOGISTS	MENDEL
1. Inter-species Crossing: sterile hybrids	1. Intra-species Crossing: fertile Hybrids
2. Most often Animal Species with long generation Time	2. Garden Pea (Pisum sativum): easy culture; short Generation Time
3. "Holistic" Approach; required description of complete Phenotypes	3. Single or only few phenotypic Characteristics, "pre"-studied in pure Varieties: "atomistic" Approach of Heredity
4. Few if any measurable Results	4. Strongly contrasting phenotypic Features, easily discernable and measurable
5. Small Number of Offspring	5. Large Numbers in Offspring Generations
6. Lack of counting Individuals in various Classes of Offspring	6. Carefully counts each Subject in each phenotypic Class of Offspring. Interested in Ratios of Numbers in each Class;
Even some intra-species Hybridizers failed counting Offspring Classes: • Wright (1759-1838)(UK) 1823 • Naudin (1815 -1899(Fr) 1864 found more Variation in F2 than in F1 Generation of Offspring	

Mendel chose Pisum sativum, the garden pea, as his most important study subject for obvious reasons. He knew the species well since his youth through the practical instructions by his father, who had observed, gathered and selected several through breeding (so-called pure) varieties, most of them differing from the wild-type (reference type) by only one clearly different, even contrasting feature that was easily recognizable and countable. One example regards the shape of the mature seed being either round or wrinkled. All seven contrasting features used separately or in some combined fashion by Mendel are listed in Table 2.

Cross-fertilization requires physical intrusion on plant flowers by removal of the terminal parts of the stamina, the anthers consisting of two lobes each one with a sac of pollen. Obviously applying pollen derived from another phenotypically different plant to the stigma must be the second step. The stigma is the name in botany of the terminal part of the ovary located at the

Table 2. Varieties of Pisum sativum Mendel used as Parents (P1) in cross-fertilizing breeding Experiments

Feature in Plant Part of interest	Contrasting Phenotype in P1	Resulting F1 Phenotype
Seed Shape	Round(W); Wrinkled(w)	Round
Seed Color	Yellow (G); Green (g)	Yellow
Flower Color	Colored (P); White (p)	Colored
Pod Shape	Inflated (C); constricted(c)	Inflated
Pod Color	Green (Y); Yellow (y)	Green
Flower and Pod Position	Axial (T); Terminal (t)	Axial
Stem Length	Long (L): Dwarf (l)	Long

end of the style, where pollen is deposited in order to enter the ovarian tissue.

Cross-fertilization of a pure round seed Pisum variant with a pure wrinkled seed variant as parents (P1) had yielded hybrid offspring (F1: first filial generation) with round seeds exclusively. Then Mendel performed "selfing" tests with these F1 plants as parents and thus generated the second generation of offspring (F2). He counted 5474 plants with round peas and 1850 with wrinkled peas and recognized a close approximation to a 3: 1 ratio. The trait "wrinkled pea seeds" disappears in the monohybrid F1 generation but reappears in unchanged manner in the F2 offspring.

He went on performing many more monohybridization experiments using parental couples (P1) that only differed by one other set of contrasting characteristics. In each of the experiments he obtained two F2 phenotypes approximatively in the same 3:1 ratio.

Table 3. Results from Mendel's Monohybrid (F1) Crosses → (F2)

Varieties of Peas used as Parents (P1) differing in only **a single qualitative phenotypic feature**

Traits in Parents (P1)	Trait in F1	Numbers & Traits in F2	F2 Ratio
round x wrinkled (seeds)	round	5474 round; 1850 wrinkled	2,96 : 1
yellow x green(seeds)	yellow	6022 yellow; 2001 green	3,01 : 1
light purple x white (flowers)	light purple	705 purple; 224 white	3,15 : 1
Inflated x constricted (pods)	inflated	882 inflated; 299 constricted	2,95 : 1
green x yellow (unripe pods)	green	428 green; 152 yellow	2,82 : 1
axial x terminal (flower position)	axial	651 axial ; 207 terminal	3,14 : 1
long x short (stems)	long	787 long; 277 short	2,84 : 1

X : symbol for cross-fertilization; "selfing" in F1 plants leads to F2 plants

When cross-fertilizing the F1 hybrid plants obtained in the previous experiment with the non-dominant P1 plants, Mendel predicted that equal numbers of two offspring types wound be obtained. The new F2 result was indeed: 193 round seed and 192 wrinkled seed offspring in the obvious 1/1 ratio. Such results were also consistently found in several back crossings using the other F1 hybrids and corresponding non-dominant P1 parents (Table 4).

Equal numbers of two phenotypes were also found following similar test-crossing experiments using some other variants as listed in Table 2.

Mendel's first set of conclusions were discussed thoroughly in his major publication (Mendel JG 1866) and form the basis of his first law of monogenic heredity.

The letter symbols used had more than a single didactic, abstract value. They were the symbols of what he called "hereditary determinants" ("Genes") and also of their phenotypic expression as contrasting qualitative traits, features or characteristics.

He clearly discerned physical appearance ("Phenotype") and hereditary constitution ("Genotype") in the species Pisum sativum, although he did not coin the terms genotype and phenotype. The hereditary determinants must be doubly represented in plant somatic cells, but only singly in

Table 4. Testcrossing (P1 nondominant) x F1) by Mendel

P1 → x → **F1** ↓	*genotype*: ww *fenotype*: wrinkled **gametes**: w(1)	**offspring (F2)** *genotype*	*phenotype*
genotype: Ww *fenotype*: round	W (1/2) w (1/2)	Ww ww	round (1/2) wrinkled (1/2)

germinal cells. They had been freely exchangeable and transferred independently, which is at complete variance of the "Mixing Theory" of heredity. Mendel defined the concepts of "Dominant" and of "Non-dominant" trait more precisely, but did not use the terms themselves. He also understood fully the terms homozygous (ex.: WW or ww) and heterozygous genotype (ex.: Ww) as he discerned since childhood the difference between pure races and hybrid variants.

Mendel's study background included knowledge of Schleiden and Schwann's Cell Theory stating that organisms are composed of cells and that any cell arises by division of a previously existing cell. In 1865 the author who provided proof of the concept "Segregation" during gametogenesis did not know about Mitosis and Meiosis (reduction division during gametogenesis) discovered in 1879 (Flemming) and 1875 (Hertwig) respectively. Chromosomes were first observed and reported in 1879 (Flemming). Mendel already discerned the concepts of "soma " and "germen" well before these have been defined more strictly by Weissman in 1887.

The first Law of monogenic heredity, Mendel's first law, the SEGREGATION LAW consists of three quintessential parts:

1. "Hereditary Determinants" are particulate: "Particles", not irreversibly mixable fluids, but separable physical elements.

2. "Hereditary Determinants" are present in Pairs in somatic Tissues.

3. Upon maturation of "Germ cells" into Gametes both elements of one Pair separate from one another (SEGREGATION) in such a way that only one of both determinants will end up in each Gamete.

Mendel went on designing cross-fertilization (out-crossing) experiments using pure Pisum races phenotypically differing by two instead of only one contrasting characteristic. If the causal hereditary determinants would segregate independently from one another into the gametes formed by self fertilization in the F1 dihybrid, the mathematician Mendel invoked the law of combining the probability of events that occur independently from one another and predicted that the ratios among the four phenotypes expected in the F2 generation would be 9:3:3:1. One example among several similar experiments performed by Mendel is provided in Table 5. The predicted F2 phenotypic ratios were indeed found among the F2 offspring.

Once more Mendel performed the corresponding test experiments where the F1 dihybrid variant was backcrossed to the non-dominant P1 pure variant. As predicted Mendel found in this F2 offspring plants equal numbers of the four phenotypic classes (1/4 of the total for each one) discerned in the previous experiment that is shown in Table 5.

Table 5. One Example of Mendel's Out-crossing Experiments and independent Segregation in Dihybrid

P1	genotype: WWGG phenotype: Round,Yellow			wwgg wrinkled, green	
gametes	**WG**		**x**	**wg**	
F1 genotype: phenotype:	F1 WwGg Round, Yellow "selfing"				
gametes		**WG**	**Wg**	**wG**	**wg**
WG	F2	WWGG Round,Yellow	WWGg Round, Yellow	WwGG Round, Yellow	WwGg Round,Yellow
Wg		WWGg Round, Yellow	*WWgg* *Round, green*	WwGg Round, Yellow	*Wwgg* *Round, green*
wG		WwGG Round Yellow	WwGg Round, Yellow	wwGG wrinkled, Yellow	wwGg wrinkled, Yellow
wg		WwGg Round, Yellow	*Wwgg* *Round, green*	wwGg wrinkled, Yellow	wwgg, wrinkled, green

This backcross (WwGw x wwgg) resulting in the F2 generation comprising equal numbers of four phenotypic classes (WwGg: Round, Yellow (1/4); Wwgg: Round, green (1/4); wwGg: wrinkled, Yellow (1/4); wwgg: wrinkled, green(1/4) provided proof of independent segregation of the hereditary determinants under study and kindled the formulation of Mendel's second law, the one about independent segregation.

In the event of independent segregation of two pairs of genes, one observes in the F2 generation by testcrossing equal numbers of "new" combinations of the phenotypic traits (Round, green) and (wrinkled, Yellow) as of the P1 parental combinations (Round, Yellow) and (wrinkled, green). The finding of 50% new combinations, also called recombinants or the results of recombination proves that the genes involved segregate independently. If however more parental combinations than recombinants would be found following similar experimental crosses, "Linkage" of the genes involved is probable and independent segregation must be excluded. Mendel found independent segregation of the hereditary determinants for all seven contrasting pairs of pure single variants at his disposition. Much later has been shown that all 7 "Determinants" encoding the 7 varieties with pairs of contrasting phenotypic features are located on different chromosomes in Pisum sativum (2N =14). Hence "linkage" or lack of independent assortment or segregation would not have been detectable by Mendel's experiments.

Second Mendel law: independent Segregation Law

Segregation of one Pair of "hereditary Determinants" ("Genes") at Gametogenesis occurs independently from the Segregation of another Pair of "hereditary Determinants" ("Genes").

It must be pointed out also that the numerical ratios of phenotypes among the F2 offspring (WG: 9; Wg: 3; wG: 3; wg: 1) or in general (AB: 9; Ab: 3; aB: 3; ab:1) in the dihybrid "selfing" WwGg x WwGg) (experiment in Table 5) or (AaBb x AaBb) and the phenotype ratios (WG:1; Wg:1; wG:1; wg:1) or (AB:1; Ab:1; aB:1; ab:1) following the testcrossing (WwGw x wwgg) or (AaBb x aabb) are expected only when W or A is completely dominant in the phenotype over w or a as should also G or B be over g or b.

Mendel has presented his set of results and conclusions orally in front of the gathered members of a local scientific association in Brünn (1865) and has published them formally as "Versuche über Pflanzen Hybriden" (1866) in "Verhandlung der Naturforschenden Vereines in Brünn" 4: 3-47. Only less than 3 dozen reprints of the original text in German were made available. The journal itself had only the limited circulation of 500 copies, 115 of which were sent to scientific institutes or German and Austrian University Libraries. Of Mendel's famous 1866 paper only 40 copies were made. A few of the latter were sent to some of the author's ex-teachers, potentiallly interested in the results. One of them was Professor Carl Nägeli, Professor of Botany at München, a former teacher of Mendel at the Vienna University.

Mendel had written several letters to Prof. Nägeli, but did not receive replies. Late in his period of experimentation with Pisum, he had even sent a well organized large package of Pisum seeds to Nägeli with the strong recommendation that he carries out experiments in order to confirm selected parts of his work at the Altbrünn monastery. Nägeli did barely pay attention to the materials sent and instead asked G. Mendel to perform repetitious experiments in "Hieracium" (Hawkweed) as in München no valuable results were obtained. There are several direct and indirect indications that Nägeli did not understand the significance of Mendel's work and conclusions. Mendel did perform sufficient work with Hieracium, but also did not obtain any result even close to what had been found in Pisum. This was a source of concern to Mendel, who started doubting that segregation and independent segregation of genes would not be a general rule or biological law in plants. Much later it has become known that Hieracium propagates itself by parthenogenesis in addition to the classic sexual hereditary transmission. This precludes finding consistent phenotypic ratios among offspring of particular crosses.

Mendel's work was quoted only few times between 1866 and 1900, but not really understood by most people not conversant with the German language. For sure the language problem was not the sole difficulty. It was mentioned in the German book by Focke (1881) that had been referred to in the US book on plant breeding by Bailey (1895). The "rediscoverers" claimed to have read the work of Mendel through the reference in Bailey after at least part of the own experimental work had already been performed.

The period of time between 1866 and 1900 can be characterized by major progress in cytology and cytogenetics, including the discovery of chromosomes, of meiosis and mitosis. Significant progress had been made in the quality of microscopes during the last half of the 19th century. Regarding the subject at hand, the impact of Charles Darwin's theory on Evolution (Darwin 1859) including the questions he did not know the answer to (Darwin 1868) was very important as well as the ever more fashionable experimental hybridization work undertaken by biologists in many European countries.

The Darwin (1809-1882) theory of Evolution also called Darwinism (Darwin 1859) holds that evolution is resulting from the forces of "Natural Selection" of the best environmentally adapted species and of the best adapted race (variety) within any species. Darwin's theory held that natural selection would not work without variation in nature, more specifically without hereditary variation. He readily admitted that he did not know how such variation arises and how such variation is inherited. Mendel's work was unknown to Darwin. However G. Mendel made the comment in his segregation paper that most likely his results in peas would be germane to the questions Darwin did not know the answer to.

Because of his ignorance about the cause of hereditary variation in nature Darwin revived and adapted the ancient "Pangenesis"theory of the "Gemmules", representative elements extracted from all body organs that circulate in the organism, collect within gonads and are thus transmitted to the next generation. He reluctantly accepted the possibility that traits acquired during life may occasionally be heritable (Lamarck theory). Darwin even started small hybridization experiments himself. E. Tschermak von Seisenegg, the main subject of the concluding paragraphs of this text, has judged Darwin' numbers of experimental subjects too small in order to warrant any meaningful conclusion. His decision to repeat these experiments with much larger numbers of plants made him ultimately one of the three rediscoverers of Mendel's laws of heredity.

During the last decade of the 19th century, three plant biologists were independently from one another performing hybridization experiments with various plant species and by carefully recording of the numerical results discovered segregation of variant features in the F2 offspring of monohy-

brids. All three claimed to have found Mendel's work in the professional literature after they themselves had already completed major parts of the experimental work. The three scientists were: Hugo de Vries (1848-1935) (The Netherlands; university of Leiden), Carl Correns (1864-1933) (Germany; university of Tübingen; Erich Tschermak von Seisenegg (1871-1961) (Austria, university of Vienna, temporarily visiting and working as graduate student in Ghent: 1898-1899).

All three have published their results in volume 18 (1900) of the German scientific journal: "Berichte der Deutschen Botanischen Gesellschaft,).

H de Vries reported a large and impressive set of results of intra-species crosses in no less than 11 different species, but no dihybrid crosses were included. The work had been started around 1890. De Vries had consulted the then new book by Bailey (1895)"Plant Breeding" that provided a survey of the most imported hybridization work in the botanic literature. It quoted Mendel indirectly as it relied on Focke's German text (1881) on the same subject that had acknowledged Mendel's work (De Vries H 1900a). De Vries mentions Mendel only briefly at the end of his large publication to the dismay of Correns. Moreover de Vries had sent an abstract in French to the "Academie des Sciences" in Paris. It (De Vries 1900b) did not mention Mendel at all and was read by secretary G. Bonnier of the "Academie".

The second report on the rediscovery was the one by Carl Correns (1900), a pupil of Nägeli who during this training had informed him only about the failure of the Hieracium experiments and not about Mendel's results in Pisum sativum. He mentions to have learned himself of Mendels's work around 1895 through the book by Focke (1881) when most of his work with Pisum and with Zea Mais was near completion. He criticized de Vries for not having mentioned Mendel in the Comptes rendus abstract. He insisted to the Editor of the "Berichte.." that Mendel's name should figure in the title of his own report (Correns C 1900).

The third report was an abstract type summary hastily sent by Erich Tschermak von Seisenegg of Austria to the same German journal (Tschermak von Seisenegg E 1900) after he had seen the reports by the Vries and by Correns. It contained the first results of his intra-species experiments with Pisum sativum that he considered to be of equal significance in rediscovering Mendel as the results reported by the two colleagues

earlier in 1900. This first part of a much larger project of his, had been done in Ghent, Belgium, before he had any knowledge of Mendel's superb paper 35 years earlier.

How did Erich Tschermak von Seiseneg a postgraduate student of Vienna, Austria end up in Ghent, more specifically in the local Botanic Institute? He was the son of Gustaf Tschermak von Seisenegg, professor of Mineralogy, University of Vienna. and the younger brother of Armin (°1870) an MD of local importance with whom he had a long standing letter correspondence that recently served a role in support of Erich's recognition as the third rediscoverer of Mendel's laws. (Sinumek et al 2011).

Father Gustav had his younger son enter the Hochschule für Bodenkultur at the Vienna University (1891-1892). From then on the young man had not only a thorough theoretical training in general biology and botany. The theoretical studies were interrupted by regular periods of practical work and training in plant improvement techniques at private production firms. Hence he had plenty of experience in designing and performing research in his field of science and in the fields. He wrote his doctoral thesis "Ueber the Bahnen von Farnstof und Salzlösungen in dicotylen Kraut- and Holzgewächsen" with Prof Kraus and defended it with success on December 5, 1895.

Dr. Sc. Renard, mineralogist at the University of Ghent had been working at the Institute of Mineralogy, University of Vienna, directed by Erich's father Professor Gustav T. von S. He induced the son to visit Ghent and the already famous floriculture firms in the vicinity. Dr. E. Tschermak von Seisenegg was for several months (1898-1899) a guest in the home of mother Renard in Wetteren.

Contact with Prof. MacLeod brought permission to use the"koude kast" at the Botanical Institute, Ledegankstraat, Ghent in order to start Hybridization experiments with the available Varieties of Pisum sativum and thus execute the first stage of experiments designed specifically to improve the work of Darwin that had been rightly criticized by Tschermak von Seisenegg who planned to redesign and repeat Darwin's experiments with larger numbers of study subjects. His appointment in early 1899 as an assistant to Prof. Liebenberg, Hochschule für Bodenkultur, Univ. Vienna, cut the stay at Ghent short. The obtained results were sent to him. The F1 and especially the F2 results confirmed the 3/1 ratios (yellow/green) and

(round/wrinkled) found by Mendel. Tschermak found out that both De Vries and Correns had already published their work and quickly wrote an abstract reporting his own data that was reported in June 1900 in the same German Journal on Botany (Tschermak von Seisenegg 1900). The more extensive results of this project were obtained in Vienna and published in several reports that appeared between 1901 and 1910. The Austrian graduate student, temporarily in Ghent for his most famous contribution to Biology, embarked upon a fertile academic career in scientific botany and produced valuable data in the field of empirical and scientific plant improvement. (Dumon A 1963)

The author of the text did not know this story until some years ago. However the Ghent University did remember and offered E. Tschermak von Seisenegg a doctorate honoris causa in 1957 on the occasion of his 85th birthday. Unfortunately, the laureate could not come to Ghent in order to formally accept the doctoral degree because of ill health related to his advanced age. The documents of the honorary degree were handed to him at his residence in Vienna by the Belgian ambassador (Langendries E 2014). No wonder that a busy medical student at the time, even though already interested in Genetics, remained unaware that his Alma Mater had been host to one of the rediscoverers of Mendel's famous laws that remain of immense importance in all facets of the science and practice of Genetics today.

Fortunately, the past and present day geneticists at the University of Ghent, in the Faculties of Sciences and of Medicine have made and are making major contributions to progress in Genetics at the present time when the first partly successful attempts of causal treatment of patients with monogenic inborn errors of metabolism are ongoing.

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Laudatio Patrick Allegaert

Jeroen Vanden Berghe

It is my pleasure and great honour, as chief administrator of Ghent University and acting director of the Ghent University Museum, to introduce you to Patrick Allegaert. And to explain why he was nominated as Sarton medallist by our museum team.

Complementary to a Bachelor degree of Philosophy, Patrick Allegaert gained a master in Educational Sciences and in Criminology. Between 1978 and 2014, he taught courses in Cultural Philosophy and Cultural Policy at the Vives University College in Kortrijk. From 2000 he did likewise at the Erasmus University College Brussels.

From an educational, socio-pedagogical background and based upon an intense vision as a teacher in critical reflection, Patrick built a strong commitment to cultural heritage and for the museum sector. Indisputable indicative of the way he later left his mark on the story of the Museum Dr. Guislain. Along with general director Annemie Calliau he inspired and he built this outstanding and unique museum to an established player in the contemporary museum landscape.

The initial impulse for the museum was given over thirty years ago out of the Dr. Guislain psychiatric centre, which in turn originated in the middle of the 19th century from the research and clinical activities of Professor Doctor Joseph Guislain (who ten years ago was nominated by the UGent community as the Greatest UGent Professor, ahead of Corneel Heymans and Gustave Magnel).

The museum took shape in 1986 when a historical collection on psychiatric care was composed. From the very beginning Patrick Allegaert was

involved in what would become an innovative experiment. From 1999 he officially joined the museum (if I am informed correctly he applied for the position that Erwin Mortier had left there, when he decided to go earn a living as a writer). Patrick worked first in the museum as research assistant, later as curator and artistic director.

In that function he was responsible for several exhibitions at the Museum Dr. Guislain. A selection from the list of more recent exhibitions illustrates the social and cultural role the museum has claims. Without trying to be exhaustive, I list: 'Shame; War and trauma (1914-2014)'; 'Soldiers and their Psychiatrists'; 'Nervous Women'; or – my personal favorite – 'Museum Dirk De Wachter. Art & Psychiatry in Borderline Times'. Patricks' activities do not limit themselves to Ghent or to Belgium; Patrick was also co-organizer and curator in foreign museums: the American Folk Art Museum in New York and the Teylersmuseum in Haarlem.

He is the author of numerous publications on cultural policy, on the history of psychiatry and on Outsider Art (Art Brut). The central thread that he weaves through all of this … calling the notion of 'normality' into question, broadening the personal mental world and demolishing the walls between the art works of psychiatric patients (the outsiders), Art Brut and those of the artists with a capital A.

If you see Patrick here before you, you may be surprised, but Patrick is already officially retired. Currently his official role is artistic advisor to the museum. But can anyone be surprised that he can and will not limit its activities to a player in the margin, to a preacher on the sidelines?

As a pioneer and as a passionate speaker, he has taken up an extremely active role in the cultural sector. As chairman of the consultation platform of Flemish museums he advocates diligently for the sector and very recently he stood up as the spokesman for the museum directors in a protest against government savings. In addition, he is among others chairman of Hospitium vzw (Belgian medical and healthcare collections), the Firmament, the centre of expertise for performing arts and the advisory committee cultural heritage of the Flemish Community. He is the co-initiator of the Dr. Guislain award that wants to turn the spotlight to a cultural initiative (person or organization) aiming to end the taboo on mental illness.

The Sarton Medal, we issue today, is obviously not Patricks first recognition. In his capacity as artistic director of Museum Dr. Guislain, he was the winner of the first Belgian Museum Award in 2006 and the Flemish Cultural Heritage Prize in 2007. In 2012 he received, together with Annemie Caillau, the price *De Maakbare Mens* for their original contribution to a better understanding of medical and biotechnological developments and the associated ethical questions.

Today, the Sarton Medal is awarded him because of his active societal commitment to increase the awareness of the history of Psychiatry. The debate on mental illness is never easy, although one out of four people ever is facing mental health problems. Dealing with the subject needs a multidisciplinary approach, not only because it is still shrouded by taboos. The museum, to which Patrick Allegaert still actively contributes, has a special merit in disclosing the turbulent history of Psychiatry. By bringing mental illness in general and psychiatry in particular out in the open, the Museum Dr. Guislain made these issues more accessible, opened up the public debate. Concepts as ‘madness’ or ‘psychiatric disorder’ are never to understand exclusively as medical; there are always socio-cultural and ideological features. On the basis of this insight, the Museum Dr. Guislain proactively sought out the public debate with its visitors. The museum developed a mission in deepening and enriching our general view on all aspects of humankind. This vision and this social commitment is reflected in a museum with a clear identity, instantly recognisable; the construction of a collection Art Brut and an impressive parade of temporary exhibitions on social relevant themes such as the current exhibition on fear.

Georges Sarton interpreted the study of the history of science as an interdisciplinary language looking for a contribution to acquire insights into the society. From the turbulent history of Psychiatry the Museum Dr. Guislain holds up a mirror to ourselves and to society; no debate is ever avoided.

Dear Patrick, grateful for your expertise that you so effortlessly share with us as a member of our Advisory Council on Academic Heritage and Archive. Grateful for our excellent collaboration (a recent example of this is the Out of the Box configuration that we were allowed to place in the museum). We, as Ghent University Museum, proudly nominated you for the Sarton medal. As a sign of our respect for your public engagement for unlocking the history of Psychiatry and your tireless efforts in breaking the

stigmas and taboos that unfortunately still rest on mental patients. As a museum team we have a special appreciation for your idea of presenting the museum as a public-oriented platform for debate in and about science and for your effort in translating the relevance of history to today's society.

Dear Patrick, by awarding you the Sarton medal we strongly hope to involve you and the entire team of the Museum Dr. Guislain even more in the Ghent University community. Heartly congratulations and I look forward to your Sarton lecture.

Psychiatry and Patrimony, Science Communication in Museum Dr. Guislain

Patrick Allegaert

This text, which is part of the talk I gave on January 11th, 2018 in "Het Pand" (UGent), is largely inspired by the work I have been doing for the past few decades at the Dr. Guislain Museum in Ghent. What guides me on the one hand is the question of how to deal with psychiatry's cultural heritage. On the other hand, I heavely rely on the work carried out by professor Jozef Guislain (1797-1869). Just as important – and it fits in with this year's Sarton medal theme – is our quest to include a wide audience in what happened, and is currently happening, in the world of psychiatry. To put it differently: one of the main questions the Museum raises is: how can we introduce scientifically sound medical and social practices – i.e. psychiatry and its history – to a broad audience in an engaging and stimulating way? Key notions here are 'scientific activity' as well as 'more widespread communication'.

Apart from my work at the Dr. Guislain Museum, I feel tremendously inspired by my role in the advisory committee for the Ghent University Museum (Gents Universiteitsmuseum – GUM), which will be open to the public as of fall 2019.

In this text I would like to shine a light on a number of core ideas that will offer a frame for what is on display at the Museum.

FIRST CORE IDEA – Science and Culture: Make a Connection

Let us take a look at *The Man Measuring the Clouds,* a sculpture by artist Jan Fabre. In Belgium, this is a rather well-known piece of art. You can see a copy of *The Man* on the roof of Flanders' international arts centre deSingel in Antwerp, and another one on the roof of the Municipal Museum of Contemporary Art SMAK in Ghent. Gita Deneckere also refers to the piece in her book on the 200-year anniversary of Ghent University.

Fabre presents himself as both a scientist and an artist who hopefully reaches out to the 'poetry of coherence', something science seems to have lost. Sarton pointed to the importance of a shared history of, and shared thought process behind, science and culture: "The history of science should be the leading thread in the history of civilization" (1917). He stressed the need to popularize science – a real challenge, with 'connection' and 'synthesis' as key concepts. Clarifying the main ideas is best achieved in a 'text' which can be grasped and understood, hence the importance of a clear, appealing synthesis.

I will elaborate on the idea of making a 'connection', and in my analysis I would like to include the GUM, which will open next year.

Before a museum opens its doors, we tend to encounter a scene characterized by heterogeneity: the collection is shattered, spread out over many sub-collections, quite similar to the current situation. One question we should ask ourselves is: How can we make sure those varied collections will work together as one? A challenge for all involved, to say the least. Quite often, and for various reasons, people feel they are 'stuck in their collection'. One of the tasks of the GUM, an initiative aiming to cross various boundaries, is to emanate opportunities. This way, the idea of 'connecting' becomes an appealing one.

I realize this is not an easy task – just think back to how the Museum aan de Stroom (MAS, 2011) in Antwerp came to be. Bringing together different collections was met with resistance. Troubles arose among the staff, which eventually lead to a complete standstill. Before the Dr. Guislain Museum was opened in September 1986, it was crucial to convince as many people involved in the (history of) psychiatry of the need to design one unified collection displayed in a single museum.

But this is about more than merely 'bringing together various collections'. I

would like to take the opportunity to address the danger of 'heterogeneity' on a broader, more profound level. Professor Gita Deneckere points out how an excessive degree of specialization can be dangerous: "Our world is currently in a state of transition, and is being confronted with problems that can only be addressed if we can once again close the gap between the 'humanities' and the 'technicalities of science'. Compared to George Sarton's time in Harvard, that challenge has grown, seeing 'arts & philosophy', 'arts & humanities' and 'language, literature and philosophy' have become fragmented themselves owing to an unsound desire to specialize and the need to pursue niches. (…) Top-end humanities researchers may well excel in their own limited domains, but they tend to lose track of the university's humanistic goal, or even believe 'vulgarization' is beneath them. Relying on their hypersophisticated jargon devoid of any hint of poetry, they are simply unable to entertain a broad audience, apart from their peers at international conferences and workshops around the world." (Gita Deneckere, *Uit de ivoren toren. 200 jaar Universiteit Gent*, 2017, p.334, translation)

I would like to link this warning to the GUM's very own quest: to encourage the university to venture outside of the academic ivory tower to show society at large what it has accomplished in the past. This should not be done because of the past's sake alone, but because we will benefit from this in the future, while aiming for as much involvement from society as possible.

For the Dr. Guislain Museum it is just as important to remain a place where questions can be asked and answered, which will render clear the broader issue of mental vulnerability, both past and present. Doing so, a debate can be initiated, involving both scientific experts and artists, as well as people who are closely involved and those that simply show an interest in the matter.

SECOND CORE IDEA – The Cultural World and Museums: Meaningful Images give rise to Empathy

I would like to refer to a book by Olga Van Oost et al, entitled *Museum van het gevoel. Musea op de huid van de samenleving* (2016).

Olga Van Oost, professor of Sociology of Art and Culture at VUB and museum advisor with FARO (the Flemish interface centre for cultural

heritage), states that museums are increasingly taking moral responsibility and she quotes: "Museums have a duty to adopt social positions, take moral responsibility, and stimulate citizenship and critical reflection" (Elaine Gurian Heumann, American Museum Studies).

Ben Okri, a British writer with Nigerian roots, shares that conviction: "Museums embody moral values. A way of life. In this day and age people realize that those who are responsible for museums must think long and hard about them, and about the fact that people constantly visit them. Those visitors do not just expect to see great works of art. They expect more, and we should try and figure out what that is exactly."

On this topic, American philosopher Martha Nussbaum mentions 'creating capabilities' (2012), and developed a 'capabilities approach'. She opposes the notion that a country's growth and wealth can only be determined relying on conventional economic indicators. A prosperous society is first of all a socially just one, in which real opportunities are made available to every single person. Nussbaum lists ten capabilities in total. Keeping in mind the cultural practices within the Dr. Guislain Museum, I have picked out three of the most interesting ones.

- **"Being able to use all of one's senses. Being free to imagine, think and reason. Having the education that enables this to be done in a civilized, human way."** Take the straitjacket, for example. Objects like that trigger people's imaginations about what kinds of tools are used in psychiatry, about why and how people were being 'immobilized', and more specifically: what that must have been like, what it 'felt' like to experience psychiatry.
- **"Being able to become attached to other things and people outside of ourselves, loving and caring for them."** For instance: people who are mentally vulnerable cannot be reduced to being 'the other'. They are actually all around us: aren't we all 'mentally vulnerable'? A work of art can potentially move the spectator, which can make him or her more understanding towards other people's experiences.
- **"Being able to consider and develop an understanding of good and evil, and to think critically about the world and one's own place in it."** Consider, among other things, how people wanted – and want – to deal with mental vulnerability. What can history teach us

about the possibilities within therapy, and about the ambitions residing in scientific actions, but also about being powerless, about therapeutic desperation?

Nussbaum draws attention to the human being as a highly emotional creature, and argues in favour of empathy: "We tend to avoid seeing others as versatile and profound, as beings who experience deep thought, have spiritual desires and feel true emotions. It is all too easy to consider another human being as just a body, and to then go on to think we are allowed to use that body, both for good and evil. It is quite the achievement to be able to see a soul within that body, and that achievement is supported by poetry and the visual arts. They ask us to be surprised about the world inside the figures we encounter, and to also ask questions about ourselves and our own inner turmoil."

In my opinion, this quotation should be read as an urgent call for contemporary exhibitions that are contagious, and inspire others to show empathy. The meaning the word 'images' holds in this context should not be underestimated. More so than words, images are able to shape feelings and fantasies, and are a force that can encourage us to think and act.

Olga Van Oost stresses the importance of personal experience in her text, which is something particularly museums can contribute to. Van Oost connects this with the ideas of American philosopher and pedagogue John Dewey. What is 'experience'? According to Dewey it is a matter of both the heart and the mind. 'The arts and visiting museums' only starts to mean something when it becomes an 'experience', which gives it both layered and holistic qualities. Dewey clarifies just how crucial sensory experiences are, as are intellectual, more rational experiences: "Experience can only truly become meaningful when it is not the final destination, but when it leads to deeper understanding and further action." When you work in a museum, this poses quite an interesting challenge: drawing attention to the history and current findings of mental vulnerability is achieved most successfully when it is done through an interesting mix of images and texts that are indeed layered, and not only cause aesthetic emotion, but also challenge your way of thinking.

When designing an exhibition choosing the exact images and texts that relate best to the exhibit's theme is no mean feat. The selection should be

characterized by a few links, but should also leave things open to interpretation. The visitor should be allowed to let his or her mind wander freely, but should at the same time be drawn into the issue of mental vulnerability.

Because of that, curating an exhibition is also taxing on several other levels. One should read about the issues at hand, know what psychiatrists and psychologists have said about it, and collect testimonials and document personal experiences. At the same time one has to present all of that information in an appealing way that makes sense, i.e. design the dramatic arch of the exhibit, and allow artists to work with the materials provided. These efforts cannot be reduced to ticking off a to do-list. A certain methodology might be discerned, but that too should be challenged time and time again.

In one way or another, here too, we end up here with the power of 'suggestion' and 'intuition'.

THIRD CORE IDEA – A Plea for Increased Awareness of Silent Signs

In 2016 psychiatrists Kristiaan Plasmans and Geert Van Asten published *De intuïtie van de psychiater. Een pleidooi voor stille signalen in therapie (A Psychiatrist's Intuition. A Plea for Silents Signs in Therapy)*. In their book, they provide an original and very topical analysis of psychiatric practices.

To them, psychiatry is clearly under pressure. Using measurable arguments, it needs to keep on showing its added scientific value. The caregiver's intuitive skills are thus not sufficiently appreciated.

The question both psychiatrists ask is therefore: what does psychotherapy mean in an age when evidence-based medicine is king? Psychiatrist Dirk De Wachter urges us to look critically towards an imminent evolution of psychiatry that threatens to be completely 'dehumanized' because of increasing 'protocolization'

Many of us assume psychiatrists and psychotherapists know what needs to happen, and how this should be done. Plasmans and Van Asten: "People expect a set of ready-made answers. Professionals and health insurers alike often presume there is a clearly structured and straightforward approach in place, which is spelled out in the treatment protocol." The ambition behind

these ideas is obvious: this way psychic symptoms can be reduced to a clearly outlined disorder. In that case the next step will be: which treatment is best-suited to this particular problem? Afterwards, a group of these patients should be compared to patients using a placebo. However successful these methods may prove to be for phenomena dealt with by natural scientists, this approach, based solely on measurability and the inclusion of all relevant factors, is not nearly enough when looking into mental illness.

Plasmans and Van Asten: "An approach to mental health care that is overly reductionist, will eventually lead to the mental reality being disregarded. By elevating a complaint to a symptom, it becomes part of the disorder, syndrome or disease. The catalyst here has been *The Diagnostic and Statistical Manual of Mental Disorders* (*DSM*): every single mental disorder is seen as a separate entity. The *DSM* allows these entities to exist. Society has taken notice and links this back to clinical practice. Neurobiological thinking has become dominant in our society because of these looping-effects. It divides society as well as our professional field. As neurobiology approaches human kind from a place of scientific truths and disorders, it shuts out another side of human beings. This mental reality needs a humanities framework in order to be fully appreciated. Without even realising it, natural scientists and human scientists find themselves on opposing sides. (…) Under these circumstances, their perspectives are all too often considered irreconcilable." (p.144)

These psychiatrists' approach relies on the idea of finding connections, and the importance of meeting one another in therapy (by which they refer to both a scientific meeting of minds, and concrete human interaction). That is exactly where the concepts of 'intuition' and 'alignment' come into play.

What is striking is just how closely this leading analysis lines up with the work carried out in any cultural institution. A museum that focuses on the history of mental healthcare can draw on the way history is dealt with intuitively, and with the emotions and critiques it raises right now – and perhaps has raised in the past.

I would like to end this text with the following lines: to work in a museum celebrating the history of psychiatry is an assignment, a mission. How can we, by dealing with the heritage of mental health care, raise awareness

about mental vulnerability, which remains taboo to this day? There is a level of urgency residing in this question, which should not be ignored.

German historian and philosopher Philipp Blom offers up a cogent analysis of this issue in his essay entitled *Was auf dem Spiel steht* (*What is at stake*) (2017). He analyses the age we live in, the way we deal with nature, and what the true meaning of 'heritage' is. The essay's tone feels threatening. He ends his text as follows: "Creating problems always comes first. For this reason this is not an age of clear answers, but of mostly sound questions. If a sufficient number of people show patience, endurance, a willingness to scale down, a sense of irony, passion, vigilance, humanity and solidarity, if they refuse to give in, this new beginning can once again become a voice that speaks loudly and convincingly, calling out for a new story. It feels rather strange to write down these sentences. They sound pathetic and oddly old-fashioned in a society populated with cool people. Yet at the same time I have never written a book that has made me feel this hopeful that people thirty or forty years from now will laugh about, thinking it was just something their predecessors – just one generation removed – would worry about. A kind of perverse hopefulness resides in that idea. Science relies heavily on models that are reductive by definition; analyses are distorted by implicitly assuming certain things, and turning a blind eye to a selected number of other things. Nobody is clear on what will happen in the future, nobody knows all factors and complex causalities, which always seems logical in hindsight. Within that uncertainty lies a possible future, an obligation even. What is at stake? Everything."

I agree with the sentiments Blom expresses here: we carry out our assignment for the museum, feel the urgency behind our work in the cultural sector and the museum, and approach it earnestly, with a sense of necessity, but simultaneously with a mildly ironical stance, hoping it will not be as catastrophically required as we once suspected.

Laudatio Peter Van den haute

Johan De Grave

It is with great pleasure that I am able to write and deliver the laudatio for honorary professor Peter Van den haute at the occasion of awarding him the Sarton Medal for the Faculty of Sciences at Ghent University.

As the Sarton Medal is awarded to a laureate who has distinguished him- or herself in the field of the History of Science, it is fitting that Peter Van den haute in fact will shine a spotlight on the fundamental parameter of history, i.e. time itself. His contribution "*the turbulent quest for the age of the Earth during the 19th century*" gives us insights into the emerging science of Geology and how the clash of more traditional fields of natural sciences, and that between great scientific egos in particular, dominated the discussion on determining the age of the Earth in this period.

It is however my honour to first shine a spotlight on the history of Peter Van den haute in this commendation.

Peter Van den haute was born in Gent on the 25th of November 1948. He commenced his studies in Earth Sciences in the late 1960's and obtained a degree as Licentiate in Geography at Ghent University in 1971 and, later in 1975, the degree of Licentiate in Geology.

In 1983, under the supervision of professor Maréchal, he graduated with a PhD on the uranium fission-track method and its applications on the mineral apatite in Precambrian rocks of Ruanda and Burundi. These subjects, both in the context of methodology as in the specific study areas and applications, are still explored at our research unit today, building further on the foundations that Peter then constructed.

Subsequently Peter Van den haute was able to start a career as postdoctoral researcher first with the Interuniversity Institute for Nuclear Sciences (IIKW, 1983-1984), and then with the Belgian National Science Foundation (NFWO, Nationaal Fonds voor Wetenschappelijk Onderzoek), during which he further explored the fission-track method. He remained affiliated with the Geology Department at Ghent University, under the wings of professor Maréchal. In 1988, he accepted a position of scientific collaborator with one of the godfathers of the fission-track method, i.e. Günther Wagner, at the Max Planck Institute for Nuclear Physics in Heidelberg, Germany.

Following this, and back in Gent, Peter was able to secure consecutive positions with the NFWO and was eventually promoted to "Research Director" in 1997. In 2000, he became lecturer with the Department of Geology at Ghent University and was promoted to Full Professor ("Hoogleraar") in 2011. Peter retired in 2014.

From this summary it is already clear that the principal research interests of Peter Van den haute can be situated in the field of Geochronology. He mainly concentrated his efforts on radiometric dating methods that use the accumulation of radiation damage in the crystal lattice of minerals as proxy for time. Indeed, the accumulation of natural radiation damage in the crystalline structure of minerals through geological time, provides a unique dating tool, a chronometer that enables us to date various geological events.

Perhaps it is fair to say that in Peter's scientific career, the most central pillar from a methods point of view is the fission-track dating method. This technique is based on the spontaneous fission decay of the isotope 238-uranium, which is present as trace element in several natural materials, including many minerals.

A paramount issue throughout his research has always been a fundamental methodological approach, i.e. the strong urge to fully explore and understand the basic principles of the method and to subsequently refine the analytical technique accordingly. It is then no surprise that some of Peter Van den haute's prime accomplishments are to be found in the method's development. Especially with respect to the absolute calibration of the fission-track dating method, the contributions of Peter have been widely acclaimed and have added to the international reputation of our research unit.

The intensive collaboration with the Institute for Nuclear Science (Instituut voor Nucleaire Wetenschappen, INW) at Ghent University, and in particular with professor Frans De Corte, proved to be quintessential in this context. Frans De Corte is an internationally renowned specialist in the field of neutron activation analysis and was in charge of the Tethis nuclear reactor at the INW. Within this unique configuration Peter and Frans were able to further the understanding of the fission-track method and its calibration significantly.

The Tethis reactor was, however, decommissioned in 2003. As a PhD student of Peter at that time, I myself was also involved in the fission-track method and was one of the last researchers to set up experiments at this nuclear facility.

Another noteworthy achievement by Peter Van den haute was his involvement in the German KTB project, i.e. the "Kontinentale Tiefbohrung der Bundesrepublik Deutschland" or the German continental deep drilling project in the late 1980's – early 1990's. In this prestigious project, Peter collaborated closely with Günther Wagner. In the wake of this project, the fundamental insights in the fission-track dating method took major leaps forward and finally culminated in the publication of the textbook "Fission-track dating" by Wagner and Van den haute in 1992; up to this day still *the* fundamental book on the basic principles of this method.

An important second cornerstone in the career of Peter Van den haute of course also involved a geochronological technique, again based on the accumulation of natural radiation damage in minerals. This is the luminescence dating method, a technique whereat he continued working very closely with the INW and Frans De Corte, and in which he also focused both on methods development with the aim of its application in the Earth Sciences.

His research related to the luminescence dating method allowed him to absolutely date archaeological ceramics for example, and also his contributions with respect to the dating of young geological windblown or aeolian sediments (such as loess and dunes) in Flanders and the Netherlands have proven to be crucial for understanding the Quaternary geology, climate evolution and geomorphology of the low countries.

In the footsteps of Peter, the applied and fundamental methodological luminescence research is still a fruitful and ongoing activity within our

research group at Ghent University. For instance, it recently led the Francqui foundation to award professor Andrew Murray with a Francqui chair at UGent, dedicated to luminescence dating. Peter closely collaborated with Andrew and co-workers at his renowned institute, the Nordic Laboratory for Luminescence dating, in Denmark.

Besides his research, Peter Van den haute has also made important contributions with respect to teaching in the Department of Geology. Especially worth mentioning here in this context, is the introduction of the courses "Isotope Geology" and "Geochronology", under his direction. To this day, both course are still compulsory modules in the Bachelor and Master studies in Geology at Ghent University.

From this non-exhaustive synopsis, it should have become obvious that Peter Van den haute, the Sarton medalist, has explored many facets of the broad field of Geochronology and that he has always paid attention to its most fundamental aspects. Reconstructing the historical foundations of the scientific discipline as such, is also one of the aspects Peter has been looking into with great interest. His lecture "*the turbulent quest for the age of the Earth during the 19th century*" is a distinct reflection of this.

The turbulent search for the age of the Earth during the nineteenth century

Peter Van den haute

Introduction

During the course of the nineteenth century, the age of the Earth became a topic of major interest and concern in the scientific world. Natural philosophers (as they were called at the time) specialized in different disciplines engaged themselves in trying to contribute to the solution of this "universal" problem and so, with the use of often very different approaches and methodologies quite a number of figures for the Earth's age were produced, especially during the second half of the century. Unfortunately, none of the determinations carried out, even by some of the highly reputed masters of their discipline, turned out to be right, not even in first approximation. Hence, from a purely scientific point of view as much as nothing of the achievements of nineteenth century science with respect to the Earth's age remains standing. For the geochronologist interested in the history of his science however, this period is of utmost interest because of the challenge that was put by the Earth's age problem to the various researchers in their different fields and the often severe debates that it caused between them. The true disputes were mostly confined within Great-Britain and hence, it is not surprising that later on, during the final decades of the twentieth century, the nineteenth century scientists themselves and their disputes became a subject of-its-own in the Anglo-American literature dealing with the history of geosciences. Some excellent publications[1,2,3] have been written by authors who dug deeply not only into the original works of their so-to-say ancient predecessors but also into their

correspondence. The interest in the efforts to determine the age of the Earth culminated in 2001 with the appearance of the book *"The age of the Earth: from 4004 BC to AD 2002"* published by the Geological Society of London[4], which includes some interesting chapters dealing with the nineteenth century.

The scope of our present paper is modest; it is only intended to provide the reader with a concise overview of the major steps that were made in the quest for the Earth's age throughout this fascinating century. We hereby inevitably rely heavily on the publications mentioned above and, when considering their volume and quality, we realize that we are hardly capable of adding any substantial novelties to them. Nevertheless because Belgian geologists are traditionally as much influenced by the works of their French as by that of their British colleagues we felt that it was our task to elaborate a little further on the thoughts and opinions of the French scientists on the Earth's age during that period, even if their contribution to the solution of this matter remained rather minor.

Hutton and Lyell: time unlimited

In the frame of a paper dealing with the age of the Earth, the first important British natural philosopher to be mentioned at the dawn of the nineteenth century is James Hutton (1726-1797), the Scottish gentleman farmer by many regarded as the founder of modern geology[1,5,6] Although Hutton died in 1797 and the first version of his "Theory of the Earth" had already been published in 1788, it was only through his friend John Playfair (1748-1819) who was a professor of natural Philosophy at the University of Edinburgh and who published his "Illustrations of the Huttonian Theory of the Earth" in 1802 that the importance of the work of Hutton himself became widely acknowledged. Hutton's own publications were written in a style that was difficult[6] to digest and most students who were interested to know more about his work rather consulted Playfair's version than Hutton's original one that moreover was impregnated by deistic natural theology. Belgian geology students typically get to know Hutton during a lecture when the significance of a major unconformity is explained to them and this, nowadays, often with the aid of a PP-slide showing the classic example at Siccar Point along the eastern Scottish coast. While looking at the Devonian sandstones unconformably resting upon the subvertically tilted and eroded Silu-

rian beds, the vastness of geological time is revealed to them, almost as it was to Hutton and Playfair at the time.

Hutton became also known as the father of plutonism as he was the first one to assign a major role to the Earth's internal heat in the geological processes. Heat, he thought, was the driving force that created mountains and that pushed up volcanoes that acted as a kind of safety valves. Mountains became eroded by atmospheric agents and the erosion products were transported and deposited as sediments into the oceans. The loose sediments were gradually buried, lithified and heated up again to melting temperatures when reaching greater depths. Granite, a rock that he identified for the first time as being solidified from a molten state, while intruding from depth into rocks at higher levels in the crust, plays a key role in his entire theory. His view was entirely opposed to the view of the so-called "neptunists" adhering to Abraham Gottlob Werner (1749-1817), the famous mineralogist and professor in the School of mines at Freiberg in Germany, during the final decades of the 18th and the early 19th century. According to Werner all rocks had precipitated from the ocean chemically or mechanically and the crystalline granite was the first rock to have been precipitated when the ocean was still hot, forming the basement or "Grundgebirge" upon which all other rocks were deposited later on.

A central idea in Hutton's theory is the cyclicity of all geological processes and events. Driven by the Earth's internal heat Hutton interpreted the Earth as a kind of everlasting machinery of endless mountain building, erosion, deposition and burial. This view shines through in his legendary statement: "*The result therefore of our present enquiry is that we find no vestige of a beginning, – no prospect of an end*." In this way, Hutton, as one of the major figures of the Scottish Enlightenment, applied ~~at~~ the line of thought of the classic Greek philosophers, known as "Panta rhei". Not surprisingly, he encountered a strong opposition from the Anglican theologians, known as the Scriptural or Mosaic geologists, for whom the answer to the question of the age and origin of the Earth had to be found in the Bible and more precisely in the book "Genesis" or the first book of Moses. Obviously, according to the Scripture, the Earth could not be older than about five to six thousand years.

The idea of unlimited time was adopted by Charles Lyell (1797-1875), who is commonly regarded as the most important of the early nineteenth century

British geologists. In his "Principles of Geology"[7], a work that appeared in three volumes between 1830 and 1833, Lyell developed a theory that was based upon Hutton's views but with some important adaptations[1,5,6]. A major novel element in his theory, is his conviction that the Earth's surface and its natural landscapes have been completely shaped by processes that are active now or that are known to have been active in historic times and this without any change in frequency or intensity (figure 1). This idea is expressed in the subtitle of his work: "*an attempt to explain the former changes on the Earth's surface by reference to causes now in operation*".

William Whewell (1794-1866) theologian and philosopher at the University of Cambridge granted Lyell's theory with the predicate "uniformitarianism"; and placed it in glaring contrast with "catastrophism", the name he invented for the theories that claimed that in the geological past processes of much stronger power and violence had been at work. A key role in the debate between uniformitarians and catastrophists was played by the diluvial deposits, sediments sometimes containing large boulders of rock that at present, are known to be of Pleistocene age (dating from the last million years) and that bear witness of a much colder climate that caused extensive glaciations in northern Europe. Uniformitarians explained the deposits by gradual changes in climate and physical geography, while for the catastrophists they were evidence of a cataclysmic invasion of the ocean waters. By the Mosaic geologists they were simply considered as remnants of Noah's flood.

As stated above, according to the uniformitarian theory, the high mountains, deep river valleys and large volcanoes on our planet can be explained by processes that we observe to be active now, allowing them to be operational for a sufficient i.e. a very long time. The example quoted by Lyell that appeals directly to one's imagination is that of the Andes. Lyell knew that along the Chilean coast severe earthquakes had caused a sudden uplift of rock of more than one meter along many tens of kilometers. Taking this as reference he concluded that the Andean mountain chain resulted from a few thousands of such earthquakes, but as such heavy earthquakes occur only once about every century, several thousands of centuries were obviously required to build up the entire chain. Apart from this uniformitarian approach, Lyell's theory was quite similar to Hutton's in a sense that it denied any directional tendency in the Earth's evolution and that it

defended the idea that all natural changes on Earth are essentially cyclic. For Hutton however, large geological phenomena such as mountains were not formed in small steps but in paroxysmal phases during the Earth's evolution. Based on their geological observations, both Lyell and Hutton came tot the conclusion that the Earth may have existed for an unlimited time and it is this idea that brought about that geology has become the science that ruptured through the limits of time[5] just as astronomy was the science that had broken through the limits of space. Geology now showed that man's occupation on Earth only represented a minute fraction of the entire time of the Earth's existence while astronomy showed that his planet Earth only represented a dust particle in the Universe.

Figure 1: The famous picture of the temple of Serapis at Pozzuoli in Italy that figures on the first page of Lyell's "Principles of Geology". The marks of marine molluscs that once lived attached to the columns demonstrate the relative changes of the sea level that had occurred with respect to the land surface, in historic times.

Lyell's books were not only aimed at professional geologists but also at the general intellectual reader. His style of writing was clear and throughout his discussions, he uses comparisons and analogies. He also elaborates on numerous examples often based on his personal field observations. The impact of his work was great and lasted for decades. To give an example: in the volume of the "Encyclopaedia Americana" that appeared in 1851[8], about twenty years after his *Principles* had been published, Lyell's work and ideas are still extensively discussed under the topic "geology". However, his extreme uniformitarianism was certainly not adopted by all geologists. As an example, although Lyell knew that numerous animal species, the fossils of which are found in the older geological formations, are now extinct, he denied that throughout time any real evolution had taken place in organic life, a conviction that was not followed by many of his contemporary geologists such as Adam Sedgwick (1785-1873) or Henri De la Bèche (1796-1857). For them the fossil record testified to an evolution both in animal and vegetable life, from more primitive to more complex, and they considered it as a clear evidence of directional change.

Nowadays, the uniformitarian theory as it was promoted by Lyell has greatly disappeared and understandably, in his book "the Age of The Earth" published in 1990, Brent Dalrymple[4] only devotes a small footnote to the theory, quoting it of little value to modern geology. Nevertheless, the basic idea that to understand the geological record, the processes that are active now have to be studied at first and thoroughly or in other words, "the present is the key to the past" remains pertinent to modern geology. A phenomenon that undoubtedly supports this paradigm and that has only been discovered during the twentieth century are the turbidity currents, large sediment-loaden water masses that precipitate from the continental slopes onto the deeper ocean floor at regular intervals. If Lyell would have been aware of them they would certainly have found a place in his "*Principles"*.

Finally, it has to be mentioned that Lyell did not really exclude neither that the Earth had a beginning nor that its existence may come to an end, but he felt that these questions were merely philosophical and that they actually fell beyond the realm of human thinking. As a consequence, he can of course hardly be considered as a founder of geochronology, the science that in its literal sense wants to know the age of the Earth. It should also not be

forgotten that although according to Lyell, the Earth may have existed for infinite time, the age of the rocks and of the geological structures that we observe at the Earth's surface may be measured in million of years or even in hundreds of thousands or years depending upon the rate and intensity of the processes that created them.

In France the Earth is cooling

In France, clergy did not interfere with natural sciences as much as in Great-Britain. So-called Scriptural geologists were absent here and were even ridiculed. A clear example is provided by Ami Boué, the first president and co-founder in 1830 of the French Geological Society. In his outline of the advances of geology of 1833, published in the bulletin of the Society[9], Boué openly mocks the English theologians for trying to reconcile geology with Genesis and he fulminates more specifically against the clergyman Frederick Nolan (1784-1864) and his lectures on "*The Analogy of Revelation and Science established* "at the University of Oxford, calling him an erring knight fighting against any progress in science.

In France, Lyell's uniformitarianism also did not break through as it did in Great Britain, neither did the idea of unlimited time[10]. The reason for this may be that France's own scientists carried out important studies that were directly relevant to the Earth's history and its physical constitution. The first one is Georges Cuvier (1769-1832), the zoologist and palaeontologist, who by some is regarded as a pioneer of catastrophism[1]. Based on his comparative research of the present and fossil animal kingdom, Cuvier concluded that the organic and inorganic history of the Earth is characterized by the occurrence of a number of catastrophic events that had caused the extinction of many species of animals. The other two are the physicist Jean-Baptiste Joseph Fourier (1768-1830) and the mineralogist Pierre Louis Cordier (1777-1861). In 1822 Fourier published his "Théorie analytique de la chaleur"[11], a thorough mathematical treatise on the conduction of heat, that was intended to acquire a better understanding of the Earth's temperature. This work was the foundation for a memoir that he presented at the French Royal Academy of Sciences in 1827[12]. In this memoir, he discusses the increase in temperature of 1°C/ 30-40m with depth in the Earth and arrives at the conclusion that it must be continuously dissipating

heat out of its interior into space. This heat forms a part of the primitive heat that the Earth had acquired at the time of its formation as a planet of the solar system and hence in consequence, it must be gradually cooling since. In an essay[13] printed in the same volume, Cordier presents an exhaustive account of temperature measurements performed in a number of mines and states that if the thermal gradient persists down towards its centre, the Earth must be in fluid state at a depth of 100 km or less. The thin upper solid crust, he concludes, is the result of the solidification of an originally entirely molten Earth.

So, for the French geologists, who adhered firmly to the studies of their fellow-countrymen, the Earth had to be of finite age. Hence, not surprisingly, in a plate published in 1832 and illustrating the evolution of the Earth's crust throughout geological time, Nérée Boubée (1806-1862), a lecturer in geology at the University of Paris, assigns an age of three hundred thousand years to the Earth, while quoting that our planet has been continuously cooling since its creation. Lyell was well aware of the work of Fourier and Cordier and he agreed with the idea that the Earth's interior may be molten, but he was not convinced that there was any evidence that our planet was cooling. For Lyell the Earth's internal heat was permanent and remained unchanged.

Darwin's blunder

When the name of Charles Darwin (1809-1882) is mentioned in a classroom, most students automatically think of the great biologist and the famous author of the "Origin of Species". Only few of them will recollect that, when Darwin embarked on the H.M.S. Beagle in 1831 to start his journey around the world, his first ambition was to become a geologist and the first investigations he was going to make on the Cape Verde Islands were indeed purely geological. During his journey, he was also guided by the first volume of Lyell's "Principles" that had appeared in 1830, a book that was, as he quotes "of the highest service in many ways".

When, later, Darwin was elaborating his theory of evolution based on natural selection, it became clear to him that he needed a vast amount of time to enable the process of natural selection to generate all the modern life forms in their full diversity. This time was supplied to him by Lyell and

in chapter nine of the first edition of the "*Origin*" that appeared in 1859, a chapter dealing with the imperfection of the geological record, Darwin reveals himself as a strong supporter of Lyell when he makes the revealing statement[14]: "He who can read Sir Lyell's grand work … yet does not admit how incomprehensibly vast have been the past periods of time, may at once close this volume". In the same chapter and in an enthusiastic effort to convince the reader about the vastness of geological time and the long duration of geological processes, Darwin presents a calculation of the time span that he thought had been necessary, to produce the Weald, a region in the southeast of England surrounded by the steep slopes of the North and South Downs (figure 2). For doing so, he assumed that the "cliffs" of the Downs were the result of marine erosion, and that the region had subsequently been uplifted out of the sea. Assuming an average erosion rate of about 2.5 cm per century he arrives at a value of about 300 million years, a result that immediately encountered severe criticism from his fellow scientists[15].

Figure 2. The "cliffs" of the South Downs, bordering the lower area of the Weald. Darwin who thought that they were result of marine erosion probably committed the most dramatic error in his scientific career when he tried to calculate the amount of time that was required for their formation.

A major representative of this criticism was John Phillips (1800-1874), professor of geology in the University of Oxford and president of the

Geological Society of London from 1858 to1860[16]. Phillips stated correctly that not only Darwin's estimate of the erosion rate was far too low but that his approach to the problem was completely wrong. Phillips additionally pointed out that erosion by rivers acts much faster and had played a major in the creation of the Weald. As a reaction to Darwin's evolution theory which he vehemently opposed, he made an attempt to calculate the amount of time that has elapsed since the beginning of the deposition of the sedimentary rocks and the first apparition of fossils[17], a time span that nowadays approximately corresponds to the Phanerozoic era. He estimated the total accumulated thickness of the sedimentary rocks at 22km, and, using the present rate of sediment accumulation in the Bay of Bengal as a reference, he estimated the average sedimentation rate in the oceans at 0.023cm per year. When keeping the sedimentation rate constant throughout time, an approach that would be in accordance with the uniformitarian view, Phillips arrived at an age of 95.9 Ma for the oldest sediments on Earth. If our planet would on the contrary be gradually cooling – the idea that he preferred – Phillips estimated the initial rate of sediment deposition to be the double of the present rate which reduces the age result to two thirds of the uniformitarian value or 63.9 million years. With this exercise Phillips, not only formulated a serious attack against Darwin, he was also the first geologist to use sediment accumulation as a time piece.

Lord Kelvin enters the scene

About fifteen years after Lyell published his "*Principles*", a young man, 22 years old, was appointed professor of Natural Philosophy at the University of Glasgow. This man was William Thomson (1824-1907) who in the scientific world has become widely known as Lord Kelvin[1,3,4], a title awarded to him by Queen Victoria in 1892. To obtain his appointment Kelvin had submitted a dissertation entitled "On the age of the Earth and its limitations as determined from the Distribution and Movement of Heat within it." Unfortunately, the content of this dissertation, probably representing his first attempt at tackling the problem of the Earth's age, has been lost but the title clearly demonstrates Kelvin's early interest in this matter. In the 1850's Kelvin, already recognized for his fundamental contributions towards the development of thermodynamics, became aware of the Lyellian view of a steady-state Earth. It was clear to him that the theory of

uniformitarianism was untenable in the light of the laws of physics as he knew them. For Kelvin, when these laws are obeyed, it could not be doubted that the amount of energy stored in the Earth had to be finite as it is essentially gravitational and dates back from the time that our planet was formed by processes of accretion. According to Kelvin, the total amount of gravitational energy transformed into heat during the formation of our planet was sufficient to create it as a molten globe which since then has cooled down to become entirely solid and as a solid it continuously goes on loosing its internal heat. At this point, Kelvin's view was in accordance with that of the French scientists and more specifically with the ideas of Fourier, which he had actually already studied when he was sixteen years old.

As a consequence, for Kelvin the Earth had a finite age that can be quantitatively determined. In further opposition to the uniformitarian theory, he stated that Earth was undoubtedly subjected to a directional evolution. The eternal heat machine of the Earth as depicted by Hutton and Lyell, simply cannot exist. Similar reasoning can be applied to the Sun, with the obvious distinction that the mass of the Sun is much greater, which explains why it is still is in an incandescent state.

In two papers[18,19] that appeared within a time span of about one year (1862) Kelvin consecutively tackled the problem of the age of the Sun and of the Earth[17,18.] We will confine ourselves here to a brief discussion of the approach that he used to calculate the Earth's age (figure 3). The major assumptions that Kelvin made for making this calculation was that, as already stated above, the Earth, after its formation as a hot liquid sphere, cooled down to became entirely solid and that as a solid, it has a homogeneous composition as far as its thermal conduction is concerned. If the initial temperature of the newly-solidified Earth and its thermal conductivity are known it becomes merely a mathematical exercise – admittedly not a trivial one – to assess the time needed for the Earth to cool down to such a degree that in its upper part a thermal gradient of 2.5°C-3°C/100m will be found, corresponding to the present-day average value. As initial temperature for the Earth, Kelvin used a value of 3870°C which he thought to be a good estimate of the melting temperature of rock and for the Earth's conductivity he chose a value intermediate between sand, sandstone and basaltic rock. Based on these values he arrived at an age of 98 million years

for the time that has elapsed since the moment that Earth became entirely solid.

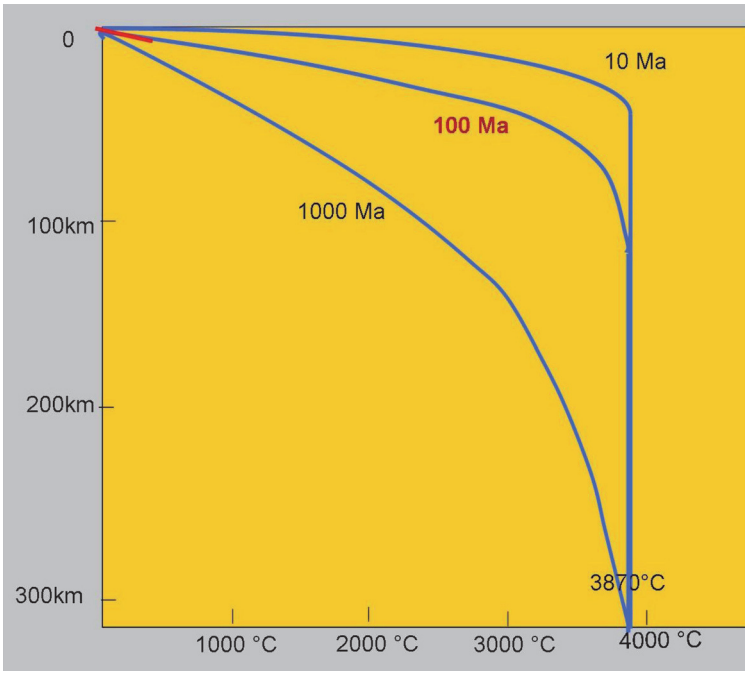

Figure 3. Diagram illustrating the method Kelvin used to calculate the age of the Earth. Based on his assumptions (see text) he calculated how the present Earth's temperature is expected to decrease from its deep interior towards the surface. Three curves are shown corresponding to three different values of the Earth's age. It is the middle curve corresponding to an age of 100 million years that, at its upper end, matches the short red line that represents the present day average geothermal gradient.

Allowing for the uncertainties in his calculation, of which he was well aware, he concluded that the minimal value had to be 20 million and the maximal value 400 million years. The calculations he made for determining the age of the Sun, yielded a central value of 20 million years with a maximum of 100 million and a minimum of 10 million years. So, although there is an overlap between both time windows, the central value of the Sun is substantially lower than that of the Earth, something that undoubtedly troubled him and that also went not unnoticed by his fellow-scientists. Kelvin's calculations of the Sun's age were also not entirely

original; as they relied on calculations made by Herman von Helmholtz (1824-1894) in 1856.

Kelvin's age of the Earth was favourably received by some geologists, especially by Philips, the man who had arrived at a comparable result based on his study of the sedimentary record. However it encountered strong opposition from others and notably from Darwin, who considered a time span of this size clearly insufficient to allow for his evolution through natural selection. Paradoxically, Darwin himself, because of his largely overestimated age of the Weald, had provided Kelvin with an incentive to carry out his assessment of the age of the Earth. Undoubtedly, Kelvin, being a defender of intelligent design as far as the appearance of man on our planet is concerned and as such being vehemently opposed to Darwin's theory, must have felt some sort of satisfaction when he published his results, knowing that they made evolution almost impossible. And so, it is easy to understand that for the decades to come the age of the Earth would become a central element in the scientific debate and more specifically in the debate surrounding Darwin's evolution theory.

Kelvin may originally have thought that his assessment would be readily and generally accepted, because it was based on well established laws of physics, undoubtedly by far the most solid of all natural sciences, but he experienced that this was not the case and that the uniformitarian ideas kept on being believed and defended. In order to put an end to this situation, he addressed the Geological Society of Glasgow in 1868 with a lecture on geological time. In this lecture he directly invokes the second law of thermodynamics, stating that in every change of mechanical energy from one form to another a part of the energy is transformed into heat and dissipated. He uses this law to discuss the tidal retardation, i.e. the difference in time for a given locality at sea between the moment that the moon passes through the meridian and the moment that the high tide attains it maximum. The retardation, he explains, is caused by friction processes and due to this friction, a part of the Earth's rotation energy, although very small, is transformed into heat and dissipated. This implies that the velocity of the Earth's rotation must gradually decrease or in other words that the duration of the day becomes gradually longer during the course of time. The effect can be compared to that caused by a brake that is gently pushed upon a rotating wheel. Kelvin went on stating that as consequence of this phenomenon, the

Earth also cannot possibly be a billion years old because, such a long time ago, our planet would have been spinning so fast that all loose sediments and rocks would have been ejected into space. Further on, the observation that our planet exhibits a flattened shape indicates that it had originally been molten and the degree of flattening not only allows to evaluate its spinning rate around the time when it was solidifying but also how long ago complete solidification has occurred. Because the Earth's flattening is small, this solidification could not have occurred many hundreds millions of years ago. The essential message Kelvin finally communicated to his audience with his lecture was that not the rate of the geological processes has been uniform throughout time but the laws of physics that govern them and these laws show that Earth has a limited age and that it continuously looses energy.

Kelvin's efforts to convince his colleagues of the softer sciences had their effect and so, while Lyell's uniformitarianism may still have been the major geological theory around 1860, about ten years later it became generally abandoned. Although the debate about its true value continued, due to the work of Kelvin, the age of the Earth had now become limited and it became accepted that our planet was subjected to a directional evolution. Kelvin's work also inspired the geologists to obtain information on the Earth's age from the sedimentary record, as had been done for the first time by Phillips. Brent Dalrymple[3] reports of 23 such studies between 1860 and 1889. The results of these studies vary between 3 million and 15 billion years, depending upon the approach that was used and hence, unfortunately, they merely seem to reflect the diversity – or was it the division? – that existed between the geologists themselves. In addition, if an average value of all these determinations would be taken, the result would not be that far off from Kelvin's 100 million years.

The French Cool

If we turn to what happened in France after 1850, we observe that the age of the Earth had become a problem that belonged to the domain of astronomy rather than to physics or geology. The age of the Earth was intrinsically related to the problem of its formation and for the French geologists this was beyond their scientific ambition. The period of time that was

of utmost importance to them was the Phanerozoic. Field mapping, elaborating a detailed stratigraphic record, studying the fossil content in detail, deciphering the structure of deformed beds in mountainous regions, those were their topics of interest. The astronomers on the other hand, seemed to be more interested in the mode of formation and in the physical constitution of the Earth and the Sun than in determining their exact age. In their studies they adhered to Laplace with his theory for the formation of the solar system and Cordier (based on Fourier) with his hypothesis for a cooling Earth with a molten core enveloped by a relatively thin solid crust. Cordier's work undoubtedly must have prevented French astronomers from accepting Kelvin's assessments without serious reservations because they knew that his calculations were based on the assumption of a rigid earth.

The French scepticism can be illustrated by the communication presented by Charles-Eugène Delaunay (1816-1872) at the Académie des Sciences in 1868[20]. In this communication Delaunay attacks Kelvin who, relying on the work of William Hopkins (1793-1866), had stated that the precession and nutation of the Earth's axis constitute proof of its rigidity. A thin solid crust, Kelvin had claimed, would exhibit a pronounced instability due to these movements and this is not observed. In response to this statement, Delaunay remarked that the changes in the orientation of the Earth's axis due to precession and nutation occur so slowly, that they affect the globe as a whole, without introducing any difference in behaviour between the solid crust and the liquid core and that, in addition, the viscosity of the core fluid might be much higher than that of a normal liquid. As another example, Albert De Lapparent (1839-1908) mentions Kelvin's determination in his "Traîté de Géologie" but promptly points at the weakness of the assumption of a homogeneous rigid Earth whereupon it is based[21].

In summary, the French scientists did not produce any absolute number for the Earth's age, as if they were convinced that such an exercise could not lead to accurate results because of the uncertainties that existed about the formation and physical constitution of our planet. Nevertheless, specific figures both of the age of the Earth and the Sun were actively circulating in the French scientific world and the numbers that did circulate were also not truly consistent for both celestial bodies. Hervé Faye (1814-1902) is another astronomer is to be mentioned here. In 1884, Faye attempted to

replace the theory of Laplace for the formation of the solar system by an alternative one[22]. His motivation to do so, was the observation of a retrograde movement of the satellites of the outer planets Uranus and Neptune, something that could not be explained by the simple spirally contraction theory of Laplace. Without going into the detail of Faye's theory, one of its major elements is that it puts the formation of the Earth well before the formation of the Sun. In the frame of his discussion Faye uses a value of 20 million years for the lifetime of the Sun while referring to Helmholtz and for the Earth he uses a figure of 100 million years for the duration of the Phanerozoic, while referring to evaluations made by geologists and zoologists, but without quoting any specific study or author. Faye regarded his theory as an important accomplishment which implies the figures that he used found at least some acceptance in France.

The final decade

Around 1890, although in Great-Britain the debate was far from finished, a number of geologists and biologists appeared to be willing to reconcile themselves with Kelvin's value of 100 million years for the age of the Earth. Indeed, was the problem of the Earth's age to some extent also not a problem of perception of time, of the comprehension of how vast an amount of 100 million years really is and whether or not this time is sufficient to allow for biological evolution and diversity as it was known? Now that the concept of limited time had been accepted, the amount 100 million years was not considered anymore as being that unthinkably low.

This situation was to be disturbed by Clarence King (1842-1901), the American geologist, known as the first director of the Geological Survey of the U.S. from 1879 to 1881. In a paper he published in 1893, he stated that an age of 100 million years was much too high[2,3]. Based upon the melting temperature of basaltic rock, in the meantime more precisely determined to amount to 1200°C at atmospheric pressure, and on the calculations that describe how this temperature gradually increases with depth, he showed that the Earth's temperature curve of Kelvin was untenable because it would introduce a molten layer in the Earth's interior of hundreds of kilometres thick close to the surface (figure 4). According to his own calculations the upper limit for the Earth's age had to be set at 24

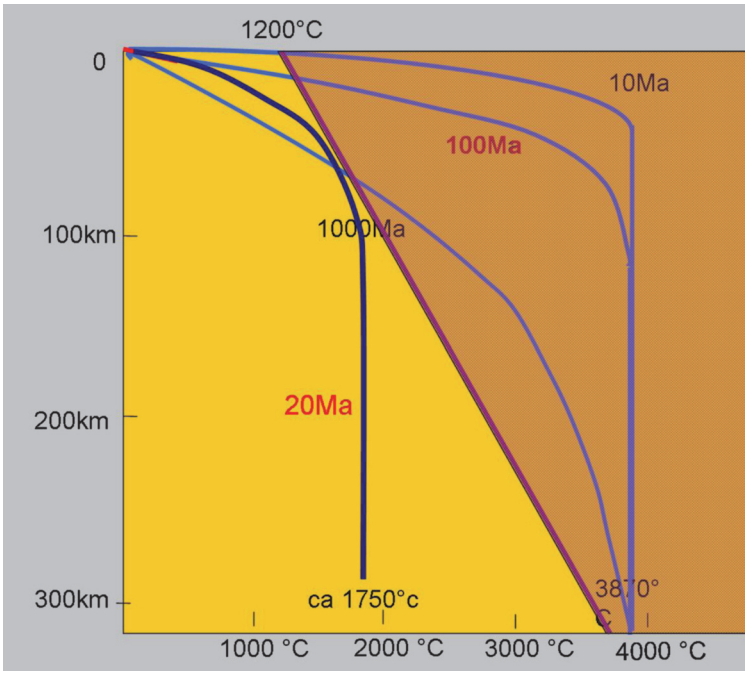

Figure 4: Diagram similar to the one shown in Figure 3 but with the adjustments that have to be introduced according to the work of Clarence King. The darker part at the right corresponds to the domain were the Earth's interior is regarded to be fluid making clear that Kelvin's 100 million year curve is untenable; the new maximum age for the Earth now becomes about 20 million years.

million years. Kelvin immediately appreciated King's work that was based on new experimental evidence and he accordingly adjusted his own calculations to arrive at a similar age of 24 million years. Such a reduction of the Earth's age obviously re-ignited the discussions with his opponents but powerless as they were against his authority and incapable of proving him wrong, they could only hope that another expert physicist or mathematician would raise to the challenge. The man they had been hoping for turned out to be John Perry (1850-1920) an engineer teaching at a technical college in London who had been an assistant of Kelvin at Glasgow in 1874 and who had the reputation of being a defender of the underdog[1,2,24]. Although Perry highly respected Kelvin and would never have done anything to offend him, once he decided to dig into his calculations he directly focused on the weak spot, namely the highly simplified assumptions; they were based

upon. He demonstrated that if the Earth was composed of an inner more conductive and an outer less conductive part, instead of being homogeneous in thermal conductivity, its age estimate could be easily increased by a factor of ten and more, depending upon the difference in conductivity between the two parts. The difference in rate of heat transfer could even be made greater still if the Earth's inner part would be fluid and therefore heat would be transported by convection instead of by conduction. Perry was not successful in changing Kelvin's opinion. On the contrary, Kelvin adhered to his age of 24 million years, not only because of the evidence supplied by King but also because the conflict with the age of the Sun now seemed to be resolved.

So, as the century approached its end, the conflicting opinions between physics, geology and biology with respect to the age of the Earth were probably greater than ever before and the problem now seemed totally insoluble. But progress of science sometimes occurs in surprising and unpredictable ways. While the debates were going on, Wilhelm Röntgen (1845-1923) discovered a new type of radiation in 1895, called X-rays and in 1896 Henri Becquerel (1852-1908) found that uranium emitted yet another new type of radiation. Radioactivity, and consequently the transformation of one chemical element into another, was being discovered, and this transformation involves the release of a huge amount of energy. Kelvin's assumption of an Earth continuously loosing its internal heat soon had to be abandoned because a source was found that provided our planet continuously with new thermal energy. Within a few decades the new discipline of nuclear physics that was being born would also allow to determine the age of rocks and of the Earth, not anymore from calculations based on assumptions but from experiments and measurements.

Epilogue

Before ending this paper, it might be appropriate to formulate a few final remarks mainly addressed to the reader who is not familiar with the achievements of modern geosciences.

First of all, the idea that radioactivity represents a major factor in the Earth's thermal budget and in the increase of its temperature from surface to core, still persists even among scientists, but at the very least it needs

some adjustment. Based on the present knowledge of the geochemical affinities of the radioactive elements such as Uranium and Thorium, we know they are greatly accumulated in the rocks of Earth's crust and that they are not homogeneously distributed in the Earth's interior as was once thought. The importance of radioactivity as a source of energy to reject Kelvin's assumptions has therefore undoubtedly been overestimated.

Secondly, thanks to the development of seismology and other geophysical disciplines during the twentieth century, not only the layered structure of the Earth's interior and the presence of a partially molten core has been established but also the, at present generally accepted, theory of plate tectonics has been elaborated. According to this theory, heat transport essentially occurs through convection in the Earth's mantle, the largest shell in its interior. So, when looking back, it appears that after all, it is John Perry's work that gives the final blow to Kelvin's age of the Earth.

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Laudatio dr. Bruno Goddeeris

Eric Cox

Bruno Goddeeris was born on 12 September 1951 in Kortrijk

In order to understand where his love for the history of immunology and in particular vaccinology comes from, I have to tell you something about the studies and career of emeritus professor dr. Bruno Goddeeris.

Bruno started his university studies in 1969 and 9 years later in 1978, he obtained his third diploma.

His first diploma was obtained at KULeuven, Faculty of Sciences, where he graduated in 1973 as a Licentiate in Sciences (Zoology).

This clearly stimulated him to learn more about animal diseases and their cure, and that same year he started studying veterinary medicine at the University of Ghent. Four years later, we are then in 1977, he again graduated with the degree of Doctor in Veterinary Medicine.

That was clearly not enough and his great love for animals and I suspect also for insects and parasites, being not only a veterinarian but also a zoologist, made him dream of the tropics. That same year he enrolled in the Master in Tropical Veterinary Medicine and Hygiene at the Prince Leopold Institute of Tropical Medicine in Antwerp and one year later he obtained the diploma of Doctor in Tropical Veterinary Medicine and Hygiene.

Of course he followed that master's degree to fulfil his dream to go to the tropics and so obtained a position at the ILRAD, the International Laboratory for Research on Animal Diseases, in Nairobi, Kenya. He became, as Associate Expert of the Food and Agriculture Organization of the United Nations, responsible for the diagnostic laboratory of the ILRAD. This was a quite busy position. In 1980 the lab received 10,774 samples: 10,351 for

serological analysis and 432 for bacteriological research. In addition, the antigens and conjugates were prepared for diagnosis. He stayed in that position for 3 years.

He most likely never had thought that he would continue his career in the field of immunology but his interest in and love for immunology gradually grew during those 3 years in the ILRAD due to contacts with the researchers and especially Ivan Morrison. In 1982, Bruno joined the pathology research group at the ILRAD as an employee of the Belgian government and more specifically, the ministry of foreign affairs, the general administration of development cooperation. This group was in a transitional phase changing its focus from the study of the pathogenesis of *Theileria Parva*, the cause of East Coast Fever in local cattle (Boran), towards unravelling the cellular immune response of cattle against this parasite. The research that took place in the Institute was groundbreaking and brought cellular immunology in cattle to new dimensions. Tissue histocompatibility molecules were characterized. Monoclonal antibodies against bovine immunoglobulins (antibodies) were produced as well as monoclonals against surface antigens of different leukocytes. This allowed to identify these cells and to study their functions. Tissue histocompatibility antigens were characterized and their role in immunity against the parasite was unravelled. Techniques to purify cells were optimized, cell lines were generated, and the stimulation of T-lymphocytes was studied with the aim of developing a vaccine against *Theileria Parva*. This groundbreaking research on the immune system of bovines and the immunity in bovines anticipated in some areas even the knowledge on the immune system of man.

Top researchers were brought together in the board of directors of the ILRAD, including Peter Doherty, a veterinarian, who joined the board in 1986. In 1996, Peter Doherty together with Rolf Zinkernagel received the Nobel Prize in Physiology or Medicine for their research on cell-mediated immunity published in 1973. You might remember that this same Peter Doherty was invited at our faculty in 2009, for a symposium in his honour during the celebration of 75 years of veterinary medicine at Ghent University.

In 1989, Bruno Goddeeris joined the KULeuven to teach immunology at the Faculty of Bioscience Engineering and in 1992 he was also appointed

at the UGent in a 20% position to teach immunology and develop immunological research at the Faculty of Veterinary Medicine.

At the ILRAD, Bruno and his colleagues were writing a part of the history in veterinary immunology. If you yourself had contributed to this history and you would know how veterinary immunology had evolved, than you would understand the interest of Bruno in the origin of the historical developments and discoveries in the field of immunology. Collecting information and even items about the history of immunizations and vaccinations has become one of Bruno's hobbies. Bruno already displayed his extensive knowledge in a number of fascinating lectures for a varied audience.

This brings us seamlessly to the Sarton Lecture this Thursday 8 March at 16.30 h by emeritus prof. dr. Bruno Goddeeris entitled "The Immunological tsunami at the end of the 19th century": Paris versus Berlin.

The immunological tsunami at the end of the 19th century: Paris versus Berlin

Bruno Maria Goddeeris

Prologue

In August 1881, the whole Medical Scientific Community was gathered together in the St James Music Hall (figure 1) in London for the International Medical Congress: "tout le beau monde était là". This magnificent music hall had been erected in the 1850's between Piccadilly and Regent street, but unfortunately was demolished in the beginning of the 20th century.

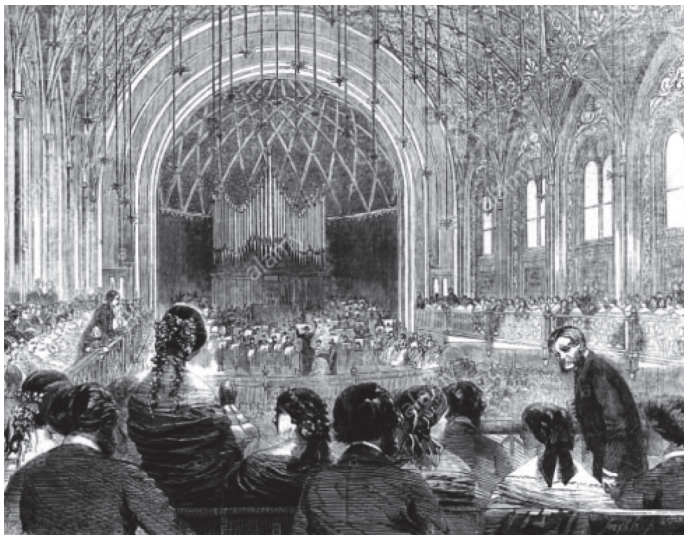


Figure 1. The St James Music Hall (Wikipedia)

There was a congregation of more than 3000 people and the supreme authority of that time on microbiology (non-spontaneous generation) was present, namely Louis Pasteur. He came to report to the scientific community his vaccination experiments against chicken cholera (pasteurellose) and anthrax. He was applauded by the whole audience. Pasteur had reached the age of 59 and was recognised as "le Maître". Other important people such as Sir Joseph Lister, the authority on the newly developed hygienic practices in surgery, and Rudolf Virchow, the "éminence grise" in anatomopathology, were also present. A much younger man of 39 years old, namely Robert Koch attended the meeting to demonstrate his culture techniques of anthrax bacilli and to display his wonderful microscopic pictures of the latter: he was a technological master in bacteriological culture and microscopic photography. Lister was diplomatic enough not to invite the young German at his dining table with the authoritarian French master but instead invited Koch to a demonstration of his technical skills in his lab where the two met each other. One has to keep in mind that in 1881 the tension between France and Germany was still high, as it had been only 10 years since the French had been annihilated in the French-Prussian war, which was not forgotten even in the scientific world and certainly not by a "bonapartiste" like Pasteur.

Napoleon III had declared the war to king Wilhelm of Prussia as he refused to answer and retract his family interests on the crown of Spain. Chancellor Bismarck was all too happy with the war declaration as it gave him the opportunity to unite the North Deutsche bund and the South Deutsche bund in the war against France. The Blitzkrieg was already finished within six months and ended in the restauration of the French Republic and even more so in the unification of the German States culminating into an apotheosis by the proclamation of King Wilhelm of Prussia as Kaiser of Germany in the mirror gallery of Versailles (figure 2).

What an insult it must have been for Louis Pasteur, a fervent bonapartiste. Indeed, Pasteur's father had served in the army of Bonaparte and Pasteur himself had established very good relations with Napoleon III and "Ll'Impératrice". Strange enough, immediately after the defeat of France, Pasteur became a good republican with strong contacts in parliament such as Paul Bert: it appeared that Pasteur was an excellent PR man, meaning political as well as public.

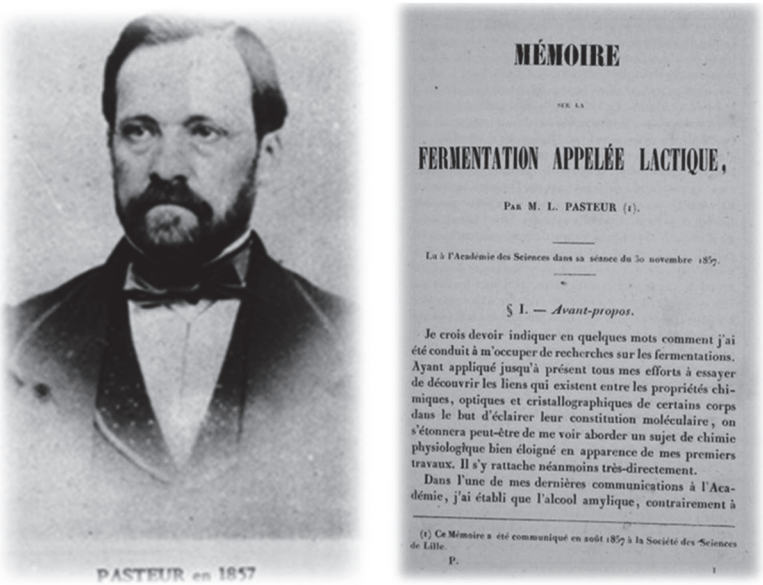
Figure 2. Coronation of King Wilhelm of Prussia in 1871 as the first Emperor of an united Germany in the Hall of Mirrors at the Palace of Versailles (painting of Anton Alexander von Werner in 1885)

The two titans

Louis Pasteur (1822-1895)

Louis Pasteur was a chemist by education and his scientific career can be subdivided in three phases: phase one (1847-1857), the physicist and chemist, phase two the microbiologist (1857-1877) and phase three (1877-1892) the medical scientist. In 1847 he obtained his M.Sc. at the "École Normale Supérieure de Paris". Although the name of that school does not reveal its full splendour, the school retains the highest educational level in fundamental sciences and engineering in France, even today. There he obtained also his PhD (doctorate in chemical and physical sciences) on rotational polarisation of light on liquids[1].

[1] «*1. Études des phénomènes relatifs á la polarisation rotatoire des liquides. 2. Application de la polarisation rotatoire des liquides à la solution de diverses questions de chimie*»


PASTEUR en 1857

MÉMOIRE

SUR LA

FERMENTATION APPELÉE LACTIQUE,

PAR M. L. PASTEUR (1).

Lu à l'Académie des Sciences dans sa séance du 30 novembre 1857.

§ I. — *Avant-propos.*

Je crois devoir indiquer en quelques mots comment j'ai été conduit à m'occuper de recherches sur les fermentations. Ayant appliqué jusqu'à présent tous mes efforts à essayer de découvrir les liens qui existent entre les propriétés chimiques, optiques et cristallographiques de certains corps dans le but d'éclairer leur constitution moléculaire, on s'étonnera peut-être de me voir aborder un sujet de chimie physiologique bien éloigné en apparence de mes premiers travaux. Il s'y rattache néanmoins très-directement.

Dans l'une de mes dernières communications à l'Académie, j'ai établi que l'alcool amylique, contrairement à

(1) Ce Mémoire a été communiqué en août 1857 à la Société des Sciences de Lille.

P. 1

Figure 3. Pasteur and his publication of 1858 on lactic fermentation (offprint from Annales de Chimie et de Physique)

In 1848 he became a teacher of physics at the lycée of Dijon (thus not so far away from his natal region of Arbois in the Jura) but took one year later already the position of assistant professor of chemistry at the Faculty of Sciences in Strasbourg and in 1852 the chair of chemistry. He obtained his first scientific breakthrough in science with his study on the stereochemistry of crystals and the demonstration of the dextro and levo form of tartaric acid crystals in wine. It allowed him in 1854 to become professor and dean of the faculty of Sciences in Lille where he started his important studies on the fermentation processes and anaerobes with contributions to the wine and beer industries in France.

In 1857 he returned to his Alma Mater, "l'École Normale Supérieure", in Paris to become administrator and director of scientific studies. He will perform there all the important discoveries and successes in his career on vaccination against infectious diseases such as chicken cholera, anthrax and rabies and some other animal diseases. He firstly produced a second scientific breakthrough with his "mémoire sur la fermentation appelée lactique" (figure 3), his first indication to abandon the theory of spontaneous generation. In 1860 he revealed in the Academy of Sciences his

desire to extend these studies into the field of infectious diseases[2] and was elected as a member of the Academy of Sciences in 1862. In 1865 he made his third important contribution to science by developing a procedure for the sterilisation, namely "pasteurization"[3].

Figure 4. Map of Paris with localisations of Pasteur and pictures of École Normale Supérieure (from 1857-88) in rue d'Ulm and Pasteur Institute (from 1888) in Rue Dr. Roux

In 1867 he stopped his function as administrator of scientific studies to become director of the new Laboratory of Physiological Chemistry at "l'École Normale Supérieure" and Professor of Organic Chemistry at the Sorbonne. That period was characterized by his work on silk worm diseases, namely pepper disease (pébrine) and flacherie, which was annihilating the silkworm industry in France. In 1868 he received a "Doctor honoris causa" from the University of Bonn but got unfortunately that same year his first hemiplegia in 1868. Napoleon III sent him to his resort in Trieste for recovery and made him "Commandeur de la Légion d'Honneur". In 1870 he wrote a book on the elucidation of silkworm diseases and dedicated it to the Empress Eugénie, a present(iment) for her and her husband before their removal from power one year later by the Germans in the French-Prussian war of 70-71. Moreover he proclaimed

2 *"Ce qu'il y aurait de plus désirable, serait de conduire assez loin ces études pour préparer la voie à une recherche sérieuse de l'origine des diverses maladies"*

3 *«Procédé de conservation et d'amélioration des vins par chauffage modéré à l'abri de l'air»*

himself the saviour of the silk industry in France which now revealed his less flattering and un-collegial nature. Indeed, he was not the first to have discovered the causes of this silkworm diseases, on the contrary during more than two years he obstinately refused to recognize the infectious nature of this disease. Indeed between 1865 and 68, it was Antoine Béchamp, professor of medicinal chemistry and pharmaceutics at the Medical Faculty of Montpellier, who suggested the parasitic nature of the disease, a "microzyma" or microbe invading the silkworm (*Bombyx mori*) from the outside, not being a spontaneous generation. He advised a treatment with creosote and the humidity control of feed leaves. He demonstrated the vibrant "microcorpuscle" to be a microphyte ferment "*Microzyma bombicis*". Pasteur who started his study in 1865 found nothing particular, claimed the corpuscles not to be pathogens, refused to acknowledge the parasitic nature but professed that the disease was a poisoning inducing the appearance of spontaneous corpuscles. It is only in 1868 that he admits that his colleague Béchamp is wright but refuses to give him credit but instead takes the credit to himself. Even the method of curing the disease by sorting the contaminated from the uncontaminated eggs or mother moths (called "grainage") had already been shown by professor Armand de Quatrefages in 1859 (Quatrefages 1860). Much later and without any shame, Pasteur redicules Béchamp publically during the Medical Congress of London of 1881 with false accusations that Béchamp pretends spontaneous generation in pebrine, i.e. an endogenous cause which Pasteur himself had maintained for that disease against the exogenous theory of Béchamp. In 1873 he was elected member of the Academy of Medicine and obtained through political relations (Paul Bert) an annuity a 25,000 Francs (equivalent today to €250,000) from "La Troisième République", not bad for a fervent Bonapartiste/Napoleoniste.

In 1877, still at the École Normale Supérieur, rue d'Ulm (figure 4), Pasteur gave up his functions as professor to concentrate on the germ theory and its application in medicine and surgery[4]. He (had) surrounded himself with top scientists such as Charles Chamberland (1875), Emile Roux (1878), Louis Thuillier (1880), Edmond Nocard (1880), Léon Perdrix (1885) and Alexandre Yersin (1886). We remember Chamberland for his successes on sterilisation as inventor of the autoclave and the ultrafiltration candles. This

[4] 1878 Mémoire *« La théorie des germes et ses applications à la médecine et à la chirurgie »*

period of ten years was the most prolific of Pasteur for which he received world attention, credit and glamour. During that period Pasteur developed attenuated strains of the agents which enabled vaccinations against several diseases such as chicken cholera (pasteurellose) (1980), anthrax (1880-81), swine erysipelas (1883) and rabies (1885). Later we will come back to some of these diseases and vaccination protocols as they were a matter of scientific dispute and intrigue on who was the first or pretended to be first in the idea, discovery or application. In any case all the attention in France towards Pasteur through his flair of PR, paved the way to one of his most important accomplishments, namely the establishment of the Pasteur Institute in Paris (figure 4) and later on in other parts of France and parts of the world under French influence. In 1886 he orated in the Academy that as now vaccination against rabies after the bite had been successful (in reality therapeutic vaccination), time had come to create a vaccination center against rabies[5]. And indeed, a public fund was created and with the help of the Government, a site was allocated to Pasteur and construction was started. In 1888, president Carnot was able to open the newly established Pasteur Institute in Paris. What a glory for Pasteur but misfortune struck again that year with a cerebral haemorhage and his second hemiplegia. Pasteur was not actively involved anymore in the lab but directed and supervised the institute for the rest of his life. Again he was able to attract passionate scientists towards the Pasteur Institute such as Ilya Mechnikof (1888), Alphonse Lavaran (1989), and Albert Calmette (1991). Lavaran received the Nobel Prize (only established in 1901) in 1907 and Mechnikof in 1908. On his 70th birthday in December 1892 Pasteur was glorified with a ceremony honouring him at the Sorbonne: supported by and holding onto the arm of the President of the Republic, Sadi Carnot, Pasteur enters into the great amphitheatre[6] where Lister is welcoming him under the applauds of 4000 invited people (figure 5). Pasteur survives his friend Carnot, President of the Republic, who got assassinated two years later. But one year thereafter Pasteur also passes away and receives a presidential funeral in the Cathedral of Notre Dame where he was laid to rest but transferred one year later into the crypt mausoleum of the Pasteur institute.

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5 1868 *« La prophylaxie de la rage après morsure est fondée. Il y a lieu de créer un établissement vaccinal contre la rage »*

6 Decorated by Puvis de Chavanne and inaugurated by Pasteur in 1889

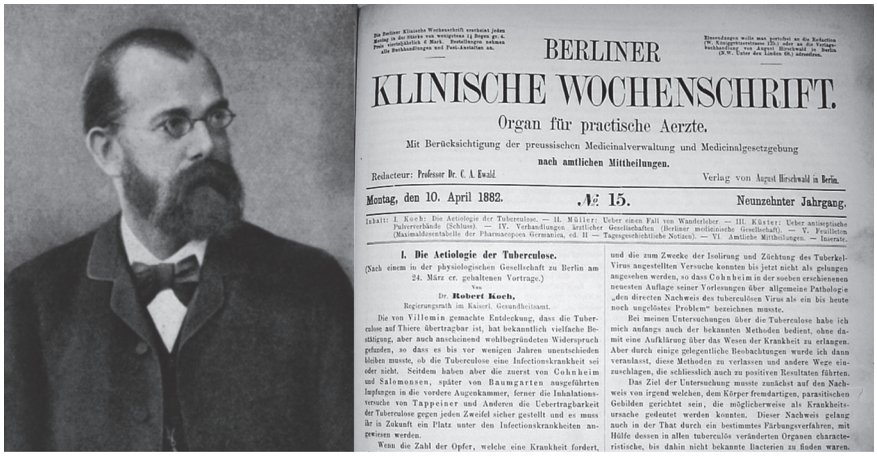
Figure 5. December 27th 1892: celebration of Pasteur on his 70th anniversary in the Grand Amphitheatre of the Sorbonne. Lister welcomes Pasteur supported by the French President Carnot (after a painting by Jean-André Rixens 1923)

Robert Koch

Robert Koch (figure 6) graduated in 1866 as a medical doctor from the University of Göttingen to follow that same year a course of the famous anatomo-pathologist Rudolf Virchow in Berlin. Also his medical and scientific career can, like the one of Pasteur, be divided into three periods: the first one as a normal medical practitioner with however important scientific realisations, the second as medical scientist and director of scientific institutes and the third as a travelling scientist with emphasis on tropical diseases of man and animals.

In the French-Prussian War of 1870-71, Koch was enlisted as an army doctor and stationed at Orleans, but he got reassigned before the end of the war to his home country as medical practitioner (Kreisphysikus) in Wollstein, now Wolstyn in Poland. During his period as medical practictioner Koch wanted to elucidate the causes of diseases and started to study anthrax as the disease was a prevalent killer among the animals of his patients. His patients captured mice and other animals used as laboratory animal models and through detailed microscopic observations and cultures he was able to identify and visualise the bacterial cause and even more importantly its sporulation and explanation of its survival and re-emergence in nature. He observed that his sporulation data were very similar to

those of the famous Prussian botany professor Ferdinand Cohn of Breslau (now Wroclaw in Poland) who was studying Bacillus and sporulation in plants. On a subsequent visit, he demonstrated his culture and sporulation and convinced immediately the professor who became a major supporter of his results and allowed Koch to publish his data in his personal journal "Beiträge zur Biologie des Pflanzes"[7]. With the help of his wife, who had to warn him of coming clouds (not enough light), he was able to capture the bacteria by photography[8]. Moreover, to improve the microscopic and photographic quality he visited in 1878 Ernst Abbe, engineer at the Zeiss plant in Jena, who developed at his requests and with his advice the condenser and oil-immersion objectives.



BERLINER

KLINISCHE WOCHENSCHRIFT.

Organ für practische Aerzte.

Mit Berücksichtigung der preussischen Medicinalverwaltung und Medicinalgesetzgebung nach amtlichen Mittheilungen.

Redacteur: Professor Dr. C. A. Ewald. Verlag von August Hirschwald in Berlin.

Montag, den 10. April 1882. № 15. Neunzehnter Jahrgang.

Inhalt: I. Koch: Die Aetiologie der Tuberculose. — II. Müller: Ueber einen Fall von Wanderleber. — III. Küster: Ueber antiseptische Pulververbände (Schluss). — IV. Verhandlungen ärztlicher Gesellschaften (Berliner medicinische Gesellschaft). — V. Feuilleton. (Maximaldosentabelle der Pharmacopoea Germanica, ed. II. — Tagesgeschichtliche Notizen). — VI. Amtliche Mittheilungen. — Inserate.

I. Die Aetiologie der Tuberculose.

(Nach einem in der physiologischen Gesellschaft zu Berlin am 24. März cr. gehaltenen Vortrage.)

Von

Dr. **Robert Koch,**

Regierungsrath im Kaiserl. Gesundheitsamt.

Die von Villemin gemachte Entdeckung, dass die Tuberculose auf Thiere übertragbar ist, hat bekanntlich vielfache Bestätigung, aber auch anscheinend wohlbegründeten Widerspruch gefunden, so dass es bis vor wenigen Jahren unentschieden bleiben musste, ob die Tuberculose eine Infectionskrankheit sei oder nicht. Seitdem haben aber die zuerst von Cohnheim und Salmonsen, später von Baumgarten ausgeführten Impfungen in die vordere Augenkammer, ferner die Inhalationsversuche von Tappeiner und Anderen die Uebertragbarkeit der Tuberculose gegen jeden Zweifel sicher gestellt und es muss ihr in Zukunft ein Platz unter den Infectionskrankheiten angewiesen werden.

Wenn die Zahl der Opfer, welche eine Krankheit fordert, und die zum Zwecke der Isolirung und Züchtung des Tuberkel-Virus angestellten Versuche konnten bis jetzt nicht als gelungen angesehen werden, so dass Cohnheim in der soeben erschienenen neuesten Auflage seiner Vorlesungen über allgemeine Pathologie „den directen Nachweis des tuberculösen Virus als ein bis heute noch ungelöstes Problem" bezeichnen musste.

Bei meinen Untersuchungen über die Tuberculose habe ich mich anfangs auch der bekannten Methoden bedient, ohne damit eine Aufklärung über das Wesen der Krankheit zu erlangen. Aber durch einige gelegentliche Beobachtungen wurde ich dann veranlasst, diese Methoden zu verlassen und andere Wege einzuschlagen, die schliesslich auch zu positiven Resultaten führten.

Das Ziel der Untersuchung musste zunächst auf den Nachweis von irgend welchen, dem Körper fremdartigen, parasitischen Gebilden gerichtet sein, die möglicherweise als Krankheitsursache gedeutet werden konnten. Dieser Nachweis gelang auch in der That durch ein bestimmtes Färbungsverfahren, mit Hülfe dessen in allen tuberculös veränderten Organen charakteristische, bis dahin nicht bekannte Bacterien zu finden waren.

Figure 6. Robert Koch and his famous publication on the identification of the cause of tuberculosis

Through his major scientific achievements and publications on anthrax and wound infections (as a village medical practitioner), Koch became known as an authority on infectious diseases to the German medical authorities, who appointed him in 1880 at the age of 37 as government advisor and director of the new Laboratory of Bacteriology at the Imperial Health Office in Berlin (figure 7). Immediately he recruited good scientists who contributed to his discoveries: Georg Gaffky (1880), Friedrich Loffler

7 1876 "Die Ätiologie der Milzbrandkrankheit, begrundet auf die Entwicklungsgeschichte des Bacillus Anthracis"

8 1877 "Verfahren zur Untersuchung, zum Conservieren und Photographieren der Bakterien".

(1880), Walther and Fanny Hesse (1880). During that period he accumulated international recognition though his work on culture techniques, antiseptics and sterilisation. His method of culturing bacteria on solid media (from potato slices, to gelatin and finally agar) was a major breakthrough. It was Fanny Hesse who got the idea of using agar from a Dutch Indonesian friend who used it in her jellies and puddings keeping them solid in hot climate. From now on the true cause of a disease could be proven by inoculating a specific colony into a host what was not possible with liquid media which consisted always of a mixture of bacteria. Koch attended in that period the Medical Congress of 1881 in London where Pasteur was the center of attention.

But in 1882 Koch became world famous with his discovery and demonstration of the cause of tuberculosis, the tubercle bacillus, which got the name Koch's bacillus[9] (figure 6). In 1884 a new major success of which we will relate more in the next section of the tsunami itself, crowned Koch during his expedition to Egypt and India with the discovery and isolation of Vibrio cholera and in 1885 its experimental transmission.

Through all these scientific successes, Koch became a world authority in microbiology, was in 1885 appointed and rewarded with a new position as the administrator and the director of the Institute of Hygiene of the University of Berlin (figure 7). With his previous colleagues and new ones such a Petri, who invented the culture dish named after him, he continued his search for vaccination against tuberculosis. At a certain moment Koch pretended to have found a vaccine candidate against tuberculosis, namely tuberculin, but could not give evidence of its effectiveness. His continued research efforts to prove the vaccine efficacy of tuberculin ended in a personal failure and loss of prestige. However, during his directorship he attracted and got surrounded by famous scientists such Shibasaburo Kitasato (1885-1891), Emil (von) Behring (1886-87, 88-94), Paul Ehrlich (1890-1896), Richard Pfeiffer (1887-91-99) and August von Wassermann (1891-1906). Kitasato and Behring became famous with their work on antiserum therapy against diphtheria and tetanus by which Behring was rewarded with the first Noble Price for physiology and medicine in 1901. Ehrlich got the Noble Price in 1908 for his work on the identification of the

[9] 1882 March 24th Communication in the Auditorium of the Physiological Society; April 10th publication "Die Atiologie derTuberkulose"

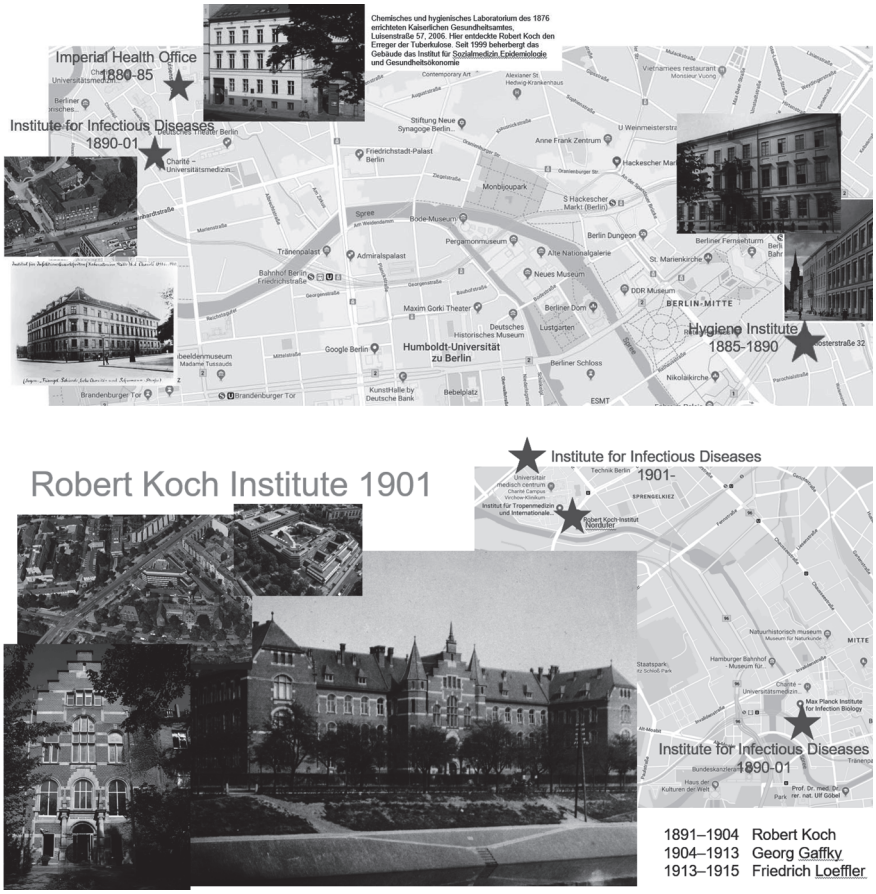


Figure 7. The different locations in Berlin where Robert Koch was in function

protective nature of antisera by antibodies, then called antitoxines as these bacterial diseases were caused by toxins. The bacterium *Pfeifferella mallei* (glanders, now called *Burkholderia mallei*) was named after Pfeiffer and Wassermann gave his name to the Wassermann-test for syphilis, a complement-fixation test. Berlin became the cradle of microbiology and immunology.

In 1991 Koch was appointed as director of the Prussian Institute for Infectious Diseases which after his death grew out to the famous the Robert Koch Institute in 1912 (figure 7). During that period, Koch became more interested in tropical diseases of man and animals and undertook in a 10 year span four scientific working missions into Africa.

- From 1896-97, he undertook his first mission to the Cape Colony for rinderpest. He demonstrated the non-bacterial nature of the cause but transmissibility by infected blood. The cause remained unattenuated by passage through animals and his antiserum, as well all other attempts failed to contain the disease.
- From 1897-98, he made his second trip to Africa, namely Tanganyika and Uganda for malaria, Texas cattle fever (babesiosis) and animal trypanosomosis, surra. For malaria he advocated the complete and systematic "quinisation" of all parasite carriers. With no scientific evidence, he suggested that the Ugandan plague might have been the source of the Justinian plague in Egypt in the sixth century AD but never isolated *Yersinia pestis*. During that mission, he rushed of to Bombay to investigate the bubonic plague and described the cannibalism among rats.
- Between 1900-01 he traveled on a government mission to German New Guinea and described acquired immunity in malaria.
- From 1903-04 he made on request of the South African Company his third trip to Africa, namely Southern Rhodesia for "East African coastal fever" (consulting fee equivalent of £200,000 today). He had one setback after another resulting in a complete failure of his so-say vaccine made from blood of recovered animals. In 1904 he was succeeded by Georg Gaffky as director (1904-1913) of the Institute for Infectious Diseases in Berlin.
- From 1905-07 he made his last trip in Africa to Tanganyika and Uganda for cattle diseases and sleeping sickness, with a short interruption to collect his Noble Price.

In 1908-09 he undertook a last trip to the United States and Japan where he visits his old PhD student the now famous Japanese professor Shibasaburo Kitasato. One year later, Koch gets a heart attack, never recovers and dies at the age of 65. His ashes are placed in the Berlin Institute for Infectious diseases where a mausoleum is built in the basement of the institute.

The Immunological Tsunami

The rush against anthrax

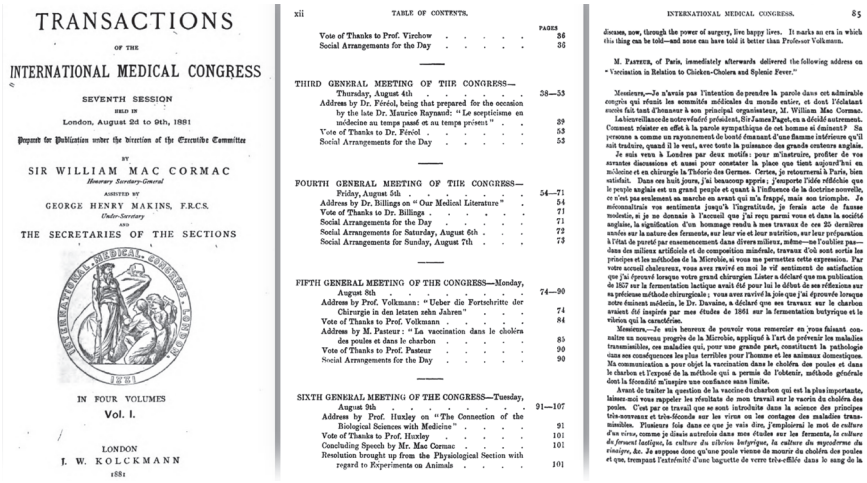
In the middle of the 19th century, France and Germany (we use here the word German although Germany got only established as a country in 1871) got involved into the quest for anthrax and its cause. In 1850 the French pathologist Pierre Rayer and physician Casimir Davaine were able to transmit the disease in sheep and observed filiform bodies in the blood. Around the same time in 1855 the German physician Aloys Pollender claimed to have observed since 1849 rods in the blood of diseased animals. But at the same period also French and German veterinary professors got involved into the study of anthrax. Professor Onésime Delafond of the veterinary institute of Maison Alford, Paris, had observed in 1838 rods in the blood of diseased animals, could produce limited cultures but did however not recognize them as a cause but rather as a consequence. Similarly, the German veterinary professor Friedrich Brauell was also able to transmit the disease in 1857 with diseased blood.

One decennium later Davaine suggested that the filiform bodies might be the germs causing the disease: the disease would be transmitted with diseased blood but not with the filtrate. However the claim for the cause did not get public acceptance as it did not give an explanation for the natural transmission as the disease always reemerges for years in the same paddock. Also the German professor Edwin Klebs (in 1883 discovered the Klebs-Loeffler bacillus, *Corynebacterium diphteriae*) and his student Ernst Tiegel came to similar conclusions as Davaine in 1871.

But it was in 1874 that Robert Koch demonstrated that the bacteria formed filaments with transparent points at regular intervals and made the first observation of spores of anthrax. In 1875 he was able to culture the bacteria in a drop of corpus vitrium fluid of rabbit with up to 8 passages and observed the same as in blood with refractive spheres in the filaments: spores with resistance to boiling and desiccation giving an answer to natural transmission. As mentioned above, in that period Koch visited professor Ferdinand Cohn, the authority on Bacillus with spore formation in plants, to present and discuss his data on anthrax. Cohn was enthusiastic

and enabled Koch to publish immediately (1876) his data in his personal journal "Beiträge zur Biologie des Pflanzes"[10].

When Pasteur presented his data on vaccination against chicken cholera and cholera in 1881[11] at the 7th International Medical Congress in London (figure 8), Koch did not give any comments. He criticized a few months later in an article Pasteur's data indicating that Pasteur was even not able to differentiate the anthrax bacteria from other pathogenic bacilli, that he never saw the septicemia in her non-complicated form and that he could not sustain the role of earthworms in bringing the bacteria from buried cadavers to the paddock's surface. In short, Koch declared that Pasteur has created rather confusion than solution. Pasteur took his revenge a few months later during the 4th international Congress of Hygiene and Demography of 1882 in Geneva where the whole press was gathered to witness the battle between Koch and Pasteur. Pasteur gave a talk on the attenuation of viruses (at that time the word virus was used for infectious organisms) and waited to the end of his talk to attack and give Koch an



TRANSACTIONS

OF THE

INTERNATIONAL MEDICAL CONGRESS

SEVENTH SESSION

HELD IN

London, August 2d to 9th, 1881

Prepared for Publication under the direction of the Executive Committee

BY

SIR WILLIAM MAC CORMAC

Honorary Secretary-General

ASSISTED BY

GEORGE HENRY MAKINS, F.R.C.S.

Under-Secretary

AND

THE SECRETARIES OF THE SECTIONS

IN FOUR VOLUMES

Vol. I.

LONDON

J. W. KOLCKMANN

1881

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disease, now, through the power of surgery, live happy lives. It marks an era in which this thing can be told—and none can have told it better than Professor Volkmann.

M. PASTEUR, of Paris, immediately afterwards delivered the following address on "Vaccination in Relation to Chicken-Cholera and Splenic Fever."

Messieurs,—Je n'avais pas l'intention de prendre la parole dans cet admirable congrès qui réunit les sommités médicales du monde entier, et dont l'éclatant succès fait tant d'honneur à son principal organisateur, M. William Mac Cormac.

La bienveillance de notre vénéré président, Sir James Paget, en a décidé autrement. Comment résister en effet à la parole sympathique de cet homme si éminent? Sa personne a comme un rayonnement de bonté émanant d'une flamme intérieure qu'il sait traduire, quand il le veut, avec toute la puissance des grands conteurs anglais.

Je suis venu à Londres par deux motifs: pour m'instruire, profiter de vos savantes discussions et aussi pour constater la place que tient aujourd'hui en médecine et en chirurgie la Théorie des Germes. Certes, je retournerai à Paris, bien satisfait. Dans ces huit jours, j'ai beaucoup appris; j'emporte l'idée réfléchie que le peuple anglais est un grand peuple et quant à l'influence de la doctrine nouvelle, ce n'est pas seulement sa marche en avant qui m'a frappé, mais son triomphe. Je méconnaîtrais vos sentiments jusqu'à l'ingratitude, je ferais acte de fausse modestie, si je ne donnais à l'accueil que j'ai reçu parmi vous et dans la société anglaise, la signification d'un hommage rendu à mes travaux de ces 25 dernières années sur la nature des fermente, sur leur vie et leur nutrition, sur leur préparation à l'état de pureté par ensemencement dans divers milieux, même—ne l'oublions pas—dans des milieux artificiels et de composition minérale, travaux d'où sont sortis les principes et les méthodes de la Microbie, si vous me permettez cette expression. Par votre accueil chaleureux, vous avez ravivé en moi le vif sentiment de satisfaction que j'ai éprouvé lorsque votre grand chirurgien Lister a déclaré que ma publication de 1857 sur la fermentation lactique avait été pour lui le début de ses réflexions sur sa précieuse méthode chirurgicale; vous avez ravivé la joie que j'ai éprouvée lorsque notre éminent médecin, le Dr. Davaine, a déclaré que ses travaux sur le charbon avaient été inspirés par mes études de 1861 sur la fermentation butyrique et le vibrion qui la caractérise.

Messieurs,—Je suis heureux de pouvoir vous remercier en vous faisant connaître un nouveau progrès de la Microbie, appliqué à l'art de prévenir les maladies transmissibles, ces maladies qui, pour une grande part, constituent la pathologie dans ses conséquences les plus terribles pour l'homme et les animaux domestiques. Ma communication a pour objet la vaccination dans le choléra des poules et dans le charbon et l'exposé de la méthode qui a permis de l'obtenir, méthode générale dont la fécondité m'inspire une confiance sans limite.

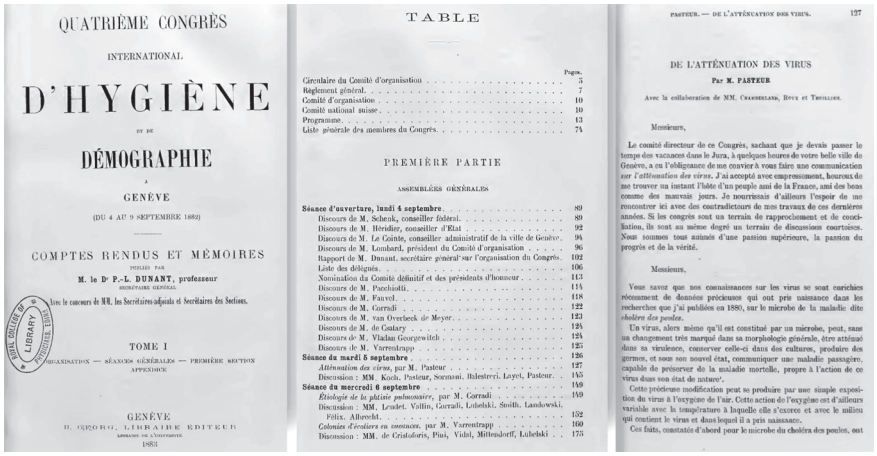
Avant de traiter la question de la vaccine du charbon qui est la plus importante, laissez-moi vous rappeler les résultats de mon travail sur le vaccin du choléra des poules. C'est par ce travail que se sont introduite dans la science des principes très-nouveaux et très-féconds sur les virus ou les contagee des maladies transmissibles. Plusieurs fois dans ce que je vais dire, *j'emploierai* le mot de *culture d'un virus*, comme je l'avais autrefois dans mes études sur les fermente, le *culture du ferment lactique, la culture du vibrion butyrique, la culture du mycoderme du vinaigre, &c. Je suppose donc qu'une poule vienne de mourir du choléra des poules* et que, trempant l'extrémité d'une baguette de verre très-effilée dans le sang de la

Figure 8. Pasteur's communication at the International Medical Congress of London in 1881

10 1876 "Die Ätiologie der Milzbrandkrankheit, begründet auf die Entwicklungsgeschichte des Bacillus Anthracis"

11 1881 "La vaccination dans le choléra des poules et dans le charbon"

answer on all his critics[12] (figure 9). Pasteur left the podium and the hall, not allowing Koch to respond. Koch only replied "I didn't hear anything new today" and formulated again in an article his rather *ad hominum* critics on Pasteur: no important news on ways of attenuation; as Pasteur is not a medical doctor, we might not expect from him correct judgements on pathological processes and symptoms; Pasteur only communicates on those experiments in favour of his claims. It is a pity that Koch such famous scientist responded in a rather personal attack and not in a scientific way.



QUATRIÈME CONGRÈS

INTERNATIONAL

D'HYGIÈNE

ET DE

DÉMOGRAPHIE

À

GENÈVE

(DU 4 AU 9 SEPTEMBRE 1882)

COMPTES RENDUS ET MÉMOIRES

PUBLIÉS PAR

M. le Dr P.-L. DUNANT, professeur

SECRÉTAIRE GÉNÉRAL

Avec le concours de MM. les Secrétaires-adjoints et Secrétaires des Sections.

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H. GEORG, LIBRAIRE ÉDITEUR

LIBRAIRIE DE L'UNIVERSITÉ

1883

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DE L'ATTÉNUATION DES VIRUS

Par M. PASTEUR

Avec la collaboration de MM. Chamberland, Roux et Thuillier.

Messieurs,

Le comité directeur de ce Congrès, sachant que je devais passer le temps des vacances dans le Jura, à quelques heures de votre belle ville de Genève, a eu l'obligeance de me convier à vous faire une communication *sur l'atténuation des virus*. J'ai accepté avec empressement, heureux de me trouver un instant l'hôte d'un peuple ami de la France, ami des bons comme des mauvais jours. Je nourrissais d'ailleurs l'espoir de me rencontrer ici avec des contradicteurs de mes travaux de ces dernières années. Si les congrès sont un terrain de rapprochement et de conciliation, ils sont au même degré un terrain de discussions courtoises. Nous sommes tous animés d'une passion supérieure, la passion du progrès et de la vérité.

Messieurs,

Vous savez que nos connaissances sur les virus se sont enrichies récemment de données précieuses qui ont pris naissance dans les recherches que j'ai publiées en 1880, sur le microbe de la maladie dite *choléra des poules*.

Un virus, alors même qu'il est constitué par un microbe, peut, sans un changement très marqué dans sa morphologie générale, être atténué dans sa virulence, conserver celle-ci dans des cultures, produire des germes, et sous son nouvel état, communiquer une maladie passagère, capable de préserver de la maladie mortelle, propre à l'action de ce virus dans son état de nature.

Cette précieuse modification peut se produire par une simple exposition du virus à l'oxygène de l'air. Cette action de l'oxygène est d'ailleurs variable avec la température à laquelle elle s'exerce et avec le milieu qui contient le virus et dans lequel il a pris naissance.

Ces faits, constatés d'abord pour le microbe du choléra des poules, ont

Figure 9. Pasteur's communication at International Conference of Hygiene and Demography in Geneva in 1882

While Koch was only busy with the finite identification of the causative organisms of diseases, Pasteur went further on by finding ways of protection against the diseases. As already mentioned Pasteur made international communications on his attenuation of viruses and their use as a vaccine against diseases. But even in France arose an even more intricate debacle on who was first in making and demonstrating a vaccine against anthrax. The veterinarian, professor Henri Toussaint of Toulouse was working on anthrax and published already in 1879 on his experiments on anthrax. On July 12th, Toussaint announced at the Academy of Sciences by communication of his veterinary supporting academy members, professors Henri Bouley of Maison Alfort and Auguste Chauveau of Lyon, the successful

[12] Quatrième Congrès International d'Hygiène et de Démographie à Genève: comptes rendus et mémoires 1, 127-145(149).

vaccination and deposited under sealed envelope his method of attenuation and vaccination. Pasteur on the other hand, who was also working on anthrax, had announced on February 18th in the Academy the successful vaccination against another disease, namely cholera in chickens with an attenuated chicken cholera vaccine but refused to reveal his methods of attenuation. After Toussaint had deposited his method against anthrax, Pasteur was again asked in the Academy to reveal his method of attenuation but still refused with a public reprimand of the Academy as a consequence. The sealed envelope of Toussaint was opened on the 2nd August and revealed vaccination by two interspaced injections with defibrinated blood, heated for 10 min at 55°C in the presence of 0.25% phenol. Pasteur also claimed to have a vaccine against anthrax and was now challenged to demonstrate the efficacy of his vaccine. The famous experiment of vaccination and challenge was performed at Pouilly-Le-Fort by his two devotees Roux and Chamberland and became a total success. Pasteur was acclaimed in the whole world and revealed his method of attenuation in cultures to be by oxygen of air. Only later on, once the lab books of Pasteur were released after his death, became it known that he used a method similar to the one of Toussaint, i.e. heat treatment and attenuation (inactivation?) with potassium dichromate. This strange behaviour of Pasteur, such a notorious and brilliant scientist, is unforgivable and a-collegial, the more that Toussaint who was an admirer of Pasteur, had procured him the strains of chicken cholera which led to his successful research and first vaccination experiments. Alas, in 1881 the unfortunate Toussaint contracted a neurological disease. In 1882 the Academy of Medicine awarded Toussaint with the Prix Vaillant et la Legion d'Honneur on his "Mémoire sur l'immunité contre le charbon" without the consent of Pasteur who was pressurized by Bouley and Chauveau to agree.

When two dogs fight over a bone, a third one carries it away. While this scientific battle on who was first in vaccination against anthrax was going on in France, professor William Greenfield of the Brown Animal Sanatory Institution of London published his results on attenuation of the anthrax bacillus and his successful vaccination experiments in the months April and June of 1880[13], thus just before Toussaint. We could conclude that

[13] *Proceedings of the Royal Society of London 1880 30, 557-560 June; Journal of the Royal Agricultural Society 1880 16, part i, April.*

when the time is ripe and the conditions are there, several scientists in different countries came up with similar ideas on attenuation of viruses and vaccination. It is good to remember that Edward Jenner preceded these ego's almost by one century in vaccination with his vaccinia virus. But as appears in science quite often, the story on vaccination against smallpox is a little bit similar to that of anthrax in that not Jenner but Benjamin Jesty, a farmer in Yetminster Dorset (not so far away from the practice of Jenner), was in 1774 the first to use vaccinia in Britain (I do not use the word cowpox as it now genotypically known that vaccinia is almost identical to horsepox and not cowpox) to protect his wife and two children against smallpox. But like it happens quite often in science, these guys were also preceded on the continent in 1769 by Jobst Bose, a teacher in Göttingen.

The fight against cholera

As mentioned already above, an epidemic of cholera dysentery broke out in Egypt in 1883 and the Imperial Office dispatched its team of Koch, Gaffky, Bernhard Fisher and Hermann Treskow a chemist of the Imperial Sanitary Office (figure 10). Only one week earlier the Pasteurians Emile Roux, Louis Thuillier, Isidore Straus, a medical doctor of the medical Faculty of Paris, and the veterinarian Edmond Nocard, professor of pathology at Maison Alfort had arrived in Egypt.

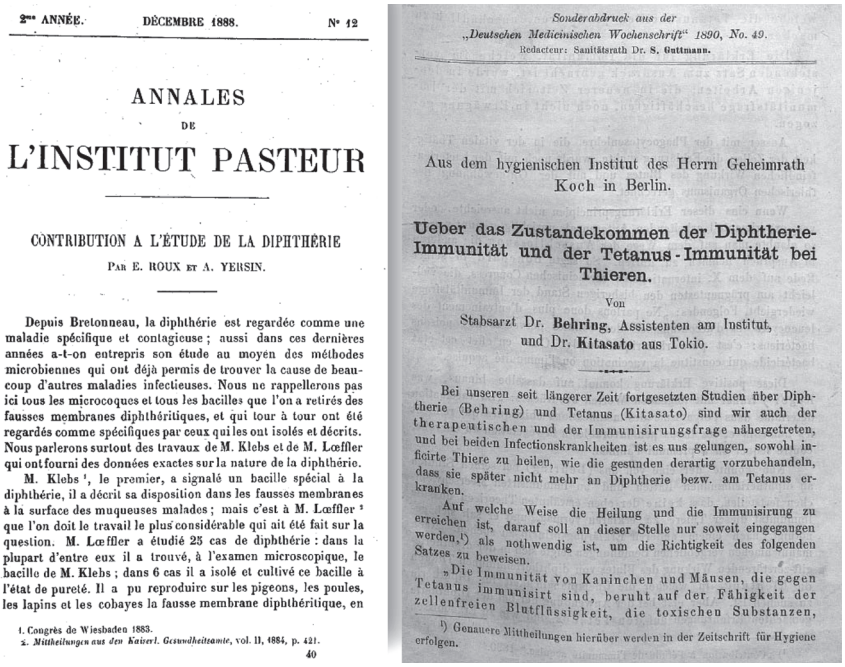
Figure 10. German mission in Egypt on cholera (1884): Robert Koch is 3rd from right. Wikimedia Commons

One month after their arrival the epidemic had stopped but great misfortune struck the French team who was now studying rinderpest, as Thuillier got sick and succumbed to cholera. Courtesy of the Germans was then very high as they attended the funeral and Koch himself served as pallbearer in the funeral ceremony for Thuillier. In spite of the failure and departure of the French, the Imperial team managed to isolate the bacteria from patients, however they obtained no culture nor transmission. But Koch wanted a deeper study on the cause and continued his journey to Calcutta (Ganges river) where another epidemic was ravaging. There again he was able to isolate the cause from patients and was now capable to culture the bacterium and even more so to unveil the source of contamination, namely the drinking water. He succeeded in halting the infections through sanitation of the latter. However he could still not transmit experimentally the disease[14]. In 1884 on his return to Berlin, Koch was received with the highest honours by the Kaiser and Bismarck and was again a most celebrated man in Europe and the rest of the world. Even more so, upon an outbreak of cholera in the French port of Toulon in 1884, the French Government invited the cholera-world-expert Koch to solve the problem to great disapproval of Pasteur who had sent also his team of Roux and Straus. Moreover, the next year Koch succeeded in the transmitting experimentally *Vibrio cholera* to guinea pig by first neutralising the acidic pH of the stomach with sodium bicarbonate (a similar system we still apply in our experimental infections with *Escherichia coli* in swine at the University of Gent) and gave finally conclusive evidence of the cause of cholera.

The rush for antitoxins

In 1883 Klebs detected small drumsticks in pseudomembranes of patients with diphtheria. One year later Friedrich Löffler of Koch's lab was able to culture bacteria from these pseudomembranes. He observed that the bacteria remain at the inoculation site and must thus produce a systemic toxin to exert its disease effects. But it were the Pasteurians Emile Roux and Alexandre Yersin who identified in 1887-88 the toxin through filtration of cultures and even asked themselves whether the host could develop

[14] "Die Bekämpfung des Typhus", "Über die Cholerabakterien"



2me ANNÉE. DÉCEMBRE 1888. N° 12

ANNALES
DE
L'INSTITUT PASTEUR

CONTRIBUTION A L'ÉTUDE DE LA DIPHTHÉRIE

PAR E. ROUX ET A. YERSIN.

Depuis Bretonneau, la diphthérie est regardée comme une maladie spécifique et contagieuse ; aussi dans ces dernières années a-t-on entrepris son étude au moyen des méthodes microbiennes qui ont déjà permis de trouver la cause de beaucoup d'autres maladies infectieuses. Nous ne rappellerons pas ici tous les microcoques et tous les bacilles que l'on a retirés des fausses membranes diphthéritiques, et qui tour à tour ont été regardés comme spécifiques par ceux qui les ont isolés et décrits. Nous parlerons surtout des travaux de M. Klebs et de M. Lœffler qui ont fourni des données exactes sur la nature de la diphthérie.

M. Klebs [1], le premier, a signalé un bacille spécial à la diphthérie, il a décrit sa disposition dans les fausses membranes à la surface des muqueuses malades ; mais c'est à M. Lœffler [2] que l'on doit le travail le plus considérable qui ait été fait sur la question. M. Lœffler a étudié 25 cas de diphthérie : dans la plupart d'entre eux il a trouvé, à l'examen microscopique, le bacille de M. Klebs ; dans 6 cas il a isolé et cultivé ce bacille à l'état de pureté. Il a pu reproduire sur les pigeons, les poules, les lapins et les cobayes la fausse membrane diphthéritique, en

1. Congrès de Wiesbaden 1883.
2. *Mittheilungen aus den Kaiserl. Gesundheitsamte*, vol. II, 1884, p. 421.

40

Sonderabdruck aus der „Deutschen Medicinischen Wochenschrift" 1890, No. 49.
Redacteur: Sanitätsrath Dr. S. Guttmann.

Aus dem hygienischen Institut des Herrn Geheimrath Koch in Berlin.

Ueber das Zustandekommen der Diphtherie-Immunität und der Tetanus-Immunität bei Thieren.

Von

Stabsarzt Dr. **Behring**, Assistenten am Institut, und Dr. **Kitasato** aus Tokio.

Bei unseren seit längerer Zeit fortgesetzten Studien über Diphtherie (Behring) und Tetanus (Kitasato) sind wir auch der therapeutischen und der Immunisirungsfrage nähergetreten, und bei beiden Infectionskrankheiten ist es uns gelungen, sowohl inficirte Thiere zu heilen, wie die gesunden derartig vorzubehandeln, dass sie später nicht mehr an Diphtherie bezw. am Tetanus erkranken.

Auf welche Weise die Heilung und die Immunisirung zu erreichen ist, darauf soll an dieser Stelle nur soweit eingegangen werden,[1]) als nothwendig ist, um die Richtigkeit des folgenden Satzes zu beweisen.

„Die Immunität von Kaninchen und Mäusen, die gegen Tetanus immunisirt sind, beruht auf der Fähigkeit der zellenfreien Blutflüssigkeit, die toxischen Substanzen,

[1]) Genauere Mittheilungen hierüber werden in der Zeitschrift für Hygiene erfolgen.

Figure 11. Publications of Roux & Yersin, 1888 and Behring & Kitasato, 1890 on the diphtheria toxins and immunity

resistance against the toxin[15] (figure 11). However their research appeared now to be delayed, probably due to preparations for the move of the Pasteur labs from Rue d'Ulm (5e arrondissement) to their completely new buildings, namely the Pasteur Institute in rue dr. Roux (15e arrondissement) (see figure 4) which was officially opened by president Carnot in 1888, indeed the same man who supported a few years later the paraplegial Pasteur when entering the amphitheater of the Sorbonne on his 70th birthday.

Now it were the Kochians Emil Behring and Kitasato Sibasaburo who took over the lead and were able to protect guinea pigs by infection with diphtheria and tetanus, respectively, and subsequent treatment with the antiseptic iodine tricloride and suggested the treatment of human diphtheria patients with sera from immunized animals, called serotherapy[16] (figure

15 Publication "Contribution á l'étude de la diphtérie" (Roux & Yersin 1888 Ann Inst Pasteur 1888); est-il possible d'accoutumer les animaux au poison diphtérique?

16 "Über das Zustandekommen der Diphterie-Immunität und Tetanus-Immunität bei Thieren" (Behring & Kitasato 1890 D Med Wochenschr); "Untersuchungen über das Zustandekommen der Diphterie-Immunität bei Thieren" (Behring 1890 D Med Wochemschr).

11). From 1890 to 1892, Behring and Wernicke started to prepare anti-diphtheria sera in sheep for human treatment but due to failure in financing they were stopped. Thanks to a grant from the Ministry of Agriculture in 1891-92, they were able to continue their efforts but now for preparation of anti-tetanus sera which resulted in successful anti-tetanus serum therapy in horses. In 1892-93, thanks to a contract with the company Meister, Lucius & Co of Hoechst (the later pharmaceutical company Hoechst), they were able to continue their work on diphtheria and performed successfully the first clinical trials in humans. Emil Behring was awarded the first Noble Prize in Medicine in 1901 for, as quoted by the committee, "his work on serum therapy, especially its application against diphtheria, by which he has opened a new road in the domain of medical science and thereby placed in the hands of the physician a victorious weapon against illness and disease". Strange that Kitasato although nominated, was not also awarded the Noble Prize as he appeared also on the first publication with Behring. It was probably due to the fact that Behring was working on diphtheria with major applications in human health and that Kitasato was rather working on tetanus in horses. Nonetheless, both should be recognized as the first to apply serotherapy for disease prevention. It was only later that the Kochian Paul Ehrlich was able to demonstrate that it was the formation and protective role of antitoxins (i.e. antibodies) which were responsible in binding and neutralizing the toxins in the body. In 1908, he but also the Pasteurian Ely Metchnikoff working on the protective role of macrophages, were awarded the Noble Prize in Medicine "in recognition of their work on immunity", as quoted by the Committee.

The Bubonic Plague in Hong Kong

In 1887 the physician Alexandre Yersin is accepted in Pasteur's lab but will also spend the next year two months in Koch's lab for his research and PhD on tuberculosis. Yersin was a Swiss but acquired later on when residing in Vietnam the French nationality. As already mentioned above, that same year he identifies together with Emile Roux the diphtheria toxin. But for the moment research at the Pasteur Institute appears not to be his priority and in 1890 he leaves for French Indochina (at that time a French colony comprising Vietnam, Cambodia and in 1893 also Laos) as a ship-surgeon

on a commercial ship line for passenger transport between Saigon and Manilla, and Saigon and Hai Phong along the Vietnamese coast. In 1891 he meets in Saigon his colleague from the Pasteur Institute, Albert Calmette, who is there on request of Pasteur to create a lab for vaccine production. The adventurer Yersin is not yet convinced to do lab work but instead will make three risky explorations between 1892 and 1894 from Nha Trang into the mainland and Cambodia. In 1894 the French Government orders Yersin to go on a mission to Yunnan, a Chinese province bordering Vietnam, to study an epidemic of Bubonic Plague ravaging that province and forming a threat to the French colony. However, with the help of Calmette who is already back in Paris, Yersin convinces the government to send him instead to Hong Kong where the pest has already spread to.

The pest, bubonic plague or black death has been responsible for pandemics and has for centuries been the main killer disease in human population. At the end of the 19th century a new pandemic[17] erupted in the province of Yunnan in South-West China, spread west into the Guangdong Province towards Hong Kong by people migrating to the west due to the Panthay rebellion of Chinese Muslims of Yunnan against the Manchu rulers of the Qing Dynasty. Later on it spread to Bombay (1897), Suez, Madagascar, Alexandria, Japan, East Africa, Portugal, Brazil (1899), Sidney, Glasgow, San Francisco (1900), Honolulu (1908), Java (1911), Ceylon (1914) and Marseille (1920), honouring its name as pandemic.

Also the Japanese government was worried with the closeness of the outbreak in Hong Kong and sent in 1894 a scientific medical mission to Hong Kong headed by Kitasato Shibasaburo. As already mentioned above, Kitasato had been working in Koch's lab in Berlin from 1885-1891, and founded on his return to Japan the Institute for Study of Infectious Diseases. In 1898, Kitasato with his student Shiga Kiyoshi was able to isolate and describe the organism that caused dysentery, a bacterium which was given later the name of *Shigella dysenteriae*. In 1921 Kitasato founded with other colleagues the Sekisen Ken-onki Corporation, which became later the Terumo Corporation.

[17] the 3rd pandemic as opposed to the 1st one or the plague of Justinianus in the 6th century (50 million deaths), and the 2nd one or the plague of the Middle Ages in the 14th century (25 million deaths)

to tighten up the arteries and diminish the systolic output of the left ventricle. In case of perforation a large dose of opium together with morphia injected subcutaneously would offer a patient the only chance.

I have now brought my address to a conclusion, and have to thank you for your patient attention. If I have in any measure succeeded in my attempt to render more clear the part played by the various morbid actions which contribute to the symptomatology of typhoid fever, and have thus made it more easy to deal with dangers as they arise or to anticipate and prevent some of them, I shall not unworthily have occupied your time and my own.

THE BACILLUS OF BUBONIC PLAGUE.[1]

BY PROFESSOR S. KITASATO.

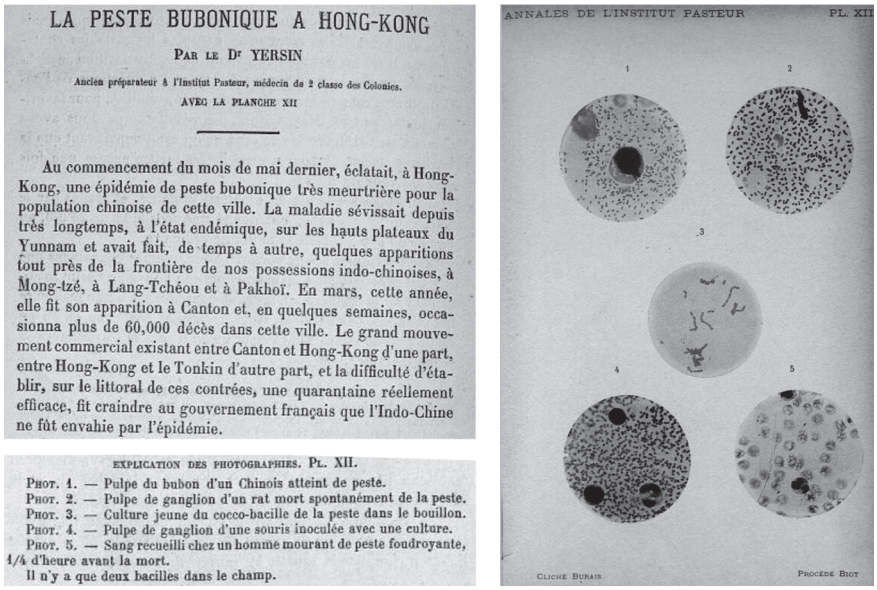
[PRELIMINARY NOTICE.]

EARLY this year an epidemic of bubonic plague broke out in the south of China and Canton, from which city the disease was imported into the neighbouring island of Hong-Kong, where it has prevailed from the beginning of May until now. The Imperial Japanese Government sent a commission to Hong-Kong in order to study the plague, especially as regarded its bacteriological character and its pathological and clinical features. The pathology and same bacilli were to be found at every post-mortem examination (of which we had upwards of fifteen) in great quantity in the bubonic swellings, in the spleen, the lungs, the liver, in the blood contained in the heart, in the brain, intestines—in fact, in all internal organs without exception—and every cultivation from any particle of these parts invariably produced the same bacilli. Suppose the contents of a bubo or a small piece of the spleen are rubbed on the cover-glass, and the latter, after having been stained, is examined under the microscope (one-twelfth inch oil immersion, Zeiss) bacilli will be discovered in the form found in pure cultivation (Reinkultur). In the spleen especially the bacilli are aggregated in heaps. Bacilli from bubonic swellings and from other internal organs are more easily stained with aniline dyes in their middle part than those taken from the blood, but any serum cultivation prepared from them produces the same form of bacillus. In any case where cultivations are prepared from parts of any internal organs or from the blood taken from the finger tips, with careful observation of all due precautions, pure cultivations (Reinkultur) of one and the same bacillus are always obtained; therefore the most intimate connexion must exist between the bacillus and the disease. Full particulars about the observations at the post-mortem examinations will be given later by my colleague Professor Aoyama; generally it may be said that the parts in the neighbourhood of the bubo were œdematous, of a colour between black and red, infiltrated with gelatinous exudation, and that the spleen was enlarged. Both phenomena are to be found in inoculated

Figure 12. Kitasato's publication in 1894 on his discovery of the cause of the bubomic plague in Lancet

They must have known each other in 1894. Yersin had been in Koch's lab in 1888 when Kitasato resided there and also by the 1888 publication of Yersin and Roux on the identification of diphtheria toxin, a research topic of Kitasato and Behring at that time. Kitasato arrives as first in Hong Kong on June 12th while Yersin arrives three days later. Kitasato and his team have nice equipment, incubators at 37° at their disposal and even more importantly, get all the help they want from the British authorities and especially from the British Dr. Lowson. They are allowed to install their lab in the nice Kennedy town hospital and receive many cadavers from pest-deceased persons for examination and bacterial isolation. Kitasato manages to isolate bacteria from blood and lungs and claims on June 14th to have found the cause of the disease, just one day before Yersin arrives. On Yersin's arrival, the British authorities obstruct his investigations by not giving him access to deceased persons neither giving him a location in the hospital for his lab. He had to build his own small mat-shed laboratory for his personal microscope next to the Alice memorial hospital (big mat-shed hospital) and was unable (refused) to obtain corpses from deceased people. Thanks to the help of an Italian father Vigano, he could obtain corpses by bribing British soldiers. Immediately, he was able to isolate bacteria from bubo's and lymph nodes, had by lack of an incubator, to culture them at room temperature, i.e. 30°C in these tropic conditions, and

successfully transmitted and produced clinical symptoms in rats. Also Yersin claims now to have found the cause of the disease but that is after the discovery of Kitasato. With the help of Lowson, Kitasato publishes his results in "The Lancet" (August 1894) (figure 12) while Yersin publishes his data in the "Annales de l'Institut Pasteur" (September 1894) (figure 13).



LA PESTE BUBONIQUE A HONG-KONG

PAR LE Dr YERSIN

Ancien préparateur à l'Institut Pasteur, médecin de 2 classe des Colonies.

AVEC LA PLANCHE XII

Au commencement du mois de mai dernier, éclatait, à Hong-Kong, une épidémie de peste bubonique très meurtrière pour la population chinoise de cette ville. La maladie sévissait depuis très longtemps, à l'état endémique, sur les hauts plateaux du Yunnam et avait fait, de temps à autre, quelques apparitions tout près de la frontière de nos possessions indo-chinoises, à Mong-tzé, à Lang-Tchéou et à Pakhoï. En mars, cette année, elle fit son apparition à Canton et, en quelques semaines, occasionna plus de 60,000 décès dans cette ville. Le grand mouvement commercial existant entre Canton et Hong-Kong d'une part, entre Hong-Kong et le Tonkin d'autre part, et la difficulté d'établir, sur le littoral de ces contrées, une quarantaine réellement efficace, fit craindre au gouvernement français que l'Indo-Chine ne fût envahie par l'épidémie.

EXPLICATION DES PHOTOGRAPHIES. PL. XII.

PHOT. 1. — Pulpe du bubon d'un Chinois atteint de peste.
PHOT. 2. — Pulpe de ganglion d'un rat mort spontanément de la peste.
PHOT. 3. — Culture jeune du cocco-bacille de la peste dans le bouillon.
PHOT. 4. — Pulpe de ganglion d'une souris inoculée avec une culture.
PHOT. 5. — Sang recueilli chez un homme mourant de peste foudroyante, 1/4 d'heure avant la mort.
Il n'y a que deux bacilles dans le champ.

Figure 13. Yersin's publication in 1894 on his discovery of the cause of the bubonic plague in les Annales de l'Institut Pasteur

In April 1895, Yersin leaves for Paris to make antiserum with Calmette and Amédée Borrel, a few months before Pasteur's death on September 28th. In June 1896 the pest epidemic had reached Canton and Amoy in China and Yersin goes there to treat successfully people with his antisera. But in March 1887 the epidemic has now reached Bombay and Yersin was unsuccessful with his antisera prepared in Nha Trang. In 1897 on request of Roux, Paul-Louis Simond travels to Bombay to replace Yersin and to threat with new antisera from Paris. Simond observes that in an outbreak rats are first dying and are full of plague bacilli, and that patients at the very early stage of the disease have a small blister filled with fluid from an insect bite. In 1898 he performs transmission experiments with fleas between rats and confirms the rat flea as the vector and elucidates the whole transmission life

cycle of *Yersinia pestis*. From 1998 to 1901 he is appointed director of the Pasteur Institute in Saigon. And what about Yersin? He returns to NhaTrang where he establishes a small bacteriology lab to produce antisera and to study cattle and buffalo diseases which will grow out to a new Pasteur Institute. Nha Trang takes care of animal diseases while Saigon takes care of human diseases. Besides being now a real veterinarian, Yersin becomes also a true agronomist with the development of Hevea (rubber) and Cinchona (quinine) plantations yielding revenues making his institute independent of external funding. Yersin dies in 1943 in NhaTrang as a true Vietnamese National hero where he is deeply honoured.

But who isolated and identified the real cause of the Bubonic Plague? The thorough analysis of the Yersin-Kitasato controversy on the diagnosis of the Plague has nicely been discussed by Bibel and Chen (1976)[18]. Already in 1895, Tanemichi Aoyama, the pathologist of Kitasato in Hong Kong had doubts on the bacilli of the blood of Kitasato, thinking they were systemic streptococci[19]. In 1900, Tatsusaburo Yabe, head of the Japanese Navy, examines the cultures in the lab of Kitasato and confirms that they are a species of pneumococci: "The honour of the discovery of the plague bacillus comes to Yersin only and we regret sincerely that our distinguished microbiologist made an unbelievable mistake on the plague microbe". It was finally the honourable Kitasato himself who announced in 1925 at the Congress of the Far East Association of Tropical Medicine: "The discovery of the plague bacillus is due to the great Yersin and not to me". The lucky Yersin had been fortunate as he had been using bubonic fluid where the pest bacteria are plenty and easy to isolate, and by lack of an incubator had to culture them at 30°C, which is the ideal temperature for culturing *Yersinia pestis*, the name given in his honour.

Epilogue

The end of the 19th century was a phenomenal period in search for the causes of infectious diseases, the birth of bacteriology and immunology. Thanks to the chemist Louis Pasteur and the medical doctor Robert Koch,

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tremendous progress was made in the discovery and identification of the pathogens responsible for diseases. Although other scientists had already suggested the infectious nature of diseases, Pasteur was fundamental in proving and demonstrating the latter. Moreover, Pasteur went further on, by demonstrating the potential of protecting against infectious diseases by culture-attenuating the causative viruses (like they were called at that time) and inoculating them to induce protection. However, one has also to recognize that Pasteur did not always give credit to who was first in discovering of bringing up the idea of a discovery. One has only to look at the un-collegiality of Pasteur in the anthrax story versus Toussaint who conversely helped Pasteur a lot in giving him access to his bacterial isolations, ideas and vaccination results. Or to the whole story on silk worm diseases and spontaneous generation with Béchamps.

Conversely, Koch, being rather an "einzelgänger", was a very meticulous doctor who succeeded through intense and laborious personal work to isolate (clone) and identify some of those infectious agents. Later on with the help of his collaborators, he performed technical skills in culturing these organisms in isolation of others by using solid media instead of liquid (like Pasteur used), enabling by cloning (single colony pick up) the undeniable identification of the causative organism, in perfecting staining techniques and in demonstrating the organisms by photographic pictures. But also Koch was a narcissist and shamefully took often refuge to personal attacks on Pasteur. Looking carefully at the scientific career of Koch, one has however to realise and admit that Koch had only been very productive for 10 years, as his tropical adventures in the second half of his career did not lead to any significant achievements or breakthroughs.

This period was not only dominated by these two masters and their collaborators, but was also enlighted by many other scientist like Lister, Bouley, Toussaint, Chauveau who contributed quite a lot to the waves that Pasteur and Koch caused in their rush for being the first or being recognized as the first in their discoveries. As scientists, we recognize and applaud the merits of these two persons, recognize that competition stimulates quite a lot of progress in science but deplore the jealousy, intricacies and un-collegiality that might accompany such scientific races. We realise also that today nothing has changed a lot in this competitive world of laboratories looking for world recognition.

In conclusion we can only advise scientists to scrutinouly review the literature and to go back in history to the original publications in order to form an opinion about who was first or second in a discovery. As it often happens the idea (but not always the real scientific prove) of an innovation had already been expressed before. To illustrate the latter, look at the worldly discovery in 1928 of the therapeutic use of penicillin against bacterial infections by Alexander Fleming who was proceeded by 30 years in this therapeutic idea by a French army doctor Ernest Duchesne[20].

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Laudatio Marc Brysbaert

Wouter Duyck

As the proximus for this Sarton medal, awarded to colleague Marc Brysbaert, it is an honour to welcome you all to this ceremony and Sarton lecture. I proposed Marc Brysbaert to the committe for his excellent book entitled 'Historical and Conceptial Issues in Psychology'. A book that he authored together with Kathy Rastle, Royal Holloway University, Londen. If I am correct, the book first appeared almost ten years ago now, in 2009. A second edition followed three years later. And that time, and still, the book constituted a unique overview of the history of psychology. Unique, because of what is revealed by the title. The book does not only cover history, but also conceptual issues. This implies that the book is not merely a description of events and knowledge at specific moments in time. It is much more than that: it attempts to grasp why, and how we look at psychological concepts and questions the way we do. It describes the evolution of research methods and paradigms, and how these evolved into the science of psychology as we know it today. Therefore, it offers an indispensible helicopter perspective to each scientist and practioner of psychology. What is the human mind? What is conscientiousness? What is personality, what is intelligence, and why did we start to consider cognitive representations? Now, It may be surprising that such a book had not been written before in Europe. But here, it is important to consider the time perspective of psychology as a science. True, some people consider the psychologist to be the oldest profession in the world. But for many other people, that is another profession. And I can see that many of you would not situate psychology there. And, indeed in the science of psychology there is a consensus that the first scientist who called himself a psychologist only

appeared in 1879. This makes us a very young science, much younger than physics, or medicine. In that year, Wilhelm Windt founfed the first research laboratory in psychology in Leipzig. Therefore, he is also considered to be the first experimental psychologist, it is therefore not a coincidence that this is exactly also the domain that Marc Brysbaert works in and the department that Marc is chairing. Actually, Wundt had a medical training, but he applied his rigorous, well controlled, scientific method to psychological questions. He was very active, with 50.000 pages of output, and his infuence was tremendous, mainly because of the many American and British scholars that studied in Wundts lab, and then exported the knowledge to the universities in the United States and the United Kingdom. Also Belgians like Albert Michotte studied in Wundts lab. The idea of a research lab for psychology was soon copied, and Alfred Binet for instance started a similar lab in Paris, at the Sorbonne, where he developped the first standard intelligence tests. Of course, these are all historical facts and landmarks that can be found in many textbooks of psychology. This is not the reason why you should read the book. You should read it for the conceptual issues: what did Galilei change for science? Why did the behaviourists believe that all behaviour was determined by nurture? Why did Freud focus on the unconsciousness as a reaction to these reductionists? Why did cognitive psychology emerge? Or more recently, what can biology and neurology teach us about behaviour. And has the availability of modern imaging techniques like fMRI advanced our understanding of behaviour? Does free will exist? Is psychology a real science? Or, more tangeible and more apploed: how did psychology affect everyday life? Why do job applications imply assessments and what can they tell us? How has our understanding of mental disorders evolved? All these questions, and many more are tackled in the back, with respect for different research traditions, a variety of methods, both quantitative and qualitative. Approaching behavior from multiple perspectives, with biological, psychological and social influences.

As the chairman of the psychology program committee, I was also delighted to be able to convince Marc Brysbaert to teach the book in the new program of psychology that started this academic year. So, from now on, all Ghent university students of psychology will be acquinted with the issues, knowledge and insights, included in this book. I consider this new course, entitled *Grondslagen van de Psychology*, to be a very valuable new

initiative in our program. Now, to close this introduction, I must honestly admit that Marc was not very keen on accepting new teaching duties, but I'm afraid he couldn't refuse. In the preface of his book, he wrote: "*why do all good degrees of psychology today include a course on historical and conceptual issues?*". It's possible that this was just a phrase suggested by the sales manager at publisher Pearson, but for Ghent University it meant that Marc had to accept the offer. Who would be better than the author himself to teach the book? Our luck. But now, it's time to give the floor to Marc Brysbaert himzelf, who will enlighten us about some of the issues discussed in the book. The rest will be for you to discover. A must-read for every psychologist.

The emergence of psychology as a science

How American history writing created an illusion of psychology schools at war with each other[1]

Marc Brysbaert

The standard textbook introduction to the history of psychology

The standard textbook introduction to the history of psychology reads as follows:

> Psychology started in 1879 when Wilhelm Wundt established the first laboratory of experimental psychology at the University of Leipzig. Wundt was a proponent of structuralism, a school that tried to understand the conscious mind by means of introspection. Wundt had many American PhD students, who returned to the US but were more interested in practical applications than in the structure of the mind. Together with William James, they created the functionalist school. In 1913, John B. Watson published a manifesto, which introduced behaviorism and was the end of functionalism in America. Meanwhile in Europe, Gestalt psychologists started to question the structuralist assumption that one can understand the human mind by breaking it down to basic elements. According to them, the whole was something else than the sum of the parts. They started a new school: Gestalt Psychology. Alongside these four schools in experimental psychology,

[1] This text is partly based on excerpts from Brysbaert, M. & Rastle, K. (2013). Historical and Conceptual Issues in Psychology (2nd edition). Harlow: Pearson Education.

there was fifth school, psychoanalysis, which was predominant in clinical psychology.

Problems with the standard account

The standard account is the received wisdom I learned when I was a student and which I propagated in two new psychology textbooks (Roediger, Capaldi, Paris, Polivy, Herman, & Brysbaert, 1998; Brysbaert, 2006). However, when I was delving deeper into the matter for a historical textbook (Brysbaert & Rastle, 2009), I discovered a lot was wrong with the account. To give but a few examples:

- Psychology was already a well-established subject at universities before 1879.
- Wilhelm Wundt had many more interests than introspection and would probably have abhorred the description of him as a structuralist.
- Almost all research in applied psychology is overlooked (e.g., the publication of the first intelligence test by Binet and Simon in 1907).
- Almost all experimental research done in Europe in the 19th century and early 20th century is ignored (see, e.g., Levelt, 2014, for a description of language-related research).
- A case probably can be made to claim that the influence of behaviorism was smaller than claimed by the standard account. Although behaviorism was strong in American experimental psychology (particularly in the psychology of learning, where it remains a big influence), it was much less influential in other parts of the world and in other research areas (e.g., developmental and social psychology).

Even worse, when one looks at the titles of talks given at scientific conferences, very few of these titles referred to topics at the core of the various schools (see, for instance, the talks presented at the British Psychological Society, listed in Brysbaert & Rastle, 2013). Even the primary textbook on schools in psychology (Woodworth, eight editions between 1931 and 1948) ended with a surprising chapter, in which the importance of schools was toned down, as can be seen in the following excerpts:

> "In view of all the divergent movements that we have surveyed, all these 'warring schools' of contemporary psychology, the reader may easily carry away the impression that we psychologists are anything but a harmonious body of scientific workers. Looked at from outside, our fraternity has seemed to be a house divided against itself … You would get a very different impression from attending one of the International Congresses of Psychology or a meeting of one of the national societies such as the American Psychological Association. You would hear papers read on various psychological topics, with very little mention of any of the schools and with discussions of the usual scientific type, free from acrimony though not of course from the give and take of doubt and criticism …
>
> Another reason for the continued unity of psychology is found in the fact that only a minority of psychologists have become active adherents of any of the schools. Some may lean toward one school and some toward another, but on the whole the psychologists of the present time are proceeding on their way in the middle of the road."

So, what was going on?

Origins of the distortions in the standard account

Gradually it became clear that the standard history of psychology in textbooks as "warring schools" was a primary example of how history writing can lead to simplification that no longer represents reality. This is what I think happened.[2]

For a start, philosophers were used to talking about schools, referring to a teacher or a small group of teachers and their students. This practice was continued by the first psychology writers. For instance, James (1890) mentioned the following schools in "Principles of Psychology" (among many others): The associationist schools of Herbart in Germany and of Hume the Mills and Bain in Britain, Charcot's school, the Hegelizer school, the analytic school, … Also, Woodworth discerned more than the 'traditional' five schools. He included associationism, Soviet psychology, analytical schools, hormic psychology, organismic psychology, and personalistic psychology.

[2] Like all history writing, my account is personal and simplified, although I hope to have done better than the situation I'm addressing. If not, I'll be happy to be corrected.

Gradually, the idea of a school as a small group of people (and sometimes a single person) got lost and schools were seen as wider movements. At the same time, the number of schools was pruned to keep things understandable. An important role in this evolution was played by Edwin Boring, who wrote the influential handbook "A History of Experimental Psychology" (1929, revised edition 1950). Boring was a student of Titchener[3], an Englishman working in the US who had studied with Wundt and considered himself as a structuralist and the true heir of Wundt (hence the association between Wundt and structuralism). In addition, Boring liked to present history with the dialectical method, consisting of thesis, antithesis, and synthesis. This allowed the historian to position himself as synthesis maker. So, we had thesis-antithesis examples between structuralism and functionalism (structure vs. functions of the mind), between structuralism and Gestalt psychology (elements vs. whole), between functionalism and behaviorism (introspection vs. scientific research), between behaviorism and psychoanalysis (scientific research vs. clinical case studies), and so on. A last influence of Boring arguably was that he limited his handbook to experimental psychology, thereby suggesting that applied psychology was of secondary importance.

Costall (2006) argued that another dialectical triad explains the continuing attraction of the simplified historical view for scientific psychologists. It goes as follows. At first, psychologists examined the right subject (the human mind) but with the wrong method (introspection). In reaction to this, Watson proposed to use the right method (scientific experiment) but applied it to the wrong subject (human behavior). Finally, synthesis was achieved when cognitive psychologists proposed to study the right subject (mind) with the right method (experiment).

Another person who undoubtedly contributed to the simplification of early psychology was Watson, who in his 1913 manifesto reduced all existing psychological research to structuralism (Europe) and functionalism (America). This considerably simplified matters and increased the impact of Watson's new behavioristic approach.

[3] The history book was dedicated to Titchener.

What is the alternative?

If the standard account of the history of psychology is a biased (even wrong) representation of what happened, what is the alternative? This is the question we were confronted with when we wrote our history book. As it happened, the alternative did not turn out to be so difficult. All we had to do, was to describe the developments in a few countries (e.g., Germany, France, US) to give a rather vivid account of how psychology sought itself a place at universities. For instance, in France the emergence of psychology as an independent discipline was hindered because Auguste Comte (the founder of sociology) had declared psychology unscientific, a branch of philosophy (metaphysics). Ribot tried to change this view by writing books about how successful scientific psychology was in Germany and the UK[4], and by helping Beaunis to establish the first laboratory for experimental psychology in France. Another input came from Charcot's work on hysteria. One of Charcot's assistants was Binet. Charcot and Binet were heavily criticized by the Belgian academic Delboeuf, who questioned their research methods. In particular, Delboeuf feared that many of the findings reported by Charcot were due to the fact that the hysteric patients could hear what the experimenter expected from them. As such, this is one of the first examples of the negative influences demand characteristics can have in psychological research (Wolf, 1964). Delboeuf's criticisms arguably contributed to the fact that Binet left Charcot's lab and sought to improve the scientific quality of his research (which eventually led to the IQ test).

The alternative account of the emergence of psychology is less centered on American psychology[5], but to our delight turned out to work even better than the standard account. It gave a much more vivid account of how psychology emerged as an independent discipline (Brysbaert & Rastle, 2013).

[4] Which for the UK actually was not the case, but that is a topic for another manuscript.

[5] It may be good to remember that the standard account of the history of psychology came to us via the American textbooks that were consulted by lecturers all over the world.

Are there no schools at all?

Attentive readers may have noticed that the discussion so far was about structuralism, functionalism, behaviorism, and Gestalt psychology. What about the fifth school: psychoanalysis?

C.P. Snow made a perceptive distinction in a lecture given in 1959. He noticed that before the arrival of the natural sciences, education was centered on subjects from the Greek and Latin civilizations. They consisted of languages, philosophy, mathematics, history, culture, arts, and sports. As the natural sciences grew in power, the classic subjects became beset by the science curriculum. However, they did not yield (completely). What happened was a fractioning of education, with some streams remaining within the traditional curriculum (Snow called them the humanities) and others focused on the new sciences. As a result, Snow argued, society became divided in two cultures, which vie for power but for the rest largely ignore each other.

By its nature, psychology fell right on the border between the two cultures. Its subject was part of the humanities, but its methods (and indeed its claim for existence as an independent branch of learning) came from the natural sciences. Although psychology's position could have turned it into a bridge between the humanities and the sciences, reality forces us to admit that this is not quite what happened (e.g., Kagan, 2009). Instead, the schism between the two cultures has resulted in a schism within psychology, with one part defending the scientific approach and the other defending the humanities approach. According to the former, psychology must investigate human functioning quantitatively making use of experimental manipulations and statistical techniques to understand the relations between phenomena. According to the latter approach, psychologists must try to understand unique persons in their context like historians try to understand past events (without the possibility to intervene and see what consequences this has). Although psychoanalysis at various times claimed to be a science, it is fair to say that it much more adhered to the humanities. In this, it was joined by other humanistic, hermeneutic, critical, and radical groups.

If any "warring" schools are to be distinguished in psychology, it is between the science-oriented school and the humanities-oriented school, a distinction that relates to a much wider divide in society (about the value of

science for human well-being). Readers interested in this topic are advised to search for the terms "mainstream psychology" and "critical psychology".

In search of a bridge

If psychology failed as a bridge between sciences and humanities, is there a substitute? Already decades before Snow, George Sarton proposed history (and in particular history of science) as the best candidate. In his own words (Sarton, 1937: 56-57):

> "To complete the integration [between humanities and science], each group must learn to understand the other. The educated people in general must obtain some knowledge and appreciation of science; the scientists must receive some historical training, must be taught to look backward as well as forward, and to look with reverence. These good offices may be rendered to both groups by the teaching of the history of science and of the history of civilization focused upon it… Between the old humanist and the scientist, there is but one bridge, the history of science…"

Similarly, history of science and history of psychology may be the bridge between the science-oriented and the humanities-oriented schools in psychology.

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Laudatio Pedro Lains

Frank Caestecker

The faculty of Economics and Business Administration has invited Prof.Lains for this Sartonlecture. Prof. Lains from the University of Lisbon is the historian of the Portuguese modern economy. His meticulous research has given us insights on how the Portuguese economy has evolved during the 19th and 20th century. He is the author of the chapters on the modern period in the well acclaimed book published in 2016 by Cambridge University Press, *An Economic History of Portugal, 1143-2010*.

An economist of training he started his research career with a Ph.d. on the Portuguese Economic Growth in the second half of the 19th century and its Foreign Trade. The Ph.d. was an example of basic historical research. He processed the available data in an extensive manner, a not very glamorous job. However when that by times painful job was done, he got the basic data right and his work became a reference in the field. During his career he pursued this path and got right the basic macroeconomic data for Portugal in the 19th century (GDP, national accounts..). The hallmark of his economic research is of course not only robust data, but also an embeddedness of these empirically sound developments in a theoretical framework of economic growth.

Lains addressed all elements which affect economic development. the structural change from a agrarian to an industrial economy, the input of production factors labour and capital and their productivity as well as total factor productivity. With the Portuguese case he showed that economic growth was not only possible with industrialisation, but that growth had multiple sources. His research on the Portuguese agricultural economy showed that this sector which dominated all over the world the pre-indus-

trial economy changed in the 19th century, also in Portugal and became more productive. The result of this research has become assessible with the volume Prof.Lains edited *An Agrarian History of Portugal, 1000-2000. Economic Development on the European Frontier* published by Leiden: Brill Freire in 2016.

The financial economy and banks in particular are another crucial variable for an economy to thrive. In 2002 Lains published his History of Banking in Portugal in which he outlined *the history of the Caixa Geral de Depósitos* in three volumes. This public bank was founded in 1876 and is now the second largest bank in Portugal. It has a history very similar to our Belgian *Algemene Spaar en Lijfrentekas* (ASLK) established eleven years earlier than this Portuguese bank, in 1865. The ASLK has been privatized since then and is now part of Fortis. The Caixa Geral de Depósitos has remained a public bank.This book is due to be published in English next year by Routledge in its Financial History series.

A historical approach to a national economy should also address the institutional framework that influences economic development at large. Also in this field of institutional economics he became an expert. Pedro Lains edited in 2010 the volume published by Cambridge University Press: *Paying for the Liberal State. The Rise of Public Finance in Nineteenth Century Europe*.

This book already gives us a hint that Pedro Lains knows more than "only" Portuguese economic history. Still I can image that the audience has been thinking why Portugal. For a national audience in Lisbon this is all interesting, but for Belgians? We have all become Europeans now and Portugal is part of the European economy, but still.

In 1976 a book was published by Cambridge with the somewhat disturbing title "Why Switzerland? " written by Jonathan Steinberg. You would probably understand why we would have invited a Swiss expert. Switzerland is a special place: it has unique institutions, its direct democracy, communal autonomy, and four national languages, all very interesting. Although officially not part of the European economy, Switzerland is above all very wealthy and we consider economic history all interesting when it brings success. *Why Switzerland?* of Jonathan Steinberg is in its three edition already.

However contemporary Portugal has nothing of that all. Still it has been successful too, but its success was not a lasting success. In early modern Europe Portugal was a rich country. We all know Vasco De Gama, the Portuguese explorer, who was the first European to reach India by sea, just after Columbus in Spanish service reached America. Da Gama's discovery of the sea route from Europe to India opened the way for an age of global imperialism. In the 16th century the Portuguese established a colonial empire in Asia, Latin America and Africa and they became the leading nation in intercontinental trade.

Pedro Lains' Ph.D. on Portuguese Economic Growth and Foreign Trade was reminiscent of the 16th century, the great century of Portugal. However Portugal was in the second half of the 19th century less successful. Pedro Lains did not study a pioneer, not a winner. He studied a backward country, a loser at the time when the world knew its great divergence. While (some countries in) Europe knew sustained and substantial economic growth the Portuguese economy was floating apart. It drifted away and remained a poor European country.

The merit of Pedro Lains is that he made Portugal into a paradigmatic case. Also Ricardo in his book *On the Principles of Political Economy and Taxation* published in 1817 used Portugal as an exemplary case. He used Portugal to explain his at that time revolutionary idea of comparative advantage. The idea of comparative advantage is still an very important concept for understanding the benefits of trade. Ricardo used trade between England and Portugal in cloth and wine to explain how it benefits Portugal to import cloth even if Portugal can produce cloth with less labour than England. Interesting that Ricardo considered that the cloth production of Portugal had a higher productivity, with less labour than England, just at a time when England his industrialized production of cotton cloth was mature enough to outcompete all cotton cloth producers over the world. Even in India which was the leading cotton cloth producer on a global scale in the 18th century spinners and weavers lost their job by the early 19th century due to British imports. Had Portugal become the laughing stock of the modern European nations achieving lasting economic growth? Did Ricardo use Portugal as the least likely European country to have a higher productivity than England? Portugal was indeed a poor country but the gap between poor and rich country only started to widen in the begin-

ning of the 19th century. A gap which would increase during the 19th and 20th century.

Portugal was indeed a backward economy, but it was not the only European country to fall behind. In the last decade Pedro Lains made Portugal in a paradigmatic case to understand the position of the periphery in European economic development. Why do some nations, economic regions within Europe prosper while others lag behind? To understand different paths of economic development within Europe he has published numerous articles and edited several volumes on how other Southern European, but also Eastern European countries have prospered in the 19th and 20th century. In the main journals dedicated to European economic history, Journal of European Economic History, European Review of Economic History, Scandinavian Economic History Review you name it Prof Lains has developed his arguments in a comparative manner. His next publication dedicated to the Iberian Peninsula address the same problem of modern economic backwardness. In this edited volume in English the Spanish and Portuguese economic history will be outlined and compared with the economic development in the rest of Europe.

In his publications no strong claims, but very prudent analysis of reliable data. The hallmark of his very productive career is economic analysis on the basis of empirically solid data and this has made him into the expert in this field. In his lecture of today European Economic History: the Contribution of the Periphery he will share with us the insights he acquired during many years in this research field. His historical reflections from the European periphery will indicate how the countries in the European periphery had a difficult time to catch up and whether there were any keys to success or failure.

European Economic History: the Contribution of the Periphery

Pedro Lains

Introduction

The economic history of Modern Europe is a moving target. As we see the consolidation of conclusions regarding older debates, such as the speed of change during the first industrial revolution or the contribution of technological innovation or foreign trade to 19th century economic growth, new questions keep emerging, such as those concerning the timing of the divergence of income per capita levels within the continent, or the impact of political borders in product or labour market integration.[1] There was a time too when typologies abounded, but we now lack grand interpretations on causes of economic growth and retardation of the European nations, and we cannot even be sure that one is needed.[2] Yet as both long-term and world economic history gain vigour, there seems to be an increasing concern with a general and coherent picture of the European economy during the period since industrialization begun. In fact, in global economic histories, Europe often shows up as a united entity, albeit of a complex type, and European industrialization often appears as a comprehensive concept in long-run development narratives.[3]

1 For the speed of British industrialization, see for example, Harley (2014); for the "little divergence", see Broadberry et al. (2015) and Pfister (2017); and for border effects, see Schulze and Wolf (2009).

2 For the earlier typologies, see Rostow (1960), Gerschenkron (1962), Landes (1969) and Abramovitz (1986). See also O'Brien (1986).

3 For the long run, see Crouzet (2000), Malanima (2009), Van Zanden (2009) and Persson and Sharp (2015). For perspectives on the place of Europe in global economic history, see Jones (1981), Landes (1998), Pomeranz (2000), Neal and Cameron (2016) and Baten (Ed.) (2016). See also Broadberry and O'Rourke (Eds.) (2010) and O'Rourke and Williamson (Eds.) (2017).

However, we may ask whether there is a unified Modern European economy and a unified economic history of Modern Europe. Does the European continent have its own history that goes beyond national and regional histories? If so, are there any lessons that we can draw from Europe that may be help the study of economic growth at the world level? Due to the complexity of the endeavour, it certainly goes beyond the scope of this lecture to provide a definite answer to such question. Instead, I want to discuss here how our knowledge about European economic change can be improved by taking into account the discussion about factors of growth and retardation on its periphery. That exercise will help us understand better the Modern European economy, and its place in the global and the long-term economic history narratives.[4]

The lecture proceeds with a review of the main phases of the historical work on European economic history. Secondly, we will look at new findings on the diversity of experiences of growth and retardation in Europe, using the core-periphery framework of analysis. The final section concludes.

The European diversity of growth

For some time, the economic history literature was concentrated on the study of the causes of the British industrial revolution and of its spread throughout Europe. There is still a lot to be known on that major theme, but our understanding has made considerable progress in the last half century. We have come a long way since the typology of Rostow (1960), according to which the British industrial revolution was the one and only solution in the transition from traditional economies, where growth was constrained by the availability of natural resources, animal force, and human labour, to modern economies, where growth is self-sustained and virtually unlimited. According to this view, the first industrial revolution was replicated first in countries closest to Britain, and the degree of success would have depended on the existence of similar sets of conditions, namely, an appropriate institutional setting where property rights and the rule of law were paramount. Thus, countries that had British like "pre-conditions" succeeded, and those that did not have that set of attributes would fail.

[4] On this see also Lains (2012).

Such narrow perspective was first challenged by Gerschenkron (1962) who suggested that there were different paths to industrialization and a wider range of institutional settings that favoured industrialization and growth. This author cites as examples of such differences German investment banks, from the 1871 unification on, and the Russian state, from last decade of the 19th century on, institutions that would have replaced what market forces had done in Britain. His vision of how industrialization progressed in Europe still holds as globally correct, although the specifics of the implicit model have been extensively challenged.[5] Following the path set by these authors, David Landes (1969, 1998) explored the causes of the British industrial revolution and its spread across Europe by looking at technological innovation, and in the process provided a vivid picture of the factors behind British industrialization and what he terms the "continental emulation". His analysis reaches out to the many varieties of responses to the new industrial world, thus providing an overall view of the European economy as a whole, and not only of countries as separated entities. Truly, the action he describes relates mainly to the largest European economies, but that is an outcome of the state of research at the time. Yet Landes provided too simple explanations for retardation, attributed to "religious and intellectual intolerance (…) and political instability".[6] Sidney Pollard (1981) somehow complements the works by Gerschenkron (1962) and Landes (1969), but he concentrates his analysis on the main industrial regions instead of countries. Pollard (1981) is also looking for big spurts in industrialization, but adds an enormous amount of detail in the description of the industrial activity across many different regions. According to him, industrialization proceeded according to certain local characteristic, which did not coincide with national borders. His view of industrialization as a process of contamination between adjacent regions which had the right pattern of supply of natural resources, above all coal and iron, and past experience in proto-industrialization, was a major contribution which is still highly valuable.[7]

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5 The best revision of this typology is still Sylla and Toniolo (Eds.) (1991). See also Acemoglu and Robinson (2006).

6 Landes (1998: 248, 253)

7 See Cameron (1985), O'Brien (1986), Lains (2012), Klein et al. (2017) and Rosés and Wolf (2018).

The perspective of multiple possible paths to growth was confirmed albeit in another framing by O'Brien and Keyder (1978)'s work on Britain and France. These authors conclude that growth in the countries they study was to a large extent related to the overall economic environment. Thus the British industrial revolution was the response of economic agents to the characteristics of the British agricultural and industrial sectors, and that a similar process could hardly have happened in a country such as France, where the economy was considerable different in terms of resource endowments. The French economy did not industrialize as fast and had lower rates of growth not because of its social or political structure but simply because its way to prosperity was diverse.[8] Moreover, the speed of British industrialization was relatively slower than previously believed, as was the rate of overall economic growth. Crafts (1985) provides further evidence to conclude that British Industrial Revolution was a slow and smooth process, as major innovations were confined to sectors with small shares of total industrial and national output. Moreover, according to the same author, Britain was ahead of all other European economies in terms of structural change before industrialization started, as the share of the agricultural sector in total labour force and national output was comparatively small. This fact would become of great relevance in the visions of comparative performance in the following years.[9]

We thus ended up with a set of valuable interpretations on European industrialization where industrial big spurts or "take-offs" are absent, the paths to growth were varied, and natural endowments and geographical distance from the first industrializers are added to the overall institutional environment as factors of growth and of its spread. We also learnt that typologies for European industrialization needed to be complemented by national and regional historical probes.[10]

The increase in our knowledge about the European economy implies that most typologies have lost their explanatory power. Consequently, we give more attention to the diversity of experiences across countries and regions, or the different paths to growth and convergence. A lot has been learnt by looking at the experience of the more developed areas of the Continent in

[8] See also Crouzet (1990).
[9] See also Harley (2014) and Broadberry et al. (2015).
[10] See also the earlier works by Milward and Saul (1973 and 1977).

the north-east. The causes of industrialization and 19th century growth are better understood by considering not only the first industrial revolution but its continental replicas in France, Germany or Austria. These changes in perspective about growth and convergence have been followed by additional research on backwardness and divergence in the south. Thus, it is important to build a general framework of interpretation of the evolution of the European economy based on the comparative performance of core and periphery. Europe needs to be studied in its diversity and that is why we should care about the lessons that we can learn from the periphery.[11]

But there has not been a grand model in this new stage of European economic history. This was a period of scattered research agendas with a widespread array of interests and purposes, and the main conclusion regarding the economic history of the whole continent is that it was as diverse and varied as the continent itself. The focus of most of the research remained the more developed economies of north-western Europe, but the research on the peripheries, particularly in the south-west advanced in a significant way.

O'Brien (1986: 333) summarizes as follows the main phases of European economic history literature that we have identified:

> Like the continent's wine European economic history displays endless and interesting possibilities. As practised in the late 20th century the craft might be distinguished into three basic varieties: the heavy clarets of scholarly surveys, the bubbly champagne of studies purporting to test grand theories of economic development and the dry whites of an inductive statistical approach. European economic history seems to have reached a stage where it is necessary to allocate more resources to the production of good dry whites. That may be the only way to create conditions for a new typology of Europe's industrialization from 1815 to 1914.

One major development from this period was the greater attention paid to quantitative data from an international perspective, and the building of large data sets on national income at the aggregate and sectorial levels, as well as on labour and population.[12] We thus entered a stage with new infor-

[11] For recent works on the southern periphery, see Toniolo (Ed.) (2013), Costa et al. (2016) and Prados (2017).

[12] On the data basis, Maddison (2001) and Bolt and van Zanden (2014).

mation and new interpretations of all fields of economics of the European past, but with little if any advances on the whole of the Continent and no general theoretical framework on how the continent developed and why it developed as it did, since the British industrial revolution.

The dispersion of the investigation and the need to look for an European economic history did not pass unnoticed as one would expect from the dynamic environment that the field has been living during what was probably one of its most lively stages. The most relevant piece of work stemming from that concern is Broadberry and O'Rourke (Eds.) (2010: vol. 1, p. xiii) who take "an explicit pan-European approach, with the material organized by topic rather than by country." These two volumes deal with a vast array of topics concerning European economic history which show how the research has been carried on in the recent decades on the topic. This is certainly one road to follow, and the book went far in compiling the available research but it still does not provide a clear picture of the European economy and we need to go further.[13]

Lessons from the periphery

The relevance of the British industrial revolution derives from the fact that it brought changes that had long lasting effects on the British, the European and the world economies. In the century that unfolded, Britain came to dominate the world trade in manufactures and its economic power was only challenged by countries or nations that went through a similar process of industrial innovation, such as Germany and the United States. To understand the Industrial Revolution we need to study the transformations it brought along in the manufacturing, the agricultural and service sectors and in the society at large. We have a rather clear picture of what happened, in both social and economic terms. We know what were the main transformations in economic activity, what changed in terms of the economic structure of the three main sectors, how labour conditions changed, how transports were revolutionized, how trade expanded, how cities changed, amongst other aspects. The picture is also clear in regard to the evolution of the main macroeconomic variables, namely, the growth and composition of GDP

[13] Examples of pan-European studies are Lains and Pinilla (Eds.) (2009) and Cardoso and Lains (Eds.) (2010).

and industrial output, growth of factor productivity, changes in investment and the growth of the capital stock, changes in the quantity and quality of labour, the role of technological change. The increase in manufacturing output using new technology and concentrated in large scale operations can be seen as the economic response to particular characteristics of the British society, economy and environment, where the Enlightenment produced the needed spiritual and technical innovations, and commerce the appropriate cost structure that favoured investment in capital goods such as the spinning jenny, the steam machine and railways.[14]

Such large economic transformations were part of the wider European history, as is shown by the fact that they were promptly adopted in the regions that had conditions closest to those of Britain, with closer commercial links. Quickly, the Industrial Revolution became a European phenomenon, either by emulation or by replication. The ensuing economic transformation was however slow, not only in Britain, lasting down to the 1870s, but also on the rest of the continent, where industrialization continued well into the 20th century. Interestingly, the speed of industrialization is closely correlated to the geographical distance from Britain, and the last industrial nations could be found in countries such as Portugal in the west or Romania in the east of the Continent.

The core-periphery framework of analysis of the modern European economy crosses the different stages of development of research.[15] Core and periphery can be defined in terms of levels of GDP per capita, which proxy levels of productivity and efficiency, or in terms of levels of industrialization. Ideally, we should take regional levels and there is already some data for regions, although there is more data for countries or nations. The definition of core and periphery begs for the delineation of a line of GDP per capita levels, which can be chosen by historical insight, taking as core countries those that converged to similar levels of GDP, and periphery, the countries or regions that diverged. Similarly, core countries were firmly industrialized by 1870, contrary to peripheral countries. These divisions are not static and countries can move from one group to the other.

[14] See Allen (2009), Mokyr (2010) and Crafts (2010).

[15] On core and periphery, see for example Bairoch (1976), Berend and Ranki (1982), Aldcroft (2006) and O'Rourke and Williamson (Eds.) (2017).

For 19th century Europe, the definition of core and periphery is now well established, thanks to a large amount of research on levels and rates of growth of national output. Considering countries, the core includes Great Britain, France, Germany, Belgium, the Netherlands, as well as the Scandinavian countries, Switzerland and Austria. Finland, Italy and Spain, Hungary and Romania pertain to the periphery but converged successfully during the 19th century, whereas Portugal, Greece, Serbia and Bulgaria also belonged to the periphery but failed to converge in the same period of time. Of the countries formed in the aftermath of World War I, namely, the Baltic countries, Poland, Hungary, Yugoslavia, Czechoslovakia and Ireland, only the latter two joined the core group.

The lessons from the periphery can be summarized as follows.[16] Backwardness did not imply absence of growth and structural change. In fact, all countries and regions in the European periphery experienced unprecedented growth rates, and even higher rates of industrial growth. Moreover, the convergence in industrial output per capita between core and periphery was faster in the industrial sector, as compared to the rest of the economy. Industrialization in the periphery was also accompanied by productivity growth, both in terms of labour and capital. In some cases, we also may see the development of manufacturing exports, although in sectors with lower technological content and lower value added, mostly food processing, such as canned fish from Portugal, wine and olive oil from Spain, or flour from Hungary. Therefore, the economic transformation and its sources were also present in the periphery, albeit with lower levels of intensity, as compared to the core economies.

Structural change in the periphery was relevant not only in what concerns the transition from agriculture to industry. In the agricultural sector, productivity gains could accrue from changes in the structure of output, as goods with higher income elasticities increased their shares in total output. Producing more wine, fruits and vegetables, meat or olive oil could lead to gains in the productivity of the entire agricultural sector. Similarly, slight increases in the value chain of manufacturing, from wool to cotton textiles, or from lower to higher end metal products, also enhanced factor productivity in manufacturing. These changes possibly replicate the changes of

[16] See Foreman-Peck and Lains (2000) and Lains (2012). See also Tortella (1995).

the industrious revolution in the core countries, with a delay of about one century.

The peripheral countries shared some characteristics which may explain why, notwithstanding increasing rates of productivity growth and industrialization in the 19th century, they remained backward. Firstly, the pressure of agricultural labour on the available land was higher, as the agrarian labour force remained large. Secondly, with a few exceptions, in the periphery the share of animal output in total agricultural output was also lower. Thirdly, peripheral countries had comparative advantages mainly in the agrarian sector, particularly in products with lower value added and traded above all with core European countries, from which they import manufactures and industrial raw materials. Fourthly, peripheral countries had lower shares of foreign trade in total national product. Fifthly, the industrial sector in the periphery was less export oriented than in the core countries, thus concentrated on sectors with larger domestic markets, such as food processing, textiles and other lower quality consumer goods. Sixthly, peripheral countries had lower levels of education and human capital per worker. Seventhly, the per worker levels of infrastructure, including housing, transport, energy, schooling and sanitation, was also lower in the periphery. Finally, institutional development in the periphery was slower, however we measure it. Yet in this regard we need to take into account that the peripheral countries followed closely the institutional developments of the core, adopting later in the 19th century the standard institutional framework of constitutional and parliamentary monarchies.

It is important to recall here that these correlations do not imply a causality relationship. Lower levels of land and animals per agricultural worker, comparative advantage in the primary sector, lower export intensity in manufacturing and lower levels in human and physical capital can both be a cause and a consequence of economic backwardness. In fact, the crux of the investigation on the causes of backwardness is precisely to sort out properly the direction of causality and a number of models, both formal and informal, address that central question.

The periphery had also a number of relevant distinctive characteristics. Firstly, country size varied substantially, in terms of area and population, which implied that the size of the domestic markets could differ. Secondly, economic policy differed substantially, as we may find highly protectionist

countries, as well mildly protectionist or even countries with very low tariff barriers. Thirdly, financial probity at the state level also differed substantially in the periphery, running from countries that kept balanced budgets and low public domestic and external debt levels, and the opposite, as well as countries that had for large periods stable exchange rates under a metallic standard, and countries which exchanges rates fluctuated significantly. Fourthly, peripheral countries also differed in terms of natural resource endowments, such as rainfall, and coal and iron deposits.

Thus study of the European periphery should no longer address questions on why the periphery did not have its own "industrial revolution" to address questions on how much growth was there and how the pace and structure of economic change of backward Europe was shaped. Instead of trying to explain why the periphery did not emulate the core, we should attempt to understand the constraints to economic growth stemming either from the institutional framework or from the limits imposed by lower levels of savings, investment and capital infrastructure, or deriving from less advantageous natural resources endowments, and the structure of comparative advantages. Factor productivity growth was harder to achieve in rye rather than in iron, and in wine rather than in textiles or machinery.

Conclusion

By taking into account the experiences of the periphery, the mix of growth narratives and causal factors inevitably becomes more complex and ultimately renders more difficult speaking of a European economy. With the periphery, the Modern European economy necessarily appears both more unified and more complex, which is of course a source of its own dynamism. That may be the ultimate conclusion of this essay, namely, that economic history is more about increasing our knowledge about how economies function than about finding simple answers to the causes of economic change or, in other words, it is more about getting the consequences right then about reaching definite conclusions about the causes.

Geographical unity in economic development was not necessarily accompanied by unification of political institutions or social and cultural values. A traveller across the European continent will find many differences in almost all aspects of day to day living which make direct connection

between the present and the more or less distant past. Those differences are not necessarily atavistic, but rather they are the outcome of a number of responses to different challenges of the economic, social or cultural environments. Europe in the last two centuries is thus the outcome of a pattern of development with many similarities, as well as the outcome of many different institutional and cultural responses. These two axes probably represent tensions that are not to be solved but only managed indefinitely and the prospects of development are most probably dependent on which forces are dominant. In the 19th century, economic integration dominated, although national interests were also present with a growing level of intensity. During the interwar period, national interests became clearly dominant, whereas the forces of economic integration somehow faded away. After the Second World War, coordination ruled again, but this time under the supervision of international institutions. In the last decades of the 20th century, all moved in the same direction again, but this time in a more open and demanding way. Undoubtedly, these patterns provide a template for developments elsewhere in the world, in what both unity and diversity are concerned.

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Laudatio Kaat Wils

Raf Vanderstraeten

Ghent University celebrated its bicentennial earlier this academic year; the university was inaugurated at the end of 1817. In comparison with several other European universities, it is still relatively young. Many other European universities have medieval or early-modern origins. Ghent University has, however, gradually developed a sensitivity for its own past. It has gradually started to look upon its own history as the foundation of both current and future projects. The way the university has celebrated its own bicentennial anniversary is indicative of this 'historical turn'. We have been able to visit expositions; we now also dispose of an official history of the university (Deneckere, 2017) and a detailed online encyclopaedia (UGentMemorie). The upcoming establishment of a Museum for the History of Science is another indication of the fact that the university takes its history seriously.

Perhaps one of the first significant indicators of the university's growing interest in its own history was the 'discovery' and celebration of the alumnus George Sarton. As we know, Sarton was born in Ghent in 1884. He graduated in 1906 and received his PhD in 1911 – both from the State University of Ghent. But he did not make a career at the university of his hometown. He spent most of his professional life in the Widener Library at Harvard University in the US. He devoted much time to the journals – *Isis* and *Osiris* – and the association – *History of Science Society* – which he founded (Pyenson, 2007). Especially in the years after the Second World War, when the field of history of science started to expand rapidly, he received significant praise for his lifework. Public recognition of his achievements was shown in many ways. Since 1955, the *History of Science*

Society annually awards the George Sarton Medal to an outstanding historian of science. George Sarton himself was the first medalist; he received the medal just a few months before his death. Some thirty years later, in 1984, at the centenary of Sarton's birthday, the State University of Ghent also decided to establish its George Sarton Medal. The first one was awarded in 1986/'87 to Robert Merton, a former student of George Sarton. With Eric Hobsbawm, we might say that this award is an "invented tradition" (Hobsbawm & Ranger, 2012). Especially in the last years, however, this tradition has also gained considerable acclaim. It has not only come to play an important part in the public presentation of self of Ghent University. Over the years, the medals have also been awarded to several of the world's leading historians and sociologists of science.

Today we celebrate the work accomplished by Professor Kaat Wils, who is Professor in the History Department at the Catholic University of Leuven. She has written extensively on the history of sociology, especially within Belgium in the decades around 1900. It is, I think, no coincidence that she thus analyzes the period in which George Sarton also came of age in Belgium: the contexts within which Sarton developed his wide-ranging interests partly coincide with the contexts within which sociology took off in the center of Europe (e.g., Wils, 2005; Wils & Rasmussen, 2012). In this context, she has also written about George Sarton himself (e.g., Wils, 2005). The historical analyses of sociology which Kaat Wils has presented are both rigorous and original. In exceptional ways, she is able to combine a detailed mastery of the historical sources with original theoretical insights. Her scholarship, which may be characterized as a cultural history of sociology, challenges the ways sociologists imagine their own history, their own past, in many different ways.

Overall, many of the social sciences have maintained an interest in their own history. Contrary to most natural sciences, the social sciences generally have not abandoned an interest in the history of their own disciplines. The history of sociology has largely remained a part of the parent discipline; it has not become incorporated into history departments. Within sociology, however, the history of sociology often is defined as a field of teaching, not as a field of research. Much is relegated to textbooks, which focus on the founding fathers, the great books and the classic articles. This history is included in curricula, especially at the Bachelor level, in order to

give students a sense of the definition and achievements of the field they have chosen, and to help imagine themselves as heirs to a great tradition. For the last decades, these sociological textbooks have concentrated on what Anthony Giddens and others have called the "Holy Trinity of Sociology": the lives and works of Karl Marx, Max Weber and Emile Durkheim. As sociologists or social scientists, we may consider why we appropriate and fetishize our past in this way; it probably has a lot to do with the fact that our disciplinary identity is vague and that it allows us to give a scholarly or theoretical veneer to studies with a predominantly empiricist orientation (Connell, 1997). As historians point out, however, such interests also limit the ways in which historical research about the discipline is conducted. Often research in the history of sociology does not aim at understanding the past on its own terms and in its own context, but rather translates it into the language of the present and present-day concerns.

During the past decades, Kaat Wils has done much to change this picture. She has, for the most part singlehandedly, drafted intriguing pictures of late nineteenth and early twentieth century sociology in Belgium and Europe – not by focusing on the stars, but by analyzing the dynamics of knowledge production, the stakes of the debates over what social science could constitute, the conceptual relations and developments, the networks within which the sociologists operated, the boundaries they tried to establish or overcome, the broader conditions under which they did their work, and so on (e.g., Wils, 2001, 2011; Wils & Rasmussen, 2012). I think that I am not mistaken when I say that her work is still better known among historians than among sociologists. Regrettably, disciplinary boundaries often stand in the way of scholarly communication. But if one looks at the quality of her work on the history of sociology, one is tempted to say that the best institutional position to conduct this kind of historical research is outside sociology. Her work has not always received the sociological attention and credit it deserves, but it is among the very best we currently dispose of.

Let me conclude: Kaat's work is a source of inspiration and reflection that helps to combat many of the biases with which we are confronted both in the social sciences in general, and in much of the current writings on the history of the social sciences in particular. I am really pleased that both my colleagues of the Faculty of Political and Social Sciences and the Sarton

Committee of Ghent University were willing to follow my suggestion to award the Sarton Medal for the academic year 2017/2018 to Kaat Wils. The George Sarton Medal might rely on an invented tradition at Ghent University, but I am and we are proud to be able to honour today Prof. Kaat Wils for her outstanding work in the history of the social sciences in general and of sociology in particular.

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Medical hypnosis, self and society in fin de siècle Belgium

Kaat Wils

It is a great honour to receive the Sarton Medal for my work on the history of sociology. I am all the more delighted as the acknowledgment comes from colleagues from the Faculty of Social and Political Sciences, the intellectual heirs or successors of the scholars I have been studying. An additional reason for delight is the fact that George Sarton figures in my own, older research on the reception of French and British positivism in Belgium. When Sarton launched his journal *Isis* in 1913, he was indeed very much inspired by Auguste Comte's ideas on the cultural role of the history of science. He contacted several representatives of Positivism as an institutionalized movement and asked them for advice. In 1926, in a letter to the British chemist and religious positivist Cecil Henry Desch he made the following confession: *'It is because of Comte, that I became a historian of science'*.[1]

I would of course love to confess that it is because of Sarton that I became a historian of science, but that would be a lie. Sarton's broad-minded, open conception of the history of science does however remain inspiring, especially for cultural historians interested in the changing boundaries of science and in cultural representations of scientific knowledge. As far as the topic of today's lecture is concerned, I was happy to find out that Sarton devoted a few lines to animal magnetism and Anton Mesmer, on whom he stated: *'Mesmer opened the door to all kinds of experiments, wise and*

[1] See Kaat Wils, *De omweg van de wetenschap. Het positivisme en de Belgische en Nederlandse intellectuele cultuur, 1845-1914*, Amsterdam: Amsterdam University Press, 2005, 397-398.

foolish; he may be considered an ancestor of Charcot and Freud on the one hand, and of Quimby and Mrs. Eddy on the other' (the latter ones being 19th century American spiritual healers).[2]

But again, it was not Sarton, but rather the history of sociology which inspired my current research on the therapeutic uses and the cultural meanings of magnetism, hypnotism and suggestion in the late-19th and early-20th centuries. At the Brussels Institute of Sociology, for instance, interest in the role of suggestion in group behaviour was outspoken. The work of authors such as Gabriel Tarde, Gustave Le Bon and also Sigmund Freud was discussed in this perspective. According to one of its members, the German psycholinguist Paul Menzerath, suggestion even constituted the fundament of social life and the central problem of sociology.[3]

Suggestibility made its entrance into late-19th century social theory in the slipstream of the popularity of hypnosis as both a therapeutic practice and a form of public entertainment. Not unlike its predecessor, mesmerism or animal magnetism, hypnosis attracted quite some public attention and uneasiness. Hypnosis not only questioned the boundaries between health and illness or orthodox medicine and charlatanism, it also seemed to address some key problems of modern mass society and undermine traditional conceptions of a stable self.[4] Today, I will sketch hypnotism's position in late-19th and early-20th century Belgium. I will thereby focus on a central paradox: hypnosis as both a symptom and a cure, both a danger and a solution for a society whose members were considered to suffer from new, modern illnesses.

Let us start in the early 1880s with the private correspondence between the Belgian doctor and socialist César De Paepe and his Dutch friend and

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2 George Sarton, 'Second Preface to Volume XXXV. Vindication of Father Hell', *Isis,* 35:2 (1944), 98.

3 Paul Menzerath, 'On W.D. Scott, *Personal Differences in Suggestibility*', *Archives Sociologiques,* 1910, review nr. 54.

4 On the public visibility of hypnosis in the period 1880-1900, see e.g. Ruth Harris, *Murders and Madness. Medicine, Law and Society in the Fin de Siècle*, Oxford: Oxford University Press, 1989, 155-207; Jacqueline Carroy, *Hypnose, Suggestion et Psychologie. L'invention de Sujets*, Paris: Presses Universitaires de France, 1991, 48-64, 89-96; Alan Gauld, *A history of Hypnotism*, Cambridge: Cambridge University Press, 1992, 297-362; Stefan Andriopoulos, *Possessed: Hypnotic Crimes, Corporate Fiction, and the Invention of Cinema*, Chicago: The University of Chicago Press, 2008, 19-41; Heather Wolffram. *The Stepchildren of Science: Psychical Research and Parapsychology in Germany, c. 1870-1939,* Amsterdam: Rodopi, 2009, 83-130; Andreas Mayer, *Sites of the Unconscious: Hypnosis and the Emergence of the Psychoanalytic Setting,* Chicago: University of Chicago Press, 2013, 93-107.

Maecenas, the socialist pastor Ferdinand Domela Nieuwenhuis.[5] Sketching in 1883 the tragic situation of his family, where both his daughter of 15 and his wife had been 'dishonoured' by a family friend while he was on a trip to America, De Paepe considered himself partly guilty of the situation. His wife suffered from a nervous disease – '*hysteric and epileptic in nature'* – and this, De Paepe confessed, was a result of his own 'abuse' of hypnotism: he had used his wife as the main subject of his hypnotic experiments. By experimenting too much on her, her sense of dignity and morality had been broken, De Paepe seemed to suggest. Three years later, in 1886, De Paepe sounded more optimistic. He wanted to cheer up his friend, who was now in prison, accused of lese majesty. Having travelled to Paris to perform a childbirth in a befriended family, De Paepe was taking a few days off in the French capital. Each time he was in Paris, he explained to Domela, he went to dr. Charcot's hospital la Salpêtrière to assist at his curious experiments in hypnotism. De Paepe explained that magnetism and hypnotism were more or less the same thing, and introduced his friend to the phenomenon of 'crimes under suggestion.' While this phenomenon might be frightening, it also opened new prospects for disciplines such as legal medicine, criminology, psychology, education and ethics, he believed. The time would come, De Paepe concluded, that one would subject criminals in prison to hypnotic suggestions that would make them perform morally good, altruistic deeds. Even the most dangerous criminals who had more in common with beasts than with human beings, could maybe, thanks to the use of hypnosis, be brought back to social life.[6] Hypnotism, in other words, could not only be used to break somebody's will or destroy his or her sense of responsibility, it might just as well be used to restore morality and rebuild a better, more altruistic society.

As a socialist fully involved in the First International, De Paepe was of course a rather atypical doctor in late-19th century Belgium, where most doctors were happy to be part of the higher ranks of bourgeois society. De

5 On the friendship between De Paepe and Domela Nieuwenhuys (who named two of his sons after his friend), see Minte Kamphuis, 'Een spreekend voorbeeld. Contact en transfer bij socialisten in Nederland en België rond 1880', *De Negentiende Eeuw*, 32(2008), 253-270; Jan Willem Stutje, *Ferdinand Domela Nieuwenhuys. Een romantisch revolutionair*, Antwerpen/Gent/Amsterdam: Houtekiet/Atlas, 2012, 81-83.

6 César de Paepe to Ferdinand Domela Nieuwenhuys, 20 June 1883 and 18 June 1886. Amsterdam, International Institute for Social History, Fonds 208 Domela Nieuwenhuis – Foreign Correspondence.

Paepe's quite utopian belief in the social potential of hypnotism certainly resonated more with his science-oriented socialist worldview than with common opinions among doctors. Quite some doctors did however share his curiosity about hypnotism and his wish to learn about it by assisting in Charcot's famous public clinical lessons. From the early 1880's on, the well-reputed French neurologist had given modern hypnotism scientific legitimacy – a legitimacy it had missed until then, due to its relationship with older forms of animal magnetism. In his public lessons, Charcot experimented on female patients who had been diagnosed with hysteria and whom he believed to be easily hypnotized, as part of their pathological condition. However, visits to Charcot would soon be paralleled by visits to Hippolyte Bernheim's clinic in Nancy, where a quite different, non-pathological approach to hypnotism was demonstrated and where patients with a diversity of complaints were treated.[7]

Notwithstanding the many divides within the community of hypnosis scholars, most doctors did share De Paepe's 'double belief' in hypnotism as both a potential cause and a cure for contemporary social illnesses and mental diseases. As I will explain, relegating hypnosis to the strictly medical domain appeared to be an elegant way out of this paradox.

Enslaved subjects in a modern mass society

In Belgium, medical interest in the therapeutic potential of magnetism had been rather marginal in the early- and mid-19th century.[8] The handful of doctors who published on the topic were open about the fact that no satisfactory scientific explanation existed as yet for the remarkable influence a magnetizer could have on a subject. They were careful to distance themselves from professional magnetizers, both lay healers and stage performers, who were said to be solely inspired by commercial motives and

7 The literature on Charcot is vast. On Charcot's lessons, see Jonathan W. Marshall, *Performing Neurology. The Dramaturgy of Dr Jean-Martin Charcot*, New York: Palgrave Macmillan, 2016.

8 The history of magnetism in Belgium constitutes almost unexplored terrain, except for G. Zorab, 'Belgium', in *Abnormal hypnotic phenomena. A survey of nineteenth-century cases* (ed. Eric J. Dingwall), vol.2, London: J. London & A. Churchill, 1967, 3-50; Marijke De Sadeleer, '"Druk Uw Handen Op Mijn Zieke Ledematen". Een Lichamelijke Benadering van het magnetisme in het negentiende-eeuwse België', *Tijd-Schrift. Heemkunde en Lokaal-erfgoed Praktijk in Vlaanderen,* 5: 3(2015), 35-47.

hence to threaten the dignity of the medical profession.[9] While the effectiveness of magnetism was recognized, potential dangers were also identified. In a presentation for the Royal Academy of Medicine, the Ghent professor Adolphe Burggraeve for instance concluded that, despite his own positive experience with magnetism as an anaesthetic tool, he had to advice against its use. Ultimately, magnetism was dependent on a perturbation of the nervous system which should not be further provoked. It affected people who were already delicate at the start and in the process became all the more vulnerable. In the end, 'they are no longer free human beings, but slaves,' Burggraeve argued.[10] The potential moral and more specifically sexual dangers of magnetizers' excessive influence on their subjects was soon also defined in a more collective fashion. In 1863, in a small treatise meant to establish magnetism's alleviating and curative medical effects, the Brussels surgeon Henri Van Holsbeek pointed to the danger that magnetism would 'disturb the tranquillity of societies and families'. To anticipate this, the government should prohibit its non-medical use, Van Holsbeek advised.[11]

15 years after Van Holsbeek's call and in the midst of a transnational wave of moral panic about the popularity of itinerant magnetizers' spectacular shows, the Belgian government judged it necessary to act against the perceived dangers of magnetism, which now also came to be referred to as 'hypnotism'. In 1888 the Catholic Minister of Justice Jules Lejeune proposed in Parliament to vote a law which would ban public shows of hypnosis and restrict its medical use on minors and mentally ill to doctors. The issue was brought up a few weeks after a series of popular performances had been held in Brussels. In these shows, a French magnetizer had put his young female subject on stage in a cage with three lions. Anxiety about the excesses of stage hypnotism now seemed justified. During the four years of public debate which followed (and which would lead to the adoption of a slightly altered text of the law), the potential dangers to which Burggraeve and Van Holsbeek had referred stood centre stage. The main

9 See for instance 'Observation d'un cas d'hystérie, caractérisé par des symptômes extraordinaires, par M. Le docteur A. Sotteau', *Annales de la Société de Médecine de Gand* 14 (1850), 177-228.

10 Adolphe Burggraeve, 'Du magnétisme animal et de ses applications à l'art de guérir', *Bulletin de l'Académie Royale de Médecine de Belgique* 2nd series, 2(1858-1859), 147-165, quotation on p. 155.

11 Henri Van Holsbeek, *Lettres sur le magnétisme animal*, Brussels: De Tircher et Monceaux, 1863, 26.

arguments that were used in favour of a ban on shows referred to the moral dangers involved. Hypnotism was said to destroy the free will of its subjects, and hence their moral capacities. Modern city life, it was suggested, added to a general increased nervous sensibility. Women, adolescents and other 'impressionable' or 'nervous' people would constitute the first victims of hypnotism's degrading effects on human dignity. It was an argument that could be merged into the new organicist political discourse on the need to protect weak members of society, a discourse which sounded all the more convincing as the dangers of hypnotism were represented in terms of disease, and its success as a new form of epidemic.[12]

Arguments of a more explicitly political nature were equally used. Hypnosis, so it was argued, killed the free will, the foundation of modern citizenship. In a report of the Academy of Medicine, which was asked for advice on the question, a comparison was made with the significance of the liberty of the nation, a cherished romantic topos in the relatively young and liberal state of Belgium. The effects of hypnotism were also compared with those of ancient slavery.[13] In Parliament, representatives referred to the comparisons that were made in France between individual criminal suggestibility and the suggestibility of the masses in the political demonstrations of French Boulangism, a popular nationalist and anti-parliamentary movement.[14] In a context of recent large scale and violent workers' protest and of an upcoming Socialist Party which strived for universal suffrage, Belgian members of Parliament associated the dangers of hypnotism with their political concern about the crowd, and about mass insurrection. Soon, this association would be made explicit by popular writers on crowd psychology such as Gustave Le Bon, who claimed that a crowd, just

[12] On this debate, see more extensively Kaat Wils, 'From Transnational to Regional Magnetic Fevers. The Making of a Law on Hypnotism in Late Nineteenth Century Belgium,' *Notes and Records: The Royal Society Journal of the History of Science*, 71:2(2017), 179-196.

[13] For the debate in the Academy, see *Bulletin de l'Académie Royale de Médecine de Belgique* 4th series, 2(1888), 19, 95-140 (with on page 113 the political references), 225-250, 312-377, 405-438, 503-557, 582-607, 633-644, 664-678, 838-897; 4th series, 5(1891), 774-777.

[14] For the parliamentary debate, see *Annales Parlementaires. Chambre des Représentants*, Sessions of 24 and 25 January 1888, 27 April 1888, 3 and 4 December 1891 and 11 and 12 May 1892 (with the reference to the Boulangist movement on 3 December 1891); *Annales Parlementaires. Sénat*, Sessions of 17 and 18 December 1891 and 19 May 1892; *Documents Parlementaires. Chambre des Représentants*, 15 April 1890, 24 April 1891, 10 March 1892; *Documents Parlementaires. Sénat*, 15 December 1891.

like a hypnotizer, made individuals into automatons without a personal will.[15]

Hostility towards public shows of hypnosis was probably also informed by gendered concerns. While Charcot experimented exclusively on female patients diagnosed with neurological disorders, Europe's most famous itinerant magnetizer, the Belgian born Donato, performed on young and healthy men.[16] Part of the attraction of his shows consisted precisely in his subjecting even the most disbelieving and resisting men, by preference men of higher social standing, and to force them literally on their knees.[17] Donato was well aware of the way in which his performances destabilized gender (and social) hierarchies, while he kept at the same time heterosexual normativity intact. 'In matters of magnetism, it is like in matters of love', he explained, 'Magnetizers are seducers. […] Our subjects, men no less than women, are quite feminine in this respect.'[18] In an era in which strength of character constituted an important element of the bourgeois male self, the 'loss' of character and masculinity could easily be considered dangerous.

Even though Donato's experiments with healthy men had been successfully replicated by the French doctor Brémaud in 1884, most Belgian doctors who were involved in the debate on the necessity of a law insisted on a more pathological interpretation, associating hypnosis in some way or another with a nervous sensibility close to illness.[19] It was a line of thought

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15 There exists a wealth of older literature on the connections between hypnotism and early popular crowd psychology. See for instance Robert Nye, *The Origins of Crowd Psychology: Gustave Le Bon and the Crisis of Mass Democracy in the Third Republic*, London: Sage Publications, 1975; Susannah Barrows, *Distorting Mirrors: Visions of the Crowd in Late Nineteenth-Century France*, New Haven: Yale University Press, 1981; Serge Moscovici, *L'âge des foules, Un traité historique de psychologie des masses*, Paris: Fayard, 1981; Jacqueline Carroy, 'Le peuple, le magnétisme et l'hypnose. De l'invention du peuple à celle des foules', in: Rose Goetz and Alain Trognon (eds.), *L'invention du peuple*, Nancy: Presses Universitaires de Nancy, 1993, 137-148.

16 On Donato's international career and the reception of his shows in Belgium, see Kaat Wils, 'Tussen wetenschap en spektakel. Hypnose op de Belgische theaterscène, 1875-1900', *Tijdschrift voor Mediageschiedenis*, 20:2(2017), 54-73. On his role in the French medical debate on hypnotism, see Harris, *Murders and Madness*, 200-201.

17 See, for instance, a press report in *La Meuse*, 2 November 1877.

18 'Il est en magnétisme comme en amour. Les magnétiseurs sont des séducteurs. […] Nos sujets, hommes autant que femmes, sont bien féminins sous ce rapport.' Donato, 'Examen du livre *Le somnambulisme provoqué. Etudes physiologiques et psychologiques* par le Docteur H. Beaunis, professeur de Physiologie à la Faculté de Médecine de Nancy', *Le Magnétisme*, 1886, 214-220, quotation on p. 220. On the gendered and political significance of Donato's performances in Italy, see Suzanne Stewart-Steinberg, *The Pinocchio Effect. On Making Italians (1860-1920)*, Chicago: Chicago University Press, 2007, 70-73.

19 Paul Brémaud, *Des différents phases de l'hypnotisme et en particulier de la fascination*, Paris: Cerf, 1884. See also Gauld, *A History*, 328.

that allowed for a pessimistic reading of the state of modern society and that facilitated pleas for a medical monopoly on the therapeutic practice of hypnosis.

Engaging the subject to cure society

The debate on the regulation of the practice of hypnotism took place in a period in which doctors were looking for ways to strengthen their (still very much contested) authority through legal claims towards a monopoly. In the case of hypnotism, doctors in Belgium were successful in having the state defend their interests (albeit in ways which did not cost anything to the state). The relative ease with which the law came into being testifies to the strength of the wave of moral panic surrounding the phenomenon of hypnosis. When the issue was first raised in Parliament, hypnotism had barely been on the medical research agenda. Medical expertise was hardly developed and there were no testified cases of crimes committed under hypnosis.

Dissident voices who had opposed Lejeune's bill did not fail to notice this. Lay magnetizers pointed to the fact that doctors regularly entrusted patients with chronic complaints to them, as hypnotism was a very time-consuming activity that required specific skills.[20] The two Academy members who did oppose a ban on public performances pointed to the fact that its defenders were unable to name problems which had resulted from performances in Belgium. They merely seemed to reiterate foreign complaints, such as the ones by the Italian doctor Cesare Lombroso against Donato.[21] The sharpest opposition against the bill came from the psychologist Joseph Delboeuf, Belgium's sole hypnosis scholar with international renown around 1890. An old-style liberal, Delboeuf defended the freedom to organize and assist at hypnosis shows, which he

[20] A. Bonjean, *L'Hypnotisme, ses rapports avec le droit et la thérapeutique, la suggestion mentale* (Paris: Alcan, 1890); A. Denis, *La voie naturelle et l'utilité de l'hypnotisme*, Paris-Verviers: Gilon, 1891; L. Lobet, *L'Hypnotisme en Belgique et le projet de loi soumis aux Chambres législatives*, Verviers: Massin, May 1891; L. Lobet, *L'Hypnotisme devant les Chambres belges. Lettre ouverte à Monsieur le Sénateur*, Verviers: Massin, December 1891.

[21] On the Italian reception of Donato, see Patrizia Guarnieri, 'Theatre and Laboratory: Medical Attitudes to Animal Magnetism in Late-Nineteenth-Century Italy,' in: Roger Cooter (ed.), *Studies in Alternative Medicine,* London: Macmillan, 1988, 118-139; Maria Teresa Brancaccio, 'Between Charcot and Bernheim: The Debate on Hypnotism in *Fin-de-Siècle* Italy,' *Notes and Records: The Royal Society Journal of the History of Science* 71(2017), 157-77.

considered both instructive and innocent. The new trend to persecute stage magnetizers in the name of public health and morality was closely intertwined, according to Delboeuf, with the desire of doctors to claim the benefits of these magnetizers' discoveries. Delboeuf also questioned the dangers of criminal suggestion, underlining the amount of role-playing which was at stake in most experiments where hypnotized subjects were incited to commit crimes. As a philosopher, he had invested a lot of his intellectual energy in reconciling scientific determinism with his belief in free will. His interpretation of hypnotism left this belief intact: he saw the patient and his willpower to which the therapist appealed as the active party in hypnosis.[22]

Stressing the societal dangers of a free practice of hypnosis and thereby positing the subject's loss of willpower had served the goal of relocating hypnotism within the confines of established medicine quite efficiently. But it surely was not a very relevant frame of reference when it came to the practice of hypnosis. Subjects turned out not to be mere 'slaves' of the hypnotizer. Even Donato, who was famous for transforming strong men instantly into unresisting automatons, explained to fellow magnetizers: 'magnetism is made of sympathy and trust.'[23] The few Belgian doctors who had already trained themselves around 1890 in the therapeutic practice of hypnotism or hypnotic suggestion stressed the active role of the subject, without whom a therapy could simply not succeed. Looking back on three years of experience, a doctor from Liège for instance concluded that when he did not succeed in hypnotizing a subject, this was most often the result of 'the bad will' of the subject, his lack of attention or his obstinate will to analyse all actions and words of the hypnotizer.[24] Clearly, there were many

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22 On Delboeuf's role in the Belgian debate, see Wils, 'From transnational to regional magnetic fevers.' On Delboeuf's scholarly work on hypnosis, see François Duyckaerts, *Joseph Delboeuf, philosophe et hypnotiseur*, Le Plessis-Robinson: Laboratoires Delagrange-Synthélabo, 1992; Jacqueline Carroy and Pierre-Henri Castel (ed.), *Delboeuf et Bernheim entre hypnose et suggestion*, special issue *Corpus. Revue de Philosophie* 32 (1997); Alan Gauld, 'Joseph Delboeuf (1831-1896): a forerunner of modern ideas on hypnosis', *Contemporary Hypnosis,* 14(1997), 216-225; A. Leblanc, 'Thirteen Days: Joseph Delboeuf versus Pierre Janet on the Nature of Hypnotic Suggestions', *Journal of the History of the Behavioral Sciences* 40(2004), 123-147; Jacqueline Carroy, *Nuits savantes. Une histoire des rêves (1800-1945)*, Paris: Editions de l'EHESS, 2012, 183-202.

23 'Le magnétisme est fait de sympathie et de confiance'. Donato, 'La sujétion persistante et la suggestion à échéance', *Le Magnétisme*, 1886, 131-137, 180-190, quotation on p. 188.

24 'Les insuccès constatés dans le civil sont généralement le résultat du mauvais vouloir du sujet, du peu d'attention apportée par celui-ci ou de son obstination à vouloir analyser tous les actes, toutes les paroles de l'hypnotiseur.' Dr. Ernould, 'Hypnotisme ou suggestion hypnotique', *Gazette Médicale de Liège*, 4(1891-92), 316-317.

ways in which a subject could resist.

Throughout the 1890s, medical interest in the practice of hypnotism increased. The newly established Belgian Society for Neurology, and its journal, the *Journal de Neurologie et d'Hypnologie,* explicitly welcomed work on hypnosis and suggestion.[25] When in 1897, the society organized an International Conference, hypnotism again was on the agenda. The theme of criminal suggestion, which had gained quite some attention in Belgium over the past few years, was discussed, in particular in relation to Delboeuf's famous refutation of it. The French lawyer from Nancy, Jules Liégeois, reiterated the school's point of view that 4 to 5 percent of the population was so impressionable that they could become unknowingly the instrument or victim of criminal suggestion. As a means of prevention, Liégeois proposed that the population as a whole would be tested and that impressionable persons would yearly receive an antidote against malicious suggestion through benevolent suggestion, 'a kind of moral vaccination', as he called it.[26] The British doctor Milne Bramwell revealed himself as Delboeuf's main defender. Starting from his clinical experiences, he stressed that the will of the subject should never be underestimated, and that this also applied to the experimental setting of so-called 'laboratory crimes'.[27] It was a point of view which was shared by clinicians who presented their therapeutic work at the conference. Doctor Prosper Van Velsen, for instance, who since 1890 ran a private institute for hypnosis and hypnotherapy in Brussels, insisted on this point: 'One still claims that

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25 The journal was launched in 1896 as *Journal de Neurologie & d'Hypnologie. Neurologie, Hypnologie, Psychiatrie, Psychologie.* In 1898, the title was changed into *Journal de Neurologie. Neurologie, Psychiatrie, Psychologie, Hypnologie.*

26 'Résumé du Rapport de M. le professeur Liégeois de Nancy. Les suggestions criminelles', *Journal de Neurologie et d'Hypnologie*, 2(1897), 371-376; Jules Liégeois, 'La question des suggestions criminelles. Ses origines – son état actuel', *Journal de Neurologie*, 3(1898), 22-49, quotation p.47. On the debates on criminal suggestion, see for instance R. Harris, 'Murder under hypnosis in the case of Gabrielle Bompard: Psychiatry in the court-room in Belle Epoque Paris', *Psychological Medicine* 15: 3(1985), 477-505; Heather Wolffram, 'Crime and Hypnosis in Fin-de-Siècle Germany: The Czynski Case', *Notes and Records: The Royal Society Journal of the History of Science*, 71: 2 (2017), 213-226.

27 'Résumé du rapport de M. le docteur Milne Bramwell de Londres, *La valeur thérapeutique de l'hypnotisme et de la suggestion*', *Journal de Neurologie & d'Hypnologie*, 2(1897), 378-382. On Milne Bramwell, see Gauld, *A History of Hypnotism*, passim; Teri Chettiar, '"Looking as Little Like Patients as Persons Well Could": Hypnotism, Medicine and the Problem of the Suggestible Subject in Late Nineteenth-Century Britain,' *Medical History*, 56: 3(2012), 335-354.

hypnosis takes away the will; the opposite is the case.'[28] Van Velsen went on to express his regret that hypnosis was barely taught at Belgian universities – to his knowledge, it was only in Brussels and in Louvain that students had the possibility to take a clinical course. His suggestion seemed to be that the only way to fight misconceptions on hypnotism was to train doctors in its use.

The reports of the meetings of the Belgian Society of Neurology show that even without much formal training, doctors experimented with different forms of hypnosis and suggestion when trying to cure or soften physical symptoms of neurological or mental disorders, such as spasms or forms of paralysis. In order to succeed, they often had to start by overcoming patients' resistance against hypnotism. The anxiety to lose one's free will through hypnosis was indeed reported on as a widespread 'superstition' among patients.[29] Complaints about patients' superstitions were nothing new, they were part of a long tradition within the Belgian medical community.[30] The efforts to overcome patients' resistance by persuasion, in turn, testify to the negotiated character of the medical encounter in an era characterized by both medical paternalism and the tentative introduction of medical consent as a legal principle.[31] In stressing the role of persuasion and confidence, doctors also echoed a more specific development that was taking place within the field of surgery. As Sally Wilde has argued, patients' confidence in (and hence their reliance on) surgery rose during the 1890s as a result of surgeons' own growing confidence in the possibility of good surgical results. Surgeons actively invested in informing patients and in persuading them to agree to surgery because they genuinely thought it

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28 'On dit encore que l'hypnotisme enlève la volonté; c'est le contraire qui se produit, réserve faite aux abus de l'hypnotisme.' 'Résumé de la communication de M. le docteur Van Velsen, *La suggestion thérapeutique*', *Journal de Neurologie*, 2(1897), 419. With the 'abuses', Van Velsen probably alluded to public performances, against which he had publicly taken a stance as early as 1888, as a student in medicine (see Prosper Van Velsen, 'Hypnotisme', *Journal de Bruxelles*, 15 januari 1888).

29 Jean Crocq, 'Trois cas de Pollakiurie psychopathique, guéris par suggestion', *Revue de Neurologie*, 3(1898), 473-482, quotation 479.

30 On complaints about superstition, see for instance Carl Havelange, *Les figures de la guérison (XVIIIe-XIXe siècles): une histoire sociale et culturelle des professions médicales au pays de Liège* (Paris: Belles Lettres, 1990), 345-398.

31 The literature on the negotiated character of the medical encounter in the 19th century is vast. See, for instance, Nancy Theriot, 'Negotiating Illness: Doctors, Patients, and Families in the Nineteenth Century', *Journal of the History of the Behavioral Sciences* 37: 4 (2001): 349-368. For the hospital context in Belgium, see Valérie Leclercq, 'Langue de bois et vérités divines: pratiques de l'information à l'ère du paternalisme médical, Bruxelles, 1870-1930', *Gesnerus. Swiss Journal of the History of Medicine and Sciences,* 73:1(2016), 123-147.

would help them.[32] In the case of hypnotism, a similar evolution seemed to take place, although the question of the patients' own will was more complicated. Indeed, doctors described the therapeutic process of suggestion itself as a 'reeducation of the will'.[33] In order for this reeducation to be successful, a balanced affective relationship between patient and doctor was needed, based on respect, trust and hope.[34] Differently from other medical disciplines, the patient's trust and confidence were more than merely ethical questions concerning the doctor-patient relationship: they were also a prerequisite for a successful therapy.

While exploring the possibilities of hypnotism, members of the Belgian Society of Neurology did not seem to be very much alarmed by the social dangers which had been associated with hypnotism. Foreign controversies on criminal suggestion or work on the relationship between suggestion and politics were reported on, but in a rather distant fashion.[35] Doctors were clearly much more interested in concrete ways to cure specific pathologies. These pathologies, however, were not unrelated to the perceived 'nervous' state of modern society and its specific public health problems. Addiction to tobacco, alcohol, morphine or cocaine, kleptomania, agoraphobia, sexual inversion (homosexuality), onanism (masturbation), neurasthenia and of course hysteria: for all these conditions – most of them having been medicalized recently – hypnosis had the potential to offer recovery, or at least alleviation.[36] Rather than constituting a danger for society, hypnotism

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32 Sally Wilde, 'Truth, Trust, and Confidence in Surgery, 1890-1910: Patient Autonomy, Communication, and Consent', *Bulletin of the History of Medicine,* 83: 2 (2009), 302-230. Wilde's study concerns Britain, Australia and New Zealand.

33 Emile Spehl, 'Un cas de tic traité par suggestion', *Journal de Neurologie*, 4(1899), 246-247, 251-252, 289-290, 294-295, quotation p. 247.

34 See for instance 'Revue d'hypnologie', *Journal de Neurologie*, 3(1898), 391.

35 See for instance Camille Moreau, 'L'hypnotisme dans ses rapports avec la criminalité. Congrès International de Médecine Légale, Bruxelles, 2 au 7 août 1897', *Journal de Neurologie*, 2(1897), 298-300.

36 See, for instance, Otto Wetterstrand, 'Le traitement de la morphinomanie, du cocaïnisme et du chloralisme par la suggestion et l'hypnose', *Journal de Neurologie & d'Hypnologie*, 1(1896), 133-134; E. Régis, 'Kleptomanie et hypnothérapie' and C. Lloyd-Tuckey, 'Quelques cas d'inversion sexuelle traités par la suggestion', *Journal de Neurologie & d'Hypnologie,* 2(1897), 57; Jean Crocq, 'Un cas de paraplégie hystérique ayant simulé, pendant seize ans, une sclérose latérale – guérison par suggestion (Présentation de la malade)', *Journal de Neurologie,* 3(1898), 363-36; Lépinay, 'Phobies neurasthéniques traitées par auto-suggestion', *Journal de Neurologie,* 4(1899), 317; Edgar Bérillon, 'L'onanisme et son traitement psychothérapique', *Journal de Neurologie,* 4(1899), 318; Dr. Bourdon, 'Tabagisme et alcoolisme guéris par la suggestion hypnotique', *Journal de Neurologie,* 4(1899), 318; Vlaviano, 'Agoraphobie traitée par la suggestion hypnotique', *Journal de Neurologie,* 4(1899), 400. It should be noted, however, that – except for neurasthenia and hysteria – all the above examples consisted of summaries of articles which had first been published in the French *Revue de l'hypnotisme* or the German *Zeitschrift für Hypnotismus.*

seemed to promise a cure for some of its illnesses, on the condition that the subjects of these illnesses were willing to actively participate in a therapeutic trajectory.

Conclusion

In a very inspiring article on hypnotism and medicine in late-19th century Britain, historian Teri Chettiar has examined the question of why, despite evidence attesting to hypnosis' wide-ranging therapeutic uses, medical hypnosis remained controversial, and was excluded from serious medical consideration by 1900. Chettiar attributes this failure to the imagined dangers associated with the patient's state of suggestibility and weakened will-power. This state departed too much from the Victorian, liberal ideal of rational individual autonomy and became associated with the perceived unruliness and irrationality of the lower classes. Participants in the late-19th century medical debate on hypnotism were chiefly concerned not with the regulation of hypnotic practice in the interest of professionalization, but with the regulation of hypnotism's potentially problematic effects, Chettiar argues.[37]

While much parallels can be drawn with the Belgian debate, I also see a crucial difference. In Belgium, the middle-class anxieties to which Chettiar refers, did facilitate, not hinder, the protection of medical professional interests as regulated by the law of 1892 – a law which was, from an international perspective, quite unique in combining a ban on public shows and a regulation of medical practice. In its turn, the law stimulated and legitimized medical interest in the topic. Anxieties on enslaved subjects and unhealthy mass-behavior were, so to speak, turned upside down: in a doctor's hands, hypnotism allowed to actively engage the patient in curing society. Although César De Paepe's dream of re-educating all prisoners might have sounded utopian to Belgian neurologists and psychiatrists, they did share his belief that hypnosis could free patients from harmful inclinations, habits and mental representations. Medical hypnosis could, in other words, liberate society from problems that in other, more public contexts had been associated with the popularity of hypnotism as a lay practice.

[37] Chettiar, "'Looking as Little Like Patients as Persons Well Could", 335-354.

Laudatio John Durham Peters

Raf Vanderstraeten & Karin Raeymaeckers

George Sarton founded the journal *Isis* – his "second child" – more than a century ago. The journal's subtitle specified its remit: *Revue consacrée à l'histoire de la science*, *publiée par George Sarton, D.SC*. In the programmatic opening essay of *Isis'* first issue, Sarton argued that intellectual energy needed to be invested in the history of science in order to counterbalance the growing specialization in science or what he called the "division du travail scientifique" (Sarton, 1913, p. 4, p. 12). In his view, the history of science had to provide a *trait d'union* between the increasing number of specializations. It had to shed light on the various interactions and interdependences, on the many commonalities, on "all the bonds that unite the different sciences" (Sarton, 1913, p. 9, p. 12). For Sarton, it was necessary to counteract the increasing specialization and differentiation within the field of science in order to contribute to a "new humanism".

As we know, the trend towards increasing specialization did not come to an end in the last hundred years or so. Although the academic world has been confronted with a variety of crises in this period, it has altogether been growing considerably. It is this expansion of the population of academics, which has continued to fuel differentiation and specialization processes. Increasing numbers of scholars produced increasing amounts of research. Specialization grew as a means of dealing with this flood of material. At present, it is for anyone impossible to obtain a good overview of scientific developments in more than a few related specializations. One might say that the field of science has lost its human scale. In most cases, specializations have also begun to lose touch with their past. A world in which so much research appears so fast inevitably tends to forget older work. Forget-

ting one's own history is a way of dealing with information overload. Sarton's hopes and prospects have not come true, they have not been realized. In fact, the field of history of science, which he helped to establish, developed itself into a quite isolated specialization, characterized by a relatively strong internal orientation, but comparatively weak ties to a few related specializations (for an analysis of citation networks, see Vandermoere & Vanderstraeten, 2012).

Our intention is not to sketch a grim picture of the world of science. For many, specialization has led to swift progress within the world of science. For Sarton, too, only disciplined inquiry could allow us to get closer and closer to the truth. But, with Sarton, it might also be said that the trend towards increasing specialization has side-effects, it also goes at costs. Some themes or questions cannot be confined to a single discipline; the lack of knowledge of one's own knowledge traditions leads to a variety of myopia diseases, and so on. At the same time, however, we think that many agree that it is now both important and urgent to keep an eye out to transdisciplinary developments, to invest in the development of broader points of view, to improve our capacity for learning from the advances made in other, neighbouring fields of study.

John Peters, who currently is the María Rosa Menocal Professor of English and of Film & Media Studies at Yale University in the United States, is often identified as a media historian and/or as a social theorist. But, however useful as they are, these disciplinary labels do not do full justice to his broad range of scholarly interests and to the transdisciplinary relevance of his contributions (e.g., Peters, 1999, 2005, 2015). Professor Peters can best be described as a true generalist. He provides us with one of the best contemporary interpretations of Sarton's *trait d'union* between different specializations.

Building upon a broad historical and strong philosophical background, Professor Peters has been one of the first and still is one of the most influential researchers who critically analyses our human ability to communicate with others. In his noted publications, John has particularly analysed how various new communication technologies change the society we live in. He has commented on the invention of printing, the diffusion of the mass media, the rapid breakthrough of the digital era.

Already in the nineteenth century, for example, new technologies made the processing of information much easier. The transport of information became decoupled from the transport of goods. "Before the introduction of the telegraph, information travelled as did any other traded community. It moved along with the cargo, and though not usually bulky, its speed was limited to that of the fastest mode of travel of the day" (Lew & Cater, 2006, p. 147). In the second half of the twentieth century, of course, it became once again much easier to convey information very speedily. The computer technology we are now familiar with has thoroughly changed the way we interact and communicate with one another. But these different technologies do not just make interaction and communication easier, they also change the basic structures of the society we live in (De Keyser & Raeymaeckers, 2012). As we see it, much of Professor Peters' work is a critical, though not a pessimistic analysis of crucial social consequences of the rapid diffusion of new communication technologies.

On this basis, Professor Peters' work shows the need to revisit our conceptual systems. Most of the so-called classical theories in the social and behavioural sciences emerged in an industrial age. Our classical theories, with their emphasis on power or action, are germane to industrial society. They mainly focus upon processes of producing and trading goods or resources (commodities). What Professor Peters' historically and philosophically informed analyses make clear is that we need theories and concepts that reflect a different social reality. We need theories and concepts adequate to the emerging information society, to the new forms of communication that are possible. We read Professor Peters' work as a critical commentary both on contemporary society and on contemporary social theory. But it also is a commentary inspired by what George Sarton called the "new humanism" (Sarton, 1988).

Let us conclude. We should first say that this ceremony has been postponed; it originally was planned to take place during the academic year 2016/2017. But we are really pleased that we are able to award today – finally – the George Sarton Medal to John Peters. If we are well informed, George Sarton was not unknown to John Peters. Although John was born two years after George Sarton had passed away, John's father was a colleague of Sarton at Harvard University. It is not difficult to imagine that certain interests of Sarton have been passed over to the new generations

(Isaac, 2012). We believe that George Sarton himself would have been proud to see that we are able to award today the medal named after him to John Durham Peters.

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The Curious Power of Names

John Durham Peters

1. Names are Media

I am extremely honored to receive this honor from one of the world's distinguished universities and in a city of such beauty and charm. Ever since I first learned to speak Dutch as a nineteen-year-old Mormon missionary in the Netherlands many years ago, I have treasured this part of the world, its people, history, culture, and even food. Thank you to Professors Raf Vanderstraeten and Karin Raeymaeckers for the generous nomination and hospitality.

I am a media scholar, and media studies is a many-splendored field. In addition to much good research on print, electronic, and digital media in all their forms and effects, there is a more exotic, less traditional approach that I follow that understands media as infrastructures, as environments in which we live, move, and have our being. In this way of thinking, many things can be worthy of media analysis such as clocks, calendars, towers, ships, fire, and even clouds, as I argued in a recent book.[1] I am interested in things that seem transparent and boring, and even more, the processes by which they come to be transparent and boring. Often it is things that seem least promising for analysis are most revealing. Media theory's call to make the familiar strange echoes intellectual traditions such as Russian formalism, philosophical phenomenology, and social theory. Of course this approach sometimes requires patience with things that might seem obvious or plain.

What if we took names as a topic of media studies? Names of people and of places are fundamentals of communication that are so basic that they are

often taken-for-granted. They are essential for marking us as persons and our life on earth. That's what we leave behind on our grave: a birth date, a death date, and a name. Our surnames carry not only cultural, ethnic, and linguistic information, but sometimes even genetic information.[2] But names almost always disappear into the background of awareness. There it no environment or infrastructure more important than language, which works best when it is least obvious. If I may be personal again, I have noticed in the last few days in reviving my rusty Dutch how that pleasure and challenge makes language conscious and present for me, not habitual and automatic as it is when I speak English. My experience learning Dutch led to my first publication in 1980, a meditation on how words mean.[3] Here I return to this fundamental question, one I have never abandoned, only postponed. I want this *lezing* (lecture) to be a bit of a *bloemlezing*, in that beautiful Dutch word meaning *anthology* or *collection of flowers* – less a specialist focus than a survey of a larger problem. The approach I take here happily confirms Professor Vanderstraeten's characterization of my work as generalist!

2. Names Have a Bad Name

The analyst of names starts with contrasting proverbs. One side insists on insignificance: "What's in a name? That which we call a rose/By any other word would smell as sweet" says Juliet to Romeo in Shakespeare's *Romeo and Juliet* (II.ii). Similarly, "sticks and stones may break my bones, but names can never hurt me" is a phrase with which English-speaking parents (like my own) try to comfort tearful children who've been called names--usually unsuccessfully! Or maybe best of all, there is the critique of the inadequacy of names in Goethe's *Faust* (part 1, lines 3454-8):

> "Nenn's Glück! Herz! Liebe! Gott!
> Ich habe keinen Namen
> Dafür! Gefühl ist alles;
> Name ist Schall und Rauch,
> Umnebelnd Himmelsglut."

"Feeling is everything;/ name is sound and smoke," thunders Faust.

On the other side of the ledger, we have the Roman saying: *Nomen est omen*: naming is destiny. One of the biblical ten commandments concerns

proper names for deity! Devout Jews will refer to Deity only as *ha-Shem*, the name. It doesn't get more serious than that!

So on which side of these proverbs should we fall? Are names weak, simple tags that we affix to persons and things, or are they strong, ontological operators that govern the world? This question has mystified thinkers since at least Plato's *Cratylus*. Especially proper names have been of intense interest. These curious linguistic elements play a significant role in deciphering dead languages, in part because they are relatively indigestible and maintain relative integrity of form across languages. Proper names such as *Ptolemy*, *Cleopatra*, and *Berenice* were crucial for unlocking ancient Egyptian hieroglyphs for Champollion in the 1820s, and in the early 1950s Michael Ventris also relied on proper names to crack the code of Linear B, an archaic form of ancient Greek. Sigmund Freud devotes special attention to the forgetting of proper names in *The Psychopathology of Everyday Life*, and as we all know as we age and memory fades, proper names are the first things to go. Zooming out a couple of clicks, we see the relevance of naming in labeling theory in sociology, which shows how, for instance, deviance is constructed by processes of naming and unnaming, or framing research in media studies, which shows how news stories are spun by the way they are narratively and linguistically formatted.

3. Calling Names Empty is Not the Only Path to Enlightenment

But it is among the philosophers that the proper name has been most extensively considered. In the seventeenth-century such empirically minded thinkers such as Thomas Hobbes and John Locke declared war on the abuse of names. They rebelled against the supersophistication about names of their Scholastic predecessors. They saw names as dangerously apt to conjure nonexistent entities into being and in need of strict analytic discipline. From John Stuart Mill in the nineteenth century to Saul Kripke in the twentieth there has been an ongoing and brilliant discussion on names in analytic philosophy; one could wander forty days and forty nights in its thickets without becoming thoroughly enlightened!

I will save a journey into these thickets for another time to make a broad point instead: the empiricist tradition insists that the bond between name and object must be weak. Strong bonds between name and object risk

superstitution, idolatry and confusion. In his *System of Logic* (1843) Mill thought the labeling or denotative function of names had to be separated from the associative or connotative function. Often in this tradition the "savage" or "primitive" serves as a counterexample of how not to think about names. Take pioneering sociologist Herbert Spencer: "In primitive thought the name and the object named, are associated in such wise that the one is regarded as part of the other – so much so that knowing a savage's name is considered by him as having part of his being, and a consequent power to work evil on him."[4] Anthropologist J. G. Frazer in *The Golden Bough* played a similar tune: "Unable to discriminate clearly between words and things, the savage commonly fancies that the link between a name and the person or thing denominated by it is not a mere arbitrary and ideal association, but a real and substantial bond which unites the two in such a way that magic may be wrought on a man just as easily through his name as through his hair, his nails, or any other material part of his person. In fact, primitive man regards his name as a vital portion of himself and takes care of it accordingly."[5] In the early twentieth century, some of the most sustained critiques of naming-run-amok came from Ogden and Richards in their semantic treatise of 1923, *The Meaning of Meaning*. (We might note that George Sarton was one of many worried about confusion in scientific nomenclature.[6]) This critique of so-called primitive thought implies (wrongly) that modern men and women somehow do not regard their names as a vital portion of themselves!

Building on this tradition is tricky. Of course we want clarity and tools to cut through lies and propaganda, more than ever at this point in political history. But we don't want to succumb to the narrative of superstition versus science as if the theory of arbitrary names is necessarily more enlightened and modern. If we do, we risk forfeiting the ability to understand the curious power of names. To acknowledge the power of the name – which any kind of sociologically sensitive viewpoint requires – is not to succumb to magical powers; it is to confront some of the ways that we humans make sense of the world. We shouldn't condemn zones of inquiry: there be dragons! The empiricist and analytic tradition knows there are dragons in the study of names, and warns us to avoid them. I want us to go to the dragons and figure out how they work. The top five global brands, for instance – Apple, Google, Microsoft, Facebook, and Amazon--have annual revenues roughly the same size as Belgium's GDP.[7] Five names

are worth half a trillion dollars: this may be some kind of dark magic, but it is certainly something real! The internet, with its keyword search terms, disambiguation of proper names, fuzzy semantics, and even identity theft has only made more clear what's in a name: a lot! Domain names are internet real estate and can be worth billions. Personal names, my main focus here, also have value: a good name is worth more than rubies.

4. Names Necessarily Carry Semantic Excess

The conceptual dam between common nouns and proper names is a leaky one. We can never purge names of their semantic excess, and it is even wrong to think of it as excess: the extra mysterious resonance of meaning around names discloses something critical about how they work. Thick semantic webs remain even when we pretend that names are only tags. You can never keep *Sinn* (sense) walled off from *Bedeutung* (reference) very long, to invoke Gottlob Frege's famous distinction. For Frege the name *Venus* and *the morning star* have the same *reference* – they point to the same object, the second planet orbiting the sun. But the two names have very different *senses* or historical and semantic haloes (classic mythology for one and outdated astronomy for the other). Philosophical questions about the tagging function of names can get puzzling indeed: how do we tag a historical person (Aristotle) or a possibly fictitious one (Moses)? Are descriptions functionally equivalent to names, i.e., does *Aristotle* have the exact same reference as *the teacher of Alexander the Great*? Obviously Aristotle could have been Aristotle without ever having taught Alexander, but for us now, looking back in time, that description seems to coincide with his name. (An old joke: "Did you hear that Homer actually didn't write the *Odyssey*? Yeah, it was written by another dead, blind Greek guy with the same name.") Descriptions that were once contingent or fluid become fixed reference points later.[8]

5. Names are not just Arbitrary

Once a name is fixed onto a person or thing – in the act Kripke nicely calls baptism – they start to fill up irreversibly with new meanings. As William James nicely put it: "Names are arbitrary, but once understood they must

be kept to. We mustn't now call Abel 'Cain' and Cain 'Abel.' If we do, we ungear ourselves from the whole book of Genesis, and from all its connexions with the universe of speech and fact down to the present time. We throw ourselves out of whatever truth that entire system of speech and fact may embody."[9] Names may be arbitrary at the point of christening, but they soon start to take on durable semantic heft.

James's colleague Charles Sanders Peirce put it with more economy: "Symbols grow."[10] When Sarton, for example, named his journal *Isis*, he was well aware that there were many potentially confusing meanings surrounding that name, but it was impossible for him to know that one day the Islamic State would occupy its semantic real estate. The name *Isis* now holds more than it once did. Arbitrariness vanishes after the giving of a name. Constraint takes over as the name goes forth into the world.

One of the most striking features of names, then, is their simultaneous ontological stubbornness and historical flimsiness. Names are philosophically odd entities that can be both historically contingent and absolutely necessary. The flow is unidirectional from contingent to necessary, from arbitrary to essential. In the historical record we can point to that ex nihilo moment when the name was not in place and that stubborn substance did not yet exist. Names invite us to consider the nature of historical ontology: to be does not necessarily mean to always be. Ian Hacking's notion of "dynamic nominalism" shows how the act of giving a name – such as "multiple personality" or "kleptomaniac" – can change the ways people and institutions operate.[11] Just because the initial condition is relatively indeterminate does not mean that the follow-up is any less binding.

6. Nothing is as Socially Regulated as a Name

Personal names have complicated, sometimes state-enforced regulations around them. US courts have prohibited names that are obscenities, pictograms, and numerals. (Prince's brief change of his name to an unpronounceable symbol has become a legendary topic for comedy). One American judge ruled that you can name your daughter *Lucia* but not *Lucía* with an acute accent over the I. There are often rules about length. Many countries prohibit names that are gender-ambiguous. Puritans gave their children admonitory names such as Faith, Prudence, Chastity, Fear-Not,

and even Fly-Fornication. It is widely illegal for siblings to have the same name. You cannot name a child R2D2, but can name him Adolf Hitler even though everyone agrees that the latter name is much more offensive. The law of names can be as maddeningly capricious as names themselves.[12] Clearly, when we say that names are arbitrary, we cannot mean that anything can be a name. We mean that grounds for choosing among a strictly prescribed range of options are not strongly determined by the object. A baby can equally be named *George* or *Ralph*, *Mary* or *Gertrude*, but not *Truck* or 3.*14159*.

To take another case, in Iceland, there are fines for each day a newborn baby goes without being named. A committee of three people oversees personal names, and serving on it is apparently one of the hardest jobs in the country, as their decisions never make everyone happy. There is a national register of names from which a name must be chosen by parents or immigrants. Names must fit Icelandic tradition and bend according to rules of Icelandic grammar. When musician Vladimir Ashkenazy became an Icelandic citizen, his petition was granted to keep his name, which means that it in theory is now part of the register. Pranksters taking a new Icelandic name have asked to be named Vladimir Ashkenazy – a bid that though unsuccessful still proves a point to which we will return: a name cannot be unique.[13]

7. Surnames Index Lost Time

Most English commoners acquired surnames between 1250 and 1350. Besides patronymics (e.g. Jones, Williams), three main sources were professions (e.g. Smith, Taylor), geography (e.g. Green, Wood), and personal characteristics (e.g. Brown, Young). Genetic evidence suggests that there is a single progenitor for many English surnames. In patrilineal cultures, surnames and Y chromosomes move in parallel, passed down from the father. Names corroborate DNA: both are records, potentially, of older time.[14] Like genes and calendar systems, names are highly conservative. English surnames hardly ever acknowledge women's work, doubly due to patrilineal transmission and male-dominated definitions of work. (At least Knitter and Nurse do exist as surnames, but those jobs have not always been gendered female.)

Profession names in English are mostly frozen from medieval crafts and guilds such as Baker, Clark, Cooper, Fisher, Hunter, Harper, Knight, Monk, Parker, Wright. Modernity rarely enters into English surnames and industrialization never does. The only modern profession names of which I am aware (e.g. Contractor, Engineer) were given by British colonists to ethnic Parsis in the Indian subcontinent. Imagine someone named Accountant, Programmer, or Therapist! English profession names have an archaic or at least artisanal feel – you can always tell what kind of concrete work was done in contrast to the mystery that surrounds most jobs today.

Dutch surnames did not get officially stabilized until much more recently in the Napoleonic period. The sources of Dutch names mirror those of other European nations such as patronyms (Hendriks, Peeters), professions (Bakker, De Clercq), geographical features (van Dijk, van den Heuvel), personal characteristics (de Groot, de Jong), animals (Mol, Vos), birds (de Haan, Kok), and plants (Blom, van der Linden). These parenthetical examples I have taken from the top one hundred Dutch and Flemish surnames but less common names have interesting features. *Zondervan*, for instance, is a meta-name that says that the name is without a *van*, a common marker. In its meta-quality it is like the German *Wohlgenannt* or the Greek *Euonymos*, both of which mean *well-named*. In ancient Greek e*uonymos* euphemistically designated the left: calling something sinister by a good name fends off bad omens! This name acknowledges what it denies.

A popular legend has it that some Dutch names were meant to troll officious Napoleonic bureaucrats--*Naaktgeboren* (born naked), *Pannekoek* (pancake), or *Poepjes* (poop). Being born naked doesn't exactly distinguish one person from another if the point of names is to differentiate people! But *Naaktgeboren* may actually be derived from the German *Nachgeboren*, i.e. born after the father's death, and surnames, including these, were in long-standing informal use before officials started registering people in 1811.[15] Dutch, like English, includes names for body parts (Adem, Hooft, Spier, Voet – breath, head, muscle, foot), precious commodities (Baargeld, Diamant, Goud, Perel – cash, diamond, gold, pearl), and things to eat (Brood, Kaas, Mandel, and of course Pannekoek – bread, cheese, almond, and pancake). In some way, the ultimate origin of names remains mysterious, the semantic motivation an eternal blank.

8. Names Are Made of Networks and Letters (among Other Things)

In the case of given names, it is easier to pinpoint the moment of baptism, since it happens (with rare exceptions) once in a lifetime rather than over the generations, even if sometimes the motivation is unclear. (Everyone knows of childrens' names they find puzzling, and some celebrities recently seem competing to see who can give the weirdest baby name.) In the contemporary US, prospective parents collectively spend millions of hours annually considering potential baby names, and books of baby names are bestsellers. Choosing a name can be an excruciating process with underwhelming results due to familial, cultural, religious, class, and personal cross-pressures.[16] Parents want a good name, but shun names with the wrong vibe. A quick look at the US Social Security Database shows that names for boys remain fashionable over generations while names for girls do not. A boy named William could be born in the 1930s, 1960s, or 2010s, but a woman named Dorothy is all but guaranteed to be old. *Dorothy* was a top ten name from 1904 to 1939, but since then steadily dropped to a historic low in the 2000s, when for a few years it was not even in the top one thousand (it is staging a mild come-back and was ranked 601 in 2017). Over the past century in the United States, roughly 36 million boys and 15 million girls received a top-ten name.[17] This is a rough measure of the relative stability and restricted repertoire of top American boy names, which tend be pulled from the Bible (John, Michael, David) and British royalty (James, William, Richard), compared to girl names, which rarely carry across generations, and have much more diverse sources.[18] The single most popular girl name over the past century, Mary, would rank only number seven on the boy's list.

Interestingly, Dutch given names show more gender parity. My working hypothesis is that in the US the volatility of female surnames generalizes to female given names. Many American women change their surnames at marriage, whereas they traditionally keep them in the Dutch-speaking world. (Things are getting more complicated in both places in recent years.) American women suffer from a precarity or at least instability in naming that American men and Dutch-speaking women do not. Obviously this hypothesis requires more research. Certainly both language groups show a recent trend toward a much wider diversification of names, and top ten

names take up a successively smaller percent of the total for each. And there could well be strategic and other advantages of having names that change quickly.

When our oldest son was born early in 1980 my wife and I were taken by surprise and hadn't yet decided on a name. We liked the name Benjamin and chose it. When Ben went to kindergarten, he discovered several classmates also named Ben; the plethora of Bens continued throughout his school years. In our name choice we were riding a wave and didn't know it. In fact, *Benjamin* in that period was the number one "high-end" name likely to be chosen by white, middle-class, educated parents. [19] How did we know to pick a name so perfectly in harmony with our demographic? What was our invisible connection to the spirit of the age? Modern people like to imagine themselves free, rational, and autonomous of influence. We think ourselves sovereign, like Adam naming the animals, when we name our children. "To give a name is the sign of majesty" said Hegel. As I recollect more closely, we had loose ties with a young couple who also named their son Benjamin before we did, but we saw no conflict as they were soon moving off to a distant city. Even more, my uncle, a pediatrician who gave much assistance when Ben was in the hospital after being born, encouraged the name. A pediatrician is necessarily an expert in baby-name fashion, and my uncle served as an onomastic bee, pollinating new parents with naming ideas. The lesson of this personal vignette is that names are as irreducibly social as anything else and intensely embedded in networks. The choice of a name is an aspirational bid to belong to some groups and not to others.[20] What we think of as the aura or vibe of a name is actually the real and imagined social network it belongs to. Name-giving today may seem more free than the practice of automatically naming children after grandparents, but moderns are no less bound by social pressures!

The psychoanalyst Jacques Lacan liked to talk about the insistence of the letter in a way that could seem obsessively literal to unsympathetic minds,[21] but recent evidence suggests that initial letters do play a role in cooperation and perhaps even mate selection. A recent study on the "name-letter-effect in groups" to avoid methodological problems of letter-effect studies such as retroactive causation did group experiments, showing that sharing initials had a positive and contagious effect.[22] The subtitle of the study was "Sharing Initials with Group Members Increases the Quality of

Group Work"; the three authors were named Polman, Pollmann, and Poehlman. Imagine the glee this team took in publishing together on this topic! In a similar way, marketing research suggests that people are more likely to donate to victims of hurricanes if the storms share an initial letter with the donor. In the US, it might be better for charitable fundraising if all storms started with J or M, the two most common initials in given names.[23] Letters may or may not have any special meaning, but they can have effects. Though the processes of name-selection and letter-affection are mysterious, they are not simply arbitrary, but are shaped by social and psychological matters.

9. Names are Classes

Names can never have only one member. Through a combination of a names, we can possibly have a somewhat rigid designation; I use Durham as a middle name to honor my mother's side of the family but also to differentiate from the many other John Peters's out there. As a class, every name holds multiple members. With surnames the principle of expandable membership is explicit. They are designed to be capable of enlargement. They function to gather kin under one umbrella as new members arrive by birth, marriage, or adoption, and part by death, divorce, or name-change. But given names are also never unique: in fact, 228 babies were named "Unique" in the US in the 1990s.[24] Beyoncé and Jay-Z tried to trademark their baby daughter's name "Blue Ivy," to no avail. (It wasn't just ego, but an effort to brand a line of infant merchandise.) A Tennessee judge in 2013 changed a baby's name from Messiah to Martin, decreeing that only one person, Jesus, could carry that name.[25] But also to no avail: *Messiah* is a name rising in popularity and has been a given name for decades. Given names are dynamic classes as much as surnames. As noted, the ways names come in and out of fashion has much to do with the kinds of people who are bearing those names.

Just as there are no singletons in personal names, placenames also multiply obsessively. There are the countries Guinea, Guinea-Bissau, Equatorial Guinea, and Papua New Guinea, not to mention Guyana, which has nothing to do with the others, but adds to the confusion. There is Galicia in Spain and Poland, Gaul in France, Galatia in ancient Asia Minor, Wales in the

British Isles, Wallachia in Romania, and Wallonia in Belgium. Vlachs are foreigners in the Balkans, and Italy in Polish is Włochy. All these names probably – the etymology is disputed in some cases--descend from a common Roman exonym: they share in common the fact of being on the outskirts of empire. Even the Dutch word for gibberish, *koeterwaals*, preserves the Roman designation for otherness.

Why do placenames recycle if names are supposed to differentiate? In the United States, dozens of places are called Springfield, Franklin, and Lebanon. The name Washington adorns no less than 88 places: a state, a district, counties, cities, towns, not to mention its abundant use for universities, schools, and of course people. Why, if the purpose of names is to specify, would names be so obviously reused? Why do names almost purposefully seem to multiply? Why do fine differences play such an outsized role in naming practices?

In his monumental *Decline and Fall of the Roman Empire*, historian Edwin Gibbon mused on the single diphthong that separated Western and Eastern Christianity, the iota that distinguished the pivotal trinitarian terms *homoousion* (same substance) and *homoiousion* (like substance): "As it frequently happens, that the sounds and characters which approach the nearest to each other accidently represent the most opposite ideas ..."[26] He astutely observed the linguistic proximity of opposites, a phenomenon many have noted with regard to such technical terms as *hyper* and *hypo*, *inter* and *intra*, *super* and *infra*, *micro* and *macro*.[27] But was he right to think such proximity accidental? Is there a method to the madness? I think there is, for at least two reasons: local knowledge and the rule of the exception.

10. Names Require and Display Local Knowledge

Names are not intuitive. They cannot be assumed apriori. Sometimes parents say that their child just looked like a Benjamin (as ours did in fact) but how often can you go up to someone and say, you look like an Anna or a Bob or a Carla, and have any chance of being right? The odds are stacked against you. You can narrow down options, but there are no rules for a pinpoint prediction. Once you know a name, you can often predict gender, national origin, generation, even class, but not beforehand. Hindsight is 20-20. Knowledge goes in one irreversible direction only.

Names have to be learned. Language is not just a system of rules, but an embedment in a life-world of persons, things, practices, and places, and names serve as some of the points at which sign-systems stick most stubbornly to things. Language possesses an astonishing power of generativity: we instantly understand and produce new utterances that have never yet existed. To do so all we need is a good knowledge of the language. But to know names, we have to have paid the price of learning what they stick to. They require homework. You can't just generate them on the spot. Remembering peoples's names is flattering because it signals your willingness to expend time and energy on something that does not generalize. The collection of people whose names we know constitute a unique index of our personal history, our kith and kin, our networks, our belonging. Each person, at least in a complex society of many overlapping networks, perhaps has a completely unique set of people whose names they know.

Names are the tax that finitude levies on language. Once, while trying to make a point to a teenage audience as a guest lecturer in another city, I mixed up the names of two local high schools, Washington and Jefferson. In my defence, there could not possibly be two more generic names for American high schools, but I totally failed in my effort at relevance. For an outsider, it was an easy mistake to make, but never for locals. A few thousand people treasured a distinction utterly irrelevant to everyone else on earth. Communities, perhaps for deep-seated evolutionary reasons, flourish by excluding strangers. To navigate *anything* named Washington you need local knowledge. With names, you can't wing it. The multiplication of identical names means that successful use requires intimate acquaintance; it shuts out apriori guesswork, and keeps speakers grounded in the humiliating particulars of the empirical. Any domain of expertise will likely feature easily confusable terms whose crucial difference is opaque to outsiders, whether *meso* and *meta* or *Washington* and *Jefferson*. Knowing names signals that you have paid the price of membership. Names are impostor-detectors. The multiplication of identical names requires people to be on their toes and pay their local dues. This is one of many reasons why we recycle the same names for many objects. Naming is an economy that imposes local taxes.

11. Exceptions Make the Rule

All names are misnomers. They have to be. Names are sticky and survive far beyond the point of any descriptive relevance – there are old people named Young or big people named Klein. The semantic meaning – Frege's *Sinn*--of the name is perhaps necessarily irrelevant. The American athletic conference the Big Ten now includes 14 member universities; the Big Twelve includes 10.[28] Nobody is calling for a name change; to do so would, as William James said, be to throw ourselves out of an entire system of speech and fact. (It would also entail a massive loss of brand value.) Everyone knows the numbers in those names are not counters. The perversity of the name's semantic irrelevance underscores its function as a name. It almost proudly displays its status as a misfit. Names are not supposed to fit – that's what makes them names. If they fit too well, they'd risk being their object, not marking it.

The Dutch scholar Karel van het Reve proposed the conjecture: "A rule cannot exist if there is no exception against which it can distinguish itself."[29] There are irregularities in all kinds of systems. Reve thinks these irregularities are not accidents but rather essential. Why in most currencies is there a coin bigger in size than another coin that is higher in value? Nickels (worth five American cents) are bigger than dimes (worth ten cents) for the same reason that *hyper* is the opposite of *hypo* and *macro* is the opposite of *micro*: all systems produce gritty internal differences that require know-how. If we can see why dimes are smaller than nickels we will have figured out one of the key principles of naming: assymetry is a massive resource for meaning-making.

Take the system used in Finland for grading matriculation exams. Here are the Latin terms ranked from highest to lowest.[30]

> Laudatur (praised)
> Eximia cum magna laude approbatur (approved with exceedingly great praise)
> Magna cum laude approbatur (approved with great praise)
> Cum laude approbatur (approved with praise)
> Lubenter approbatur (eagerly approved)
> Approbatur (approved)
> Improbatur (not approved)

At first glance, being approved with exceedingly great praise would seem to outshine being praised. The highest end of a complex ranking system can't just increase step-wise but must go in a different direction, even at risk of apparent backtracking. Millionaries wear suits; billionaries wear hoodies. If you didn't know the rules, you might think Donald Trump richer than Mark Zuckerberg. A bend in the system marks power at the top. Ludwig Wittgenstein's father grandiosely refused the ennobling prefix *von*, considering it a mere redundancy that confirmed an already established fact. But hiccups in the Finnish system occur not only at the top. *Eagerly approved* would also seem to outrank *approved with praise*, but not so: here the nickel is bigger than the dime. (*Eagerly* might also imply that it is given with more mercy than merit.) A quantitatively even progression in rank is hard to keep hold of in everyday use without some markers. (Compare the tennis scoring system of 15-30-40.) The third item might seem to outrank the fourth, and the sixth than the seventh, but not to a competent user. The irregularities reveal how the system operates.

Irregularities in commonly used items allow for practical differentiation. In English, the most common verbs are the most irregular. Such local-level differences as *I am, you are, he is, I was, you were, I have been* would be superfluous in rarer verbs. Counting terms are likewise irregular for small numbers: *one two three, first second third, solo duet trio*, etc. but there is no need for such marking among numbers in the teens and twenties, let alone hundreds or thousands. Asymmetry in linguistic form seems proportional to frequency of use. A big-data study of the historical tendency of strong (irregular) verbs (sing/sang/sung) to morph into weak (regular) ones (walk/walked/walked) in English shows that a verb used 100 times more often will regularize 10 times more slowly. Common usage maintains and benefits from functional irregularity.[31] Names are among the most frequently used terms, and their apparently capricious, perverse or empty meanings play a role in their function. The formal irregularity of frequently used words is analogous to the semantic irregularity of names. To know the Big Ten has 14 universities or that *laudatur* is better than *eximia cum laude approbatur*, you need to be a fluent user of the system.

12. The Semantic Repressed Returns

It seems in bad taste to make fun of someone's name. There has never been a name that is not capable of being abused. My given name John, probably the most generic male name possible in English, means in slang both a toilet and a client of a prostitute. Peters is slang (as is Johnson) for penis. No wonder I use Durham! But my case is not unique: no name is safe from excess meaning or abuse.

The term euonym (good name) is often used for a surname that matches a person's life, most often their career. Some examples (given name included only where relevant):

- Diner, historian of American food
- Ditto, Trump's deputy communications director
- Grab, rock-balancing artist
- Hooker, sex researcher
- Horney, psychoanalyst
- Chuck Long, American Football Quarterback
- Prose, essayist
- Smart, philosopher
- Speakes, Ronald Reagan's spokesman (textbook case)
- Wagoner, ex-CEO of General Motors
- Webb, director of *The Amazing Spiderman*
- Wisdom, philosopher

In reading these as euonyms, a proper name slowly blossoms into a part of speech. It can take a while to get the joke, as each name loses its prohibition on meaningfulness. We are tempted to say that euonym hunters take names (too?) literally, but they are simply lifting the curtain from the infrastructure.

Euonyms show us that names, to function as names, are semantically neutralized. They are ritually set apart from ordinary use. "What is your numb?"[32] This question from James Joyce's *Finnegans Wake* nicely expresses the anaesthetization of names. What the empiricist tradition sees as a path to sanity and clarity is actually one half of how names work: semiotic resonance is suspended in favor of tagging. But what the empiricist tradition does not always see is the other side: the always lurking absurdity of names is part of how they work. Without the semantic mismatch, it

wouldn't be a name, it would just be a description. Names cannot be their objects. The noncorrespondence of names enables their status as names. The semantic misfit calls attention to their labeling function. It numbs them into use as pointers.

The sciences counter the alarmingly sticky excess of names by a love of supposedly empty names. The algebraic x is the chief example.[33] A variable is necessarily and deliberately obscure. Science loves names for unknowns or ineffables. We have S-waves and T-waves, alpha, beta, and gamma rays, T-tests and Chi squares, Alpha-males, beta-versions, and many other uses for Greek letters. Cronbach's alpha and Pearson's rho are intermediate forms between eponymy and letters. Eponymy, the practice of naming a phenomenon after its putative discoverer, has a distinguished history in scientific nomenclature. Diseases, mathematical theorems, and chess openings are all named after long-dead worthies. Eponymy was studied by Edwin G. Boring, a psychologist whose surname perhaps provided him with extra motivation to think about the *omen* in *nomen*, and Stephen M. Stigler. The latter famously coined what he called Stigler's law of eponymy: No discovery is named after its first discoverer. For Stigler's law to be self-confirming, he notes that it was discovered by Robert K. Merton.[34]

There is much that could be said about this law, but I will conclude with this one thought: the name comes second. Stigler's law underscores the privilege of the second, the doubleness of identity. *Je est un autre*, wrote Rimbaud, and we experience this everytime we use our names, though we are usually numb to it. The contrast between common and proper names is ideological before it is grammatical. Its job is to provide metaphysical comfort against the frightening thought that names are both their objects and not their objects. The way we know a name is not a person or an object is by the patent wrongness, even absurdity, that is often before us. I am John, but I am not John also. That label is me, and is also not me. It is many people. Nothing is as dialectical as a name. It is the thing it stands for, but it is not also not the thing it stands for. A name has to have enough of semantic mismatch to remind us that it is a name, and not the thing. It has to have enough semantic bite to be irrelevant for purposes of sense.

But most of this work happens infrastructurally, environmentally, blindly. If we really thought about all the work that names do, they would not be

names; they would become little poems, jokes, absurdities, little bursts of semantic meaning that is both familiar and alien. They have to keep their work hidden, as infrastructural media do. In thinking about names, we delve into the delicious mysteries of how we use one very peculiar medium to make sense of ourselves, each other, and the world.

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[14] Nicholas Wade, *Before the Dawn* (New York: Penguin, 2007), 237ff.

[15] See Leendert Brouwer, "De naammythe van Napoleon" (2006), http://www.naamkunde.net/?page_id=162.

[16] Lauren Collins, "Notes from a Baby-Names Obsessive," *New Yorker* 7-14 August 2017, https://www.newyorker.com/magazine/2017/08/07/notes-from-a-baby-names-obsessive.

[17] https://www.ssa.gov/oact/babynames/decades/century.html.

[18] James is technically not a biblical name, though it has become one. It was substituted for Jacob in the translation of the New Testament book we now call James during (yes) the King James Translation.

[19] Steven D. Levitt and Stephen J. Dubner, *Freakonomics* (New York: Harper, 2009), 192-3.

[20] This is the richly exemplified thesis of Levitt and Dubner.

[21] Jacques Lacan, "L'instance de la lettre dans l'inconscient, ou, la raison depuis Freud" (1957), http://aejcpp.free.fr/lacan/1957-05-09.htm.

[22] Evan Polman, Monique M. H. Pollmann, T. Andrew Poehlman, "The Name-Letter Effect in Groups: Sharing Initials With Group Members Increases the Quality of Group Work," PLOS one (13 November 2013), http://journals.plos.org/plosone/article?id=10.1371/journal.pone.0079039.

[23] Adam Alter, *Drunk Tank Pink* (New York: Penguin, 2013), 14-16.

[24] Levitt and Dubner, *Freakonomics*, 168.

[25] https://www.yahoo.com/news/tenn-judge-changes-infants-name-messiah-215423538.html?guccounter=1.

[26] Edward Gibbon, *The History of the Decline and Fall of the Roman Empire* (1776; New York: Harpers, 1826), vol. 2, 250.

[27] Compare Freud's 1910 essay, "The Antithetical Meanings of Primal Words."

[28] Another sports example is the NBA teams the Los Angeles Lakers and the Utah Jazz. No one worries that the team names do not fit their adopted hosts though the names were chosen for their original homes in Minnesota and New Orleans.

[29] Karel van het Reve, "Reves vermoeden," *Afscheid van Leiden* (van Oorschot, 1985), and Slavoj Žižek, *The Ticklish Subject* (London: Verso, 1999), 99.

[30] https://en.wikipedia.org/wiki/Academic_grading_in_Finland.

[31] W. Tecumseh Fitch, "An invisible hand," *Nature* 449 (11 October 2007): 665-666. Fitch is discussing an article by Lieberman et al. published in the same issue, 713ff.

[32] James Joyce, *Finnegans Wake* (1939) http://www.trentu.ca/faculty/jjoyce/fw-546.htm.

[33] Mary Louise Pratt, "Xploits," *Tabloid: A Review of Mass Culture and Everyday Life* 1:3 (1981): 10-11, shows how X is anything but a blank, but laden with semiotic richness.

[34] Stephen M. Stigler, "Stigler's Law of Eponymy," *Transactions of the New York Academy of Sciences* 39 (1980): 147-157.