SARTONIANA

Volume 32
2019

Sarton Chair of History of Science
Ghent University

SARTONIANA

Volume 32

2019

Editors: Robert Rubens and Maarten Van Dyck

Sarton Chair of History of Sciences
Ghent University, Belgium

Sartoniana, Gent, Belgium
Ghent University

ISBN 9789492944887
D/2019/3988/57

Layout en druk:
www.mirto.be
Mirto Print cvba-so Drongen

Table of Contents

Authors

Prof. dr. Robert Rubens
Chairman Sarton Committee, University Hospital Ghent, Department of Endocrinology, C. Heymanslaan 10, B-9000 Gent, Belgium
robert.rubens@UGent.be

Prof. dr. Maarten Van Dyck
Secretary Sarton Committee, Ghent University, Centre for History of Science, St.-Hubertusstraat 2, B-9000 Gent, Belgium
maarten.vandyck@UGent.be

Prof. dr. Steven Vanden Broecke
Ghent University, Faculty of Arts and Philosophy, Department of History, Blandijnberg 2, B-9000 Gent, Belgium
steven.vandenbroecke@UGent.be

Prof. dr. Robert S. Westman
Department of History/Science Studies Program, University of California, San Diego, USA
rwestman@ucsd.edu

Prof. dr. Dirk Heirbaut
Ghent University, Faculty of Law and Criminology, Department of Interdisciplinairy Study of Law, Private Law and Business Law, Universiteitsstraat 4, B-9000 Gent, Belgium
dirk.heirbaut@UGent.be

Prof. dr. Paul Brand
Oxford University, Emeritus Fellow All Souls College, English Legal History, Oxford OX1 4AL (U.K.)
paul.brand@all-souls.ox.ac.uk

Prof. dr. Gert de Cooman
Ghent University, Faculty of Engineering and Architecture, (ELIS–IDLab) — Durham University (Department of Mathematical Sciences), Technologiepark-Zwijnaarde 914, B-9052 Zwijnaarde, Belgium
gert.decooman@UGent.be

Prof. dr. Glenn Shafer
Rutgers Business School-Newark and New Brunswick, 1 Washington Park, Newark, New Jersey 07102, USA
gshafer@business.rutgers.edu

Dr. Marjan Doom
Ghent University, Ghent University Museum, Krijgslaan 281, Campus Sterre, S30, B-9000 Gent, Belgium
marjan.doom@UGent.be

Prof. dr. Gita Deneckere
Ghent University, Dean of the Faculty of Arts and Philosophy, Department of History, Blandijnberg 2, B-9000 Gent, Belgium
gita.deneckere@UGent.be

Prof. dr. Raf Vanderstraeten
Ghent University, Faculty of Political and Social Sciences, Department of Sociology, Universiteitsstraat 8, B-9000 Gent, Belgium
raf.vanderstraeten@UGent.be

Prof. dr. Pauli Kettunen
Professor of Political History, Faculty of Social Sciences, University of Helsinki, P.O. Box 54, Snellmaninkatu 14 A, FIN-00014
pauli.kettunen@helsinki.fi

Prof. dr. Glenn Rayp
Ghent University, Faculty of Economics and Business Administration, Department of Economics, Tweekerkenstraat 2, B-9000 Gent, Belgium
glenn.rayp@UGent.be

Prof. dr. Claude Diebolt
Université de Strasbourg, Faculté des Sciences Economiques et de Gestion, 61 Avenue de la Forêt Noire, 67085 Strasbourg Cedex, France
cdiebolt@unistra.fr

Prof. dr. Christian Burvenich
Ghent University, Faculty of Veterinary Medicine, Department of Nutrition, Genetics and Ethology, Salisburylaan 133, B-9820 Merelbeke, Belgium
christian.burvenich@UGent.be

Prof. dr. Peter Bols
University of Antwerp, Faculty of Pharmaceutical, Biomedical and Veterinary Sciences, Gamete Research Center, Campus Drie Eiken, Universiteitsplein 1, B-2610 Wilrijk, Belgium
peter.bols@uantwerpen.be

Prof. dr. Marc Van Meirvenne
Ghent University, Dean of the Faculty of Bioscience Engineering, Campus Coupure A, Coupure 653, B-9000 Gent, Belgium
marc.vanmeirvenne@UGent.be

Prof. dr. Erik Thoen
Ghent University, Faculty of Arts and Philosophy, Department of History, Universitair Forum, St.-Pietersnieuwstraat 35, B-9000 Gent, Belgium
erik.thoen@UGent.be

Prof. dr. Matthieu Lenoir
Ghent University, Faculty of Medicine and Health Sciences, Department of Movement and Sports Sciences, Watersportlaan 2, B-9000 Gent, Belgium
matthieu.lenoir@UGent.be

Prof. dr. Roland Renson
KU Leuven, Faculty of Kinesiology and Rehabilitation Sciences
Private: Grezstraat 6, B-3054 Oud-Heverlee, Belgium
roland.renson@kuleuven.be

Introduction

R. Rubens

In 1984 the University of Ghent started the Sarton Chair for the history of sciences, remembering its alumnus. Since that date yearly a volume of Sartoniana is dedicated to the lectures of the chair holder and the medallists. Respecting the original idea of G.Sarton the medallists are scattered over the faculties of the university.

Dr. Westman, the Chair holder, gave two lectures, one devoted to the Kepler-Fludd polemic, another one to the ideas of Salk, the discoverer of the polio vaccination.

The first paper gives a detailed analysis of the relation between Wolfgang Pauli, the famous physicist and the psychanalysts Rosenbaum and Carl Gustav Jung reflecting from the important paper by Pauli about the polemic between Kepler and Fludd in the XVI-XVII century.

The second paper analyses thoroughly the genesis and growth of the Salk Institute. The latter represents a new experiment to perform the basic idea of Sarton of reconciling exact science with philosophy.

The lecture of Paul Brand from Oxford gives a vivid account of the birth of English Common Law. The historical background with the Eyre and the structuring of the assisses explains the peculiar evolution of the English judicial system, so different from the European continental habits. Each lawyer planning to study English Common Law should be obliged to read it as basis to his knowledge.

The beautiful outline of Schafer about probability, stakes and methods of calculation of those, is very informing for the incoming statistician. The birth of these disciplines now so important for our life and the exact sciences is due to the work of Pascal and Huyghens.

The biographical notice about G.Sarton and the founding of the science museum in our university By G. Deneckere nicely highlights the proposed new university museum in Ghent. It furthermore explains a lot about the rationale of science museums in academic institutes.

The report about the history of the Scandinavian model of modernising the nation state has inspired numerous researchers in sociology. The new form of society enhancement combined with its new organisations, such as the ILO, is the basis of the study of Dr. Kettunen. The internationalism of the ILO was however no longer the standard model in the work of Myrdal. An analysis of his ideas is developed in the second part of this paper.

Diebolt and Haupert have submitted a paper concerning the need for economic history as a research tool. As founding fathers of cliometrics both authors stress the importance of the inclusion of the historical viewpoint in economic research and training. Some recent popular authors, as Piketty and important thinkers in the field, Keynes and Schumpeter also stress the importance of economic history.

The paper by P.Bols gives a nice overview of the development of veterinary medicine. The evolution from hippiatrist to scientific veterinary surgeon got an enormous impetus due to the work and books of Bourgehat in the latter part of the eighteenth century.

Erik Thoen amply proves the importance of multidisciplinary work. His detailed study about rural economy and landscape in pre-industrial Flanders is an important contribution to a better understanding of the nowadays planification problems of Flanders. The analysis of the social and historical aspects combined with the geography of the region explains the patchwork organisation of the rural countryside in Flanders.

The discussion about the Halma (or long jump in Greek) by Renson quotes a long-standing problem in athletics. It is a major technical problem to explain the long distance quoted in the Greek antique texts together with the details about the carrying of the halters.

The editors also wish to thank the members of the Sarton Committee, who reviewed the manuscripts and provided peer review for those papers. Without however the painstaking follow-up by Mrs.Lievens-Malfliet, who

volunteers as a secretary to the Sarton Committee during more than a decade the lectures and Sartoniana would never be the same.

We hope the new number of Sartoniana may again interest everybody researching in the history of science.

Laudatio Robert S. Westman

Steven Vanden Broecke

Unlike fast food, televisions or presidential ego's, there is something to be said for supersizing History. In only a few pages, David Christian's "big history of everything" explains how Columbus brought discovery, discovery brought modern science, science brought technological power, and technology brought progress. "By the 18th century," Christian writes:

> "(…) educated Europeans (…) thought of Newton as the greatest of scientists; they knew Earth orbited the sun; they did not take magic, the histories recounted in ancient legends, the stories of unicorns, or (most) stories of miracles seriously; they believed in the advancement of knowledge and something like progress".[1]

The universal is personal, of course. Far more than offering big history, narratives such as these seek to answer big questions: "Who am I? Where do I belong? What is the totality of which I am a part?".[2] Interestingly, the aforementioned passage also illustrates the immense role assigned to the history of science in answering these questions – especially the story that leads from Copernicus to Newton – by the Origin Stories that most of us live by.

For almost 50 years, Bob Westman has continually revisited the foundation myth that leads from Copernicus, over Newton, to ourselves. It is fair to say that his work on this topic is unparalleled in continuing to change how we

1 David Christian, *Origin Story: A Big History of Everything* (London: Penguin Random House, 2019), p. 205.
2 David Christian, *Maps of Time. An Introduction to Big History* (Berkeley CA: University of California Press, 2004), p. 1.

understand the shared story of the birth of modern science. In his recent and monumental *The Copernican Question* (2011), Bob Westman describes how we traditionally understand this shared story:

> "The narrative of the "Copernican Revolution" is organized around discovery, diffusion, reception, and assimilation. Theoretical illumination or breakthrough provides the narrative center; the subsequent epistemic history charts theoretical amplification, empirical verification, and sometimes obdurate resistance to truth".[3]

Notice the extent to which Big History rests on this traditional narrative. Science is like a suddenly lit beacon, whose illumination you either 'get' and embrace, or 'don't get' and resist. If you don't get it, that simply means you are not of the "educated", behind the times, and probably dabbling in magic, unicorns and miracles instead. The story has a sense of necessity and inevitability about it that, on closer inspection, is far from surprising, since the outcome of the story provides the measure for deciding who gets a part in the first place. Paradoxically, history of science thus becomes the story of how science gradually extracts itself from the morass of history at the hands of semi-historical geniuses working in comfortably appointed centres of excellence. The story tells us, then, that science may come forth *in* history, but is not truly *historical.* Frankly, it rewrites history in accordance with what makes us feel good about ourselves.

This is precisely what Bob Westman has not done, and what he has taught several generations of historians not to do. In painstaking detail, he has demonstrated that there is nothing inevitable or superhuman about the story that led from Copernicus to Newton.

In Bob Westman's able hands, local questions, not universalizing flashes of insight, drive the story of Copernicanism. Scientist's answers to these questions are not simply distributed and accepted, but received and reconfigured in accordance with local needs and sensibilities.[4] The towering geniuses that populate our traditional narratives often become less decisive than a multitude of obscure characters barely flying above the historian's

3 Robert Westman, *The Copernican Question. Prognostication, Scepticism, and Celestial Order* (Berkeley CA: The University of California Press, 2011), p. 3b.

4 Robert S. Westman, ed. *The Copernican Achievement* (Berkeley and Los Angeles: University of California Press, 1975).

radar.[5] Circulating manuscripts, copied notes, and scholarly correspondence become a crucial site of exchange, often more so than the world of printed books and scholarly articles. Psychological and generational dynamics, codes of conduct and social distinction, shared literary cultures shape the course of science at least as much as intra-disciplinary norms and standards.[6] Different sites of scientific labour, like papal courts, princely courts, and universities, become entangled to such an extent that the very meaning of being a scientist changes beyond recognition.[7] In other words, humans become the agents who make science, not merely discovering it. The shared story of the rise of modern science becomes profoundly human and historical after all.[8]

As a result, Bob Westman's work has had a tremendous influence on how we understand and practice the history of science. It would be difficult to pick up *any* study in the history of early modern science that, directly or indirectly, is not informed by the interpretations, approaches and methodologies that Bob Westman often pioneered. A paper or book by Bob Westman is invariably the result of long and meticulous research; is not afraid to experiment with new and interdisciplinary interpretive options; and has the rare distinction of frequently becoming an instant classic.

Yet perhaps you are still wondering: "Why should we care?" Allow me to tell you a little more about the strange beast we call "historian". Historians have three faces. One face is that of the public storyteller. In this guise, the historian is the modern successor to the epic poet of times past, providing entire societies with an essential sense of origin, identity and direction. The second face is that of the myth buster. In this guise, society entrusts the historian with the difficult task of maintaining a difference between histories and stories, between reality and fiction. The third face of the historian is that of the heterologist. Here, the historian does not construct big histories or empires of knowledge. Instead, he uses history to seek out the limits of what we consider to be 'rational' or 'common sense'. In this guise, the

---

5 Robert Westman & Owen Gingerich,*The Wittich Connection: Conflict and Priority in Sixteenth Century Cosmology* (Philadelphia: The American Philosophical Society, 1988).

6 Robert Westman & David C. Lindberg, eds. *Reappraisals of the Scientific Revolution* (Cambridge: Cambridge University Press, 1990).

7 Robert S. Westman, *Copernicus and the Astrologers* (Washington, D.C.: Smithsonian Libraries, 2016).

8 Robert S. Westman & David Biale, eds. *Thinking Impossibilities: The Legacy of Amos Funkenstein* (Toronto: Toronto University Press, 2008).

historian opens up a future by exploring how the past has shaped his own preconceptions. He engages us in a constant invitation to let strangers have the last word, even if they are dead and gone.

My initial example of Big History illustrates how most historians have a clear preference for only one of these faces. Nevertheless, each offers something to society that is as indispensable as the air we breathe. Very few historians are capable of playing each of these three parts, let alone combining them in a single oeuvre of historical narration, carefully worked out over the course of 50 years (and counting). Bob Westman is such a historian. He has set an example of what history of science is capable of, endlessly reinventing his work, pushing against the boundaries of what we thought possible. On behalf of the history of science community at Ghent University, I would like to thank Bob Westman for setting such an inspiring example of what it can mean to be a historian of science, and the Sarton Committee for recognizing this by awarding him the Sarton Chair for the academic year 2018-9.

Carl Gustav Jung, Wolfgang Pauli and the Kepler-Fludd Polemic: Either/Or vs. Both/And

Robert S. Westman

Is there a legitimate place for psychological explanation in writing the history of science? And if so, what would such histories look like? Because there are many psychologies, ranging from the experimental to the hermeneutic, what kinds of descriptive and explanatory resources might be taken to be relevant to this question? This paper takes a *historical* – rather than a global – approach to these questions. It examines the case of a famous physicist who suffered deep emotional distress in his life and who later, as part of his own continuing struggle to deal with his personal pain, composed a brief, but influential study of an episode in the history of early modern science.

My interest in this episode was aroused over thirty years ago on the occasion of Brian Vickers' invitation to contribute to a conference in Zürich on the theme of "Occult and Scientific Mentalities in the Renaissance."[1] Having worked for many years on various aspects of Kepler's philosophical and scientific thought, I immediately thought of an essay by the physicist Wolfgang Pauli (1900-1958), entitled "The Influence of Archetypal Ideas on the Scientific Theories of Kepler" – first published in German in 1952 and translated into English in 1955. Pauli's essay analyzed a sharp

1 Robert S. Westman, "Nature, Art, and Psyche: Jung, Pauli, and the Kepler-Fludd Polemic," in ed. Brian Vickers, *Occult and Scientific Mentalities in the Renaissance* (Cambridge: Cambridge Univ. Press, 1984), pp. 177-229.

polemic between the astronomer Johannes Kepler (1571-1630) and the polymathic English physician Robert Fludd (1574-1637), the latter usually characterized as an alchemist and a follower of the mystical Rosicrucians.[2] To my knowledge, Pauli's study was the first extensive treatment of this contentious episode. Much of the controversy centered around the sort of reality that geometric forms possess in relation to the world and what kind of knowledge those forms might allow. Did geometry and arithmetic serve a purely symbolic function, standing for certain spiritual or mystical qualities, or did the meaning of mathematics lie exclusively in its capacity to quantify natural entities, thereby enabling measurement of particular features of the physical world? This controversy over the epistemological status of visual images occurred between 1618 and 1622 when Europe was saturated with religious disputes; and thus, it is not altogether surprising that Kepler and Fludd's natural philosophies and the style of their dispute drew upon richly-articulated resources of religious controversy.[3]

More specifically, Kepler had worked out a finely-articulated theological metaphysics that infused his natural philosophy and which, in turn, informed his theories of celestial order. He believed that God made himself visible to human perception in the natural world through the mystery of the Holy Trinity. And the Trinity, so he claimed, was made visible to the human intellect through the geometry of the sphere. He then hit on the idea that each of the five perfect polyhedra, (cube, octahedron, dodecahedron, tetrahedron, icosahedron) could be inscribed within a sphere and the spheres then nested together following the Copernican order such that the radii of the spheres from the sun corresponded – just about – to the actual values of the planetary distances (at least as available to Kepler). It was not a perfect correspondence, but it was close enough to excite and motivate him for the rest of his life. [Figure 1]

---

2 A general method for addressing the question of the relevant authorial categories for early modern historical agents is to ask how an author, such as Fludd, described himself in his own publications. Fludd used the print identity, "Oxford Doctor of Medicine and Esquire [*Armigerus*]."

3 For a valuable study of religious controversies in early seventeenth century England, see Peter Milward, *Religious Controversies of the Jacobean Age: A Survey of Printed Sources*, London: The Scolar Press, 1978.

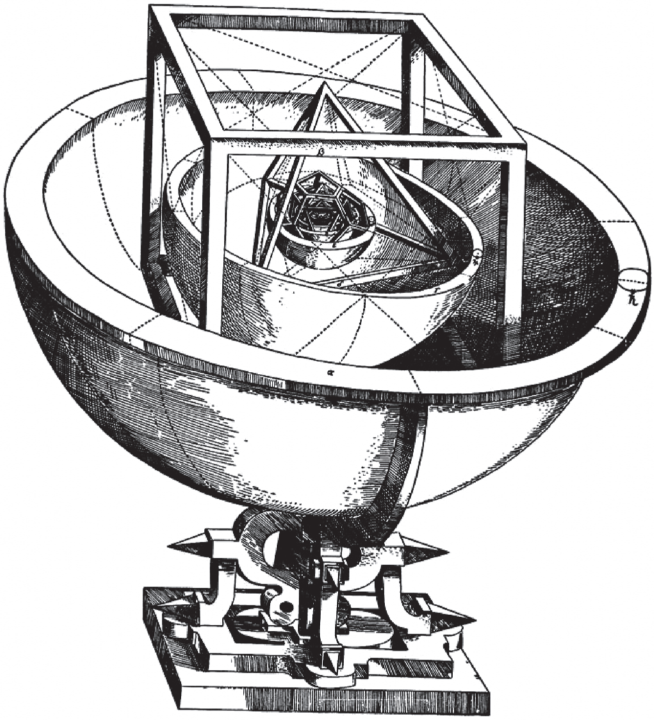


Fig. 1: Johannes Kepler: Polyhedral Hypothesis 1596

Fludd's cosmic theology, on the other hand, may be thought of as a symbol-picture of the divine creation, a visual commentary on the book of Genesis using geometrical shapes that lack any measurable parameters corresponding to the natural world. In one illustration, Fludd displayed Light and Dark as interpenetrating pyramids. [Figure 2] The upper triangle was said to represent the divine light conveying its spirit to the sun; the moving sun, the heart of the world giving life to the earth through its rays. The lower triangle represented darkness. The divine light was alleged to create by giving form to the darkness. Using standard Aristotelian categories, Fludd then divided the created universe into celestial and terrestrial regions, the macrocosm and microcosm. [Figure 3]

Developing his argument by means of this visual epistemology, he located man at the center of the universe. He foregrounded the body of a male figure against the backdrop of spheres consisting of the four Galenic

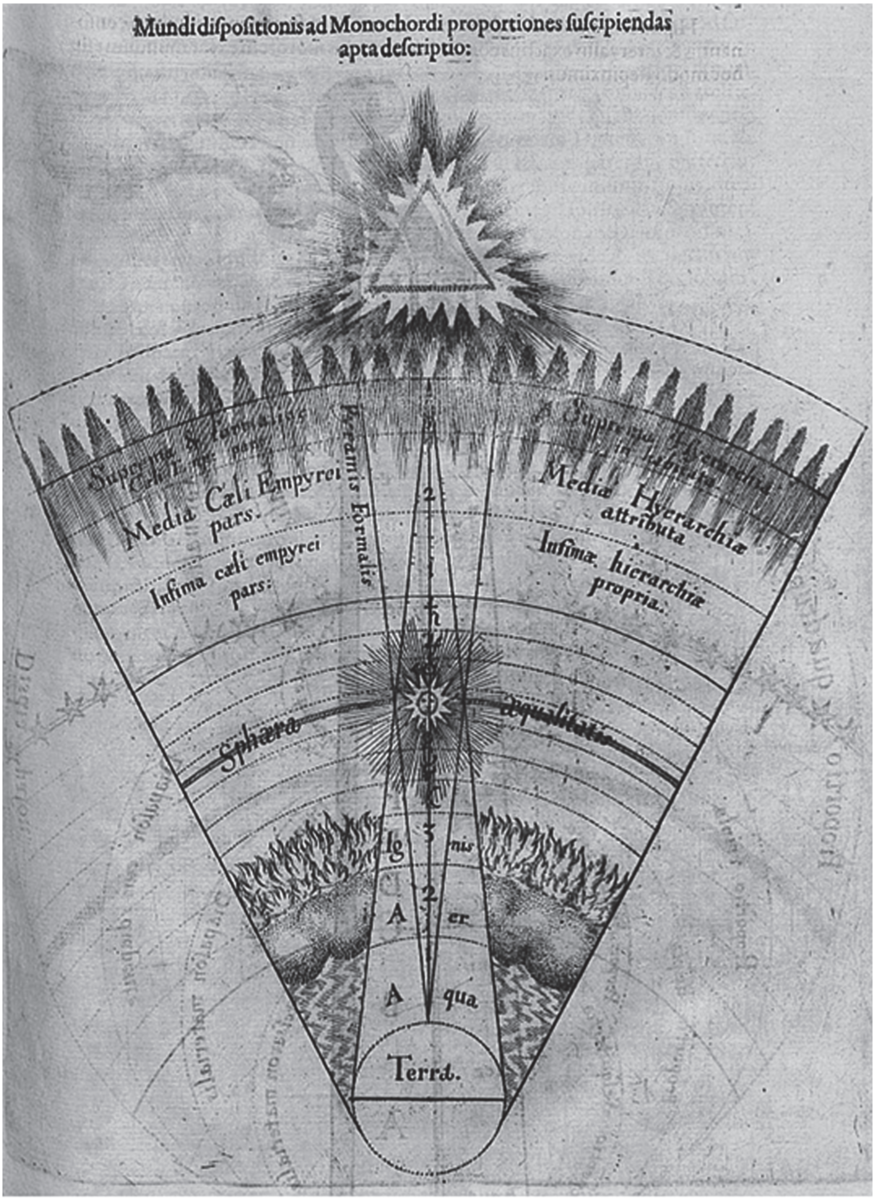


Fig. 2: Robert Fludd. Interpenetrating pyramids of Light and Darkness.
Utriusque Cosmi Historia 1617-1621

Fig. 3: Robert Fludd Microcosmic Man mirrors macrocosm.
Utriusque cosmi historia (1617-1621)

humors (blood, black bile, yellow bile, phlegm), the body's parts subject to various astral influences. In another, even more complex image, man appears as an imitator – the "ape of nature," linked by a golden chain to the soul of nature (the female figure) and, in turn, the soul linked to the hand of God. [Figure 4]

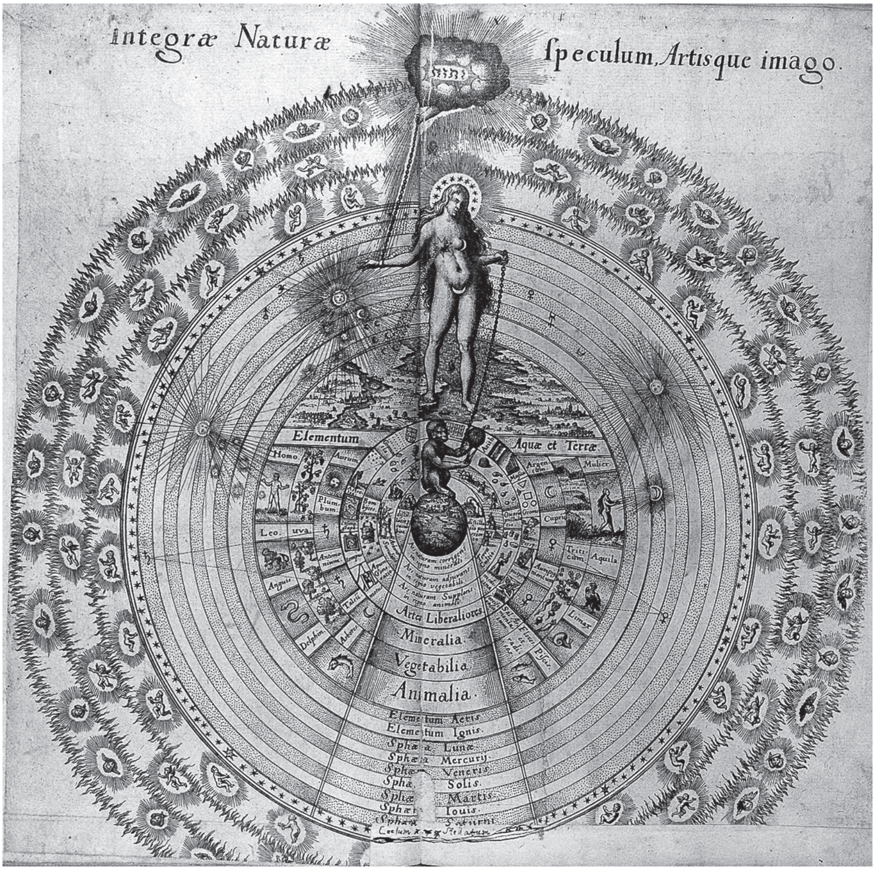


Fig. 4: Robert Fludd The Ape of Nature?
Utriusque Cosmi Historia (1617-1621)

Guided by the metaphor of the mirror, the scheme purported to represent how man should be led to knowledge of God first by learning the traditional seven liberal arts and then by mastering the practical arts, such as time-keeping, fortification, statics, perspective and painting – the latter subjects not usually included in the academic curriculum. Fludd regarded his engravings not merely as illustrations but as ways of knowing, demonstrating and remembering.[4]

In 1618, Kepler had just concluded his book on the harmony of the world, *Harmonice mundi*, when at the Frankfurt Book Fair he came across the first volume of Fludd's *Utriusque cosmi maioris scilicet et minoris metaphysica physica atque technica historia* (*Metaphysical, Physical and also Technical History of Both the Macro- and the Micro-Cosmos*) – the book that contained the images described above. The surface similarity between Kepler's and Fludd's approaches to harmony provoked Kepler to add a very short appendix to his own work to distinguish his own study from that of Fludd.[5] And it was this appendix that kicked off a heated controversy. But ten years earlier, in 1608, Kepler had already stated his position quite succinctly and eloquently in a private letter:

> "I too play with symbols and have planned a little work, Geometric Cabbalah, which is about the Ideas of natural things in geometry; but I play in such a way that I do not forget that I am playing. For nothing is proved by symbols; things already known are merely fitted [to them]; unless by sure reasons it can be demonstrated that they are not merely symbolic but are descriptions of the ways in which the two things are connected and of the causes of these connections."[6]

For his own part, Fludd's exchange with Kepler was only the first of three extensive controversies in which he engaged. Until his death in 1637, he showed a great affinity for ostentatiously defending his ideas, engaging in further debates with the French priest-philosophers Marin Mersenne and Pierre Gassendi – debates that Pauli did not mention in his essay. With his profuse and compelling images one might imagine that historians could easily cast Fludd in the role of the backward-looking "last man of the Renaissance" with Kepler as the representative of the forward-looking New Science. But this is not exactly what happened.

---

4 See Christoph Lüthy, "What Does a Diagram Prove that Other Images Do Not? Images and Imagination in the Kepler-Fludd Controversy," in eds. Christoph Lüthy, Claudia Swan, Paul Bakker, Claus Zittel, *Image, Imagination, and Cognition* (Leiden: Brill, 2018), pp. 227-274: Westman, "Nature, Art and Psyche," p. 181.

5 Johannes Kepler, *The Harmony of the World*, trans. E. J. Aiton, A. M. Duncan and J. V. Field (Philadelphia: American Philosophical Society, 1997), pp. 503-508.

6 Johannes Kepler, *Gesammelte Werke*, eds. Max Caspar et. al. (Munich, 1937-), *16*, p. 158 (hereafter cited as *GW*); quoted in "Nature, Art and Psyche," p. 205.

Pauli's essay of 1952 became quite well known and was often cited by historians of science for the next thirty years or so, during the era when "The Scientific Revolution" was the history of science profession's central narrative. Allen Debus, for example, published numerous studies on Fludd, making it his mission to show that Fludd was a figure of intellectual substance and that chemistry and alchemy deserved to be a significant part of the general narrative, if not also to undermine the very notion that the Scientific Revolution was a revolution in physics alone.[7] The Harvard physicist-historian Gerald Holton, an important contributor to the history of science and later president of the History of Science Society, highly valued Pauli's article in a seminal paper "Kepler's Universe: Its Physics and Metaphysics."[8] In 1964, at the Warburg Institute in London, Frances Yates – also citing Pauli – found in Fludd the perfect representative of what she called the "Hermetic Tradition" and emphasized that Fludd's diagrams functioned as both memory aids and as symbols.[9] Some later historians indirectly absorbed Pauli's historical conclusions through Holton's and Yates' studies. For example, without citing Pauli, Carolyn Merchant used Fludd's diagram of the macrocosm/microcosm [see figure 4] to substantiate her claim that Renaissance Neoplatonic philosophers believed that the world possessed a soul represented by a female figure.[10]

Now, it had always struck me as strange that no one mentioned that Pauli's essay appeared in a small volume together with an essay by Carl Gustav Jung, entitled: "Synchronicity: An Acausal Connecting Principle" and that their little joint production appeared under the auspices of the C.G. Jung Institute in Zürich. Thus, when I discovered that Brian Vickers' conference was to be held at the institution where both Jung and Pauli had once taught – the Swiss Federal Institute (or ETH) – and in the very city where Jung's Institute was located, the topic seemed exactly right for the occasion. Nonetheless, although there was clearly some sort of intellectual association

7 *The Chemical Philosophy: Paracelsian Science and Medicine in the Sixteenth and Seventeenth Centuries*, 2 vols. (New York, 1977), I, pp. 256-260.

8 "Johannes Kepler's Universe: Its Physics and Metaphysics," *American Journal of Physics*, 1956, *24*: 340-351.

9 Frances Yates, *Giordano Bruno and the Hermetic Tradition* (London: Routledge and Kegan Paul, 1964), p. 440, note 3.

10 Carolyn Merchant, *The Death of Nature* (New York: Harper & Row, 1980), pp. 11-12.

between Jung and Pauli, and although I had long suspected that there was some kind of deeper personal connection, it was not until I returned to Los Angeles that another piece of the puzzle dropped into place.

I was a member of a small group of faculty at UCLA who regularly met to discuss the history and philosophy of psychoanalysis. One of the members of that group was my colleague, Peter Loewenberg. When Loewenberg informed me that he was planning a trip to the Jung Institute, I discussed my project with him and asked him to find out what he could about the Jung-Pauli relationship. It was in this manner that Loewenberg learned from Jung's successor, Carl Alfred Meier, that Pauli had been in treatment with one of Jung's pupils, a young therapist named Erna Rosenbaum. Loewenberg also learned from Aniela Jaffé, Jung's former secretary and biographer, that a large group of mandala dreams published by Jung were those of Wolfgang Pauli. The Indian mandala is a spiritual or religious symbol with a circular structure, often containing squares and triangles within it. Jung regarded the mandala as a symbol of the self and in his analysis of Pauli's dreams, with the identity of the dreamer concealed, Jung reproduced one of the familiar Fludd diagrams showing the female world soul as the link between two realms, the macrocosm and the microcosm. I will return to this point later.

To this day, I am not quite sure why this information about Pauli's identity was so readily divulged to Loewenberg and to me in 1982. In fact, subsequent research reveals that there was considerable resistance – indeed, a certain embarrassment – about making Pauli's true identity public. As Suzanne Gieser has shown in her important study of Jung and Pauli, the committee in charge of the Pauli Archive initially tried to prevent the so-called "psychological letters" from being made fully accessible, although they eventually agreed with the sensible judgment of Karl Von Meyenn, the editor of the Pauli correspondence, that "it is of no importance what we think of Jung and his psychology. The important thing is that Pauli was a convinced adherent of Jung's teachings. One cannot therefore leave out this part of his writing and his estate."[11] Nonetheless, after Pauli's death in 1958, his second wife, Franca, burned a bundle of letters found in a locked drawer in his office. The letters were between Pauli and Marie-Louise von

[11] Suzanne Gieser, *The Innermost Kernel: Depth Psychology and Quantum Physics: Wolfgang Pauli's Dialogue with C.G. Jung* (Berlin: Springer, 2005), pp. 4-5.

Franz (1915-98), a Jungian psychologist who, thanks to her early training in classical philology, was responsible for the Latin translations that Pauli used in his essay on Kepler and Fludd. Von Franz was also responsible for translating many of the alchemical texts that Jung analyzed. Pauli's wife suspected that something more than translation was occurring between them. This purging of the archive may also remind us of another, well-known episode involving the protection of the image of a major scientific figure – the selling off of the alchemical and theological papers of Isaac Newton by Sotheby's in 1936 because it was thought inconceivable that the Great Man could have authored such nonsense.[12] By now, of course, historians of science have made Newton's unpublished alchemical and theological writings a serious concern. Thanks to Rob Iliffe, all of this material is now readily available with a few clicks at "The Newton Project."[13] But attention to psychological issues is only just beginning to receive comparably serious attention.

In this lecture, I would like to summarize briefly where the conversation about Jung and Pauli has moved since Brian Vickers' volume appeared in 1984 – what have we learned and what new questions have arisen. Most importantly, thanks to the welcome publication of Pauli's correspondence under the direction of Von Meyenn and the studies of Suzanne Gieser and Arthur I. Miller, there is new and important information about Pauli's life, to some of which I have already made allusion.

Wolfgang Josef Pauli was born in 1900 to a secular Jewish family in Vienna. The family was originally from Prague and Pauli's grandfather, Jacob Pascheles, was "a leading member of the Jewish community" in that city. As an indication of his stature, he presided over the Bar Mitzvah ceremony of Franz Kafka.[14] To mitigate the socioeconomic restrictions and political dangers of antisemitism, however, his father converted to Catholicism. This was a common sort of political accommodation in the nineteenth century: for example, the father of Karl Marx converted to Protestantism in order to improve his prospects for professional advance-

---

12 See Sarah Dry, *The Newton Papers: The Strange and True Odyssey of Isaac Newton's Manuscripts* (Oxford: Oxford Univ. Press, 2014), pp. 142-160.

13 Rob Iliffe, http://www.newtonproject.ox.ac.uk

14 Arthur I. Miller, *137: Jung, Pauli and the Pursuit of a Scientific Obsession* (New York: Norton, 2009), p. 20. Previously published in hardcover under the title, *Deciphering the Cosmic Number (137): The Strange Friendship of Wolfgang Pauli and Carl Jung*.

ment as a lawyer.[15] In 1898, the father of our subject also changed his name from Pascheles to "Pauli" and the following year, he married a Catholic woman named Bertha Camilla Schütz; their son Wolfgang was thus born with the name Pauli.[16] In 1928, Wolfgang Pauli would renounce Catholicism and "unofficially adopt his father's original religious identity [as a Jew]."[17] In 1906, Pauli's mother gave birth to a baby sister, Hertha Ernestina, who would grow up to become an actress. Pauli was extremely devoted to his mother, who was a highly intelligent, literate person, a sometime newspaper journalist, politically a socialist, who wrote theater reviews and historical essays for the liberal *Neue Freie Presse*.

In 1927, with the precocious Pauli already recognized as an extremely accomplished and brilliant physicist, his father had an affair with a younger woman. … and his wife Bertha commit suicide. Pauli was extremely distraught and also intensely angry with his father. Yet, at the same time, his career was soaring: in 1928, he was appointed to the Chair of Theoretical Physics at the ETH in Zürich. But, in the aftermath of his mother's death, Pauli fell into a deep depression, he turned to drinking and began to make trips to Hamburg and Berlin where he frequented the red light districts.

It was in Berlin, in 1929, that he met an attractive cabaret dancer named Käthe Deppner, a friend of his sister, Hertha. Pauli was immediately smitten and, rather impulsively, he asked for her hand, she accepted, and they were quickly married. Just as quickly, the marriage turned into a disaster and a few months later, in November 1930, Käthe left him for someone with whom she had been involved earlier, a chemist (Paul Goldfinger) for whom she still had strong feelings. "If it had been a bullfighter," Pauli remarked, "with someone like that I could not have competed – but with an average chemist?"[18] In the ensuing emotional crisis, Pauli fell into a depression, self-medicating with alcohol and tobacco, and eventually pushing himself to the realization that his talents as a mathematical physicist could not save him and, thus, finally led him to seek out Jung for help.[19]

---

15 See Jonathan Sperber, *Karl Marx: A Nineteenth-Century Life* (New York, Norton, 2013), pp. 16-20.

16 Her maternal grandfather, however, was Jewish (Miller, *Jung-Pauli*, p. 115).

17 Ibid., p. 114.

18 Ibid., p. 117.

19 Ibid., p. 120; Gieser, *Innermost Kernel*, pp. 142-43.

What were the therapeutic options available to someone of Pauli's social class, professional status and cultural background faced with an emotional problem of this nature in the late 1920s? The recent experience gained by physicians during World War I had caused a few doctors to examine the new and expanding psychoanalytic literature. The horrific injuries sustained in the industrialized killing had presented military physicians with soldiers who had no apparent physical injuries but who were verbally incoherent and clearly could not function physically in battle. The presenting symptoms could include tics, convulsions, muscle spasms, paralyses, uncontrolled shaking, a blank stare and memory loss. This syndrome came to be known as "shell shock" or "war neurosis." Many military commanders, sometimes backed by army physicians, believed that these soldiers were faking their symptoms. The hardline position was either to send these men back to the front or to execute them. And hundreds of men were shot. But an opposing diagnosis also developed among some English physicians who had been influenced, to one degree or another, by Sigmund Freud's new theories. Charles Myers (1873-1946), who had helped to establish the "Diploma in Psychological Medicine" at Cambridge in 1912, took the term "shell shock" that the soldiers were already using amongst themselves and made it into a respectable diagnostic category. Myers's view was that the men should be seen as suffering from psychological trauma, a mental condition amenable to psychotherapy – the latter term one that he was among the first to use.[20] And the therapy that he advocated was to try to bring back forgotten memories, as he put it, "by obtaining persuasively the recall of repressed memories, with or without the aid of light hypothesis."[21]

The turn to psychological explanations can be regarded as one manifestation of the secularization of mental illness in the late nineteenth and earlier twentieth centuries. When Sigmund Freud (1856-1939), a physiologist and aspiring physician, left Vienna for Paris in 1886 to study the method of hypnosis with Jean-Martin Charcot (1825-1893), he found himself at the Salpêtrière, a hospital that the Third Republic had recently removed from the control of the Catholic Church. Charcot's reputation rested on his use

[20] See John Forrester and Laura Cameron, *Freud in Cambridge* (Cambridge: Cambridge Univ. Press, 2017), pp. 252-255; Sonu Shamdasani, "'Psychotherapy', the invention of a word," *History of the Human Sciences*, 2005, *18*: 1-22.

[21] Forrester and Cameron, *Freud in Cambridge*, p. 253.

of hypnotism to treat hysterics.[22] In 1900, Freud proposed another approach: analysis of dreams through the method of free association. The interpretation of dreams, he alleged, was "the royal road to the unconscious." Hidden, but unfulfilled wishes, whose real meanings were distorted in the dream, could be decoded by listening carefully to what the patient said as she or he made associations to the dream.[23]

It is important to appreciate that in the 1920s Freud's ideas and methods were still quite fresh and held great appeal among certain intellectual elites. In London, Freud attracted a considerable following, among whom were James Strachey (1887-1967) and Freud's biographer, Ernest Jones (1879-1958). Strachey was analyzed by Freud during two years in Vienna and then, upon return to London, became the chief translator of Freud's writings. But although London was – and still is – a well-known center of psychoanalysis, an important recent study by John Forrester and Laura Cameron has shown convincingly that Freud's ideas found fertile soil in Cambridge. Cambridge is an unexpected locus for psychoanalysis because there were no Freudian practitioners in that university town and Freud himself never set foot there. Nevertheless, there was tremendous interest – if also some curiously over-intellectualized construals of Freud – by a few unexpected parties, among whom it is worth mentioning the philosophers Bertrand Russell (1872-1970) and Ludwig Wittgenstein (1889-1951). Russell's wife Dora wrote: "Freud delved into the world of our dreams and subconscious in an attempt to understand our aberrations. Bertie, though he intellectually 'took in' Freud, was, I think, too well barricaded within the intellect really to comprehend Freud's meaning."[24] Wittgenstein claimed that philosophy could be therapeutic: philosophy could make a problem disappear by showing that language had been misused. It offered a solution to a "grammatical" difficulty.[25] And, several Cambridge intellectuals actually went to Vienna to be analyzed by Freud himself. Sitting in 1920s

---

22 See Frank Sulloway, *Freud, Biologist of the Mind: Beyond the Psychoanalytic Legend* (New York: Basic Books, 1979), pp. 28-35.

23 Sigmund Freud, *The Interpretation of Dreams* (1900) in *Standard Edition*, trans. James Strachey (London: Hogarth, 1953), vols. 4-5. First published as *Die Traumdeutung* (1900). Sonu Shamdasani contests Freud's representation of the earlier history of dream interpretation (*Jung and the Making of Modern Psychology: The Dream of a Science* [Cambridge: Cambridge Univ. Press, 2003], p. 101ff.)

24 Forrester and Cameron, *Freud in Cambridge*, p. 334.

25 Ibid.

Vienna, Freud must have learned a great deal about the private, emotional lives of Cambridge dons in that very small university town.

Thus, Pauli's encounter with Jung may usefully be seen in terms of its local possibilities. Had he lived in London or Cambridge or Vienna, it is extremely *unlikely* that he would have entered Jungian treatment. And, when Pauli approached Jung in 1931, Jung recommended that he should consult his young, recent disciple, Erna Rosenbaum (1897-1957). Unlike the Cambridge dons who traveled to Vienna to consult Freud, Jung took a quite different approach. At a seminar in Bailey Island, Maine in 1936, he laid out the treatment procedure he had recently used for Pauli, but without naming him or Rosenbaum:

> "I saw him at first for only twenty minutes. I instantly perceived that he was in a way a master mind, and I decided not to touch his intellect. I therefore proposed to him to go to my then most recent pupil, a woman who knew very little about my work. She was right in the beginning of her own analysis; but she had a good instinctive mind. She was not a fool, but had a good deal of common sense, and was, of course, highly surprised when I told her that I was going to send such a fellow to her. Naturally, I had to do some explaining. I told her why I was doing it and also suggested to her how to deal with him. I told her I had instructed him to present his dreams to her; that he must write them out very carefully, and that she should listen and nod her head; and, in case she was astonished or puzzled, should say so. She should not, however, try to understand or analyze these dreams. Now she was, of course, quite glad that she had to play a more or less passive role, and astonishingly enough, that man incidentally saw the point too. He understood what I told him. I said, 'I don't want to influence your own mind, which is valuable. If I should do it for you, you would never be convinced; therefore, I shall not even try. You go to this woman doctor and she will listen to your dreams.'"[26]

Ironically, Jung's effort to exercise some kind of "surveillance of the self" in the interest of a scientifically distantiating "mechanical objectivity" – to use Lorraine Daston and Peter Galison's analytic terms – was strikingly at

[26] Quoted in Shamdasani, *Jung and Modern Psychology*, p. 154; a new edition of the Bailey Island Seminar, with commentary by Suzanne Gieser, is forthcoming from Princeton University Press in Fall, 2019.

odds with Pauli's clear rejection of the observer's neutrality, as expressed in his Kepler essay:

> "In microphysics … every observation … interferes on an indeterminable scale both with the instruments of observation and with the system observed and interrupts the causal connection of the phenomena preceding it with those following it. This uncontrollable interaction between observer and system observed, taking place in every process of measurement, invalidates the deterministic conception of the phenomena assumed in classical physics."[27]

In the essay that Jung published as a companion to that of Pauli ("Synchronicity: An Acausal Connecting Principle"), he was clearly moving – or perhaps, staggering – towards Pauli's epistemology in holding that "natural law possesses a merely statistical validity and thus keeps the door open to indeterminism."[28] However, in that same essay, he devoted considerable attention to astrology but failed to comment on the position he had taken earlier in the Bailey Island Seminar.

During the relatively short eight-month period of his treatment with Rosenbaum, Pauli produced a considerable number of dreams, all of which he wrote down and all of which were communicated to Jung. As we notice in Jung's account, Rosenbaum was instructed *not* to interpret the dreams. But after only five months, she decamped for Berlin although Pauli continued to send her descriptions of his dreams. It is notable that, unlike the Freudian approach to the meaning of dreams – with its emphasis on the accompanying free associations – the Jungian approach, at least at that historical moment, regarded the meanings of the dreams, quite apart from the accompanying associations, as paramount.[29] After Rosenbaum left Zürich, Pauli then met regularly with Jung between 1932 and 1934, and during this period he communicated to him some 400 dreams.

---

27 Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007), pp. 115-190; Wolfgang Pauli, "The Influence of Archetypal Ideas on Kepler's Theories," in *The Interpretation of Nature and the Psyche* (London: Routledge and Kegan Paul, 1955), p. 211.

28 *The Interpretation of Nature and the Psyche*, p. 17.

29 For further discussion of the Jungian account of dreams, see Shamdasani, *Jung and the Making of Modern Psychology*, pp. 152-162.

Then, in 1934, Pauli met a fashionable young woman at a party in Zürich, Franziska Bertram (1901-1987).[30] They were married in April of that same year and, not long afterward, Pauli officially ended his sessions with Jung, largely it seems at his new wife's insistence.[31] Whether the therapeutic encounters with Jung can be regarded as a "success" is an interesting question – but one that is difficult to answer. However, the fact is that the marriage lasted and Pauli clearly developed a close *intellectual* relationship with Jung of which the later Kepler-Fludd essay was one expression. In sum, while Jung made use of Pauli's dreams in his lectures and publications – especially in his *Psychology and Alchemy* (1944) – Pauli absorbed and made use of Jung's theories about the structuring and functioning of the different, opposing parts of the self: Thought and Feeling, Intuition and Sensation.[32]

Returning to Pauli's historical work, this brings me to two preliminary observations. First, in privileging Jung's innate universal structures or "archetypes"[33] in his study of the Kepler-Fludd controversy, Pauli believed that the history of science could explain something important about creativity and commitment to particular concepts – specifically, where certain concepts come from and especially why they acquire unusual force. In his view, the operation of the Jungian collective unconscious, with its manifestation in certain symbols, motivated intense preoccupations and emotional investment in opposing parts of the self, one part conscious and illuminated, the other part dark and repressed from consciousness. Applying this notion to Kepler's heliocentrism, Pauli wrote:

> "In Kepler the symbolical picture *precedes* the conscious formulation of a natural law. The symbolical images and archetypal conceptions are what *cause* him to seek natural laws. For this reason, we also regard Kepler's view of the correspondence between the sun with its surrounding planets and his abstract spherical picture of the Trinity as primary … and by no means the other way around, as a *rationalistic* view might cause one erroneously to assume."[34]

30 Miller, *Jung-Pauli*, pp. 158-160.
31 Gieser, *Innermost Kernel*, pp. 146-148.
32 See Miller, *Jung-Pauli*, 131.
33 See Shamdasani's helpful discussion, *Jung and the Making of Modern Psychology*, 2003, p. 88.
34 Pauli, "The Influence of Archetypal Ideas," p. 171

Essentially, Pauli can be understood to be saying that Kepler's geometrized archetype of the Holy Trinity delivered an affective or emotional charge to Kepler's Copernican conviction. According to Pauli, Kepler's commitment was not based in reason, first and foremost, but rather in the unconscious force of the Jungian archetype.[35]

Thomas Kuhn also wanted to explain why scientists commit to a general conceptual scheme. In *The Copernican Revolution* (1957), he suggested that the longevity of the two-sphere, Aristotelian-Ptolemaic universe might be explained both by the tight logic of its internal elements as well as by the feeling of "at-homeness" that lay behind its structure.[36] In 1962, he famously replaced this psychological explanation with the paradigm as a communal process of group socialization that involved learning to solve problems in a shared way. This shared social learning experience – rather than an innate universal structure – explained why scientists come to see the world in a particular way and why they then resist alternative ways of seeing it.

Here, we might note that Kuhn's explicit motivating analogy came from Gestalt psychology. Members of a paradigm can only see a picture of a duck; but the scientific revolutionaries are able to see the duck as a rabbit. [Figure 5] Like Pauli, Kuhn himself had had his own therapeutic experience, having undergone two years of Freudian psychoanalytic treatment between 1947 and 1949.[37] It was also during this period that Kuhn began to read history of science when he was invited by James Conant, the President of Harvard, to teach that subject for non-science students. Kuhn often recalled the experience he had while trying to make sense of Aristotle's

---

35 In 1956, this is just the claim about Kepler that Gerald Holton found worthy of emphasis: "As Wolfgang Pauli has pointed out in a highly interesting discussion of Kepler's work as a case study in 'the origin and development of scientific concepts and theories,' here lies the motivating clue: 'It is because he sees the sun and planets against the background of this fundamental image [*archetypische Bild*] that he believes in the heliocentric system with religious fervor'; it is this belief 'which causes him to search for the true laws concerning the proportion in planetary motion …'". Note that Holton's translation of *archetypische* erases the Jungian connotation that Pauli intended (see further, Westman, "Nature, Art and Psyche," p. 227n76).

36 In his earliest work, Kuhn was already thinking of commitment to a conceptual scheme as possessing both logical and psychological functions: "For example, the psychological craving for at-homeness … can be satisfied by a conceptual scheme only if that scheme is thought to be more than a convenient device for summarizing what is already known." (*The Copernican Revolution* [Cambridge, Mass., Harvard Univ. Press, 1957, p. 38).

37 Jenine Andresen, "Crisis and Kuhn," *Osiris*, 1999, *90*:S43-S67; John Forrester, "On Kuhn's Case: Psychoanalysis and the Paradigm," in *Thinking in Cases* (Cambridge: Polity Press, 2017), pp. 25-64.

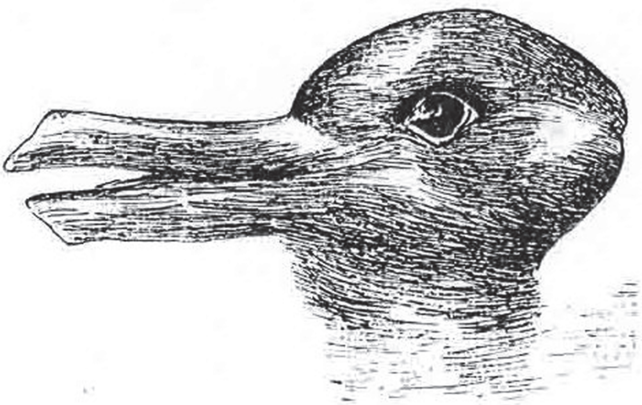


Fig. 5: Duck-Rabbit Gestalt

physics. How could the obviously very intelligent Aristotle have held so many erroneous views? In struggling with this issue, Kuhn had a flash of insight. He asked himself: *What was the question that Aristotle was asking*?[38] One might say that Kuhn became "unstuck" from his original way of reading Aristotle as a modern physicist. He learned to *empathize* with Aristotle's question – to see the problem from Aristotle's point of view rather than from his own. That capacity to see a problem from another's position is a possible source of Kuhn's idea about scientific revolutions and its roots may well lie in what he learned about himself in Freudian psychoanalysis and later in Gestalt psychology.[39]

Of course, both Pauli's and Kuhn's appeals to psychological explanations can be shown to run into problems of complexity that simply did not exist in the historiographies of the 1950s. For example, one might ask what unconscious archetype lay behind the commitments of *other Copernicans* of Kepler's generation – that is to say, the third generation after Copernicus's book first appeared in 1543? It need hardly be said that Galileo was just as strongly committed to the Copernican theory as was Kepler. Yet,

38 Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: Univ. of Chicago Press, 1977), pp. xi-xii.

39 See David Kaiser, "Thomas Kuhn and the Psychology of Scientific Revolutions," in eds. Robert J. Richards and Lorraine Daston, *Kuhn's* Structure of Scientific Revolutions *at Fifty: Reflections on a Science Classic* (Chicago: Univ. of Chicago Press, 2016), pp. 71-95; Robert S. Westman, "Two Cultures or One? A Second Look at Kuhn's *The Copernican Revolution*," *Isis* 1994, *85*:79-115.

Galileo was consciously averse to Kepler's metaphysics although, unlike Robert Fludd, he did not directly attack the Keplerian formulation; he simply ignored it. In fact, he entirely ignored Kepler himself by not writing to him for thirteen years after Kepler had extended to him a warm and enthusiastic invitation to join forces and to collaborate.[40] In that case, one might ask what unconscious archetype fueled Galileo's own endorsement of the Copernican arrangement? Perhaps a supporter of Pauli's approach could argue that Galileo's rejection of the Keplerian archetype *explains* why Galileo refused to join Kepler in a Copernican alliance in 1597? Or again, to take another example, why did both Kepler and Galileo reject Giordano Bruno's infinite universe with its innumerable suns and heliocentric systems?

The historical problem here is that the evidence points unambiguously to the striking philosophical diversity and social disunity of the Copernicans at the end of the sixteenth century and to their failure to form strong and coherent alliances of mutual support.[41] The term "Copernicanism," still commonly used in recent historiography, directs attention away from these differences.[42] Pauli himself did not try to explain this intellectual diversity both because his investigation was limited in scope and because, in spite of his gesture at a larger historical framework, he was fundamentally involved in a different kind of project. Pauli explicitly framed Kepler's historical context in highly generalized terms as "an intermediary stage between the earlier magical-symbolical and the modern, quantitative descriptions of nature."[43] *Psychologically*, however, he saw himself quite literally mirrored in the controversy between Kepler and Fludd. In 1953, he expressed himself tellingly in a remarkable letter to his friend, the physicist and sometime Newtonian scholar Markus Fierz: "I am both Kepler and Fludd."[44] *Both/and*. Pauli's Kepler was the scientist who saw a world made up of quantities – in Jungian terms, the masculine, "thinking" type of personality. Fludd represented the feminine "feeling" type – the part of

---

40 See Robert S. Westman, *The Copernican Question: Prognostication, Skepticism and Celestial Order* (Berkeley: Univ. of California Press, 2011), pp. 357-360.

41 This is one of the important conclusions of *The Copernican Question*, pp. 423-426.

42 Pauli refers to Kepler's "heliocentric creed – as I should like to call it, in intentional allusion to religious creeds – with the particular form of his Protestant-Christian religion in general and with his archetypal ideas and symbols in particular …" (Pauli, "The Influence of Archetypal Ideas," p. 155).

43 Ibid., p. 154.

44 Pauli to Fierz, 19 January 1953; quoted in Gieser, *Innermost Kernel*, p. 194n.

Pauli that was intuitive and emotional – and in the "shadow". The problem, as Pauli saw it, was to integrate the two parts of the self – of him-self. Here, the crucial image for Pauli was *balance* and *imbalance*, the "either/or" structure of binary thinking as against the complementarity of "both/and" constructions for which he was, at once, striving – and resisting. Pauli was focused on the huge cost exacted on himself by the psychic energy it took to dissociate his work with abstractions and quantities from his experience of being in an intimate relationship with a woman, the imbalance between the extreme rationalism of his work in physics and his underdeveloped emotional self. It was against such binary thinking that Pauli became absorbed with the concepts of "both-and" and with complementarity. And, in the 1920s, it is no coincidence that complementarity was a major theme of the quantum physics to which Pauli was a contributor. The observer now had to take into account the conditions under which the world was observed; every observation was a unique act. And the observer was now understood as inseparable from the act of observation. "From Pauli's perspective," as Suzanne Gieser has observed, "this means the beginning of the return of the feminine principle to the Western worldview. *Eros* shows how things are interrelated, linked to each other. But anima is also linked with the deepest mysteries of existence – the rhythm of life and death and the creation of the unique."[45] Rather less Jungianly, we might put the matter this way: Pauli became aware that the work he did during the day was connected to his dreamworld at night. *Both/and*.

The limitations of Pauli's approach as a historian were, in one respect, merely the same ones that characterized much of the historiography of his era – a historiography largely premised upon the ideas of great thinkers and their world views.[46] Yet, whatever one thinks about Pauli's psycho-historical conceptualizations, his essay on Kepler and Fludd still raises an important and, for some people, an uncomfortable, question: What should historians of science themselves do about the affective realm – the realm of emotion and the unconscious? – in short, the part of the self that remains largely unnoticed and inaccessible and may announce itself in the form of all kinds of emotions, from excitement or anger to vague anxieties that arise

45 Gieser, *Innermost Kernel*, p. 342.
46 See H. Floris Cohen, *The Scientific Revolution: A Historiographical Inquiry* (Chicago: University of Chicago Press, 1994), pp. 39-45.

as the historian reads historical sources or perhaps the works of other historians? Put simply: How should the historian understand his or her own feelings?

This question was raised several years ago in another context by the historian Dominick LaCapra. LaCapra suggested that the Freudian psychoanalytic concept of "transference" is relevant to the historian's investigations.[47] In choosing a topic for study, what is it that draws us? Why do we choose certain topics, questions and methodological approaches? The customary answer is given in such familiar terms as personal skills, intellectual interest, fascination, novelty, excitement: "no one has done it before"; "you are the first one to do it"; "you can get funding," and so forth. But, because the transference is unconscious, the investigator also brings along or transfers hidden conflicts underlying the feelings that may repeat earlier, unresolved conflicts and relationships. Thus, the inner conflict is displaced onto the topic of study where it is unconsciously repeated. From this perspective, one might read Pauli as trying – successfully or not – to understand his own transference.

Finally, there is a serious challenge to the Jung-Pauli project that rejects alchemy as the basis for any sort of psychological explanation. Most recent historians of alchemy explicitly disavow and reject Jung, while showing a sophisticated appreciation for their subject's material dimension. The alchemists, they argue, were not mainly involved with spiritual and mystical imaginings, let alone unconscious projections onto material processes. They were actually doing things with matter. As Lawrence Principe and William Newman contend, the medieval and early modern alchemists were actually experimenting and making stuff. In this light, they judge Jung's readings of alchemical texts exclusively for their symbolic content to be entirely misleading and to radically obscure what the alchemists were really doing.[48] They argue as follows:

> "… if the images used in alchemical texts are in fact irruptions of the unconscious, then there would be no possibility of 'working backward' from them to decipher such images into actual, valid laboratory practice.

---

[47] "Is Everyone a *Mentalité* Case? Transference and the 'Culture' Concept," in *History and Criticism* (Ithaca, NY: Cornell Univ. Press, 1985), pp. 72-73.

[48] Lawrence M. Principe and William R. Newman, "Some Problems with the Historiography of Alchemy," eds. William Newman and Anthony Grafton, *Secrets of Nature: Astrology and Alchemy in Early Modern Europe* (Cambridge, Mass.: MIT Press, 2001), pp. 401-408.

> Nonetheless, we have presented comprehensive decodings of alchemical symbolism into modern, replicable chemical terminology. Even some of the most allegorical writings – even when describing operations intimately linked with the making of the philosophers' stone – can be sensitively 'decoded' and the chemical effects reproduced in the modern laboratory."[49]

They go on to argue that the "extravagant alchemical imagery was *consciously* constructed to hide actual laboratory operations and how the very same alchemists who penned bizarre allegorical descriptions in print were able routinely to express their knowledge in clear, unambiguous 'chemical' terms in private communications."[50] This claim suggests that the alchemists had different languages in which they communicated and that the symbolic-mystical language served the conscious function of cloaking and protecting their secrets, somewhat like bakers protecting their best recipes in a private discourse. Finally, they claim that,

> "the alchemical texts, even highly emblematic and chrysopoetic ones, are not mere irruptions of the unconscious; they are descriptions of laboratory operations consciously and purposefully outfitted in sometimes outlandish guise. The alchemists' images are not unconscious productions, but rather expressive metaphors developed under the guidance of actual observation of chemical reactions coupled with the need to maintain secrecy and the outlook of the 'emblematic world-view' characteristic of the premodern period."[51]

It is clear, then, that chemical referents for alchemical texts can be demonstrated. But, did Jung fail to notice? In *Psychology and Alchemy*, he wrote:

> "The basis of alchemy is the work (*opus*). Part of this work is practical, the *operatio* itself, which is to be thought of as a series of experiments with chemical substances. In my opinion, it is quite hopeless to try to establish any kind of order in the infinite chaos of substances and procedures [of the alchemists]. Seldom do we get even an approximate idea of how the work was done, what materials were used and what results were achieved. The reader usually finds himself in the most impenetrable darkness when it

49 Ibid., 406.
50 Ibid., pp. 406-407.
51 Ibid., p. 407.

> comes to the names of the substances – they could mean almost anything."[52]

In this passage, Jung ostensibly acknowledges a practical side of alchemy. But it is also obvious that he recognized no obvious chemical correlates, no meaningful referents translatable into the framework of post-Lavoisier chemistry and hence, although not in principle anti-realist with respect to material substantiality, he simply made no effort to explore the substances described in the various alchemical treatises that he studied. Yet, there is a plausible explanation for Jung's failure to investigate the practical/chemical side of alchemy. Jung explicitly treated his medieval and early modern sources by an *ahistorical*, comparative method, grouping different kinds of sources from different times and places into what he took to be repositories of archetypal symbols, symbols that he believed were universally shared. One of those sources was dreams.

Jung devoted nearly half of his 1944 book *Psychology and Alchemy* to an analysis of the dreams of one of his patients. Of four hundred dreams, Jung analyzed fifty-nine of them. And all of these dreams were those of Wolfgang Pauli, his still publicly-unidentified friend, partner, colleague and one-time psychotherapeutic client. The dreams were really the primary material in which Jung was interested because he believed them to represent a key to Pauli's emotional life. The alchemical material, then, was just one component of his interpretive tool-kit.

Right from the start, Jung set up his approach to these materials against Freud's method of dream interpretation: "The psychological context of dream-contents [for Freud] consists in the web of associations in which the dream is naturally embedded."[53] These are the thoughts and feelings that occur as the patient is reporting his or her dreams. From the Freudian perspective, the free associations are the key to interpreting the meaning of the dream. On this point, however, Jung quickly separated himself from Freud: "… the method I adopt in the present study seems to run directly counter to this basic principle of dream interpretation. It looks as if the dreams were being interpreted *without* [my italics] the least regard for the context. And in fact, I have not taken up the context at all, seeing that the

[52] Jung, *Psychology and Alchemy*, 2nd ed. (Princeton Univ. Press, 1968), p. 288.
[53] Ibid., p. 44.

dreams in this series were not dreamed … under my observation. I proceed rather as if I had had the dreams myself and were therefore in a position to supply the context."[54] In fact, as mentioned earlier, Pauli had reported the dreams to Erna Rosenbaum who, in turn, communicated them to Jung. Recall, furthermore, that Jung told Rosenbaum *not* to try to interpret the dreams but simply to record what the client reported. Jung then acknowledged that it would have been "a gross technical blunder" if he had only analyzed isolated dreams.

"But here," he said, "we are not dealing with isolated dreams; they form a coherent series in the course of which the meaning gradually unfolds more or less of its own accord. *The series is the context which the dreamer himself supplies*. [my italics] It is as if not one text but many [texts] lay before us, throwing light from all sides on the unknown terms, so that a reading of all the texts is sufficient to elucidate the difficult passages in each individual one."[55] In short, the dream series as a whole rather than the associations to individual dreams was one important sense in which Jung's approach was comparative. Another method was to use materials from early modern alchemical authors – mostly in the form of published illustrations – essentially, adding more texts, comparatively, to the hermeneutic tool-kit.

One of these illustrations was the *anima mundi* image published by Robert Fludd in 1617. [See figure 4] It shows man, the ape of nature, seated on the earth, in his left hand a golden chain linking him to the female figure of the *anima mundi*, and her right hand, in turn, linked to the hand of God. In Dream #40, Jung offers the following interpretation: "The idea of the *anima mundi* coincides with that of the collective unconscious whose center is the self. The symbol of the sea is another synonym for the unconscious." In his 1952 essay on archetypal ideas in Kepler and Fludd, Pauli argued that for Kepler the *anima mundi* was "no more than a kind of relic" as compared with the "magical symbolical attitude" of Fludd, who was a feeling-intuitive type of personality. Following Jung, therefore, Pauli believed that Fludd's pictures represented symbols of the collective unconscious. In contrast, he portrayed Kepler's conception of the soul "almost as a mathematically describable system of resonators." In this important

54 Ibid., pp. 44-45.
55 Ibid., pp. 45-46.

sense, neither Jung nor Pauli was using the dreams or the historical sources to construct a temporal, historical account.

Dream #59 is called the World Clock Dream. In it, Jung states that the dreamer reported "the most sublime harmony." I will not provide the full description of the dream but only a few of its key elements:

> "There is a vertical and a horizontal circle, having a common center. This is the world clock. It is supported by the black bird.
> The vertical circle is a blue disc with a white border divided into 4x8 = 32 partitions. A pointer rotates upon it.
> The horizontal circle consists of four colors. On it stand four little men with pendulums, and round about it is laid the ring that was once dark and is now golden (formerly carried by children)."[56]

The rest of the dream describes this clock and its rhythms and pulses.

Jung interpreted Pauli's dream as that of a mandala: "We shall hardly be mistaken if we assume that our mandala aspires to the most complete union of opposites that is possible, including that of the masculine trinity and the feminine quaternity on the analogy of the alchemical hermaphrodite."[57] In short, Jung read Pauli's dream as wishing to be "both-and."

Conclusion

At the time that my essay on Jung and Pauli appeared in 1984, the Pauli correspondence had not yet been published. I was fortunate then to be able to confirm some of my hypotheses about Jung and Pauli through the unanticipated "leak" of Pauli's identity that I mentioned earlier in my lecture. In subsequent scholarship that makes use of the correspondence, we are now in a position to begin to answer questions like: "What did Pauli think about the scientific status of Jung's theories?" In her excellent study, Suzanne Gieser has shown that as early as 1934 – that is to say, just after the end of his treatment with Jung – Pauli wrote to the physicist Ralph Kronig: "I also made the acquaintance of psychic things, which I did not know before and which I would summarize under the name *autonomous activity of the soul*.

56 Ibid., pp. 203-204; see also, Westman, "Nature, Art and Psyche," pp. 219-220.
57 Ibid.

It is to me beyond doubt that there are things here, spontaneous growth products […] that can be designated symbols, something objective-psychic, which cannot and may not be explained as resulting from material causes."[58] Yet, nearly twenty years later, in April 1951, just before the appearance of his Kepler-Fludd essay, Pauli wrote to Jung's disciple Anne-Marie von Franz: "I also represent the proposition that the future of the psychology of C.G. Jung does not lie at all in therapy and the therapist, but leads to natural philosophy, or at any rate to the philosophical faculty."[59] This interesting statement raises the question of whether Pauli regarded Jung's system as offering a philosophy of the psyche *without* therapeutic applications. That possibility points to a tension between the hermeneutic or humanistic disciplines that Jung used to make his interpretations – history, religion, mythology, linguistics and alchemical symbolism – and, on the other hand, the absence of empirical studies involving quantitative methodologies.

When Jung founded an institute in Zürich in 1948, Pauli agreed to join, believing that he could influence Jung's project in a more empirical direction. This was also the time when he was working on his Kepler essay.[60] But several years later, he became quite irritated that the trustees of the Institute were failing to act on this goal. Pauli wrote to the administration of the Institute: "… I should like to point out that psychology always used to be counted as one of the *humanistic* sciences, but it was precisely C.G. Jung himself who emphasized the scientific nature of his ideas, and it was through his works that the way was paved for an integration of the psychology of the unconscious into the natural sciences. It is my opinion that the progress that has been made in this respect is being *seriously jeopardized by the administration of the C.G. Jung Institute*."[61] Pauli's complaints were several: (1) the absence of any empirical investigations; (2) a focus only on individual analyses – of which he believed there were too many; and most interestingly: (3) the need to study the dreams of "normal" people, that is people who were not coming to the Institute to be

58 Pauli to Ralph Kronig, 3 August 1934; quoted in Gieser, *Innermost Kernel*, p. 165.

59 Pauli to von Franz, 18 April 1951; quoted in Gieser, *Innermost Kernel*, 167n.

60 "Two Lectures by Pauli at the Psychological Club of Zürich," in *Atom and Archetype: The Pauli/Jung Letters, 1932-1958*, ed. C.A. Meier, trans. David Roscoe (Princeton: Princeton Univ. Press, 2001; first German ed., 1992), pp. 203-209.

61 Pauli to the Curatorium of the C.G. Jung-Institute, 22 July 1956; cited in Gieser, *Innermost Kernel*, p. 168.

analyzed. This last proposal shows both Pauli's independence but also his wish to move Jung's psychology in the direction of the most elementary methods of empirical research.

Pauli's proposal – should it perhaps be called "Keplerian"? – touched directly on the uncertain scientific status of the hermeneutic approach to psychology. In the end, his recommendations were rejected. He was told that only the dreams of patients were to be analyzed. Carl Alfred Meier, the president of the Institute, resigned. Nothing changed. Out of respect, Pauli decided that he could not push Jung himself and would wait to reassert his demands until after Jung died. But, as fate would have it, Pauli himself preceded Jung in death and any hope of either modifying the Institute's psychological research program or pursuing further studies using historical materials died with him. Yet, because of his enormous authority as a leading physicist of the quantum revolution and the novelty of the evidence he assembled in his study of Kepler and Fludd, Pauli's essay had a substantial impact on the historiography of science for nearly thirty years. As we have seen, however, those historians who did absorb his ideas into their work managed to side-step the Jungian associations. And, quite apart from Jung's own aspirations for the scientific status of his psychology, the broader question of psychological explanation in the history of science still remains a significant challenge.[62]

Thus, to conclude. In the work and in the lives of Jung and Pauli, scientific creativity and progress were juxtaposed with human misery, aggression and sexual desire. The opposition between conscious thought and unconscious feeling manifested itself in conscious "either-or" thinking and in dreams of symbolic unity, of "both-and". In Jung's view, Pauli paid an emotional price for living a one-sided daytime life of abstract thought – a burden that showed up in the symbolic images of his rich dream-life and in the relief that he sought in vain as he wandered the nighttime streets of Hamburg and Berlin. Jung and Pauli were not alone in seeking to make sense of these issues in the years that followed a war that had decimated

---

62 Recently, Geoffrey Cantor has shown how historians of science might benefit from applying psychological understanding of personality types to historical figures ("Humphry Davy: A Study in Narcissism?," *Notes and Records of the Royal Society*, 2018, *72*:217-237); among historians of science, Betty Jo Teeter Dobbs was unique in defending a Jungian approach to the study of Newton's alchemy (*The Foundations of Newton's Alchemy or "The Hunting of the Greene Lyon"* [Cambridge: Cambridge Univ. Press, 1975]), pp. 26-35; 40-43; 48; 80; 84). But, surprisingly, she makes no references to Pauli's essay.

nearly a generation of men and which left some of its survivors preoccupied with irrationality in a secular, modernist idiom. At the very moment that Pauli was struggling with these matters in his own life, Jung's former collaborator, Sigmund Freud, published a sober essay entitled *Civilization and Its Discontents* in which he argued that the price of modernity is the "flight into neurotic illness."[63]

**Acknowledgements*. I am deeply honored by the generous and unexpected invitation to deliver this lecture and for which I am very grateful to the members of the Sarton Committee.

[63] Sigmund Freud, *Standard Edition* (London: Hogarth Press, 1927), vol. 21, p. 84.

The 'Two-Cultures' Question and the Historiography of Science in the Early Decades of the Salk Institute for Biological Studies

Robert S. Westman

Introduction

Aspirations for the unification of knowledge have a long and diverse history in the changing disciplinary arrangements of academic knowledge. But rarely has that quest been addressed in the specific context of a scientific research institute. This paper examines Jonas Salk's short-lived project to create an environment conducive to both scientific research and humanistic studies, an objective inspired by C.P. Snow's image of a sharp divide between what he called the "cultures" of scientists and literary intellectuals. Snow himself believed that the huge progress of science and its industrial applications in the early twentieth century – "the scientific revolution," as he called it – could be applied to solving problems of hunger and disease that afflicted much of the world but that the British government was dominated by officials whose traditional classical education and scientific illiteracy had made them resistant to such policies.

Snow took no notice of earlier binary formulations that resembled his own dichotomy, especially the tension between secular and religious authority. This paper argues that attention to that resemblance will enable a deeper appreciation of the uses to which Snow's conception was put in the 1960s and 70s.[1] Precursors to the Snovian binary were associated with three historical developments in Britain and America, the first two of which noticeably emerged in the second half of the nineteenth century. These were: first, the emergence of the scientific career as a stable and recognizably independent social and economic structure; second, the gradual dissociation of the churches from the universities; and, third, in the twentieth century, the direct involvement of scientists in the creation of warfare technologies. As one consequence of these long-term processes, the history of science as a scholarly subject evolved as a particular form of secular literacy, disciplinary unification and science education that, for a brief moment, played an important role in the Salk Institute's enterprise.

The Founding of the Salk Institute

The founding of the Salk Institute in La Jolla, California in 1960 occurred at a moment of rising post-war American military and economic ascendancy. San Diego was a major naval port during World War II and, after the war, a significant locus of major Cold War industries, most prominently, the defence contractor, General Atomics.[2] In January 1961, when Dwight D. Eisenhower famously – and somewhat unexpectedly – warned of the dangers of a "military-industrial complex" in his presidential "Farewell Address to the Nation" he could easily have referenced San Diego.[3] It is no accident that the oceanographer Roger Revelle, the leading promoter of a new campus for the University of California in San Diego, advanced his case based largely upon the economic benefits that a scientifically- and technologically-oriented university campus could bring to the city's powerful defence industry. In addition, the village of La Jolla, although then ethnically and racially segregated, seemed an ideal location for Salk's enterprise because of the already well-established Scripps Institution of

1 This paper is an expanded version of my Sarton Medal lecture, delivered on October 12, 2018. I am indebted to the Sarton Committee of the University of Ghent for their very generous invitation. This paper could not have been written without the encouragement and support of Virginia Gordon, Lynda Claassen, Peter Salk, Jonathan Salk, and Rachel Klein.
On family resemblance and its origins, see Carlo Ginzburg, "Family Resemblances and Family Trees: Two Cognitive Metaphors," *Critical Inquiry*, 2004, *30*:537-556.

2 https://en.wikipedia.org/wiki/General_Atomics#History. Consulted July 7, 2019.

3 https://en.wikipedia.org/wiki/Eisenhower%27s_farewell_address#The_speech. The full speech can be read and viewed at this site. Consulted July 7, 2019.

Oceanography, the organization that Revelle himself directed from 1950 to 1964.[4]

In 1960, still riding the crest of the extraordinary public acclaim accorded him by his conquest of the dreaded polio virus in 1955, Jonas Salk (1914-1995) chose to locate his institute in this untested environment – adjacent to, but not within, a newly-founded university and supported entirely by private, Cold War philanthropy rather than within the already well-established framework of Stanford University.[5] The central appeal of this unusual setting appears to have been its openness to new intellectual possibilities, the rare chance to participate directly in the design of a completely new space for scientific investigation, including the very architectural structure that would house the whole enterprise.[6] Indeed, the intellectual architecture of Salk's vision was as unusual as its concrete design. First and foremost, it would be an institute for biological research. But it was also to be a place where Salk intended to bring together humanists with scientists of the stature of Francis Crick (1916-2004), the co-discoverer of DNA and Leo Szilard (1898-1964), whose demonstration of a nuclear chain reaction was crucial to the production of the atomic bomb.[7] In this regard, Salk's thinking was significantly influenced by C.P. Snow's Rede Lecture, *The Two Cultures and the Scientific Revolution*, delivered in 1959 at Cambridge University. In November 1960, Salk observed of Snow's dichotomy: "It is not only that the gap exists, but that the posture of scientists and artists seems to be back-to-back, rather than face-to-face … As each of them moves forward in his own way, the gap increases." And then: "There ought to be a place for biological studies but which also contained the conscience of man."[8] A few years later, he referred to Snow's two-cultures in a telling image as analogous to a diseased body in need of medical attention and, in another revealing image, like the divide between

---

4 Mary Ellen Stratthaus, "Flaw in the Jewel: Housing Discrimination against Jews in La Jolla, California," *American Jewish History,* 1996, *84*:189-219.

5 See Salk's sketch of pros and cons in Suzanne Bourgeois, *Genesis of the Salk Institute: The Epic of Its Founders* (Berkeley: Univ. of California Press, 2013), p. 66. Hereafter cited as "Bourgeois, *Salk Institute*."

6 See Stuart Leslie, "A Different Kind of Beauty: Scientific and Architectural Style in I.M. Pei's Mesa Laboratory and Louis Kahn's Salk Institute," *Historical Studies in the Natural Sciences*, 2008, *38*:173-221.

7 Bourgeois, *Salk Institute*, pp. 97-106; Charlotte DeCroes Jacobs, *Jonas Salk: A Life* (Oxford: Oxford Univ. Press, 2015), p. 232. Hereafter cited as Jacobs, *Jonas Salk*.

8 Nicholas Wade, "Elitest Pursuit of Biology with a Conscience," *Science* 1972,*178*: 846-849; also discussed in Jacobs, *Jonas Salk*, p. 232.

the ’60s youth counter-culture and his own Depression-era generation.[9] It was the dream of finding a “cure” for this divide that powerfully motivated Salk and fortified him against the actual obstacles that faced him even before he could establish himself in his new setting.[10]

Importing British Humanist-Scientific Intellectuals

When Salk invited C.P. Snow (1905-1980) to join the Institute as a resident fellow, it is unclear how fully he appreciated the personal roots of the Snovian two-cultures formulation. Surely, he was aware of basic elements of Snow’s life story, notably, his training as a chemist and his remarkably successful, prolific second career as a novelist. Indeed, by 1928, Snow had published a considerable number of scientific papers before taking his doctorate at Cambridge in 1930. But a paper that he co-authored in 1932 on producing Vitamin A by the photochemical transformation of carotene ended in disaster when it was shown that its calculations were faulty.[11] Although deeply discouraged, Snow drew on unusual inner resources to develop a different part of himself and to produce a remarkable succession

9 “Not so many decades ago medicine was practiced as an art and did not have the benefit of the knowledge and understanding that now exists for treating and controlling disease and for enhancing health. At this point in time there is need to develop the equivalent of the basic science that contributed to the advancement of the practice of medicine – the equivalent for practitioners in human affairs who must take into consideration the whole man, the integrated person, whose health depends upon the easing of the unhealthy conflict between body and mind, between body and soul. This is not unlike the problem to which C.P. Snow addressed himself in his essays on two cultures, nor is it different from the extremes that we see, of hippies on the one hand and the mechanized man on the other. There is the need to understand and reduce the distance between different and often antagonistic forces in man and in society.” Jonas Salk to John D. Rockefeller, IV, Sept. 4, 1968 (Jonas Salk Papers [hereafter JSP], UCSD Special Collections, Box 66, folder 4).

10 Among these obstacles were the formal and informal barriers against Jews buying homes in La Jolla; in addition, the land for the proposed institute was already promised to the university, a problem that caused a serious split between Salk and his major patron, Roger Revelle. In the end, the matter was settled by a vote of the community which deeded the land to the Institute (Jacobs, *Jonas Salk*, pp. 237-239. Bourgeois, *Salk Institute*, pp. 54-70). Meanwhile, Revelle, fully conscious of the issues at stake for the prospective university, told the local community: “I said, and consistently said it, always from 1950 on, you can’t have a university without having Jewish professors … The Real Estate Broker’s Association and their supporters in La Jolla had to make up their minds whether they wanted a university or an anti-Semitic covenant” (Stratthaus, “Flaw in the Jewel,” p. 215).

11 “I was extremely miserable. Everything, personal and creative, seemed to be going wrong” (J.C.D. Brand, “The Scientific Papers of C.P. Snow,” *History of Science*, 1988, *26*:119); Guy Ortolano, *The Two Cultures Controversy: Science, Literature and Cultural Politics in Postwar Britain* (Cambridge: Cambridge Univ. Press, 2009), pp. 31-32.

of novels. Snow's younger brother Philip later wrote: "The trauma after all that publicity put Charles off scientific research irrevocably, arguably to his advantage. He might have recovered his nerve and gone on to make his name through molecular research but, in retrospect, one can feel more confident that literature, combined with his liaison with science, was his métier."[12] This all-too-brief observation about Snow's emotional and cultural life, helps to explain his choice of science and literature as the "two cultures" – rather than, say, science and the social sciences. At the beginning of the war, Snow's scientific training enabled him to move into the state bureaucracy where, in the Ministry of Labour, he recruited scientists for research on radar. But it was his Rede Lecture that captured popular attention, catapulted him to international public fame and also precipitated an explosive reaction within the elite world of Cambridge and Oxford.[13]

A second humanist-scientist intellectual of the post-war era approached by Salk was C.P. Snow's friend and close contemporary, Jacob Bronowski (1908-1974), known as "Bruno". A Jew whose family had emigrated to Germany from Poland during World War I and, in 1920 to England, the trilingual Bronowski shared with Salk the cultural tendencies of a secularized, free-thinking, liberal Judaism. But, ultimately, the common elements of this identity could not override significant differences arising from their quite different family and educational backgrounds. Salk's father was a Russian émigré who never completed grade school and worked in the New York City garment district designing women's blouses and neckwear.[14] In the early 1930s, Salk attended the City College of New York – renowned for some famous faculty like the philosopher Morris Raphael Cohen (1880-1947), but also for the fierce competition for admission, a robust tradition of student political radicalism and the free tuition that enabled the children of poor and lower middle class immigrant families, like Salk's, to attend. With only a smattering of courses in the humanities and social science, Salk then entered NYU Medical School in 1933 after only three years of under-

12 Philip Snow, *Stranger and Brother: A Portrait of C.P. Snow* (New York: Scribner, 1983), p. 35.

13 For further details aroused by the debate, see Guy Ortolano, *The Two Cultures Controversy*; F.R. Leavis,*The Two Cultures?: The Significance of C.P. Snow,* Introduction by Stefan Collini (Cambridge: Cambridge Univ. Press, 2013); "Special Issue: Two Cultures?" *History of Science* 2005, *43:*109-208.

14 David Oshinsky, *Polio: An American Story* (Oxford: Oxford Univ. Press, 2005), p. 95.

graduate study.[15] For Jewish boys of Salk's social class to become a doctor or a lawyer was considered among the highest ambitions. His mother wanted him to become a rabbi.[16]

Unlike Salk, Bronowski inherited the cultural capital of a family with intellectual aspirations. His father was a haberdasher but also a Torah scholar; he spoke German rather than Yiddish, the language of the masses.[17] In the same year that Salk commenced his medical studies, the slightly older Bronowski graduated from Cambridge with highest honours in mathematics. Along the way, he also wrote poetry and co-founded a student literary magazine in 1928, called *Experiment*.[18] Throughout his life, he continued to pursue an unusual combination of scientific, literary, philosophical and historical concerns. And as a member of Jesus College, Bronowski also absorbed the informal social codes essential for passing successfully within the Christian collegiate culture. Like Snow, he too had wartime experience that utilized his technical skills. He worked in the Ministry of Home Security, applying his statistical abilities to studying and optimizing bomb damage on German industrial and civilian targets.[19] Immediately after the war, indeed, from 1946 onward, Bronowski became an exceptionally well-known and admired BBC radio personality, offering commentaries on scientific topics and, in particular, on the ethical implications of science. Many of these talks were gathered together and published as small volumes. In the late 1950s, he moved his popular presentations effortlessly to the new medium of television. Therefore, in July 1960, when

15 For the entry of free-thinking Jews into New York City academic life from the 1920s onward, see David A. Hollinger, "Jewish Intellectuals and the De-Christianization of American Public Culture in the Twentieth Century", in *Science, Jews, and Secular Culture: Studies in Mid-Twentieth-Century American Intellectual History*, (Princeton: Princeton University Press, 1996), pp. 17-41; for Salk's undergraduate studies at CCNY, see Oshinsky, *Polio,* pp. 96-98; Charlotte Jacobs, *Jonas Salk,* pp. 14-15.

16 On the importance of public schools for early twentieth century Jewish immigrant families, see Hasia Diner, *The Jews of the United States, 1654-2000* (Berkeley: Univ. of California Press, 2004), pp. 145-147. For a time, as an undergraduate, Salk had intended to become a lawyer but then he discovered science: "My father was totally smitten by the scientific method, by chemistry." Peter Salk interview, August 8, 2018.

17 Judith Bronowski interview, June 14, 2019.

18 See Robert Bud, "Life, DNA and the Model," *The British Journal for the History of Science*, 2013, *46*:8.

19 He was also under surveillance by MI5 as a security risk which later blocked his working for the Atomic Energy Authority. See Ralph Desmarais, "Jacob Bronowski: A Humanist Intellectual for an Atomic Age, 1946-1956," *The British Journal for the History of Science*, 2012, *45*:573-589; Lisa Jardine, "The Sorcerer's Apprentice: C.P. Snow and J. Bronowski," *The Tanner Lectures on Human Values*, (New Haven, CT: Yale University Press, 2012), p. 89n.

Salk approached Snow and Bronowski, he was not only seeking to bring his recent encounter with the two-cultures to America but he was also inviting key members of an elite Oxbridge debate to the new and quite different setting of a Cold War-era American scientific institute.

Victorian Resonances

One of the appeals of C.P. Snow's formulation was that it readily served as a convenient referent for the gulfs created by academic specialization that had become glaringly evident in the universities of post-World War II America and Britain. However, apart from the immediate controversy that it ignited, the two-cultures question has some resemblance to much earlier social and intellectual divisions that marked Victorian-era struggles between science and religion. The first involved small, elite circles of Anglican aristocratic "gentlemen of science" who dominated the old English universities of Oxford and Cambridge in the first half of the nineteenth century. The second was a group of Scottish Presbyterian scientific investigators, supported by Northern British industrialists, who still retained the medieval/early modern term "natural philosophy" to describe their domain of investigation. Exemplary of the Anglican approach was the Earl of Bridgewater's will endowing a series of annual lectures on natural theology and presented between 1833 and 1840 on the theme: "To explore the Power, Wisdom, and Goodness of the Creation." William Whewell, Master of Trinity College, Cambridge delivered the first lecture, titled *Astronomy and General Physics Considered with Reference to Natural Theology* (1833).

By the mid-nineteenth century, however, the cultural authority of this reigning Anglican aristocratic establishment had come under serious pressure from lower middle-class men the likes of Thomas Huxley (1825-1895), the physicist John Tyndall (1820-1893) and the philosopher of evolution, Herbert Spencer (1820-1903). These so-called "scientific naturalists" – men without inherited wealth and all allied with Darwin – fought to elevate natural, causal explanations and empirical evidence *above* matters of theology. This was not just an English phenomenon. As Bernard Lightman has observed, "Victorian scientific naturalism represented the English version of the cult of science that dominated Europe during the second half of the nineteenth century connected with such intellectual currents as scientific materialism and scientific socialism …"[20] But beyond their own investigations, the scientific naturalists, and most notably Thomas Huxley, publicly argued that science teaching should have a significant place in a curriculum then dominated by the bible and study of the ancient classics.

In 1871, the Universities Tests Act allowed non-Anglicans – including non-Christians – to hold professorships and fellowships and disallowed religious tests for academic degrees, except that of divinity. Anglican dons, once quasi-monastic bachelors, could now move out of the colleges, build their own houses, marry and raise families.[21] In the same year, Newnham College at Cambridge was established, allowing women to reside and attend university lectures although, appallingly, unable to take degrees until 1948.[22] This abolition of religious tests further opened up the possibility of a more robust status for science teaching in the schools and, in turn, enabled socioeconomic mobility and independence for "men of science". The debates of the 1870s and 1880s between Thomas Huxley and Matthew Arnold (1822-88) publicly aired these issues.[23] And in his Belfast Address of 1874, the scientific naturalist John Tyndall boldly asserted: "We shall wrest from theology the entire domain of cosmological theory."[24] C.P. Snow, himself from a lower middle class family, can be regarded as a late descendant of this nineteenth-century movement.[25] Nearly a century later, in 1958, he would recommend that the British

---

[20] Bernard Lightman, "Science and the Public," in *Wrestling with Nature*, eds. Peter Harrison, Ronald L. Numbers and Michael H. Shank (Chicago: Univ. of Chicago Press, 2011), p. 339.

[21] See Sheldon Rothblatt, *The Revolution of the Dons: Cambridge and Society in Victorian England*, London: Faber & Faber, 1968.

[22] https://www.newn.cam.ac.uk/about/history. Consulted June 27, 2019.

[23] Paul White argues that both Huxley and Arnold still shared a common commitment to Anglican culture which embraced an underlying complementarity between science and the classical learning of Hellenic civilization (Paul White, "Ministers of Cultures: Arnold, Huxley and Liberal Anglican Reform of Learning," *History of Science*, 2005, *43*:115-138).

[24] Quoted in Lightman, "Science and the Public," p. 339; see further, Lightman, "The Victorians: Tyndall and Draper," in Jeff Hardin, Ronald L. Numbers and Ronald Binzley eds., *The Warfare between Science and Religion: The Idea That Wouldn't Die* (Baltimore: Johns Hopkins Press, 2018), p. 76.

[25] Philip Snow explicitly described the family as lower middle class, mentioning "at least four unpleasant features about the house … no hot water taps or wash-basins, no bathroom (only a partitioned-off section of an out-house near the scullery where buckets of water heated on a stove had to be lifted across to the bath) and in the backyard a single lavatory which was scarcely less than arctic in any season" (*Stranger and Brother: A Portrait of C.P. Snow* [New York: Scribner, 1982], p. 9).

Labour Party advocate policies and programs to increase the numbers of scientists and technicians drawn from both genders and all social classes – no longer to overcome the Church's authority but rather to ground rational government planning of atomic energy, automation and other technical issues in the hands of scientifically well-trained decision-makers.[26]

The Historiography of Science as a Site of Secular-Religious Conflict

In the decades following the end of the Civil War between the states, there was a notable rise of tensions between secular and religious ideals of education that drew upon a historical framework already influential in European scholarship. That powerful resource was Auguste Comte's (1798-1857) exuberantly secular vision of the social world, a new religion that taught the worship of humanity rather than a god or gods – a "religion of humanity." Undergirding this account was a big-picture account of historical phases. The plot of Comte's narrative was that knowledge progressed from explanations based exclusively upon the gods – supernatural agents – to a second stage where these agents become abstract and metaphysical and finally to the enlightened or positive stage, where reason and observation alone become the basis for discovering laws of nature and human behaviour.[27] Comte's vision grew out of his formative years in the École Polytechnique, the military-engineering academy founded on secular, anti-clerical principles in 1794 in the heat of the French Revolution.[28]

In the aftermath of World War I's grinding, industrialized killing, the Belgian émigré George Sarton (1884-1956) took up some important elements of the Comtean program, proposing that the history of science was the singularly ideal bridge between science and the humanities. Sarton advanced this vision not only as an educational reform but as a professional scholarly ideal – a new kind of humanism in which science would be the object of historical study – a position that he began to advocate passion-

26 Guy Ortolano, *The Two Cultures Controversy*, p. 175.
27 See John Tresch, *The Romantic Machine* (Chicago: Univ. of Chicago Press, 2012), pp. 273-84.
28 Ibid., pp. 257-259.

ately in the 1920s.[29] He was not alone in appropriating that conception. Another variant of the humanism of the interwar period that was pregnant with strong secular meanings and associations was the Vienna Circle's "scientific philosophy" of logical empiricism which banished metaphysics and religion entirely from the philosophical study of science.[30]

When in 1924 Sarton first invoked the phrase "new humanism", he meant that unlike other humanistic disciplines, the history of science revealed the unity and universality of knowledge. First, unlike political history or the history of art, *only* the history of science demonstrated a story of "continual accumulation and improvement" – that is to say, of progress.[31] Second, it shared with the humanism of the Renaissance a spirit of independence and freedom – as he put it, a "refusal to submit blindly to authorities."[32] In this regard, the "new humanism" shared with its predecessor a rejection of what Sarton took to be the undue reverence for authority characteristic of medieval scholasticism. Third, evoking the post-war spirit of internationalism, the history of science showed that science is a global phenomenon; its principles and discoveries are not the property of one group or one nation. It then followed that scientists should recognize that the very nature of their own knowledge had both a universal and a specifically historical character. By contrast, old-style, "anti-scientific" humanists who looked for eternal truths about beauty and justice in history, literature, philosophy and classics were missing the uniquely progressivist character of science.[33] Thus, if progress was the uniquely defining feature of science, then by its very nature it could only occur in historical time.

When Sarton advanced this eirenic vision of humanism in 1924 he was able to draw on a body of nineteenth-century European historiography and philosophy of science but also, without hesitation, on the secure intellectual authority of science itself. That authority had been growing steadily and relentlessly in the half century since the 1860s, increasingly at the expense

---

29 Sarton, George, "The New Humanism," *Isis*, 1924, *6*:6-42; ibid., *The History of Science and the New Humanism*, Cambridge, Mass.: Harvard University Press, 1937.

30 See Friedrich Stadler, "George Sarton, Ernst Mach and the Unity of Science Movement: A Case Study in History and Philosophy of Science," *Sartoniana* 2018, *31*:63-122; Alan Richardson and Thomas Uebel eds., *The Cambridge Companion to Logical Empiricism*, Cambridge, Cambridge Univ. Press, 2007.

31 "The New Humanism," *Isis*, 1924, p. 31.

32 Ibid., p. 32.

33 Sarton, *The History of Science and the New Humanism*, p. 11.

of the humanistic academic curricula of the British and American universities.[34] And it received a further boost during World War I. As one measure of that growth, in the nearly thirty years between 1894 and 1923, the number of industrial research laboratories In the United States jumped from 18 to 441. Between 1890 and 1920, membership in the American Chemical Society increased from 238 to 15,600 and the American Association for the Advancement of Science increased from 807 in 1875 to 11,500 in 1920.[35] These developments were underwritten by the amassing of huge concentrations of wealth in the hands of a few men in the decades that followed the Civil War, notably, the formation of big corporations, like John D. Rockefeller's Standard Oil Company (f. 1870) and Andrew Carnegie's steel company in Pittsburgh [f. 1892]. Several of these new companies sponsored their own research laboratories: General Electric (1900), DuPont (1902) and American Telephone and Telegraph (1907).[36] With big business came an ethos of managerial efficiency which was tied to corporate profits, to the exploitation of labour and the often-corrupt, corporate manipulation of the federal government.

Carolyn Winterer has argued that these developments had a profound impact on the university curriculum of both new and old institutions:

> "As the modern university rose, the classical languages were dethroned. The proliferation of new studies in the curriculum, such as modern languages, modern history, and social sciences, as well as the advent of elective study, helped to push Latin and especially Greek to the side. Before this time, Greek and Latin had been required for admission, all students once in college had pursued the same language-heavy load, and all students had emerged with the same degree, the bachelor of arts."[37]

---

34 See esp., Richard Yeo, *Defining Science: William Whewell, Natural Knowledge, and Public Debate in Early Victorian Britain* (Cambridge: Cambridge University Press, 1993), pp. 32-38; One witness to this development was the classics scholar Richard Claverhouse Jebb (1841-1905) who noted in his 1899 Romanes Lecture at Oxford that, "in the last thirty years the position of the humane letters, relatively to other studies, has been altered in several respects. The study of the natural sciences is now firmly established in schools and universities [i.e. since the late 1860s]; it can no longer be said that a haughty and exclusive humanism keeps them out of the educational field: indeed there are not a few seats of learning where they hold a clear dominance" ["Humanism in Education," p. 538]. https://en.wikisource.org/wiki/Essays_and_Addresses/Humanism_in_Education#cite_note-1.

35 See especially Arnold Thackray, "The Pre-History of an Academic Discipline: The Study of the History of Science in the United States, 1891-1941," *Minerva*, 1980, *18*:452.

36 Ibid., p. 450.

37 Carolyn Winterer, *The Culture of Classicism. Ancient Greece and Rome in American Intellectual Life, 1780-1910* (Baltimore and London: The Johns Hopkins University Press, 2002), p. 101.

Phrases like "modern life," "modern times," and "modern science" were associated with this development and noticeably increased in frequency from 1870 onward.[38] The emerging ideals of education of this period were the values of utility and practicality and both of these values were seen to be connected with science and progress. Students should be prepared for "real life." In 1890, a professor at New York University articulated this development when he stated that "the college has ceased to be a cloister and has become a workshop."[39]

The industrial image of the workshop contrasts sharply with the predominantly religious character of antebellum American colleges and universities.[40] Colonial-era colleges – such as Harvard, Princeton and Yale – were largely training grounds for the Christian ministry.[41] The outstanding exception of the pre-revolutionary era was Benjamin Franklin's Academy and College of Philadelphia, the forerunner of the modern University of Pennsylvania. Founded in 1749, it was exceptional in defending the teaching of modern languages and science in addition to Latin and Greek.[42] Crucially, therefore, the classics curriculum was directly connected to the goals of the religious colleges.

Thus, with the enhanced intellectual authority of science and the rejection of Greek and Latin in the increasingly secularized universities following the Civil War, it is no accident that two of the most important and influential American *histories* of science of the Gilded Age were organized around the theme of science and religion – or rather, science *versus* religion. The first of these well-known works was John William Draper's *History of the*

---

38 Robert Bud and Morag Shiach, "Being Modern: Introduction," in *Being Modern: The Cultural* Impact *of Science in the Early Twentieth Century*, eds. Robert Bud, Paul Greenhalgh, Frank James, Morag Shiach (London: UCL Press, 2018), p. 3.

39 Lawrence Veysey, *Emergence of the American University* (Chicago: Univ. of Chicago Press, 1965), pp. 13, 61ff.; Winterer, *Classicism*, p. 104.

40 See Frederick Rudolph, *The American College and University*: *A History*, Introd., John Thelin (Athens, GA: University of Georgia Press, 1990; first pub. 1961), pp. 3-22.

41 As Craig Wilder has shown, these early colleges were also "instruments of Christian expansionism, weapons for the conquest of indigenous peoples, and major beneficiaries of the African slave trade and slavery" (Craig Steven Wilder, *Ebony and Ivy: Race, Slavery, and the Troubled History of America's Universities*, [New York: Bloomsbury Press, 2013], p. 17).

42 Benjamin Franklin, *Proposals Relating to the Education of Youth in Pensilvania* (Philadelphia, 1749), p. 25: "All intended for Divinity should be taught the *Latin* and *Greek*; for *Physick*, the *Latin*, *Greek*, and *French;* for Law, the *Latin* and *French*; Merchants, the *French*, *German* and *Spanish*: And though all should not be compell'd to learn *Latin*, *Greek* or the modern foreign Languages; yet none that have an ardent Desire to learn them should be refused; their *English*, Arithmetic, and other Studies absolutely necessary, being at the same Time not neglected."

Conflict between Religion and Science (1874). Draper (1811-1882) was a "man of science" (to use the term then current) and a historian: a chemist, an early pioneer of photography who emigrated from England to the United States in 1831, trained in medicine at the University of Pennsylvania (1836) and later a founder of the New York University Medical College (1841).[43] The second notable history of science of this period was authored by Andrew Dickson White (1832-1918), the first president of the newly-founded Cornell University – unlike Draper, a historian by training, and later a noted diplomat. White, along with his good friend Ezra Cornell (1807-1874), wished to found a secular institution in which there would be no requirement for faculty to be members of the religious ministry – a conception that met with considerable resistance in the New York State Legislature (in which he served as a state senator from 1864-67).[44] White admired Draper's book but, as he further investigated the subject for himself, he became convinced that the real conflict was between science and *theology* – hence, his title: *A History of the Warfare of Science with Theology in Christendom* (1896). By focusing on theology, White was more explicitly than Draper invoking Comte's earlier-mentioned threefold-stage theory of human knowledge. The Comtean version of positivism as the highest historical phase of civilization, manifested itself not only in the work of Draper and White but also in that of Sarton himself, although Sarton regarded some of Comte's exuberances as (literally) "crazy", such as his "Positivist Calendar" of famous historical figures.[45]

Recent scholarship has decisively undermined the Draper-White "warfare thesis" (as it is now known), but that narrative is still of historical importance as evidence of secular-religious tensions within late-nineteenth

---

43 See further, Lightman, "The Victorians," pp. 65-83.

44 David C. Lindberg and Ronald L. Numbers, "Introduction," *God and Nature: Historical Essays on the Encounter between Christianity and Science* (Berkeley and Los Angeles: University of California Press, 1986), pp. 3-18; Donald Fleming, *John William Draper and the Religion of Science*, Philadelphia: University of Pennsylvania Press, 1950.

45 "Auguste Comte was a great man, one of the greatest of his time, even if he was crazy. He was a martyr to his own genius. We should respect him and be very grateful to him. He was one of the first men to popularize the history of science. Every thoughtful user of the Positivist Calender must have tried to understand the dedication of each day, and if he did it regularly, day after day and year after year, he must have learned that many of the saints were men of science, that men of science might be saints and often were" (George Sarton, "Auguste Comte, Historian of Science: With a Short Digression on Clotilde de Vaux and Harriet Taylor," *Osiris*, 1952, *10*:357; for discussions of Comte's Positivist Calendar, see John Tresch, *The Romantic Machine,* pp. 253-256; John Brooke and Geoffrey Cantor, *Reconstructing Nature: The Engagement of Science and Religion*, pp. 47-57).

century American colleges, and beyond. Most notably, their books were hugely successful in their own time – and may even continue to be so in the popular media.[46] Draper's work alone went through some fifty printings in the United States and was translated into ten languages.[47] One of its attractions for a general readership was that, like a newspaper article, it contained no footnotes and no bibliography. Draper represented Christianity – and especially the Catholic Church – as an inherently repressive institution, completely intolerant of science. The Middle Ages, in his opinion, was a period of dogmatic scholasticism, the authority of the Church Fathers was paramount, Holy Scripture contained "the sum of all knowledge [and] discouraged any investigation of nature" and, by contrast, the cultivation of scientific knowledge was far more advanced in the medieval Islamic world.[48]

Beyond Comte's influential framework, Draper's provocative comparison of Islam to the detriment of Christianity drew upon a significant authority in mid-nineteenth century European biblical and philosophical scholarship: Ernest Renan's (1823-1892) widely-read 1852 dissertation on medieval Arabic philosophy. Renan argued that the real progress of European science and philosophy was indebted to the twelfth-century Muslim philosopher Averroës and his secular reading of Aristotle as against the alleged compatibility of Aristotle with Christian theology imposed by medieval Catholic interpreters.[49] As Harun Küçük has shown, both Draper and White incorporated Renan's sympathetic rendering of Islam and science into their accounts.[50] And then, in the late 1890s, the Renan-Draper thesis travelled into the Ottoman world when a Turkish translation of Draper's work by the journalist Ahmed Midhat (1844-1912) became a best-seller. Alper Yalcinkaya has shown that Midhat used Draper's work to promote a vision of Islam as more compatible with the sciences than Christianity and

---

46 In 1981, the Draper-White "warfare thesis" was the explicit target of David Lindberg and Ronald Numbers in their editorial introduction to the influential volume of papers offered at a conference at the University of Wisconsin and subsequently published as *God and Nature: Historical Encounters of Science and Religion* (Berkeley and Los Angeles: University of California Press, 1986), pp. 1-3.

47 Ibid., p. 2; see Lightman, "The Victorians," pp. 77-78.

48 Draper, *A History of the Conflict between Religion and Science* (New York: Appleton, 1874), pp. 157-158; 171.

49 See B. Harun Küçük, "Islam, Christianity and the Conflict Thesis," in Thomas Dixon, Geoffrey Cantor, and Stephen Pumfrey eds., *Science and Religion: New Historical Perspectives* (Cambridge: Cambridge Univ. Press, 2010), pp. 111-130.

50 Ibid., p. 115.

as essential for the good Ottoman citizen who should be learned in the European sciences and ever-obedient and deferential to the Sultan.[51]

Galileo's infamous trial by the Catholic Church in 1633 was the crucial pivot in Draper's narrative, resonating with many contemporary readers who were concerned by the First Vatican Council's official proclamation of papal infallibility under Pope Paul IX in 1869.[52] Draper's construction of Galileo was the perfect vehicle for nineteenth-century anti-clericalism:

> "On his knees, with his hand on the Bible, he was compelled to abjure and curse the doctrine of the movement of the earth. What a spectacle! This venerable man, the most illustrious of his age, forced by the threat of death to deny facts which his judges as well as [he] himself knew to be true! He was then committed to prison, treated with remorseless severity during the remaining ten years of his life, and was denied burial in consecrated ground. Must not that be false which requires for its support so much imposture, so much barbarity? The opinions thus defended by the Inquisition are now objects of derision to the whole civilized world."[53]

Of course, Galileo's judges did not believe that the earth moved and Galileo was never sent to jail.[54] But, leaving aside these exaggerations and factual distortions – by now well-known – the point to be underscored is that Draper's account was completely in harmony with the institutional developments of the 1870s. The history of the struggle between science and religion or science and theology – as well as efforts to reconcile the two spheres – was an expression of the same movement to secularize the academic curriculum, to reject the old humanism as embodied in the pre-eminence of moral philosophy and the classical language requirement and then, following Comte, to relegate religion to the status of a "stage" or "phase" prior to the emergence of positive knowledge or science.[55]

---

51 See M. Alper Yalcinkaya, "Science as an Ally of Religion: A Muslim Appropriation of 'the Conflict Thesis'," *British Journal for the History of Science*, 2011, *44*:161-181.

52 Daniel Kertzer, *The Pope Who Would Be King: The Exile of Pius IX and the Emergence of Modern Europe*, New York: Random House, 2018. Draper was reacting to Pope Pius IX's encyclical of 1869.

53 Draper, *Conflict,* pp. 171-172.

54 See Maurice Finocchiaro, "That Galileo was Imprisoned and Tortured for Advocating Copernicanism," in *Galileo Goes to Jail And Other Myths about Science and Religion*, Ronald L. Numbers ed. (Cambridge, Mass.: Harvard Univ. Press, 2009), pp. 68-78.

55 For efforts to harmonize the two spheres, see Jon Roberts, "Science and Religion," in *Wrestling with Nature*, pp. 256-265.

These historiographical trends coincided with the evolution of the scientific career as an independent reality. Almost fifty years after the appearance of Draper's *Conflict,* Huxley's debates with Arnold and John Tyndall's Belfast Address (1874), Max Weber described science as a "vocation" (*Beruf*) in a famous speech delivered at Munich in 1917.[56] In the last quarter of the nineteenth century, in England and Germany, but now too in America, the secularizing universities and the massive growth of private industry were making it possible for men – although rarely women – to enter a career in science as a specialty and without the requirement of being in holy orders or the son of a wealthy man.[57] The role-designating term "scientist" – famously coined in 1834 by William Whewell in conscious analogy with the word "artist" – acquired a secure point of social reference in the United States although it still met with mixed reception in Britain as late as the 1890s.[58] Although natural philosophy, a knowledge category of the premodern, ecclesiastical universities – and also one that for centuries presupposed Latin literacy – continued to be used by the North British physicists, eventually it dropped out of the scientific lexicon and became largely a category of historical interest. In the late-nineteenth century sense that I have been describing, one might even go so far as to say that Isaac Newton would not have recognized the vocation to which the word "scientist" referred.[59]

From the 1870s onward, as the major American and British universities gradually reduced or altogether rejected their religious associations, together with the classical language requirement, the humanistic disciplines lost their earlier institutional justification.[60] The scientific humanism that George Sarton advocated in the 1920s can be regarded as a late expres-

---

56 Max Weber, "Science as a Vocation," trans. H.H. Gerth and C. Wright Mills in *From Max Weber: Essays in Sociology* (New York: Oxford Univ. Press, 1946), pp. 129-56 (first delivered as "Wissenschaft als Beruf" at the University of Munich, 1917); Richard Yeo, *Defining Science*, pp. 34-35.

57 See Lightman, "Science and the Public," pp. 343-350.

58 See Ibid., 337-338; Sydney Ross, "'Scientist': The Story of a Word," *Annals of Science*, 1962,*18*:65-85; Laura J. Snyder, *The Philosophical Breakfast Club and the Invention of the Scientist* Washington, D.C.: Smithsonian Institution, 2011.

59 David Wootton argues that "the word 'scientist' was merely a new and useful word for a type of person who had long been in existence" (*The Invention of Science: A New History of the Scientific Revolution* [New York: HarperCollins, 2015], p. 29).

60 See Paul White, "Ministers of Culture: Arnold, Huxley and Liberal Anglican Reform of Learning," *History of Science*, 2005,*43*:115-138; David Bebbington, "The Secularization of British Universities since the Mid-Nineteenth Century," in *The Secularization of the Academy*, (New York: Oxford Univ. Press, 1992), pp. 259-277.

sion of this process. By then, “humanism” had functionally replaced religion or theology as the realm with which science was to be engaged. In fact, Sarton himself signalled that replacement in a revealing letter that he wrote to Andrew Dickson White himself in March 1918 seeking his personal support for a position at Harvard. In this letter, Sarton said quite explicitly:

> “I have tried to show that the history of science – i.e. the history of the real foundations of human progress – is not simply of immense interest in itself, but is even of greater importance in that it affords the best means of humanizing science and reconciling positive knowledge and idealism. I firmly believe that there is no other way to solve the great education problem [of] ‘science vs. the humanities’ than to introduce a little of the disinterested and historical spirit of the humanities into the scientific studies. Moreover, I have shown that to be true, the history of civilization should be focused on the history of science.”

And he concluded: “As a result of my work since 1911, I am now a recognized leader and authority in the history of science not simply in America, but abroad.”[61]

Sarton’s supplicating letter shows that the kind of humanism he was defending in the interwar period took for granted an entirely secular justification.[62] In this vision, the history of science was essential to the scientist’s understanding of his or her own subject, but also desirable for students of the humanities.[63] Indeed, in attacking (unnamed) “humanists of the old type,” Sarton was also offering an alternative solution, if somewhat obliquely, to the problem of justifying humanistic subjects against the

---

[61] George Sarton to Andrew Dickson White, March 31, 1918, Andrew Dickson White Papers, Division of Rare and Manuscript Collections at Cornell University Library (hereafter cited as: White Collection, and reel number 124. Cited by James C.Ungureanu, https://jamescungureanu.wordpress.com/2017/02/14/george-sartons-appeal-to-andrew-d-white/. Consulted Sept. 29, 2018.

[62] Ultimately, he won the support of the Carnegie Foundation and was able to spend the remainder of his career at Widener Library, Harvard University (Thackray and Merton, “On Discipline Building: The Paradoxes of George Sarton,” *Isis*, 1972,*64*:489-90).

[63] Tacitly, it also included nonspecialist patrons of learning, such as university and government administrators and private collectors of rare scientific books whose patronage he cultivated. See especially Arnold Thackray, “The Pre-History of an Academic Discipline: The Study of the History of Science in the United States, 1891-1941,” *Minerva*, 1980, *18*: 458-461; *The Dibner Library of the History of Science and Technology at 25 Years: Celebrating a Collector’s Vision and Its Legacy*, Washington, DC: Smithsonian Institution Libraries, 2001.

encroachment of science into the academic curriculum and to the disruptive effects of a technocratic society.[64]

From Interwar Britain to 1960s America

When C.P. Snow and Jacob Bronowski were students at Cambridge in the late 1920s, both secular and religious meanings of the term "humanism" were in use. Between the 1930s and 1950s, a small group of writers at Oxford, calling themselves "The Inklings," styled themselves "Christian humanists." Among its most famous members were J.R.R. Tolkien and C.S. Lewis. They were deeply suspicious of what they regarded as the evils of modern industrial civilization and its direct association with science. Their vitalist, anti-reductionist views of the nature of life harkened back directly to nineteenth-century conflicts between theistic natural philosophers and materialist scientific naturalists. Chief on their enemies list was the brilliant John Desmond Bernal (1901-1971), an X-ray crystallographer whose seminal work on the structure of proteins laid the foundations for a profoundly new approach to the understanding of life. Bernal's student, Max Perutz (1914-2002), was the supervisor of Francis Crick (1916-2004). Robert Bud attributes the opposition between the Christian humanists and the Bernalians largely to the experience of the Great War: "Religious believers mourning their dead were comforted by thoughts of meaning, of life eternal and of self-sacrifice. To others, the emergence of the Soviet Union gave hope for a new age, a new economy and a new culture here on Earth. European societies, then, were sharply divided between conserva-

64 Sarton was frustratingly spare in his citation of sources. But, for a salient example of the problems faced by humanistic studies after the loss of their earlier institutional justification, see esp. Irving Babbitt, *Literature and the American College* (Boston: Houghton Mifflin, 1908): "The humanities need to be defended today against the encroachments of physical science, as they once needed to be against the encroachments of theology" (p. 31); "Some of our higher institutions of learning are in a fair way to become what a certain eminent scholar thought universities should be – 'great scientific workshops'" (pp. 90-91); "In the educational institutions, especially the large universities of the Middle West, the men flock into the courses on science, the women affect the courses in literature" (p. 118); "… at Leland Stanford University a student may enter, not only without Latin and Greek, but without any language or non-scientific subject whatsoever except English composition, and then receive his Bachelor of Arts degree on completing a certain number of hours' work in mechanical engineering. At this rate, the Bachelor of Arts degree may soon come to be granted to a student as a reward for getting his professional training as a plumber!" (p. 210). For the situation a half century later, see John Higham, "The Schism in American Scholarship," *The American Historical Review*, 1966,*72*:16.

tives with strong religious views and modernizers keen to disenchant the world."[65]

C.P. Snow was clearly in the modernizing camp. His conceptualization carries significant residues from his formative period at Cambridge in the 1930s when, as Stefan Collini has argued, he was influenced especially by the radical, left-wing physicists Bernal and Patrick M.S. Blackett (1897-1974):

> "He saw science as the great hope in a world which the traditional elites had mismanaged and led into economic depression and to the brink of a second devastating war. He also saw it as the one true meritocracy, in which sheer ability could overcome social disadvantages to obtain its true reward. And in more parochial terms, the young Snow developed an antipathy to 'literary intellectuals', especially to what he identified as their snobbish and nostalgic social attitudes, which was never to leave him."[66]

Among other recent commentators, Guy Ortolano has observed that Snow's superficially historicized conception was "an example of the tendency to drape [a] venerable argument in the language of novelty and urgency."[67] He argues further that Snow was a "technocratic liberal" and that the real issue at stake in post-war Britain was a political and economic one: equal opportunity for social ascent by virtue of merit rather than class and inherited wealth.[68]

Meanwhile, 1960, the year after Snow's Rede Lecture, was no ordinary moment. The Catholic John F. Kennedy broke the long Protestant domination of the presidency and, soon after, his outgoing predecessor Dwight D. Eisenhower, delivered his "military-industrial complex" speech. The Reverend Martin Luther King, Jr. was actively mobilizing the civil rights campaign against racial segregation and economic injustice. The Governor of California, Edmund "Pat" Brown, pushed through the state legislature a "Master Plan for Higher Education" which stated that "some form of higher

---

65 Robert Bud, "Life, DNA and the Model," *The British Journal for the History of Science*, 2013,*46*:312.

66 Stefan Collini, Introduction to C.P. Snow, *the Two Cultures* (Cambridge: Cambridge Univ. Press, 1998), p. xxiii.

67 Guy Ortolano, "F.R. Leavis, Science and the Abiding Crisis of Modern Civilization," *History of Science,* 2005,*43*:181n5.

68 Guy Ortolano, *The Two Cultures Controvery*, pp. 24-25; 28-30.

education ought to be available to all regardless of their economic means, and that academic progress should be limited only by individual proficiency."[69] This radical program of free tuition opened the doors of a major public university to students from all socioeconomic classes. Other major post-war American universities, while not suspending tuition, began to open up to previously excluded or underrepresented groups, among which were Jews, and with that development the "de-Christianization" of academic faculties accelerated in previously restricted disciplines.[70] Within a few short years, the San Diego campus, built on the site of a World War II military camp, became just one of many sites of student protest against military intervention in Vietnam.

C.P. Snow's "Two Cultures" thus landed in the new moment not as a challenge to religious authority, as it might have done a century earlier, but as a metonym for disciplinary and religious pluralism as well as interdisciplinary research collaborations.[71] However, in praising scientists for their greater openness and in specifically targeting modernist literary intellectuals – "the English Department," as David Hollinger has quipped – Snow's conceptualization left hardly any place for historians and social scientists.[72] Indeed, although Snow explicitly spoke of "The Scientific Revolution," his reference was to the 1920s and 30s and, especially, to the enormous gap in wealth and opportunity between the industrialized and non-industrialized countries.[73] This designation entirely overlooked the subject of the history of science, its evolution over the previous century and Sarton's aspirations to make it the premier "bridge" subject.

---

69 https://en.wikipedia.org/wiki/California_Master_Plan_for_Higher_Education. Consulted July 10, 2019.

70 See David Hollinger, "Jewish Intellectuals and the De-Christianization of American Public Culture in the Twentieth Century," in *Science, Jews, and Secular Culture* (Princeton: Princeton Univ. Press, 1996), pp. 17-41; Jerome Karabel, *The Chosen: The Hidden History of Admission and Exclusion from Harvard, Yale, and Princeton*, Boston/New York: Houghton Mifflin, 2005.

71 On the rise of interdisciplinarity, see Jamie Cohen-Cole, "The Creative American: Cold War Salons, Social Science, and the Cure for Modern Society," *Isis*, 2009, *100*:253-254.

72 In 1963, Snow acknowledged as much in his candid *The Two Cultures: A Second Look* (Cambridge: Cambridge Univ. Press, 1963), pp. 70-71; D. Graham Burnett, "A View from the Bridge: The Two Cultures Debate, Its Legacy and the History of Science," *Daedalus* 1999, *128*:193-218; Cynthia Pyle, "The Two Cultures and Renaissance Humanism," *Interdisciplinary Science Reviews*, 2008, *33*:122.

73 Snow, *Two Cultures*, (chap. 4: "The Rich and the Poor"), p. 41.

Jacob Bronowski's "Two Cultures" and the History of Science.

As a laboratory research scientist with strong aesthetic intuitions but little formal humanistic training, Jonas Salk was in a limited position to appreciate either the long historical lineage of Snow's formulation or the immediate controversy that it spawned. But as the founder of a new institute, he clearly understood that he needed someone who could help him to interpret and develop Snow's formulation in relation to the objectives of a modern biological research laboratory. And this was the mission that fell to C.P. Snow's immediate Cambridge contemporary Jacob Bronowski.

Like the scientists who were lured to Jonas Salk's fledgling institute in 1962, Bronowski's life experience, as with most of the other members of the Institute, was coloured by World War II.[74] Immediately after the atomic bomb was dropped, the British government sent him to Nagasaki to survey and report on the damage and its consequences for future British defence policy. The scene he found shook him deeply and affected him for the rest of his life.[75] In the years after the war, when his public reputation as a radio science commentator was rising, moral questions raised by the use of the bomb on a civilian population became a central consideration: Was science inherently a force for good or evil? Should scientists themselves have played an active role in the decision to drop the atomic bombs on Japan? "Science and Human Values," the title that Bronowski chose for his 1953 lectures, featured these kinds of moral questions. What were the values of the scientists who were capable of creating a previously unimaginable weapon that could destroy the inhabitants of a whole city? For Bronowski, the basic question was: Were these professionals acting on the basis of values that were *inherent* in the vocation of science itself? Were scientists themselves morally complicit actors?[76] These last questions, as Charles Thorpe has shown, were already anticipated by Max Weber at the end of

---

[74] See Bourgeois, *Salk Institute,* pp. xxvii-xxxiv.

[75] Jacob Bronowski, *Science and Human Values* (New York: Harper Torch, 1965; first pub. 1956), pp. 3-4; idem, "The Psychological Wreckage of Hiroshima and Nagasaki," *Scientific American*, 1968, *218*:131-135.

[76] Bronowski, *Science and Human Values*, pp. 70-71: "[M]assacre is not prevented by sticking to gunpowder; the Thirty Years' War is proof of that. Massacre is prevented by the scientist's ethic, and the poet's, and every creator's; that the end for which we work exists and is judged only by the means which we use to reach it … it is not the scientist who can govern society; his duty is to teach it the implications and the values in his work." Ian Hacking reads Bronowski as "wanting to restore the Enlightenment vision of science," "science as a human endeavor." *The Social Construction of What?* (Cambridge, Mass.: Harvard Univ. Press, 1999), p. 61.

World War I in his reflections on the scientist's vocation as a value-sphere of secular, specialized experts devoted to discovering facts and laws about the natural world but entirely separate from the violence of the political sphere.[77] But with the development of the atomic bomb, World War II became what Daniel Kevles has called "a physicists' war" and the experience of the physicists who created the bomb now urgently raised anew the problem of the relationship of scientists to their vocation.[78]

In turn, these morally-urgent mid-century problems raised a further question for historians: how should the narratives of the history of science be told? And who should tell them? Did the problems facing post-war scientists – especially physicists – originate in the twentieth century or were they already manifest in the age of Copernicus, Kepler, Galileo, Descartes and Newton? From the perspective of Europe, with many of its major cities in ruins, the sixteenth and seventeenth centuries appeared to represent something creative and unique: the origins of a peaceful and profound conceptual "revolution", a radical metaphysical transformation of thought – indeed, the site of the origins of modern science itself.[79] Thus, as Europe rebuilt its bombed-out cities, it was not the Marxist class revolution, which had inspired J.D. Bernal's vision of a socially contextualized history of science, but rather a purely intellectual revolution that provided the foundations of the historiography of the 1950s and 60s – and that would significantly influence Bronowski's thinking.

Bronowski's political connections and access to academic networks sheds helpful light on how he would seek to fulfill his mission at the Salk Institute, including his use of the history and philosophy of science. After the war, just as his career as a public intellectual was on the rise, Bronowski was already quite well-connected in British government circles – espe-

---

77 See Charles Thorpe, "Violence and the Scientific Vocation," *Theory, Culture and Society* 2004, *21*:59-84.

78 Citing an interview with Lee DuBridge, Daniel Kevles wrote: "The atom bomb only ended the war. Radar won it" (*The Physicists: The History of a Scientific Community in Modern America* [New York: Vintage, 1971], pp.137, 320); Roy MacLeod, "The Great War and Modern Science: Lessons and Legacies," *Sartoniana* 2015, *28*:13-32.

79 Frank James rightly observes that Herbert Butterfield, Rupert Hall and Marie Boas, although writing about the "origins" of modern science, completely ignored the impact of scientific developments on the modern world ("'The Springtime of Science': Modernity and the Future and Past of Science," in *Being Modern*, pp. 140-142). Of course, nineteenth-century historiography of science, looking back to Whewell, already gave considerable weight to the seventeenth century. See H. Floris Cohen, *A Historiographical Inquiry* (Chicago: Univ. of Chicago Press, 1994; I. Bernard Cohen, *Revolution in Science* (Cambridge, Mass.: Harvard Univ. Press, 1985).

cially with the Labour Party. In 1956, Bronowski, Snow and Bernal were all members of a policy study group led by the Labour Party leader Hugh Gaitskell and where the two-cultures theme was nascent. Their charge was to formulate Labour's positions on science at a moment when there was much anxiety about Britain's decline as a major power and in anticipation of the time when the Party would be returned to power. The group's main recommendation was that the leadership of the government must possess the necessary scientific literacy to be able to make well-informed decisions about such matters as the peacetime use of nuclear energy or which areas of medical research to fund – training that the traditional arts curriculum did not provide. Significantly, the group advocated diverting the annual allocation of 1.5 billion pounds to non-military scientific work rather than to the armed forces and further development of the atomic bomb.[80]

But aside from these high-level, active political associations, Bronowski was socially and intellectually extremely well-connected in the British and American academic and literary worlds. This is clear both from his reading notes and from the many prominent names that appear in his diaries of the mid-1960s, soon after he arrived at the Institute. Among those mentioned are: the historian and philosopher of science, Stephen Toulmin;[81] the evolutionary biologist Julian Huxley;[82] the novelist Aldous Huxley;[83] the Cambridge literary critic I.A. Richards;[84] the Marxist historian, Eric Hobsbaum;[85] and, at Harvard, the historian of science I. Bernard Cohen and the physicist-historian, Gerald Holton.[86] But most prominently, the philosopher of science, Karl Popper – a frequent visitor to the Salk Institute. References to Popper abound in Bronowski's diaries between 1966 and 1967[87] as well as many entries for Popper's brilliant Hungarian protégé, Imre Lakatos.[88] And, after his move to the United States, there are

---

80 Lisa Jardine, "The Sorcerer's Apprentice: C.P. Snow and J. Bronowski," (*The Tanner Lectures on Human Values*, New Haven, CT, 2012), p. 99, 104, 119; Guy Ortolano, *The Two Cultures Controversy*, pp. 173-177.

81 Jesus College, Bronowski Collection, Bronowski diary, Feb. 5, 1962. Hereafter cited as "JCB".

82 JCB, Bronowski diary, December 16, 1963.

83 JCB, Bronowski diary, December 15, 1963.

84 JCB, Bronowski diary, Sept. 21, 1962.

85 JCB, Bronowski diary, July 12, 1962.

86 JCB, Bronowski diary, February 23, 1963.

87 JCB, Bronowski diary, July 28, 1966; January 24, 1967. Popper spent two months at the Institute in the Winter, 1966 (Bourgeois, *Salk Institute*, p. 162).

88 JCB, Bronowski diary, 1965: January 25, February 4; April 22; May 22; June 30; August 11; November 25; December 8, 20; 1966: August 16, October 14; November 24.

numerous references to Paul Oskar Kristeller,[89] the Columbia University historian of Renaissance philosophy and humanism. In addition, a symposium at the Institute on the work of the scientist-philosopher, Michael Polanyi;[90] and, at the newly established campus of the University of California, San Diego, there are entries for the philosopher Richard Popkin,[91] historian of scepticism and first chairman of the Department of Philosophy as well as to Popkin's famously radical colleague, Herbert Marcuse.[92]

This was not Jonas Salk's intellectual world. Unlike Salk, Bronowski circulated among many of the same intellectual and political networks of the British intelligentsia as did C.P. Snow. Indeed, Bronowski's daughter Lisa Jardine convincingly argued that key elements of the two-cultures formulation were already anticipated by her father – a credit only acknowledged by Snow himself in 1963.[93] Furthermore, unlike Snow and Salk, Bronowski actively followed the historiography and philosophy of science of the 1950s and 60s when the metaphor of "scientific revolution" was at the height of its popularity and especially as it developed under the influence of the anti-Marxist Russian émigré and philosophical historian Alexandre Koyré and his Cambridge acolyte and influential populariser, Herbert Butterfield.[94] Bronowski also monitored the debates between Thomas Kuhn and Karl Popper in which Kuhn defended a politically safe, non-Marxist sociology focused on the early training of scientists socialized through the learning templates of textbook problem-solving ("normal science").[95] Although Kuhn claimed that Popperian falsification regularly occurs in normal science, he also maintained that the paradigm's basic tenets are never questioned. Popper adamantly rejected this radical account

89 JCB, Bronowski diary, 1965: May 15; October 27; November 12; December 16; 1966: February 19; April 8
90 JCB, Bronowski diary: March 6, 1966
91 JCB, Bronowski diary, 1965: July 4, 12, 18, 26; August 9, 15; September 7, 21.
92 JCB, Bronowski diary, 1966: December 30; 1967: February 14; July 3, 27; December 29.
93 L. Jardine, "The Sorcerer's Apprentice," pp. 88-94; C.P. Snow, *The Two Cultures: A Second Look* (1963), pp. 55-56.
94 Snow's reference to the "Scientific Revolution" owed nothing to the prevailing Koyrean usage of the 1950s. On Koyré's method, see Nick Jardine, "Koyré's Kepler, Kepler's Koyré," *History of Science* 2000, *38*:363-376; H. Floris Cohen, *The Scientific Revolution*, pp. 73-88.
95 Bronowski's first reference to Kuhn's *Structure of Scientific Revolutions* occurs in a diary entry for January 25, 1966 (JCB), six months after the London conference that featured dueling presentations between Kuhn and Popper as well as memorable contributions by Lakatos, Toulmin and Paul Feyerabend (*Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan Musgrave, Cambridge: Cambridge Univ. Press, 1970).

as "dogmatic" for its denial of the principle that a conceptual framework is scientific by virtue of its vulnerability to criticism and potential rejection.[96]

These reading interests and evolving scholarly commitments fail to align predictably with Bronowski's political views of the 1930s and 40s, especially those of J. D Bernal. In 1941, for example, Bronowski presented a lecture on Marxism to the Socialist Group at University College Hull; and in 1943 he joined the Ministry of Home Security's Research and Experiments Department under Bernal's own direction.[97] It was in this capacity that Bronowski conducted his statistical analyses of the bombing of German industrial targets.[98] Bernal, like a number of other intellectuals of the 1930s (including the Sarton-trained sociologist Robert Merton), had been powerfully impressed by the Soviet physicist Boris Hessen's famous lecture, delivered in 1931 at the Second International Congress of the History of Science in London.[99] Hessen argued that the problems that preoccupied Newton were created by the forces of production that characterized the state of late-seventeenth century capitalist development – problems of warfare, mining, ship-building and manufacturing. One way or another, all these problems involved questions involving the nature of force; and the analysis of force, of course, lay at the heart of Newton's novel mathematical solution to the conceptual inadequacies of both ancient and contemporary natural philosophy.[100] Because of Bronowski's personal relationship with Bernal during the war, it would be interesting to know how exactly he regarded Bernal's Hessen-inspired emphasis on the social context of science. Indeed, although Bernal, like Bronowski was conversant with post-war Koyrean historiography, he was largely alone in urging that, "The decisive role of science in shaping the future of the world is no

---

[96] "In my view the 'normal' scientist, as Kuhn describes him, is a person one ought to be sorry for … The 'normal' scientist, in my view, has been taught badly. I believe, and so do many others, that all teaching at the University level (and if possible below) should be training and encouragement in critical thinking. The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination" (Karl Popper, "Normal Science and Its Dangers," in *Criticism and the Growth of Knowledge*, pp. 52-53).

[97] Desmarais, "Jacob Bronowski," p. 576.

[98] Ibid.

[99] Boris Hessen, *The Social and Economic Roots of Newton's* 'Principia', New York: Howard Fertig, 1971; see also Gary Werskey, *The Visible College: A Collective Biography of British Scientists and Socialists of the 1930s*, London: Allen Lane, 1978.

[100] Hessen had his own difficulties with the "vulgar Stalinist" party line which his account skillfully side-stepped; see Loren Graham, "The Socio-Political Roots of Boris Hessen: Soviet Marxism and the History of Science," *Social Studies of Science* 1985, *15*: 705-722.

longer in question. For its wise use, it will still be of value to study its history in its social context."[101] Yet, Bronowski's writings bear no significant evidence of Bernal's approach.

Instead, upon his arrival at the new Institute in La Jolla in 1962, under the influence of Salk's nascent vision, Bronowski began to forge a quite new framing of C.P. Snow's two-cultures in the form of an *evolutionary* account of the history of science. In designating the history of science as the right path for bridging science and the humanities, Bronowski shared a common, although unacknowledged goal with George Sarton. But, unlike Sarton, Bronowski assigned a central role to literature, biology and the philosophy of science. The source of Bronowski's central emphasis is apparent in the foreword to his 1973 book *The Ascent of Man*:

> "There has been a deep change in the temper of science in the last twenty years: the focus of attention has shifted from the physical to to the life sciences … the interested spectator is hardly aware yet how far-reaching the effect is in changing the image of man that science moulds. As a mathematician trained in physics, I too would have been unaware, had not a series of lucky chances taken me into the life sciences in middle age."[102]

That "lucky chance" was, of course, Jonas Salk's invitation to join his new enterprise.[103] In following a path from physics to biology, Bronowski was following a career shift already pioneered by, among others, two highly eminent physicists, Leo Szilard and Max Delbrück.[104]

As early as 1960, Bronowski had proposed to Salk a "Department of Humane Study" that would concern itself with the philosophy of the biological sciences committed to "the study of human aspirations and values, seen as a natural expression of the biological nature of man."[105]

---

[101] J. D. Bernal, *Science in History* (London: Watts, 1954; 1957), p. xvi. Bernal's bibliography contains no references to Bronowski. Nonetheless, it shows that he had kept well abreast of the recent idealist historiographical literature – including, for example, Koyré's Études galiléenes (1939) and Rupert Hall's *The Scientific Revolution* (1954) – in addition to many works by Marx, Stalin and Lenin that do not appear in the former.

[102] Jacob Bronowski, *The Ascent of Man* (Boston: Little Brown, 1973), p. 13.

[103] Salk flew to England on July 9, 1960 and, soon after, met with Francis Crick, Jacob Bronowski and C.P. Snow (Bourgeois, *Salk Institute*, p. 96).

[104] On Delbrück, see Daniel J. McKaughan, "The Influence of Niels Bohr on Max Delbrück: Revisiting the Hopes Inspired by 'Light and Life'," *Isis* 2005, *96*:507-529; Bourgeois, *Salk Institute*, pp. 41-46.

[105] Jacob Bronowski to Jonas Salk, JCB 10/1/1, October 7, 1960.

Once Bronowski arrived in La Jolla, he began to marshal his many social connections and ideas to advance the Salk Institute's humanistic mission. However, his friend C.P. Snow would not be personally involved. In 1965, he had suffered a heart attack and remained a non-resident fellow.[106] Thus, the Institute's humanistic project would be left primarily under Bronowski's control, constructed entirely in Bronowski's own image and exceptionally well fitted to one of Bronowski's comfort zones: the popular media. Even as Bronowski successfully organized symposia and lectures for the Institute's Council for Biology in Human Affairs, devoted to informing the public about the social consequences of biological discovery, these activities were interrupted in 1968, by a proposal that he could not refuse.[107] He was approached by the BBC to do a TV documentary on the history of science, modelled after Kenneth Clark's highly successful art history series *Civilisation* (first aired in 1969). Bronowski could not have chosen a more natural assignment. It also fit perfectly with one of Salk's central aims – to communicate science to the general public – and it drew him directly to a subject that he had long followed and occasionally indulged. For the final few years of his life, he would devote enormous energy to it.

It should be emphasized that *The Ascent of Man* was intended principally for an early television audience rather than for the classroom. Viewers could not yet pre-record programs. In this young, evolving medium a viewer might tune in at any moment in the presentation and would need to be held captive by some clever aside or visually attractive scene. Not all-American audiences were drawn favourably to Bronowski's high-brow British style. But what made *Ascent* a model to be emulated was his inherent sense of narrative timing and his undeniable pedagogical skill in presenting accessible explanations of deep scientific concepts, enhanced throughout by classical period music and extraordinary filming locations. Later efforts in this genre, such as those of Carl Sagan's *Cosmos* (1980), and James Burke's *The Day the Universe Changed* (1985) faced similar challenges of sustaining viewer attention; only David Goodstein's *The*

---

[106] Jacobs, *Jonas Salk*, p. 244.

[107] For brief discussion of the Council's financial difficulties, see ibid., pp. 356-57; for some important successes of the Council's program, see Bourgeois, *Salk Institute*, pp. 161-171.

Mechanical Universe (1985) was explicitly staged in a classroom setting and produced as a "telecourse" with an accompanying textbook. For all that, Bronowski's early effort set the example and can still be viewed (or read) with profit as much for its sweeping embrace of the entire history of western science as for its pedagogical value.[108] Of course, the great majority of its claims would have to be critically modified or rejected in the light of subsequent scholarship. That aside, *Ascent*'s relevance for this study is that it was explicitly engaged with the two-cultures theme and at just the moment that the Salk Institute was in its early, formative phase.

The first episode begins with the biological evolution of the human species. How did humans succeed in adapting to their harsh environment? How did they secure their food supply? What did they do when the Ice Age descended? In short, what pre-historical adaptations allowed humans to survive? Throughout the whole account, Bronowski inflects what he takes to be a unique human attribute: the power of the mind to make images, such as primitive cave paintings and other artistic representations, to visualize and thereby to anticipate the future. For Bronowski, this emphasis on imagining or image-making lay at the heart of *cultural* evolution. It developed, first and foremost, from people working with concrete, material objects, like stones (for weapons or houses or churches) or using fire to process copper (for making swords) or gold (for making crowns, jewellery and other ceremonial objects) or clay to make pottery. Bronowski continued this theme when his narrative reached the pre-Socratic Greek philosophers, the classic starting point of the history of science. The Greeks were divided on the question of where true reality lay – in the form or the matter of an entity. Giving form to matter evolved into the capacity for making abstract representations, i.e. thinking about forms, such as geometrical structures, apart from any material substrate. Science was, then, to be seen as the highest expression of cultural evolution and science, like art, involved the active use of the imagination.

[108] *The Ascent of Man*; You Tube. In 1975-76, Bronowski's series was assigned for credit in several hundred colleges. See Nasser Zakariya, *A Final Story: Science, Myth and Beginnings* (Chicago: Univ. of Chicago Press, 2017), pp. 286-87.

Bronowski's historical narrative side-stepped both Kuhn's philosophically sophisticated account of scientific revolutions and Bernal's much cruder gestures at social contextualization in favour of Karl Popper's method of conjectures and refutations.[109] Popper rejected the view that science should seek confirmations of hypotheses because one could never be certain that all particular instances of a hypothesis had been found. And the singular instance that had not been found might negate the hypothesis. For example, if one receives a grant to study the general proposition that "all swans are white" and only white swans are observed, one is not justified in concluding that the proposition has been confirmed. A black swan might yet be discovered. Therefore, Popper argued the reverse: that one should seek to falsify claims about nature; if a proposition survives efforts to refute it, then its probability is thereby strengthened and that is how knowledge grows. Essentially, scientific knowledge grows by a method of criticism. Absolute truth is ultimately out of final reach and science is an unending quest.

But where do conjectures or hypotheses come from? Logical positivists, such as Hans Reichenbach (1891-1953) – but also the falsificationist Popper – regarded the discovering of new ideas as a question to which philosophy of science could give no logical answer. That problem was relegated to what was called the "context of discovery" as opposed to the "context of justification."[110] Bronowski's predictable reply was that new ideas come from the imagination: "The scientist or the artist takes two facts or experiences which are separate; he finds in them a likeness which had not been seen before; and he creates a unity by showing the likeness."[111] C.P. Snow's two cultures were thus united by an act of imagination in the context of discovery.

One of Bronowski's favourite historical examples was that of Copernicus. In *The Ascent of Man*, Bronowski's Copernicus asks: "Why are the paths

[109] See David R. Topper, "Jacob Bronowski: A Sketch of His Natural Philosophy," *Leonardo*, 1979, *12*:51-53.

[110] See Hans Reichenbach, *The Rise of Scientific Philosophy*, Berkeley: Univ. of California Press, 1951.

[111] Bronowski, *Science and Human Values*, p. 27. Bronowski's account was not inconsistent with the physicist Wolfgang Pauli's idea – familiar to Bronowski at least from July 7-8, 1963 (JCB) – that the source of symbolic images comes from within, that it lies in the collective unconscious and is made manifest in dreams. Bronowski cites Pauli's 1952 essay on Kepler and Fludd in the 1965 edition of *Science and Human Values* (p. 22n).

of the planets so complicated?" And his answer was: "Because we are looking from where we happen to be standing" – to which Copernicus then supposedly asks: "Why not look from another place, the sun?"[112] In Bronowski's construction of the "context of discovery", here was a premier example of the creative scientist, like the artist, actively forging new connections among old data and finding simplicity in a leap of the imagination. Copernicus, in this plot, had "good Renaissance reasons" for his proposal – notably, the alleged humanist rejection of authority. Bronowski's explanation resonated with George Sarton's secular "new humanism" of the 1920s and in the episode on Galileo incorporated, quite explicitly, the ever-ready foil of the Church's dogmatic authority that it shared with Draper and White's "warfare" narrative.[113] But, in its focus on ideas devoid of social context it was also clearly influenced by the new, more sophisticated Koyrean historiography of the 1950s and 60s – in this case Thomas Kuhn's *The Copernican Revolution* (1957), which Bronowski had read at least by 1965.[114]

In his account of the nineteenth and twentieth centuries, Bronowski reversed the arrows of influence between the "two cultures." Rather than the "external" stimulus of Renaissance humanism, science itself became the source of new conceptions for literature and art. For example, the early nineteenth century Romantic poets appropriated the "new concept of nature as the carrier of energy" exemplified by the steam engine. "They loved the word 'storm' as a synonym for energy," Bronowski exudes, "in phrases like *Sturm und Drang, 'storm and thrust'."* And: "Poets and painters were suddenly captured by the idea that nature is the fountain of power, whose different forms are all expressions of the same central force, namely energy."[115] Here, the unstated message was that the *literati* were both knowledgeable about science and willing to metabolize its concepts within their own productions. At the same time, Bronowski emphasized the Snovian theme of the Industrial Revolution as a social revolution that,

112 Bronowski cites Kuhn's *The Copernican Revolution* (1957) in *The Ascent of Man*, p. 196; he also cites Kuhn in *Science and Human Values*, p. 12n.

113 See esp., "The Starry Messenger" (episode 6).

114 Bronowski, *Science and Human Values*, p. 22n.5; following E.A. Burtt, Kuhn had stressed what he described as "Neoplatonic elements in the new humanism" (*The Copernican Revolution* [1957], pp. 127-131); Edwin Arthur Burtt, *The Metaphysical Foundations of Modern Physical Science*, New York: Doubleday, 1924 (rev. ed., 1932), pp. 53-56.

115 Bronowski, *Ascent of Man*, pp. 282-285.

whatever its cruelties, eventually helped to bring about equal rights. Contrary to Bernal's view of the evils of industrial capitalism, Bronowski argued:

> "Where would a man like me be, where would you be, if we had been born before 1800? We still live in the middle of the Industrial Revolution and find it hard to see its implications, but the future will say of it that in the ascent of man it is a step, a stride, as powerful as the Renaissance. The Renaissance established the dignity of man. The Industrial Revolution established the unity of nature."[116]

Then, at the close of the nineteenth century and the beginning of the next, Bronowski portrays a cascade of revolutionary developments in physics that would become a fundamental inspiration for the painters of the early twentieth century. With J.J. Thomson's discovery of the electron, the atom was shown to be divisible; Roentgen's X-rays revealed bone beneath the skin; Ernest Rutherford imagined the invisible atom to be a miniature solar system with electrons orbiting the heavy nucleus; and, in 1913, Niels Bohr synthesized the Rutherford atom with Max Planck's notion that energy comes in lumps or quanta, interpreting the atom's visible spectrum as a kind of "stained-glass window" through which to observe its "fingerprint."[117] For Bronowski, all these discoveries were acts of imagination, of picturing by analogy.[118] And, in turn, such models of the invisible realm inspired the Cubists, led by Pablo Picasso, to shift attention below the surface of appearances to the underlying architecture of the sensory world.

Yet, unlike Koyré's narrative of revolutions as deep, metaphysical transformations, first set forth in his *Études galiléenes* (1939), Bronowski did not leave out the central ethical dilemma of his own era. Arguably, the most powerful episode in *The Ascent of Man* addresses the other side of scientific humanism: the ethics of scientists themselves and the use of their discoveries for the destruction of life. It comes near the end of the series when Bronowski visits the remains of the Auschwitz concentration camp where many members of his own family had perished. Dressed formally in a suit and tie, he approaches a rain-soaked field where the Nazis had thrown

[116] Ibid., p. 286.
[117] Ibid, pp. 332-336.
[118] Ibid., p. 340.

the ashes of the dead. And in a scene whose monologue was unrehearsed beforehand, he wades into the water, stoops down, picks up a handful of mud and speaks:

> "It is said that science will *dehumanise* people and turn them into numbers. That is false, tragically false … This is where people were turned into numbers. Into this pond were flushed the ashes of some four million people. And that was not done by gas. It was done by arrogance. It was done by dogma. It was done by ignorance. When people believe that they have absolute knowledge, with no test in reality, this is how they behave."[119]

Survival, Bronowski here argues, occurs thanks to the unique human capacity to imagine and to test. State actors, he implied, and not scientists themselves, were responsible for applying the products of science to evil ends. Against the backdrop of Truman's decision to drop the atomic bomb and Bronowski's first-hand assessment of the bomb's damage at Nagasaki, Bronowski held up his Salk Institute colleague Leo Szilard as an example of the scientist's moral rectitude in the face of hopeless odds:

> "Always Szilard wanted the bomb to be tested openly before the Japanese and an international audience, so that the Japanese should know its power and should surrender before people died … Szilard failed, and with him the community of scientists failed. He did what a man of integrity could do. He gave up physics and turned to biology – that is how he came to the Salk Institute – and persuaded others too."[120]

In representing Szilard as the paragon of the ethical physicist-turned-biologist, Bronowski can be read as implicitly speaking about himself and the unprecedented dilemmas in which scientists found themselves in the postwar era.[121] Indirectly, he was also defending the central role played by

---

119 Ibid., p. 374 (my italics).

120 Ibid., p. 370.

121 Desmarais is extremely critical of Bronowski's postwar media presentations for failing to reveal his own role in providing statistical assessments for the bombing of Germany and Japan and for neglecting to mention Britain's atomic weapons program while promoting the peaceful uses of atomic energy (Desmarais, "Jacob Bronowski"). But he overlooks Bronowski's citation of Szilard and entirely neglects Bronowski's experience of the Holocaust.

Jewish scientists, many of them refugees from the Holocaust, in constructing the bomb. And he was also trying to make sense of how a society like Germany, where he had spent his early childhood and that had produced so many profound scientific discoveries, could have created a nightmare.

Finally, although the two-cultures theme had initially drawn Bronowski and Salk together, there is not a single mention of Jonas Salk himself in the thirteen-part *Ascent of Man*. A partial explanation for this strange omission lies in the genre in which that production was conceived. Although engaged with the two-cultures theme, *Ascent* still retained its triumphalist roots in the great man/great discoveries narratives of Comte, Draper and White, and Sarton.[122] Thus, the debate within the medical research community about whether polio could be prevented by the killed vs. the live virus belonged in a different category from the discovery of the structure of the DNA molecule. In fact, it is now known that the Nobel Prize committee did consider Salk and concluded that the proof that inactivated poliovirus could confer immunity was “nothing new.” And further: Salk had “exploited discoveries made by others.”[123] In Kuhn’s terms, Salk was engaged in normal problem-solving activity in which the scientific community already knows what the world is like.[124] Thus, although Jonas Salk became widely known and acclaimed by the public as the healing, constructive face of science in the middle decades of the Cold War, the Nobel Committee did not give prizes for “normal science”.[125]

Nonetheless, by 1973, when Bronowski’s series first aired, Salk’s vaccine had literally saved millions of lives and the general public viewed his achievement not with the suspicion of the current “anti-vaxxer” movement but against the backdrop of the recent allied victories in World

122 Thomas Kuhn’s *Structure of Scientific Revolutions* begins with the striking claim that science textbooks function as ideological vehicles for a misleading image of science and its history, an image of science something like a tourist brochure that erases all but great landmarks (p.1).

123 Erling Norrby, *Nobel Prizes and Life Sciences* (Singapore: World Scientific Publishing, 2010), pp. 139-140; see also, Jacobs, *Jonas Salk*, p. 212.

124 *The Structure of Scientific Revolutions*, p. 5.

125 Four years after Salk’s death, in 1999, at the invitation of KPBS, San Diego public television, Bronowski’s daughter Judith, an accomplished film producer, produced *Jonas Salk: Personally Speaking*. The film movingly and effectively tells the polio story. It celebrates Salk’s courage, his dedicated and dogged determination to develop a vaccine based upon killed virus, and, in a famous interview with the TV news broadcaster Edward R. Murrow, his refusal to patent his discovery. Murrow: “Who owns the patent on this vaccine?” Salk: “There is no patent. It belongs to the people. Would you patent the sun?”

War II.[126] "The March of Dimes," the private philanthropy that crucially supported Salk's polio research, had astutely incorporated into the rhetoric of its fund-raising campaign the imagery of a battle that could be won.[127] Salk's attraction to the two-cultures question was fundamentally shaped by that experience – a wish to educate the general public about the true nature of scientific – especially biological – investigation and its practical and moral consequences. In their different ways, Salk, Snow and Bronowski shared with their predecessors as far back as the Victorian era, a common goal of promoting a basic level of public science literacy. And, in that regard, history was often enlisted as a preferred resource of communication.

However, after Bronowski's sudden and unexpected death in 1974, his ambitious, historically-grounded humanistic program collapsed.[128] The new Institute director, Frederic de Hoffman, a physicist and founding member of General Atomics, had no interest in pursuing a two-cultures initiative. Furthermore, there was no one willing, let alone able or inclined, to continue organizing the annual symposia on Renaissance history, science and literature that Bronowski had initiated in 1969.[129] And even that early, there is evidence that some of the Institute's scientists did not share Salk's and Bronowski's humanistic goals and that such emphasis was "peripheral to the Institute's basic purpose" and could "dilute the Institute's

126 On January 19, 2019, *The New York Times* published an editorial, citing the World Health Organization's judgment that public resistance to vaccinations is one of the top ten health threats in the world.

127 More than ten years after the release of the vaccine, Salk continued to receive many letters from school children, such as the following: "Dear Dr. Salk, We are studying about you in Science and I have decided to write this letter saying how wonderful you are. You have impressed me because of your discovery of the polio vaccine. Also for taking the time, patience, and effort that you have put into your work. You are very dedicated in your work, jut like I would like to be when I get older.
In our text book [sic], there are a few pictures of you, but they aren't very good ones. If you would be so kind, Doctor Salk, I wonder if you would take the time to send me a picture of yourself. I would like to place it in my Science Notebook next to my report on you. Your great admirer …" April 21, 1966 (UCSD, Jonas Salk Papers, 1966, Box 61, Folder 5).

128 See Suzanne Bourgeois, *Salk Institute*, pp. 163-64.

129 Bronowski's diaries from Spring 1969 onward reveal a special interest in the secondary literature on Renaissance humanism (e.g. Marie Boas, *The Scientific Renaissance* [JCB, Feb. 10, 1969]; Paul Oskar Kristeller, "The Medieval Antecedents of Renaissance Humanism" [JCB, February 17, 1969]; Eugenio Garin, *Italian Humanism* [JCB, February 26, 1969]). In May, Bronowski organized a symposium on Renaissance history. This was followed by two symposia on Renaissance science in 1970 and 1973, which I attended.

output in its primary field."[130] Thus, at best, he had few allies within his own Institute.[131] As Stuart Leslie has observed: "Under De Hoffmann the Institute all but abandoned the idea of bridging the 'two cultures'." The Institute became "a place of gruelling and often tedious work with little time for quiet meditation on the meaning of life. Who had time for philosophical reflections in the midst of a race for a Nobel Prize?"[132]

The Two Cultures and the Sigmoid Curve

In the early 1970s, as Bronowski was completing his film series and Institute scientists began to turn away from the humanistic mission, Jonas Salk was working to develop his own interpretation of the two-cultures question. Over the decade, building on the scheme that he first set forth in *Survival of the Wisest* (1973), he brought together his ideas with his son Jonathan in *World Population and Human Values: A New Reality* (1981), basic elements of which were later summarized succinctly in a symposium honouring Bronowski's memory.[133] Over-population, which C.P. Snow had highlighted as a central problem in 1959, was still very much at the centre of Salk's thinking. So was the experience of World War II. At the 1984 Bronowski memorial symposium, Salk made explicit some associations that previously he had not made so obvious. One of these was the connection between his and Bronowski's preoccupation with science and human values and the bombing of Hiroshima and Nagasaki. "My mind is haunted," he wrote, "by the unanswerable question of what might have

---

130 Within the Institute's core foundational group Leo Szilard was a genuine supporter of Salk's humanistic goals; but Leslie Orgel, Edward Lennox, Renato Dulbecco and Melvin Cohn "didn't care about the 'Two Cultures' from the beginning" (Interview with Peter Salk, August 8, 2018). Minutes of a Board of Trustees meeting in February, 1969 report that Senator Jacob Javits (R-NY) recommended the appointment of a Fellow trained in the social sciences in order to study "problems of transplants, control of pharmaceutical products and the nature of the Government's patent policy." The report continues: "Dr. Orgel cautioned that too much of this work could be peripheral to the Institute's basic purpose and therefore dilute the Institute's output in its primary field." (Salk Institute Files, Box 383. MSS. 1. Folder 2, June 5, 1969). Charlotte Jacobs further infers that Orgel "expressed the unspoken opinion of other Institute scientists" (*Jonas Salk*, p. 357). But, it is unclear exactly how early and how widespread was the early opposition.

131 Interview with Peter Salk, August 8, 2018. See also, James Day's interview with Jonas Salk (https://www.youtube.com/watch?v=j0Lyn18HH6s).

132 Stuart Leslie, "A Different Kind of Beauty: Scientific and Architectural Style in I.M. Pei's Mesa Labouratory and Louis Kahn's Salk Institute," *Historical Studies in the Natural Sciences*, 2008, *38*: 215-216; quoted in Jacobs, *Jonas Salk*, p. 362.

133 Recently updated and titled *A New Reality: Human Evolution for a Sustainable Future*, (Stratford, CT: City Point Press, 2018).

ensued if physicists had been working side by side with others concerned with human values."[134] Unlike Bronowski, however, Salk conceived his framework at the level of the whole species rather than that of human history. His approach was forward-looking and reparative: "Being a physician as well as a scientist, I cannot help but wonder what can be done to treat, correct and ultimately prevent what appear to be diseases, defects or errors in living and human systems. I am interested in moves to prevent the self-destruction of the human species."[135]

This revealing statement captures Salk's fundamental motivation and provides some basis for appreciating what he was trying to accomplish. But it also reveals the limits of his enterprise. As a physician and a scientist with no significant academic training in history, philosophy or the social sciences, Salk recognized that he was in no position to continue Bronowski's approach. Therefore, he had no choice but to accept Bronowski's historical narrative of scientist's engagement with the natural world. When he was invited to create his own television series in something like the format of *The Ascent of Man*, Salk explicitly stated: "I thought what was needed was to reveal not only from where we have come but where we might be going in the evolutionary scheme of things. I proposed the title 'The Next Evolutionary Step in the Ascent of Man in the Cosmos.'"[136] This next step was cast in the form of a prognostication, a "metabiological" scheme whereby he would create "a kind of abstract art, intended to bring facts and meaning together as one would in bringing together science and philosophy."[137] Here, Salk appears to have wanted to reinvent himself as a scientist-philosopher but without directly engaging himself in the philosophical issues and scholarly debates of his own historical moment.[138] For example, although Karl Popper was an important visitor to the Institute in the mid-1960s, Salk makes no reference to

134 Jonas Salk, "The Next Evolutionary Step in the Ascent of Man in the Cosmos," *Leonardo*, 1985, *18*: 237-242, p. 238. Other contributors to this issue were Gerald Holton and the UCSD biologist, Clifford Grobstein.

135 Ibid., p. 239.

136 Ibid.

137 Ibid.

138 Salk's friend, the UCSD psychiatrist Arnold Mandell, said that he had often recommended that Salk read Kuhn's *Structure of Scientific Revolutions*, but Mandell did not believe that he had ever done so (Arnold Mandell, interview, October 30, 2018) – an impression consistent with my study of the Salk and Bronowski archives. Besieged by frequent insomnia, Salk would write down his thoughts on a notepad next to his bed and some of these dissociated ideas were later incorporated into his writings. I have been unable to consult these "Night Notes."

Popper's views about the evolution of knowledge as a process of trial and error-elimination.[139] The decision to avoid citations and to direct his "*meta*-biological" theorizing to a general audience – much as he had announced his vaccine to the press – meant that his books would be subjected to review by ordinary newspaper journalists and without the benefit of critical academic refereeing.[140]

Undaunted, Salk laid out an original and suggestive scheme, succinctly summarized in his 1984 Bronowski memorial article. This framework, unlike Bronowski's, was entirely cast in evolutionary rather than historical time. He broke down "universal evolution" into three kinds of relationships, each composed of different levels of order: "prebiological" (probabilistic interactions of atoms); "biological" (cells, composed of genetic information and soma that emerge together as life); and, finally, "metabiological evolution" where consciousness emerges (somehow) from intuition and reason, creativity and choice. In short, evolution explained the clustering and survival of certain group characteristics. The driving force of evolution was the growth and decline of population. In what he designated as Epoch A, population rises and there is pressure on the food supply, housing, access to health care and, consequently, certain social values, attitudes and strategies become adaptive. Salk listed these values as: individual power, competitiveness, independence, extremes, and either/or thinking in social relationships. But as war, disease and food scarcity level off the population, a different set of more humane, psychosocial values and strategies emerge in Epoch B. These are: a concern with the group rather than the individual alone, collaborative work with others, a valuation of interdependence, a search for agreement and wholeness and, finally, an acceptance of human contradictions. In this optimistic, anti-Malthusian account, Salk represented Epochs A and B with a sigmoid or S-shaped curve – the first segment concave, the second convex, and the transition between the epochs as the inflection point of change. [Figure 1]

Salk's philosophical writings provoked some criticism because his conceptualizations seemed too vague, a perception encouraged, in the first instance, by his failure to ground his claims rigorously in the philosophical

139 Karl R. Popper, *Objective Knowledge: An Evolutionary Approach* (Oxford: Clarendon, 1972; rev. editions, 1973, 1974, 1975, 1979), pp. 206-284.

140 "In contrast to his scientific work, my father was not a scholar." Interview with Peter Salk, August 8, 2018.

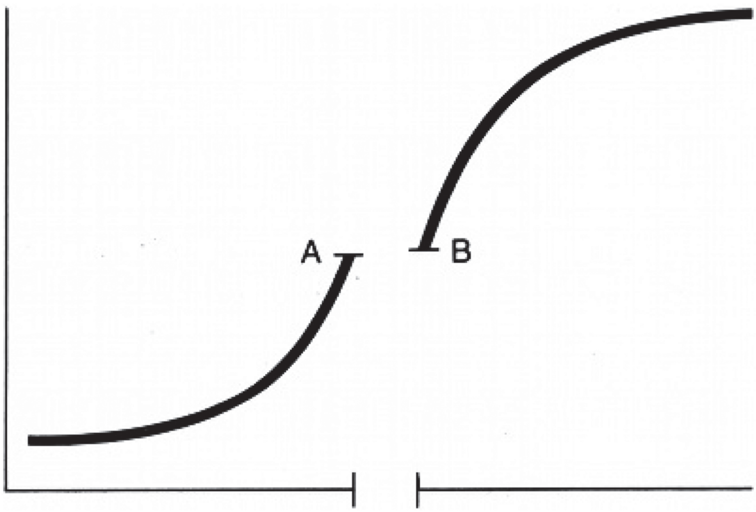


Fig. 1

literature, to locate himself in a scholarly discussion.[141] That neglect appears to have been fed by his choice of intellectual models – the polymath inventor Buckminster Fuller, Norman Cousins, editor of the *Saturday Review*, and the architect Louis Kahn – rather than the philosophers who passed through the Institute.[142] One may wonder whether he might have captured the broader audience he hoped to reach by casting his account in the literary genre of a scientific romance or a utopian society, such as H.G. Wells' *The Time Machine* (1895), J.B.S. Haldane's *Possible Worlds* (1927) or Aldous Huxley's *Brave New World* (1932).[143]

---

141 In a frank letter to the Hungarian scientist-philosopher Michael Polanyi, Salk seemed conscious that he needed to learn how to do philosophy, but he also seemed tentative and anxious about it: "I was so pleased to see you here and wish that it were possible to see you more easily. I need a bit more encouragement to spend time on philosophical questions but as yet have not been able to make the necessary sacrifice from ambitions in biology. I don't know how long this situation will continue. I have begun to write and will try to send you some indications of this. A copy of a talk given recently to a lay audience will be sent to you. I enjoyed the exchange between you and Bar-Hillel. But, especially did I like your paper on the 'Modern Mind' and on the 'Logic of Tacit Inference.' If you send me copies of your current thoughts I might use these to prod myself." (Salk to Polanyi, May 24, 1966, [JSP, UCSD Special Collections, Box 61, Folder 5]).

142 Jacobs, *Jonas Salk*, pp. 314-315.

143 See esp. Mark Adams, "Last Judgement: The Visionary Biology of J.B.S. Haldane," *Journal of the History of Biology*, 2000, *33*:457-491.

But, I suggest that there is a reading of Salk's scheme that reveals a basic coherence to his approach that has been overlooked. The sigmoid curve can be understood as Salk's effort to theorize in evolutionary terms a binary analogous to C.P. Snow's division between the poor, non-industrialized countries and the rich, industrialized countries empowered by the twentieth-century "scientific revolution". Consonant with "modernization theory", a conception prevalent in the social sciences of the 1950s and 60s, Snow believed that through education, infusions of capital and the export of scientific and engineering talent, "The West" could assist the underdeveloped countries to gain the benefits of industrialization.[144] In mid-century Britain, Snow laid the failure to undertake such initiatives at the feet of a government dominated by men trained in the classics and with no scientific literacy – "intellectual luddites," as he called them. However, Snow believed that the eventual outcome was inevitable, that the poor countries would finally prevail but not without a struggle: "Just because they [the poor] have noticed it [the disparity in wealth], it won't last for long. Whatever else in the world we know survives to the year 2000, that won't."[145]

But rather than the modernization of the poor countries, Jonas Salk's futurological sigmoid theory foresaw a shift between two psychosocial regimes – an evolutionary adaptation compelled, ironically, by increasingly grievous conditions of scarcity. Unlike Bronowski's *Ascent*, however, the temporal scale of his account was only distantly and abstractly historical. And unlike Snow, the normative element was absent: Salk made no policy recommendations – no criticisms of science policy, the scientific illiteracy of government officials and literary intellectuals or of the limitations of academic specialization. The process of evolution was both natural and inevitable. And, although convinced that a transition between epochs would occur, he had the good sense not to make any specific forecasts for the year 2000.

---

[144] Snow, *The Two Cultures*, pp. 41-51.
[145] Ibid., p. 42.

The Other Bruno: Latour, Woolgar, the Salk Institute and the Ethnographic Turn

In 1975, a year after Bronowski's death, Roger Guillemin (1924-) invited a young French anthropologist named Bruno Latour (1947-) to come to the Salk Institute to make his laboratory an object of study – on the condition that Latour could provide his own independent funding.[146] In 1979, Latour, in collaboration with the British sociologist Steve Woolgar (1950-), produced an account which they described as "the first attempt at a detailed study of the daily activities of scientists in their natural habitat."[147] Latour and Woolgar approached the Institute *as if* – that is the key move – as if they were strangers, like anthropologists studying a foreign culture. What the Institute scientists described as facts, Latour and Woolgar treated as something that the scientists *made* at a particular time and place and which, after they were constructed, left no trace of the historical and the social circumstances of their production.[148] In developing this radically ethnographic approach, Latour and Woolgar proposed to lay bare, in all their specificity, the hitherto hidden, local, social circumstances of the production of scientific facts.[149] In so doing, they explicitly separated themselves from the prevailing approach to the sociology of science pioneered by Robert Merton in the 1930s and 40s, an approach that purported to discover scientists' norms and values but which also relinquished any claim to be able to describe and analyse the actual content of science. They also brushed aside historians of science – citing no examples – as having an unwarranted, godlike view of the past and who, so they claimed, treated all science as a special kind of knowledge with a single, universal structure. And finally, they completely bypassed the moral issues that had preoccupied Salk, Bronowski, Szilard, and other scientists whose experience had been profoundly shaped by the war. What was needed, in their view, was a first-hand study of scientists rather than a reliance on scraps of indirect testimony from the past. The wider implication of their provocative thesis

146 Bruno Latour and Steve Woolgar, *Labouratory Life: The Construction of Scientific Facts* (Princeton: Princeton University Press, 1986; first pub. 1979), p. 274. The actual field research was carried out between October 1975 and August 1977 (ibid., p. 39).

147 Ibid.

148 Ibid., p. 107.

149 For an important ethnographic study of a biological labouratory at UCSD, see Rebecca Anne Hardesty, "Biology and Pedagogy: From Public Outreach to Training Future Experts," unpublished doctoral dissertation, University of California, San Diego, 2019.

was that scientific practices differ from one local setting to another and, therefore, that science should be seen not as one of two cultures but rather as itself consisting of *many* different cultures.[150] But, in that case, then, how is it that scientific knowledge travels from one site to another? And, how do scientists ever arrive at universal statements about the natural world? Those were but two of the many new research questions quickly adopted by the budding conglomerate of fields that soon came to be known variously as "Science Studies" or "Science and Technology Studies."

Jonas Salk wrote the introduction to the first edition of *Laboratory Life*. With his customary generosity and openness to new perspectives, he endorsed the claim that "the social world cannot exist on one side and the scientific world on the other," but he did so with unmistakably qualified enthusiasm: "I am always stimulated by attempts to show that the two 'cultures' are, in fact, one."[151] He acknowledged that the book might be beneficial in helping to correct the public image of scientists as "magicians" and in "dissipating the mystery that is believed to surround our activity." Perhaps, in the future, scientific institutes and laboratories should even employ an "in-house philosopher or sociologist." But he did not agree with Latour and Woolgar's claim that the human element is entirely erased from fact production.

Salk's encounter with Latour and Woolgar can be viewed against deeper, long-term changes in historical studies of science. Scientists overwhelmingly dominated the writing of the history of science in the nineteenth century and that tradition continued well into the twentieth, with Bronowski's *Ascent of Man* as a late expression of that trend. But from roughly the 1950s onward, a competing development emerged as *historians* began to write history of science and history, philosophy and sociology of science began to acquire the status of specialties in their own right.[152] George Sarton's journal was an important platform for many of these changes.[153] However, unlike the chronic financial difficulties that Sarton's enterprise faced before World War II, in 1958 the U.S. National

---

150 On the notion of science as a singular enterprise, see Jan Golinski, "Is It Time to Forget Science? Reflections on Singular Science and Its History," *Osiris* 2012, *27*: 19-36.

151 Jonas Salk, "Introduction," *Labouratory Life*, pp. 11-14.

152 See Floris Cohen, *The Scientific Revolution*, pp. 112-114; Michael Bentley, *The Life and Thought of Herbert Butterfield*, Cambridge: Cambridge Univ. Press, 2011.

153 See Gerald Holton, "George Sarton, His *Isis* and the Aftermath," *Isis* 2009, *100*: 79-88.

Science Foundation created a relatively substantial funding source explicitly for the history and philosophy of science. Soon, the NSF was supporting everything from conferences to research travel grants to fellowship leaves for faculty and students and even the translation and publication of medieval scientific texts.[154] Then, when Thomas Kuhn's brilliantly suggestive *Structure of Scientific Revolutions* appeared in 1962, with its central claim that science and scientific change are indelibly social, communal activities, that work stimulated a short-lived "marriage" between the history and philosophy of science and gave birth to a rebellious offspring that called itself "the sociology of scientific knowledge."[155]

The two-cultures framework proved completely inadequate to capture the emerging questions and debates within and between these newly-stabilizing specialties and their progeny. History and Philosophy of Science now often had their own departmental and funding structures in a few major research universities which further helped to consolidate their separate professional identities. "Interdisciplinarity", already a watchword of mid-century think-tanks, suggested a conversation among equals but it also concealed serious tensions and opposing purposes that pulled these specialties away from mutual engagement.[156]

In the early 1970s, as Jacob Bronowski circled the globe filming *The Ascent of Man*, he was certainly conscious of the early controversies generated by Kuhn's book – especially the famous 1965 London conference where Kuhn engaged Popper and his followers.[157] But neither his published writings nor his diaries provide evidence that he ever entered directly into those debates just as he had avoided any scholarly encounter with Boris Hessen's Marxist challenge of 1931. And in 1979, Jonas Salk also gave no indication that he was conversant with any of these trends.

---

[154] See Margaret Rossiter, "The History and Philosophy of Science Program at the National Science Foundation," *Isis* 1984, *75*: 95-104.

[155] See John H. Zammito, *A Nice Derangement of Epistemes: Post-Positivism in the Study of Science from Quine to Latour* (Chicago: Univ. of Chicago Press, 2004), pp. 52-150; *Kuhn's* Structure of Scientific Revolutions *at Fifty: Reflections on a Science Classic*, eds. Robert J. Richards and Lorraine Daston, Chicago: Univ. of Chicago Press, 2016.

[156] See Cohen-Cole, "The Creative American," on "interdisciplinarity", which he dates to think tanks of the 1950s and 60s.

[157] *Criticism and the Growth of Knowledge*.

At the Salk Institute, there would be no further effort to bring together the humanistic disciplines of history and philosophy of science – let alone with sociology and anthropology – such as happened in 1989, right across the road at the University of California.[158] But that is a story for another occasion.

[158] In 1988, the faculty who met to discuss the formation of a "Science Studies Program" included Paul Churchland (Philosophy), Gerald Doppelt (Philosophy), Philip Kitcher (Philosophy), Bruno Latour (Sociology), Chandra Mukerji (Sociology), Martin J.S. Rudwick (History), Andrew Scull (Sociology), Steven Shapin (Sociology), and Robert S. Westman (History).

Laudatio Paul Brand

Dirk Heirbaut

Paul Brand was born in 1946. In 1964, he went to Oxford to read history which marked the start of a distinguished career. In 1970, he began working in the Medieval Records Section of the Public Record Office in London. Although he left these archives six years later, Paul Brand has always remained a specialist of the sources of medieval English law. If anything, his publications build upon a large wealth of archival documents and the reader can only admire his mastery of the primary materials. In 1976, Paul Brand moved from London to Ireland, where he lectured on law and legal history. However, in 1983, he resigned his position and became a private scholar, which allowed him to fully pursue his academic dreams. Many institutes and universities in both the United Kingdom and the United States have invited him, first, as visiting fellow and later as visiting professor. Finally, in 1997, it was the privilege of Oxford University to hire him permanently as a fellow and finally as professor of English legal history. In 2014, he retired and joined the ranks of the emeritus fellows of All Souls College. Paul Brand's university career only shows one part of his activities. He contributed to many learned societies, but here we can only mention a few: Fellow of the Royal Historical Society, Council Member and later Vice-President of the Selden Society, Council Member and Treasurer of the Pipe Roll Society, Fellow of the British Academy and Chairman of its research projects Committee, Vice-President of the Jewish Historical Society of England and Corresponding Fellow of the Medieval Academy of America. For his work he received, the Donald W. Sutherland prize of the American Society for Legal History on two occasions and the Irish Legal History Society awarded him its Gold Medal in 2006. Not only historians and legal historians bestowed honours upon him. In 2000, the American Law Institute elected him as a member, whereas in 2014 the Middle Temple called him to the bar and made him an Honorary Bencher, an honour reserved only for royalty and the most distinguished academics.

Paul Brand has found all this recognition thanks to his impressive body of scholarship. Interestingly and almost unheard of in today's era of producing rapid results, he limited himself to publishing articles at first. However, by 1992, his reputation was already built and, subsequently, a book containing no less than twenty articles saw the light. Of particular importance was the first article, which dealt with the same subject as a monograph published in the same year. In both book and article, Paul Brand showed how the institutional reforms of the twelfth-century King Henry II gradually led to the professionalisation of lawyers in English law. Paul Brand was particularly interested in the thirteenth century, which had also been at the heart of his 1974 thesis. Aiming for the highest possible standard of academic excellence, Paul Brand did not immediately publish his thesis, but constantly strived to strengthen its documentary foundations. He wrote several smaller studies, which finally culminated in his 2003 magnum opus: *Kings, barons and justices: the making and enforcement of legislation in thirteenth century England*. The academic community has hailed this book as a true masterpiece, which rendered all previous scholarship obsolete. It studied two important texts, the 1259 Provisions of Westminster and the 1267 Statute of Marlborough and showed not only how this legislation came to be, but also how the courts interpreted and applied it. Whereas previous scholars had looked at these statutes as isolated documents, Paul Brand firmly situated them in the broader political and legal context.

Paul Brand's importance goes beyond his own publications. He has edited many of the oldest sources of the English common law. For the purpose of researching English law, historians can consult the Year Books, which contain reports of English case law. In response to the fact that a great deal of the thirteenth-century material was absent from the Year Books, Paul Brand brought together a lot of pre 1290 reports in four voluminous books, published by the Selden Society, thus enabling new research. Historians can also thank Paul Brand for reediting and translating the Parliament Rolls for the years 1275 to 1307. He has further contributed to other valuable projects of fundamental research, like the Dictionary of Medieval Latin from British Sources and the Anglo-American Legal Tradition website.

Given the many publications of Paul Brand, it is impossible to list all his achievements here. Nevertheless, I would like to single out a few articles, because they caught my attention at one moment or another. Very important in my opinion is 'Westminster Hall and Europe: European aspects of the English medieval common law'. His countrymen may sometimes show a very insular mentality, but Paul Brand has always been aware of the wider European context and he has written many chapters in books edited by continental colleagues. He also devoted special attention to the realities of the law. Reading Paul Brand, English kings and barons, as well as courtroom scenes come to life. Paul Brand may not be a linguist, but he has dealt with the linguistic diversity of medieval England (Latin, Law French and English) in several publications, including the situation in Ireland. In fact, in one of his articles he describes the birth and early development of a colonial judiciary: not India in the eighteenth century, but English judges in Ireland in the twelfth and thirteenth centuries. As this example shows, Paul Brand is a master in finding attractive titles for his publications. Another example is 'Chief Justice and Felon', i.e. highest judge and felon heading an article, which chronicles the career of Thomas Weyland. However, the article does not actually dwell on this infamous character from thirteenth-century English legal history, but on the topic of which Paul Brand is generally recognised as the undisputed master: the beginning and early history of the English common law.

The Beginnings of the English Common Law (to c. 1350)

Paul Brand

I

The recorded history of law in England begins before there was any kind of political or administrative unit called 'England', with the Roman conquest of what soon came to be called the province of *Britannia* in the first century AD. The Roman conquest of *Britannia* (like other Roman conquests elsewhere) brought Roman law to the new province. We hear of the famous jurist Papinian hearing cases in the forum at York and we learn from the Digest of Justinian of a decision on a matter of trust law relating to Roman officials resident in *Britannia*. When the Romans withdrew from Britain in the early fifth century, they left a long-term legacy of Roman roads and Roman-founded towns (like London and York and Bath) but they left no substantial legal legacy. Legal historians agree that those elements of Roman law found in the English Common Law of later centuries are not a survival from the period of Roman rule but the result of much later borrowing.

Roman withdrawal soon led to the establishment of a multitude of separate kingdoms whose rulers and nobles were invaders who came from northern Germany, Frisia and southern Denmark and who called themselves Angles, Saxons and Jutes. There survive texts of legislation promulgated by the kings of two of these kingdoms, those of Kent and Wessex (the laws of Aethelberht of Kent of c.600, of his successors Hlothere and Eadric of the last quarter of the seventh century and of Wihtred of the late seventh or eighth century and those of Ine of Wessex of the late seventh or early eighth century). These resemble the legislation of other Germanic rulers of the same period on the Continent in their coverage but differ from them in being written not in Latin but in the language which we now call Anglo-Saxon, a distant forerunner of modern English. A single unified kingdom of ‘England’ did not emerge before the tenth century and its emergence was linked to the political and military shock of repeated Viking attacks from Scandinavia and the successful reconquest of areas occupied by the Vikings being led by the kings of Wessex who soon became kings of England. The legislation promulgated by successive kings of the new kingdom (also in Anglo-Saxon) has a reasonable claim to be called the first English law. Its kings also created an administrative framework of shires or counties, each under the control of their own shire-reeve (or sheriff) and with their own shire or county court, and they asserted a royal claim to oversee the provision of justice in the country as a whole, with a particular focus on major crimes such as murder and theft.

The Norman Conquest of England in 1066 brought William ‘the Bastard’ (or ‘the Conqueror’) duke of Normandy to the throne of England and led to the dispossession over the following twenty years of the existing ruling class (both lay and ecclesiastical) and its replacement by William’s followers and associates. These came not just from Normandy but also from other areas of northern France and Flanders. The results of this process can be seen in Domesday Book, the survey of English lay tenants in chief and their landed possessions, compiled in 1087. It also led (though the exact chronology of this has long remained controversial) to the introduction of the universal feudal tenure of land in England, often with multiple feudal links between the actual tenant in possession and the ultimate lord of whom all land in England was held (the king), and to the creation of feudal courts held not just by the barons who were the king’s tenants in chief but also by lords at various intermediate levels of the feudal structure right down to village level. The new Norman kings of England claimed to be the legitimate successors of their Anglo-Saxon predecessors but when they did promulgate legislation it was in Latin, not Anglo-Saxon, and there is little to suggest that these Francophone rulers and their followers were aware of the legislative legacy of their predecessors or even able to understand it in the absence of any usable translation of this legislation prior to the twelfth century. Not surprisingly, later English law owes little to any pre-Conquest legislative inheritance. What was utilised and preserved by the Norman rulers of England and their successors was the system of local administration created by the Anglo-Saxon rulers of counties and sheriffs and county courts. But it is difficult during the century or so when the Norman kings ruled England to see anything which can be described as 'English' law, perhaps because this was a period during which there existed a multiplicity of overlapping local courts (feudal and communal) each with their own customs and subject to little overall royal control or pressure for uniform rules, but also perhaps because there is so little surviving evidence for law and its application anywhere in England and good reason to believe that little of what the courts did was ever recorded in writing.

II

Henry II, king of England from 1154 to 1189, was the first to belong to the Angevin dynasty. He was descended through his father from the counts of Anjou and Poitou in the middle of France and inherited from his mother Maud (the daughter of king Henry I) his entitlement to the kingdom of England and to the duchy of Normandy. His wife Eleanor brought him the duchy of Aquitaine in the south-west of France which passed to their heirs as kings of England and was held by them till the mid-fifteenth century. It was during the second half of his reign (from 1176 onwards) that Henry and his advisers began to create a new kind of royal court in which a group of justices appointed by, or in the name of, the king for the first time not only presided over the court but were also themselves directly responsible for making that court's judgments. In all previous English courts (royal and non-royal) there had existed a clear division between the court's presiding officer and a group of 'suitors', men with no special expertise obliged to regular attendance at the court who made any judgments which the court required. One of these new courts, or types of court, was the Eyre. From 1176 onwards Henry II arranged every two or three years for each county in England to be visited by groups of justices to bring civil and criminal justice and making more general enquiries there into matters of interest to the king (for which they were provided in advance with a written list of articles 'of the Eyre' for enquiry and action). At each countrywide visitation a number of groups of justices travelled from county to county within their own separate circuits, but the number of circuits and the counties allocated to them varied from visitation to visitation. A second such court emerged gradually out of the Exchequer, the body that handled royal finances and which from early in Henry II's reign had taken up permanent residence in the royal palace of Westminster close to London. The leading officials of this institution were from the 1160s onwards given additional responsibility for hearing and determining ordinary civil litigation between the king's subjects but it was only in the mid-1190s after Henry II's death (during the reign of his son Richard I) that the judicial functions of the Exchequer were hived off to a separate institution with its own justices and its own place in Westminster Hall (the hall of the royal palace) which became known as the Bench, later the Common Bench (to distinguish it from a court of King's Bench) and much later the Court of Common Pleas.

These two courts also differed from earlier courts in England in several other ways. The first related to the duration and frequency of their sessions. All previous courts seem (as far as we can see) to have held sessions lasting only a single day or part of a day and at infrequent intervals. The justices in Eyre seem from the first to have done business in each county they visited all day every day (other than Sunday) for periods of a week or longer and then gone on to hold similar continuous sessions in the other counties on their circuit. The officials of the Exchequer seem to have fitted their legal business in between their more important financial business but to have been in almost continuous session for much of the year. Once the Bench emerged as a continuous institution its justices began holding daily sessions during each of four terms (Hilary, Easter, Trinity and Michaelmas) which covered around half the year with vacations in between whose incidence was determined by the requirements of the Church.

A second difference related to their record keeping. Although there are a few private accounts of litigation and trials in English courts prior to 1154 and a few royal charters reciting and confirming their outcome, no courts seem to have routinely made or kept a record of what they did: for that they relied on the fallible memories of their suitors. The new royal courts probably began keeping records of their business from c. 1176 although the earliest surviving plea rolls (official records of their business recorded on parchment) come only from 1194. These rolls record on a term by term basis all the pleading between parties and any judgments given on it as well as the court's authorisation of process to secure the attendance of defendants and others involved in litigation and they do so in roughly chronological order. The earliest records of the settlement of litigation in these courts made under the auspices of the court (known as final concords or fines), of which both parties had their own copies, come from the 1160s, and the surviving fines are an important source of our knowledge of when and where these courts were sitting and who were the justices (or others) named as presiding over the courts. From the summer of 1195 onwards, the king's courts also began routinely to make a third copy of each fine, with this copy (the foot of the fine) being transferred to the king's treasury for permanent preservation and consultation when required.

A third distinctive characteristic was that the justices of these courts, despite having been themselves appointed by the king, required specific authorisation from him to do their business. In civil litigation that authorisation by the end of Henry II's reign (and for an unknowable period prior to this) took the form of a royal order (a writ) in the king's name and sealed with his seal issued by the king's chancery. This outlined the nature of the claim or complaint being made and ordered the relevant local sheriff to have the defendant summoned to appear at the king's court on a specific day (in the case of the Bench and earlier the Exchequer) or when royal justices next came to the county (in the case of the Eyre) and to send the writ to the court with a report of what had been done. It was this 'original' writ as 'returned' to the court which then became the court's authority for hearing the case. Royal writs (sealed written orders from the king) went back to the late Anglo-Saxon period but their use as the sole method of initiation of litigation in royal courts was new in Henry II's reign. Also new was the standardisation of the writs for initiating litigation ('original' writs) available from chancery. Plaintiffs could in general only initiate litigation in the king's courts if they could bring their claim or complaint within the overall parameters of the small number of standard types of writ which had been authorised for issue by chancery. In the case of criminal proceedings (normally heard during Henry II's reign only by the justices in Eyre) the authority conveyed was a general one to hear and determine 'pleas of the crown', major criminal offences, but the articles of the Eyre also included articles authorising the justices to seek information from a local jury of presentment of those suspected of serious criminal offences.

A fourth characteristic of the new courts was that they were the first courts in England to possess a 'nationwide' jurisdiction. The Bench (and before it, the Exchequer) could and did hear cases from all over England (with the exception of the two counties of Durham and Cheshire that were long to remain outside the jurisdiction of the central courts). Each individual session of the Eyre heard cases only from a single county but Eyre visitations were planned to ensure that the different groups of justices between them heard pleas in all the counties of England (except Durham and Cheshire) and exercised the same jurisdiction and followed the same procedures in each of them. The fact that it was these justices rather than the suitors of the individual county courts who made judgments at the Eyre sessions helped to create and maintain a national uniformity in legal procedures and legal rules; and the fact that the justices of these Eyres included core members of the group of justices (or barons) who ran the Exchequer and later the Bench also helped to ensure uniformity as between these courts. It was the creation and continued existence of these national courts which helped to create and sustain a single English national customary law, the English 'Common Law'.

It was also during the reign of Henry II that the use of the collective verdicts of juries as a fact-finding mechanism became a regular part of the English legal system. Local juries of presentment came to be required to give information twice yearly at special sessions of the local hundred or wapentake courts on those they suspected of having committed both serious and less serious offences and similar juries were also required to provide information on serious offences at Eyre sessions. If and when suspects were captured and stood trial, however, their innocence was determined not (as later) by jury trial but by the ordeal of cold water or hot iron. In civil cases, Henry II's reign saw the introduction of the petty assizes of novel disseisin (to remedy the unjustified dispossession of land), mort d'ancestor (to remedy an heir being prevented from taking possession of his ancestor's inheritance) and darrein presentment (when the right of a patron to present the next rector to a vacant church was contested by another claimant). In all of these a jury of twelve men was selected in advance and given notice of the question or questions to which they were going to be required to give their collective answer (their verdict). From 1179 onwards the defendant (or tenant) in the action of right for land, where the plaintiff was claiming land on the basis of the prior rightful possession of that land by the claimant or his ancestor (or predecessor), also gained the right to opt for jury trial with the verdict being given by a specially chosen jury of twelve local knights (the grand assize) as an alternative to trial by battle (with the champions of both parties engaging in combat) as a way of determining the question of rightful entitlement. Jury verdicts were intended to operate on the basis of the jury telling the king's justices what the members of the jury already knew or had been able to discover from local sources of information. This is in marked contrast to the modern jury in England (and in the USA) which requires jurors to give their verdict on the basis of their assessment of the evidence presented to them in court and the competing interpretations put on that evidence by counsel for the Crown and the accused or the parties in civil litigation and the presiding judge. Henry II and his advisers seem to have made a conscious choice to use the collective verdict of a jury whose members were chosen by the local sheriff (or in the case of the grand assize by the four knights chosen as electors) but who were also subject to removal for any suspicion of partiality in preference to the use of testimony provided by individual witnesses nominated by the parties but examined by the court which was just becoming the preferred method of fact-finding of the ecclesiastical courts in England and courts elsewhere in Western Europe.

The English legal historian is fortunate to possess in the treatise known as *Glanvill* which was completed not long before the death of Henry II in 1189 a snapshot of the new English Common Law as it had developed over the course of Henry II's reign. Ranulph de Glanvill was a leading royal justice of the period and held the office of the justiciar (chief minister) for the last ten years or so of the reign, but the ascription of the treatise to him is not contemporary (it is first found almost a century later) and has generally been rejected by legal historians. Various other candidates for authorship have been suggested. None have found general support. The treatise is avowedly an account of the law and procedure of the king's courts, but it also reveals in passing how far the county courts and the lord's courts of England had already come by 1189 to be integrated into a single national legal system.

III

During the period of about a century and a half following the death of Henry II which takes us up to the middle of the fourteenth century and the first visitation of the Black Death to England (and Western Europe as a whole) a number of significant developments occurred in the English legal system.

The first was the emergence of a third central royal court, the court *coram rege* ('before the king') or court of King's Bench. A court 'before the king' which travelled round England with him had existed occasionally in the reign of Henry II but he had spent too much time elsewhere for it to become securely established as an institution, and there is no sign of a such a court in the relatively short reign of his son Richard I (1189-99), who spent little time in England. Such a court had a much more continuous existence under king John, Richard's youngest brother, after he had lost most of his family's ancestral possessions in France in 1204 and for a five year period between 1209 and 1214 it even replaced the Bench as the main, indeed the only, royal court for the hearing of civil litigation. This was unpopular with John's English subjects and so one of the provisions of the Charter of Liberties of 1215 (later known as Magna Carta) granted to his subjects by the unwilling king conceded that 'common pleas' would be held or heard in future at a fixed location. This did not prohibit the existence of a travelling court accompanying the king. It did, in effect, prohibit that court from hearing 'common pleas', ordinary cases, particularly cases about title to land. When king John was succeeded by a nine-year old child in the middle of a civil war in 1216 there was no question of resuscitating the court and this did not happen until 1234. The revived court was, perhaps from the first, intended to be a rather different court from its predecessors: one that continued to function term by term every year whether or not the king was in England and even when the king was a child (as happened again in the last quarter of the fourteenth century). It also developed a distinctive jurisdiction hearing pleas of special interest to the king (and which could not be described as 'common pleas'). These included the review of cases heard in other royal courts in England, and cases from the English lordship of Ireland and (for short periods) from Scotland. From 1234 down to the early fourteenth century the court moved round England with the king (or not far from him) and writs requiring defendants to appear there stipulated their appearance on a particular day 'before us wherever we may then be' without specifying any particular location, but from the second decade of the fourteenth century it was normally resident (like the Bench) at Westminster but in another part of Westminster Hall. Despite the court's name and the wording of writs requiring attendance there, successive kings did not play any direct role in the running of the court. It was run and its judgments made by the court's justices and those justices normally needed specific authorisation by royal writ to hear cases there.

Eyre visitations of individual counties became much less frequent during the reign of Henry III (1216-72) when each county was visited on average only once every eight years and consequently each county session began to take much longer to complete. Under Henry III's son and successor Edward I (1272-1307) the idea of countrywide Eyre visitations was abandoned, and two permanent circuits of justices established who were to visit counties to conduct a significantly wider range of business there. Both Eyre circuits were suspended in 1294 when war broke out with France and thereafter there were only one-off Eyres in particular counties (apart from a failed attempt to revive Eyre visitations in 1329-30) and the Eyre (after a century and a quarter) ceased to be part of the English legal landscape.

That was only possible because much of its more urgent business came from fairly early in the thirteenth century to be heard in other courts. The most urgent kind of civil business was the hearing of two of the petty assizes (novel disseisin and mort d'ancestor) and other related cases. Various arrangements were tried during the course of the thirteenth century for hearing such business. From 1273 county assize circuits, with groups of justices holding assizes in several counties, became the norm and commissioning just two justices for each circuit became the norm in 1293. The most urgent kind of criminal business was the trial of prisoners being held in gaol on serious criminal offences. By the 1220s it had become the practice to commission local knights to hold 'gaol deliveries' to try the prisoners in a single gaol. A major reorganisation in 1294 transferred responsibility for gaol deliveries to circuit panels covering the gaols in several counties whose members included royal justices from other courts, other royal officials plus some local knights. In 1328 (under the Statute of Northampton) it became the practice to appoint the same men as assize and gaol delivery justices in groups of counties and to appoint men who during law terms were justices of the Bench and of King's Bench plus a few leading professional lawyers and this became the main vacation activity of these justices and lawyers. There was also a third element in the work of these justices: the taking of jury verdicts in cases pleaded to issue in the Bench or King's Bench from the counties where they held sessions and then returning the verdicts given to the relevant court for judgment in those cases. These three elements were the main constituents of the work of the Assize Justices in England for several centuries and the Assize Justices (and their sessions, the 'Assizes') continued down to the twentieth century.

Magna Carta had required 'common pleas' to be held in 'some fixed place'. By the seventeenth century this was being construed to mean that the Bench's session had to be held in a specific place in Westminster Hall. But in the thirteenth and fourteenth centuries the court moved elsewhere for periods of time without any apparent difficulty. In the thirteenth century it moved elsewhere in London (to the church of St Bride's in Fleet Street and to the London Guildhall) while Westminster Hall was being used for other events and the court moved to Shrewsbury close to Wales and to York in the north of England (but still 150 miles from the border) when the king took military action against the Welsh and the Scots. The most important development, however, over the course of the century was the increase in business coming before the court. This multiplied thirtyfold over the course of the century.

In the course of the thirteenth century we begin for the first time to get clear evidence for the existence of professional lawyers in the English Common Law courts (by which I mean men recognised as having a specific legal expertise, who were willing to make that expertise available to clients in return for remuneration and who look likely to have derived their main income from practice), as also for the development of a recognisable legal profession with special rules governing the activity of members of that profession, special rules for admission to practice in individual courts and the emergence of arrangements for the education of future lawyers). Professional lawyers first become visible during the reign of Henry III when we have references to a small groups of 'counters' or 'serjeants' speaking and arguing on behalf of clients in the Bench at Westminster in 1239 and 1267 and can identify some of its members. In the reign of Edward I (1272-1307) multiple sources of information allow us to identify the individual serjeants of the Common Bench and to see that a significant number of them practised in the court for up to thirty years and that they were drawn from all over England. During the same period, we can also see (though less clearly) that there was a second and much larger group of professional lawyers in the court, the attorneys, who acted as their clients' link with the serjeants and with the justices of the court and who functioned as their clients' representatives for appearances before the court. In the same period, we can see that the same two groups of professional lawyers (and in some cases the same individuals) were also found in the Eyre, in the court of King's Bench and before the justices of Assize and that there were also small groups of professional serjeants in county and city courts. The first general legislation to control the activities of professional lawyers and to punish their misbehaviour was enacted in 1275 (as c. 29 of the Statute of Westminster I) but the courts themselves also controlled the activity of the lawyers who appeared before them and the city of London enacted a more general code for lawyers acting in the city courts in 1280. By the last decade of the thirteenth century there seems to have been a mechanism to control the number of serjeants practising in the Bench which kept the number stable at around 30, but an attempt made in 1292 to control the numbers of attorneys practising in the same court by imposing county quotas with an overall cap on the numbers at around 140 was evidently ineffective. By the 1270s lectures were being given for the instruction of common lawyers (and we have the notes of some of those who attended them) and by the final decade of the thirteenth century we hear of a group of 'apprentices' of the court present in the Bench (with their own special place in the court) to hear (and record) existing practitioners and justices as they argued and gave judgment in court and sometimes commented on a case for their benefit, and there is good evidence to suggest that these apprentices subsequently used their reports of such cases to teach the next generation of apprentices. When the new style of royal court was first created under Henry II there were no legal experts with an expertise in the new Common Law, but there were men with a training in Roman and canon law and some were recruited to serve in the new courts. A relatively small number of university-trained lawyers followed them into service in the royal courts over the course of the thirteenth century, but this had ceased by 1300. The first source of men with a specifically Common Law expertise to become available was that provided by men who had served as clerks to royal justices and had sat in the courts to write judicial writs and record proceedings and perhaps even (like their later counterparts) to participate in courtroom argument. The best known examples from the first half of the thirteenth century are Martin of Pattishall who had been in the service of the first long-serving royal justice Simon of Pattishall in the early years of the thirteenth century and then himself became a royal justice; William of Raleigh, who served as a clerk to Martin of Pattishall before becoming a justice of the Bench and of King's Bench; and Roger of Thirkelby and Henry of Bratton, who had served as clerks of William of Raleigh before themselves becoming royal justices. In the second half of the century the best-known example is that of Ralph de Hengham who had been a clerk of Giles of Erdington and other justices from the mid-1250s until his appointment as a royal justice in 1271 and went on to become chief justice of King's Bench and subsequently of the Common Bench. The clear emergence and wider recognition of the expertise of the elite of professional serjeants is reflected in the appointment of such serjeants as regular justices of the royal courts. The first to be appointed was Richard Boyland who became an Eyre justice in 1279 followed by Alan of Walkingham appointed an Eyre justice in 1281. It was not, however, until 1290 when most of the existing justices were dismissed from their posts for misconduct that we find serjeants being appointed to act as justices in the Common Bench (Robert of Hartforth, William of Gisleham and William of Bereford) and in King's Bench (Gilbert of Thornton). Serjeants came to dominate appointments to these courts in the reign of Edward II (1307-27) and to gain a virtual monopoly of such appointments after that. Serjeants had some obvious advantages over clerks when it came to making judicial appointments. They were less likely to suffer from divided loyalties when king and pope were at odds and they were able to participate in the full range of judicial activities including presiding over criminal trials (from which the clerks' clerical status debarred them) and the expertise they had gained in pleading for clients before the courts came to be seen as the best preparation for the judicial role.

It is only after there begin to survive records of the proceedings of the royal courts (from the mid-1190s onwards) that we can see that there was no single 'official' record of those courts but multiple records made for each of the justices sitting in the court plus the senior clerk appointed directly by the king (the keeper of writs and rolls) which were in theory (but not necessarily in practice) identical records. And although these rolls were clearly intended to be a permanent record there seems to be no evidence of any special measures being taken to ensure they were handed in to the king's treasury for permanent preservation before the middle of the thirteenth century. It is thus hardly surprising that despite the making of multiple originals plea rolls now survive for only about half the terms when the Bench held sessions prior to 1270 and a similar rate of losses for the Eyre and King's Bench. Things only got better after 1270 when it is rare for there not to be at least one surviving roll for each term for each court or each county Eyre session.

From around 1270 it also becomes possible to supplement the evidence provided by the official record from the evidence of the first surviving law reports. These are very different in form from the individual entries (enrolments) on the plea rolls relating to the same cases. They generally report what was said in court (or at least some of what was said) in the form of direct dialogue ascribed to the individual serjeants and justices involved in the cases and are written in the Anglo-Norman spoken in court, as contrasted with the anonymous Latin of the enrolments which misleadingly suggest that what was said in court was spoken by the parties or their attorneys. Such reports are found in manuscripts which show no sign of ever having been in any kind of official custody or of having been drawn up for any kind of official purpose. While it is known that the plea rolls were drawn up by clerks working for the justices it is more difficult to discover who compiled these law reports and they may have been compiled by different groups for different purposes. But perhaps almost from the first the most important compilers may have been apprentices trying to master the law by listening to proceedings in the courts. During the first two decades of law reporting law reports survive only in relatively small quantities and none is part of a dated collection of reports ascribed to a particular court. It is only as from 1291 that law reports begin to survive in much larger quantities and often (but not invariably) in collections ascribed to particular courts and particular terms and years (or to Eyre sessions in particular counties). Even before 1291 it is by no means uncommon for there to survive as many as three or four independent reports of a single case and this becomes even more common after that date. There are as many as six different collections of reports for Michaelmas term 1302. They do not all cover the same cases but there is enough overlap between them and what the enrolment has to tell us to allow us to produce a much richer synthetic account of what had gone on in court than any of the sources by itself would have allowed.

A major increase took place after 1189 in the number of standard types of original writ available from chancery to initiate litigation in the king's courts. Some were specifically authorised by legislation or the new writ form was copied on to a chancery roll when it was authorised but for most we are reliant on evidence from surviving private 'registers of writs', collections of standard writ forms and their variants copied into private MSS. with other legal material and often difficult to date precisely, or to tracing the first appearance of new writ forms in the formal records of litigation, which reproduce much though not all of the original writ. *Glanvill*, the earliest treatise of the Common Law, completed by 1189 contains just fifteen standard original writs. These had grown to around nineteen by c. 1230 but had risen to over sixty-five by 1272 and to over one hundred by 1307.

There was also a major expansion after 1189 in the use of the jury as a fact-finding mechanism. In civil litigation there was a growth in the number of different types of action to assert title to land, especially actions (writs of entry) in which a claimant asserted that there was a specific flaw in the current tenant's title to land he was holding which meant that the claimant was better entitled to that land, and also a large scale expansion of civil remedies outside land law and of the availability of mechanisms for the removal of cases out of the county courts into the king's courts. In all of these jury trial was the norm but did not wholly exclude other fact-finding mechanisms where members of a jury could not be expected to have knowledge of the matter. In criminal matters a first major expansion of the use of the jury took place in 1194 with the introduction of the office of coroner. From this date onwards local village communities were under a legal obligation to notify their local coroner of any suspicious death which taken place in their locality and the coroner obliged to hold a jury inquest on the circumstances and cause of the death and to secure the arrest of anyone suspected of causing it. But it was to be another two decades before jury trial became the normal way of determining guilt or innocence for those accused of serious crimes, whether by the coroner's inquest or by an indictment jury at the Eyre, and then only because the Church in the legislation enacted at the Fourth Lateran Council of 1215 prohibited any kind of clerical participation in the two types of unilateral ordeal (by cold water and hot iron) which had hitherto decided such questions. Since the working of the ordeals was thought to depend on the invocation of God's judgment by the clergy the unilateral ordeal had to be replaced. What replaced it was, after an initial period of experimentation, the verdict of a jury. This was not initially the classic jury of twelve men but a larger jury, of varying sizes. But the king's courts evidently did not believe that the accused could simply be tried for his life by such a jury without his consent and so he needed to put himself 'for good and ill' on the verdict of the jury, that is to say agree to the jury's verdict. Sometimes, but relatively rarely, those accused refused. Legislation of 1275 (the Statute of Westminster I, c. 12) prescribed that in future in such cases the accused was to be remanded to a 'strong and hard' gaol until he did so. Initially that harshness was mainly one of diet (stale bread one day and stale water the other) but within a quarter of a century the use of heavy chains to restrict the prisoner's movement had begun. This soon mutated into the use of weights heavy enough to press the prisoner to death. Down to the eighteenth century some prisoners preferred death without conviction to the possibility or certainty of a guilty verdict and subsequent execution which would disinherit their heirs. Trial by battle (whose efficacy did not require any clerical presence or participation) continued in a limited number of criminal cases where a criminal who had already confessed his guilt agreed to bring appeals against his alleged companions in crime (as an 'approver') and to prove their guilt in single combat with them and if the approver vanquished enough of those he appealed he was allowed to live but in exile from England.

III

Initially the royal Common Law courts were in the main concerned with two areas of the law and claimed something close to an exclusive jurisdiction over them. One was serious crime ('felony') and in particular offences against the person (homicide and serious bodily assault ('maiming') plus rape) and property offences (robbery, theft and arson). These had long been considered 'pleas of the Crown', matters of special concern to the king, but the king had relied on local courts and largely on private initiative for punishing these wrongs against his peace. This began to change under Henry II when the justices in Eyre began to take the initiative in seeking information about the commission of such offences and those responsible for committing them and (as we have seen) from 1194 onwards the coroner's inquest provided information about all suspicious deaths. By 1200 the royal courts had also begun to insist that even private criminal prosecutions (appeals) could take place only before royal justices. Although there was no direct attempt to discourage private prosecutions by victims or (for homicide) their kin and these took precedence over crown prosecutions, private prosecutors had only a year within which to initiate their suits and any technical defect in the prosecution or failure to prosecute would lead to the accused being tried at the king's suit instead. The king benefitted from successful prosecutions since the movable possessions of those condemned came to the crown and the right to one year's income from their lands, but the king's virtual monopoly of such jurisdiction was important for other reasons as well. The king was in effect providing a guarantee of the life, limb and property of all of his subjects (free and unfree) through the infliction of a harsh punishment (death) on those who breached that guarantee and the knowledge that he and his courts were doing this helped to buttress wider royal authority over the king's subjects. The early common law of crime has long had the reputation of being neither complex nor intellectually sophisticated and it is certainly the case that most defendants in criminal cases pleaded not guilty and the jury (after 1215) gave a blank verdict of conviction or acquittal and there is little sign of any formal presentation of evidence to the jury from which a law of evidence could emerge. But English criminal law was not devoid of rules or broader ideas. The Common Law distinguished, for example, between culpable and non-culpable homicide and within the latter between accidental, justifiable and excusable homicide (even if those were not quite the terms used) and treated each of them differently. It also made a clear distinction between principals (those who had committed a crime) and accessories (those who had assisted in or contributed to its commission, whether before or after the fact) and had rules about what conduct or actions made someone an accessory and had (and observed) the rule that accessories could not be convicted until at least one of the principals had been convicted. There were also rules about the age at which persons could be held liable for criminal actions.

The second area of significant royal court jurisdiction was in respect of rights over land and other types of property right considered analogous to land. By 1189 the king's courts were offering through the assize of novel disseisin a quick and effective remedy against arbitrary or unjust dispossession (whether by their lords or by others) and whether or not the land had subsequently passed to third parties. Over time this led the development of an ever more elaborate set of rules about whether and when dispossession could be justified but these did not compromise the general principle that dispossessions needed to be justified, if challenged, in the king's court and would be remedied if they were not. They also offered a remedy (the assize of mort d'ancestor) for the heir who was being denied his entitlement to take possession of land held by a heritable title by his ancestor at the time of his death provided he was in a relatively close degree of kinship to that ancestor. More distant heirs were given a remedy (but not an assize) from the 1230s onwards when their kinsman or kinswoman had died in possession through the writs of aiel and cosinage. Writs of entry provided a remedy for claimants who could show that there was one specific flaw (there were different types of writ for different types of flaw) in the chain of title of the current tenant which had led to the wrongful dispossession of the claimant or his ancestor. And the other major, residual category of claim was the writs of right in which the claimant simply asserted his right to the land based on prior rightful possession by that claimant or his ancestor or predecessor. The king's courts also came to develop and enunciate a clear set of rules about the conveyance of rights in land: about who could and could not make valid grants; about the formalities which needed to be observed to make a valid transfer; about what controls could be exercised over grants by close kin and the lords of whom the land was held, and about what kinds of interest could be granted (the latter covering not just immediate rights of possession but also future interests). They were also engaged from an early date in the recognition, protection and enforcement of rights created by the operation of the law, by the default legal rules of the Common Law. These included the widow's right to one-third of the land held by her husband at the time of the marriage and of any land he had subsequently acquired (her dower) irrespective of whether or not the husband still held at his death, generally enforced by the action of dower *unde nichil habet* brought in one of the king's courts (most commonly the Bench). Over time the courts and the legislature developed explicit rules in relation to dower: about the minimum age for the court to be able to assume that the widow could have 'earned' her dower through sexual intercourse with her husband; about whether the marriage needed to have been formally celebrated at the church door; about whether the widow could forfeit her entitlement by living apart from her husband in adultery during his lifetime without subsequent voluntary reconciliation. The courts also developed and applied not just in the assize of mort d'ancestor but also in a wide range of other types of land litigation rules about entitlement to intestate inheritance, the only form of intergenerational post-mortem transmission of land known or allowed by the Common Law. These default rules favoured descendants, gave preference to the eldest male son or his descendants (if he had died first) but in the absence of sons divided the inheritance in equal shares between daughters and in the absence of descendants allowed inheritance between collateral kin of the side of the family from which the land had descended. Bequest of land by will was only in general allowed in some towns and cities, though after 1350 the development of the use (a precursor of the modern trust) created a way of doing this more widely elsewhere as well.

To these were added well before 1350 a body of explicit rules (and in most cases a recognisable jurisdiction) in a series of other areas of law. One was in what Roman law would have called the law of persons. The English royal courts, as early as the reign of Henry II, were claiming a monopoly of deciding the question of whether a particular individual (and his descendants) were free or unfree. Over time the courts developed a set of criteria for doing this which normally required a lord claiming his tenant to be unfree to show that he or his ancestors had been in receipt of various distinctively unfree customary dues from him and his ancestors and also to produce in court close kinsmen of the unfree tenant willing to acknowledge their unfree status. The court also developed rules about the legal capacity of married women and how far they could be bound by the actions of their husbands in respect of grants of their own landed property or in defending their rights to that land. The royal courts (mainly the Bench) also came to exercise a significant 'feudal' jurisdiction over disputes between lords and tenants over the services and customs owed by tenants in respect of the lands held by the tenants such as homage, fealty, relief, rent, knight service or scutage and suit of court and what seems to have been an exclusive jurisdiction over disputes between lords and third parties or tenants over other profitable incidental rights exercised over tenants such as the right of wardship over the lands of underage heirs and over their persons (including the right to arrange their marriages). The Common Law developed rules about what was required to show entitlement to specific services and customs or to rebut a lord's claim to them and also about the categorisation of the different forms of tenure between lord and tenant, which was important since it might determine whether the lord was entitled to exercise some of the incidental rights.

A further important area of royal jurisdiction by 1350 was in exercising controls over various forms of self-help and coercion in addition to the controls over dispossession exercised through the assize of novel disseisin. A lesser form of self-help was the use of distraint by movables or chattels, the seizure of animals or movable but inanimate goods and their retention by the distrainor in a pound until the person distrained had met some specific demand made by them. Perhaps the most common use of distraint was lords seeking the performance of feudal services owed by the tenant when it could be used both against that tenant and against any of his subtenants. It could also be used in disputes over rights of common (where the demand was for compensation for damage caused by animals grazing where they were not entitled to do so) and when enforcing the process and judgments of lower courts of all kinds (lords' courts and county and hundred courts). From the later twelfth century onwards the action of replevin could be used to test the justice of such distraints. This action could only be initiated in the county court but from the mid-thirteenth century onwards such actions were commonly removed into the king's courts, mainly the Bench. We see here the development of rules about when and under what circumstances distraint could be used and we see the courts and legislation imposing rules about where and when distraints could be made and about the value of what could be taken in distraint. These were enforced partly through civil actions in the king' courts and partly through presentments made at the Eyre. A third area of rules intended to safeguard the king's subjects from unjust coercion also started developing over the use of arrest and imprisonment. From the early thirteenth century onward, there was a quasi-executive writ available from the king's chancery (*de homine replegiando*) to secure the release of those who thought they had been unjustly imprisoned and also a trespass action for damages for those claiming they had been arrested and imprisoned without proper cause.

The second main Common Law treatise, *Bracton*, is now thought to have been written and revised at various dates between the late 1220s and the mid-1250s but it seems not to have got into wider circulation until after the death of Henry of Bratton, whose name was soon attached to the treatise but who seems to have been one of the revisers and not its main author, in 1268. *Bracton* was a much larger book than *Glanvill* and went into much greater depth but its overall coverage was much less wide than that of *Glanvill* and the work of revision was left unfinished, making it difficult to use. Although *Bracton* was cited by at least one chief justice (John of Mettingham) in 1294 as the work which the serjeants appearing before him should read for information on the point at stake in the case it did not and could not have become a work of authority. What could claim a more authoritative status were the judgments recorded on the plea rolls and we do sometimes hear of searches made for past judgment on particular matters. But there were no indexes to individual plea rolls, let alone to the whole series and the best chance of individual judgments being known was through private copies of enrolments being made, especially if they were then copied (perhaps with law reports of the same cases) into collections arranged by the type of action being brought. But for the most lawyers and justices seem to have relied on their memories of what had been done in the past and their sense of what was properly to be done in the case they were hearing. The main exception to this was legislation. The volume of legislation picked up in the course of the thirteenth century. No single decade of the century passed without some legislation and later in the century legislation was being enacted every year (and sometimes several pieces of legislation in a year). We still have private manuscript collections of legislative texts from the last quarter of the thirteenth century onwards which were intended for private consultation and use. From around 1300 onwards statutory texts were also being used in legal education as the basis for 'readings' (lectures) on the statutes, explaining their meaning and significance. Legislation was also specifically cited in original writs based on specific chapters of statutes and justices and lawyers are to be found in law reports arguing about the meaning of specific chapters or phrases in them. In these respects, it can be said that the English Common Law had become in part a text-based, written law well before 1350.

IV

The history of English Law begins well before the last quarter of the twelfth century but it is only in the last half of the reign of Henry II that it begins to make sense to talk about an English Common Law as a set of distinctive legal institutions and legal ideas and rules which apply throughout the kingdom of England and it becomes possible to track the development of those institutions and those legal ideas and rules over time through the surviving evidence of their working and their invocation and application. Knowing this earlier history helps to explain some of the distinctive features of the modern Common Law as it exists not just in England but also in the other countries to which the Common Law has been exported (beginning with the Lordship of Ireland from 1210 onwards) such as the use of juries for fact-finding, a legal profession divided between an elite of advocates and legal agents doing other legal business with a lower status and that profession's monopoly of senior judicial posts, a continuing focus in that system on remedies over rights (a legacy of the writ system), a law of property which still knows the fee simple and the multiplicity of rights in land made possible by the doctrine of estates, of co-existing rights to present and future enjoyment of rights in land. But even were that not the case the beginnings and the early development of the English Common Law would still be a subject of interest in its own right for what it has to tell us about the origins of what is now a world-wide legal system and one that (while indebted in some respects to Roman law and the civilian tradition) is not part of the family of legal systems descended from Roman law and its interpretation in the medieval and early modern universities. There was another way for a relatively sophisticated and effective legal system to develop, even in our close brotherhood of Western European nations within which we share so much common history and values.

Laudatio Glenn Shafer

Gert De Cooman

My first scientific romance – call it an infatuation, I was 15 at the time – was astronomy, and my first real love was physics, a few years later. But the love of my life is probability. Scientifically speaking, of course.

I came to probability rather late, only after finishing my PhD. And it is because of this, I think, that I have always looked at the field with more distance than people who were trained as probabilists and statisticians. I tend not to confound probability theory with a subfield of measure theory, as is so often done nowadays. I have always been interested in its foundations and ideas, much more than in its immediate successes and applications.

My views on probability theory were strongly influenced by a number of people. People whom I admire, learned a lot from, and consider to be great thinkers.

The first – and I sadly never got the chance to meet him – was Bruno de Finetti.

My second and third teachers were Peter Walley and Teddy Seidenfeld, whom I did get to meet, and became friends with. They taught me a few things: (i) that probability theory is about inference and decision under uncertainty, and (ii) that in probability theory, as in logic, a little knowledge goes a very long way – and with little knowledge I mean partial knowledge, or probabilities that are only partially specified. This runs counter to the mainstream in the field, which, in its methods and formalisms, keeps insisting on complete representations: on sample spaces and completely specified probability distributions. Even though here and there 'partial' things cannot be kept from popping up: Markov's inequality, or martingales, to give only a few important examples.

And so today I get the chance to honour and congratulate my fourth teacher in probability theory – Glenn Shafer. I knew of Glenn when I started my PhD work in 1988 – who in the field hadn't heard of the Dempster-Shafer theory of belief functions at the time! But it wasn't that part of his work that caught my attention. What made me prick up my ears was the discovery of his 1996 book *The Art of Causal Conjecture,* in 1997, in a small bookshop in Los Alamos. This book prepared me for the appearance of another book called *Probability and Finance, It's Only a Game!* Glenn published this book in 2001 with Volodya Vovk. These two books made me combine their dynamical ideas with the partial probability specifications that I had been working on before. This 'choc des idées' allowed me to come up with workable strategies for dealing with partial probability specification in dynamical systems, one topic that is now taken further by my research group here at Ghent University.

But there is more. Much more. There's more because the language, the formalism and the methods of the *Probability and Finance* book are strange. They seem very strange to the ears and minds of people trained in probability theory: people steeped in the formalism and approaches of measure-theoretic probability. In fact, they were perceived to be so strange that a number of established figures – measure theorists and functional analysts – famously failed to see their significance and potential. Why? Because the probabilistic arguments in this book seem far away from Pierre de Fermat's idea of full probability specification, far away from sample spaces, and far away from probability distributions. And because the book casts partial probability specification in a formalism that harkens back to Pascal and Huygens's approach to probability, not to Fermat's.

And, indeed, this difference in approach brings us to today's lecture and ceremony, and to my reasons for proposing Glenn Shafer as a Sarton medallist. For not only has Glenn been a productive scholar and historian of probability theory. He has also, in my view, achieved what few of us are able to: to go back to the sources, to delve into the history of scientific ideas, and to re-emerge with ideas that had been dormant, that hadn't quite made it, because of historical or cultural reasons. And then to revive them, to give them a new lease of life, so that they may yet realise their full potential.

It is for this reason, Glenn, that I am very proud and happy that I have been able to play a small role in making you the recipient of this fully deserved honour.

Pascal's and Huygens's game-theoretic foundations for probability

Glenn Shafer

Blaise Pascal and Christiaan Huygens developed game-theoretic foundations for the calculus of chances – foundations that replaced appeals to frequency with arguments based on a game's temporal structure. Pascal argued for equal division when chances are equal. Huygens extended the argument by considering strategies for a player who can make any bet with any opponent so long as its terms are equal.

These game-theoretic foundations were disregarded by Pascal's and Huygens's 18th century successors, who found the already established foundation of equally frequent chances more conceptually relevant and mathematically fruitful. But the game-theoretic foundations can be developed in ways that merit attention in the 21st century.

1. The calculus of chances before Pascal and Fermat

We are often told that probability theory began with an exchange of letters in 1654 between Blaise Pascal (1623-1662) and Pierre Fermat (1607-1665). As Florence Nightingale David put it,

> The name of Blaise Pascal is always linked with that of Fermat as one of the "joint discoverers" of the probability calculus.[1]

[1] [1], p. 75.

We can trace this attribution back to Laplace, who told his students at the École Normale in 1795 that probability theory

> owes its birth to two French geometers of the 17th century.[2]

Laplace repeated these words in 1812, in the first edition of his *Théorie analytique*,[3] but he tempered them two years later in the history of probability theory with which he concluded his *Essai philosophique*:

> For quite a long time, people have ascertained the ratios of favourable to unfavourable chances in the simplest games; stakes and bets were fixed by these ratios. But before Pascal and Fermat, no one gave principles and methods for reducing the matter to calculation, and no one had solved problems of this type that were even a little complicated. So we should attribute to these two great geometers the first elements of the science of probabilities …[4]

Many of Laplace's successors have found the nuances unnecessary. Lacroix, for example, began his 1816 probability textbook with this unqualified attribution:

> The probability calculus, invented by Pascal and Fermat, has never since ceased exciting the interest and exercising the wisdom of their most illustrious successors …[5]

Similar unqualified statements by mathematicians and historians of mathematics abound, throughout the 19th and 20th centuries and up to the present day. But Laplace was surely correct when he conceded that people had been counting chances and using the counts to fix stakes and bets long before Pascal and Fermat.

Counting chances

People have been making finely balanced dice for millennia, and they have probably been counting the chances for throws of these dice for just as

2 Apparently not published at the time, Laplace's lecture was reproduced on pp. 146-177 of Volume XIV of his complete works [2]. The words translated here come at the end of the lecture. Except when otherwise noted, all translations are mine.

3 [3], p. 3.

4 [4], p. 89.

5 [5], p. iii.

long. But the earliest documentary evidence for such counting appears to be the Latin poem *De Vetula*, probably written around 1250 by a teacher of the quadrivium (arithmetic, geometry, astronomy, and music) at the University of Paris.[6]

A long poem, touching on philosophical, religious, and scientific topics, *De Vetula* begins by warning its readers against the temptations of erotic love and gambling. In the case of gambling, the author warns that a gambler faces ruin even if he knows how to count chances, then proceeds to count them anyway for the sum of the points on three dice. There are 216 chances, he explains, all of equal force and frequency. But the sum of the points can range from 3 to 18, and these 16 possibilities have unequal force and frequency. There are 108 chances that the sum will be between 3 and 10, distributed unequally:

Sum of points	3	4	5	6	7	8	9	10	Total
# of chances	1	3	6	10	15	21	25	27	108

There are another 108 chances that the sum will be between 11 and 18, distributed similarly:

Sum of points	18	17	16	15	14	13	12	11	Total
# of chances	1	3	6	10	15	21	25	27	108

David Bellhouse has called *De Vetula* a "medieval bestseller". It was often quoted. Nearly 60 manuscript copies survive. The first printed edition appeared in about 1475. Not everyone who reproduced it understood how the author counted chances. But some did, including the editors of editions printed in 1479, 1534, and 1662.

There are at least two other surviving documents in which mathematicians counted the chances for dice before 1654: a book by Cardano, who died in 1576, and a letter by Galileo, who died in 1642. Neither appeared in print in its author's lifetime. Cardano's *Liber de Ludo Aleae* was published in his collected works in 1663,[7] and Galileo's letter appeared in his collected works in 1718.[8] Both Cardano and Galileo counted the chances for the sum

6 [6], [7], [8], [9].

7 Geralamo Cardano, *Liber de Ludo Aleae*, in [10], volume 1, pp. 262-276. English translation in [11], pp. 182-241.

8 [12], pp. 591-594. English translation [1], pp. 192-195.

of points on three dice. As Bellhouse has pointed out, Cardano's presentation suggests that he may have been influenced directly by *De Vetula*, whereas Galileo obtains the counts in a different way.[9]

As mathematics developed, mathematicians' ability to count chances improved. Galileo mentions that the number of equally frequent chances is multiplied by 6 every time a die is added to the throw. There are 216 equal chances in the case of three dice because $6 \times 6 \times 6 = 216$. The author of *De Vetula* had not mentioned this.

Fixing stakes and bets

The whole point of counting chances is to use them to fix stakes and bets. The author of *De Vetula* does not bother to explain how this is done, but readers adept in mathematics would have known what to do: use the rule of three.

The universities of medieval Europe prepared young men for careers in the priesthood, law and medicine. To learn practical mathematics, you went elsewhere – to teachers who prepared young men to work in trade. We know what these teachers taught, because countless of their manuals – *commercial arithmetics*, we call them – have survived. This being a lecture in honour of George Sarton, I pause to recall Sarton's interest in these manuals. As he pointed out in 1933, they were being written in both Arabic and in Spanish in Spain in the 11th century.[10] They spread throughout Europe as trade developed.[11]

The rule of three was the main tool of the commercial arithmetics. After learning how to add, subtract, divide and multiply, merchants and their clerks need to understand proportions. If you buy 15 bushels of wheat for 10 shillings, what price should you charge someone else for 3 bushels? For us, this is a matter of algebra: $15/3 = 10/x$, and so $x = 2$ shillings. But al-Khwarizmi's 9th-century algebra was all in words, and the medieval commercial arithmetics still had only words. Algebra with symbols emerged only in the Renaissance. It was largely developed by the authors of commercial arithmetics – the Italian abacus masters and the German

9 [13].
10 [14], [15].
11 See for example [16].

reckoning masters. But even in the 19th century, commercial arithmetics emphasized the non-symbolic rule of three, deploying it in problem after problem in which you find an unknown fourth number in a proportion from three that are known, problems about trading in goods, dividing profits, changing currencies, pricing alloys, etc., etc. Occasionally, for fun, an author might throw in a problem about a game.

Here are two questions that could have been answered by anyone who was adept at the rule of three and could count the chances for three dice.

Q1. Three dice are to be thrown repeatedly until either a 9 or a 15 appears. Player A bets on 9 and Player B bets on 15. Player A puts 5 shillings on the table. How much should Player B put on the table?

Q2. What should Player A pay in order to win 80 shillings if he throws an 11 on a single throw of three dice?

Permitting ourselves a bit of algebra rather than trying to imitate a 13th-century abacus teacher's use of the rule of three, we can answer these questions as follows.

A1. To answer Q1, we recall that there are 25 chances of throwing a 9 and only 10 chances of throwing a 15. The chances have equal frequency. So Player B wins 10 times for every 25 times Player A wins. Player A has put 5 shillings on the table for Player B to win. If we write x for the amount Player B puts on the table, then Player B wins 10×5 shillings every time Player A wins $25 \times x$. This is fair if $x = 2$ shillings.

A2. To answer Q2, suppose Player B is the counterparty. Player A gives x to Player B, and Player B gives back 80 shillings if Player A throws an 11. Player A has 27 chances of getting the 80 shillings. Player B has 216 chances of getting x. So Player A gets 27×80 shillings every time Player B gets $216 \times x$, and this is fair if $x = 10$ shillings.

These answers deploy the notions of frequency and fairness. Frequency was basic to everyone's understanding of chances for dice. Fairness comes along with the rule of three. Commercial arithmetics always sought the fair price. What actually happens is another matter; the merchant will surely ask for a bit more.

Laplace's assertion that no one before Pascal and Fermat gave principles and methods for calculating stakes and bets seems to be correct so far as the surviving public record is concerned. But we do find explicit arguments for proportionality in Cardano's 16th-century *Liber de Ludo Aleae*. Concerning bets on a throw of two dice, where there are 36 equally frequent chances, Cardano writes as follows:

> If, therefore, someone should say, "I want an ace, a deuce, or a trey," you know that there are 27 favourable throws, and since the circuit is 36, the rest of the throws in which these points will not turn up will be 9; the odds will therefore be 3 to 1. Therefore, in 4 throws, if fortune be equal, an ace, deuce, or trey will turn up 3 times and only one throw will be without any of them; if, therefore, the player who wants an ace, deuce, or trey were to wager three ducats and the other player one, then the former would win three times and would gain three ducats, and the other once and would win three ducats; therefore in the circuit of 4 throws they would always be equal. So this is the rationale of contending on equal terms; if, therefore, one of them were to wager more, he would strive under an unfair condition and with loss; but if less, then with gain. Similarly, if the 4 be included, there will be 32 favourable throws, and the number of remaining throws will be only 4. Therefore, the player will place a stake eight times as great as his opponent, because the proportion 32 to 4 is eightfold, and similarly for the other cases …[12]

The qualification "if fortune be equal" is important here. As Bellhouse has emphasized, Cardano's discourse emphasized fairness, not exact prediction.[13]

2. The division problem

Among the documents that Pascal left behind was a memorandum in Latin dated 1654, setting out his agenda for mathematical research and listing treatises he plans to complete.[14] It is addressed to an informal academy of Paris mathematicians, a group whose regular meetings Pascal was attending. By all accounts, this group descended from the equally informal scientific academy that Marin Mersenne had organized in 1635. Pascal's

12 Pp. 200-201 of [11].
13 [13].
14 Mesnard provides the Latin text, a commentary, and a translation into French on pp. 1021-1035 of Volume II of [17].

father Etienne Pascal had been part of Mersenne's circle, and Blaise had first become known as a mathematician after his father brought him into the circle as a teenager. We do not know exactly when in 1654 Pascal wrote the memorandum, but Jean Mesnard, the most assiduous of his many biographers, has argued persuasively that it was written before Pascal's correspondence with Fermat. In the memorandum, Pascal describes one of the topics on which he plans to write as follows:

> A field of research that is completely new and concerns a matter that is completely unexplored, namely structure of chances in games subject to chance, what we call in French *faire les partys des jeux*, where the uncertainty of fate is so well overcome by the rigor of calculation that each of two players can see themselves assigned exactly what they have coming. This must be sought all the more vigorously by reasoning, because there is so much less possibility to find it by experience. In fact, the uncertain outcome of a random event should be attributed more to the chance of contingency than to the necessity of nature. This is why the question has remained unsettled. But now, even if it has been a rebel to experience, it could not escape from the empire of reason. We have reduced it to an art with so much surety, thanks to mathematics, that having gained part of mathematics' certitude, it can now advance audaciously and, by virtue of the union thus achieved between mathematical demonstrations and the uncertainty of chance, and by the reconciling of these apparent opposites, it can take both names and lay claim to the surprising title *Geometry of Chance*.

This paragraph's sense of excitement is palpable; Pascal believes that he has solved a problem others had tried and failed to solve. This problem is new as a field of mathematical research but so familiar to his countrymen that it has a French name. The French noun *parti*, here spelled *party*, can be translated as *share* or as *division into shares*, and so we can translate *faire les partys des jeux* as *divide into shares in games*.

Departing from established usage, I will call the problem of how to *faire les partys* the *division problem*. Since the early 18th century, it has usually been called *le problème des partis* in French and *the problem of points* in English. But these names can be a source of confusion when we try to understand what Pascal actually wrote in his letters to Fermat.

Pascal's solution of the division problem

We learn more about what Pascal meant by *faire les partys* in the first five pages of another short document that he left behind, printed but not published and bearing the title *Usage du triangle arithmétique pour detérminer les parties qu'on doit faire entre deux jouers qui jouent en plusieurs parties*.[15] This title can be translated as *Using the arithmetic triangle to determine the divisions one should make between two players who play in several rounds*. Here I will refer to it simply as Pascal's *Usage*.

By "play in several rounds", Pascal meant that the stakes are won by the first player who wins a specified number of rounds. If the players agree to stop when neither has yet won the specified number, how should the stakes be divided?

Pascal reasons backwards from situations where the appropriate division is clear. Suppose, for example, that Players A and B have each put 32 pistoles on the table. Player A is one round short of winning the entire stakes, and Player B is two rounds short. If the players were to play one more round, the division would be clear:

- If Player A wins the round, he gets all 64 pistoles.
- If Player B wins the round, the two players are even, both being one round short of winning. So they should split the 64 pistoles evenly, each getting back the 32 pistoles he put up.

Player A is thus certain of getting at least 32 pistoles and has an equal chance of getting the other 32. Pascal argues that he can therefore claim the first 32 and half of the second 32, for a total of 48, leaving 16 for Player B.

Having found what each player is entitled to when Player A is one round short and Player B is two rounds short, we can then find what each is entitled to when Player A is one round short and Player B is three rounds short. In this case, another round would either give all 64 pistoles to Player A or put the players in the situation just analysed (Player A one round short and Player B two rounds short), where Player A is entitled to 48 pistoles. So Player A is entitled to (1) the 48 he will have in either case, and (2) half the remaining 16, for a total of 56, leaving only 8 for Player B.

[15] The treatise is reproduced in Volume II of [17] and in other editions of Pascal's works.

As Pascal explains in great detail, at this level of formality, we can reason backwards in this way to find the entitlements of the two players no matter how many how rounds each lacks. He then mentions that there are also two other ways of solving the problem: using combinations and using the arithmetic triangle. He then proceeds to explain how the arithmetic triangle enables us to obtain the answers more quickly.

We know, from Pascal's letter to Fermat dated 29 July 1654, that Pascal mastered this use of the arithmetic triangle only after that date. But from the claim he made in his earlier memorandum to his Paris colleagues, we may assume that he had discovered his method of backward recursion before beginning his correspondence with Fermat. Having understood how he could use the arithmetic triangle in the course of the correspondence, he folded his proposed *Geometry of Chance* into his *Usage*.[16]

Published antecedents

There is a slight but interesting difference between the way Pascal describes the division problem to his Paris colleagues and Fermat and the way he describes it later, in his *Usage*. In the memorandum, he writes about games *subject to chance*. In the letter of 29 July to Fermat, he writes about the two players having an *equal chance* (*le hasard est égal*). Here he could be talking not only about dice games but also about ball games and other competitions that involve both skill and chance. Such games are subject to chance, and when players play on equal terms it is not unusual to say that each has the same chance as the other, even if they do not have the same skill. It is also not unusual for players who disagree about who is more skilful to think it fair that they should bet on even terms. But in his *Usage*, Pascal specifies that he is considering *games of pure chance*.

This difference is of some significance, because previous solutions of the division problem, in handwritten commercial arithmetics and in printed

16 This speculation rests on Mesnard's conclusion that Pascal wrote his memorandum before his correspondence with Fermat. A. W. F. Edwards has challenged this conclusion on the grounds that Pascal solved the division problem only after his letter of 29 July; see [18], p. 86, reprinted in [19]. This overlooks the fact that the argument in the first five pages of Pascal's *Usage* fully solves the problem. The letter of 29 July shows Pascal struggling to obtain the more efficient and elegant solution that he later obtains using the arithmetic triangle, but it does not refute the hypothesis that he had already solved the problem.

books beginning with Pacioli's *Summa* at the end of the 15th century, had considered games where skill enters – ball games and archery competitions. Perhaps the authors also had dice games in mind, not mentioning them in order to avoid any hint of impiety, but in any case they apparently thought that their arguments applied to games where skill also enters. Their solutions of the division problem usually involved some application of the rule of three. The rule of three can be applied in various ways (in particular, do we consider the number of rounds won or the number lacking?), and so different authors obtained different answers. None of them obtained Pascal's answer, and historians of probability usually express this by saying that their answers were all wrong.[17] But it is also reasonable to conclude, with Tartaglia, that there is no single right answer.

It is also reasonable to conjecture that Pascal was aware of some of the previous efforts to solve the division problem. Would Mersenne not have known about the published work of Pacioli, Cardano, and Tartaglia? Pascal's comment about the question remaining unsettled may be a reference to their disagreements.

Unpublished antecedents

Although none of the previously published treatments of the division problem obtained Pascal's solution, we have learned in recent decades that two unpublished manuscripts by Italian abacus masters, both writing around 1400, did obtain his solution. The first, a fragment noticed in the National Central Library of Florence in 1985 by Laura Toti Rigatelli and subsequently studied by several authors, used an intricate argument to arrive at Pascal's answer for the case where one player is one round short and the other is two rounds short. The second, a commercial arithmetic noticed in the Vatican Apostolic Library in 2003 by Raffaella Franci, develops Pascal's method fully, even for more than two players. Both of these manuscripts have been discussed thoroughly by Norbert Meusnier.[18]

The author of the Vatican commercial arithmetic cautions his students not to divulge his method for solving the division problem but to study it and

[17] See for example [20], pp. 34-36.

[18] [21]. As Meusnier notes, the Florence manuscript is concerned with games of chess, supporting the hypothesis that the division problem dates back at least to Arabic sources.

stand ready to use it, perhaps to dazzle town leaders or merchants who might employ them to teach. This evidence of a secret tradition centuries before Pascal raises tantalizing but unanswerable questions. How widely was the division problem discussed by teachers of commercial arithmetic in Pascal's time? Had the Vatican manuscript's method survived in an oral tradition? As Ivo Schneider has noted, commercial arithmetic did constitute an oral tradition.[19]

Pascal was not one to cite predecessors. As A. W. F. Edwards has noted,

> Pascal was … a little forgetful about his sources. Practically everything in the *Traité* except the solution of the important "Problem of Points" will have been known to Mersenne's circle by 1637. It seems likely that Pascal absorbed most of this as a young man, and then, more than a decade later, his correspondence with Fermat stimulated him to compose the *Traité*, which he did in the space of a few weeks. The evidence is that, with the passage of time, he had lost most of the details whilst retaining the outline. Just as a lecturer often lectures best when, after careful preparation, he forgets his lecture notes, so Pascal poured forth his mature view of the Arithmetical Triangle and its uses, uncluttered with peripheral detail.[20]

It is conceivable, if unlikely, that Pascal's solution of the division problem is also something that he had picked up in his youth and then forgotten. All we can say with confidence is that Pascal believed in 1654 that it was his own new discovery.

3. Pascal's game-theoretic foundation

In his memorandum to his Paris colleagues, Pascal was concerned with the problem of dividing stakes between two players. This question comes up in his letters to Fermat, but the questions he has posed to Fermat appear to involve a more subtle kind of division, which for clarity I will call *apportionment* rather than *division*. How do we apportion a player's gains to the successive rounds of a multi-round game?

Pascal repeatedly mentions that he and Fermat have different methods for solving questions of apportionment. Fermat was using the venerable

[19] [22], pp. 269-279.
[20] [19], p. 58.

method of counting chances, which he wielded with a mathematical power unmatched by any predecessor. Pascal was using backward recursion.

In a game involving multiple rounds or multiple dice, the chances we count can also be called combinations (*combinaisons* in French), as each chance tells how all the rounds or dice come out. Pascal sometimes called Fermat's method the method of combinations. In his first surviving letter, dated 29 July, he mentions that he too had first used combinations but claims that his own method is quicker, at least in some cases. In his second surviving letter, dated 24 August, he makes a more aggressive case for his own method, claiming that it is

- more universal, applicable to any kind of apportionment under any imaginable conditions, and
- more fundamental, carrying its demonstration in itself.

As the correspondence continues, Fermat appears to convince Pascal that the method of combinations is also universal and computationally efficient. On 27 October 1654, in his final letter to Fermat that year, Pascal writes,

> I admire your method for apportionment, all the more because I understand it quite well. It is entirely yours, having nothing in common with mine, and arrives easily at the same end.

But Pascal does not retract the claim that his method is more fundamental, and from a philosophical point of view, this is the most interesting aspect of his contribution. He vindicates the claim he made in his memorandum by giving an argument for his method of backward recursion that relies on reason alone, not on experience. Because backward recursion arrives at the same end as the established method of combinations, this is also a justification of that established method.

Because Pascal's method reasons about the play of the game rather than about frequencies, we may call it *game-theoretic*.

Enter the Chevalier de Méré

The legend of Pascal's and Fermat's invention of probability was embellished in 1837 by Siméon-Denis Poisson, who began his book on probability with this sentence:

> A problem about games of chance proposed to an austere Jansenist by a man of the world was the origin of the calculus of probabilities.[21]

Jansen was a Dutch theologian, and Pascal was the Jansenist. The man of the world was Antoine Gombaud, the Chevalier de Méré. Many authors have concluded that Gombaud introduced Pascal to the problem of division we discussed earlier. It is also possible, and perhaps more likely, that Gombaud's only posed some particular questions about apportionment.

By the early 1650s, Pascal was a close friend of the wealthy and powerful Duke of Roannez. Gombaud, a nobleman of modest means, was occasionally part of the Duke's entourage. At the age of 61 (in 1668, after Pascal's death), he began to publish his letters and essays and became well known as a stylist and moralist, participating in the 17th-century French debate concerning what it means to be an honourable man (*honnête homme*). He made a great virtue of having good manners and pleasing others. He practiced these virtues, and he claimed to have taught Pascal to enjoy himself. He also claimed credit for mathematical discoveries concerning chance. It is possible, though perhaps unlikely, that Gombaud introduced Pascal to the whole topic of calculating chances.

At the beginning of his letter of 29 July, Pascal mentions that he and Fermat had been discussing two questions of apportionment that Gombaud had proposed: apportionment for dice (*les partis des dés*), and apportionment for rounds (*les partis des parties*). Fermat, he acknowledges, has answered the questions using combinations, but at this point Pascal thinks his own method is quicker. What were the questions? How were they related to what I have been calling the division problem, the problem of how to *faire les partys* that Pascal discussed in his memorandum to the Paris mathematicians and solved in the first five pages of his *Usage*? And how are they solved by Fermat's method and by Pascal's method?

In the second section of his letter of 29 July, Pascal explains how to *faire les partys* in the case where two players play to win three rounds, but he does not stop there. After explaining how to find the value to a player of each possible position (as he also did in his *Usage*), he then finds, by subtraction, how this value changes when the player wins a round. Gombaud's question about apportionment for rounds, it seems, concerned

---

21 [23]

not the value of a *position* in the game but the value of a *round* in the game. How much of his opponent's money does a player gain by winning the round? To use the language of later centuries, the question is not about *expectations* but about *changes in expectations*.

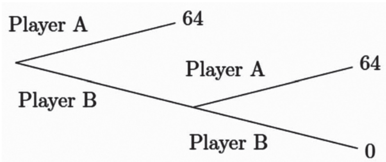
What exactly was Gombaud's other question, the question about apportionment for dice? Not having the previous letters between Pascal and Fermat, we cannot be certain. But we do have an undated fragment of one previous letter from Fermat, and it suggests some possibilities. In this fragment, Fermat says that Pascal has asked about a player who has undertaken to get a six in 8 throws and has already lost the first three. How much should he be compensated for not making his next throw? The answer depends on whether the compensation is taken out of the stake on the table, or whether that stake will all remain for him to try to win with one of his remaining throws. But Pascal must have made a slip, because the answer he gave Fermat, 125/1296 of the stake, is not correct in either case. Fermat finds a rather different question that does have 125/1296 as its correct answer.

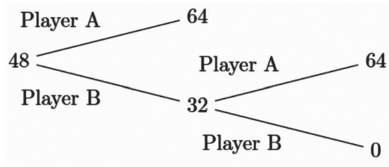
Carrying its demonstration in itself

Whereas Fermat delighted in solving problems, Pascal was more interested in getting to the bottom of things. What are the true principles? What is the real starting point?

On the first two pages his *Usage*, Pascal explains that his method is based on two fundamental principles. First, a player should take any portion of the stakes that will be his regardless of whether he wins or loses. Second, if *the game is one of pure chance*, there is as much chance for the one player as the other to win a certain sum, and they want to stop playing, then they should divide the sum equally.

Pascal makes his assertion that his method is more fundamental than the method of combinations in his letter of 24 August, in the course of explaining how he had defended Fermat's use of the method of combinations to his Paris colleague Gilles de Roberval, a teacher of mathematics at the *Collège royal*. To understand Roberval's objection to Fermat's method, consider again the classic case where Player A is one round short of winning and Player B is two rounds short. If they are playing for 64 pistoles, as in Pascal's presentation of the problem in his letter of 29 July, then we can use diagrams to picture Player A's possible gains and Pascal's argument:[22]





As indicated in the diagram to the left, Player A wins the 64 pistoles if he wins the first round; otherwise they play a second round and Player A may win either 64 or 0. As indicated in the diagram to the right, Pascal concluded that Player A's position is worth 32 pistoles right after he loses the first round and therefore 48 pistoles at the outset. Fermat solved the problem in a different way; he supposed that two rounds are played no matter how the first comes out, so that the four equally frequent chances are

> Player A wins the first round, Player A wins the second round;
> Player A wins the first round, Player B wins the second round;
> Player B wins the first round, Player A wins the second round;
> Player B wins the first round, Player B wins the second round.

Because Player A wins the 64 pistoles in three out of the four equally frequent chances, he is entitled to three-fourths of the 64 pistoles at the outset. Pascal reports to Fermat that Roberval objected to the fiction that the players would play two rounds no matter how the first came out. He then reports what he said to Roberval, including the following:

> I responded to him that I relied not so much on this method of combinations, which was not really appropriate for the problem, as on my other universal method, which misses nothing and carries its demonstration in itself, and which finds precisely the same division as the method of combinations …

In other words, the method of combinations is correct only because its results agree with Pascal's method of backward recursion.

---

[22] Pascal did not draw any such diagram; historically the first appearance of such a diagram seems to be in an unpublished note written by Huygens in 1676. See pp. 151-155 of Volume XIV of [24] and pp. 380-384 of [25].

The method of combinations does not carry its demonstration in itself, because its counting of chances relies on experience. To see the full force of Pascal's argument, we need to notice that the appeal to experience becomes less and less convincing as the number of rounds becomes greater and greater. Do we really have enough experience to know that the 6^{10} ways 10 throws of a die can come out have equal frequency?

4. Huygens's game-theoretic foundation

Although neither Pascal nor Fermat published their work on games of chance, the problems they had discussed soon became widely known through the work of Christiaan Huygens (1629-1695). Son of the prominent Dutch diplomat and poet Constantijn Huygens, Christiaan Huygens was steeped in French culture, but his first visit to Paris was delayed by the turmoil of the times until 1655. During that visit, he learned something about Pascal's and Fermat's ideas from their Parisian colleagues.

From what he heard in Paris, Huygens saw an opportunity to apply the new understanding of algebra that he had learned from Descartes through his teacher Francis van Schooten, and this led him to write an account of calculation in games of chance that Van Schooten could publish as an appendix to his forthcoming textbook on algebra. He drafted it in Dutch in 1656. Van Schooten translated it into Latin for the Latin version of his textbook, which appeared in 1657. The Dutch version appeared in 1660.[23]

By casting the matter in terms of algebra, Huygens deepened Pascal's foundational argument, making it more game-theoretic. Instead of relying on the principle that chance gives contending players equal claims, Huygens's argument relies merely on the players' willingness to contend on equal terms.

What did Huygens learn in Paris?

What did Huygens learn from the Paris mathematicians about the problems Pascal and Fermat had discussed? In an insightful article published in

23 Discussion between Huygens and Van Schooten concerning the translation is preserved in letters published in [26].

1982,[24] Ernest Coumet called attention to three letters written by Huygens that cast light on this question. In a letter to Van Schooten dated 20 April 1656, Huygens wrote:

> Here is what you wanted concerning games of chance … You can judge the difficulty of this material from the fact, among others, that Pascal, a young man with the most penetrating mind, said that he had never encountered anything so obscure, and that nothing had ever required more effort from him. For his part, he certainly went deeply into the questions I consider, or most of them, as did Fermat. But what principles did they rely on? I think no one yet knows.

On May 6, Huygens wrote again to Van Schooten:

> It would be appropriate to put at the beginning, as a preface, a letter from me giving some explanations about the material itself and who first undertook to study it, along with what I learned in France about Pascal's discoveries in this domain. Very little I suppose, but just the same I don't think I can conceal it.

Then on July 21, Huygens wrote to the English mathematician John Wallis:

> I have recently used demonstrations of this type [by algebra] in a treatise on the use of calculation in matters of chance, which Van Schooten proposed to publish with his own work, now being published. I came by the opportunity in France, where mathematicians had asked me questions like this: In how many throws can one expect to get a six with a die like those now usually used? Or to get a double six with two dice? And many more of the same type, for whose solution it was not at all easy to find the first principles.

The tone of these passages suggests that calculation in games of chance was not a surprising topic for mathematicians in the 1650s. Huygens did not learn much that was new in Paris. We also see that Huygens, like Pascal, was a seeker after first principles.

We may surmise that the Paris mathematicians who posed the questions to Huygens could answer some or all of them, and that they remembered that Pascal had a way of justifying the answers that went deeper than counting

[24] [27], reprinted on pp. 437-452 of [28].

chances, but that they had never fully understood Pascal's arguments. Perhaps Pascal never fully explained everything he could do with his two principles.

From April 1656 to March 1657, Huygens corresponded with Pierre Carcavy and the Paris mathematicians Roberval and Claude Mylon. The correspondence with Carcavy put him in indirect touch with Pascal and Fermat, who both provided additional questions that he added to his treatise with answers but without solutions. Coumet saw in this correspondence a further attempt on Huygens's part to learn Pascal's and Fermat's first principles, ultimately unsuccessful because he did not ask his questions directly, not wanting to reveal how much or little he himself already understood.

Huygens's preface did acknowledge that renowned French mathematicians had worked on his topic. He added that

> though they have tried to solve many a difficult question by corresponding with each other, they have concealed their own mode of invention. I, therefore, was obliged to examine everything from the beginning to the end and am not yet sure that the point whence we started was the same.

This passage can be taken as a claim by Huygens he did not learn anything from the Parisians about how to *faire les partys*, but Huygens's letters to Van Schooten and Wallis support the skepticism about such a claim that has been expressed by Coumet, Edwards, and Schneider. Coumet asks whether we may be misunderstanding Huygens's words. Did his 17th century audience read "mode of invention" (*manier van uytvinding*) as merely a way of finding an answer or, as the sentence following it might suggest, something deeper?

We may also ask whether Huygens was really unaware of what the authors of commercial arithmetic had said about how to *faire les partys*, Ivo Schneider argues that the form of *De Ratiocinniis* (formal propositions, with full explanations, followed by problems with numerical answers but no explanations) suggests familiarity with the work of the reckoning masters.[25]

[25] [29], p. 182.

The first sentence of *De Ratiocinniis*, in its Latin version at least, suggests that the treatise is concerned with games that depend only on chance. Here is the sentence in Latin:

> Etsi lusionum, quas sola sors moderator, incerti solent ese eventos, attamen in his, quanto quis ad vincendum quam perdendum propior sit, certam Semper habet determinatiionem.

Here it is in Dutch:

> Al-hoewel in de spelen, daer alleen het geval plaets heeft, de uytkomsten onseecker zijn, soo heeft nochtans de kansse, die yemandt heeft om te winnen of te verliesen, haere seeckere bepaling.

The editors of Huygens's complete works translate this somewhat archaic Dutch into French in a way that makes it agree with the Latin:

> Quoique dans les jeux de hasard pur les résultats soient incertains, la chance qu'un joueur a de gagner ou de perdre a cependant une valeur déterminée.[26]

The French is easily translated into English:

> Although outcomes in games of pure chance are uncertain, the chance a player has to win or lose nevertheless has a definite value.

To the extent that he was concerned with games of pure chance, Huygens was following Pascal. But whereas Pascal emphasized that he was considering only games of pure chance in order to justify his second principle, Huygens makes an argument that applies equally well to the mixed games that had been considered by authors in the tradition of the commercial arithmetics.

Using algebra

Huygens invented his own first principles, and they went deeper than Pascal's. A concise and insightful explanation of Huygens's principles was provided by Hans Freudenthal in 1980.[27] Here is Freudenthal's translation from the Dutch of Huygens's first proposition.

---

26 [24], p. 60.
27 [30]

> PROPOSITION I. If I have the same chance to get a or b it is worth as much to me as $(a + b)/2$.
> In order not only to prove but also to discover this rule, I put x for what the chance is worth to me. Hence having x I must be able to arrive at the same chance by an equitable game. Let it be the game which I play against another with stake x, where the other is also staking x; and let it be agreed that the one who wins shall give a to the one who loses. This game is equitable, and it appears that by this I have an equal chance to win a, that is, even if I lose the game, or $2x - a$ if I win, because then I get the stakes $2x$ from which I must give the other a. Suppose that $2x - a$ were as much as b, then I would have the same chance for a and b. So I put $2x - a = b$, and it follows that $x = (a + b)/2$ for the value of my chance. The proof of this is easy, because having $(a + b)/2$, I can venture against another who will also stake $(a + b)/2$, with the stipulation that the one who wins the game shall give a to the other. Therefore I will have an equal chance to get a, that is to say if I lose, or b if I win, because then I take $a + b$, which is the stake, and from this I give him a.

Huygens begins with the principle of fairness that the players must be treated the same. If two players both put up $(a + b)/2$, and the winner gets a and the loser gets b, then the two players are being treated the same.

Huygens's proof of his first proposition is a nice illustration of the new role of algebra in the 17th century. As Fermat had learned from Vieta and Van Schooten had learned from Descartes, you can use algebraic equations to discover solutions to geometric or physical problems, but to achieve certainty you must translate this discovery into a proof in the style of the Euclid and the other ancients.[28] In Huygens's proposition, as in geometry, the synthetic proof has a constructive character. It says that $(a + b)/2$ is the right value because I can use this amount to reconstruct my position. In the contemporary language of finance, it is the cost of hedging the position.[29]

From his first proposition, Huygens moved quickly to his third proposition:

> PROPOSITION III. If the number of chances I have for a is p, and the number of chances I have b is q, then assuming that every chance can happen as easily, it is worth to me as much as $(pa + qb)/(p + q)$.

---

28 [31,32]
29 [33]

Here is the synthetic version of Huygens's proof: a fair arrangement where I risk $(pa + qb)/(p + q)$ to get p chances for a and q chances for b. Consider a game where I and $p + q - 1$ other players each have an equal chance of winning. Each player puts up $(pa + qb)/(p + q)$ and the winner takes it all; this is evidently fair. I make a fair side bet with each of q of my opponents: if one of us wins, he will give the other b. I also make a fair side bet with each of my remaining $p - 1$ opponents: if one of us wins, he will give the other a. If one of the q opponents wins, I end up with b. If one of the $p - 1$ opponents wins, I end up with a. If I win, I get the $(pa + qb)/(p + q)$ put up by each other $p + q$ players, myself included, but I pay b to q opponents and a to $p - 1$ opponents, netting

$$(p + q)\ (pa + qb)/(p + q) - qb - (p - 1)a = a.$$

So I have p chances for a and q chances for b.

Here Huygens has done something left undone by Pascal. He has derived from first principles a general rule for calculating from equally possible chances how stakes should be fixed. In the writings Pascal left behind, we see this done only for the case where there are only two equal chances; Pascal called this a "lemma" in his *Usage*.

Ivo Schneider has raised an objection to Huygens's argument. Huygens's fundamental principle is that players should be treated alike. But here one player gets to set the bets. He arranges side bets with many players, and as a result his position is different from that of the others.[30] As this objection illustrates, Huygens's notion of fairness is not defined with mathematical precision. We cannot say that Huygens has a game-theoretic foundation that meets the standards of rigor of modern game theory, in which the rules of play are clearly specified.

Huygens also uses algebra in his last proposition, which I paraphrase as follows:

> PROPOSITION XIV. Player A and Player B take turns throwing two dice. Player A wins if he throws 7 points before Player B throws 6 points. If Player B throws first, what is the ratio of their chances?

Whenever it is Player A's turn to throw, he has 6 chances out of 36 to win on that throw; whenever it is Player B's turn, he has 5 chances out of 36 to

30 [29].

win on that throw. Huygens wrote a for the stakes for which they are playing; let us simplify by setting $a = 1$. Huygens wrote x for the value of Player A's chance at the outset and y for the value of his chance if and when he gets to throw again, after Player B has lost his first throw. At the outset, Player B has 5 chances to win and 31 chances to put Player A in the position where the value of his chance is y. So

$$x = (5/36) \times 0 + (31/36) \times y.$$

If Player B loses his first throw, then Player A has 6 chances of winning on his first throw and 5 chances of returning to x. So

$$y = (6/36) \times 1 + (30/36) \times x.$$

Solving the two equations, we find that $x = 30/61$. So the ratio of Player A's chance to Player B's is 30 to 31.

This argument may have seemed a little intricate at the time, but it is an impressive advance on what medieval mathematicians could do. Player A first throws two dice, and if he loses Player B throws two dice again. So solving the problem by the rule of three requires somehow considering the 1296 chances for the result of throwing four dice.

5. Back to frequency

Laplace got it right in 1814. The calculus of games of chance, in a mathematically rudimentary form, goes back centuries if not millennia before Pascal and Fermat. Born from the experience of dice players, this calculus had always been a calculus of frequencies. So it is not surprising that Pascal's and Huygens's game-theoretic foundations quickly disappeared, pushed aside with little ado by the deeply entrenched concept of equally frequent chances.

Huygens's immediate successors in the development of the calculus of probability were Montmort, De Moivre, and Bernoulli. Each, in his own way, favoured and developed Fermat's method of combinations, not because it was Fermat's method, but because it was everyone's method.

Montmort

Pierre Rémond de Monmort (1678-1719) published his own book on games of chance in French in 1708.[31] Montmort explains that he learned the elements of the subject from Pascal's *Usage*, but he relies primarily on the method of combinations. He ignores Pascal's and Huygens's foundational arguments, returning to the rule of three to argue that if a player has m chances out of $m + n$ to get A, his expectation (*sort*) is $mA/(m + n)$.[32] Accused by De Moivre of following Huygens (because he had used some algebra), he denied having learned anything important from Huygens, dismissing what he called Huygens's "lemma" as mere common sense.[33] (Here he was probably referring to Huygens's Proposition III, perhaps also confusing it with Pascal's lemma.)

De Moivre

Abraham De Moivre (1667-1754) published a far-reaching article in Latin in 1711.[34] He began with two principles. First, if two players contend for the sum a, and p out of $p + q$ chances favor the event that the first player wins, then his expectation is worth $pa/(p + q)$. Second, multiplication is used to find the numbers of chances for events that have no dependence on each other. These are essentially the medieval principles, updated by explicit reference to multiplication and to the concept of an event. De Moivre later explained that he had learned the elements from Huygens, but that he was determined to use combinations rather than Huygens's (algebraic) method.

Bernoulli

Jacob Bernoulli (1655-1705) worked on probability well before Montmort and De Moivre, but his book on the topic was published after his death, in 1713.[35] The book begins by reproducing Huygens's treatise with commen-

---

31 [34,35]
32 [34], pp. 3-4; [35], pp. 75-76.
33 [35], p. xxx.
34 [36,37]. See also [38,39].
35 [40,41]

tary. But Bernoulli then turns to the method of combinations. His commentary on Huygens's first three propositions suggests that he does not find Huygens's constructive argument necessary. He suggests that it can be replaced by reasoning that is "more popular" and "more adapted to common comprehension", using merely the assumption that two players together are sure to win the entire stakes and should be treated equally. He then notes that Huygens's rules are analogous to the rules for mixtures used in business mathematics.[36]

6. Conclusion

George Sarton saw the history as essential to a scientist's understanding of his subject. As he once wrote,

> 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.[37]

Marie-France Bru and Bernard Bru have made the same point with these words:

> To penetrate to the reasons of things, look at how they have gradually been revealed in the course of time, in their progression and in their ruptures …[38]

Since the time of Laplace, successive students of probability have pursued this historical method in their quest for a clearer understanding of probability. Here are some thoughts about how the preceding perspectives on Pascal, Fermat, and Huygens can help us with contemporary puzzlements.

Conceptual revolution?

The legend of Pascal and Fermat was embellished yet further in the 1970s, when philosophers and historians reviewed the history of science in search for examples of conceptual change. Most famously, Ian Hacking argued

---

36 [41], pp. 134, 138.
37 Quoted by Stadler [42], p. 74.
38 [8], p. 287.

that the correspondence between Pascal and Fermat marked the emergence of a dual concept of probability, combining belief and frequency. In 1975, Hacking wrote:

> Probability, as we now conceive it, came into being about 1660. It was essentially dual, on the one hand having to do with degrees of belief, on the other, with devices tending to produce stable long-run frequencies.[39]

This thesis of a conceptual revolution for probability has been widely repeated and further embellished, in both scholarly and popular contexts. Here, for example, is an assessment offered by Keith Devlin, a widely read writer on mathematics:

> The Pascal-Fermat correspondence showed that it is possible to use mathematics to see into the future.[40]

The history recounted in this article suggests a greater conceptual continuity. In the case of dice at least, Pascal and Fermat connected frequency with betting on the future in the same way as the author of *De Vetula* had 400 years earlier, and we have every reason to suppose that dice players had been making the same connection for millennia.

The advances that we see in Pascal's and Fermat's reasoning, then in Huygens's treatise and the following work by Montmort, De Moivre, and Bernoulli, are primarily advances in mathematics, not conceptual changes. These scholars' increasing facility with numbers made it possible for the first time to fix stakes and bets in games that were, as Laplace put it, even a little complicated. Perhaps Pascal's and Fermat's most important contribution was to offer to Huygens the more difficult problems that he stated at the end of his treatise, with answers but without explanations. Montmort, De Moivre, and Bernoulli all began their work on probability by solving these problems.

The arguments advanced by Pascal and Huygens did contain the seeds of a conceptual revolution, one that retained the role of fairness but replaced frequency with reasoning about the structure of the game. But this was an aborted revolution, because the connection between frequency and betting was so firmly entrenched.

---

39 [43], p. vi.
40 [44], p. 164.

The most important conceptual development spurred by Pascal and Huygens was the ambition that their mathematical successes awoke, in Bernoulli and his 18th century successors, to extend their calculations from games of chance to other problems of uncertainty, thus making the calculus of chances a calculus of probability. But this is another story.

Modernizing the game-theoretic foundation

In the 20th century, mathematical probability became pure mathematics. Attribution of meaning to its terms is now an exercise undertaken after the theory is first developed by pure reason, without any intrusion of ideas about fairness or frequency.

The pure mathematics of probability can be developed either measure-theoretically or game-theoretically. The measure-theoretic development is an abstract generalization of the counting of chances; probabilities and the corresponding expected values being taken as given.[41] The game-theoretic development is an abstract generalization of Huygens's picture of a player who is allowed to construct betting strategies.[42] In both developments, frequencies enter the picture through Bernoulli's theorem and its many generalizations. In the measure-theoretic development, these theorems say that basic probabilities (which play the role of the classical equal chances) will be approximated by frequencies with high probability. In the game-theoretic development, they say that the player has a strategy that multiplies the capital he risks by a large factor if the approximation fails. The measure-theoretic development can then be connected with frequencies in the world through the presumption that easily specified events with high probability will happen, while the game-theoretic development makes the same connection through the presumption that simple betting strategies will not succeed.

The modern game-theoretic formulation begins with a game in the sense of modern game theory, defining players, rules for play, and a rule for who wins. This takes us away from Huygens's and Pascal's notion of fairness as symmetric treatment of players. As Schneider's objection to Huygens

[41] Usually cited in this connection is Andrei Kolmogorov's *Grundbegriffe der Wahrscheinlichkeitsrechnung*, [45]. See also [46].

[42] See [47].

shows, the notion of a strategy for betting fits awkwardly with such symmetry. The game we need has instead one player who gives odds or prices, another who is allowed to choose how to gamble at those odds or prices and can therefore construct strategies, and another who decides outcomes.[43]

Acknowledgements

This article is dedicated to the memory of my first teacher of the history of probability, Florence Nightingale David (1909-1993).

I owe debts of gratitude to all those who have shared with me their passion for the history of probability and statistics, beginning with Ms. David in 1970. On the topics of this article, I have especially benefitted from conversations with Tom Aldrich, David Bellhouse, Bernard Bru, Gert de Cooman, Hans Fischer, Nell Painter, Ivo Schneider, Martina Schneider, Steve Stigler, Edith Sylla, Lukas Verburgt, Volodya Vovk, and Sandy Zabell.

References

[1] David FN. Games, Gods, and Gambling. London: Griffin; 1962.

[2] Laplace PSd. Œuvres complètes de Laplace, Volume XIV. Paris: Gauthier-Villars; 1912.

[3] Laplace PSd. Théorie analytique des probabilités. 1st ed. Paris: Courcier; 1812.

[4] Laplace PSd. Essai philosophique sur les probabilités. 1st ed. Paris: Courcier; 1814.

[5] Lacroix SF. Traité élémentaire du calcul des probabilités. Paris: Courcier; 1816. Second edition 1822.

[6] Robathan DM. The Pseudo-Ovidian De Vetula: Text, Introduction, and Notes. Amsterdam: Hakkert; 1968.

[7] Bellhouse DR. De Vetula: A medieval manuscript containing probability calculations. International Statistical Review. 2000;68(2):123-136.

[8] Bru MF, Bru B. Dice games. Statistical Science. 2018;33(2):285-297. Translation of pp. 299-314 of Volume 2 of [9].

[9] Bru MF, Bru B. Les jeux de l'infini et du hasard. Besançon, France: Presses universitaires de Franche-Comté; 2018. Two volumes.

[10] Cardano G. Opera Omnia. Lyon: Huguetan and Ravaud; 1663.

[11] Ore O. Cardano: The Gambling Scholar. Princeton; 1953.

[43] In addition to [47], readers may consult the working papers at www.probabilityandfinance.com for the development of this picture.

[12] Galilei G. Opera. vol. 8. Firenze: Barbera; 1898.

[13] Bellhouse DR. Decoding Cardano's Liber de Ludo Aleae. Historia Mathematica. 2005;32:180-202.

[14] Sarton G. Query no. 23. Arabic "commercial" arithmetic. ISIS. 1933;20(1):260-262.

[15] Gandz S. The rule of three in Arabic and Hebrew sources. ISIS. 1934;22(1):220-222.

[16] Høyrup J. Fibonacci: protagonist or witness? Who taught Catholic Christian Europe about Mediterranean commercial arithmetic? Journal of Transcultural Medieval Studies. 2014;1(2):219-247.

[17] Pascal B. Blaise Pascal: Œuvres complètes. Desclée de Brouwer; 1964-1992. 4 volumes, edited by Jean Mesnard.

[18] Edwards AWF. Pascal and the Problem of Points. International Statistical Review. 1982;50:259-266. Reprinted on pp. 144-156 of [19].

[19] Edwards AWF. Pascal's Arithmetic Triangle: The Story of a Mathematical Idea. 2nd ed. Baltimore: Johns Hopkins; 2002.

[20] Hald A. A History of Probability and Statistics and their Applications before 1750. New York: Wiley; 1990.

[21] Meusnier N. Le problème des partis bouge … de plus en plus. Electronic Journal for History of Probability and Statistics. 2007;3(1).

[22] Schneider I. Christiaan Huygens' contribution to the development of a calculus of probabilities. Janus. 1980;LXVII:269-279.

[23] Poisson SD. Recherches sur la probabilité des jugements en matière criminelle et en matière civile, précédés des règles générales du calcul des probabilités. Paris: Bachelier; 1837.

[24] Huygens C. Œuvres complètes de Christiaan Huygens. vol. XIV. La Haye: Nijhoff; 1920.

[25] Shafer G. The Art of Causal Conjecture. Cambridge: The MIT Press; 1996.

[26] Huygens C. Œuvres complètes de Christiaan Huygens. vol. I. La Haye: Nijhoff: 1888.

[27] Coumet E. Sur « le calcul ès jeux de hasard » de Huygens. Dialogues avec les mathématiciens français (1655-1657). In: Taton R, editor. Huygens et la France. Paris: Vrin; 1982. p. 123-138. Reprinted as pp. 437-452 of [28].

[28] Martin T, Roux S, editors. Œuvres d'Ernest Coumet. vol. 1. Besançon: Presses universitaires de Franche-Comté; 2016.

[29] Schneider I. Christiaan Huygens' non-probabilistic approach to a calculus of games of chance. De zeventiende eeuw. 1996;12(1):171{185.

[30] Freudenthal H. Huygens' foundations of probability. Historia Mathematica. 1980;7:113-117.

[31] Mahoney MS. The Mathematical Career of Pierre de Fermat (1601-1665). 2nd ed. Princeton: Princeton University Press; 1994.

[32] Guicciardini N. Isaac Newton on Mathematical Certainty and Method. Cambridge, Massachusetts: The MIT Press; 2009.

[33] Stigler SM. Chance is 350 years old. Chance. 2007;20(4):33-36.

[34] Montmort Pd. Essay d'analyse sur les jeux de hazard. 1st ed. Paris: Quillau; 1708.

[35] Montmort Pd. Essay d'analyse sur les jeux de hazard. Seconde édition. Revûe et augmentée de plusieurs lettres. Paris: Quillau; 1713.

[36] Moivre AD. De mensura sortis, seu de Probabilitate Eventuum in Ludis a Casu Fortuito Pendentibus. Philosophical Transactions of the Royal Society of London. 1710-1712;27:213-264.

[37] De Moivre A. The Doctrine of Chances: or, A Method of Calculating the Probabilities of Events in Play. London: Pearson; 1718. Second edition 1738, third 1756.

[38] Schneider I. Der Mathematiker Abraham de Moivre (1667-1754). Archive for History of Exact Sciences. 1968;5(3-4):177-317.

[39] Bellhouse DR. Abraham De Moivre: Setting the Stage for Classical Probability and Its Applications. Boca Raton: CRC; 2011.

[40] Bernoulli J. Ars Conjectandi. Basel: Thurnisius; 1713.

[41] Bernoulli J. The Art of Conjecturing, together with Letter to a Friend on Sets in Court Tennis. Baltimore: Johns Hopkins University Press; 2006. Translation of [40] and commentary by Edith Sylla.

[42] Stadler F. George Sarton, Ernst Mach, and the Unity of Science Movement: A Case Study in History and Philosophy of Science. 2018:31:63-121,

[43] Hacking I. The Emergence of Probability. New York: Cambridge University Press; 1975. Second edition 2006.

[44] Devlin K. The Unfinished Game: Pascal, Fermat, and the Seventeenth-Century Letter that Made the World Modern. New York: Basic Books; 2008.

[45] Kolmogorov AN. Grundbegriffe der Wahrscheinlichkeitsrechnung. Berlin:Springer; 1933. English translation; Foundations of the Theory of Probability, Chelsea, New York, 1950, second edition 1956.

[46] Shafer G, Vovk V. The sources of Kolmogorov's Grundbegriffe. Statistical Science. 2006;21(1):70-98.

[47] Shafer G, Vovk V. Game-Theoretic Probability and Finance. Hoboken, New Jersey: Wiley; 2019.

Laudatio Gita Deneckere

Marjan Doom

Professor Gita Deneckere is a Full Professor at the History Department, where she heads the *Social History since 1750* research group. She kick-started her academic career in 1993, when she completed a doctorate in History with a doctoral dissertation on social history and collective action in the nineteenth and twentieth centuries. She later cofounded the inter-university *Institute for Public History* (IPG) that in its capacity as a centre of excellence translates and valorises historical research to be presented at museums, in exhibitions, books, and cultural heritage institutions, among other things. Up until last year, Professor Deneckere was a member of the Ghent University Board of Governors. At the start of this academic year, she became dean of the Faculty of Arts and Philosophy.

More so than anyone else, historians realise that their discipline is the product of the society they live in, yet at the same time it helps shape that society. For years, history was sometimes looked down on, and perceived as part of a bourgeois middleclass education – just think about the *'vaderlandse geschiedenis'* (or: 'History of our Fatherland'). Moreover, historians would deal in myths, including those surrounding the battle on the Groeningekouter. Luckily, nowadays this appropriation of history is largely a thing of the past. Or, is it? In an age when identity movements are making sure their voices are heard, we should not forget that historians themselves are part of history. In fact, this perfectly illustrates what George Sarton once stated, i.e. "History of science is the history of mankind."

In the jubilee book commemorating our *alma mater* Ghent University's bicentennial, Professor Deneckere discusses in detail just how interwoven scientific research and the role of research output in society are. The title as such reflects its mission: *From the Ivory Tower.* In the book's introduction, the author herself puts out "an active call for the university to step out in society and to strive for societal impact." This echoes a modern translation of Ghent University's motto, and is in keeping with the current mission, which "aims to situate its educational and research activities within the broader social context and to remain in continual dialogue with all parties concerned." Incidentally, fully integrating science and scientific research is one of the main aims of the Ghent University Museum. Obviously, we were quick to approach Professor Deneckere to become a member of the brand-new museum's scientific committee. Therefore, our admiration for this committed academic should not come as a surprise.

Similar to a university, a museum should aim for societal impact, by offering the audience a public forum acting as a catalyst for a dialogue between its collection, its genesis and its actively engaged visitors. As one should expect from a university museum, the debate it stirs up should be rooted in knowledge and expertise. In order to fulfil its societal role, this museum does not have to keep to the side-lines and is allowed to make itself heard in the public debate. Although I am slightly hesitant to use the word 'activist', I am quietly confident that Professor Deneckere would appreciate that term. In her literary ode to our beloved alma mater, she puts it like this: "In the Ghent University Museum our university literally steps down from its ivory tower to show society at large what it has meant in the past. Not because of the past itself, but with a view to the future. That sense of wonder that incites research and the dialogue with society are the museum's guiding principles." According to Professor Deneckere, the question "Can science save the world?" follows on seamlessly from Sarton and Van de Velde's humanist vision of science.

Researchers should communicate with one another in a standardised international scientific language in order to verify results and analyses with their peers. However, this does not mean scientists should not attempt to popularise their work. What is crucial here is that democracy in its broadest sense is being supported, and not merely the fact that in the end the taxpayer funds academic research.

The more we lose touch with the nuanced gaze science provides us with, and the less we believe in the power of that gaze, the bigger the democratic deficit. However, in a time when scientific findings can quite easily be put aside as ‘fake news’, researchers’ responsibility to communicate – and to communicate well – about their work is growing. An exhibition is an excellent way to invite audiences to experience academic research, and for researchers to valorise it. Moreover, it opens up possibilities for the opinions and ideas of society to be voiced to the scientists. Professor Deneckere does not need convincing when it comes to this point, as she supervises the project that is responsible for designing the walk through the exhibition on the history of psychiatry in museum Dr. Guislain.

Professor Deneckere scientifically coordinated *City and university. Since 1817,* an exhibition that looks back at the interaction between city and university over the last 200 years, in the Ghent city museum STAM. As part of that exhibit, and in order to provide some additional context, she organised so-called *‘Salongesprekken’*, or salons. For these conversations, she invited a pair of scientists on Sunday afternoons to discuss their academic life stories. She would ask them about their take on science and their commitment to society. During these ‘drawing room discussions’ the speakers would not just ponder the past, but would also think about the role Ghent University could play in the 21st century society. The history of scientific endeavours at an academic institution, thinking about how they helped shape society, and their important role when it comes to keeping society on course – Doesn’t that sound incredibly similar to the ideals Sarton promoted?

In her book *Leopold I: de eerste koning van Europa* (or *Leopold I: The First King of Europe*) Professor Deneckere made use of Leopold’s personal correspondence to paint us a portrait of a melancholy monarch who was able to balance personal matters and professional affairs like none other. In 2013, she was awarded the Henriette de Beaufort prize for this work, and in 2014 she was presented with the Prix Jean Stengers for that same biography. Critics described the book as “a very lively read.” The jury who awarded the Henriette de Beaufort prize agreed this *Leopold* I is an undeniably delightful book. They stated that this gripping, broadly historical tale promises to be enjoyable for all readers precisely because, through the king’s personal letters, a truly relatable human story is told. Additionally, it was written in an intelligible and easily accessible form, using clear and plain language.

Similarly *1900: België op het breukvlak van twee eeuwen* (or *1900: Belgium at the Intersection of Two Centuries*) was lauded because of its "vivid representation of politics and culture." The anthology *Een geschiedenis van België* (or: *A History of Belgium*) was also celebrated for its "in-depth analysis tied to an understandable synthesis." Regional history is also part of Professor Deneckere's research, for instance in her work entitled *Van scholen zonder God, verlos ons Heer* (or: *Deliver us, Lord, from Schools without God*), which was the City of Ronse's Delghust prize.

In a way, the *UGentMemorie* project, in which Professor Deneckere is the driving force, and which serves as the virtual memory of our university, should also be categorised under 'local history'. The project is characterised by a bottom-up approach, as it relies on the valuable input of non-historians. This is further proof of Professor Deneckere's focus on including so-called 'forgotten history' in her oeuvre. There is ample room for the role and function of, for instance, women, in various social contexts. Gender history is a common theme throughout her career as well as her research projects. Both the emotional expression of juvenile delinquents, and the architecture of *'volkshuizen'* (or: 'people's homes') feature in her work. Historiography is regarded as an ongoing process, and not as a means to reveal 'the' truth; Reality is seen as a kaleidoscope of alternating faces, not as a snapshot frozen in time. This is our university's slogan 'Dare to Think' at its best. It is crystal clear how our intentions with the Ghent University Museum are linked to Professor Deneckere's work. We primarily want to offer a metaperspective on science, not simply through the eyes of a single discipline, but from the diverse viewpoints of the humanities, natural sciences, and social sciences. Moreover, our aim is to do so in dialogue with the arts. It is our goal to emphasise the importance of a continuous line of questioning, and a drive to keep investigating. After all, not being able to reach any kind of absolute truth, is what spurs on knowledge development.

For all of these reasons, the Ghent University Museum and I feel proud – and privileged – to be able to put forward as this year's Sarton medallist … Professor Gita Deneckere.

The History of Science and the New Humanism

George Sarton's Legacy: a Source of Inspiration for the Ghent University Museum (GUM)

Gita Deneckere

"Scientists from future generations who will study my life, will at times wonder whether I was mad; I was not mad, but seemed to be, since I was overwhelmingly dominated by two passions, a passion for science, and an equally ardent passion for the 'humanities'." George Sarton spoke these words in 1955 when he, one year before he passed on, received the very first George Sarton Medal awarded by the History of Science Society (HSS). He might have founded that association himself back in 1924, yet the HSS's Counsel agreed that none of the living science historians were more deserving of the medal than Sarton himself. For that reason, Sarton was given this extremely prestigious medal for lifetime achievements featuring his own portrait. At the back of the medal was an image of the goddess Isis, a copy of a drawing made by Sarton's spouse Mabel Elwes for her husband's ex libris.

I have discovered my own passion for the history of science just fairly recently. It only truly blossomed when I was writing my book, *From the ivory tower,* about Ghent University's bicentennial.[1] I could never have written that book in such a limited period of time without the digital treasure trove filled with stories about our university I could peruse via

[1] Deneckere G., *Uit de ivoren toren. 200 jaar Universiteit Gent.* Ghent, Tijdsbeeld, 2017; translated as *From the Ivory Tower. 200 years of Ghent University.* Ghent, Tijdsbeeld, 2018.

UGentMemorie, our alma mater's virtual memory.[2] I wish to express my warmest and sincerest gratitude to my UGM-colleagues Fien Danniau, Ruben Mantels, Davy Verbeke, Frank Cotman and Christophe Verbruggen, and also to all students and other authors who helped shape this collective memory, through both words and images, with this university's societal impact as a common theme running through each of the stories. They share in the honour bestowed upon me today as I receive this 'local' Sarton-medal; it is fair to say, that without them, I would have never been considered for this award.

I 'rediscovered' 'our' George Sarton in the contexts of UGentMemorie and the bicentennial of Ghent University in 2017 – In fact, those projects made me realise just how great his work really is. Sarton's achievements inspired me to such an extent that I am now convinced we should continue to use his legacy as a source of inspiration, not only when it comes to the Ghent University Museum (GUM) – which I will try to demonstrate in this talk –, but also within 21st century higher education, especially within the humanities. In this respect, Sarton's *nachleben* at Ghent University is particularly interesting. It seems Sarton enjoys a periodical revival with every new generation. Three decades after the previous generation of Sarton-devotees handed out the very first medal I gladly pick up the torch.

In George Sarton's opinion, exploring the history of science was the best possible way to bring science closer to the public at large, or in other words, to humanise it. He did not mind the biographical method in the least, as this could provide an extremely 'vivid' view on the progress being made within scientific research, which could then also be incorporated in social and cultural history. I will be using the biographical method as well, and based on Sarton's life story I will attempt to convey his passion for the history of science as a new type of humanism.

The origins of Sarton's passion for the history of science can be traced back to his student days in Ghent. He was born in 1884, and was a student at this university from 1902 to 1911. After feeling disappointed by his studies in philosophy, he decided to study mathematics instead. He dedicated his doctoral thesis to Isaac Newton. "Science saved my life," he would later write down.

---

2 www.ugentmemorie.be.

George had a – what we would now call – ‘flexispace’ (a shared office space) at *het Pand*. I am not sure whether this could be used as an argument in favour of keeping *het Pand* as a UGent conference centre, but it is an interesting little historical titbit nonetheless. Back in Sarton’s days, the very exciting *belle époque*, the old Dominican monastery was being ‘hijacked’ by artists, thinkers, and students. He visited regularly, and in that bohemian setting, he met British-born Mabel Elwes, who studied Art and Design at the Academy of Arts. They got engaged during a short boat trip down the river Leie while enjoying a spot of lunch consisting of asparagus and hardboiled eggs à la Flamande. The couple married in 1911 and settled down in Wondelgem. The following year, the pair welcomed their daughter May. She would later become a celebrated author in the US, where her parents emigrated to when she was just three years old.

Of particular interest for our story is that Sarton’s experiences and connections as a student served as a breeding ground for his new humanism. Here, we can note a connection to Julius Mac Leod, who is best known as a key actor in the process of the Dutchification of the University of Ghent, and of intellectual life in Flanders in general. The important task he took upon himself within the Flemish Movement was – and this might be less well known – closely linked to the social Darwinism he was so passionate about as a biologist and a precursor of plant genetics. Being a student of Charles Van Bambeke’s *‘Gentse Morfologische School’* (or Ghent Morphological School), he focused on Darwin’s theory of evolution, and thought about how this process of natural selection could be popularised. When the Nazis came along, social Darwinism was quickly sent to history’s doghouse. Before, however, this way of thinking about evolution, civilisation, and progress was widely accepted, in particular among left-wing intellectuals and scientists, including Mac Leod. The ‘enlightener of the Flemish people’ developed a somewhat quirky theory of social inheritance, which is in line with his belief in the malleability of mankind.

In 1887, Mac Leod became director of the Botanical Garden, which, at the time, was situated near the Baudelo abbey. When he was appointed to this position, he founded the *‘Kruidkundig Genootschap Dodonaea’* (Herbalist Society Dodonaea), a society that aimed to unite everyone with a passion for biological sciences.

The Botanical Garden is a location of considerable importance to the Ghent University Museum. It will serve as the entrance to the museum and will play an important part in its operations. The current Garden was created at the end of the 1890s when the City of Ghent and the University decided to relocate the new Botanical Garden to the other side of the brand-new Citadel Park. That way, they were able to establish a physical boundary between the city's park made for the inhabitants of Ghent on the one hand, and the botanical gardens dedicated to research and education on the other. Moreover, in 1900, the University's *Botanisch Instituut* (Botanical Institute) was constructed in the Ledeganckstraat. In the 1950s and 1960s, the Institute had to make way for what we now refer to as *'de Ledeganck'*, and where, at this very moment, the low-rise block is being made into … the Ghent University Museum. Of course, the Botanical Institute, a colourful Neo-Gothic building designed by Ghent City architect Louis Cloquet (well known for designing both Gent-Sint-Pieters railway station, and the *Rommelaere Instituut*) would have been a more appealing building to accommodate a museum. In 1903, Mac Leod taught his very first class there – for its time, the Botanical Institute was especially well equipped, with an auditorium, an orangery, a herbarium, a museum and modern laboratories. Unfortunately, the only detail left to remind us of the original complex, is the beautifully finished fence designed by Cloquet.

Julius Mac Leod was a truly inspiring man, both as a professor and as the driving force behind the Flemish Movement. As a proponent of the usage of Dutch at the University of Ghent, he contributed enormously to the 'development' of the Flemish people. Virginie Loveling once stated he had *"eene wonderkracht om de piepjonge jeugd te fanatiseeren"*, which can be translated as having "the unique power to inspire young people." His main research interests were 1) (a mathematical approach to) heredity, and 2) the role of scientific knowledge in the history of human civilisation, a passion he shared with Sarton. Mac Leod was an enthusiastic teacher, who did not limit his teaching to the confounds of the auditorium. It is very likely he also inspired Sarton, although there is no evidence of any direct influence.

Mac Leod was actively involved in the anticlerical student association *'t Zal Wel Gaan*. However, he was not very happy with the moderate position the association took on the Flemish cause. In 1904, he founded *Ter Waarheid*, an even more left wing, anti-bourgeois study group, which welcomed radical nonconformists including Hendrik De Man, Paul Kenis, Leo Picard, Paul Van Oye, Peter Hoffmann, as well as George Sarton.

In fact, the academic scene Julius Mac Leod, being the 'old' *maître à penser*, had created, was what stimulated Sarton to conceptualise the history of science as a new humanism. In 1904-1905, he founded *Reiner Leven* – or Living More Purely – together with his soul mate Irenée Van der Ghinst, who would later become a stomatologist. This nonconformist yet anything but libertine group of young men and women wanted to live a morally superior life by pledging total abstinence in line with the Lebensreform movement. They would gather at Café La Tempérance, located in the Bagattenstraat. Their slogan *'Pour être fort, sois pur*' – '*In order to be strong, be pure*' – summarised their mission, i.e. to raise students' moral standards. *Reiner Leven* wanted people to adopt new sexual mores, increased openness, sexual education, and more access to information on sexual health. The diverse scientific backgrounds of the *Reiner Leven* members provided the breeding ground for Sarton's New Humanism. Let me zoom in on the women in the group. They were Vera Tordeur, the first female student of Mathematics and Physics, and Bertha De Vriese, the first female doctor to graduate from the University of Ghent, who opted for an academic career and specialised in research into the brainstem's blood vessels. Her doctoral dissertation on the morphology of the cerebral artery became one of the most authoritative studies on the subject, yet Bertha did not manage to stay at the university. We should also mention another female member, Augusta de Taeye, who studied Physical Education. She would meet the man of her dreams through *Reiner Leven*, i.e. Leo-Michel Thiery – they would later become parents to Herman Thiery (better known under the pseudonym Johan Daisne) and twins Leo and Michel Thiery, who were twelve years younger.

Augusta De Taeye and Vera Tordeur also belonged to a group of young feminists who called themselves *De Flinken;* or The Sturdy Ones. The group included both working women and female students. They were pacifists and ate strictly vegetarian, idolised William Morris and John Ruskin's Arts and Crafts movement, read Leo Tolstoy, Maurice Maeterlinck and Emile Verhaeren, and attempted to build bridges between intellectuals on the one hand, and the working classes on the other. "We wanted to change the world, and improve it, until it would become a paradise where everyone would be equally happy” – This is what biologist Paul Van Oye wrote, when reflecting on his student days as a member of *Reiner Leven*. This might sound very endearing, but the same seemingly naïve idealism continued to inspire Sarton, even after both World Wars.

Fanny Maertens was another interesting young woman who joined George Sarton’s *Reiner Leven* society, together with her husband Julius Mac Leod. The worlds of nature and culture were intertwined, in the couple’s opinion. The theory of social evolution, which – as I mentioned before –, was flourishing in leftist circles, served as the link. Julius and Fanny were both mesmerised by anarchism. Julius was involved in *Van Nu en Straks*, and corresponded with August Vermeylen, Jacques Mesnil and Ferdinand Domela Nieuwenhuis. Fanny’s cousin was painter and graphic artist Frans Masereel. She studied Russian, and started to correspond with pen pals in Russia, including the anarcho-communist revolutionaries Sofja and Pjotr Kropotkin. In 1904, Fanny Mac Leod-Maertens translated Kropotkin’s social theory of evolution into Dutch. It was entitled *Wederkeerig dienstbetoon. Een factor der evolutie*, *(Mutual Aids, A Factor in Evolution)*, and included a preface written by her husband.

Next to their utopian ideals concerning social progress and world peace, the members of *Reiner Leven*, *de Flinken* as well as those belonging to Dodonaea also held strong ‘ecological’ beliefs *avant la lettre*. These ideas were linked to their fascination with natural sciences, and to a desire to return to nature, which was inherent to modern man at the start of the twentieth century. Professor Mac Leod would organise botanical excursions to the *Drongense meersen* (or the brooks near Drongen). Leo-Michel Thiery and Augusta De Taeye often took part in these excursions. The love they shared for botany led to their own private idyll. In the early days, the young lovers could barely make ends meet, and were forced to live in a small worker’s cottage near Ekkergem. Thiery refused to become the headmaster of the municipal school in the *Geitstraat*, where he worked as a teacher. He was working class, and was determined to remain part of it. As a sixth grade teacher, he had been a proponent of experience-focused project-based learning since before the First World War. He would always start his teaching from current events: the first snowfall of the season, a mining disaster, a nature walk. The windowsills in his classroom were covered in plants, freshwater creatures swam around in small aquariums, and he created a garden and a pond on the playground. Thiery would invite working class children along to the Botanical Garden, the Flemish Ardennes, the forest, the Royal Belgian Institute of Natural Sciences … He was well aware of the fact that those children would attend school until the age of fourteen at the very most. Thiery would become a key player in Ghent intellectual life, especially when it came to nature conservation and ecology. The breakdown of natural beauty because of industrialisation truly broke his romantic spirit. He was one of the first people to speak out publically against these 'crimes' against nature.

The congenial spirit cultivated by these researchers, literati, social scientists, artists, nature lovers, and activists allowed both Mac Leod's and Sarton's ideas about the history of science and humanism to mature. In fact, in 1913, that same spirit lead Sarton to found the – originally quadrilingual – magazine *Isis. An International Review devoted to the History of Science and its Cultural Influences*, in Wondelgem-lez-Gand. After the First World War, Sarton wanted to revive the energy that emanated from *Reiner Leven* and *de Flinken*, by making a plea for a stronger link between natural sciences and humanities.

Sarton never strayed from his mantra, "*History of science is the history of mankind*" – He lived by it, even before he left for the United States in 1915. In 1916, he began his professional career at Harvard. Julius Mac Leod left Belgium behind in August of 1914 – as soon as the war broke out – taking with him a manuscript on how science fits in its historical context. The document was translated during his exile in Manchester and was published in 1915 as *The Place of Science in History* (1915). It held the utopian message that history of science demonstrated "the spectacle of progress achieved by peaceful work" and should therefore be present at all levels of education, and by way of various forms of science popularisation. The originator of the *Hoger Onderwijs voor het Volk*, or the People's Higher Education, was convinced that the history of science would 'better' mankind, which is exactly what George Sarton made his life's work under the header 'new humanism'.

"The New Humanism is essentially the humanization of science or rather its re-integration with other elements of our culture." (George Sarton, *Isis*, 1923). The history of science fits into a broader humanist ideal, and an effort to unite science and culture.

Soon after his return from exile, Mac Leod fell ill with Spanish Flu. He died in Ghent on March 3rd, 1919. Last year, Ghent University decided to sponsor his gravestone at the *Westerbegraafplaats*, or Ghent Western Cemetery (his soul mate Bertha De Vriese's headstone is also being sponsored by our university).

After his death, Mac Leod's widow Fanny (Maertens, who translated Kropotkin's work) donated her husband's private collection of shells, minerals, and insects to her close friend Leo-Michel Thiery. This bit of information might be significant for the future of the Ghent University Museum.

In 1922, Victor Willem, who was a former student of Felix Plateau's, as well as his successor as the director of the Zoology Museum, had part of the university's *Cabinet Zoologique* transported to the *Beroepsschool van het Boek*, the former school for compositors at *het Berouw* (the old road connecting the *Kartuizerlaan* with *Vogelenzang*). At the insistence of Willem, Leo-Michel Thiery would later use this location for his School Museum. The scientific value of the university's zoological collection was rapidly declining, due to the rising popularity of the theory of evolution. The general systems we had been using to classify plants and animals, which had been preoccupying botanists and zoologists ever since the seventeenth century, had become outdated. Except for a limited number of lectures, the zoological collection remained under lock and key, and stopped being expanded. The collection, however, was able to stimulate schoolchildren's fascination with science. In 1924, the School Museum opened its doors to the public. The museum consisted of nine themed rooms, four of which were named after Ghent University professors: room Victor Willem (mammals, fish, and birds), room Julius Mac Leod (seashells), room Joseph and Felix Plateau (Physics and Chemistry), and room Henri Pirenne (History and Civilisation). In essence, the Ghent School Museum was an enormous classroom in which Thiery, being the sole teacher, taught classes (to children and adults alike). However, he did not lecture his pupils in the traditional classroom layout. The Garden, for one, was extremely important. Thiery had created a large garden, which surrounded his museum, located in the heart of a working-class neighbourhood. The 7,500 square metre garden was home to about one thousand different plants, an orchard, a vegetable patch, and a pond. Although the garden still exists in the same location, it is now called *de Tuin van Kina* (or The world of Kina: the Garden). In 1960, the School Museum was closed down, as it had become hopelessly outdated. Fortunately, it was given a new lease on life at the St. Peter's Abbey. As a child, I used to love spending time there.

In the meantime, back in the US, Sarton was contemplating the way in which his ideas and ideals concerning the history of science and a new humanism could be put into practice. He considered combining a non-academic institution dedicated to the history of science, where manu-scripts, documents, and various objects would be collected, with a museum. He would call it a 'clearing house' for the history of science. The importance of such an institution devoted to the history of science was addressed in a 1917 article featured in *Science*. "The history of science should be the leading thread in the history of civilization": the more specialised research became, the more pressing the need for a kind of synthesis, which would keep academics in the loop on each other's achievements. Increasing specialisation gave rise to certain blind spots and a limited intellectual scope, both of which had a negative effect on civilisa-tion. The only way to prevent complete disintegration was to uncover all evolutions, connections and interdependences across all scientific domains to the best of one's abilities. Sarton intended to create a gathering place for scientists with a keen interest in history, for historians with a passion for science, and for cultivated philosophers – a place where everything that connected various types of scientific research could be investigated and, consequently, passed on. This seemed the only way the gap between science and the humanities could be bridged in education as well. Sarton understood that the role science plays in education would only become more important. However, it could simply not happen without injecting 'a little of the humanistic spirit' in scientific and technological research. A closer collaboration between science and the humanities would serve a higher purpose, i.e. increased knowledge, more beauty, enhanced enjoy-ment, and greater justice. In this way, the Centre for New Humanism Sarton had in mind, promoted a new ideal, which was constantly evaluated against state of the art scientific knowledge.

In 1918, Sarton published an article on the 'teaching of the history of science', based on his own experiences as a teacher in America and

Canada. In the article, Sarton gave the readers a few practical pointers: 1) There should be a rich array of experiments in education, and it should be made concrete and be sufficiently contextualised in the history of human civilisation. Every new scientific development should provide a response to a societal need, or query. According to Sarton, the single most fascinating aspect of the history of science was the intimate and complex, 'organic' relationship of science with life itself, instead of with some fancy theory. 2) When teaching the 'historical facts', one should pay attention to the continuous interaction between science and art, science and religion, science and industry, science and justice, science and politics, et cetera. A science historian will uncover those interactions in order to write the cumulative, progressive, and international story of humanity's effort to achieve beauty, stable institutions, and various forms of cohabitation. Here, it becomes clear how Sarton gave prominence to the biography. There was no better way to spark students' interests than to recount in great detail the lives of the heroes of science, and thus raise awareness of the greatest human achievements in the fields of knowledge, beauty, and justice. Clearly, Sarton did not expect the science historian to list names and facts in a tedious, lifeless way. 3) When teaching scientific facts, one should do this in the most specific and concrete way possible. This implied that all necessary paraphernalia should be available, including maps, graphs, tables, models, devices and instruments. Teaching students about Vesalius' anatomical discoveries was simply impossible without the help of an anatomical model, or a drawing. Effective teaching should be straightforward, clear, convincing, and interesting. Ideally, the science historian would teach 'science in the making' based on experiments, but also relying on the historian's accuracy.

Evidently, the history of science could not be taught within the confines of an auditorium. Only the laboratories and the places where science was actually being 'done' would do. Or, alternatively, the university museums where every object and instrument was available, including the maps and models, the globes and hemispheres, the microscopes and telescopes, the alembics, the surgical instruments and those used in obstetrics, the lost collections, and possibly – yet not necessarily – a portrait gallery. Sarton was not just concerned with the practical, material value of science, but also with its beauty and majesty, its 'unexplored beauty' and 'fresh inspiration in the realm of science'. Consequently, the examples given by Sarton were – and still are, for that matter – quite significant, i.e. the *Conservatoire des Arts et Métiers* in Paris, the Science Museum in Kensington, London, and the Ashmolean in Oxford.

Considering the pace of scientific development throughout the twentieth century, growing specialisation could not be avoided. For that reason, there was a need for that broad humanistic basis the history of science offers, which could encourage scientists to move beyond their intellectual boundaries. Being a clever physicist would not suffice. Rather, one should strive to become generous and open-minded. Things should not become all too philosophical or erudite, or be taught be those who only had a superficial understanding of science.

World War I did not cause a break in Sarton's new humanism. In fact, the War only made his beliefs stronger. In *War and Civilisation*, which was published in 1919, Sarton wrote that the war had made him realise that science was not all good, and could be employed by groups who were intent on destruction. However, in that case, we should turn to the same tried and tested recipe: science mitigated by humanity, that mix of a historical and a scientific mind-set, which would lead to beauty, truth and justice. Without that crucial element of humanity, science would do more harm than good. The history of the twentieth century or 'the age of catastrophe' taught us that building bridges between the humanities and the technicalities of science is a matter of life and death. Nevertheless, Sarton would never turn cynical, and continued to insist on the importance of the history of science as a source of humanism and civilisation.

Albert – A.J.J. – Van de Velde was another firm believer in those same ideals. In 1948, after he had accepted emeritus status, this intellectual all-rounder and Sarton contemporary founded the Museum for the History of Sciences in Ghent. Van de Velde was also one of Mac Leod's former students; he did not just inherit Mac Leod's passion for botany, but also had the same fondness for the history of science. The museum was supposed to be named 'Museum Prof. Julius Mac Leod' and found a home in the premises of the *Oudheidkundig Museum van de Bijloke*, which is now known as STAM, or the Ghent City Museum. In 1965, the Museum for the History of Sciences was brought under the university's control and was moved to Korte Meer.

Furthermore, A.J.J. Van de Velde's story is a great example of Sarton's belief that the biographical method could bring the history of science 'to life': the emergence of biochemistry in Ghent is again related to Mac Leod who encouraged Van de Velde to engage in scientific research at the intersection of chemistry and biology. However, equally important to the personal aspect of the teacher spurring on a student to think outside the box of their own discipline, was the institutional element, i.e. *de brouwerij-school* – or 'brewery school' – where A.J.J. was a teacher, and would later become headmaster. There, the seed was sown for the biochemistry programme. Originally, this was categorised under 'vocational training' – clearly, biochemistry was not considered to be on a par with university education, as it was even dismissed as *Schmierchemie*, or 'messy chemistry', by chemists. As it happens, this links up beautifully with the story of Marc Van Montagu, Jef Schell and Walter Fiers and the origins of biotechnology.

In any case, it seemed the brewery school as an environment was more open to scientific innovation and interdisciplinary research than the university itself was at the time. Just how restrictive Ghent University policies were when it comes to large-scale interdisciplinary projects such as Sarton's is evidenced in the following story of complete failure.

The year is 1964. Then Rector Jean-Jacques Bouckaert had used *dies natalis* to proudly herald the founding of a Centre for Interdisciplinary Synthesis. His starting point was a few 'sons' holding a place in the pantheon of the university, who "although truly remarkable specialists in their fields, managed to retain a deep-seated and active interest in certain cultural domains that a priori appeared to be far removed from their own, and who were thus able to merge in their minds various aspects of knowledge and culture." Bouckaert referred to Henri Pirenne's ability to synthesise, Joseph Bidez and August Vermeylen's numerous areas of expertise, and ophthalmologist Daniël Van Duyse's literary predisposition. He directly opposed "the ability to synthesise and bring together the most diverse domains of human knowledge in your mind" to the far-reaching subject specialisation, which in the 'era of technology' prevented any synthesis. The university was at risk of increasingly educating people, who – although certainly technically qualified – were lacking a synthetic take on things, whereas Bouckaert's own experience with 'empirical' science taught him that scientific progress could no longer be made without interdisciplinary collaboration. In many cases, the boundaries between what we would call 'classic' branches of science, and those between the different faculties, were long outdated. One had to wonder in which ways the university could encourage this interdisciplinary collaboration. Rector Bouckaert was inspired by C.P. Snow's essay, *The Two Cultures and the Scientific Revolution* (1959). It remains surprising how tragically topical this British molecular physicist and novelist's accurate description some sixty years ago of the gap between the humanities and the natural sciences, alpha and beta, between fundamental and applied sciences, between the arts and science, was and still is. He described how the gap had become so wide that we could actually conclude we are dealing with different cultures and different languages, and 'scholars' and 'scientists' who no longer could – or even wanted to – understand each other. Bouckaert agreed that the way forward would be a new kind of education, which would transcend the inevitable tendency to specialise.

In the US, Bouckaert was able to determine that the MIT engineers were not simply offered a superficial introduction to the humanities, but that they were asked to really immerse themselves in literary sciences and culture studies. The disconnect between humanist and scientific education had a negative effect. Clearly, any university that aimed to be more than a vocational school would simply have to provide the necessary structures in order to encourage synthetic thinking. Specifically, Bouckaert had The Centre for Interdisciplinary Synthesis in mind, which was supposed to look for – and strive for – connections in an 'omnidirectional' (multiperspectivist!) fashion. That way, a synthesis of the highest level, i.e. the level of science and philosophy, could be achieved. The rector was particularly pleased that the members of the Philosophy Department at Ghent University "eagerly agreed with this perspective". Indeed, the Centre for Interdisciplinary Synthesis was a dream come true for Leo Apostel, that other 'son' in the university's pantheon, who was able to move beyond narrow-mindedness in his commitment to great projects.

A task force was established, which consisted of interested colleagues from various disciplines and 'schools of thought'. Support came from the industrial world, where it was noticed that industry would not just experience the need for a technically skilled staff, but also for people with a sound general education. The centre as it was envisaged would comprise three levels: a section dedicated to Natural Sciences; a section devoted to Culture Synthesis, and a superstructure concentrating on Theology, where education would be conceived in a pluralistic manner. “Through mutual contact between believers of various religious traditions and non-believers” this would foster “a spirit of understanding, tolerance and togetherness vis-à-vis those holding a different set of beliefs.”

“Would”, for if this visionary plan had not “been discussed to death in too many committees” in May 1965, and finally completely dismissed by the Faculties of Medicine and Engineering, the *Rijksuniversiteit Gent* would definitely have been a pioneer in Europe in the field of interdisciplinarity. Many years later, Apostel would describe it as follows: the interdisciplinary project in Ghent, the big May 68 dream, had already failed three years before due to the faculties’ ‘reality’ principle. Similar to Sarton, Mac Leod and Van de Velde, Apostel saw science popularisation, in the proper sense of the word, as a necessary condition for connection and synthesis. In the context of the 1984 Sarton Centennial, his student Marc De Mey compared the dreams and ambitions of his teacher to those of Sarton, and found some striking parallels. The Centennial in 1984 marked the start of the annual, inter-faculty George Sarton Chair for the History of Science, and the Sarton medals for the history of science. A significant detail in my personal history is that emeritus Michel Thiery, son of Thiery senior, and the first president of the Sarton committee, helped bring me into the world in 1964.

Thus, we have come full circle, and I can now finish my story about George Sarton.

However, I cannot really conclude this story without mentioning the interdisciplinary Sarton Centre for the History of Science, which was founded back in 2003 by enthusiastic Romanist Fernand Hallyn, and is currently headed by philosopher and physicist Maarten Van Dyck. The continuation of Sarton’s ubiquity at Ghent University answers the urgent need for a new project connecting the humanities to science and future society with an enduring source of inspiration. We live in a world in transition; we are confronted with a series of problems that can only be dealt with if the gap between the humanities and the technicalities of science is closed once again. Compared to Sarton’s time at Harvard, that challenge has only become bigger, in the sense that arts & philosophy, arts & humanities en *'letteren & wijsbegeerte'* themselves have become fragmented by an often-bewildering urge to specialise and the continuous creation of niches. Along with the era of the Great Narratives, the desire to synthesise has been lost in the mists of time. Consequently, science's long-term efforts disappeared, which is precisely what clings to you and leaves a lasting impression. Today, the urgent need for interdisciplinary research requires that contribution from humanities in education, which simultaneously raises the question of a general education or *studium generale* in all scientific domains, across the borders of various disciplines and faculties.

At the Faculty of Arts and Philosophy we have turned the future of the humanities into an important 'testing ground', which we intend to focus on for the next couple of years, by means of a think-tank across departments and disciplines. Moreover, we are contemplating a university-wide chair, and obviously, not in the least, a close collaboration with the Ghent University Museum.

For that story has not yet come to a conclusion – quite the contrary: it has only just begun.

We are all familiar with its history. A.J.J. Van de Velde's Museum for the History of Science's overarching ideal had transformed into a fragmented story of multiple collections – medicine, science, morphology, ethnography, archaeology, anatomy, zoology, the Botanical Garden –, each characterised by their own specific nature. Put together, the Ghent University museums hold some 640,000 objects, jointly representing the largest collection of academic heritage in the Benelux. In 2013, the inter-faculty partnership Ghent University Museums (GUM) was founded. This partnership works towards a brand-new, contemporary museum located in the Ledeganckstraat. The museum is scheduled to open its doors in Spring 2020. And yes, this could truly become a wonderful story at the interface between science and culture. In the Ghent University Museum, the university literally gets down from its ivory tower in order to demonstrate to society what it has accomplished in the past. Not because of the past itself, but in view of our future. A sense of wonderment that makes you want to engage in research and start a dialogue with society; those are the museum's guiding principles, rather than the diversity of its collections. The all-important question "Can science save the world?" fits seamlessly with Sarton and Van de Velde's humanist take on science. The museum will be perfectly situated in the heart of the Arts Quarter, right across from the Museum of Fine Arts and the Museum of Contemporary Art (S.M.A.K). The Botanical Garden acts as a 'natural' entrance to the wondrous world of culture and science, which will be made accessible in the Ghent University Museum.

"*Making the most of our university and encouraging our university to make the most of us*" is part of the mission statement of the Oxford Ashmolean, the mother of all university museums. According to logistics officer Jeroen Van den Berghe, one of the driving forces behind the Ghent University Museum, we are not aiming too high when we also set this as our goal. It is our intention to make this into a participatory and dynamic museum, and to actively involve the academic community. In the vein of Sarton, Van den Berghe sees the museum as a powerhouse of teaching and research, which puts knowledge in a broader societal context, and which facilitates the exchange of expertise. In actual fact, this museum will be the ideal location for people to engage in exactly the kind of history of science George Sarton has been advocating throughout his life.

Laudatio Pauli Kettunen

Raf Vanderstraeten

Let me start this laudation with a contrast. In general terms, the social sciences can look back at a relatively long history. In handbooks, it is common to trace the origins of the social sciences back to the Enlightenment era. For example, Jean-Jacques Rousseau's *On the Social Contract* (or in French: *Du contrat social; ou Principes du droit politique*), which first appeared in 1762, often figures prominently in handbooks on classical social theory. In this book, Rousseau theorized about the best way to establish a political community in the face of the problems and inequalities, characteristic of the 'commercial society' of his time. Alternatively, the origins of the social sciences can also be linked with the origins of early 'welfare' state regimes. In the era around 1800, problems about public health, such as mortality and morbidity rates in different segments of the population, or problems about pauperism, poverty, and indigence became an object of broader concern. At that time, individual scholars and state authorities also started to gather information about these problems by means of a variety of both systematic and unsystematic methods, including population censuses, household surveys, and vital statistics. Anyway, the origins and early history of the social sciences are closely connected with the ambitions of social reformers.

Despite these early origins, however, it took quite some time before the social sciences could establish themselves, next to the natural sciences and the humanities, as academic disciplines. For sociology, it took until the period around 1900. As we all know, this period is often identified with scholars such as Emile Durkheim, Max Weber, Ferdinand Tönnies, Edward Westermarck, and others. To gain credit as an academic discipline, the so-called founding fathers clearly tried to stay away from the ideological conflicts, which played such a pervasive role in the period around 1900. They put much emphasis on the objectivity, impartiality or value-neutrality of their analyses. Much work of the founding fathers was published before the First World War, but the emphasis on 'value-neutral' analyses also remained strong during and after the Great War. Even the applied or policy-directed kinds of research that have been undertaken by academics during the twentieth or early twenty-first century have remained far from the interventionist ambitions of the 'enlightened' social reformers. Much work on the history of the social sciences also chooses to focus on these founding fathers and their claims and ambitions.

But let me now add a further observation. In the period after the Second World War, and as no other countries in the world, the Nordic or Scandinavian countries have been able to develop extensive welfare regimes. It is often said that the Nordic welfare model distinguishes itself from other types of welfare states by its emphasis on high labor force participation, gender equality, extensive social benefits, and fiscal policies focused upon income redistribution. It might also be said that the Nordic welfare model offers a clear example of how the social sciences have been able to change the principles or basic structures of modern welfare states. Despite all the problems with which they have to face, the Scandinavian welfare regimes are often admired by social scientists and social reformers for what they are able to accomplish. They constitute the place where one can see the social sciences at work, so to say. To a scale not elsewhere seen or dreamed of, they make use of the social sciences to identify, address and alleviate social problems.

Today we celebrate the work of Professor Pauli Kettunen from the University of Helsinki in Finland. He is not only a first-hand beneficiary of the Nordic welfare model. Pauli Kettunen has also pioneered social and historical research about the ways in which the social sciences have helped to constitute the welfare state regimes in the Nordic countries. Throughout a very productive career, which spans a period of four decades, he has covered much ground. His work has become highly visible. He was, for example, the principal investigator of the Nordic Center of Excellence *NordWel* (i.e. The Nordic Welfare State: Historical Foundations and Future Challenges). Within the Sino-Nordic Welfare Research Network (SNoW), he has in recent years also started to conduct comparative research about social welfare regimes, especially by comparing regimes in Europe and Asia. Altogether, Pauli Kettunen has been able to open up a variety of new perspectives to write the history and sociology of the social sciences. His work has also stimulated research that moves beyond the founding fathers of academic sociology and looks at the broader history and sociology of the social sciences.

When George Sarton emigrated to the United States after the German invasion of Belgium during the Great War, he took the journal *Isis*, which he had founded and edited in Wondelgem-lez-Gand, with him. The second volume of his journal was published in the US in 1919. Beset by the devastations of the First World War, Sarton also used at that time a new subtitle: Isis was *An International Review Devoted to the History of Science and Civilization.* For Sarton, studies on the history of civilization could serve to shed light on the social benefits of the diffusion of scientific principles and scientific findings. When we celebrate today the work of Professor Pauli Kettunen, we also underline the value of this broader, civilizational ambition of George Sarton for us and for the social sciences today.

Conflicting interests and science-based planning in the making of the welfare state

Pauli Kettunen

Introduction

One of the reviews of George Sarton's *The History of Science and the New Humanism* was published in *The Annals of the American Academy of Political and Social Sciences* in 1938. The reviewer was C. W. Churchman, a young philosopher who later became known as a developer of systems science. He was impressed by how Sarton turned the history of science into a new humanism with the potential to solve the problems of today.[1]

The journal issue too focused on contemporary problems and how to solve them. It was a special issue on 'Social Problems and Policies in Sweden'. The issue was motivated by the 300th anniversary of the New Sweden colony in Delaware, yet it was primarily an endeavour to brand the current new progressive Sweden and, apparently, to support Franklin D. Roosevelt's New Deal politics in the United States. The discourse on the history of science, which Sarton with his notion of a new humanism elaborated upon, had few direct links with those social policy efforts of the 1930s aimed at redefining the relationships between the state, society and the economy, often by means of the concept of planning. Yet there was a shared framework: an interest and confidence in the role of science-based knowledge in conceiving and solving social problems.

[1] C. W. Churchman (1938) Review of George Sarton: The History of Science and the New Humanism. Cambridge, Mass.: Harvard University Press, 1937, *The Annals of the American Academy of Political and Social Sciences*, Vol 197, Issue 1, 264-265.

Let me take a closer look at the special issue of *The Annals of the American Academy of Political and Social Sciences* on Sweden as a means of specifying the topic of my presentation. The issue consisted of 19 articles on all fields of Swedish social life. The authors included two leading Social Democratic cabinet members, Ernst Wigforss and Gustav Möller, civil servants, and prominent scholars, most notably two representatives of the so-called Stockholm School, Bertil Ohlin and Gunnar Myrdal, who had developed the theoretical foundations for anti-crisis counter-cyclical economic and employment policies, drawing from the lessons of the Great Depression at the same time as John Maynard Keynes.

As the editor of the issue, the Swedish-American sociologist Thorsten Sellin wrote, Sweden had evoked international attention, not only due to its rise from a poverty-stricken nation to material prosperity, but even more due to 'the intelligent manner in which Sweden is earnestly and successfully striving to achieve social justice and to make democracy work in an age when the democratic ideal is being boldly challenged by totalitarian ideologies'.[2] Indeed, earlier in the 1930s, this message had been mediated in, for example, reports by the International Labour Organisation (ILO), which praised the Swedish policy-planning together with the New Deal as the best examples of overcoming and preventing economic crises.[3]

The Swedish economic and social policies and the US New Deal were certainly not the only national varieties of what the ILO reports and other contemporary accounts portrayed by means of the concepts of planning or planned economy. The Labour Plan drawn up by Henri de Man in Belgium in 1933 became a widely cited manifestation of 'planism'.[4] A 'planned economy' was a concept linked with the fascist modes of intertwining the state and society in Italy and Germany as well. Most notably, at the same time as the Great Depression had begun to affect the capitalist world

---

2 Thorsten Sellin (1938) 'Foreword', *The Annals of the American Academy of Political and Social Sciences*, Vol 197, Issue 1, xi.

3 Pauli Kettunen (2013) 'The ILO as a Forum for Developing and Demonstrating a Nordic Model', in Sandrine Kott & Joëlle Droux (eds.) *Globalizing Social Rights: The International Labour Organization and Beyond*. Basingstoke: Palgrave Macmillan, 218-221.

4 Tommaso Milani (2017) *«Les Belles Années du Plan»? Hendrik de Man and the Reinvention of Western European Socialism, 1914-36 ca.* A thesis submitted to the Department of International History of the London School of Economics for the degree of Doctor of Philosophy, London, September 2017.

economy, Joseph Stalin had launched the first Five-Year Plan in the Soviet Union.

The debates of the 1930s reflected contemporary experiences, including the Great Depression, confrontations between societal systems, and struggles concerning the fate and contents of democracy. However, they also implied new ways of interlinking and confronting older definitions of social problems and solutions, developed in the 19th century along with the notion of a modernising nation-state society. As a result of the debates and policies of the 1930s, this notion altered in a way that created the preconditions for the transformation we retrospectively often characterise as the making of the welfare state.

In what follows, I will first briefly discuss the emergence of the notion of a modernising nation-state society as a framework for defining social problems and as a target of knowledge and politics. Then, I identify divergent arguments developed as part of the nineteenth century discourse on the so-called social question, arguments concerning the relationships between the social and the economic, and the national and the international. I further move on to two post-World War I institutionalised modes of dealing with these relationships, the ILO, founded in 1919, and the formation of a pattern of social change and reform currently often called the Nordic model. The final part of the paper focuses on one of the authors of the special issue of *The Annals of the American Academy of Political and Social Sciences* on Sweden in 1938, Gunnar Myrdal, the theoretician of a notion of self-reinforcing social progress based on a combination of divergent interests and science-based planning. In conclusion, I briefly discuss the questioning of this notion of virtuous circles in the time of globalised capitalism.

Modernising nation-state society

In his work *Modernity and Ambivalence* (1991), the Polish-British sociologist Zygmunt Bauman argued that the confidence in knowledge was at the centre of modernity:

> Modernity could dismiss its own uncertainty as a temporary affliction. Each uncertainty came complete with the recipe for curing it: just one more problem, and problems were defined by their solutions. (Societies, Marx

> insisted, never put before themselves tasks until means for their execution are available.) The passage from uncertainty to certainty, from ambivalence to transparency seemed to be a matter of time, of resolve, of resources, of *knowledge*.[5]

The use of the imperfect tense in this quotation implies that we can no longer trust in the ability of knowledge to lead us from uncertainty to certainty, from ambivalence to transparency. For Bauman, postmodernity means an ability to live with ambivalence.

Moving from chaos to order was, in Bauman's view, a main characteristic of modernity, but one may argue that the tension between chaos and order made modernity Janus-faced in nature. The British sociologist Mike Featherstone distinguishes between two images of modernity. 'The first image of modernity is one of order and entails the progressive control, domination and regulation of the natural and social worlds through the application of rational knowledge.' However, there is also 'the second image … of modernity producing endless disruption and social disorganization as it pacifies and controls nature for human purposes and tears down the old structures of social life to make way for the new'.[6] We find the most famous reflection of this image of modernity in a pamphlet of two young radical intellectuals published in 1848:

> Constant revolutionizing of production, uninterrupted disturbance of all social relations, everlasting uncertainty and agitation, distinguish the bourgeois epoch from all earlier times. All fixed, fast-frozen relationships, with their train of venerable ideas and views, are swept away, all new ones become obsolete before they can ossify. All that is solid melts into air, all that is holy is profaned, and man is at last compelled to face with sober senses, his real conditions of life, and his relations with his kind.[7]

The distinction between the two faces of modernity can be elaborated upon further. The inherent conditions of modernity included two aspects of the traditional as well: an image of traditional irrationality and an image of

---

5 Zygmunt Bauman (1991) *Modernity and Ambivalence*. Cambridge: Polity Press, 237, italics in original.

6 Mike Featherstone (1995) *Undoing Culture. Globalization, Postmodernism and Identity.* London: Sage Publications, 147-148.

7 Karl Marx & Friedrich Engels (2017 [1848]) *The Communist Manifesto*. New York: International Publishers, 7.

traditional community. In processes of modernisation, one can recognise an influential ideational orientation that combines the will to rationalise the world of traditional irrationality with a concern for the destruction of the traditional community. Rationalisation was linked with the inventing and modifying of traditions. The most important type of 'invented traditions' (Eric Hobsbawm) is nationalism, the ideology of the making of the nation, an 'imagined community' (Benedict Anderson).[8]

In the nineteenth century, the intertwined temporal and spatial ideas of historical progress and transnational interdependence became crucial ingredients for the construction of a modernising nation-state society. Modernisation and the construction of the nation-state – often associated with colonial and imperial ambitions – broadly overlapped in the horizons of expectation in nineteenth century Europe. Within the framework of modernising nation-state society, problems were defined from two intersecting perspectives: as issues of rationalisation or as problems of maintaining, creating and restoring social cohesion. At the intersection of these two perspectives, the questions that Michel Foucault examined as issues in the making of the subject[9] appeared. In connection with these questions, norms and criteria associated with age, gender and social class were constructed by means of statistics, the 'science of the state', in order to define and assess the positions and capacities of people as actors in the market, in production, in the family and in the national community. [Figure 1]

For example, in the late nineteenth century debates on labour protection, the objectives of technological and organisational rationalisation, the promotion of industrial peace and harmony and the education of self-disciplined workers became intertwined in a way that proved to be very tight and permanent.[10]

---

8 Eric Hobsbawm (1983) 'Mass Producing Traditions: Europe, 1870-1914', in Eric Hobsbawm & Terence Ranger (eds) *The Invention of Traditions*. Cambridge: Cambridge University Press; Benedict Anderson (1983) *Imagined Communities: Reflections on the Origin and. Spread of Nationalism*. London: Verso.

9 Michel Foucault (1983) 'The Subject and Power', in Hubert L. Dreyfus & Paul Rabinow, *Michel Foucault – Beyond Structuralism and Hermeneutics.* Second Edition. With an Afterword and an Interview with Michel Foucault. Chicago: The University of Chicago Press, 212-215.

10 Pauli Kettunen (1994) *Suojelu, suoritus, subjekti. Työsuojelu teollistuvan Suomen yhteiskunnallisissa ajattelu-ja toimintatavoissa*. Helsinki: Societas Historica Finlandiae.

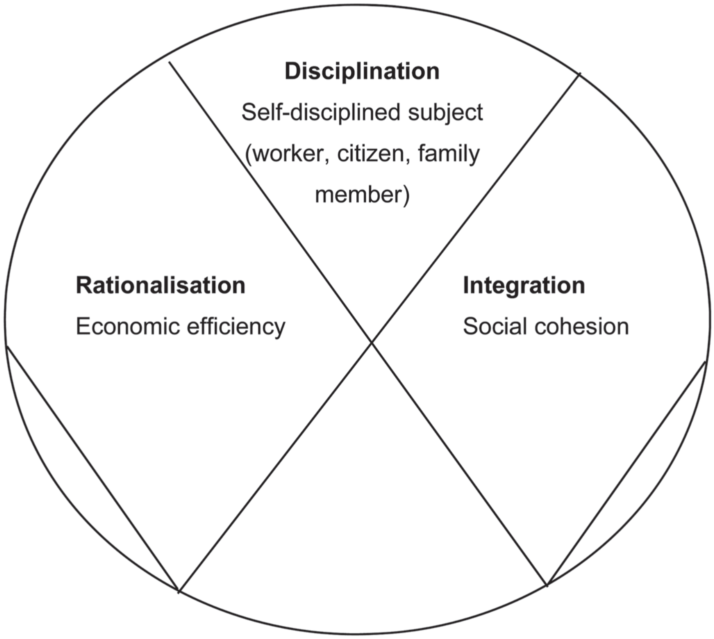


Figure 1. The notion of modernising nation-state society as a framework of defining problems and solutions

There are different historical portrayals of the role of knowledge in the processes of rationalisation, integration and the making of the subject. One is a story of professionalisation, often told with references to British and American professionals who defined social reality in terms of various problems to be solved only through their expertise and who tried to increase the value of their expertise by making it a scarce resource on the market. Another story portrays the German *Bildungsbürger*, who identified their interests with nation-building and state-making, and with the process of bureaucratisation.[11] However, by getting beyond the national stories, one can see professional expertise and *Bildung* as two different aspects of the role of knowledge in the notions of a modernising national society that were related to each other in different ways in different countries.

11 Contributions to these histories are referred to in Pauli Kettunen and Ilkka Turunen (1994) 'The Middle Class, Knowledge and the Idea of the Third Factor', *Scandinavian Journal of History,* Vol 19, Issue 1, 65.

In the constitution of science-based professional expertise, the exclusive practices of the educated as well as their activities as educators were oriented against what was seen as traditional irrationality. The old customs expressed an ignorance that had to be cleaned up by means of science and education. There were, however, also other kinds of relations between modernity and tradition. They were realised in education as *Bildung*. Modernity presupposed traditions not only as targets of enlightened expertise and education, but also as powers for the main agent of modernisation, the nation,[12] and *Bildung* represented the knowledge of continuity and integration.[13]

Both aspects of knowledge and education – professional expertise and *Bildung* – clearly appeared in the countries of Europe's northern periphery, for example, in the former eastern provinces of Sweden that had in 1809 formed the Grand Duchy of Finland in the Russian Empire. In the late 19th and early 20th centuries, representatives from all groups with professional knowledge or aspirations to professional status travelled abroad, with Germany being the most popular destination.[14] The *national* mission to which these professional groups devoted themselves was to acquire *transnational* knowledge (technical, medical, hygienic, socio-political etc.) through their *international* contacts, so as to be in a position to judge, on the basis of a comparative perspective, the opportunities for applying this knowledge in a domestic context. The destroying of traditional experience-based knowledge by means of professional expertise was closely associated with the inventing and making use of traditions by means of *Bildung*. These were the two sides of the same nation-building and state-making process.

Comparative reflexivity became a crucial part of defining social problems and solutions. The national elites, and later also the leaders of popular movements, including the labour movement, adopted a mode of thought

12 Drawing a distinction between modernity and modernisation, Alain Touraine points out that the nation is not "the political figure of modernity", but it is "the main actor of modernization". Alain Touraine (1995) *Critique of Modernity.* Translated by David Macey. Oxford: Blackwell, 137.

13 On *Bildung* as an "*Integrationsideologie*", see Hans-Ulrich Wehler (1987) 'Wie bürgerlich war das Deutsche Kaiserreich?', in Jürgen Kocka (ed.) *Bürger und Bürgerlichkeit im 19. Jahrhundert.* Göttingen: Vandenhoeck & Ruprecht, 268.

14 Marjatta Hietala (1992) *Innovaatioiden ja kansainvälistymisen vuosikymmenet. Tietoa, taitoa, asiantuntemusta. Helsinki eurooppalaisessa kehityksessä 1875-1917 I.* Historiallinen Arkisto 99:1. Helsinki: Suomen Historiallinen Seura, 209-244, 249-261.

and action that can be described as peripheral avant-gardism. Problems should be anticipated and solutions should be planned by acquiring information on the experiences, solutions and mistakes in what were conceived as the centres of industrial modernisation. The distinction between centre and periphery included a self-definition of the nation in spatial and temporal terms. International comparisons, which were oriented toward the horizon of expectation associated with modernisation, became during the 19th century an important factor in the construction of national politics, national economies, national societies and their collective actors – a dynamics of making use of 'the advantages of backwardness', to use Alexander Gerschenkron's phrase.[15]

Social-economic and national-international

The trans- and international dimensions of the notion of modernising nation-state society included more than the idea that nations were at different stages of development. The awareness of transnational interdependencies, notably those created by the international economy, was crucial in the defining of the so-called social question and its solutions. A widely shared view was that class conflicts threatening national society were caused or fuelled by the international economy. Political conclusions on their solution diverged, however.

The tensions between national society and international economic competition – or the relationships between two dichotomies, national-international and social-economic – had been discussed since the late eighteenth century.[16] In the late nineteenth-century international discussion on the social question, one can distinguish between six arguments that were contrasted or intertwined with one another.[17]

---

15 Alexander Gerschenkron (1962) *Economic Backwardness in Historical Perspective. A Book of Essays*. Cambridge, Mass.: The Belknap Press of Harvard University Press, 356-363.

16 See, e.g. Koen Stapelbroek & Jani Marjanen eds. (2012) *The Rise of Economic Societies in the Eighteenth Century: Patriotic Reform in Europe and North America*. Basingstoke: Palgrave Macmillan.

17 I have elaborated this distinction in Pauli Kettunen (2006) 'Power of International Comparison – A Perspective on the Making and Challenging of the Nordic Welfare State', in Niels Finn Christiansen, Klaus Petersen, Nils Edling & Per Haave (eds.) *The Nordic Model of Welfare – a Historical Reappraisal*. Copenhagen: Museum Tusculanum Press, 31-65.

According to the first argument, social political reforms were necessary in order to diminish the social and political threats of class conflicts caused by the international economy. The second argument was that international economic competition presented obstacles to national social policies, as such policies would weaken a nation's competitiveness. Alternatively, however, international economic competition could be seen – so the third argument went – as the point of departure for international social norms that would be binding for all competing countries and firms. A fourth argument that also appeared at an early stage in the discussions claimed that national social policies would actually support the success of the national economy by improving the quality of labour power and productivity and by increasing purchasing power.

In addition, influential arguments were developed according to which the logic of the capitalist economy was so powerful that it itself would lead humankind and nations either to happiness or to destruction, and it would itself produce the solutions to the problems it had caused or else generate the powers that would overthrow it. The former variant was manifested in efforts to show that demands for economic efficiency and social harmony could both be realised through measures taken at the level of individual enterprise, for example by the paternalistic provision of social welfare by employers for their workers or through scientific management techniques. The latter variant, in turn, was the main message of the revolutionary socialist critique of capitalism.

After the First World War, all of these arguments played a role in the struggles between different visions of societal change. The visions were internationalised in a new way through war-time experiences and the political upheavals and radical mass mobilisation at the end phase of the war and after its resolution. Conflicting collective interests and science-based knowledge were in divergent ways combined in ideas about the political agency needed for defining and solving social problems.

International social policies and national models of society

The International Labour Organisation (ILO) was founded after the First World War as part of the Treaty of Versailles to function as an autonomous part of the League of Nations. While the ILO had its background in the long project aiming to establish international social norms, it has also been characterised, and with good reasons, as 'the answer of Versailles to Bolshevism'.[18] With the foundation of the ILO, the vision of world revolution was confronted by the vision of an international social policy. Divergent variants of the latter vision appeared, ranging from reformist socialism to social conservatism. The ILO also embodied conclusions drawn from war-time national experiences of economic regulation, in which varying corporatist forms of participation by employers' and workers' organisations were introduced.

Since its foundation, the ILO has been a very particular type of intergovernmental organisation due to its tripartite structure of representation. Besides the government delegates, workers and employers are represented in national delegations and in the whole organisation. Governments were supposed to nominate the worker and employer delegates from the candidates proposed by the most representative organisations of these two groups. In its very structure the ILO came to reflect the notion of a modern society in which organised capital and organised labour together with the government generate social regulations, resolving the tensions between the international economy and national society.

In general, tripartite representation has been far from an unproblematic principle. For reformist labour leaders and social liberal reformers who were active in the founding of the ILO, tripartism as well as collective agreements on labour market were an extension of political democracy. According to the radical labour movement, in turn, the organisational structure of the ILO aimed to integrate the working class into the bourgeois state. For many employers, the tripartite principle represented a dangerous recognition of trade unions. On the other hand, a corporatist representation of economic interests in national political processes could also be seen as a means to bar the threats inherent in political democracy that had made its breakthrough after the First World War. It was possible to interpret tripartism as a representation of the different functions of society rather than as a representation of conflicting interest – an argument that was later used to facilitate the ILO membership of the Soviet Union and Eastern Bloc coun-

---

18 Abdul-Karim Tikriti (1982*) Tripartism and the International Labour Organization: A Study of the Legal Concept – Its Origins, Function and Evolution in the Law of Nations.* Stockholm: Almqvist & Wiksell, 125.

tries.[19] However, disputes concerning the legitimacy of the mandates of the worker or employer representatives in national delegations were a frequent phenomenon in the ILO conferences.

An incentive for international social regulation was included in that section of the Treaty of Versailles that contained the ILO's charter. According to the argumentation that can be read from the lofty text, political stability within countries was a precondition for international political stability. In its turn, political stability within countries, i.e. that the working masses remained pacified, depended on placing social limitations on the free play of the capitalist economy. These social limitations had to be enshrined at the international level, by means of international conventions, because international economic competition prevailed in the world.

However, insofar as the international standardisation of social norms was achieved, it occurred through national solutions rather than through subordination to international regulation. During the Great Depression in the early 1930s the ideas of an international economic co-operation and associated social norms proved to be powerless in relation to protectionism. It is reasonable to say that the ILO has exercised a larger influence on changes in labour law and wider social policy by producing and transmitting knowledge than it has through international law. The ILO has advocated arguments that can be deployed in the national political struggle, and it has produced comparative knowledge, not least statistical classifications and categorisations, which can be used in national policies.

The ILO became an international centre for a discourse that connected the themes of economic rationalisation, social integration and the rights of workers within a society based on wage labour. During the 1930s, the leadership of the ILO played a part in disseminating propaganda in favour of 'economic planning' and Keynesian ideas concerning the desirability of pursuing a contra-cyclical economic policy. In 1944, the Declaration of Philadelphia, which belonged to the same context of international post-war planning as the Bretton Woods system, demanded an ambitious role for the ILO in regulating the international economy. Its task would be 'to examine and consider all international economic and financial policies and measures' in the light of the fundamental objective that 'all human beings, irre-

19 Kettunen 2013, 221-226.

spective of race, creed or sex, have the right to pursue both their material well-being and their spiritual development in conditions of freedom and dignity, of economic security and equal opportunity'.[20] However, the ILO did not achieve any role within the Bretton Woods system. After all, the essential core of the Declaration of Philadelphia also consisted of guidelines for social and economic policy at the national level – full employment, the interdependence of social equality and economic growth, the principle of collective agreements, and the participation of both employers and workers in the formulation and implementation of social and economic policy.

Divergent interests and virtuous circles

In the 1930s, the Scandinavian novelties of anti-crisis policies were praised in the ILO reports. Later, the post-war development in Scandinavia, especially in Sweden, was perceived not only by some Nordic citizens but also by many others outside the Nordic region as uniquely consistent steps along a universally applicable road to progress, notably the one described in the ILO's Philadelphia Declaration. No doubt, more than one candidate for a universally applicable road existed in the Cold War world. The notion of 'the third way' or 'the middle way', as it was associated with Sweden and sometimes with the whole of *Norden*, that is, Denmark, Finland, Iceland, Norway and Sweden, included a particular claim to universality.

The special issue of *The Annals of the American Academy of Political and Social Sciences* on Sweden in 1938 was a manifestation of the confidence in virtuous circles to be achieved by a combination of compromises between divergent interests and rational planning. Reflecting both existing class structures and conclusions drawn from the economic crisis and rise of fascism in Europe, the Scandinavian class compromises of the 1930s included political coalitions of 'workers and farmers', or social democrats and agrarian parties, and the consolidation of national systems of collective labour market negotiations and agreements. These class compromises recognised the existence of divergent interests and institutionalised a confi-

---

[20] *Declaration concerning the aims and purposes of the International Labour Organization (Declaration of Philadelphia).* https://www.ilo.org/dyn/normlex/en/f?p=1000:62:0::NO:62:P62_LIST_ENTRIE_ID:2453907:NO#declaration.

dence in positive-sum games connecting the interests of worker-consumers and farmer-producers as well as of workers and industrial employers.

However, the virtuous circle included something more than just positive-sum compromises between different economic interests. It was also a virtuous circle between equality, efficiency and solidarity, which, in a sense, can be seen as being based on three different ideological strains of Nordic modernisation processes: the idealised heritage of the free peasant, the spirit of capitalism and the utopia of socialism. In terms of political objectives, and of future expectations, the virtuous circle came to be connected with increased social equality, economic growth and expanding democracy. Different ways of interpreting these objectives and expectations appeared, yet in the post-World War II period they came to play a hegemonic role in the sense that political conflicts tended to be struggles on the right way to represent and promote these objectives and expectations, and to conceive their interconnectedness.

In the special issue on Swedish social problems and policies in 1938, written for an American academic audience, one of the authors especially focused on the relationship between rational social policy and economic growth in a democratic society. He was the social democratic economist Gunnar Myrdal. Let me in the final part on my presentation discuss Myrdal as the theoretician of a society of virtuous circles.

Immanent critique and circular cumulative causation

In 1934, Gunnar Myrdal, together with his wife Alva, published a book called *Crisis in Population Question*. The Myrdals argued for preventive, 'prophylactic social policies' aiming to increase the birth rate and improve 'the quality of human material' in Sweden. The book is often referred to as an evidence of how nationalism and the productivistic ethos of rationalisation became united in the making of the Swedish and, more broadly, Nordic welfare state at the same time as social democracy became a focal point of national integration.[21]

[21] Alva Myrdal & Gunnar Myrdal (1934) *Kris i befolkningsfrågan*. Stockholm: Bonniers; Maribel Morey (2015) 'The Swedish roots to Gunnar Myrdal's An American Dilemma (1944)', in Pauli Kettunen, Sonya Michel & Klaus Petersen (eds.) *Race, Ethnicity and Welfare States: An American Dilemma?* Cheltenham: Edward Elgar, 3-27.

In his article on population policies in the special issue of *The Annals of the American Academy of Political and Social Sciences*, Gunnar Myrdal admitted that ‘a mild sort of nationalism’ inspired the Swedish population policy plans: ‘We are not interested in national expansion’, but ‘we in Sweden are all striving to build up a social and cultural structure of our own, better than the one we inherited’. He urged a move from means-tested social assistance to universal tax-paid services that were based on national solidarity and functioned as investments into human capital. ‘Science has its part to do’, he concluded, but he also pointed out that science ‘becomes a force only in conjunction with the irresistible claims arising out of a situation and a development which, by common opinion, is deemed to be disastrous and which therefore must be prevented’.[22]

Common opinion was an important question in Myrdal’s view on the political role of science. It was associated with a type of social criticism that can be called immanent critique: society is criticised by means of criteria that are conceived as its own normative standards.[23] In 1938 Myrdal travelled to the United States on invitation from the Carnegie Foundation to lead a large research project on the racial question, or ‘negro problem’, as it was called. The outcome of the project, the influential book *An American Dilemma* (1944), was actually based on the idea of immanent critique. ‘The American Creed’ consisted of the widely shared norms of freedom and equality, but it was not being realised due to racial inequalities and needed to be taken as a force for problem-solving politics. What is important here is that the criteria of immanent critique became identical with the value premises of scientific research. Myrdal insisted that social scientists had to make their value premises explicit and that such premises needed to be relevant, significant and feasible regarding the society under study. In *An American Dilemma* the value premises were derived from what he called the American Creed. In *Asian Drama* (1968), a large study on ‘underdevelopment’, he wrote: ‘Among all the heterogeneous and conflicting valuations that exist in the countries of the region, we have

22 Gunnar Myrdal (1938) ‘Population Problems and Politics’, *The Annals of the American Academy of Political and Social Sciences*, Vol 197, Issue 1, 203-204, 209, 213, 215.

23 On immanent critique in Marx’s critique of political economy, see Georg Lohmann (1986) ‘Marx’s Capital and the question of normative standards’, *Praxis International*, 6 (3), pp. 353-372.

deliberately selected the new ones directed toward "modernization" …, … "modernization ideals"…'[24]

In both cases, it is evident that Myrdal thought that his value premises corresponded to the best normative standards of the society itself and served as the criteria of its immanent critique. The choice of value premises was actually based on his view on development and underdevelopment. Social reality consisted of self-reinforcing processes, 'circular cumulative causation' between economic, political, cultural and other factors.[25] The task was to turn vicious circles into virtuous ones. Thus, social criticism should be immediately followed by social planning and social engineering. Social planning would contribute to the positive cumulative causation of social processes in which efficiency, equality, solidarity and democracy promoted each other.

The researcher would become the agent of a political process in which the normative standards of a society were used to criticise the actual circumstances in the society. However, as it appeared in Myrdal's 1938 account on the crucial role of common opinion in bridging the gap between science and politics, a researcher should not just identify the relevant normative standards of a society as his or her value premises, but also have an impact on those normative standards. Thus, education in the spirit of the Enlightenment became an essential task.

Created harmony and welfare world

In 1958, Myrdal delivered a series of lectures at Yale, and he elaborated them into a book called *Beyond the Welfare State. Economic Planning and its International Implications* (1960). He developed a vision of the perfection of planning in what he called 'created harmony' and based his argumentation on an 'enlightened citizenry' and 'the international idealism of all people'. In Western welfare states, he recognised two problems. One was the tendency of detailed bureaucratic control, and the other was nation-

24 Gunnar Myrdal (1944) *An American Dilemma. The Negro Problem and Modern Democracy*. New York: Harper & Row, 23; Gunnar Myrdal (1968) *Asian Drama. An Inquiry in the Poverty of Nations*. *Volume I*. New York: Pantheon Books, 54; Kettunen 1997, 162-163.

25 Gunnar Myrdal (1957 *Economic Theory and Under-Developed Regions*. London: Duckworth; Sebastian Berger (2008) 'Circular Cumulative Causation (CCC) à la Myrdal and Kapp – Political Institutionalism for Minimizing Social Costs', *Journal of Economic Issues*, Vol 42, Issue 2, 1-9.

alism. He divided the history of the planning of the welfare state into three phases. Its prehistory included uncoordinated public interventions as attempts to solve the problems that had been caused by 'the quasi-liberal state of mass poverty, much social rigidity, and gross inequality of opportunity'. Then, attempts at coordinating these interventions through planning were initiated and increasingly expanded. This meant increasing state intervention, which caused many people to 'confuse planning with direct and detailed state regulations'. However, in the third phase, when planning would proceed toward a 'created harmony', direct state intervention was likely to decrease:

> The third phase could thus mean an actual decrease of state intervention. The assumption is a continued strengthening of provincial and municipal self-government, and a balanced growth of the infrastructure of effective interest organizations. This would, in its turn, presume an intensified citizens' participation and control, exerted in both these fields.[26]

It was clear to Myrdal that there was no natural harmony or equilibrium of private interests. Neither was it through collective compromises per se that common good would be achieved. But the 'created harmony' would also not be the outcome of a Great Plan. The inseparable connection between planning and education was essential for Myrdal; an 'enlightened citizenry' was at the centre of this vision. Planning presumed and promoted the overcoming of short-term interests, the lower-level valuations connected with them and the lower-level knowledge that often served as disguising rationalisations for those interests and valuations. Planning also presumed that all relevant interests were institutionally articulated *and* that nobody, especially not the powerful, had any right to claim that their interests were universal. With such as 'created harmony', everybody would be able to see her or his interests reflected in a more general reference to society.

The normative standards associated with welfare states, such as equality and inclusion, could also be turned into a critique of the national limits of their current implementations, and this was what Myrdal did in this particular book. In the late 1950s, Myrdal was critically aware that '*the democratic Welfare State in the rich countries of the Western world is*

26 Gunnar Myrdal (1960) *Beyond the Welfare State. Economic Planning and Its International Implications*. New Haven: Yale University Press, 67-68.

protectionist and nationalistic' and that there was a discrepancy between 'National Integration versus International Integration'.[27]

At the national level, the creation of harmony meant the need 'to recondition the national community in such a way that for the most part it can be left to the cooperation and collective bargaining of the people themselves, in all sorts of communities and organizations beneath the formal state level, to settle the norms for their living together'. Internationally, the creation of harmony meant that 'economic balance in the world, and at the same time national stability and progress in all countries, should be secured by intergovernmental planning and concerted action, directed towards a coordination of national policies in the common interest'.

The message of *Beyond the Welfare State* had an overt linkage with Cold War confrontations and with conflicts related to decolonisation. Contrasting the Western welfare states with an unregulated market economy and the all-encompassing interventions by a Soviet-type state, Myrdal's vision of a nationally created harmony was one of varying Western proposals for a third way or a middle way, and for a convergence of the different trajectories of modern industrial society. Myrdal was assuring his American audience that the welfare state and economic planning, correctly conceived, would not lead to a Soviet-type system. His vision of internationally created harmony, or 'a Welfare World', in turn, was associated with contemporary expectations regarding the growing significance of Third World voices in solving global problems, especially within the framework of the United Nations system that Myrdal wished to reinforce rather than regional integration projects that had emerged in Europe. His theory of the different phases of planning recognised different roles for nationalism in different parts of the world: while nationalism had become an obstacle to progress in the Western welfare states, it could play a progressive role in the development of 'underdeveloped countries' and in their integration into the world economy.

A critical reader might find here a problem concerning agency. Myrdal put his confidence in 'the international idealism of all people, which I believe is a reality'.[28] He was actually applying his general principle that social research and political reforms should start from an empirical identification

27 Ibid., 111, 162, italics original.
28 Ibid., 214.

of prevailing values and the conscious choice to promote them as value premises for research and reform. As an empirical fact that Myrdal believed to be true, 'the international idealism of all people' provided value premises for extending the welfare state into a welfare world.

We can interpret the images of a 'created harmony', an 'enlightened citizenry' and 'the international idealism of all people' as a means of planning, that is, as criteria for the immanent critique of current circumstances and as objectives for education. However, we can hardly say that the Myrdalian self-critique of the state-centredness of the welfare state would have led towards a perfection of planning in a created harmony and a welfare world. True, ideas of decreased state intervention gained new power after the 1970s, yet they were inspired by Myrdal's vision of progressive planning much less than by the arguments for a spontaneous order developed by Friedrich Hayek, the cowinner, together with Myrdal, of the 1974 Nobel Prize in economics.

Concluding remarks

The way Gunnar Myrdal described the 'prehistory' of planning seemingly has much in common with how Karl Polanyi pointed out in *The Great Transformation* (1944) the political making of the market economy and the uncoordinated reactive emergence of attempts to plan. 'Laissez-faire was planned; planning was not', as he put it.[29] However, when Polanyi portrayed 'the discovery of society' as part of the counter-movement against the market economy, he especially highlighted the ideas of an innovative planner, Robert Owen, a Scotch manufacturer. In addition to Polanyi's classic book, we also encounter Owen in histories of the socialist labour movement as one of the so-called utopian socialists of the early nineteenth century, in histories of the international cooperative movement and in histories of the ILO as an early initiator of the international regulation of working conditions.

Indeed, the history of the welfare state is also part of a long history of plans for how to establish a created harmony and a welfare world. In varying

[29] Karl Polanyi (2001 [1944]) *The Great Transformation. The Political and Economic Origins of Our Time*. Foreword by Joseph E. Stiglitz. Introduction by Fred Block. Boston: Beacon Press, 147.

ways, the utopian charge of these visions – including 'the new humanism' of George Sarton – has been turned into a practical force of social critique and reform. It has facilitated the articulation of and compromises between conflicting interests in the defining of social problems and solutions.

In the current changes affecting welfare states, however, it is difficult to find any utopian dynamics, or any 'concrete utopia', to use the expression launched by Ernst Bloch.[30] In a world of increased cross-border mobilities of capital, information and people, the practical implications of the nationalism of Western welfare states have become more evident than they were at the time of Myrdal's critical account. However, the national welfare state is not expanding to form a Myrdalian welfare world, but rather being modified to serve the competition-state and security-state functions of the nation-state that aims to provide attractive operational environments to globally mobile economic actors and to prevent the entry of unwanted people.

[30] Ernst Bloch (1959) *Das Prinzip Hoffnung*. Frankfurt am Main: Suhrkamp.

Laudatio of Claude Diebolt

Glenn Rayp

It is my pleasure and honour to introduce our colleague Claude Diebolt from the University of Strasbourg and the BETA research centre. Let me explain briefly why the faculty of Economics and Business Administration has nominated him for a Sarton award.

First, with professor Diebolt, the faculty and the Sarton committee honour an outstanding scholar and academician, with an impressive list of publications in terms of quantity, scope and depth. As yet, Claude Diebolt has published 139 articles in peer reviewed journals and 47 books, book chapters and special issues. His publications span a range going from the American Economic Review and the Journal of Monetary Economics to Explorations in Economic History, the European Journal of the History of Economic Thought and, of course, Cliometrica, of which he is the founding and managing editor. Amongst his major scientific contributions, I would consider the recent Handbook of Cliometrics he edited together with Michael Haupert, a publication that is becoming a reference work on the methodology and the scope of (new) economic history and will determine the shape and evolution of the field in the years to come, as well as his work on long term economic growth, human capital and education systems, which is in particular relevant for our understanding and the development of unified growth theory.

Yet, outstanding scholarship and academic performance are not sufficient for a Sarton award. A Sarton nominee is invited to give a lecture on the history of science in the inviting faculty and may therefore be expected to have a significant contribution in this respect. How should we describe this in professor Diebolt's case?

Claude Diebolt is not only known for his research in cliometrics as such but as well for his relentless effort to convince economists of the relevance and value of historical research for their discipline. As editor in chief of Cliometrica, president of the Association Française de Cliométrie, organisor of the (8^{th}) World Congress of Cliometrics, member of the board of trustees of the Economic History Association or as president of the management board of the Cliometric Society, he fosters and encourages historians as well as economists to engage in (new) economic history research. To this aim and in defense of (new) economic history, he argues in his different publications how historical research has contributed to economics and in this way shows developments in economics as a science to which usually less attention has been paid. This I consider as his contribution to the history and methodology of economics.

The wedge between history and economics can be traced back to the *Methodenstreit*, when economics decided to follow the deductive approach advocated by Menger and the historical school retreated in a merely narrative approach. However, about Alfred Marshall, the founder of the neoclassical school in economic thinking in which the deductive, formal and mathematical analysis in economics materialized, Keynes (1924, pp.321-322) still wrote that:

> "The study of economics does not seem to require any specialized gifts of an unusually high order. Is it not, intellectually regarded, a very easy subject compared with the higher branches of Philosophy and pure science? Yet good, or even competent, economists are the rarest of birds. An easy subject at which very few excel! The paradox finds its explanation, perhaps, in that the master-economist must possess a rare *combination* of gifts. He must reach a high standard in several different directions and must combine talents not often found together. He must be mathematician, historian, statesman, philosopher – in some degree. He must understand symbols and speak in words. He must contemplate the particular in terms of the general, and touch the abstract in the light of the past for the purposes of the future. No part of man's nature or his institutions must lie entirely outside his regard. He must be purposeful and disinterested in a simultaneous mood; as aloof and incorruptible as an artist, yet sometimes as near the earth as a politician. Most, but not all, of this ideal many-sidedness Marshall possessed. But chiefly his mixed training and divided nature furnished him with the

> most essential and fundamental of the economist's necessary gifts – he was conspicuously historian and mathematician, a dealer in the particular and the general, the temporal and the eternal, at the same time."

To the early neo-classical economists, it seemed obvious that a capacity of synthesis was as necessary as that of analysis and an historical perspective remained clearly present in their work. More decisive for the split between history and economics has been the step of economics to systematically test theories and propositions quantitatively by means of statistical methods and techniques, i.e. the rise of econometrics. Statistical testing of hypotheses demand large scale data, which were difficult to provide by economic history in the pre-digital era granted that there was an interest to go beyond the narrative: "Econometrics was on the rise and economic historians were divided between those who abhorred it, and those who embraced it." (Diebolt and Haupert, 2016, p.977).

Yet, even from the early years of econometrics, the quantitative approach was not considered to imply a break between economics and history. In his History of Economic Analysis, Joseph Schumpeter, one of the founding members of the Econometric Society, wrote:

> "What distinguishes the 'scientific' economist of all the other people who think, talk, and write about economic topics is a command of techniques that we class under three heads: history, statistics and 'theory'. The three together make up what we should call Economic Analysis.
>
> Of these fundamental fields, economic history – which issues into and includes present-day facts – is by far the most important. I wish to state right now that if, starting my work in economics afresh, I were told that I could study only one of the three but could have my choice, it would be economic history that I should choose. And this on three grounds. First, the subject matter of economics is essentially a unique process in historic time. Nobody can hope to understand the economic phenomena of any, including the present, epoch who has not an adequate command of historical *facts* and an adequate amount of historical *sense* or of what may be described as *historical experience*. Second, the historical report cannot be purely economic: therefore, it affords the best method for understanding how economic and non-economic facts *are* related to one another and how the various social sciences *should* be related to one another. Third, it is, I believe, the fact that most of the fundamental errors currently committed in economic anal-

> ysis are due to a lack of historical experience more often than to any other shortcoming of the economist's equipment." (Schumpeter, 1954, pp.12-13)

Yet, as argued by professor Diebolt, it is in the shape of *cliometrics* that economic history has had the most substantial impact on economics and is a part of present-day "Economic Analysis". More than the mere application of econometrics on historical data, cliometrics is "theory applied to history" (Diebolt and Haupert, 2016, p.973), historical research driven by a definition of a problem for which relevant facts are searched. "Indeed, unless it is accompanied by statistical and/or econometric processing and systematic quantitative analysis, measurement is just another form of narrative history." (Diebolt and Haupert, 2016, p. 980).

Claude Diebolt identifies a threefold contribution of (new) economic history.

First, as a branch of history, using quantitative techniques and economic concepts to provide answers to questions about the past, like in the recent paper by Barjamovic et al. (2018) in which a gravity model is used to try to identify the location of lost Assyrian cities. At the crossroads of economics and history, the impact of cliometrics on historical research is probably as large as on economics.

Second, as a component of economics, using historical data to test economic theories and propositions. This was pioneered by Robert Fogel in the 1960s and 1970s and one of the few and recent contributions in economics with rock-star features can be considered as cliometric. Thomas Piketty's Capital in the 21st Century is nothing but an attempt to reconstruct meticulously long-run aggregate wealth series and their distribution for the major industrialised countries, analysed using a standard neo-classical Solow model, implying a major revision of the Kaldor stylized facts of economic growth. Data availability used to form an important obstacle for this dimension of cliometrics, but this seems ever less the case for two reasons. First, large and highly qualitative historical data are increasingly digitialised and made available for economic and historical research, requiring the typical cliometric skills, i.e. not only skillful application of statistic and econometric techniques, but as well historical insight to understand the meaning of the data and to interpret them correctly. Second, the more frequent use in economic research of event studies in a natural experimental setting implies that the turning points in history, unanticipated exogeneous shocks and random variation have become a rich source for economic analysis.

Finally, cliometrics contributes to the study of economics by allowing new approaches. With the work of Douglass North, a pioneer of the new institutionalist school and as such a forerunner of the institutionalist perspective on economic development (of which Acemoglu and Robinson are probably the best-known present representatives), cliometrics played a major role in the imbedding of institutionalist thinking in mainstream economic theory. As economics tend to become less axiomatic and more empirical about the behaviour of agents and the structure of markets, by analyzing the causes and the nature of economic change in a broad range of institutional settings, cliometrics may play a substantial role in this respect.

To present-day cliometrics may apply what Paul Samuelson once said about economics in general:

> "To a person of analytic ability, perceptive enough to realize that mathematical equipment was a powerful sword in economics, the world of economics was his or her oyster in 1935".

What cliometrics just may need in this respect, is what you'll find at the bottom of every e-mail message of Claude: "Que la force" (May the Force be with you).

References

[1] G. Barjamovic, T. Chaney, K. Cosar and A. Hortacsu, 2018, "Trade, Merchants, and the Lost Cities of the Bronze Age", https://www.aeaweb.org/conference/2019/preliminary/paper/BnebaNA2

[2] C. Diebolt and M. Haupert,2016, "Clio's Contributions to Economics and History", in *Revue d'Economie Politique*, 126 (5), p.977)

[3] J.M. Keynes,1924, Alfred Marshall, 1842-1924, *The Economic Journal*, Vol. 34, nr. 135,

[4] J. Schumpeter, 1954, History of Economic Analysis, Oxford University Press, New York.

We are Ninjas: How Economic History has Infiltrated Economics

Claude Diebolt & Michael Haupert

Abstract

We look at the evolution of the economic history discipline over the past century and note its growth, decline, and acceptance as a tool, but less so as a separate discipline. We contend that this has not led to the end of the discipline, but its acceptance as a standard part of the lexicon.

"I do not approve of Economic History courses quite unaccompanied by any Economic Theory."[1]

Introduction

In 1994 Christina Romer wondered whether economic history had come to the end of its useful life. While she quickly admitted that this statement was intentionally controversial and even misleading, she believed that the field of economic history had evolved to a point where it was no longer a separate, and oft poorly regarded stepchild of economics, but was now infused into the entire discipline. Her point was that economic history had not, in fact, ended, but been assimilated. She felt that the most exiting recent development in economic history was that the rest of the profession had recognized its value.

1 Sir William Ashley (1927).

That observation, along with some of the more somber, and we believe premature, reports of the demise of economic history, serves as the impetus for this work. We argue that Romer is correct. Economic history is not in a death spiral, but indeed has permeated the discipline. Further, we argue that this has long been the case. Perhaps the way to think of economic history is not as a separate discipline that specialists within economics practice, but an essential tool that appears in most economic research.

What is economic history?

Economic history is a subset of history. Both economists and historians are trying to tell plausible stories about the past, and they succeed or fail by narrative standards to connect one event to another. The new economic history (cliometric) movement in the late 1950s transformed the study of economic history from a narrative to a mathematical format. In the process, economic historians have contributed to the development of both economics and history by combining theory with quantitative methods, constructing and revising databases, and adding the variable of time to traditional economic theories. This has made it possible to question and reassess earlier findings, thus expanding the frontier of our knowledge of the past and its ability to portend the future. The use of history as a crucible to examine economic theory has deepened our knowledge of how, why and when economic growth and development occurs.

As long ago as 1892 Sir William Ashley, who occupied the world's first chair designated for economic history, made a case for the inclusion of economic history in the curriculum. He eloquently argued that the mere gratification of natural curiosity, of a desire to know about our past, what created it, and what led us to our present was motivation enough to study it. If for no other reason, economic history was needed to "widen [the] sympathies [of its students], enlarge [their] conceptions of the possible, and save [them] from the Philistinism of the market-place … and finally, there may be some who will be drawn to this field of inquiry by a hope … that they may thereby arrive at a more satisfying and intelligible conception of the evolution of human society."[2] More than a century later Peter Temin

2 Ashley (1893: 134-35).

(2016) picked up on that theme, arguing that economic history and economic development were two sides of the same coin, the only difference being the tendency of development economists to focus on poor countries outside of Europe, and the focus of economic historians on the development of wealthier countries. But he notes the close interrelation of the two lines of inquiry. They both analyze the growth of economies with new technologies, and both are concerned with the incentives that exist to encourage the adoption of new techniques, innovations, and institutions.

But Temin was hardly the first to recognize this link between economic history and economic development. In 1926 E. B. Lyon argued that economics "ought to be a theory of development and not merely an explanation of the method or manner by which humanity produces wealth and shares its income under a given set of social conditions."[3] Rondo Cameron argued that because the fundamental role of the economic historian is to describe, analyse and explain change, "any theory of structural change must, in order to command respect, be tested against historical or long-term data. The symbiosis of history, theory, and policy in application to problems of economic development is therefore a natural consequence."[4] And in the 1960s Hugh Aitken and Robert Gallman emphasized this link while making the case for economic history in the curriculum. "Economic historians have to be concerned with variables that the theorist normally excludes from his system … Economic history … requires a theory of economic development."[5] Economic history has a definite role to play in the education of all economists. "It will play this role best if it speaks explicitly of economic development."[6] The fundamental role of an economic historian "

Richard Tawney identified the role of economic history by focusing more broadly on the role of historians as chroniclers of social behaviour under a variety of conditions and environments with the object of identifying the characteristics of different types of civilization in order to "discover the forces in which change has found its dynamic, and to criticize the doctrines accepted in each epoch as self-evident truths."[7] The purpose of economic

3 Lyon (1926: 241).
4 Cameron (1965: 114).
5 Aitken (1960: 91).
6 Gallman (1965: 109-11).
7 Tawney (1933: 11).

history, indeed all history, is "ultimately to widen the range of observations from the experience of a single generation or society to that of mankind."[8] John Nef (1944) argued that economic history was an inexhaustible subject, tasked with providing a framework for the collection and presentation of mass quantities of information of all kinds and values.

At one time, when Purdue was at the centre of the new economic history, it required a graduate course sequence in economic history because it was the empirical part of economics. The skills taught in the economic history courses were designed to "provide the student with a basic knowledge of economic institutions and their evolution … [and] emphasize the impact of these institutions on economic processes." And since "all empirical work is by its very definition economic history, the [courses] introduce the student to the techniques of empirical testing of economic hypotheses. In particular it introduces the student to the sources of economic data and, in connection with the course in research methodology, the formulation of hypotheses in forms that are subject to test."[9] On a different note, bemoaning the frequent misuse of history, Rondo Cameron (1965) cited a more basic role for economic history as the watchdog to assure that it is used properly.

Ultimately, perhaps the best answer to the question "what indeed, *is* economic history?" might be a tongue-in-cheek remark tossed out by the late Professor H.W.C. Davis, who is alleged to have replied, in answer to this very question: "Economic history is that kind of history which requires a knowledge of economics."[10]

But the final word on the topic will be given to Joel Mokyr, because his view corresponds so closely to our own. He compares economic history to a small open economy. "Economic history has never been and should never be anything like a closed field in which practitioners converse mostly with one another. Instead, it stands at a busy intersection of history and the social sciences, where economists, political scientists, sociologists, anthropologists, demographers, and historians come and go."[11] We believe that

---

8 Tawney (1933: 11).
9 Cameron (1965: 113).
10 Clark (1932: 107).
11 Mokyr (2003).

economic history is exactly there, in the middle of that very busy, well recognized intersection.

The evolution of the economic history discipline

Economic history emerged as a distinct discipline during the course of the revolt against the deductive theories of classical economics, led by the likes of Gustav Schmoller in Germany and Sir John Clapham in England. The original aim of the historical school was to replace what they believed to be the unrealistic theories of deductive (the gathering of facts leading to a certain conclusion) economics with theories developed inductively (the development of theories providing evidence of the truth) through the study of history. They held that history was the key source of knowledge about humans and human organizations, and because it was culture and time specific, it could not be generalized over time or space, hence general theories were useless. Their view was that economics was best approached from the vantage point of empirical and historical analysis, not abstract theory and deduction.

Before economic history there were political economics departments and history departments, and neither was a natural home for economic history. Political economics departments tended not to focus on history. And the general approach by scholars trained in history departments in the 19th century was to consider economic factors as only one cause of change, and not always necessarily the most important one. Economic history set its first serious footings in 1895 when the London School of Economics opened its doors. It was founded in opposition to the tenets of orthodox economics. As a result, economic history was an important presence from the beginning. In 1901 it became the first British university to offer a degree in economics, and economic history became a possible specialty. The first teachers of the subject were W. A. S. Hewins, the inaugural director, and William Cunningham, author of the first English language textbook on economic history, published in 1882.

At the dawn of the 20th century it appeared that the attempt of the historical school to replace deductive theory with inductive reasoning had failed. In fact, the economics discipline was moving toward a more deductive approach. The movement to turn economics into a science, which grew out of the rising stature of the natural sciences, gave way to a new understanding that for economics to take its place at the pinnacle of the social sciences, it needed to formalize and rely more on mathematical models.

Economic History in America

Harvard was the incubator of economic history in the US. Charles Dunbar, founder of the Harvard economics department, along with his colleagues Frank Taussig, and J. Lawrence Laughlin, who later would found the University of Chicago economics department, offered courses in a variety of US economic history topics beginning in 1883. In 1892 Dunbar and Taussig were responsible for the hiring of William J. Ashley to the first chair of economic history in the world.

Ashley was strongly influenced by German scholarship, as was his Harvard successor, Edwin F. Gay. Gay imparted the standards and techniques of the German academy – the methodological principle of sticking to the facts, of telling history as it really was – on his colleagues and students. He used a multidisciplinary approach and taught his students that hypotheses had to reflect several approaches, including social, political, international, and psychological, as well as economic.

In the first decades of the 20th century economic history spread across departments, if not in influence within the discipline. Chairs in economic history were created at many leading institutions, but the discipline had difficulty gaining traction due to the lack of a dedicated journal or society to promote its research. Contributing to the problem was the growing fascination with the scientific method and its potential applications to economics, exemplified by the theoretical approach espoused by Marshall in the UK and soundly rejected by economic historians. In the US this manifested itself in the growth of economic forecasting, which eventually led to the creation of the *National Bureau of Economic Research* (NBER).

During his service to the U.S. government during WWI, Edwin Gay became convinced of the need for better economic statistics. He and Wesley Mitchell headed the Central Bureau of Planning and Statistics, responsible for the gathering and reporting of statistical data. Together they helped found the NBER to stimulate the collection and interpretation of historical statistics.

Mitchell served as research director at the NBER for its first quarter century. He gathered tremendous amounts of empirical economic data in order to draw inductive generalizations from it, combining his historical approach to understanding cycles, which he saw as a global phenomenon, with an urgent call for more data collection from around the world. The NBER was central to this data collection effort and served as a sort of haven for statistical economists. The mission of the NBER was to gather empirical information about the American economy in order to create a robust foundation for theoretical generalizations.

After WWI this expansion and increased proficiency in the use of statistical materials took attention, students, and resources away from economic history. Enrolment in economic history courses held steady since major universities required a semester of it in their graduate programs, but writing it as a field declined.

The NBER ultimately served as a catalyst for the change in emphasis from narrative to quantitative studies in economic history. Mitchell, Simon Kuznets, Arthur Burns, Solomon Fabricant, and Harold Barger produced a series of quantitative descriptions of American economic growth while at the NBER that measured growth as far back as the 1870s.

By 1941 Gay felt that the work of the historical economists had not been able to displace the "theoretical school," but did modify it. By then the use of the deductive method had become more guarded and the practitioners of this "dark art" had increased the range and depth of their contemporary observations, and their viewpoint had expanded to become less individualistic and more social. In conclusion, he called for the reunification of economic history and theory, noting that the economic historians knew a great deal about the long trends of productive energies and social pressures leading to economic growth, which could be combined with the tools of the theorist to lend greater insight into the growth process. Far from incompatible, he felt that true philosophical objectives and the careful assembling of data were complementary.

Over time economic history presented itself as empirical and multidisciplinary. Empirical in that it dealt with the facts of the past. The facts could be quantitative, as the NBER emphasized, or qualitative (as the German school believed was the responsibility of economic historians). It was also empirical in that economic historians saw history as a laboratory where they could test economic hypotheses.

The New Economic History Movement

After WWII, with the American economy booming, economists gained cachet. Economics with its rigorous models, tested from an abundance of numerical data by use of advanced, mathematically expressed formulae, came to be regarded as the paradigm of the social sciences.

At the same time economists were becoming more interested in the determinants of economic growth and what they saw as the widening gap between so-called developed and underdeveloped regions of the world. They saw the study of economic history as a source of insight into the issues of economic growth and economic development, and the new quantitative methods as the ideal tools for analysis.

The timing of the cliometric movement corresponded to the success of the quantitative growth studies of Simon Kuznets, a reflection of the infatuation economists had developed for the national accounting approach. This predisposed them to view the past through this same lens and altered their definition of historical evidence. Robert Fogel credited his mentor Kuznets as the primary inspiration for the work of the new economic history.

Kuznets may have inspired the cliometric movement, but it was Fogel who reunified economics and history. He used the latest techniques of modern economics and gathered reams of historical data to reinterpret American economic growth in sectors as diverse as railroads, slavery, and nutrition. Rather than conjecture about the causes of growth, he carefully measured them. He pioneered the use of large-scale cross-sectional and longitudinal data sets harvested from original sources to examine policy issues.

The cliometric revolution pitted economic "theorists" against "traditional" economic historians who were more likely to be historians and less likely to rely on quantitative methods. They accused the newcomers of bringing economic theory to history without a proper understanding of the facts (a familiar battle cry). The disagreement was about the choice of models. Traditional, or "old" economic historians claimed that realistic models had to be too highly generalized or too complex to allow the assumption of mathematical relationships. The "new" economic historians, however, were primarily interested in applying operative models to economic data. There was a difference in method between new and old economic historians that could not be ignored.

The main achievements of cliometrics have been to slowly but surely establish a solid set of economic analyses of historical evolution by means of measurement and theory, and, following the path blazed by Douglass North, to recognize the limits of neoclassical theory and bring into economic models the important role of institutions. Indeed, this latter focus ultimately spawned a new branch of economics altogether, the new institutional economics. Nothing can now replace rigorous statistical and econometric analysis based on systematically ordered data. Impressionistic judgements supported by doubtful figures and inadequate methods padded by subjective impressions have now lost all credibility.

The decline of economic history

The New Economic Historians threw their lot in with the econometricians. They turned to the collection and accumulation of historical data and their use in testing hypotheses about economic activity. In this way, cliometrics brought economic history into the mainstream of economics as it was developing. Economic history is now dominated by the cliometric method, so much so that it may be a contributing cause to the demise of economic history positions and courses. To non-historians it appears that economic history is little more than the application of economic theory to historical data. Departments facing declining resources feel they can do without a specialist in economic history when anybody can apply theory to old data … should they choose to do so.

The growing popularity of cliometrics led to a rift between economists who practice it and historians who practice economic history without the use of the formal models, which they argue miss the context of the problem and have become too enamoured of statistical significance at the cost of contextual relevance. Boldizzoni (2011) attacked cliometrics, focusing his sharpest criticism on the quantification of history at the perceived expense of its humanity. On the other side, cliometrics has lost some of its significance with economists, who see it as another application of economic theory, albeit using historical data. While applied economics is not seen as a bad thing, cliometrics is not seen as anything special. Rather, it is often perceived as the application of theory and the latest quantitative techniques to old data instead of contemporary data. In that world view, a cliometrician is just a theorist with a more limited repertoire – and hence a luxury in an environment of shrinking resources. As a result, cliometrics has been blamed to a degree for the demise of economic history positions in many economics departments. As early as 1986 William Parker foreshadowed this problem when he observed that what was lost in the move to theory and econometric emphasis was the humane interest of the old British political economy and social welfare and the idealistic German historical economist's concern for the whole society.

Economic history has been written off many times before. One of its earliest and most persistent doomsayers was Norman Gras, who in 1920 wondered whether economists were losing interest in economic history because "historical economics has become discredited, or because the statistical method as applied to historical data has failed, or because economic history has neglected to keep pace with the change in interest from production to distribution."[12] Ten years later he gloomily summarized the state of economic history as being neglected by universities, who regarded it as a very special subject, but one suffering a lack of intellectual resilience.[13]

A generation later, Hugh Aitken, perhaps doubting the ability of the nascent cliometric movement to deliver, warned that "there is no scarcity of evidence to suggest that economic history is at present in critical condition … Economic theory today, in most of its branches, neither draws on economic history for its data nor goes to economic history for empirical verification. Economic history, for its part, commonly uses only the crudest of the tools in the economist's tool-box and displays almost complete indifference to the refinements in analytical methods that occupy the theorist's working time."[14] And a quarter century later, Robert Solow expressed an equal degree of pessimism. When commenting on the recent work in economic history he expressed "the sinking feeling that a lot of it … gives

12 Gras (1920: 222).
13 Gras (1930).
14 Aitken (1960: 87).

back to the theorist the same routine gruel that the economic theorist gives to the historian. Why should I believe, when it is applied to thin eighteenth-century data, something that carries no conviction when it is done with more ample twentieth-century data?"[15]

More recently, we have heard that "the field of economic history … is in deep trouble … from both history and economics, it is in dire straits in each of these disciplines."[16] In 2003 Lars Magnusson referred to economic history as "a now rather defunct species."[17] That same year Robert Whaples commented on "the vast body of ahistorical economists who flip right past the economic history articles that still appear in the leading mainstream journals and wouldn't even consider picking up a journal or book with the word 'history' in the title."[18] And in his presidential address to the Economic History Association Paul Hohenberg warned that "our discipline is not exactly prospering and needs to keep proving its value in a competitive academic ecosystem. Why [it] is struggling [in North America, at least] is no secret: the underlying disciplines of economics and history have diverged sharply."[19]

So how bad is the situation? Since economists and economic historians alike have been predicting he proverbial falling sky of economic history for a century now, need we pay any heed at all? After all, we are obviously still here. While the demise of economic history has been staved off now, the fact that it has not yet succumbed does not mean it is immortal. It is resilient, but does face some significant challenges, despite the fact that it may be more widespread now than ever.

The disappearing economic history course

Recent scholarship has highlighted the drop in economic historians and economic history course requirements at leading PhD granting institutions.[20] Two examples will suffice to illustrate the problem.

15 Solow (1985: 330).
16 Coclanis and Carlton (2001: 93).
17 Magnusson (2003: 928).
18 Whaples (2003).
19 Hohenberg (2008: 340).
20 Haupert (2005), Mitch (2011), Temin (2016).

Temin (2016) noted that when he first joined the MIT economics department in 1965, the approach to graduate education had long since been a three-legged stool consisting of theory, econometrics, and economic history. Today, the three legs of the stool are micro theory, macro theory, and econometrics. Economic history is no longer required, nor is it listed as a subfield available to graduate students. In fact, among the 46 courses listed in the current graduate curriculum, six are statistics and econometrics courses, four are micro theory, and none are economic history.

In 2005, research by Haupert (2005) indicated that 7.1% of the economic historians then listed on eh.net had earned their PhD at the University of Chicago. This was second only to Harvard, which had produced 7.4% of economic historians, and just ahead of UC-Berkeley at 6.3%. Like MIT, the University of Chicago no longer requires a field course in economic history at the graduate level. Also like MIT, there are no economic history courses listed in the graduate course catalogue. The three core areas of study at Chicago are price theory, quantitative methods, and the theory of income. The decline of economic history at Chicago began with “the elimination of the economic history requirement for the PhD in the early 1980s, in the decline in the percentage of doctoral dissertations written in the field after 1990, and in the shift of the two remaining economic historians into other fields, and in the termination of the economic history workshop.”[21]

While the decrease in economic history positions is discouraging, many young economic historians market themselves as specialists in other fields, and indeed continue to publish in the economic history journals as well as other field journals. However, the drop in required economic history courses presents a grave concern for the future production of economic historians.

The disappearance of economic history from leading economics graduate programs is problematic. Without the tools being taught, without specific instruction in the methodology and approach, we risk extinction. We as economic historians don’t need to convince ourselves about the difference between economists using historical data and economic history, but apparently economics departments don’t see the difference.

21 Mitch (2011: 263).

Where are we now

Economic historians have contributed to the development of economics by combining theory with quantitative methods, constructing and revising databases, discovering and creating new ones entirely, and adding the variable of time to traditional economic theories. This has made it possible to question and reassess earlier findings, thus increasing our knowledge, refining earlier conclusions, and correcting mistakes. It has contributed greatly to our understanding of economic growth and development. The use of history as a crucible to examine economic theory has deepened our knowledge of how, why and when economic change occurs.

What makes economic historians unique is not their use of historical data or their focus on the past, but that they study the growth and evolution of economies over the long term. In this way, economic history's closest kin is development economics. In addition, the attention that economic historians give to noneconomic factors, such as legal and political systems, distinguishes them from economic theorists. Given the longer time span economic historians consider, doing so gives fuller attention to changes in institutions.

We are not at present attempting to measure this change over time, but rather arguing that economic historians have always had, and continue to have an impact far beyond its own discipline. We measure this impact by looking at citations of JEH articles and where they land.

Economic history is a field that crosses many disciplines, as can be seen by the JEL code distribution of economics articles and the broad range of journals publishing economic history articles. Finally, the citations of JEH articles occur mostly in non-economic history journals.

Hope may be found in Figure 1, reprinted from recent work by Ran Abramitzky, which indicated the rise in the percentage of economic history articles in top general economics journals over the past forty years. This optimism is buttressed by our analysis, over a longer time period, of who is reading the work of economic historians.

Our more recent look at a slightly more diverse group of economics journals supports these findings, as Figure 2 illustrates. We look at eight leading general and non-economic history subspecialty journals, and find a general increase in economic history articles, as designated by use of the

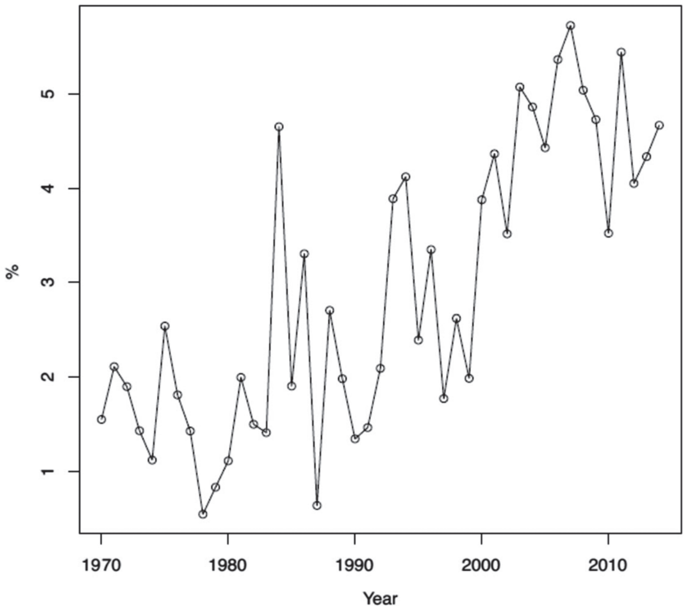


Figure 1: Percentage of Economic History Publications in the Top Five Economics Journals

Notes: From Abramitzky (2015) p 1243. Top five journals used: American Economic Review, Econometrica, Journal of Political Economy, Quarterly Journal of Economics, Review of Economic Studies.

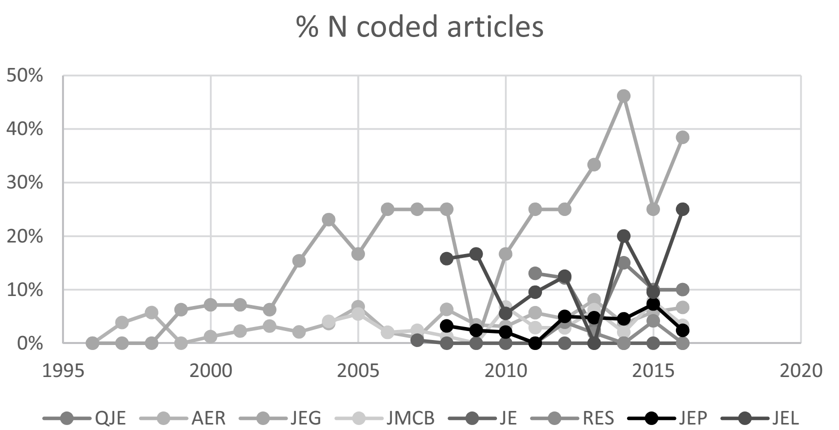


Figure 2

Notes: Quarterly Journal of Economics, American Econmic Review, Journal of Economic Growth, Journal of Money Credit and Banking, Journal of Econometrics, Review of Economic Studies, Journal of Economic Perspectives, Journal of Economic Literature

JEL code N in their descriptors. If we widen our definition of economic history to include history of thought and development, we see even greater reason to be optimistic (Figure 3). This corroborates Abramitzky's observation that the current generation of economic historians are likely to associate themselves with other fields within economics while still practicing the art of economic history.

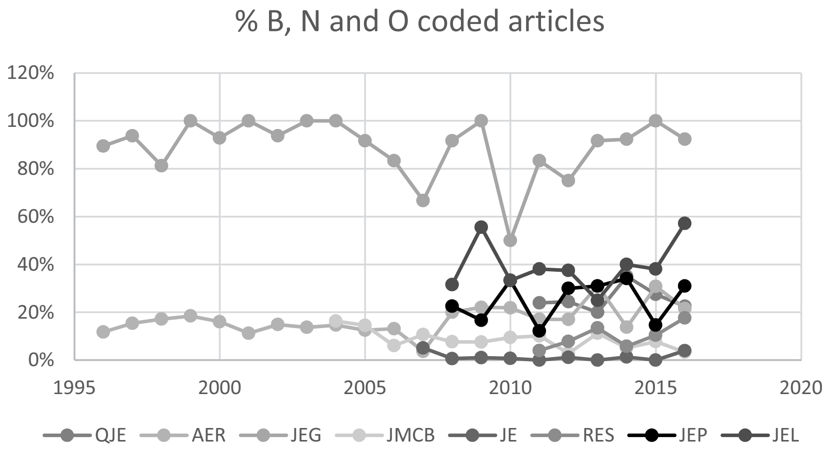


Figure 3

Table 1 illustrates the citations of economic history articles by authors publishing in economic history journals, identified as primary and secondary, and non-economic history journals. Primary economic history journals are identified as those that cater specifically to economic historians (think *JEH, EEH,* and *Cliometrica*, for example), while secondary economic history journals are focused on the history of specialized genres of economics (e.g. labour) or geography within the history field (e.g. Asia or central Europe). A complete list of the journals in each category can be found in the notes to Table 1.

The take-away from this table is the large and persistent percentage of citations in non-economic history journals. These data are limited only to citations of articles originally published in the *Journal of Economic History*. As our research progresses to include articles from other economic history journals, the number of citations outside of economic history journals will only increase.

Keeping in mind that in 1942 there were only two dedicated economic history journals (the *JEH* and *The Economic History Review*) it is not surprising that only 20% of citations were found in economic history journals. But even when defining economic history journals broadly, a healthy 50% of the citations of the work of economic historians currently appear in journals that are not economic history related. The low point of such citations was the 38% in 2015, and of course there have been only two years in which to cite that research.

The wide reach of economic history is a reflection of what John Nef argued in 1944 when he commented on the relationship between the various social sciences: "Any attempt to separate the economic side from the rest of life leads to a narrow view of history … the past work of economic historians has provided a hunting ground for anthropologists, sociologists, philosophers, political historians, economists, and for almost all other kinds of scholars is an indication of the relevance which economic history has for all other subjects."[22] That is still true today. One needs to look no further than the sources of citations of economic history articles (ranging from the *American Journal of Public Health* to the *Annual Review of Political Science* or the *Journal of Social Issues*) or the creation by economic historians of journals dedicated to the study of anthropometrics and institutional economics, to cite two recent examples.

Romer recognized this trend 25 years ago when she wrote "the field of economic history is no longer a separate, and perhaps marginal, subfield of economics, but rather, is an integral part of the entire discipline."[23] Economic history has always had a dicey relationship with the economics profession as a whole, but its very success, in the form of the cliometric revolution, which showed that economic historians could use the same techniques and theories as any other subfield, ultimately may have proved to be its undoing.

Despite the widely held esteem for the work of economic historians both modern and ancient (i.e. pre clio) as measured by the citations of the work and the wide array of journals in which it is both published and cited, there is the concern over the decreasing presence of economic historians begetting other economic historians.

---

22 Nef (1944: 16).
23 Romer (1994: 49).

Year	Articles	Citations	Citations in primary economic history journals	Citations in secondary economic history journals	% citations in primary economic history journals	% citations in secondary economic history journals	% citations in non economic history journals
2016	33	35	11	6	31%	17%	51%
2015	37	96	51	9	53%	9%	38%
2014	34	115	42	16	37%	14%	50%
2013	32	145	70	17	48%	12%	40%
2012	31	266	85	36	32%	14%	55%
2002	27	259	103	21	40%	8%	52%
1992	40	551	152	59	28%	11%	62%
1952	21	49	7	6	14%	12%	73%
1942	21	69	4	10	6%	14%	80%

Notes: primary economic history journals include Australian Economic History Review, Cliometrica, Economic History Review, European Review of Economic History, Explorations in Economic History, Historical Methods: A Journal of Quantitative and Interdisciplinary History, Investigaciones de Historia Economica, Journal of Economic History, Research in Economic History, Scandinavian Economic History Review. Secondary economic history journals include Accounting Business & Financial History, Accounting History, Accounting History Review, Annales, Business History, Business History Review, Central European History, Comparative Studies in Society and History, Economic History of Developing Regions, European Journal of the History of Economic Thought, Financial History Review, Geschichte und Gesellschaft, Histoire & Mesure, History, History and Technology, History Compass, History of Economic Rationalities, History of Economic Thought and Policy, International Review of Social History, Journal of African History, Journal of Policy History, Journal of the Economic and Social History of the Orient, Journal of the History of Economic Thought, Journal of Global History, Journal of African History, Journal of Chinese History, Journal of Economic and Social History of the Orient, Journal of Interdisciplinary History, Journal of Management History, Journal of Policy History, Journal of the Economic and Social History of the Orient, Journal of the History of Economic Thought, Labor History, Labour History Review, Law and History Review, Library & Information History, Management & Organizational History, Media History, Modern Intellectual History, Revista de Historia Economica, Rural History, Scandinavian Journal of History, Social History, Social Science History, The European Journal of the History of Economic Thought, The Historian, The Historical Journal, The History of the Family, Urban History.

The situation is not the same today as it was in 1932, when G. N. Clark claimed that "everywhere, the study [of economic history] is now pursued by more people and with greater interest than ever before."[24] And even as recently as 1965 when Rondo Cameron was able to boast that "the vast majority of professional economists are trained in graduate schools that require their students to take course work or examinations in economic history."[25] The view that economic history is a useful tool, and that the

24 Clark (1932: 100).
25 Cameron (1965: 112)

research of its practitioners is useful, has not translated into the belief that it is important to teach it as an independent course in graduate programs. Instead, many economists see economic history not as an enhancement of economic history, but just another application of it, different from its application to labour, or trade, or banking only by the age of the data used in the regressions.

It is not just self-preservation that underlies this concern for the disappearance of economic history courses. The economics profession does not appear to share the view of economic history espoused by Ashley, that a desire to know about our past is reason enough to study it. Today the typical economist cares about the past "only to the extent that it sheds light on the present. This is unfortunate and we can (and should) keep arguing that this is a narrow view of social science."[26] We risk missing many important contributions, or worse, failing to investigate them in the first place.

What do economic historians have to offer?

Economic historians have contributed to the development of economics in many ways, combining theory with quantitative methods, constructing and revising databases, and discovering and creating entirely new ones. This has made it possible to question and reassess earlier findings, thus increasing our knowledge, refining earlier conclusions, and correcting mistakes. In addition, this field has added greatly to our understanding of economic growth and development, affording the economic historian the valuable element of time as a variable, which the traditional theorist does not enjoy. The use of history to examine economic theory has deepened our knowledge and understanding within fundamental areas of research as to how, why, and when economic change occurs. It is perhaps in this area where the greatest contributions of economic historians have appeared.

Economic historians have contributed large and expansive data sets for researchers. The accumulation of the data is in itself monumental in many respects, but its usefulness has been expanded by the rapid growth of computing power. The ability to handle "big data" is not an economic issue by itself, but the construction of significant, important historical data

26 Abramitzky (2015: 1242).

sets, which can then be analysed using the latest econometric techniques and computer programs, is very much a contribution of economic historians.

Revisionist history is not a complimentary term, but the revision of misunderstandings in history is certainly both important and necessary, not just for the reason of setting the record straight, but helping us understand how and why economies grow (or do not grow, as the case may be). A clear understanding of the causes of economic growth is among the most important things an economic historian can do. Cliometricians have played a leading, and not always appreciated role here, overturning some accepted wisdoms, leading to hard feelings, resentment, and controversy. However, they have also pushed forward the frontier of our understanding of economic growth and development.

Among the notable "revisions" made by cliometricians were the findings of Conrad and Meyer (1958), Yasuba (1961) and Sutch (1965) that slavery was indeed a profitable investment. Easterlin (1961) used revised GNP figures to show that income in the antebellum South grew at a faster rate than previously believed, and Fogel (1964) showed that the railroad was not the determinant of American economic development that it was believed to have been.

Finally, economic historians have spawned entire new approaches to the study of economics. At the forefront are the new institutional economics, pioneered by Douglass North, and anthropometrics, which counts among its initial practitioners Robert Fogel. It is no coincidence that these two were recognized with the Nobel Prize in Economic Science in 1993.

Economic History plays an important role in the training of economists: Milton Friedman's classic treatise on money, as well as Simon Kuznets's path-breaking work on economic development, for example, were, to a considerable degree, based on historical analysis. We analyse the dynamic processes of development over time by formulating explicit formal models and econometric methods. We test hypotheses formally in order to enhance our understanding of such major determinants of the way we live today as the industrial revolution, industrialization and the information revolution. We use historical (often archival) data to test the extent to which economic theory can be validated or improved upon in a wide array of ways, spawning totally new perspectives, such as counterfactual history.

The granting of the 1993 Nobel Prize in Economics to two economic historians, Douglass North and Robert Fogel, is a clear recognition of our unique scientific contribution to the discipline.

But should we even have to argue for a place for economic history? "At the least pragmatic level, indeed, the worth of economic history is that of intellectual activity generally, and nothing should be easier than convincing professional intellectuals that such activity is worthwhile."[27] Economic history provides more and better economic facts, better economic theory, better economic policy, and does so over a longer period of time and greater variety of institutional settings than any other field of economic study can provide. The practical value of historical scholarship is not necessarily in its direct or immediate application. It is, rather, an indispensable part of the combined labour of the social sciences.

Conclusion

The meaning of the word "empirical" for (American) economic historians has varied considerably with the passing of time. One can observe a shift from a concept of empirical fact as understood by the "classical historian" (for whom anything, as opposed to only quantitative data, retrieved from archives can be used in his demonstration) to one as understood by (applied) economists (the empirical aspect consists of analysing numerical time series) and a convergence of theoretical viewpoints of historians and economists thanks to a common interest in the building of theories of development.

This (inductive) view is therefore intimately linked with the historical current in economics, the *German Historical School*, despite the use of more sophisticated techniques. It could be said that the two disciplines became closer, but probably within the frame of 'inductive' economics. On top of that, despite those early interests in building a kind of historically (i.e. inductively) grounded development economics, economic history mainly tries to provide answers to *historiographical* questions – and therefore speaks more to the historian than to the standard economist. As cliometricians have demonstrated, econometric techniques may be used, with

[27] McCloskey (1976: 438).

the reconstitution of time series and identification of missing figures by interpolation or extrapolation – something, by the way that annoys professional historians. But such cliometric procedures have nonetheless a historical vocation – that of shedding light on historical questions – considering economic theory or econometrics as auxiliary disciplines of history. And when the cliometric approach was mobilised to build a development theory based upon clearly measured facts, it developed an economics more akin to the objectives of the German Historical School than one participating to the movement towards highly abstract and deductive theory that characterised the development of the neo-classical school of the time.

A conventional belief among economists (in fact, that of Lord Kelvin) is that “qualitative is poor quantitative”. But could it not be possible that “quantitative is poor qualitative” might also sometimes be true? A big difference between economists and historians is the sense of so-called historical criticism and the desire to avoid any anachronism. In addition to close examination of the historical sources, this involves the close examination of the institutional, social and cultural context that forms the framework constraining the players’ behaviour. It is true that the (new) economic history will not build a general theory – it shares too strongly the belief in the necessity of examining economic phenomena in their context – but it could suggest a few useful ideas and insights, based upon solid investigations and correctly estimated stylised facts, to economists who are attempting to develop laws of economic behaviour (unlike history, economics is still a nomological science). Economists and economic historians can also cooperate and jointly author research. This is a view shared by Daron Acemoglu, Simon Johnson, James Robinson, and Oded Galor, among others, trying to use the material derived from traditional history to build new ideas useful for economic theorists.

In summary, it could be contended that good economic history is not an easy exercise. Becoming too narrowly “economic,” it would not be possible to answer certain questions that would require, for example, more information about the microstructure of financial markets or the actual functioning of stock exchanges during the period under scrutiny – it would only measure phenomenon that it cannot explain. It would require the specific approach (and extraneous information) of the historian to describe the reasons for the lack of relevance (or understand the shortcoming) of such an economic theory in a given context (precise place and period). It is perhaps only in this regard that economic history can provide something for economists by suggesting lines of research. However, if it became too "historical," it would cease to appeal to the economics profession. It is indeed a delicate balancing act, but one worth the effort to perfect.

Perhaps the biggest challenge facing economic history is that in its attempt to pursue truth, economic history is at the same time too vast and too small. In a historical sense, we try to accurately compile all the facts relevant to a given topic of study. The smaller the topic, the easier it becomes to gather and arrange all the relevant facts, and the more rigorous the result is likely to be. Thus the momentum of economic history is in the direction of further subdivision and specialization – to the point of disappearing altogether, indeed of being assimilated into every other branch of economics. But for the historian who aims to create general truths, the economic, like any other conventional division of the subject matter of history, is too narrow a conception.

John Nef recognized this challenge long ago when he prescribed a solution to what he saw as the declining relevance of economic history. "What economic history should become is an instrument for reducing rather than for increasing the number of compartments into which scholarship is now divided."[28] This sentiment was echoed more recently by William Collins when speculating on the future of economic history: "I believe that the boundaries of economic history, which have always been permeable, will grow less distinct."[29] To paraphrase Deirdre McCloskey (1976), the past does indeed have useful economics, and it is the job of economic historians to deliver that message.[30]

References

[1] Abramitzky, Ran, "Economics and the Modern Economic Historian." *The Journal of Economic History* 74, no. 4 (December 2015): 105-47.

[2] Aitken, Hugh G. J., "On the Present State of Economic History." *The Canadian Journal of Econmics and Politic Science / Revue Canadienne d'Economique et de Science Politique* 26, no. 1 (Feb 1960): 87-95.

[28] Nef (1944: 15).
[29] Collins (2015): 1232.
[30] Diebolt (2016): 1-4.

[3] Arrow, Kenneth J., “Maine and Texas.” *American Economic Review* 75, no. 2 (May 1985): 320-23.

[4] Ashley, William J., “On the Study of Economic History.” *The Quarterly Journal of Economics* 7, no. 2 (Jan 1893): 115-36.

[5] Ashley, William, “The Place of Economic History in University Studies.” *The Economic History Review* 1, no. 1 (Jan 1927): 1-11.

[6] Cameron, Rondo, “Has Economic History A Role in an Economist’s Education?” *The American Economic Review* 55, no. 1/2 (Mar 1, 1965): 112-115.

[7] Clark, G. N., “The Study of Economic History.” *History*, New Series 17, no. 66 (July 1932): 97-110.

[8] Clarke, Conor, “A two part interview with Paul Samuelson,” *The Atlantic*, June 17-18, 2009, https://www.theatlantic.com/politics/archive/2009/06/an-interview-with-paul-samuelson-part-one/19572/, downloaded January 2019.

[9] Coclanis, Peter, and Carlton, David, “The Crisis in Economic History.” *Challenge* 44, no. 6 (November-December 2001): 93-103.

[10] Collins, William J., “Looking Forward: Positive and Normative Views of Economic History’s Future.” *Journal of Economic History* 75, no. 4 (December 2015): 1228-33.

[11] Diebolt, Claude, “Cliometrica after 10 Years: Definition and Principles of Cliometric Research.” *Cliometrica* 10, no. 1 (January 2016): 1-4.

[12] Diebolt, Claude, and Haupert, Michael, “An Introduction to the Handbook of Cliometrics.” in Claude Diebolt and Michael Haupert, eds., *The Handbook of Cliometrics*, Berlin: Springer Verlag, 2016: v-xiv.

[13] Ellison, Glenn, “How Does the Market Use Citation Data? The Hirsch Index in Economics.” *American Economic Journal: Applied Economics* 5, no. 3 (July 2013): 63-90.

[14] Gallman, Robert E., “The Role of Economic History in the Education of the Economist.” *American Economic Review* 55, no. 1/2 (March 1, 1965): 109-11.

[15] Goodrich, Carter, “Economic History: One Field or Two?” *Journal of Economic History* 20, no. 4 (Dec 1960): 531-38.

[16] Gras, N.S.B., “The Present Condition of Economic History.” *Quarterly Journal of Economics* 34, no. 2 (Feb 1920): 209-24.

[17] Gras, N.S.B., *Industrial Evolution*, Cambridge Mass.: Harvard University Press, 1930.

[18] Haupert, Michael, “History of Cliometrics.” in Claude Diebolt and Michael Haupert, eds., *The Handbook of Cliometrics*, Berlin: Springer Verlag, 2016: 3-32.

[19] Haupert, Michael, “Economic Historians: A Preliminary Review of Society Responses.” *Newsletter of the Cliometric Society* 20, 2, (Summer 2005): 56-59.

[20] Hohenberg, Paul M., “Toward a More Useful Economic History.” *Journal of Economic History* 68, no. 2 (June 2008): 339-54.

[21] Knight, Frank, “The Limitations of Scientific Method in Economics.” in *The Ethics of Competition and Other Essays*, Mansfield Centre, CT: Martino Publishing, 2014 [1924]: 105-47.

[22] Lamoreaux, Naomi, “The Future of Economic History Must Be Interdisciplinary,” *Journal of Economic History* 75, no. 4 (December 2015): 1252-1256.

[23] Liebowitz, S. J., and Palmer, J. P., "Assessing the Relative Impacts of Economics Journals." *Centre for the Economic Analysis of Property Rights, Working Paper* 83-01, U. of Western Ontario, Jan. 1983.

[24] Liebowitz, S. J., and Palmer, J. P., "Assessing the Relative Impacts of Economics Journals." *Journal of Economic Literature* 22, no. 1 (March 1984): 77-88.

[25] Lyon, E. B., "Economic History and Its Place in the College Course." *Social Science* 1, no. 3 (May, June, July 1926): 238-42.

[26] Magnusson, Lars, "Review of *The Problems and Methods of Economic History* by Wittold Kula." *Journal of Economic History*, (September 2003): 928.

[27] McCloskey, Deirdre N., "Does the Past Have Useful Economics?" *Journal of Economic Literature* 14, no. 2 (1976): 434-61.

[28] Mitch, David, "Economic History in Departments of Economics: The Case of the University of Chicago, 1892 to the Present." *Social Science History* 35, no. 2 (Summer 2011): 237-271.

[29] Mokyr, Joel, *The Oxford Encyclopaedia of Economic History*, vol 1, New York: Oxford University Press, 2003.

[30] Nef, John U., "What is Economic History?" *Journal of Economic History* 4, Supplement: The Tasks of Economic History (Dec 1944): 1-19.

[31] Price, L. L., "The Study of Economic History." *The Economic Journal* 16, no. 61 (March 1906): 12-32.

[32] Romer, Christina D., "The End of Economic History?" *Journal of Economic Education* 25, no. 1 (Winter 1994): 49-66.

[33] Roy, Tirthankar, "Economic History: An Endangered Discipline," *Economic and Political Weekly* 39, no. 29 (Jul 17-23, 2004), pp 3238-3243

[34] Solow, Robert M., "Economic History and Economics." *American Economic Review* 75, no 2, Papers and Proceedings of the Ninety-Seventh Annual Meeting of the American Economic Association (May 1985): 328-331.

[35] Tawney, R. H., "The Study of Economic History." *Economica* 39 (Feb 1933): 1-21.

[36] Temin, Peter, "Economic History and Economic Development: New Economic History in Retrospect and Prospect." in Claude Diebolt and Michael Haupert, eds., *The Handbook of Cliometrics*, Berlin: Springer Verlag, 2016.

[37] Usher, Abbott Payson, "Two Notable Contributions to Economic History." *Quarterly Journal of Economics* 48, no. 1 (Nov 1933): 171-180.

[38] Whaples, Robert, "Review of John Black *A Dictionary of Economics.*" Economic History Services, March, 2004.

Laudatio Peter Bols

Christian Burvenich

As Proximus and on behalf of the Faculty of Veterinary Medicine of Ghent University (UGent) I am honoured to introduce my colleague Veterinary Physiologist at the University of Antwerp, Professor Peter Bols.

It is impossible to introduce Peter without a personal introductory note from myself. Though I know Peter since his PhD period in the Laboratory of Reproduction in the Veterinary clinic of Ghent University at the end of the 90s, I started knowing him much better at the occasion of the retirement of my colleague in Veterinary Physiology, André Houvenaghel at the University of Antwerp. As young and dynamic Veterinary clinician, Peter applied to the University of Antwerp for the vacant position in Veterinary Physiology. In 2001 he was appointed as associate professor in Veterinary Physiology and after three years he was promoted to full-professor.

I have always been impressed by Peters' broad interest in teaching, research and public services. As a young scholar he wanted to contextualise his broad expertise in animal reproduction in a multidisciplinary academic and social environment in which he wanted to collaborate with colleagues, scholars and, PhD and graduate students, even far behind his own discipline. Thanks to the academic freedom he was able to broaden his scientific horizon towards the history of sciences around the Veterinary profession. Peter succeeded to transform one of his dreams into an opportunity to acquire knowledge in the history of Veterinary Medicine at research level.

I met Peter several times at meetings of the "Museum committee" of our faculty in Ghent with Dr. Luc Devriese and Dr. Paul Desmet. Being a "science history" and "old book" lover (collector) myself, I appreciated

Peters' growing interest in "historical literature of Veterinary Medicine and Hippiatrics". In 2015, I was bestowed with the title of Professor Emeritus at Ghent University and at that occasion the Department of Comparative Physiology and Biometrics, UGent (Chairman: Luc Duchateau) organised an international conference in the Aula of Ghent University, entitled "Historia Physiologiae". The conference was introduced by international key-note speakers and one of them was Peter Bols holding an impressive historical talk, entitled: "Le Jumart, Myth or Mystery in Animal Reproduction?". His contribution was published in the proceedings of the conference, a book with a mixture of Arts and History of mainly "Lactation Physiology" (ISBN 9789058644091). From the beginning on Peter liked the concept of the conference and the book. One of my Dutch friends, veterinary surgeon and the President of the "Veterinair Historisch Genootschap, VHG", Drs. Rob Back, was also interested to attend to this conference where he met Peter.

In 2015 Peter became a member of VHG and in 2017 he was invited to present a talk, entitled "Veterinarian – cavalry commander, another field of tension in the Grande Armée of Napoleon" at the fall meeting of VHG in Antwerp where he was also acting as co-organiser. In this fall meeting a talk was also presented by Professor Peter Koolmees (Sarton medallist, 1998-1999) from the University of Utrecht. Peters' contact with VHG also resulted in a training in "Veterinary historiography" taught by Professor Peter Koolmees at the Faculty of Veterinary Medicine in Utrecht. Eventually, this course resulted in a dissertation on the relationship between "Military Commanders and veterinarians under their command". His cooperation with VHF, his growing network with Science historians and the invitations by several organisers who invited him to introduce a congress with the historical background of the scientific event, boosted Peters' motivation to persist on a historical side track within his academic career as veterinary physiologist, teacher and researcher.

So far, in a nutshell, the history of the relationship gradually forged between veterinary physiologists in Antwerp and Ghent, both interested in the history of veterinary and physiological sciences. Peter was especially interested in the development of the veterinary education and profession during the 17th and 18th century where as I was more interested in the history of physiology and experimentalism during the 19th century (Claude

Bernard who shaped modern medical research at the university and pharma industry). We got many interesting discussions on the "Enlightenment".

Meanwhile the idea to propose Peter for the "Sarton Medal" also grew. In its session of 22nd of November 2018, the board of the Faculty of Veterinary Medicine Ghent University, agreed to award the "Sarton Medal" in the academic year 2018-2019 to Professor Peter Bols, veterinary physiologist at the University of Antwerp. The proposal to the Faculty was made by myself and my colleague Professor Luc Duchateau, both tenured academic staff at the Department of Nutrition, Genetics and Ethology (Chairman: Professor Luc Peelman). The proposal was motivated as following: "Peter Bols evolved in a short period of time into a promising authority on the history of the first veterinary schools in Europe (Lyon, Alfort) & veterinarians in the "Grande Armée" of Napoleon (1805-1815)". The Sarton Committee (Chairman: Professor Robert Rubens) agreed and approved our proposal.

Let's continue now with some aspects of the Academic career of Peter Bols.

Peter E. J. Bols was born in Belgium, Niel, 30th of October 1964. After having finished his humanities (Latin-Mathematics), he graduated in 1986 as "candidate" in Veterinary sciences at the University of Antwerp (RUCA by that time) with "summa cum laude". He graduated at the Veterinary Faculty of Ghent University in 1989 with "cum laude". One year later he fulfilled his military service from which he was discharged honourably in 1990 as Lieutenant. He then joined the Department of Reproduction, Obstetrics and Ambulatory Veterinary Medicine at the Faculty of Veterinary Medicine, Ghent University, obtaining his PhD in 1997 (supervisor Professor Aart De Kruif) "summa cum laude" with a thesis entitled "Transvaginal Ovum Pick-Up in the cow: technical and biological modifications".

From 1997 to 1998 Peter completed a post-doc at the University of Connecticut, CT, USA in the research group of Professor Jerry Yang. At his return Peter fulfilled a number of research positions in the Pharmaceutical industry: at INTERVET in the Netherlands and France, and at JANSSEN ANIMAL HEALTH in Belgium. He was appointed as expert in the Veterinary Commission in Brussels of the "Federal Agency for Medicines and Health Products, FAMHP". As mentioned afore Peter succeeded

Professor André Houvenaghel. Peter has been teaching 18 years Veterinary (patho)physiology to bachelor students in Veterinary Medicine, and master students in Pharmaceutical Sciences at the University of Antwerp. Peter also fulfilled various management positions within the Department of Veterinary Medicine at the University of Antwerp.

As "Founding Diplomate" he was also involved in the creation of the "European College of Animal Reproduction". This European organization is operating under the umbrella of the "European Board of Veterinary Specialization, EBVS". It is responsible for the training of Veterinary specialists. After 3 renewals of his Diplomate status every 5 years, he has been nominated as member of the "Examination Committee" (2019).

In 2005 Peter was laureate of the "Veterinary Research Prize" of the "Royal Academy of Medicine of Belgium, KAGB". In 2012 he was listed as "ordinary member" of the Academy, and in 2014 he was nominated as member of administrative board. Since January 1st, 2019, Peter is Secretary General of the afore-mentioned Academy.

Peter has always been active in the internationalization of both research and education. Over the last 15 years he got good relationships with colleagues from Brazil. Several were visiting researchers in his laboratory. He was promoter of a VLIR-UOS project (2010-2015) project in collaboration with scientists from the "Centro de Investigaciones para el Mejoramiento Animal de la Ganaderia Tropical, CIMAGT" in Havana, Cuba. Within this project he was involved in the creation of an "in vitro embryo production laboratory". A new VLIR-UOS project is currently ongoing with the aim of increasing milk production capacity in cows in Camaguey, Cuba. He is the promotor in collaboration with colleagues from the Faculty of Veterinary Medicine in Ghent. Since January 1st, 2019, he is also the representative of the University of Antwerp in the board of the 'Vlaamse Interuniversitaire Raad – Universitaire Ontwikkelingssamenwerking VLIR-UOS'.

As I mentioned afore, Peter succeeded to transform his dream into opportunities for acquiring the necessary knowledge in the "History of Veterinary Medicine". Very early he became a passionate collector of old and rare bindings. Peters' first choice subject were old veterinary (hand) books, edited in France during the 18th century (1720-1820) associated with the creation of the 1st European Veterinary schools (Lyon and Alfort in France). Later, inspired by the work of Johan Op de Beeck (with whom he co-authored an article), Peters' interest was also drawn to the veterinarians in the "Grande Armée" of Napoleon (1805-1815).

Since 2014, seven "historical papers" (including one in the "Journal of Equine Veterinary Science") have been published on the role of the horse as a trigger in the creation of veterinary training in 18th century France. His historical papers are listed below.

1. Bols PEJ, De porte HFM. De handbibliotheek van de eerste studenten diergeneeskunde (Frankrijk 18de eeuw). Deel 1: Van voor Vegetius tot de Garsault. Vlaams Diergeneeskundig Tijdschrift 2014, 83:42-48.
2. Bols PEJ, De porte HFM. De handbibliotheek van de eerste studenten diergeneeskunde (Frankrijk 18de eeuw). Deel 2: Van Bourgelat en Lafosse tot de kennisexplosie aan het begin van de 19de eeuw. Vlaams Diergeneeskundig Tijdschrift 2014, 83:81-88.
3. Devriese L, De porte HFM, Bols PEJ. Aderlatingen en etterdrachten verdrijven het 'kwaad' uit het lichaam. Vlaams Diergeneeskundig Tijdschrift 2015, 84:101-109.
4. Bols PEJ, Dumas E, Op de Beeck J, De porte HFM. De Maréchal-vétérinaire in de Grande Armée van Napoleon (1805-1815). Vlaams Diergeneeskundig Tijdschrift 2015, 84:333-342.
5. Le Jumart, Myth or Mystery in animal reproduction? Bols PEJ and De porte HFM. In: Historia Physiologiae, Editors C. Knight and C. Burvenich. Ghent University 2015. ISBN 9789058644091
6. Bols PEJ, De porte HFM. Le Jumart, myth or mystery in animal reproduction? Vlaams Diergeneeskundig Tijdschrift 2016, 85:175-182.
7. Bols PEJ, De porte HFM. The horse catalysed birth of modern veterinary medicine in 18th century France. Journal of Equine Veterinary Science 2016, 41: 35-41.
8. Bols PEJ, De porte HFM. Cavalry officers in Napoleon's Grande Armée: self educated hippiatrists or ignorant commanders? (in preparation).

Peter has been invited 11 times by congress organisations to present a keynote lecture on the history of the origin of the veterinary profession.

During his talk at the solemn session and the handover of the Sarton medal, Peter will deal with a question: “Is modern day Veterinary Medicine a product of the Age of Enlightenment?”.

The “Age/era of Enlightenment” is considered as a turning period in the intellectual history of the West. Nevertheless, it remains difficult to define it in one immovable phrase that englobes uniform thoughts and thinkers. Without any doubt it has been a dynamic process that occurred during 18th century Europe. In this century a progressive evolution occurred in the veterinary profession: from quackery and so called “pseudo medical horse care” into an institutionalized training towards “Maréchal Vétérinaire”, which initially was intended to serve the military and a small group of noblemen.

Garabed Eknoyan from Baylor College of Medicine, Houston, concluded that the “Enlightenment”or “Age of Reason” was long lasting and that it has to be divided into two main stages: (1) a first stage during which new concepts and methodologies were developed; and (2) a second stage when they were tested, studied and applied. The dominant figures of the first stage introduced measurement, mathematics and physics (Descartes, Newton, Harvey,…). The second stage was characterized by increased literacy, easier access to knowledge (journals, books, salons and academies) facilitating the creation of the “scientific method”.

A unique event in the 19th century was the rise of the experimental method in physiology and its reliance on mechanical explanations to interpret the generated data. Experimental research was from the beginning on a reductionist discipline (a hypothesis driven science) trying to explain physiological phenomena (nowadays with a statistical probability). It is a Newtonian science that wants to explain and to predict. It was introduced in the academic medical training of physicians and veterinarians because it was thought to be useful in the clinic to cure men and animals.

Peter Bols is both physiologist and veterinarian. With his talk on the history of the academic training of veterinary surgeons he has to cope with two evolutionary aspects of “Veterinary Sciences”: experimental research and veterinary clinics. His story goes far behind the technical history of experimental research. This is an exciting but difficult challenge. Peter is treating completely new aspects in the history of “Veterinary training”. Peter tries to compile all evolutionary aspects that influenced and enabled the creation of modern “Veterinary medicine” at the middle of the 19th century with the breakthrough and specialization into different veterinary disciplines that still exist today.

In a first attempt the description of this evolution will turn around some well-known individuals by that time: from adventurers such as father and son De Saunier to generalists such as Solleysel and De Garsault, up to the “budding scientists” such as Bourgelat and Lafosse motivating their veterinary practice on (scientific) facts.

Dear Peter, I enjoyed our discussions over the last weeks/months. I and Professor Luc Duchateau remain thankful that our faculty and the Sarton committee agreed to offer you the Sarton Medal. There is no doubt that you deserve it and that you are a promising authority on the history of the first veterinary schools in Europe. We and all colleagues wish you good luck with your future historical research and academic career in general.

Is modern-day Veterinary Medicine a product of the Age of Enlightenment?

Peter E.J. Bols

Prologue: A Very Brief Look at *'The First 2000 Years of Veterinary Medicine'*

Summarizing the ancient history of the veterinary profession is a huge challenge that is largely beyond the aim of this paper. However, some aspects might serve a better understanding of the prevailing atmosphere in 18th century France when the cradle of Veterinary Medicine stood in the shade of a (r)evolution.[1] Numerous books[2,3,4] have been written on the subject which makes a brief prologue per definition incomplete. Going back as far as the domestication of animals, Jared Diamond[5] clearly pointed out that this complex process evolved in different ways at distinct places around the world. The first evidence of veterinary medicine goes far back to the region that is now Egypt, an area that was very rich in cattle, somewhere between 3.000 and 2.200 BC. A papyrus role discovered by Flinders-Petrie in 1888 mentions organ failure, colic and bloodletting therapies. This Eastern and Egyptian knowledge was inherited by the Greeks, among which Hippocrates was one of the most important protagonists (5th century BC), influ-

1 Bols, P.E.J. & H.F.M. De porte, 'De handbibliotheek van de eerste studenten diergeneeskunde (Frankrijk 18de eeuw). Deel 1: Van Vegetius tot de Garsault. *Vlaams Diergeneeskundig Tijdschrift* 83 (2014) 42-48.

2 Leclainche, E., *Histoire de la Médecine Vétérinaire*. Toulouse 1936.

3 Wester, J., *Geschiedenis der Veeartsenijkunde*. Utrecht 1939.

4 Dunlop, R.H., & en D.J. Williams, *Veterinary Medicine. An Illustrated History.* New York 1995.

5 Diamond, J., *Guns, Germs and Steel. The Fates of Human Societies*. New York, London 2000.

encing medical thinking patterns until the 20th century[6]. He developed the so-called 'humoral-pathology' principles referring to the four body fluids: blood, phlegm, black and yellow bile. While good health could only exist when these four fluids were in perfect balance, disease was considered to be the consequence of an imbalance causing 'sharp fluids' to materialize inside the body, which in turn resulted in fever. These 'caustic' fluids needed to be evacuated from the body, which is why numerous abortive therapies were developed, such as bloodletting and the use of diuretics, emetics, clysters and sweat inducing therapies[7]. The common basics of (veterinary) medicine such as anatomy and physiology were mainly absent and empiricism was the most important lead in diagnosis and therapy. Another important Greek author was Aristotle (384-322 BC), who is widely considered to be the founding father of comparative anatomy. Although he had a good understanding of the general body functions, he lacked insight into the functions of the different organs. He appreciated the role of the heart, but had little understanding of the function of the cardiovascular system. After Greek slaves passed on a number of medical concepts to the Romans when their retired soldiers started a sedentary life as farmers, general attention for agricultural sciences increased considerably, as evidenced by the work by Varro (116 BC), Plinius (1st century) and Celsus. The most influential author from this era was Galen (130-201), who promoted his Galenic principles (poly-pharmacy, Galenism, *Galenica*) that would continue to influence medical thinking until the 19th century[8]. During the first century AD, the Roman agronomist Columella published his 'Re Rustica' in which he mentioned veterinary medicine concerning '*bestia veterinaria*', the beasts of burden such as cattle and oxen, while using the expression '*veterinarius*' for the first time in history. Other Latin authors were Salonius[9] and Vegetius, a Roman aristocrat, who published the most complete work on veterinary medicine in antiquity (4th century), in which he described the symptoms of laminitis and colic in the horse and advised the use of '*polypharmacy*'. After the fall of the Roman Empire,

---

6 King, L.S., *When, Where and What is the disease? In: Medical thinking: An Historical Preface.* Princeton 1982, 165-183.

7 Devrieze, L., H.F.M. De porte, Bols, P.E.J., Aderlatingen en etterdrachten verdrijven het 'kwaad' uit het lichaam. *Vlaams Diergeneeskundig Tijdschrift* 84 (2015) 101-109.

8 Bols, P.E.J., & H.F.M. De porte, The horse catalyzed birth of Modern Veterinary Medicine in 18th century France. *Journal of Equine Veterinary Science* 41 (2016) 35-41.

9 Fisher, K.-F., The first Latin treatment on horse medicine and its author Pelagonius Salonius. *Medizinischeshistorisches Journal* 16 (1981) 215-226.

many of the writings of the Greek hippiatrists were compiled into what is called the 'Hippiatrica', a handwritten codex that was found in a Hungarian monastery, probably after being abandoned there by the Turks. Its first translation into Latin was published by the French physician Jean Ruelle (Ruellius) in 1530. However, at this time the center of gravity of veterinary medicine slowly moved to the East where the Arabs further developed medicine, pharmacy and chemistry and became the heirs of Greek veterinary knowledge.

Not many publications are known from the Middle Ages, with authors such as Jordanus Ruffus (equerry or '*Marescallus*' at the court of Frederic II, 1212-1250) and Laurentius Rusius (1228-1347), an Italian hippiatrist in Rome. His '*Hippiatria Sive Marescalia Laurentii Rusii*' was the first ever-printed veterinary work (1531). The most important animal species deserving of medical attention during this period were the horse, dog and … falcon, all of which were strongly connected with hunting, as demonstrated by the '*Livre de Chasse'* (book of hunting) of Gaston Phoebus (1389). The first printed book on equine veterinary medicine was published in Venice in 1472 by an unknown author. As can be expected, most of the early writings or printed books had only limited availability because book printing had not yet been scaled-up to produce many copies of a single work. While most of the books were stored in (private) libraries and monasteries, only very few people could actually read, not to mention that nearly all books were published in Latin. As a consequence, the dissemination of knowledge was slow and fragmented.

The 16th century was characterized by both enormous scientific progress and great advances in the field of book printing, which led to faster propagation of new ideas. Entrepreneurs like the Antwerpian Christoffel Plantijn[10] scaled-up the art of book printing to a level that allowed for the mass production and replication of printed copies of important books. During this century, approximately 60 books from different scholars all over Europe (with a dominant proportion from Italy and France) were published on veterinary medicine, mainly focusing on the horse[11]. Philologists, physicians, equerries, noblemen, and politically important men, everyone

10 Langereis, S., *De woordenaar. Christoffel Plantijn, 's werelds grootste drukker en uitgever (1520-1589)*. Amsterdam (2014).

11 Dejager, J., *Great books on horsemanship. Bibliotheca Hippologica Johan Dejager.* Leiden (2014).

started publishing books on equine medicine. However, it was Andreas Vesalius that caused a revolution in 1543 when he published his extraordinary tome known as '*De Humani Corporis Fabrica Libri Septem*' in Brussels. This masterpiece was not only a landmark study on human anatomy but was also an artistic work of high aesthetic quality that would inspire many authors. The most famous of whom, Carlo Ruini (1530-1598), was one of the most noted horse anatomists of the 16th century. Ruini's '*Anatomia del Cavallo*'[12] (1590) was the first book to focus exclusively on the structure of a species other than man and its splendid images were often plagiarized for years to come. In 1599, the French physician Jean Héroard, inspired by Vesalius and Ruini, wrote his '*Hippostologie*'[13] and introduced the term '*vétérinaire*' in France.

The publication of early 'veterinary' reference books ran parallel with the development of the art of equitation, beginning in Italy, where court life flourished and noblemen started to qualify in horseback riding. This is nicely illustrated by an engraving in Antoine de Pluvinels (1552-1620) most famous work, '*L'instruction du Roy en l'exercice de monter à cheval*' published in 1623[14]. During the following two centuries, horses became extremely popular among the upper class, not only as a riding and companion animal but also as an indispensable member of the foxhunt and as a draft-animal of the most prestigious carriages. Obviously, horses kept on fulfilling their indispensable role in warfare as military 'tools' on battlefields on a more or less parallel track. Wealthy citizens and noblemen started to establish stud farms and riding schools, hence creating an unmistakable need for caregivers for their horses. These equerries, most of whom were self-educated in Germany, Italy and France gave rise to a new literature genre, the so-called '*Traités Hippiatriques'*.

Traités Hippiatriques: The Onset of a New Approach in Veterinary Literature

The fact that a very heterogeneous group of people started to publish on 'horses' led to a remarkable mix of general knowledge on horse handling,

12 Ibidem, 216-218.
13 Ibidem, 142-143.
14 Ibidem, 354-374.

specific details on equitation and early veterinary concepts in most of these books.[15] One could find not only accurate descriptions on horse anatomy, long lists of diseases and potential therapies and descriptions of the harnesses, but also instructions on how to breed, trade, handle and ride horses. Indeed, in most of the works an important pathology section was included in which most of the attention was laid on external abnormalities. Etiologies, however, were often absent and the dynamics or pathogenesis of a certain disease was usually poorly or not at all understood. The most influential hippiatrists bequeathed some interesting books listed in a few excellent bibliographies such as the 'Essai de bibliographie hippique'[16], which includes thousands of books published in France before 1919, including biographies of the authors and exact bibliographical descriptions. An earlier bibliography was composed by Musset-Pathay[17] in a much broader perspective on agricultural sciences.

A detailed description of early handbooks on veterinary medicine[18] is far beyond the purpose of this paper. However, an interesting interplay can be visualized when the most important publications (1600-1800) related to the horse are categorized on the basis of their contents on 'veterinary medicine' and 'equitation' topics. This process is depicted in Figure 1. While some books treat both subjects with the same detail, others clearly chose one side. More interestingly, some authors (de Saunier, Bourgelat) publish both topics separately in different books, showing a progressive tendency over time to define 'veterinary medicine' and 'equitation' as separate topics. One of the oldest veterinary handbooks on horses that includes numerous etchings copied from the masterpiece of the Italian senator Carlo Ruini, and published in French (1647)[19] was 'La vraye cognoissance du cheval, ses maladies et remedes' by Jean Jourdin. As indicated by the title, this book only covers veterinary aspects of the horse as it is a compilation of ancient

---

[15] Bols, P.E.J., & H.F.M. De porte, De handbibliotheek van de eerste studenten diergeneeskunde (Frankrijk 18de eeuw). Deel 2: Van Bourgelat en Lafosse tot de kennisexplosie aan het begin van de 19de eeuw. *Vlaams Diergeneeskundig Tijdschrift* 83 (2014) 81-88.

[16] Mennessier de la Lance, G., *Essai de bibliography hippique.* Paris (1915-1921).

[17] Musset-Pathay, V.D., *Bibliografie agronomique ou dictionnaire raisonné des ouvrages sur l'*économie rurale et domestique et sur l*'art vétérinaire.* Paris (1810).

[18] Bols, P.E.J., & H.F.M. De porte, De handbibliotheek van de eerste studenten diergeneeskunde (Frankrijk 18de eeuw). Deel 2: Van Bourgelat en Lafosse tot de kennisexplosie aan het begin van de 19de eeuw. *Vlaams Diergeneeskundig Tijdschrift* 83 (2014) 81-88.

[19] Dejager, J., *Great books on horsemanship. Bibliotheca Hippologica Johan Dejager.* Leiden (2014) 382-383.

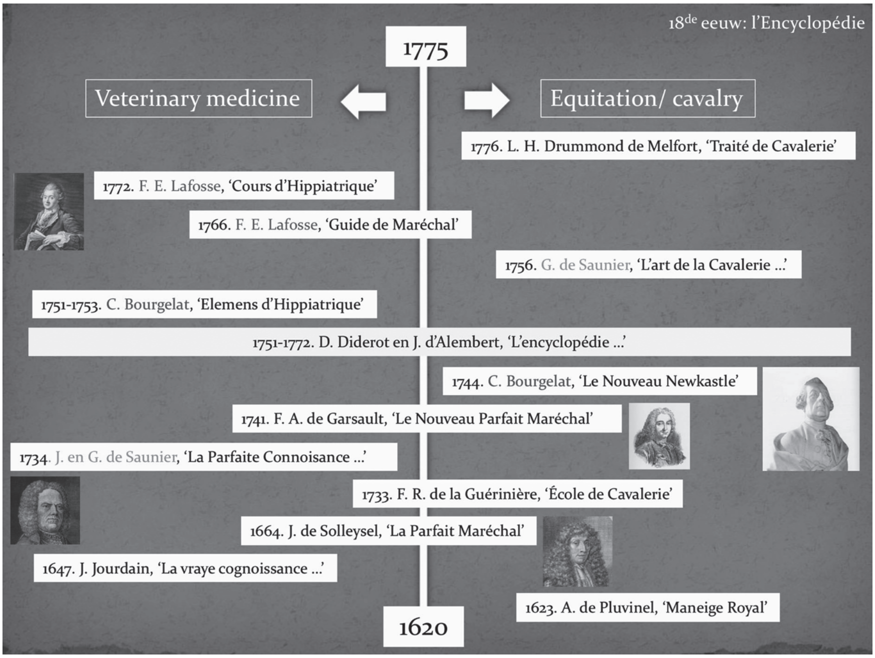


Figure 1: A summary of some of the most important titles of veterinary literature form the 18th century published in French. The title of the book is positioned on a horizontal axis with 'veterinary books' on the left hand side and purely 'equitation' books on the right hand side. Towards the end of the century, the basic orientation of textbooks becomes clearer: either equitation or veterinary medicine

and 'modern' texts on horses in general, horse medicine and anatomy. One of the most famous early equerries was Jacques de Solleysel (1617-1680), whom published with '*Le Parfait Maréchal*'[20] (1664) one of the very first French reference books on hippiatrics. Solleysel was generally considered more a 'veterinarian' rather than an equerry. His famous book had an enormous impact because of more than 10 re-editions (including a translation into German and two re-editions in English) of which the final one was published in 1798. 'Le Parfait Maréchal' covers a wide variety of subjects, including topics related to the art of equitation and more veterinary subjects, such as lists of diseases, anatomical descriptions, ageing by dentition and dozens of 'so-called' therapies. This book influenced the 'horse-scene' for about 150 years. An icon among publishers on hippiatry and

20 Ibidem, 394-403.

equitation from the 18th century is undoubtedly François Robichon de la Guérinière. He published his ‘École de Cavalerie. Contenant la connoisance, l’instruction et la conservation du cheval’ for the first time in 1733[21]. This outstanding work on horsemanship (published in the *‘in folio’* format) is basically focused on equitation and horse handling and became the equestrian bible and the foundation of classical horse dressage as it is still known today.

With a steady increase in the number of books on horses, the importance of the author was often reflected by the luxury and grandeur of the published edition. Some invested a fortune on compiling prestigious publications with larger plates, often plagiarizing earlier work. A beautiful example is ‘*La Parfaite Connaissance des Chevaux*’[22], dedicated by Gaspard (1663-1748) to his father Jean de Saunier. While the usual format of most books was *‘in-4’* (a little bit smaller than the current ‘*in quarto’* paper format) the book from father and son de Saunier is a publication *‘in-folio’* (26x40cm). The only edition was published in 1734 in The Hague (the Netherlands). It was an impressive project containing more than 60 large plates, although some of them were plagiarized from the work of Carlo Ruini, and only focused on veterinary topics such as anatomy and horse diseases. The adventurous history of both father and son de Saunier illustrates that not all equerries were of irreproachable reputation. Although father de Saunier is generally considered to be one of the best hippiatrists of his time, he and his son were both actively involved in dueling and had to leave France at a critical moment in their careers. Gaspard de Saunier finally ended up in Leiden – which is probably not a coincidence, see below – where he headed the riding school for more than 30 years. A few years after his death, some of his students published a second *in-folio*, ‘*L’art de la cavalerie, ou la manière de devenir bon écuyer*’[23], which was entirely dedicated to the art of equitation. De Saunier is one of the first 18th century authors publishing both on veterinary and equitation subjects in clearly distinguished books, indicating the start of a progressive separation of both subfields.

However, the final generalists’ horsebook influencing horse literature again for more than 100 years was still to be printed and reprinted for at

21 Ibidem, 578-593.
22 Ibidem, 594-598.
23 Ibidem, 599-601.

least 17 times with the last edition well into the 19th century (1843): 'le Nouveau Parfait Maréchal' by François Alexandre de Garsault (1693-1778), first published in 1741[24]. De Garsault was a French author, zoologist, botanist, designer and '*Capitaine des Haras du Roy*' covering the most diverse subjects. As the head of the royal stud farm, he was often sent out to study horse breeding while he was responsible for the practical organization of the French stud farm network, reporting to the government on this important branch of the national economy, basically for military purposes. His publication '*in-4'* summarizes all contemporary knowledge on horses in the broadest sense. Although the book can merely be appreciated as a compilation, it is valued because of its logical structure based on the methodological and meticulous attitude of de Garsault. On top of the information already contained in the '*Parfait Maréchal'*, de Garsault added 49 plates based on copper etchings, most of which he designed himself. He also included a dictionary with equitation terms and a set of remarkable drawings on botany with lists of available contemporary 'pharmaceuticals'. A first peculiar plate deals with the outer abnormalities that can possibly be detected on an individual horse. By assigning all possible defects to one and the same horse, he created the original concept of the 'defective horse' (Figure 2) which was then copied by famous authors later on (see below). A second remarkable plate depicts comparative anatomical structures between horse and man[25] (Figure 3). Published in an era when the catholic church heavily dominated daily life by positioning men in the center of creation, this must have been at least 'a novelty'. In general, the book contains a balanced mixture of both veterinary and equitation topics. The importance of this work is illustrated by the large number of re-editions (see above) by which this book can be considered widely available. Based on this, the book is believed to have had an enormous impact on contemporary equine literature and certainly inspired many subsequent authors. Additionally, to aid cavalrymen who were often on the road with their horses, de Garsault further cemented his reputation by publishing a pocket format booklet a few decennia later, the 'Guide du Cavalier'[26] (1770) that was much easier to carry around and contained the most important information on the care of horses.

24 Ibidem, 602-603.
25 de Garsault, F.A., *Le Nouveau Parfait Maréchal (Quatrrième edition)* Paris (1770) Plate II, 32.
26 de Garsault, F.A., *Le Guide du Cavalier*. Paris (1770).

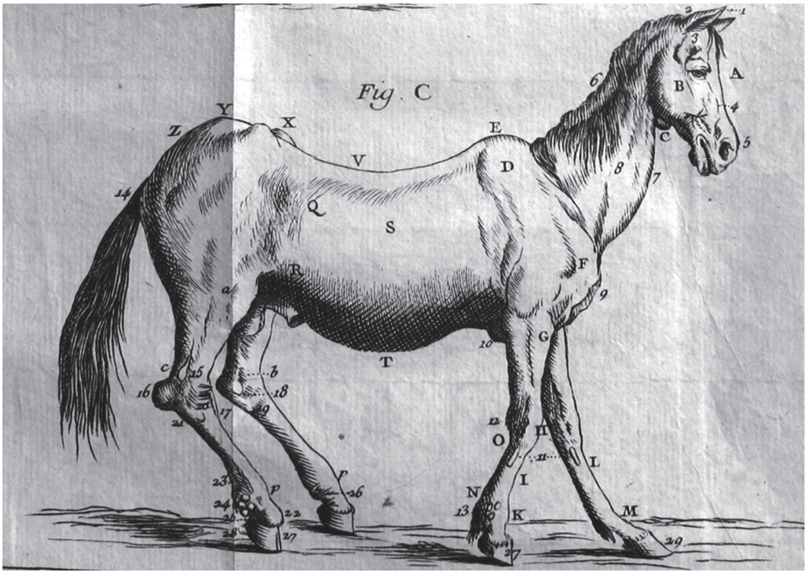


Figure 2: The ‘defective’ horse by François Alexandre de Garsault in his Nouveau Parfait Maréchal, published for the first time in 1741

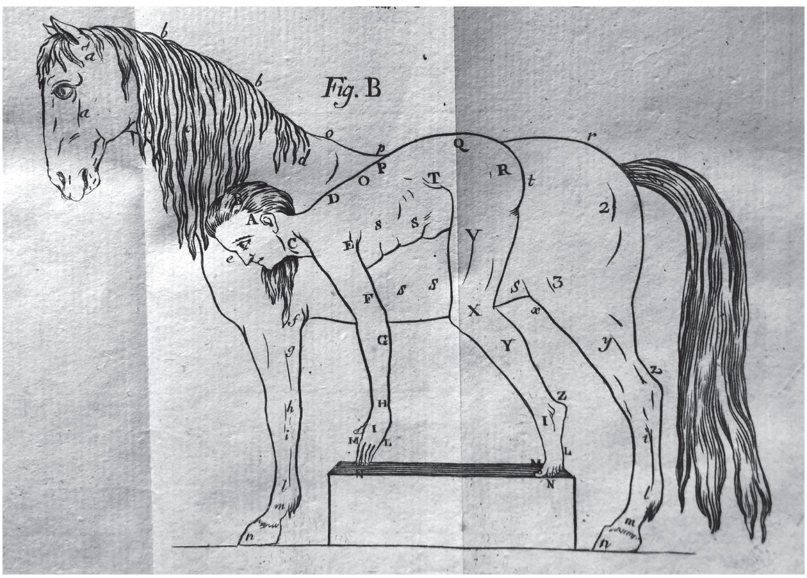


Figure 3: Comparative anatomy ‘avant la lettre’ by François Alexandre de Garsault in his Nouveau Parfait Maréchal (1741). The author was fully aware of anatomical similarities between man and horse

The 'Encyclopédie' of Diderot and d'Alembert, a Beacon of Change

Amidst many titles on horses appearing in contemporary literature, another project of totally different dimensions was on the eve of changing the worlds' view on the French 18th century. This 'Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers par une société de gens de lettres, mis en ordre par M. Diderot de l'Académie des Sciences et Belles-Lettres de Prusse et quant à la partie de mathématique, par M. d'Alembert de l'Académie royale des Sciences de Paris, de celle de Prusse et de la Société royale de Londres' was a general encyclopedia published in France between 1751 and 1772. The complete work comprised 28 volumes of which 17 volumes were published between 1751 and 1765. The remaining eleven volumes of plates were finished in 1772.[27] The history of the creation of this enormous endeavor is vividly narrated by Philipp Blom in his 'Encylopédie, the triumph of reason in an unreasonable age.'[28] He states that this Encyclopédie by the philosophers Denis Diderot and Jean Le Rond d'Alembert and the medical scientist Louis de Jaucourt, is not the largest ever published nor the first or most authoritative. However, the most significant event in the intellectual history of the Enlightenment is that with the publication of this Encyclopédie, a revolution was initiated against the Church and the establishment bringing free thought, secularism and private enterprise to the forefront.[29] Because of these often contested points of view on different subjects, the encyclopedia's privilege was suspended several times. Fortunately, it had several highly placed hidden supporters among which Malesherbes and Madame de Pompadour, making it possible to continue writing and compiling 'in secret'. The whole project did actually turn into a commercial enterprise employing dozens of people and serving more than a thousand subscriptions that could not be suspended anymore.

The Encyclopédie was very innovative, not in the least because it was the first one to list the different lemmas in an alphabetical order. Before, subjects were classified in the order of importance as decided by the authors and editors. As a result, theological and relevant political topics

---

[27] Dejager, J., *Great books on horsemanship. Bibliotheca Hippologica Johan Dejager.* Leiden (2014) 614.

[28] Blom, P., *Encyclopédie, The Triumph of Reason in an Unreasonable Age.* London (2004).

[29] Ibidem, p. xiii.

were usually included in the first parts of encyclopedias. In addition, the life's work of Diderot and d'Alembert was the first encyclopedia to focus on the mechanical arts in great detail[30]. By doing so, attention was drawn to the skills of regular people, crafts men and laborers instead of publishing the regular biographies of kings, emperors, popes, politicians etc … Needless to say, these innovations were not the changes that the establishment was happy with. Because the ultimate aim of the philosophers Diderot and d'Alembert was to include all contemporary available knowledge in the Encyclopédie, they had to expand their team to a wide range of contributors from all subfields of science. In addition, many of the most important protagonists of the French Enlightenment had sent in their texts on an enormous variety of subjects. Among them were Diderot, Voltaire, Rousseau, Holbach and Montesquieu. Besides these, a significant number of doctors and professors of medicine and chemistry, civil servants, members of the parliament in Paris, engineers, engravers, army officers, historiographers, bankers, economists and draughtsman contributed to this remarkable work.[31] For horse-related lemmas[32], most of them were written by one of the most important equerries whom would soon enough stand at the cradle of institutionalized veterinary medicine … Claude Bourgelat.

The Establishment of a Formal Veterinary Education during a Century of Change

As mentioned above, the importance of the horse in both agricultural and military terms during the 17th and 18th century can hardly be overestimated. However, the horse was still basically either a luxury product or a military fighting machine, and more was needed to finally initiate the establishment of a formal veterinary training in 18th century France. For a basic understanding of the complex contemporary social context, a very brief description of the (political) status of France at that time is necessary. The second half of the 17th and the 18th century was characterized by growing discontent within a large section of the French population, 80% of whom were dependent for their income on agriculture. To start with, climatological

[30] Ibidem.
[31] Ibidem, 142-143.
[32] Dejager, J., *Great books on horsemanship. Bibliotheca Hippologica Johan Dejager.* Leiden (2014) 614-619.

circumstances were extremely bad because Western Europe was going through a short 'ice age' that started around 1570 and had a negative effect on agricultural production until 1750[33]. The average temperature decreased with a few degrees, winters were extremely cold with lakes and rivers frozen for a long time. Summers were very wet, with extreme stormy weather and a low amount of sunshine. All of this resulted in poor crops and problematic consequences for the farmers lacking means to feed their livestock and themselves. With the French State being virtually bankrupt[34], a chronic famine gripped the entire territory as the result of a defective agriculture policy. The famous King Louis XIV was occupying the throne for several decades and many expensive wars had to be financed by additional taxes driving farmers and laborers into extreme poverty.[35] This was evidenced by a slowly growing opposition, which would finally culminate in the 1789 French Revolution.

To further worsen this desperate situation, regular outbreaks of rinderpest started to decimate cattle populations from 1714 onwards[36]. Massive cattle mortality resulted in decreased milk production and a chronic shortage of manure to fertilize the land, further negatively impacting crop production. Due to the lack of a well-established cattle-breeding policy, genetic selection of production animals, including sheep, had completely stopped. As a result, wool, a very important commodity, was extremely bad in quality. Thus, a huge gap was created between the classes, with the high end of the population being extremely interested in horses, and a large group of poor farmers which were frustrated because of the lack of a coherent political interest in agriculture and beasts of burden. Fortunately, within the political establishment, a small group of economists adhered to a doctrine called 'physiocracy'. They believed that the prosperity of a nation would depend on the development of agriculture as a basis of demographic growth. In other words, rebuilding a country would only be possible when sufficient healthy and well-fed laborers were available. Gradually, agricultural development became a priority again, which in turn allowed the spark to create a formal context for the establishment of veterinary education.

33 Blom, P., *De opstand van de natuur/ Die Welt aus den Angeln.* Munchen (2017).
34 Duruy, V., *Histoire de France, Nouvelle édition*. Paris (1880).
35 Op de Beeck, J., *De Zonnekoning. Glorie en Schaduw van Lodewijk de XIV.* Amsterdam (2018).
36 De Herdt, R., *Bijdragen tot de geschiedenis van de veeteelt in Vlaanderen, inzonderheid tot de geschiedenis van de rundveepest*. Gent (1970).

A key protagonist Claude Bourgelat, born in Lyon in 1712, was the son of a prosperous merchant.[37] Following a problematic childhood, he studied law in Toulouse. After a brief career as a lawyer, he probably joined the 'Musketeers', where he started his training in the art of equitation by the best equerries of France. In 1744, at the age of 32, he published his first book on hippiatrics 'Le Nouveau Newcastle'. He forced his major breakthrough by his next publication, the triptych 'Elemens d'Hippiatrique' published in 1750, 1751 and 1753 respectively.[38]

This triptych is generally considered to be the first 'modern style' veterinary reference handbook and certainly was innovative for several reasons. First of all, it is written in an interview style, as a discussion between a master and his apprentice. By answering question by question, Bourgelat describes the anatomy of the horse in profound detail. The exterior of the horse, the so-called '*hippometrie*' (Figure 4), would turn out to be one of his obsessions. In addition, a good deal of attention is given to the correct conformation of horses' legs including any possible defects. Secondly,

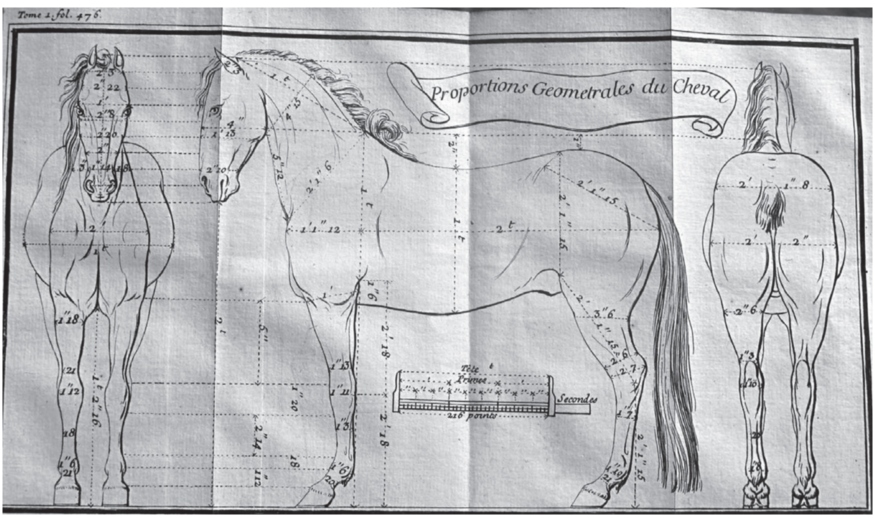


Figure 4: Hippometrie, or the 'science' of the 'ideal horse'. Claude Bourgelat was obsessed by it. Drawing taken from volume I of his triptych 'Elemens d'Hippiatrique', published between 1750 and 1753

[37] Cottereau, P., & J. Weber-Godde, *Claude Bourgelat. Un Lyonnais fondateur des deux premières écoles vétérinaires du monde (1712-1779)*. Lyon (2011).

[38] Bourgelat, C., *Elemens d'Hippiatrique*. Lyon (1750, 1751, 1753).

Bourgelat openly criticized age-old therapies such as bloodletting, describing them as dangerous and ineffective thus advocating for new approaches to veterinary care. And finally, the format of the book is very modest both in dimensions (*in-12,* 10x16cm) and due to the absence of illustrations (except for 2 allegoric frontispieces and one small folding plate, Figure 4), a trend break in an era when luxury books had started to be the golden standard. These innovations can certainly be considered as signs of educational engagement and pedagogical skills and indicated a true intention of making scientific knowledge more accessible to a broader public. The 'Elemens d'Hippiatrique' assured Bourgelat of major respect among his fellow hippiatrists and resulted in his appointment as a member of the Academy of Sciences in Paris, an important step in his national recognition.

Claude Bourgelat as an Encyclopedist

While a complete description of Claude Bourgelat's life and accomplishments is far beyond the scope of this paper, a few words on his contributions to the Encyclopédie of Diderot and d'Alembert are in place, within the context of the Enlightenment. Following publication of volume IV of this remarkable piece of art in October 1753, both editors announced new 'authors' of their Encyclopédie among which Claude Bourgelat[39]. Correspondence among the three of them shows that Bourgelat edited about 200 entries, establishing his status as 'Encyclopédiste' that could address all kinds of subjects. Diderot specifically praised his contributions under the heading 'Hippiatrique' and confirmed his direct collaboration on volumes V, VI and VII between 1755 and 1757. The next volumes were published in 1764, at the time that Bourgelat published a series of veterinary handbooks. The total of about 200 lemmas, differing in length and detail, counts for around half of the contents focused on veterinary medicine in the Encyclopédie.

Finally, Bourgelat confirmed in a letter from 1754 to Malesherbes, one of the chief censors of the government, that he was involved in the corrections of entries to the Encyclopédie. According to Cottereau and Weber-

[39] Cottereau, P., & J. Weber-Godde, *Claude Bourgelat. Un Lyonnais fondateur des deux premières écoles vétérinaires du monde (1712-1779)*. Lyon (2011).

Godde[40], this particular correspondence also revealed some important characteristics of Bourgelat's position and personality. He had a privileged relationship with Malesherbes and Diderot. While he contributed to numerous other entries beyond his specialism of equitation and horses, thereby showing his vivid interest in mathematics and technical subjects, he was praised for his scientific attitude. Bourgelat was a valued corresponding member of the prestigious Académie des Sciences de Paris. In addition, he could be considered a French nationalist whom, on different occasions, defended the values that the Encyclopédie stood for against the threats of being condemned by the Church. Finally, it seems interesting to have a closer look at one of the allegoric frontispieces that were included in Bourgelat's tryptich 'Elemens d'Hippiatrique' (Figure 5). Here, light is shed from a Greek temple on a dissection scene where a veterinarian is investigating the intestines of a horse. Almost literally, the enlightenment of veterinary medicine is depicted by Bourgelat, halfway the 18th century.

Figure 5: Allegoric frontispiece taken from volume 2 of Bourgelat's triptych 'Elemens d'Hippiatrique' with a clear reference to a Greek temple from which the light is shed on the intestines of a horse, a literal interpretation of the 'Enlightenment' of veterinary medicine

40 Ibidem, 197-198.

Conclusions and Further Research

The question if 'Modern-Day Veterinary Medicine' is a product of the Age of Enlightenment is a difficult one to answer in a few sentences and certainly merits further research. Undoubtedly, Claude Bourgelat was a key protagonist whose contribution to the evolution and institutionalization of the veterinary profession is beyond comparison and furthermore, clearly linked to the Encyclopédie of Diderot and d'Alembert. If this remarkable encyclopedic project is indeed an exponent of the Enlightenment, the answer might be positive. However, another question remaining is to what extent Bourgelat was able to transfer his enlightened ideas to his successors and under which conditions veterinary medicine entered the 19th century? How was Bourgelat perceived by his peers and what remained of his revolutionary therapies to treat and cure the horses on Napoleon's battlefields …?

Acknowledgements

The author acknowledges Prof. Peter Koolmees for his everlasting support and his enthusiasm to treat this topic on the history of Veterinary Medicine and Dr. Bronwen Martin for editing the manuscript and valuable suggestions on the structure of the paper.

Laudatio Erik Thoen

Marc Van Meirvenne

Until last year Prof. Erik Thoen was chairman of the Department of History of the "Faculty of Arts and Philosophy" at the Ghent University, where he currently is still a guest professor since his retirement. He is specialized in the field of rural history, and in particular the long term history from the middle ages until the 19th century. He stimulated research in different fields such as economic history, historical geography, rural techniques, environmental history and historical demography.

He is a member of the research group "Economies, Comparisons, Connections", which brings together historians and social scientists who study the interaction of historical processes at varying geographical, social, political, environmental and economic scales. As Director of the International Research Community "*Comparative Rural History of the North Sea Area" (CORN)* and as a key member of many other international organizations, he stimulated international comparative research all over Europe. He was also director of the editorial team of the four volume book series "*Rural Economy and Society in North-Western Europe, 500-2000"* (published 2010-2013). In the 1990s he was the first to introduce in Belgium the new field of "environmental history". Nowadays this topic is thought at most universities, often by his doctoral students or their students. He published more than 120 papers and books and received many scientific awards, such as the prestigious *Francqui Chair* in 2012 at the (Dutch) Free University of Brussels.

His contributions to the history of sciences are twofold:

1. He contributed to the historical study of agricultural techniques in

Europe, and particularly in Flanders, since the middle ages, with an emphasis on the social-economic evolution within the rural societies.

2. He contributed to the historical analysis and reconstruction, use and transformation of old culture landscapes, particularly in the pre-industrial periods. He focused on the ecological consequences of technical evolutions throughout the pre-industrial times, such as the transformations of the coastal areas in The Netherlands, as a consequence of over-exploitation by man in the Middle Ages.

Personally, I know Prof. Erik Thoen since some 6 years when we, together with colleagues from his department and the Department of Archaeology, succeeded in obtaining an FWO-project on the Zwin area and its impact on the rise and decline of the harbour of Bruges. As a soil scientist I was always triggered by his ability to "read" present-day landscape features and link these to past structures such as field boundaries, ditches or former villages.

Therefore, it is a real pleasure for me that I can announce to you Prof. Erik Thoen as beneficiary of a Sarton medal 2019. He track record fits perfectly in several activity domains of our faculty, such as agriculture, environmental technology and socio-economics related to agriculture. I am convinced that he will bring us a most interesting presentation on "Peasants, farmers and landscapes in pre-industrial Flanders".

Rural economy and landscape organization in pre-industrial Flanders

Erik Thoen

Introduction

Today, landscape organization primarily intends to provide people with a pleasant living environment. Until the 19th-20th century, however, this was not the case. In the past most landscapes were primarily intended to secure incomes and survival and were therefore shaped by the rural economy and (as compared to today) the completely differently structured social organization in mind. How did it happen and what were the consequences for our landscapes even until today? This is what this article is about. Geographically it mainly focuses on the rural part of the former county of Flanders, an early well populated area which roughly coincided with the current provinces East and West Flanders but also included parts of northern France and Zealand Flanders.

1. What is a landscape and why were landscapes important in past societies?

A landscape is the *visual* part of our environment. It is determined by both natural and human influences.[1] Since the past two millennia, landscapes have in many areas of our planet *mainly* been shaped by *human transformations* such as agricultural cultivation, the boundaries of land plots,

[1] Antrop and Van Eetvelde 2017

building, infrastructural elements (roads, canals) and vegetation. The features of these human transformations are basically influenced by the features of *social* organization. The social organization was shaped by the way survival was organized. The organization of social collaboration and survival was determined by property and power structures (rules) that could be different from one area to another and from one period to another.

It has been shown that in pre-capitalist societies, to a large extent, the social organization was situated at a rather regional level, while other elements of that organization such as religion, parts of infrastructural organization etcetera were influenced by supra-regional features.[2] The importance of regional variation was due to differences in family structures and family survival strategies. It was also supported by religion but was also the consequence of limited transport possibilities while also physical aspects of landscapes as well as regional climate variation required particular (often regional) forms of social organization. It led to a variety of regional formal and informal rules and agreements that one is calling today often 'institutions.

Production systems of an area where people produced according to comparable institutions and in the context of comparable social relations have been called *social agro-systems*. The regional variety of social agro-systems differed from one region to another as well as over time. However, gradually, and in some regions earlier than in others, old régime social relations were evolving towards more capitalist social agro-systems. Almost always, however, features or material results such as elements of landscape organization dating back to the pre-capitalist rural production systems were and still are surviving and are often embedded in the capitalist world economy. Indeed, changes in landscape design by humans was mostly not synonymous with making a *tabula rasa* of former landscapes: even when the societal structures had been changed, one always tried to integrate older elements into the new landscapes even when they had lost their original function; only in very rare situations were landscapes completely swept away. This also creates a number of benefits for historians since landscapes can also be seen as historical and even prehistoric sources in their own right.

2 Thoen 2004

Of course, besides human elements, natural elements too, such as soil structures, climate and relief (mainly changing due to natural factors although human influence was often present as well), shaped landscapes to a large extent. This variety of natural elements explains why in spite of a rather similar social organization a wide regional variety in landscape features is far from uncommon although it is at the same time possible to see common features as well.

History showed that, in general, past landscape design and evolution was especially shaped for the sake of food production and the survival of the majority of society and to benefit the power and prestige of a small (ruling) minority that determined the production structures, since the possession of land and landscapes was also considered as a way to externalize power and social prestige. Therefore, past landscapes can only be understood if we understand the way past societies organized their survival and power.

Since mankind had adopted a sedentary lifestyle (slowly since about 12,000 years ago), people could not organize income and survival individually. Collaboration between families and social groups delivered the necessary surpluses to organize the survival and to increase labor productivity up to the level that family survival and reproduction was possible. At the same time, as soon as people gave up living only from hunting and gathering, a hierarchical social structure (social organization) emerged in the rural societies that cultivated the land and produced foodstuffs.

As we have mentioned, within Western Europe that social organization was to a large extent organized in a rather regional way and it showed many regional differences as well as differences over time. These differences were also largely determined by the period during which intense land occupation took place but also by economic-geographical elements such as the vicinity of towns and markets, the rigidity of existing social and even political structures and power structures or the rise in investment costs.

2. The rural economy and 'social agro-systems'

An area with a particular social organization that organized its rural production and survival during a certain (mostly rather long) period according to the same social relations and in accordance with the environment, has been labeled in previous publications as a 'social agro-system'.[3] A comparable definition could be to envisage a social agro-system as "a production system of an area where people produced according to comparable institutions and in the context of comparable social relations and power structures". For reasons mentioned above, there was a rather considerable variety of social agro-systems from one region to the next as well as over time. On the other hand, social agro-systems with a lot of similar features may have existed in different areas and during different time periods.

While they often retained their features for centuries, these systems were not stable over time everywhere. While in some areas the social organization was evolving towards more capitalist social agro-systems – i.e. towards systems where (increasing) profit making and enlargement of holdings was the main goal of the majority of the farms – features of a *pre*-capitalist social agro-systems based on the *survival of the family* in many cases remained the main goal, rather than profit making and engrossment of holdings. In some areas, features of an economy based on family survival are even today still surviving and embedded in the capitalist world economy.

These changes in social agro-systems resulted to a certain extent in the adjustment of landscapes in which people lived. However, in general people were looking for the easiest way and a new social agro-system (a new way of collaborating to make money or to survive) did not lead to a completely renewed landscape! Older elements of former landscape organization were either integrated in new landscapes or became landscape elements that lost their original meaning.

In what follows we will focus on the influence of these agro-systems on landscape evolution. Some features are still visible today in the current landscape design.

---

3 Thoen 2004

3. The social organization in rural inland Flanders from the 12th century until the 19th century: the evolution of the peasant society in the preindustrial period in relation to other social layers

On the eve of the late medieval period (c. 12th-c. 13th century – in some places later, in others earlier), in general the social agro-systemic outlines (or and the social structures) had evolved towards one rather similar system in most areas of the County of Flanders. Slowly from about the 14th century on, only in the (large) ‘coastal part’ of Flanders did a new and even divergent system develop, as will be discussed further on (paragraph 4). In the core area of Flanders, with its sandy and sandy-loamy upper soils and early intensive colonization, the survival system developed in the classic middle ages lasted until well into the 19th century.

It is important to know that as early as about 1250 most areas of Flanders were already intensively reclaimed, which means that most woods and poorly used areas had disappeared and even that most common fields had been reclaimed, put under the plow or changed into well drained meadow lands and, most importantly, privatized. In a European context, this was rather exceptional. In many neighboring areas situated within the Northern and Southern Netherlands, this was not the case: in the Belgian Campine area (in north-eastern Belgium) for example and in many other sandy areas of other countries such as France, the Netherlands and Germany, large acreages of common, less intensively used and non-privatized fields, survived until the 18-19th centuries and covered in many parishes often more than half the surface. This was in large part due to specific power structures: less lordly power, fewer towns as well.

In the county of Flanders, however, until about 1200-1250, similar large areas of common fields and woods must have been abundant everywhere. Before that time, as was the case in most areas of Western Europe, a seignorial structure had developed: a network of local lords – some probably relatives of old Carolingian nobility, but others in all likelihood mostly richer farmers – could profit from the decline of the central power of the Kings of France and of Germany to usurp parts of the originally royal jurisdiction that generated power and money at the expense of the peasants. However – and this was peculiar to the Flanders area – the power structures quickly changed in favor of the most powerful of lordly class, the count of Flanders. From the 11th century on, the count managed to counter the local influence and power of the regional and local lords. The origins of the latter have been elaborated in a previous paper[4] However, the role of the count himself but also the role of the towns (being very powerful at least since the 12th century) cannot be underestimated as driving forces in that process. Indeed, from about 1100, a tactful management of the count of Flanders using rivalry and alliances between social noble and non-noble groups and the growing class of the bourgeoisie, as well as his strategy using titles and functions to eventually make the nobility and bourgeoisie dependent on him, simultaneously with the self-destruction of part of the nobility due to its urge for luxury and short-term income in devaluating cash money, eventually led to a rather specific situation in many areas of the Flemish countryside. It was due to the latter evolution and tactical play between all these social groups that the lordly power of the local lords was going down in favor of the power of the Flemish count and partly also of the towns, which had accrued real political power especially between the 13th and 16th centuries.

More importantly, peasants too profited from the described evolution and that influenced landscape evolution. This took place mainly in two ways:

Firstly because the previously mentioned rivalry between the social groups made sure that taxes (and other forms of surplus extraction) stayed relatively low until the late 16th century and the incomes of many Flemish peasants stayed stable or increased, which encouraged the holdings to split up; an equal split-up of holdings between family members, both sons and daughters, had become common and moreover formed an extra stimulus for reclamation and intensification of land use.[5]

Secondly because the foundations of power by the local lords were undermined due to the above-mentioned power struggle; also the lords themselves mostly had chosen to encourage the increase of the number of inhabitants in their local seigneuries by letting out the seigneurial lands and common fields. More people generated a higher income, while the peasants could profit as long as reclamations were possible, which was until about 1250 only. Moreover, one could ask for the new reclaimed lands rents in

4 Warlop 1975; Thoen 1988.
5 Thoen and Soens 2008.

money instead of rents in kind which was easier to use for their increased needs to buy modern luxury goods brought in or made by the townspeople. In the middle of the 13th century, however, that evolution halted since only marginal, only fewer valuable **lands** were available for reclamation. Many of the traditional sources of income of the lords lost their value. Moreover, from the late 13th century on, these lords were also losing power because they lost their judicial grip on the wealthiest population inside their seigneuries. Indeed, due to the growing power of the cities, it had become common practice for people living in the countryside to also obtain the status of a citizen (a 'burgher'). This practice, supported by the count of Flanders, weakened the power of the local lords since members of the "bourgeoisie foraine" (or "buitenpoorters" in Flemish), as these burghers were called, fell under the jurisdiction of the towns instead of the local lords.[6]

Moreover, most lords had been deprived of the possibility to generate additional new incomes based on the traditional seigneurial power. It is known most of the attempts of a seigneurial reaction failed since the count had deprived them of the legal basis to do so due to the fact that they had lost the higher judicial rights in their own seigneurie in favor of the more powerful central authority. However, some 'old' and many 'new' lords could become active in new activities or generate new income linked to business (toll-income); others obtained an administrative role in governmental administration. These new activities led to a new, more powerful and modern nobility, linked to a more modern state regime, joining or replacing the older noble families, at an accelerated pace from the Burgundian period.[7] However, becoming lord of seigneuries still remained important, not so much anymore for the income or direct power over tenants living in the seigneuries, at least not anymore, but especially for reasons of prestige. Therefore, since the late 13th century, many lords became 'collectors of seigneurial titles', in many cases probably without actually having set foot in the territories of these seigneuries – many seigneuries even did not get a real castle. Many nobles preferred to live in the neighbourhood of the central court or in the towns.

---

6 Thoen 1988; Thoen and Soens, 2015.

7 Buylaert 2010.

The restriction of the local power of the lordly class certainly lowered the tax burden, which favored the evolution of the incomes and of the legal status of the peasants living in their rural seigneuries.

The town councils and the urban *bourgeoisie* class tried to take over the power over the countryside, but they never really succeeded because the count of Flanders and later the dukes of Burgundy and the Habsburg kings tried to keep the property in and power over the countryside from falling into the hands of the wealthy citizens as much as possible.[8] Nevertheless, the towns partly succeeded in getting a grip on the country via the above-mentioned "*bourgeoisie foraine*"[9], the conquest of seigneurial titles, the purchase of land and the foundation of new farms. However, these towns never obtained full power over their hinterland, and certainly not in the sandy part of inland Flanders.

Finally, the central government ('the count') couldn't get full authority over the countryside either. Indeed, since the late 13th century and more regularly since the late 14th century, the count needed the consent of the estates ("staten") and stations ('*standen'*): the representatives of the large towns (commons), of the clergy and of the lordly class co-decided over the tax burden of the direct taxes.[10] Different from the English system in the Kingdom of France nobility and clergy did not make one estate but each station had its own estate.

Mainly due to this process, tax burden remained relatively low in Flanders until about the second half of the 16th century. This also worked in favour of the development of the countryside and of the peasants who lived in this county: it helped inland Flanders to become a very crowded area of many small poor peasants, who nevertheless managed to secure an income, allowing them to survive and feed relatively large families.[11] In addition, the residents of inland Flanders were given the opportunity to have their sons and daughters (often temporarily) work in the neighbouring agro-system of the coastal area.[12]

---

8 Thoen 1988.
9 Thoen 1991.
10 Blockmans 1978, Boone 2005.
11 Thoen and Soens, 2008.
12 Devos et al., 2011. About the social agro-system of the coastal area: see below.

3.1. The structure of the peasant society of inland Flanders: features

Mainly due to the described evolution of the power balances, these group of small peasants became the backbone of the rural economy in rural sandy-loamy inland Flanders. During the Old regime, an average of 50 to 70% of the surface of the land remained in the hands of these small peasants.[13] From the 13th century, they had become homesteader-possessor of most of their own lands. Due to long-term inflation, the rents (often going back to the early middle ages) they originally had to pay to the lordly classes gradually became rather unimportant. Moreover, from the late 13th century, they could appeal to quite sophisticated credit systems and developed a rather well-functioning charity system (in some areas up to 10 +% of the regional product[14]) for the poor who temporarily fell out the system, as recent research has shown. The village communities and solidarity systems were relatively strong. The amount of common fields, however, was limited[15].

During the old regime, the majority of Flemish villages were structured as follows. Most of them had only one to a few larger farms per village (with an average size between 20-80 ha), which were cultivated through a leasing system and owned by citizens and nobility. The large majority of the acreage was part of smaller 'family'-holdings owned by the peasants themselves (in exchange for a rather symbolic rent). The peasants also leased land between each other. Some land (gradually a larger part of the total acreage) was bought also by externals but most peasants succeeded in keeping their homesteads in property. That (small) homestead being property of the peasant was and remained the backbone of the survival system until the 19th century. Beside this homestead, land plots were sold and bought a lot and this was mainly attuned to a life cycle system: new families started with a small amount of land, increased their holding with an eye to the enlarged family needs and possibilities and they restrained their holdings again at the end of their active carrier as peasants. The few larger farms – mostly between 1 and 5 per parish – were integrated in the system. They

13 See the many data in the 16th century tax registers called 'penningkohieren' (c. 1570) e.g. Between the many studies of that source the most useful is Abbeele van den,1985.

14 van Bavel et al. 2015.

15 Vanhaute and Lambrecht 2005.

delivered extra labor during the high season. These farms lived in a kind of *symbiosis* with the smaller peasants who worked on the lands of the larger farmers, mostly as part-time servants only.

As early as the Middle Ages, the described family-survival system was also sustained by so-called protoindustrial activities. Indeed, it is a mistake to think that a rural society was mainly involved in agriculture only. In prehistoric times already, peasants tried to gain additional incomes from industrial activities. In inland Flanders, especially flax processing and linen production and in some areas the making of woolen drapery was very well-developed. Despite the very low labor productivity of these activities, they could provide an additional income and were an aid for the survival of the family.[16]

At least from the 17th to early 19th century, in (inland) Flanders, linen, woven on the countryside, was exported in large quantities to the Americas to dress the slaves in that continent, but the activity was well-known from the high middle ages. In this way, peasant societies were gradually integrated into the processes of the growing world economy. Apart from textiles[17], other protoindustrial activities were common in the countryside such as the making of clogs (made in the sandy areas adjacent to the polder areas, but mostly exported to these clay-polder areas, where they were very handy not to get stuck in the boggy soils)! In some areas also barrel making as well as basket making were popular as ancillary activities as well (baskets were made with willow, which was quite abundant in Waasland)[18].

3.2. The land and labour productivity in inland Flanders

The intensive use of land by the peasants in inland Flanders is responsible for an exceedingly high *land* productivity in agriculture, probably from the 13th century onwards already.[19] This means that, probably almost nowhere else in the world was the production of land per surface (e. g. per ha) as high as in (the current provinces East and West) Flanders. Therefore, it is not surprising that, especially from the 17th century on, until the mid-19th century, so many foreign visitors and agronomists were full of praise and

---

16 Thoen and Soens, 2015 a and b.
17 Vandenbroeke 1977, Mendels 1975.
18 Dewulf, 1979
19 Thoen, 1989.

even described and studied the techniques used in order to introduce them in other countries such as in England, France and Italy.

And rightly so, of course: many of the techniques mentioned were not only applied here but also invented and developed in Flanders (see below).

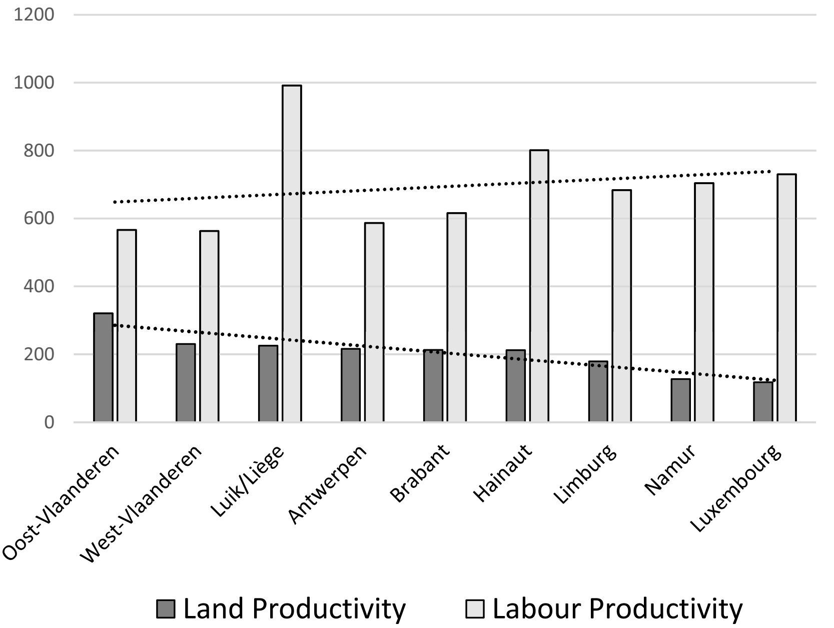


Figure 1: Land and labour productivity in Belgium compared (a°1812).[a] Note the high land productivity but low labor productivity in the provinces of East and West Flanders being core areas of the former inland part of the County of Flanders. The coastal area of Flanders is today meanly situated in the Netherlands (not on the graph)

a. Data based on Goossens 1993; see also Dejongh and Thoen 1999.

This is not to say, however, that (inland-) Flemish agriculture boasted a high labour productivity and high welfare. Quite the reverse was true: Flemish peasants had to work very hard for a relatively low income.[20] The majority of holdings were very small. Peasant family incomes were necessarily complemented with child labor. This can explain why population pressure was high until the 19th century, when birth control became general (and the number of family members was going down) and when the peasant

[20] Vanhaute, 2001

economy lost its privileged position and was gradually replaced by an economy based on wage earning.[21]

3.3. Well-developed rural techniques as a consequence: Flanders was ‘the garden of Europe’

Due to the above explained evolution, peasant holdings were small in size. Roughly, one can say that most holdings were between 1 and 2 ha in size only, which is very small. However, this put a pressure on family holdings to use their land as intensively as possible.

Indeed, we have proof that already from the late 13th century onwards, the field systems[22] and crop rotation systems that had become of common use gradually became more free, although regional differences continued to exist. Due to a structural lack of manure – common all over Europe and typical of the old Régime – it was only in the late 18th century that ‘long fallow’ disappeared completely out of the crop rotations as one of the earliest areas in Europe but in Flanders this evolution started at least in the 13th century and first on the (large majority of) small holdings. From the 17th-19th centuries, many agronomists were admiring the intensive agricultural techniques.

These techniques were mainly based on the application of the following techniques that had an influence on former landscape design:

- A well-developed drainage system of the fields. From the 14th century at least, cultivation was applied on rather narrow high-backed ridges. It was typical for these raised beds (Dutch: ‘*beddebouw*’) to be eliminated after the season and completely reconstructed during the next season. The system of drainage with underground pipes was only slowly introduced in agriculture from the 18th century on.
- In some areas, cultivation on those kinds of ridges was not applied. Instead, as on the sandy part of the *Waasland* from the (14-?)16th century, lenticular fields were made. In these areas, every field plot was also enclosed with deep fosses and tree rows, often pillows. This system had the same draining function as the high-backed ridges elsewhere in Flanders.

---

21 Thoen, forthcoming

22 Thoen, 1988, 1990, 1998

- The sorts of crops cultivated led to more bread grains being introduced in the field systems.
- The use of productive tools such as the Flemish hook (pick) with a short but broken arm that allowed a very fast harvesting and cutting the straw rather low close to the surface level. It was also useful for labor division; before, it was mainly women who harvested with the scythe (dating back to the prehistoric period).
- The cultivation of very labor and nutrient demanding plants (hops, flax).
- The use of turnips as green fodder and for tubers (esp. since the late 15th century).
- Intensive plowing (up to 6 times per season) as well as the use of the spade (in some areas, 30% of the acreage was not plowed but dug with the spade by the smaller peasants. Those who had enough money but no horses rented horses to cultivate their land.
- Intensive weeding (by all family members).
- Cultivation of dye plants (madder, woad, weld).
- The use of so called ‘up and down husbandry’ (13th century) in areas were the fertile layer of the soil was still small.
- The integration of fodder crops in the field systems, giving oxygen to the soils (*leguminosae*: all kinds of beans and peas increasingly cultivated from the 14th century) (on the long fallow and short fallow), also used as green fodder.
- The increasing application of stable feeding and the use of stable manure.
- The use of clover on the fallow (since late 16th century) (clover cultivation improves land fertility).
- The Flemish farmers were he first to use fluid manure (with animal urine) from the 18th century.[23]
- The combination of wood culture with agriculture (with wide living plot boundaries, e.g. in the light, sandy region around Ghent; 17th – 19th century).
- The gradual elimination of long fallow (before sowing winter cereals) and later also the short fallow (turnips/ clover).
- As a consequence, high yields not only of bread grains (rye, wheat) but also of oats (fodder for horses and other animals).

---

23 As has been shown recently by De Graef 2018

3.4. Other consequences of agriculture for landscapes in inland Flanders

One of the consequences was that the countryside was composed of a large network of very small land plots. Regional differences occurred, however, both in size and in the shape of land plots, often resulting in a kind of 'beehive-structure' with different forms of the cadastral land surveys. Only in the areas where the larger farms were situated did the average size of land plots remain larger.

Another consequence is that in Flanders the large majority of land plots were surrounded by hedges and trees. Indeed, one of the general features of almost all Flemish past regional landscapes is that, despite the absence of larger woods since about 1250,[24] Flanders was *not* a county without trees as sometimes has been mentioned in publications, although most of the former early medieval woodlands have been reclaimed before that date for the reasons summarized above. Being the main building material, wood was particularly valuable before the 19th century! It was also indispensable for cattle breeding to build fences and moreover, it was essential for heating. It was also indispensable for most manufactural activities, especially in an area where the degree of urbanization was very important.

However, especially until the 16th century, the area around the North Sea had the advantage to have access to another combustible, namely peat, which was available in large quantities in the coastal areas. Until the 14th century, it was rather easy to dig it and to ship it towards the inland areas using a network of rivers and canals. After that period, it became scarcer due to a massive amount of late medieval floods and extensive peat digging, which also caused the demand for firewood to rise again.

Coppice wood has always been important for the peasant survival system, but after the shrinking availability of peat in the later middle ages, its value even went up[25] (see figure 2).

---

24 Flanders had become an area 'with a lot of threes and a scarce amount of woods' (transl. from Latin) (State Archives Ghent, Sint-Pieters abbey, Liber Inventarius, nr. 125 (a°1281).

25 Dua, 1985 showed the increasing value of coppice wood between the 14th and 16th century.

Figure 2: Coppice wood and pollard rows in the area around Ghent and in the Waasland (number of mentions in conserved leasing contracts)

<table>
<tr><td colspan="8">Coppice wood in the area near Ghent (Oudburg) (after Picavet)</td></tr>
<tr><td>century</td><td>contracts</td><td colspan="2">willow</td><td colspan="2">alder</td><td colspan="2">oak</td></tr>
<tr><td>14th</td><td>51</td><td>30</td><td>59%</td><td>17</td><td>33%</td><td>4</td><td>8%</td></tr>
<tr><td>15th</td><td>36</td><td>17</td><td>47%</td><td>7</td><td>19%</td><td>12</td><td>33%</td></tr>
<tr><td>16th</td><td>89</td><td>37</td><td>42%</td><td>1</td><td>2%</td><td>51</td><td>47%</td></tr>
<tr><td colspan="8">Mentions of wood and timber in leasing contracts of Waasland (sandy area/after Dua 1986)</td></tr>
<tr><td>century</td><td>contracts</td><td>Hedges with coppice wood</td><td>Pollard rows</td><td>Timber trees</td><td>Tempo-rary fences</td><td colspan="2" rowspan="3"></td></tr>
<tr><td>15th</td><td>17</td><td>47%</td><td>35%</td><td>18%</td><td>12%</td></tr>
<tr><td>16th</td><td>18</td><td>89%</td><td>31%</td><td>56%</td><td>11%</td></tr>
</table>

There were also new woodlands planted in Flanders, especially from the 16th century. But the need for agricultural land did not allow this on a large scale. As a result, the amount of natural fences with hedges, coppice wood and tree lines near the borders of parcels was going up almost everywhere. The same happened with the amount of lower hedges, which were gradually mixed with pollard trees and even hedgerow trees. "Open areas" became scarce in early modern Flanders. The number of parcels fenced with (sometimes very) wide parcel borders planted with shrubs and trees or even fenced with hedges in the form of wide earth banks planted with wood also became popular in certain areas (e.g. the Ghent area). Only when coal as a new combustible was introduced for heating houses and to be used in manufactures in the course of the late 18th century did their importance dwindle. So wood was for a long time an essential element in the peasant survival system!

Village structures too were subject to the survival systems. Indeed, while in the coastal area, from the 14th century onwards, a dispersed settlement with large farms became common practice, in inland Flanders, clustering in hamlets was mostly the rule. This made sense: in a survival economy, there was a strong sense of solidarity between families and a lot of common services were made available in the villages, e.g. for health care or care for elderly people, but also for leisure. In this context, it is striking that archery had become a common leisure practice since the old regime and the Flemish 'café culture' situated in the hamlets and townships was widespread and often the only leisure option. In inland Flanders, even the scarce larger farms were often situated right in the village centers! In coastal Flanders, on the other hand, where, from the 14-15th century on, a different social agro-system had developed, living in hamlets was less common; the (increased number of) large farms that came into existence in that area from the later middle ages on were mainly situated at a distance from the village centers since the farmers preferred to live close to and even in the middle of their fields to lower the labor costs and to distinguish themselves from the small peasants who lived in the village. Moreover, most village centers in that area were shrinking from the late Middle Ages on and some even disappeared and became 'lost villages' (cf. archeology). But as mentioned, the opposite trend can be observed in sandy and sandy-loamy (inland) Flanders.[26]

Figure 3: Hedges and tree-lines north of Ghent c.1770
(source: Ferraris, Carte de Cabinet)

[26] Soens, Tys et al. 2014.

3.5. Regional landscapes and rural economy: some examples

However, a rather similar social organization did not necessarily result in identical landscapes. Some landscape features could evolve in an identical way, but some were also influenced by the regional variety of natural soils and environmental characteristics. So, landscape designing shows a large variety. This statement does not, however, prevent the possibility to elaborate a regional typology of landscape organization.

As mentioned above, one and the same social agro-system could result in different landscapes since also the geographical elements such as climate, soils and relief should be considered. Also, the date of reclamation of the area sometimes played an important part in landscape design.

• In central-sandy Flanders

Central Flanders was mainly characterized by a very light sandy upper layer. Moreover, the largest part of the area north of the river Scheldt was probably never integrated in early medieval large demesne systems.[27]

In the middle ages, this area developed towards a form of *infield-outfield* system.[28] Mainly due to a lack of capital (manure, horses …) the peasant society collaborated to work as intensively as possible only a part of the available land: these intensively cultivated micro-areas were exclusively used for grain cultivation and most of the manure was brought – manure was the gold of the former peasant! – towards these micro-areas, which were called "*kouters*" (a Dutch language term, describing a landscape similar to the open field system divided into selions in England) from the 11th century on. Most villages cultivated only one *kouter* with a size between of about 10 and 80 ha. The fields in these *kouters* were rather small in size. They had an open character, which means that the land plots on the *kouters* were not surrounded by hedges nor by any other permanent fences. This 'open character' is due to the fact that one wanted a maximum output in view of a maximum input. An additional advantage was that the fields on the kouters could be worked in an easy way without any hindering fences or hedges casting shadows. Working the *kouters* was to a certain

27 Verhulst 1995.
28 Thoen 2018.

extend also organized in a collective way (an identical crop rotation was obligatory).

Next to these *village kouters, seigneurial kouters* existed: many of them were probably older (some date back to the 9th century), but the principle was the same: dung was collected and concentrated on these micro-open fields, but in the case of *seigneurial kouters*, it was only the larger farmers who received the yields.

Originally, these kouters were surrounded by a large, more extensively cultivated 'outfield', which was less manured and cultivated in a less intensive as well as in a more individual way and with a larger freedom of cultivation.

Gradually, from the later 13th century on already, the system had lost much of its original meaning. The division between *outfield* and *infield* became more vague and became less meaningful due to more intensive cultivation. Gradually, though (sometimes) quite fast, individually managed fields – mostly surrounded by hedges and permanent enclosures – were subjected to the same intensive cultivation (in Flanders often with the spade …!) and since freedom was more important here, there was more experimentation with new techniques and new crops the result being that the yields on these fields (with completely free crop rotations) in some cases even exceeded those on the 'traditional' *kouters* …

• In sandy-loamy area in the South of the county

A similar system emerged on the more loamy soils south of Ghent. Here too a similar infield-outfield practice became the rule and originated partly in the early Middle Ages. However, the system lasted longer and the infields became bigger still between the 12-13th centuries (often habitation centers developed a system with *three kouters* for the application of a three-field crop rotation system, see figure XXX). The lands outside the *kouters*, in a similar way reclaimed towards clusters of open fields (and called *velden*), were also gradually more intensively cultivated, but the process was slower than in sandy Flanders due to soil and relief differences. Moreover, an open field system outside these *kouters* was more developed and could hold up longer because these areas were blessed with very large and rich natural meadowlands near the rivers, where cattle breeding (and manure production) could flourish; there were no large areas of woodland

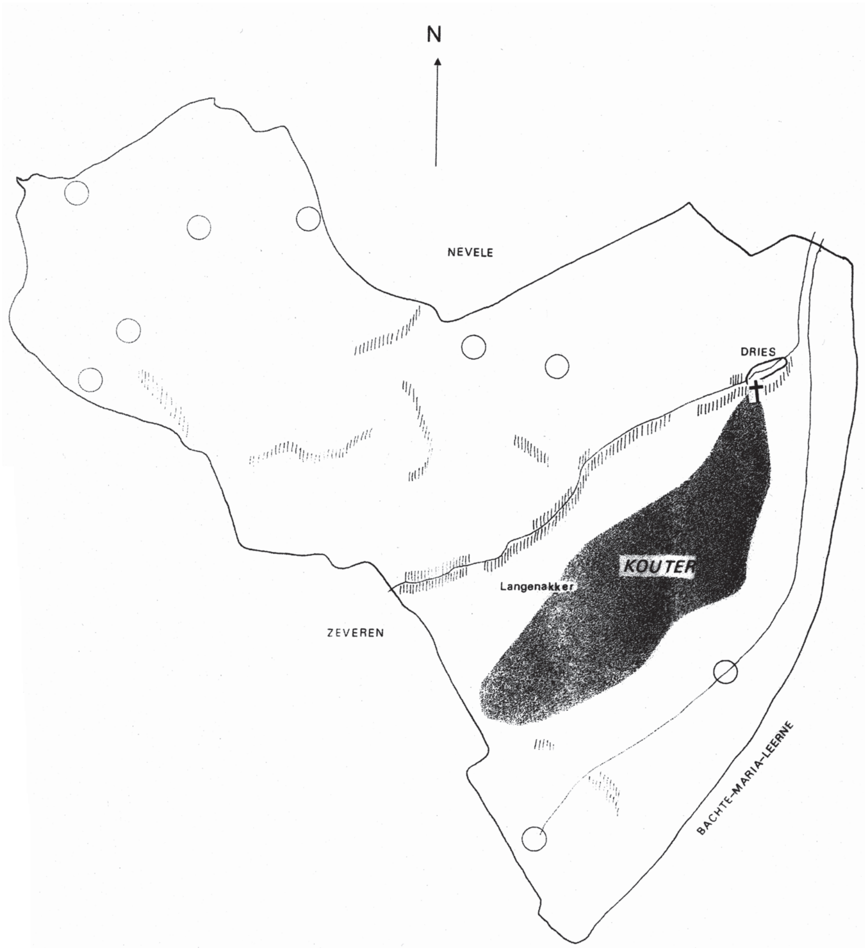


Figure 4: Reconstruction of the medieval (village-) *kouter* area (in black) in the village Meigem (near Deinze) based on a 17th century land register (figure, Thoen). The village center is situated NW of the *kouter*. It was split up since the medieval period in many smaller plots that were cultivated by the peasants.

nor (a lot of) enclosures, but instead, a large amount of smaller scattered woodlands supplied many areas with wood. However, here too regional differences were common (e.g. the Courtrai area did evolve towards an area with more enclosed fields).[29]

[29] See also Thoen 2018.

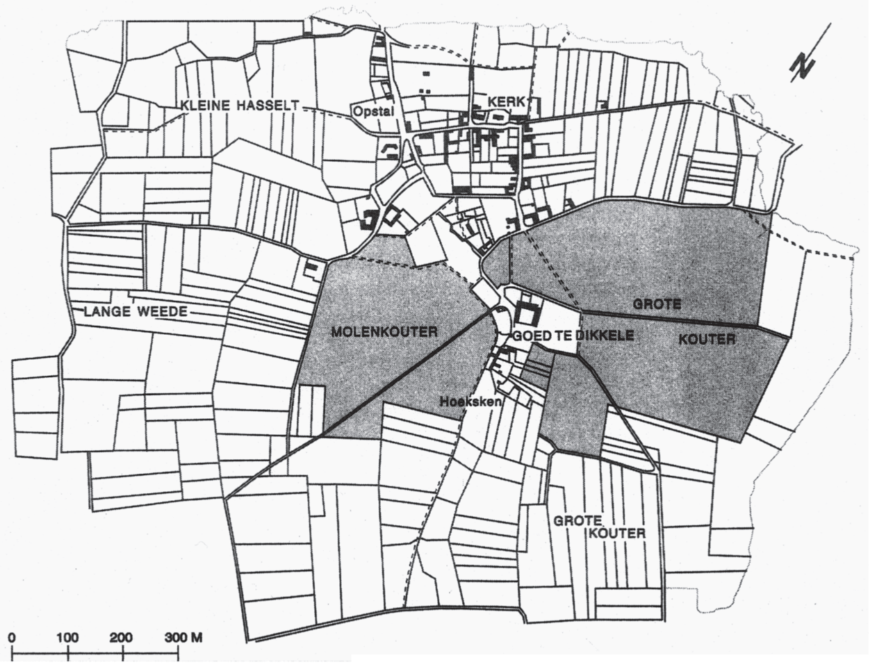


Figure 5: The medieval (hof-)*kouters* ('court'-infields) in the village Dikkele (South of Ghent) reconstructed (Thoen 2011). The largest part of these *kouters* and the farm that cultivated the largest part of them was owned by the Ghent abbey of Sint-Peters since the 10th century. Today the kouter-openfield landscape and its large land plots is still clearly visible in the current landscape.

• In the sandy part of the Waasland: 'bombed fields' (Dutch: *bolle akkers*)

In the sandy area of the Pays de Waes (Land van Waas), so-called 'bombed fields' became common ('*gebombeerde'*, '*bolle akkers'*). They consisted of a network of rather lenticular squared fields and were plowed according to a special technique to keep the typical curving shape of the fields. Contrary to the rest of Flanders, the land was not cultivated according to a raised bed system (for drainage).[30] In these areas, every field plot was also enclosed by deep fosses and tree rows, often pillows. This system had the same draining function as the high-backed ridges elsewhere in Flanders. The shape of the fields was supposed to be sufficient for drainage. Arche-

30 Snacken 1971; Van Aelbroeck 1823.

ologically, some are dated to the 16th century.[31] Probably at least part of these 'bolle akkers' date back to the later middle ages, but that requires further verification.

The rather particular features of the field pattern in the "Land van Waas" has been said to be linked to fact that the area has been reclaimed rather late compared to the rest of Flanders. A large part of the area belonged to the so called "*Koningsforeest*,"[32] a large comital forest dating back to the early Middle Ages and especially used for hunting by the count and his court.

Moreover, the influence of abbeys and citizens was more restricted (see figure X). Indeed, contrary to elsewhere in Flanders, abbeys had erected only a limited number of large demesnes in this area.[33] This was due to the afore-mentioned power of the count of Flanders in the "Land van Waas", where he would levy high rents as a 'local' lord. Therefore, a large number of individuals and free peasants who were gradually allowed to reclaim parts of the forest in the 12-13th centuries could organize their landscape[34] more freely. Apparently, the forest was first divided into rather wide elongated parcels (typical of forest management), which were divided into small square fields in a later stage.

The village Zaffelare (near Ghent)

Church/ecclesiastical institutions	34% of the available acreage
Nobles and burghers of cities	30%
Rural population	36%

The village Sinaai (Land van Waas)

Church/ecclesiastical institutions	12% of the total available acreage
Nobles and burghers of cities	11%
Rural population	77%

Figure 6: Average property structures in the sandy area near Ghent compared to those in the Land van Waas ca. 1570

31 Van Hove 1997.
32 Verhulst 1995.
33 Vervaet 2009.
34 Abbeele van den 1985.

4. The coastal area: a different social organization emerged in the late Middle Ages

4.1. Over-exploitation of the coastal area ruined the original dune and peat landscape[35]

The coastal area was/is a vulnerable environment. Before 1100, only a small part was already diked. A dune barrier was still strong enough to allow only some rivers to enter the hinterland. Behind the dune walls, tidal marshes but especially a thick layer of peat mostly composed of mosses had developed at the surface. Originally mainly used for sheep breeding and wool production and fishing, the environment had gradually turned into an area where small and independent peasants also operated.[36] Agricultural activities on the tidal flats but peat digging too became increasingly popular since there was a growing demand for that product from the growing large towns of Flanders. The fragile environment suffered from over-population and overly intensive activities. Dune barriers were weakened, soils were sinking, peat layers were compressed and as a result huge floods became common: a (to a large extent) human-triggered catastrophe took place. The Belgian and Dutch coastlines were pushed backwards. Investments in protection were too expensive for the original inhabitants, who were small peasants. Due to increased flooding, the land was covered with marine mud instead of peat. This could still be used for sheep breeding (and wool for the peasants' and towns' textile production), but gradually – importing English wool became more profitable – diking of tidal marsh areas was ramped up between the 12 and 14th centuries (although a few dates back to a much earlier period). The peasants could no longer burden the high costs anymore, except with the aid of rich investors such as rich religious institutions and later on also city burghers, who gradually strengthened their grip on the area.

Due to these huge environmental changes in the coastal areas, – contrary to inland Flanders were the same structure survived into the early modern period – an entirely new social structure came into existence. Before the

[35] For an overview of the state of the art see Thoen 2013. For more details and literature see: Tys 2013, Soens 2009 and Soens, Tys and Thoen 2014.

[36] Tys 2013.

middle of the 13th century, the small peasants in these areas were relatively independent and the area seemed to be well populated. They were also rather numerous – at least much more numerous compared to later periods – as one can see in the sources. Religious institutions gradually gained influence, but in general, most peasants managed to keep their independence. Only when the above-mentioned environmental problems and changes occurred and increased (the sinking surface, the problems due to peat digging, the inundations as a consequence etc.), most peasants lost their property rights due to financial problems and expropriation (supported by the count). This was due to the fact that the environment needed huge investments: the mud flats needed to be diked and an expensive network of canals and locks was now necessary to protect the area against the sea. This could only be financed by external rich burghers and abbeys. The new heavy soils could only be cultivated by larger farmers since the area had become only livable for much larger holdings. Probably many of the former's peasant-owners of smaller holdings became wage earners on these new, larger demesnes. But wage earning in agriculture is for the most part a temporary job and linked to the seasons. Therefore and because labor had become scarce in the coastal area, labor was now to a large extent *imported* from other areas, viz. from the sandy part of Flanders, where another social structure based on small survival farming had survived and even had further developed (see above). Part-time work, especially done by younger temporarily coastward migrating male and female unmarried workers who were saving money to later take over (part of?) the parental holdings in inland Flanders, was very welcome to overcome that stage in their life cycle. This is how the two 'social agro-systems' (the one from inland Flanders and that in coastal Flanders) became 'linked' to each other to a certain extent.

4.2. The development of landscapes as a consequence of social change in coastal Flanders

The coastal area can be considered as a combination of both a 'ruined landscape' (due to overexploitation and the loss of many lands swallowed by the sea) as well as in a later stage an 'enriched landscape' (due to the new rich soil formation with clay) but always remained a very 'vulnerable landscape'.

Due to the evolution of geographical and soil changes in the area, the landscape in the coastal area had been changed completely. The original small peasants were driven out of the areas and had lost their land. Many new but (often very) large farms popped up, mostly settled far away from the village centers – in some villages more than 20 – and were founded by bourgeois families, religious institutions and noble men who had bought land and had invested in the (re-) reclamation and drainage of the area in the hope of making a lot of money in the short term with this early form of ground speculation. As mentioned, only these richer classes had enough money to invest in the expensive infrastructure works (dikes, canals locks …). Only larger horse spans (only affordable by these lager farmers) were able to cultivate the land. Moreover, larger farms were easier to lease out and to administrate and in addition gave more prestige as well for the owner as for the farmer. The owners also expected these farmers to eventually earn enough money to invest in infrastructure themselves. At first, this was not the case: due to underinvestment new inundations took place in the 14th to 16th century: since then, a large part of the Belgian coast was covered by the sea and the Western Scheldt river emerged as a 'new' river but swallowed a huge number of former villages … However, from the 15-16th centuries on, partly with the aid of the central government, what was left from the coastal became a bit safer and a new elite of rural dwellers, the large farmers, elites developed.

These new farms were partly operated on the basis of more modern capitalist principles. Many specialized, although not completely. In the Southern part of the coastal area, farms mainly specialized in dairy products (butter, cheese) and bovine meat, in the northern part (near the Scheldt river) more in cereals (wheat, barley). A partly new canal system opened these areas to the markets in the rich cities (Ghent, Bruges, Ypres and many others).

The elites (farmers) of the described (new) *agro-system* had the advantage of the coastal area being situated next to an agro-system where many poorer people were clustered in inland Flanders (as we described). As mentioned, here a stock of people was available that could work as temporary wage earners on these larger farms. This is exactly what happened and allowed these large farms to survive with relatively low labor costs. It has been shown that during the early modern period, the elites of these villages have tried to prevent wage workers from gaining a permanent home in the coastal polders to prevent the plebs from mixing with the new elite farmers.[37]

However, and despite the (voluntary) regional segregation, economically a kind of symbiosis emerged between the two main groups of Flemish social agro-systems which lasted until the 19th century, when mechanization in agriculture made extra labor on the larger farms much less pressing. A social labor crisis was imminent. The Flemish peasants had to turn to other extra labor possibilities. However, many of them had to wait until the later 19th century before that was available in the towns and especially in the mines as well as the heavy industry in Wallonia and in Northern France.

In summary: the more capitalist social structures had the following consequences for the landscapes in coastal Flanders.

- Shrinking villages between the 13-17th centuries
- A spread of large farms in the same period
- The increase of grasslands (and cattle breeding) in the South, of cereal fields in the North of the coastal area (where the subsoils were too salty for cattle, but where the soils gave high cereal yields)
- A well-developed drainage and canal infrastructure
- Larger and reshaped land plots
- ‘Open’ fields without hedges
- Large woodlands in between the areas of inland Flanders and coastal Flanders (especially for construction-wood, which was necessary to build the large farms in the coastal area). Some of them were only intensively reclaimed from the 18th century on.

5. Non-material elements shaping landscapes as well

The main concern of this article has been to look at our former landscapes from a rather ‘materialistic’ angle. This is not to say, however, that one should not pay attention to non-material elements of landscape formation such as the role of mentality, religion and customs.

Of course, these elements were important as well. The role of the catholic church and of superstition, for example, was without ~~a~~ doubt very impor-

[37] Thijs Lambrecht et al. 2018

tant for landscape structures. However, as economic historians have proven, the catholic church, to a large extent delivered the theoretical and theological support for social and economic inequality in society as we have described.[38]

Yet, the church definitely also played a part in village formation. Indeed, there was, the since the 12-13th century, increased belief in the positive effect of clustering habitation around churches to live near the house of God and to live near the graves of the deceased family members who were believed to rise from dead at God's final judgement. To a certain extent this had an impact on village formation.

Since the peasant mentality of collaboration on the open fields was also partly encouraged by the mentality of solidarity this too can partly be considered as a non-material aspect in landscape formation.[39]

The same holds for the attraction and concentration of habitation towards wealth centers. This was probably the case in the 11th-13th century, when power centers such as 'seigniorial moats' contributed to village formation in inland Flanders (seigneurial moats='castrale mottes' (Fl.) = towers on an artificial hill and surrounded by a moat, mostly used as symbols of power for the local lords; some of these castral moats in a later stage developed to larger castles). These symbols of power – typical of the western part of our continent – were centers of consumption and luxury and contributed to the development of a lot of village structures in inland Flanders.

6. A concluding remark

In this paper, it is demonstrated that studying historical landscapes is complex. Indeed, landscapes cannot be understood without insight into the functioning of the economy and mentality of the people who lived in these landscapes, nor without the study of the relations between the different social groups in a given society, even not without taking into account the political balance as well as the technical abilities of that society and also not without understanding the role of mentality and religion … In addition, one also needs to have an insight into the evolution of soils and nature …

38 J. Sánchez-Pardo et al., 2015

39 Dyer et al. 2018.

Understanding former landscapes and their evolution therefore is not an easy task. This is why understanding rural and landscape history requires a highly interdisciplinary approach, which still is, unfortunately, currently only rarely the case. This is a pity since the landscapes we are living in are the material witnesses of our past and our identity.

References

[1] Antrop, Marc and Van Eetvelde, Veerle 2017. *Landscape Perspectives. The Holistic Nature of Landscape*, Springer, 2017.

[2] Bavel, Bas van and Thoen Erik 2013. "Rural history and the environment. A survey of the relationship between property rights, social structures and sustainability of land use", in: Bas van Bavel and Erik Thoen, eds., *Rural Societies and Environments at Risk. Ecology, Property Rights and Social Organisation in Fragile Areas (Middle Ages –Twentieth Century),* COST, Rural History in Europe, 9, Turnhout, pp. 15-42.

[3] Bavel, Bas van and Auke Rijpma, 2015. "How important were formalized charity and social spending before the rise of the welfare state? A long?run analysis of selected western European cases, 1400-1850" in: The Economic History Review, 2015 (online)

[4] Blockmans, W.P. 1978. De volksvertegenwoordiging in Vlaanderen in de overgang van Middeleeuwen naar nieuwe tijden (1384-1506), Brussel.

[5] Boone, Marc 2005. "'In den beginne was het woord'. De vroege groei van 'parlementen' in de middeleeuwse vorstendommen der Nederlanden. In: Bijdragen en mededelingen betreffende de geschiedenis der Nederlanden, 120, afl. 3, pp. 362-378.

[6] Buylaert, Frederik 2010. *Eeuwen van ambitie: de adel in laatmiddeleeuws Vlaanderen*. Verhandelingen van de Koninklijke Academie voor Wetenschappen, Letteren en Schone Kunsten van België. Klasse der Letteren. 21.

[7] De Graef Pieter 2017. "A green revolution from below? A social approach to fertiliser use in eighteenth-century Flanders", in: *Continuity and Change*, 32, 3, pp. 379-410.

[8] Dejongh, Guy and Thoen Erik 1999. "Arable productivity in Flanders and the former territory of Belgium in a long-term perspective (from the Middle Ages to the end of the Ancien Régime)." in: Van Bavel, B.P.J. en Thoen, E. (eds.), *Land productivity and agro-systems in the North Sea Area. Middle Ages-20th century. Elements for comparison,* Turnhout, CORN Publication Series 2, pp. 30-65.

[9] Devos, Isabelle, Thijs Lambrecht and Richard Paping 2011. "The Low Countries, 1000-1750" in: *Rural economy and society in North-Western Europe, 500-2000: making a living: family, income and labour*. pp.157-184.

[10] Dewulf, M. 1979. "De mandenmakerij in het Waasland", in: *Koninklijke Oudheidkundige Kring van het Land van Waas,* 82, pp. 107-133.

[11] Dua, Vera, 1986. *Onderzoek naar de historische en sociaal-ekonomische achtergronden van de evolutie en variatie inzake lineaire beplantingen en aanverwante landschapselementen in het Land van Waas,* onuitgegeven doctoraatsverhandeling Universiteit Gent, Gent.

[12] Dyer, Christopher, Erik Thoen and Tom Williamson, eds. 2018. *Peasants and their fields, 700-1800: the rationale of open-field agriculture.* CORN Publication Series 16, Turnhout.

[13] Goossens, Martine 1993. *The economic development of Belgian agriculture: a regional perspective 1812-1846*, Leuven.

[14] Lambrecht, Thijs, Winter Anne and Van den Broeck, Nick 2018. "Preindustrial welfare between regional economies and local regimes: rural poor relief in Flanders around 1800", in: *Continuity and Change.* 33, 2, pp. 255-284.

[15] Lambrecht, Tys 2002. *Een grote hoeve in een klein dorp. Relaties van arbeid en pacht op het Vlaamse platteland tijdens de 18e eeuw*, Gent.

[16] Mendels, Franklin. F. 1975. "Agriculture and peasant industry in eighteenth-century Flanders.", in: W. N. Parker and E. L. Jones, eds*., European peasants and their markets. Essays in agrarian economic history,* Princeton, pp. 179-204.

[17] Ronsijn, Wouter 2014. *Commerce and the Countryside: The Rural Population's Involvement in the Commodity Market in Flanders, 1750-1910. Economy, Environment and Demography in History*. Academia Press, Gent.

[18] Sánchez-Pardo, J. and M. Shapland (eds.) 2015. *Churches and Social Power in Early Medieval Europe. Integrating Archaeological and Historical Approaches*, Turnhout.

[19] Snacken, F. 1971. "Les champs bombés du pays de Waes", in: *L'habitat et les paysages ruraux de l'Europe. Comptes rendus du symposium tenu à l'Université de Liège, du 29 juin à 5 juillet*, Luik.

[20] Soens, Tim, 2009. *De Spade in De Dijk?: Waterbeheer En Rurale Samenleving in De Vlaams Kustvlakte (1280-1580),* Historische economie en ecologie, Gent.

[21] Soens, Tim, Tys, Dries and Erik Thoen 2014. "Landscape transformation and social change in the North Sea Polders, the example of Flanders (1000-1800 AD)", *Siedlungsforschung.Archäologie-Geschichte-Geografie* 31, pp.133-160.

[22] Thoen, Erik 1988. *Landbouwekonomie en bevolking in Vlaanderen gedurende de late Middeleeuwen en het begin van de Moderne Tijden. Testregio: de kasselrijen van Oudenaarde en Aalst, (eind 13e eeuw – eerste helft 16e eeuw)*, Gent.

[23] Thoen, Erik 1990. "Technique agricole, cultures nouvelles et économie rurale en Flandre au bas moyen âge", in: Flaran 12, *Plantes et cultures nouvelles en Europe occidentale, au Moyen Age et à l'époque moderne*, 51-67.

[24] Thoen, Erik 1991. "Rechten en plichten van plattelanders als instrumenten van machtspolitieke strijd tussen adel, stedelijke burgerij en grafelijk gezag in het laat-Middeleeuwse Vlaanderen. Buitenpoorterij en mortemain-rechten ten persoonlijken titel in de kasselrijen van Aalst en Oudenaarde vooral toegepast op de periode rond 1400, in: Handelingen van het 13de Internationaal Colloquium Spa in 1986. pp. 469-490.

[25] Thoen, Erik 1998. "The birth of 'the Flemish husbandry': agricultural technology in medieval Flanders". In: Langdon J. and Astill G. (eds.), *Medieval Farming and Technology. The Impact of Agricultural Change in Northwest Europe*, Leiden, pp. 69-88.

[26] Thoen, Erik 2011. "Le paysage flamand et les structures de la société médiévale: nouvelles hypothèses sur l'origine et la fonction des 'kouters'." In: JM Yante, ed., Etablissements humains: finages et communautés rurales entre Seine et Rhin (IVe-XIIIe Siècles), pp.379-399.

[27] Thoen, Erik 2001. “A ‘commercial survival economy’ in evolution. The Flemish countryside and the transition to capitalism (Middle Ages – 19th century).” in: Hoppenbrouwers, P. and van Zanden, J.L. (eds.), *Peasants into farmers? The transformation of rural economy and society in the Low Countries (Middle Ages-19th century) in light of the Brenner-debate*, Turnhout, CORN publication Series 4, pp.102-157.

[28] Thoen, Erik 2004. ‘Social agrosystems’ as an economic concept to explain regional differences. An essay taking the former county of Flanders as an example (Middle Ages-19th century), in: van Bavel B.J.P. and Hoppenbrouwers, P. (eds.), *Landholding and land transfer in the North Sea Area (late Midle Ages – 19th century)*, Turnhout, CORN Publication series 5, pp. 47-66.

[29] Thoen, Erik and Tim Soens 2008. “The social and economic impact of central governement taxation on the Flemish countryside (end 13th-18th centuries)”. In: La fiscalità nell’economia europea secc. XIII-XVII, Fondazione Istituto Internazionale di Storia « F. Datini » Prato, Firenze University Press, 2008. pp. 957-971.

[30] Thoen, Erik 2013. “Clio defeating Neptune: a pyrrhic victory? Men and their influence on the evolution of coastal landscapes in the North Sea area, especially during the Middle Ages and the Old Régime” in: G. Borger et al., eds. Landscapes or seascapes? The history of coastal environment in the North Sea area reconsidered. In CORN Publication Series 13, pp.397-428.

[31] Thoen, Erik and Tim Soens 2015a. “The family or the farm: a Sophie’s choice? The late medieval crisis in the former county of Flanders”, in: John Drendel, J., ed., *Crisis in the later Middle Ages: beyond the Postan-Duby paradigm*. Series: The Medieval Countryside 13, Brepols Publishers, Turnhout, pp.195-224.

[32] Thoen, Erik and Tim Soens 2015b. “The Low Countries, 1000-1750” in: Erik Thoen and Tim Soens, eds., *Struggling with the environment: land use and productivity*. In Rural Economy and Society in North-western Europe, 500-2000 (RES) pp. 221-258.

[33] Thoen, Erik 2018. “Open fields, capital land labour in medieval and early modern Flanders” in: Dyer et al., 2018, pp. 163-182.

[34] Thoen, Erik, Lies Vervaet, and Wouter Ronsijn forthcoming. “Real Landscapes and Imaginary Landscapes: The Land Van Waes in the Old Regime Landscape History.” in: *Landscape History* (in press).

[35] Thoen, Erik forthcoming. “‘Social agrosystems’. A plea for a regional and comparative approach in the field of rural economic history of the Old Régime.”

[36] Tys, Dries 2013. “The medieval embankment of coastal Flanders in context”, in: Thoen, Erik; Borger Guus; de Kraker Adriaan: Soens Tim; Tys Dries; Vervaet Lies and Weerts Henk, eds., *Landscapes or seascapes,* Turnhout, pp. 199-239.

[37] Van Aelbroeck, J.L.1823. *Landbouwkonst der Vlamingen*, Gent.

[38] Van den Abbeele, H. 1985. De penningkohieren als sociaal-ekonomische en demografische bron. Het Land van Waas omstreeks 1571. Unpublished master thesis, Gent University.

[39] Van Hove, Rudy 1997. “De “klassieke” bolle akkers van het Waasland in archeologisch perspectief”, in: *Annalen van de Koninklijke Oudheidkundige Kring van het Land van Waas,* pp. 283-328.

[40] Vandenbroeke, Chris 1977-1983. "Landbouw in de Zuidelijke Nederlanden 1650-1815", in: *Algemene Geschiedenis der Nederlanden*, VIII, Haarlem, Fibula-Van Dishoeck, 1977-1983, pp. 73-101.

[41] Vanhaute, Eric, and Guy Dejongh. 1999. "Arable Productivity in Belgian Agriculture, C. 1800-c. 1950", in: Land Productivity and Agro-systems in the North Sea Area (Middle Ages-20th Century): Elements for Comparison, ed. Erik Thoen and Bas JP van Bavel, pp. 65-84. Turnhout.

[42] Vanhaute, Eric 2001. "Rich Agriculture and Poor Farmers: Land, Landlords and Farmers in Flanders, 18th and 19th Centuries." In: Rural History : Economy, Society, Culture 1, pp. 19-40.

[43] Vanhaute, Eric and Lambrecht,Thijs 2011. "Famine, exchange networks and the village community. A comparative analysis of the subsistence crises of the 1740s and the 1840s in Flanders", in: *Continuity and Change 26 (2*), pp. 155-186.

[44] Verhulst, Adriaan 1990. *Précis d'histoire rurale de la Belgique*, Editions de l'Université de Bruxelles, Brussel.

[45] Verhulst, Adriaan 1995. *Landschap en landbouw in middeleeuws Vlaanderen*, Brussel.

[46] Vervaet, Lies 2009. "De sociaal-economische organisatie in een Wase plattelandsgemeenschap en de relatie met het landschap: Sinaai in de 17^{e} eeuw", in: *Annalen van de Koninklijke Oudheidkundige Kring van het Land van Waas,* CXII, pp. 19-106.

[47] Vervaet, Lies 2012."Agrarisch woon-werkverkeer in zeventiende-eeuws Sinaai: de ruimtelijke organisatie van landerijen op het vroegmoderne Vlaamse platteland", in: *Tijdschrift voor sociale en economische geschiedenis*, 9, 3, pp. 2-26.

[48] Warlop, Ernest 1975. *The Flemish nobility before 1300*, Kortrijk.

[49] Weston, Richard 1950 (1644-1645). "A discourse of Husbandrie used in Brabant and Flanders, shewing as a pattern for our practice in this Common-wealth", London, Translation published with an introduction by P. Lindemans as *'Verhandeling over de landbouw in Vlaanderen en Brabant 1644-1645'* Brugge, 1950.

Laudatio Roland Renson

Matthieu Lenoir

Roland Renson was born in Sint-Truiden (Belgium) during the second World War. His parents had a bicycle store, so the seeds of his interest in sport might have been present from the very beginning of his life. The combination of sport with his choice to study Greek and Latin during his secondary school period further paved the way for his professional career. The next logical step was to study Physical Education at the Catholic University of Leuven. After obtaining his Master in PE (a 'licentiate' at that time), he continued with a Master in Physiotherapy, and a Master in Social and Cultural Anthropology at the same university. With this threefold proof of his capabilities, he entered the academic world, in which he would develop himself in three main areas of research and service to the community.

As a young researcher he joined the team of the Leuven Growth Study of Belgian Boys (1968) and Flemish Girls (1979), a research project that would become a standard in the area of physical development from preadolescence until far into adulthood. These projects largely expanded our knowledge on the short-term development of boys and girls before and during puberty, as well as the long-term relationship of developmental characteristics with aspects of growth, physical activity, and health at adult age.

During his own PhD research, Roland Renson charted the social stratification of sport participation in Flanders. Since 1969, the 'popularity' of each sport and the socioprofessional characteristics of its participants has been reflected in the well-known stratification pyramids, drawn by Roland himself. Despite the apparent simplicity of this drawing, this research and the updates of the piramid that would follow at a pace of one per decade, have helped to develop the governmental policy for sports in Flanders in the past 50 years.

Roland Renson however earned most respect with his work for the preservation of the Flemish cultural heritage concerning sport and pastime activities. The painting of Pieter Brueghel the Elder depicts about 80 folk games and activities that were popular in Flanders in the 16th century. Much of these traditional activities and the knowledge on it were threatened to be forgotten in the course of the 20th century. Roland has committed himself during more than 50 years for his cause. Apart from numerous publications in which he connected developments in sports and leisure to political, social, and scientific trends that emerged in a society, he has passionately served as a guardian of this heritage in Flanders. This passion has resulted in the magnificent 'Sportimonium' in Hofstade (https://www.sportimonium.be/en), a museum for sports and the (Belgian) Olympic history. The first piece in the collection of the Sportimonium was the famous 'skittle from Schulen'. After this start in the early 1970's, Roland Renson started a true crusade for funding to make this unique project come true. Colleagues who participated in the board of directors of the Sportimonium have witnessed the (financial) obstacles that had to be crossed to preserve this marvellous collection of artefacts and knowledge at Hofstade. But step by step the Sportimonium and its reputation grew, and the ultimate recognition came in 2011 when it was awarded the title of museum of the International Olympic Committee, an honour that has been granted to a few musea worldwide only. Flanders definitely owes Roland Renson for this achievement.

Roland Renson was president (and is now honorary president) of the International Society for the History of Physical Education and Sport. His encyclopaedic knowledge on the research area has made him a frequently solicited speaker at international conferences, and he has spent many periods as a research fellow or guest professor at well-known institutes and universities worldwide, like the University René Descartes (Paris V – Sorbonne (FRA)), Queensland University of Technology (Brisbane, AUS), University of Otago (Dunedin, NZL), and University of Western Ontario – London (CAN) to name a few. In 2002 he was granted the Sport Science

Award for Social Sciences of the IOC President in recognition of his scientific work in the field of sport and physical education and of outstanding sport science accomplishments. In 2008 Roland Renson was granted the title of 'Doctor Honoris Causa' on behalf of the Ghent University, at the occasion of the centenary of the Physical Education and Movement Science program at UGent. He received the accompanying honorary decorations from Prof. Paul Van Cauwenberge, pro-rector of the Ghent University, and current president of the Sportimonium. In his lecture at the occasion of the award ceremony of the Sarton medal, Roland Renson gives proof of his encyclopaedic knowledge on the history of sports, and in casu on the enigma of the ancient Greek long jump. He not only provides an overview of 2500 years of discussion on how Greek athletes managed to perform long jumps of more than fifteen meters, using hand-held weights, but also how authors on this topic have influenced the development of ideas on the topic in a way no one else can. I take the freedom to add my own personal interim conclusion on the enigma, which I hope will underline that we have not come to a final conclusion yet. Building upon the concluding paragraphs of Roland Rensons' lecture, two groups of 'solutions' can be discerned. Either the halma was a long jump preceded by a run-up, or a multiple jump. If we consider that all in all, there is relatively little criticism on the 15-meters performances of different ancient athletes, the limits of human performance should be considered too. Performing a 15-meters long jump with halteres, after a run-up as we know it from modern long jumping, is simply impossible in a single jump, and would, in the unlikely event that an athlete would ever be capable of doing so, result in serious or even fatal injury. From this point of view, the possibility of a multiple jump, either preceded by some form of short run-up or not, cannot be excluded yet. The current collection of pictorial and written testimonials on the ancient Greek long jump are, as is also evident for Roland Rensons' overview, inevitably subject to interpretation and discussion, and as such will remain 'circumstantial evidence' rather than solid proof in favour of one or another hypothesis. I look forward to the discovery of some unknown artefact or written sources that might help to unravel the enigma of the ancient Greek long jump.

The Enigma of the Halma

An Attempt to Unravel the Technique of the Ancient Greek Long Jump

Roland Renson

Greek philologists, archaeologists, sport historians, as well as sport biomechanists, have tried to unravel the enigma of the *halma* or ancient Greek long jump. The *halma*, from the verb *hallomai* (to jump), was practised with dumbbells or *halteres*, ranging from 1.5 to 2.5 kg, in each hand. The *halma* was a quintessential part of the *pentathlon*, from *pente* (five) and *athlon* (prize), a combined contest of five athletic disciplines: discus throw, long jump, javelin throw, stadion run and wrestling. Athletes where thus 'prize contesters' and these prizes could be highly symbolic like olive wreaths in Olympia or laurel wreaths in Delphi, but also valuable material and financial rewards as well as honorific statues or inscriptions and honorary functions in the athlete's home city.

The *halma* has raised many controversial questions about its technique because the athletes jumped with *halteres* in their hands while leaping spectacular distances. Through a review of the ancient primary sources, which are rather scarce, a first 'status questionis' will be made. Then a 'hop-step' will be made to the Renaissance period and then to modern secondary sources, which appear from the beginning of the 19th century, before 'jumping to conclusions'. Unfortunately, an in-depth research on the iconography of ancient Greek vase paintings, could not be included in this publication because of the multitude of images. However, these images were shown and amply discussed during my Sarton Lecture on "*The* *enigma of the halma: an iconographic approach to unravel the technique of the ancient Greek long jump*." (Renson 6 May 2019, Sportimonium, Zemst). Biomechanical studies concerning the technique of the ancient Greek long jump will also be discussed and a synthesis will be presented of the most plausible explanation of the *halma* enigma.

Images from Roman mosaics and Etruscan frescos were not included in this analysis.

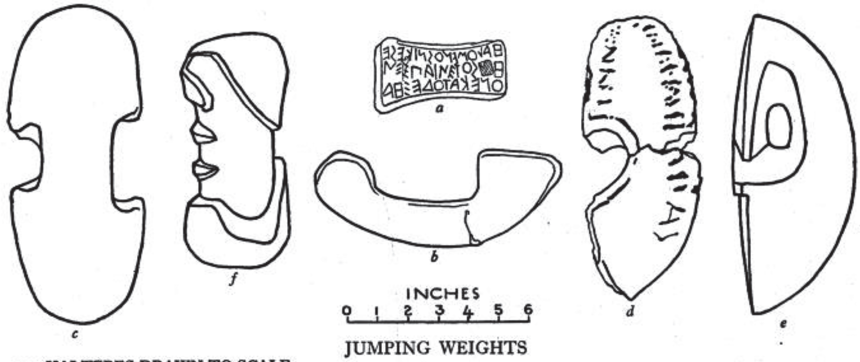
Ad Fontes: Ancient Sources on the Halma

In the chapter Nemea 5 of his *Epinikea*, Pindaros (522-443 BC) cites "So prepare me the ground for a jump, which will carry me far from here; agile is the momentum of my knees." (Translation A. Puech) (*Van Hove 1993a: 94, Pfeijffer 1999: 130)*. The ground for the jump was the so called *skamma* or landing area.

Well known is the epigram dating from the 5th century about the famous Phayllos from Kroton (Southern Italy), who won three victories in the Pythian Games in Delphi, where he was honoured with a statue. He once jumped 50 + 5 feet (ca 16.3 m) and threw the discus 100 – 5 feet (24.7 m). Thus he landed outside the landing pit and – according to some sources – broke his leg (Harris 1960: 3).

The famous philosopher Aristoteles (384-322 BC), founder of the so called Peripathetic School, wrote in his *Problemata* (V 8): "One jumps better, he who has halteres, as the one who does not have them." (Saurbier 1955 (1969: 23)). The importance of the *halteres* is stressed here and images of the *halma* with such dumbbells in each hand are omnipresent. *Halteres* weighed between 1.5 and 2.5 kg and were made of stone, lead or bronze and varied widely in shape (**See figure 1**).

An *halter* made of lead, weighing 2.199 kg, carried the inscription "Epainetos won the jump and therefore [dedicated] these halteres." (Sweet 1987: 47). The Spartan pentathlete Akmatidis, after his Olympic victory at the end of the 6th century BC, donated a heavy stone *halter* to the Olympic sanctuary, where it was found. This 'ex voto' *halter* weighs about 4.6 kg and mentions that Akmatides won the pentathlon "*akoniti*" (Sweet 1987: 82; Zarnowski 2013: 90-91).



100. HALTERES DRAWN TO SCALE.

a. Leaden halter found at Eleusis. Athens, National Museum, 9075. 4½ × 1½ in., weight 4 lb. 2 oz. 6th century. Inscription: *'Αλλόμενος νίκησεν 'Επαίνετος οὕνεκα τοῦδε*: ά, 'Epainetos by means of this won the jump'.

b. Leaden halter, one of a pair. British Museum. The type is that usually depicted on fifth-century vases. Length 8½, width at grip 1½ in. Much worn, about 2 lb. 3 oz. A similar pair found at Athens are shorter but weigh 3¼ and 3½ lb.

c. Stone halter found at Olympia, 11½ in. long. Weight more than 10 lb. (4.629 kg.). Olympia.

d. Marble halter found at Sparta. 9 in. long. About 3 lb. Inscription: *τᾶι 'Αθαναίαι Παιτιάδας*, 'Paitiadas to Athene'. 5th century. *B.S.A.* xxvii, p. 251.

e. Stone halter. One of a pair found at Corinth. Athens, National Museum. 10½ × 4 × 3 in. About 4½ lb. *c, d, e* are of the type commonly shown on sixth-century vases.

f. Stone halter found at Rhodes. British Museum. 7½ in. long. Nearly 5 lb. This is the type represented in later art, e. g. Fig. 70.

For *halteres* see *J.H.S.* xxiv, p. 181; *Gk. Athletics*, p. 298; Jüthner, *Ant. Turn.* p. 3. The cylindrical halteres are fully discussed by Jüthner in *Röm. Mitt.* xliii, p. 13. He conclusively disproves the conjecture of Bruno Schröder that they were a sort of weapon used by boxers.

Figure 1: Halteres drawn to scale (Gardiner (1930); 1980: 140)

This means that he was proclaimed as victor before the last of the five events, namely the wrestling in the sandpit, and thus finished "without any dust on him". He had thus either already won three of the foregoing events or he had no opponent or his opponent accepted defeat instead of competing against him in wrestling (Jüthner 1941).

Quintilianus (ca 35-100 AD) writes in *De institutione oratoria* (10.3.6): "This we see to happen in the jumping contest, the competitors seek a longer attempt and bring themselves in a run to the jumping pit; similarly, in throwing the javelin, we draw back our arms, and in archery pull back the bow-string to propel the shaft." (Grodde 1997: 43; Lee 2003: 156). Here the run up to the take-off point of the *halma* is explicitly mentioned. The take-off was made from the *bater* and the jumpers landed in what was called the *skamma*, a temporarily dug up area. Seleukos (1st Cent AD) mentions "Bater is the beginning of the skamma" and Symmachos (2nd or 3rd Cent AD) states that "Bater is the middle of the jump" (Schröder 1927: 114; Jüthner & Brein 1968: 205; Sweet 1987; 46). These sources confirm that the *halma* was a run-up jump.

The well-known travel writer Pausanias (ca 115-180 AD), wrote in his *Description of Greece* (*Ellados periegesis:* Elis V, 26, 3) (173 AD): "Among the consecrations of Mikithos, there is *Agôn* carrying halteres … they are made in such a way that the fingers of the hand can pass through, like through the straps of a shield. "(Badinou s.d.: 231). This description of the *halteres* is a noteworthy statement because it refers to the military function of the *halma*. Wolfgang Decker (1995: 93) has stressed that the *halma* originated from military training and that the halters functioned as 'rudiments' of the Hoplite's shield and spear "… with this armament he had to be able to keep his firm position, even in uneven terrain." (p. 98). Also Judith Swaddling (2008: 69) writes that every event was originally intended as training for warfare and "… the long-jump might have been useful for crossing obstacles like a ravine or stream." (p. 69). She also mentions the possibility that "… weights were used not to facilitate the jump at all but to deliberately make the event harder and more physically demanding …" (Swaddling 2008: 70).

Pausanias also mentioned the 55 Delphic feet jumped by Phayllos of Kroton." (Mezö 1930: 112), but that famous jump of Phayllos (16.31 m) was even 'surpassed' by Chionis of Sparta. Sextus Julius Africanus (ca 160-240 AD) praised Chionis for his jump of 52 Olympic feet (16.66 m) (Mezö 1930: 111). Chionis was listed by Eusebius of Caesarea (ca 260-340) as three times victor of the stadion race in Olympia in 664, 660 and 656 BC.

The physical demands of the *halma*, were also paraphrased by Artidoros of Daldis (ca 135-200 AD) in his *Dreambook* (*Oneirokritika* I.57): "… often on the other hand the pentathlon means also sorrows and worries because of the leaps one makes in the jump with the halteres: for about people who complain about the sudden misfortunes which happen to them, we say that they leap from grief; …" (Badinou s.d.: 228).

A significant observation was made by Julius Pollux (2nd century AD) in his *Onomastikon* (Language *t*hesaurus*s*): "The place from which the jump is made is the *bater*, whence the expression, 'He has hit the *bater* with a bang.'" (III, 151) "… *bater*, but to some the *balbis*." (III, 147) (Van Hove e.a. 1992a & b: 107; Lee 2007: 158). Hugh Lee has therefore pointed out that "The *balbis* also seems to have been the *bater*, the take-off point, for the jump, as well as the foot marker used by javelin and discus throwers." (Lee 2007: 158).

Phlabios Philostratos (ca 170-247 AD) wrote several clarifying details on the halma in his *Gymnastikos* (ca 219 AD). "… the pentathlon comes from the union of the two categories: wrestling and discus throwing are the heavy events, whereas javelin throwing, the jump and the run are the light events. Before Jason and Peleus there was a separate prize for the jump and another one for the discus throw …" (*Gym* 3 in Jüthner 1909: 137; Badinou s.d.: 229). Philostratos further stated that "The pentathlete should be heavy rather than light, and light rather than heavy. He should be tall, well built, with good carriage, and with musculature which is neither superfluous nor inadequate; His legs should be long rather than strictly proportionate, and his hips should be flexible and limber for the backward bending of throwing the *akon* and the *diskos* and for the *halma*. He will jump with less pain and less likelihood of breaking something in his body if he can land softly by letting his hips down gradually." (*Gym* 31 in Jüthner 1909: 159; Badinou s.d.: 228; English transl. Miller 1991/2004: 39). The role of the *halteres* and the assistance of the *auletes* (double-flute player) were stressed as follows: "The halter is a discovery of the pentathletes which was invented for use in the halma from which it gets its name. The rules regard jumping as the most difficult of the competitions, and they allow the jumper to be given advantages in rhythm by the use of the flute, and in weight using the halter. This is a sure guide for the hands and leads to a clear and firm landing on the ground. The rules show the value of this point, for they do not allow the jump to be measured unless the footprints are perfect." (*Gym* 55 in Jüthner 1909: 181; Robinson 1927 (1955); Badinou s.d.: 231; English transl. Miller 1991: 39).

A rather cryptic remark was made by Themistios (317-390 AD) in his *Notes on Aristotle's Physics* 5.3: "For those jumping in the pentathlon do not make a continuous movement, because they interrupt part of the interval in which they are moving." (Sweet 1987: 50). Could this mean the moment of the take-off, situated between the run-up and the jump? Adepts of the 'multiple jump' thesis of the *halma*, have 'jumped' on this passage as proof of their thesis (Hupperts e.a. 1989: 70). Sweet, who cited this remark by Themistios, commented: "… However, there is no solid ancient evidence, either literary or pictorial, to support the theory of the multiple jump …" (Sweet 1987: 50).

After hopping through the ancient sources, let us now make a step to the

Renaissance authors before jumping to the modern scholars of ancient Greek athletics.

The Renaissance of the Interest for Ancient Greek Athletics

The first publication which can be seen as a 'renaissance' of the interest for ancient Greek athletics, is *De arte gymnastica* (1569) by Hieronymus Mercurialis (1530-1606), an Italian physician and philosopher. Mercurialis distinguished three types of gymnastics: military, athletic and medical and he described the gymnastic program in the ancient gymnasiums. Whereas Plato saw no utility of the *halma* for warfare, Vegetius on the contrary mentioned that the young soldiers were militarily trained so that they could 'sine labore' jump over a ditch or over a vertical obstacle (ed. 1601: p.118). Mercurialis further cited Aristoteles, Theophrastos and Pindaros, and commented the following terms connected with the *halma*: *halteres, bater, kanoon* (measuring device), *skamma* and *eskammena* (end of skamma) (ed. 1601: p. 119). The British sport historian Peter McIntosh (1984) has dedicated an interesting overview of Mercurialis's magnum opus.

Another important 'revival' of the interest for ancient athletics was the publication of *Agonisticon sive de re athletica* ... (1592) by Pierre du Faur (ca 1532-1600), alias Petrus Faber (Spivey 2005: 240). The pentathlon receives little attention in this extensive overview of ancient Greek athletics. Faber points out that not the pankratiasts, who combined the two athletic skills of wrestling and boxing, excelled in their athletic versatility, but the pentathletes (1595: 195-203). The long jump is, however, not focused upon in this extensive treatise.

It is quite remarkable that the publications on ancient Greek athletics by another French author, Pierre-Jean Burette (1665-1747), rather recently 'discovered' by Richard Waller (2008), has remained so far 'unnoticed' in the sport-historical literature. In his 'éloge' as passed away member of the Royal Academy of Inscriptions and Belles Lettres (Eloge 1754), the Parisian medical doctor Burette with a great passion for ancient physical exercises and ancient music, was praised for the dissertations he had written on several aspects of these topics. One of these was his *Dissertation sur ce qu'on nommait Pentathle dans l'ancienne gymnastique* (Burette 1717). According to Aristoteles, these pentathletes were skilled in five different kinds of 'combats' (p. 442). Burette cites a verse of Simonides [ca 556-468 BC], in which he lists the five exercises: jump, run, discus, javelin and wrestling (p. 443). He further cites a passage by Arrianus [2nd Cent. AD] on his former master Epictetus, mentioning that "… he who is successful in the pentathlon gains no honour in wrestling …". Burette remarks that – although wrestling belonged to the pentathlon – "The ability in the three exercises of the Jump, the Discus and the Javelin, was the major capital of the athletes, who dedicated themselves to the Pentathlon." Burette quotes Pausanias, who testified that the runners and the pentathletes shared the same premises in the gymnasium in Olympia, but he also remarks that – although these athletes also share the same physical qualities – it was very exceptional that an athlete won both the foot-race and the pentathlon in the same Games. That was why Xenophon of Corinth was highly praised by Pindaros when he had won both events [79th Olympiad 464 BC], which had never happened before. Burette further criticizes his compatriot and predecessor Pierre du Faur, alias Petrus Faber, who in his *Agonisticon* made several unhappy conjectures "… despite his exceptional erudition." (p. 450). He further quotes Sophocles [496-406 BC], who stated that the five events of the pentathlon were contested on the very same day (pp. 551-552). Strangely enough no mention is made in this rather exceptional dissertation on the technique of the long jump nor of the use of *halteres* or the musical accompaniment by the *auletes*.

The remarkable publication by Theodorus Antonides (1647-1715) *Olympia dat is Olymp-Speelen der Grieken* (1732) [Olympia that is Olymp-Games of the Greeks], edited by his son Meinart in 1732, has also received little attention, most probably because it was written in Dutch (De Witte 2010). This minister of the Reformed Protestant Church describes the major characteristics of the *halma*: *halteres – bater – skamma – eskammena – kanoon* (pp. 294-299) and mentions that "…some jumped with empty hands … others with heavy weights …" (p.295). Antonides cites Aristoteles description of the jump with *halters*, but his Dutch translation is rather confusing: "… in which one runs and swings them with the hands. While the one who has halteres runs [leaps] further as the one who does not have them, subsequently he who swings them runs [leaps] faster, than he who does not." (literally translated p. 296). He also cites Mercurialis, who spoke of the use of *halteres* for health reasons, but also that others used them to gain a certain idle glory: "Those who jumped longer and higher with heavier weights, were credited with higher prizes and honours." (p. 299).

Probably unaware of Antonides' *Olympia* (1732), Martha Zebrowski (2012) considers Gilbert West's *Dissertation on the Olympick Games* (1749) as "… the first modern history of the ancient games" (p. 239). The Englishman West (1703-1756) found his sources on the ancient games in the publications of two Frenchmen: *Agonisticon sive de re athletica* (1592) by Pierre du Faur (alias Petrus Faber) and the diverse dissertations published by Pierre-Jean Burette (1665-1747), but he was less interested in the athletic events (text) than in their political significance (context). For him, the ancient Olympic Games "… were a school for both fitness and virtue, prize fighters and jockeys trained in order to be citizens and soldiers, and exercise and competition were patriotic acts." (Zebrowski 2012: 245). Although West mentioned the *pentathlon*, the *halma* was but a small detail in a much larger Olympic political ideology, "… which a wise and prudent British statesman should read … and establish sport upon great political views." (Cit. In Zebrowski 2012: 246).

Greek Classical Philologists and Sport Historians Discuss the Ancient Halma Technique

A renewed interest in ancient Greek athletics was stimulated in Germany by the founding fathers of German gymnastics Gutsmuths and Jahn. When Johann Christoph Friedrich GutsMuths (1759-1839), author of *Gymnastics for youth* (1793), was appointed at the Philantropin School in Schnepfenthal, he became familiar with the so called *Dessauer Pentathlon* which comprised running, jumping, throwing, balancing and carrying exercises. Friedrich Ludwig Jahn (1778-1852) oriented the new physical education trend into the German nationalist Turner movement. After the German archaeologist and historian Ernst Curtius (1814-1896) had travelled in Greece between 1836 and 1840, he obtained in 1874 the exclusive agreement with the Greek government to excavate the site of Olympia. All this stimulated the interest for ancient Greek physical culture in his home country.

Johann Heinrich Krause published in 1838 (1972) his *Olympia oder Darstellung der grossen Olympischen Spiele*, in which he cites the exceptional jump of Chionis of Sparta and mentions the *halteres*, the *bater* and *skamma* as essential components of the *halma* (1972: 261). In his later publication *Die Gymnastik und Agonistik der Hellenen* (1841; 1971) Krause mentions Phayllos 55 foot jump and adds other elements of the *halma* such as *eskammena*, *kanoon*, *auletes* and *skapheion* (pickaxe for preparing the skamma) (1971: 383).

Julius Bintz (1843-1891) mentions in his 1878 publication *Die Gymnastik der Hellenen* (Reprint 1973: 39-46; 65-70) a run up to the *bater* and the *skamma* with *haltere*s, the role of the *auletes* and the spectacular 55 foot jump of Phayllos. He stresses the landing on two feet and points out that the *halma* was not a hop-step-jump nor a (Greek traditional) triple jump. He mentions that a German officer jumped 23 feet with *halteres* in uniform and without much training (1971:43) and reminds us that "The Hellenic antiquity, which was used to quite different physical performances, …" (1973: 42) and "… a performance which stood unequalled for centuries among a people, which practised physical exercises like no other one later; …" (1973: 43).

The German philologist and Turner Karl Wassmannsdorf (1821-1906), on the contrary, explains in *Monatschrift für das Turnwesen* (1885) the distance jumped in the *halma* as a triple jump, consisting of two steps ('Sprungschritte') and a jump (p. 270, cit. in Gardiner 1904: 74).

The Czech-Austrian classical philologist and archaeologist Julius Jüthner (1866-1945) has made some of the most valuable contributions to our knowledge of ancient Greek athletics. In his 1909 publication *Philostratos über Gymnastik* (reedited in 1969) he quotes: "(The pentathlete) should rather have long than proportial legs and mobile loins … for he will jump painless and not break any body parts when he, lowering his hips slowly, ends in a firm position." (p. 159). He further cites Philostratos, who saw "War as preparation for gymnastics and gymnastics as preparation for war …" (p. 171) and who stated that "The halter is an invention of the pentathlete …" (p. 181). Friedrich Brein edited in 1965 and 1968 two volumes of Jüthner's earlier publications under the title *Die athletischen Leibesübungen der Griechen.* In his description of the *halma* (1968: 159-221) Jüthner speaks of a small spreading start position, the arms forward, *halteres* in the hands, a run up to the *bater* (according to Seleukos for take-off, according to Symmachos 'in the middle'), the *skamma* as landing pit and *eskammena* as end, the *kanoon* not as a rod but as the standard length of the jump, the musical assistance by the *auletes*, the jump as a triple jump (either hop-step-jump or hop-hop-jump), landing on two feet, the *halteres* swung back but retained, the marks (*symeia* or *symata*), the *skapheion* (pickaxe) and – of course – the two 'record jumps' by Phayllos of 55 Delphic feet (16,28 m) and Chionis of 52 Olympic feet (16,66 m). Moreover, the 1965 and 1968 (re)editions of Jüthner's 'oeuvre' by Brein, contain a large overview of Greek vase paintings, depicting crucial scenes of the complicated Greek long jump, which make this publication a 'sine qua non'.

Also the modern sport movement in Great Britain, which originated in the elite Public Schools and fostered the ideal of amateurism, tended to 'project' their amateur sport ethos to ancient Greek athletic 'cultus'. Percy Gardner (1846-1937), a classical archaeologist who is considered the founding father of the British ancient Greek sport historians, wrote in 1880 an article 'The pentathlon of the Greeks' in the *Journal of Hellenic Studies* (1: 210-223). He made the strange statement that: "The leap would appear from the numerous representations which we possess of it on ancient monuments to have been taken standing." (sic). This is moreover contradicted by the image shown in his article (Plate VIII) of four pentathletes with the jumper in a run up or take-off position with legs spread forwards and the arms with halteres swung upwards. He further cites the expression "to jump over the eskammena", which he interpreted – referring to Pindar – as the marks drawn with a fork to mark the distance of the jumps …" (p. 213). Both David Young (1984) and Donald Kyle (1990: 24) have seriously criticized Percy Gardner and his 'follower' E. Norman Gardiner, who claimed that early Greek athletes were all idealistic noble amateurs and that athletics degenerated with professionalism in the fifth century, which was nothing else than a vision "… seen through the gentleman amateur's eye".

The British sport historian E. Norman Gardiner (1864-1930), who was generally considered as the leading authority on Greek athletics in the Anglophone world, published in 1904 an article on 'Phayllus and his record jump' in the *Journal of Hellenic Studies* (24: 70-80). He describes the function of the *halteres*, the *bater*, the fact that the *skamma* and *eskammena* were the same landing pit, the pegs used as markers of previous jumps and the *kanoon* as measuring rod. He stresses that there was a short run-up: "… the Greek jumper … takes a few short, springy steps …" and that it was "…

Figure 2: An exceptional 'movement analysis' of the *halma*. This scene depicts an athlete during his run-up, swinging his arms backwards and forwards (three images from left to right), then swinging both arms upwards during his take-off on the *bater*. It is unclear why he looks backwards on the first two images. This looking backwards during the run-up appears also on some other vase paintings. (Attic red figure kylix ca 510-500 BC attributed to the Nikosthenes painter; Los Angeles (Ca): Paul Getty Trust).

not possible that the Greek long jump was a series of three jumps … The various attempts to explain such a jump are unsupported by any evidence." (**See figure 2**). He repeated the same vision in 1925 in his well-known *Olympia: Its history and remains* 1925 (1973: 304-305). In *Athletics of the ancient world* 1930 (1980: 144-153), Gardiner further explained that there was a one-foot take-off and that a regular impress of the feet was required at the landing. He mentioned the role of the *auletes* and commented that a jump of more than 50 feet was: "Either … not a single jump or the record is pure fiction" (p. 152). Strangely enough, he accepted the existence of a 'standing long jump without weights' (pp. 145; 151) as depicted on an Attic red figure *krater* and an Attic red figure *pelike* [two types of vases]. The latter picture which was later also shown by Finley and Pleket (1976: 90) although it does not represent the starting position of a jump but of the stadion run, "errare humanum est". Donald Kyle commented "For decades Gardiner was authoritative and his followers continued in the same vein. Since the mid-1970s Gardiner has been challenged more and more, especially about his comparison of the so-called 'decline' of ancient athletics and the waning of the amateur ethos in modern sport …" (Kyle 1991: 28).

Bruno Schröder stated in *Der Sport im Altertum* (1927) that there exists no artistic nor literary proof of a standing broad jump and that there was an upright start position. He quoted Phayllos' 55 feet jump as "Spielerei" (pure phantasy) from a "Verseschmied" (versifier).

His compatriot Franz Mezö, however, in his *Geschichte der Olympische Spiele* (1930) mentioned both standing and running jumps in which the arms were swung backwards for steady landing and he explained the Phayllos and Chionis jumps as the sum of three jumps.

Also the American philologist Walter Woodburne Hyde (1938) was of the opinion that – taking into consideration the 8.06 m jump "… by the American negro (sic) Jesse Owens at the 11th Olympics, Berlin, 1936." – single jumps of 50 or 52 feet are manifestly impossible and that "… the pentathlum jump was a multiple one."

The French historian Henri Irénée Marrou, in his *Histoire de l'éducation dans l'Antiquité* (1948; 1964) described the *halma* as a long jump with a short run-up.

Edgard Pouelmans, in his remarkable licentiate thesis on the *pentathlon* presented at the University of Leuven in 1948, distinguished (in the Gardiner tradition) a standing broad jump without *halteres* from a long jump with run up and *halteres* and found no evidence for a triple jump.

Bruno Saurbier in his *Geschichte der Leibesübungen* (1955; 1969: 22-23), considered – like Mezö (1930) –, the *halma* as three one legged jumps. Whereas Ulrich Popplow in *Leibesübungen und Leibeserziehung in der griechischen Antike* (1959; 1967) stated that the *halma* started with a run-up and that the take-off took place from the *bater*, a fixed threshold in the ground. "In many cases the starting device (Ablaufvorrichtung) of the run was used for this purpose." (1967: 153-154).

Willy Zschietzmann published *Wettkampf- und Übungsstätten in Griechenland* in two volumes: *I. Das Stadion* (1960: 35-38) in which he mentions the end zone in the Olympic Stadion: 8 to 10.5 m between *balbis* and 'West End'; the end zone in Delphi: between *balbis* and *sphendone* (kind of amphitheatre). These elements will play an important role in explaining the distance covered by the long jumpers as we will see later. In volume *II. Palästra – Gymnasion* (1961: 55-56; Fig. 10 p. 100) he depicts a double track for training triple jumps in the *palaestra* of the Olympic *gymnasion* with a 18 m run-up zone to the *bater* and a 10 m *skamma* landing zone and 5 m extra *eskammena* (according to Carl Diem). However, we have nowhere found any archeological evidence of such an infrastructure!

Harold Arthur Harris (1902-1974) is often considered as the successor of E. Norman Gardiner in England. He published in 1960 'An Olympic epigram: the athletic feats of Phaÿllos'. Harris wrote the following "… possible explanation of the Greek jump … That it consisted of a single leap with a restricted run-up, and that this leap was measured from the start to the run-up; that there was a final take-off, the *bater* … at the end of the run-up; … that the dug-up pit lay beyond this final take-off; … that the distance normally achieved was such that the pit ending 50 feet from the starting point was thought enough adequate until Phaÿllos' effort. In practice the number of steps between the starting-point and the final take-off would have become "… as standardized as the modern hurdler's three strides between the hurdles …" (p. 7). Here Harris thus formulated the solution of the enigma of the *halma*, eight years before Spaak (1968) and forty-seven years before Lee (2007) would propose a similar scenario of the *halma* as a single jump with *halteres* after a standardized restricted run-up! Harris stuck to this opinion in his 1964 publication *Greek athletes and athletics*: "… although vase paintings sometimes depict an athlete possibly about to make a standing jump, the vastly greater number showing a jumper running with his weights suggests that the Greek jump, like ours, was done with a run; the standing jump was no doubt part of the training routine. The take-off for the jump was probably the starting line of the runners" (p. 83). However, eight years later, in his *Sport in Greece and Rome* (1972: 35-36), he drastically changed his opinion. Now he suggested a multiple, double, triple or quintuple jump and: "That the ancient technique was highly elaborate is suggested by the number of vase-paintings which depict athletes practising with them (*halteres*)." (p. 36).

The former East-German classical philologist and sport historian Joachim Ebert (1930-1999) occupies a pivotal position in the *halma* discussion. Ebert graduated in 1960 with a thesis on the ancient pentathlon and presented his 'habilitation' in 1968. However, because he refused to become a member of the Socialist Unity Party, he had to wait till 1983 before he became a full professor at the University of Halle (Luppe 2001). He combined his double education as a philologist and sport scientist at the university of Halle to prove that the 'record jumps' of 55 feet by Phayllos and of 52 feet by Chionis could be equalled by a modern athlete, performing five consecutively standing broad jumps with *halteres* of 2.5 kg in each hand (Ebert 1963). He showed this 'reconstruction' in a photographic movement analysis, which would later inspire several sport biomechanists to test his thesis. Ebert's publication, which was extensively commented by the Dutch classical archaeologist Jaap M. Hemelrijk (1966), was a kind of *kampter* (turning post) in the *halma* literature because from now on most authors turned to the multiple jump's thesis!

Before summarizing the diverging opinions regarding the *halma* enigma in a chronological table (See Table 1), we would like to add one rather exceptional interpretation of the *halma*, presented in 1964 by the Dutch sports journalist Bob Spaak in his book *Gods in the stadion: the Olympic Games in Antiquity* (in Dutch). Spaak, who cited the Phayllos jump and noticed – from vase images – that the *bater* was often marked with a pole (*kampter*) or with javelins planted in the ground, came up with the original proposal that: "It could be that the Greeks added up the run up and jump, which implies that all athletes had to start from the same spot." So he already suggested the inclusion of a standardized run-up zone in the total distance of the *halma* performance, 43 years before Hugh Lee (2007) would present this thesis!

The diverging opinions about the nature of the *halma* can be divided into two factions: those who are convinced that it was a running long jump on the one hand and those who claim that it was a multiple jump on the other hand. These opposite opinions are listed in Table 1: Authors in favour of the *halma* as a series of multiple jumps versus those in favour of a running long jump. Percy Gardner (1880) occupies an 'apart' position as he spoke of a "standing leap". Also indicated are the authors who claim that the *halteres* were either dropped before the landing and those who suggest that they were swung backwards but retained at the landing.

The table shows the major impact which Ebert's publication (1963) had on the different authors cited. We are not able to comment each of these publications but would like to make three exceptions: Brein's publication of 1978, Harald Schmid's doctoral thesis of 1997 and Lee's publication of 2007.

MULTIPLE JUMPS	RUNNING LONG JUMP
	Bintz 1878
Gardner 1880: standing leap	
Wassmannsdorf 1885: triple jump	
	Gardiner 1904/25/30
Jüthner 1909: triple js -halteres swung back-	
	Schröder 1927: No st br j
Mezö 1930: triple js -standing & running js-	
Hyde 1938: multiple jump	
	Marrou 1948
-standing l j without halters-	Pouelmans 1948
Saurbier 1955: run & 3 1 leg js	
	Popplow 1959
Zschietzmann 1960/1961: multiple js	Harris 1960/1964
Ebert 1963: 5 standing br js	
	Spaak 1968: run + j = halma
Harris 1972: multiple/double/triple or quintuple js	Patrucco 1972
Finley & Pleket 1976: 5 separate or contin. js	
-halteres dropped-	Paleologos 1977: short run up
Barney 1978: quadruple h-s-s-j	
Brein 1978: quintuple jump	
Renson 1980/1992: 5 standing br js	
Swaddling 1980/2008: multiple j -halteres swung back-	
-pictures of sbjs-	Sweet 1987: no multiple js
Derksen 1988: multiple sbjs -halteres dropped-	
	Hupperts 1989: short run 1 ft take-off
Van Hove e.a. 1992/1993: 5 standing br js	
Schmid 1997: triple jump	
	Grodde 1997
Golden 1998: multiple js: double/triple/quadr/ separ/5 standing br js	
Schöbel 2000: 5 standing br js	
Sinn 2000/2004: 5 standing br js	
Bernand 2003: 5 success. js	
-halteres swung backwards-	Young 2004
-halteres swung back and dropped-	Miller 2004
Lenoir e.a. 2005/2006: 5 standing br js	
Decker 2006: 5 standing js	
	Crowther 2007
Bäumel 2007: 5 standing br js	
-halteres dropped-	Kyle 2007
	Lee 2007: from end stadion
-halteres dropped-	Jajcevic 2008
Manas & Rodriguez 2010: standing triple jump	
	Beale 2011
Clarysse & Remijsen 2012: 5 standing br js weights swung backwards	
Mouratidis 2012: triple j with run	
	Zarnowski 2013
TOTAL: 26	21

Table 1: Authors in favour of the halma as a series of multiple jumps versus those in favour of a running long jump

Friedrich Brein (1978), who has edited Jüthner's magnum opus (Jüthner & Brein 1965; 1968), suggests that the *skamma* was a 50 feet long dug up, which started at the *balbis* (starting blocks) in the direction of the stadion run (p. 93). Brein opts – in line with Ebert (1963) – for a quintuple long jump with *halteres* and accompanied by the sound of the *aulos*, whereby the athlete took off with one foot from the *balbis* and continued with standing broad jumps in the dug up *skamma* (sic: p. 107).

Harald Schmid, former European champion both in the 400 m hurdles and 4 x 100 m relay and Olympic bronze medallist in the 4 x 100 m relay in Montreal in 1976 and in the 400 m hurdles in Los Angeles in 1984, published his doctoral thesis in 1997 entitled *Zur technik des Weitsprungs in der griechischen Antike* (two volumes). This sport scientist presents an excellent review of literature, an extensive iconography of the *halma* and makes interesting references to contemporary traditional long jumps in Greece and Cyprus (without *halteres*) and modern hop-step jumps. But, after all these academic endeavours, he comes to the following stunning conclusions that there is no indication of a *bater* nor *skamma* nor clear *landing marks*, nor in literature nor in iconography (Vol. 1: pp. 160-161). Referring to his study of folkloristic triple jumps, still practised in Greece and Cyprus, he concludes that such a triple jump without *halteres* is the only solution to solve the question of the ancient long jump (p. 161) and that "The vase images don't depict athletic jump contests but jump training with halteres." (p. 162) [sic]. No further comments …

In his much cited publication 'The Halma: a running or standing jump?' Hugh M Lee (2007), professor of Classics at the University of Maryland (US), first confronts the 'Running long jump school' (Gardiner, Jüthner, Hyde, Harris, Patrucco, Yalouris [actually Palaeologos in Yalouris], Sweet and Miller) with the 'Standing broad jump school' (Ebert, Gardner, Schröder, Drees, Finley & Pleket, Decker and Sinn). He points to the publication by Olaf Grodde on *Sport bei Quintilian* (1997) from which he cites "The competitors seek a longer attempt and bring themselves in a run to the jumping pit." (p.156). He further illustrates this explicit mention of the run-up with the visual evidence of two jumpers on a red figure kylix by the Telephos painter, preserved in the Boston Museum of Fine Arts (**See figure 3**).

One of these 'iconic' images had already appeared in a chapter by the Belgian archaeologist Doris Van Hove in her exhibition catalogues in

Figure 3: Pentathlon scene with *akontist* (javelin thrower) and jumper with *halteres* during take-off on the *bater,* watched by an official or a trainer. Here the *bater* is the *balbis*, marked by a *kampter* (turning post). (Attic red figure kylix attributed to Telephos painter ca 470-460 BC; Boston: Museum of Fine Arts).

Dutch and French *Sport in Hellas / Le sport dans la Grèce antique* (Van Hove e.a. 1992a & b: 107) (**See figure 3**). Although Van Hove identifies the vase painting as “Athlete hits the bater with a bang” (Pollux III 151), she remains persuaded that the *halma* was a combination of five standing broad jumps, based on the laboratory experiments by Lenoir (1991). [… a *penthalma* if you allow me this word play …]. Thus Lee ‘reformulates’ the thesis, already put forward by Harris in 1960 (but who later changed his opinion) and by Spaak in 1964, that the *halma* combined a run-up from the border of the stadium [ca 10.5 m] with a take-off at the *balbis*, marked by a *kampter*, to a landing in the *skamma*.

Biologized by the Halma: the Biomechanists

Two of the major pioneers of human movement analysis, Eadweard Muybridge (1830-1904) and Georges Demenÿ (1850-1917) photographed the sequences of the long jump. Muybridge (1907) developed the high speed photography with different cameras. Demenÿ, who was the assistant of Etienne-Jules Marey (1830-1904), considered the father of chronophotography, analysed both the running long jump as well as the standing long jump. In his 1904 publication *Mécanisme et éducation des mouvements* (reprinted in 1924) Demenÿ showed an illustration of a circus acrobat, who jumped over a horse-drawn carriage, throwing away the *halteres* (fig. 427 p. 333) he held in his hands at the take-off, which "… increased the horizontal speed of his body. Like in the explosion of a bomb, the parts projected forwards, will touch the ground at a further point." The circus artist in question was the Englishman John Higgins (1872-?), who made jumps with *halteres* his specialty (Denis 2016). However, no reference was made to the ancient Greek pentathletes, although Guillaume Depping in his *Merveilles de la force et de l'adresse* (1886) had already pointed out the role of the *halteres*, "… which provided a greater momentum and energy to the jumper and acted as counterweights when he landed on the ground." (p. 143).

The impact of Ebert's biomechanical thesis of a series of five consecutive standing broad jumps with *halteres* has already been stressed (Ebert 1963). He not only convinced a whole generation of sport historians but also inspired sport biomechanists to test his standpoint.

The article by Minetti and Ardigo (2002) is probably the most cited biomechanical explanation of the ancient Greek long jump, probably because it was published in *Nature.* Inspired by Ebert (1963), they tested whether *halteres* could extend a standing broad jump. However, they used a software model of a jumper with weights varying from 0 to 20 kg but in a vertical jump. Moreover four subjects were asked to perform maximal vertical jumps on a force platform with a pair of *halteres*, which ranged between zero to 17 kg. They concluded that a greater distance of at least 0.17 m in a 3-m jump can be achieved during a loaded horizontal jump (which they did not test …) and: "That the mass range of the halteres that enables all these effects to be optimized (about 2-9 kg) corresponds closely to the actual size range of archaeological halteres specimens … suggesting that athletes in ancient times had worked this out for themselves." (sic).

Butcher and Bertram (2004) conducted a simple lab exercise to investigate the effect of carrying pairs of handheld weights of 1.4, 2.3 and 3.6 kg in a standing long jump. Only two subjects, a male of 90.9 kg and a female of

57.3 kg, were tested. The effect on each of the jumpers was very similar, but the larger individual had a greater advantage by using heavier weights.

The article by Lenoir, De Clercq and Laporte on "The "how" and "why" of the ancient Greek long jump with weights: a five-fold symmetric jump in a row?" (2005) is of a very different level than the two foregoing ones. The authors refer to Ebert (1963) and Minetti and Ardigo (2002) and combine an in depth-review of literature and pictorial sources with a filmed biomechanical analysis of five consecutive standing broad jumps performed by four trained athletes with and without weights. All four jumped further with weights of 2.3 kg each than without weights. The only problem is that Lenoir and his colleagues are so 'impressed' by the performances of Phayllos of Kroton and Chionis of Sparta, who jumped respectively 50 Delphic and 52 Olympic feet, that they seek the solution of the enigma in Ebert's thesis of five standing broad jumps and thus decline all other visual evidence offered by vase paintings: "… presenting the athletes with the legs spread apart is very likely the result of artistic considerations rather than a reliable reproduction of jumping technique." (p. 1036) or "The second objection against interpreting pictorial remains as evidence for a running jump is that halteres were also used for training exercises." (p. 1037). They claim that "This technique is also compatible with the written and pictorial remains of that era." (p. 1042). Nevertheless these University of Ghent colleagues remain their academic 'cool' when they conclude that "The ancient Greek pentathlon jump could have been a five-fold standing broad jump … the dynamical version of the five-fold jump remains a plausible explanation of the 55 jump of Phayllos." (p. 1042). A Dutch version of this article appeared in *Sportimonium* (Lenoir; De Clercq; Laporte 2006).

Decatoire, Monnet and Junqua (2009) studied the effect of releasing the weights during a long jump performed by one of them, which resulted in a gain of 21 cm. Results from a master thesis in engineering at Grand Valley State University showed that performance in a standing broad jump improved with weights by an average of 9 cm. Further it was proven that releasing the weights before landing added an additional 3-7 cm on average (Filush 2012). Although several authors have suggested this technique of dropping the *halteres* (Palaeologos 1977; Derksen 1988; Miller 2004; Kyle 2007; Jajcevic 2008), there exists – to our knowledge – not one single image of it in ancient Greek iconography, but it has been spread through a

Figure 4: Jumper in mid-flight during *halma* practice in the palaestra with trainer and other jumper. (Attic red figure kylix ca 500-490 BC attributed to Onesimos painter; Boston: Museum of Fine Arts).

'visual reconstruction' by K. Iliakis, which appeared in the chapter on 'Jumping' by Klianthis Palaeologos (1977: pp. 178-179) in *The Olympic Games in ancient Greece*, edited by Nicolaos Yalouris. This is an example how modern iconography can 'infect' ancient images.

A synthetic overview of publications and congress reports on the effect of using *halteres* in standing long jumps has been given by McKenzie and colleagues (2014) from Auckland University, New Zealand. However, as not one single image of such a standing long jump with *halteres* could be traced in our extensive iconographic analysis, we will not go into further detail on this matter. Nevertheless, the 'Ebert standing broad jumps thesis of the *halma*' has inspired many school teachers, university professors and sport fans to try out jumping with handheld dumbbell weights. Such an example was, for instance, the collaboration between the School of Art and the Athletics Department of the Texas Tech University at Lubbock. Four participants tested a standing broad jump with 'homemade' bronze *halteres* of the phone receiver type. They jumped three times with no weights and three times with weights of 3, 2 and 1 kg. Remarkably enough, three of the four participants jumped farthest on average with no weights while one jumped farthest on average with 3 kg weights (Friedman & Miller 2017). All in all this experiment was more a successful collaboration experiment between a Classics department and an Athletics department than an elaborate biomechanical experiment.

Jumping to Conclusions with the Help of Iconography

Presenting an article on the technique of the ancient Greek long jump without referring to the extensive iconography related to the topic, is almost like participating in the *halma* without *halteres*. During the Sarton Medal Lecture (Renson 2019) we have extensively referred to – and shown – the multifaceted iconography on the topic, which stands in strong contrast to the scarce written sources. These images, which "… say more than a thousand words", are rather abundant but need critical interpretation. Jüthner & Brein (1968: 159) pointed out that the literary sources of how the long jump training took place are very sparse and that we are almost exclusively dependent on pictorial sources. "The question is whether the research based on these literary sources of the pentathlon jump is confirmed by the visual images … The challenge is thus to interpret the entire material integratively ("zusammenfassend")." (Jüthner & Brein 1968: 213). Others are more than sceptical about iconography. Howland (1967: 381), for instance, has stressed that "…vase paintings are our main source of information, but Harris (1964) rightly emphasizes that this kind of evidence is unreliable [sic]. Artists are not photographers and are not always well versed in athletics. Runners, for instance, are frequently depicted in an impossible attitude …"

Taking into account these 'anti-iconographic' warnings, one cannot deny the overwhelming visual evidence that the pentathletes competing in the *halma*, started from a spread position with the *halteres* in their hands. Then followed a run-up with the *halteres* swung rhythmically, assisted by the sound of the *auletes*, followed with a take-off on the *bater* with the arms swung up high. During the flight phase the arms are first swung forwards (**see figure 4**) and just before landing backwards and forwards again to secure a stable landing with clear footmarks (**see figure 5**). David Young (2004: 35) commented: "… Many scholars have accepted the hypothesis of the eminent German scholar, Joachim Ebert (1963), namely that the ancient jump consisted of a series of five standing broad jumps. But the evidence from art excludes any possibility of a standing jump." We thus share Harris's original vision on the long jump (1960; 1964), explicitly formulated by Spaak (1968) and by Lee (2007), that the *halma* included a run-up of ca 10.5 m from the border of the stadion (often a semicircular *sphendone* as seen in the pictures by Sturzebecker 1985) to the stone *balbis*, which served as starting line of the stadion run but also as the *bater* or take-off point for the jump. The *bater* is also the beginning of the *skamma* or dug up area and pickaxes are often depicted, which were used for that purpose. The *skamma* in which the jumper had to land with clear footmarks, was about 5 to 6 m long, which explains why Phayllos of Kroton gained such fame when he landed outside the dug up zone with his 16.3 m jump.

Let us end this enigmatic history with the picture of a jumper who has just landed in the *skamma* in a stable position with the *halteres* in his hands, depicted on a kylix from the 5th century BC. We hope that our conclusion is as stable as this landing!

Figure 5: Pentathlete finishing his *halma*-jump in the *skamma* with the *halteres* in his hands and a pickaxe (*skapheion*) in the background (Red figure kylix 5th Century BC, with inscription '*kalos*' (beautiful), attributed to the Louvre Group; Lecce (It): Museo Provinciale Sigmondo Castromediano).

References

[1] Antonides Theodorus, 1732, *Olympia, dat is Olymp-Speelen der Grieken …*, Groningen: J. Cost, 496 p. & index.

[2] Badinou Panayota, s.d., *Olympiaka: anthologie des sources grecques*, Lausanne, Comité International Olympique, 287 p.

[3] Barney Robert Knight, 1978, The ancient Greek pentathlon jump: preliminary reinterpretive examination, in Landry Fernand; Orban William Andrew Robert (eds), *Philosophy, theology and history of sport and physical activity* (Int. Congress Phys. Activity Sciences Series 8), Miami (Fl): Symposia Specialists, 279-288.

[4] Bäumel Linda, 2007, Das Pentathlon – Der antike Fünfkampf, *Forum Archeologiae* 42 (3) (htpp://farch.net).

[5] Beale Alan, 2011, *Greek athletics and the Olympics*, Cambridge: Cambridge University Press, 196 p.

[6] Bernand André, 2003, *The road to Olympia: origins of the Olympic Games*, London: Periplus, 300 p.

[7] Bintz Julius, 1878, *Die Gymnastik der Hellenen*, Güthersloh: Bertelsmann, 175 p.; Reprint 1973, Walluf bei Wiesbaden: Martin Sändig.

[8] Brein Friedrich, 1978, Die Leibesübungen im alten Griechenland, in Überhorst Horst (ed), *Geschichte der Leibesübungen: Band 2*, Berlin: Bartels & Wernitz, 82-167.

[9] Burette Pierre-Jean, 1717, Dissertation sur ce qu'on nommait Pentathle dans l'ancienne gymnastique, in *Mémoires de literature tirés des registres de l'Academie Royal des Inscriptions et Belles Lettres* (Depuis MDCCXII jusqu'à MDCCXVIII; Tome quatrième), La Haye: Pierre Goesse, 441-457.

[10] Butcher Michael T; Bertram John EA, 2004, Jump distance increases while carrying handheld weights: impulse, history, and jump performance in a simple lab exercise, *Journal of Science Education and Technology* 13 (2): 285-297.

[11] Clarysse Willy; Remijsen Sofie, 2012, Long jump -, in *OlympiaGamesEvents & athletesBackgroundImpact* (http://ancientolympics.arts.kuleuven.be).

[12] Crowther Nigel B, 2007, *Sport in ancient times*, Westport (Conn): Praeger, 183 p.

[13] Decatoire Arnaud; Monnet Tony; Junqua Alain, 2009, Le saut en longueur sans élan ancient … et moderne (sautsanselan.pdf-Google: 22 janvier 2009), 16 p.

[14] Decker Wolfgang, 1995, *Sport in der griechischen Antike*, München: C.H. Beck, 255 p.

[15] Decker Wolfgang, 2006, Long jump, *Brill's New Pauly* (BrillOnline.com) 2 p.

[16] Demenÿ Georges, 1904; 1924 (4th ed.), *Mécanisme et éducation des mouvements*, Paris: Alcan, 523 p.

[17] Denis Dominique, 2016, John Higgins, sauteur aux haltères, *Artistes, Tout sur le cirque* (www.circus-parade.com/2016/05/09.sauteurs-aux-haltère/).

[18] Depping Guillaume, 1886, *Merveilles de la force et de l'adresse* (Quatrième édition), Paris: Hachette, 376 p.

[19] Derksen JJVM, *Om de eer: De Olympische Spelen in de Oudheid en nu*, Amsterdam: Allard Pierson Museum, 87 p.

[20] De Witte Philippe, 2010, *Essay: Olympia. That is the Olymp-Games of the Greek* (Essay for Module 1: Ethics, values and Olympic education of the Master of Arts in Olympic Studies; 20 p.).

[21] Ebert Joachim, 1963, *Zum Pentathlon der Antike: Untersuchungen über das System der Siegermittlung und die Ausführung des Halterensprunges* (Abhandl. Sächs. Ak. der Wiss. zu Leipzig 56 (1)), Berlin: Akademie-Verlag, 64 p.

[22] Eloge de M. Burette, 1754, in *Histoire de l'Académie Royale des Inscriptions et Belles Lettres* (Tome 2&), Paris: Imprimerie Royale, 217-233.

[23] Faber Petrus, 1595, *Agonisticon sive de re athletica*, Lugduni: Symptibus Thomae Soubron, 684 p. & index.

[24] Filush Austin, 2012, Effect of using hand-weights on performance in the standing long jump. *Master theses*: 28 (htttp://scholarworks.gvsu.edu/theses)

[25] Finley MI; Pleket HW, 1976, *The Olympic Games: the first thousand years*, London: Chatto & Windus, 138 p.

[26] Friedman Hannah; Miller Peter J, 2017, Reconstruction of the ancient Greek long jump – an opportunity for multidisciplinary collaboration, *Ancient Technology* 3 (https://exarc.net:issue-2017-3/at), 14 p.

[27] Gardner Percy, 1880, The pentathlon of the Greeks, *The Journal of Hellenic Studies* 1: 210-223.

[28] Gardiner, E. Norman, 1904, Phayllus and his record jump, *The Journal of Hellenic Studies* 24: 70-80.

[29] Gardiner E. Norman, (1925) 1973, *Olympia: Its history & remains*, Washington DC: McGrath, 316 p.

[30] Gardiner E. Norman, (1930) 1980, *Athletics of the Ancient World*, Chicago: Ares, 246 p.

[31] Golden Mark, 1998, *Sport and society in Ancient Greece*, Cambridge: Cambridge University Press, 216 p.

[32] Grodde Olaf, 1997, *Sport bei Quintilian* (Nikephoros Beihefte 3), Hildesheim: Weidmann, 103 p.

[33] Harris Harold Arthur, 1960, An Olympic epigram: the athletic feats of Phaÿllos, *Greece & Rome* 7 (1): 3-8.

[34] Harris Harold Arthur, 1964, *Greek athletes and athletics*, London: Hutchinson, 244 p.

[35] Harris Harold Arthur, 1972, *Sport in Greece and Rome*, London: Thames and Hudson, 288 p.

[36] Hemelrijk Jaap M, 1966, Het pentathlon, *Hermeneus* 17 (5): 116-126.

[37] Howland RL, 1967, Handbook of Greek athletics, *The Classical Review* 17 (3): 381-383.

[38] Hyde Walter Woodburn, 1938, The pentathlum jump, *American Journal of Philology* 59 (4): 405-417.

[39] Hupperts Charles; Beets Marianne; De Fraiture Beatrice; Jans Elly; Twisk Yvonne, 1989, *Sport en opvoeding in de Griekse oudheid: een bronnenboek,* Leeuwarden: Eisma, 200 p.

[40] Jajcevic Zdenko, 2008, *Anticke Olimpijske Igre: i moderni Olimpijski pokret do 1917. godine*, Zagreb: Libera Editio, 304 p.

[41] Jüthner Julius, 1909, *Philostratos über Gymnastik*, Leipzig: Teubner, 336 p.

[42] Jüthner Julius, 1941, Akoniton-akoniti, *Glotta* 29 (1-2): 73-77.

[43] Jüthner Julius; Brein Friedrich (ed), 1965, *Die athletischen Leibesübungen der Griechen I: Geschichte der Leibesübungen*, Graz: Hermann Böhlaus, 206 p.

[44] Jüthner Julius; Brein Friedrich (ed), 1968, *Die athletischen Leibesübungen der Griechen II: Einzelne Sportarten,* Graz: Hermann Böhlaus, 260 p.

[45] Krause Johann Heinrich, 1838, *Olympia oder Darstellung der grossen Olympischen Spiele*, Wien: Beck's Universitäts-Buchhandlung; Reprint 1972, Hildesheim: Georg Olms, 436 p.

[46] Krause Johann Heinrich, 1841, *Die Gymnastik und Agonistik der Hellenen*, Niederwalluf bei Wiesbaden: Martin Sändig, 994 p.

[47] Kyle Donald G, 1990, E. Norman Gardiner and the myth of the decline of Greek sport, in Kyle Donald G; Stark Gary D (eds), *Essays on sport history and sport mythology*, College Station: Texas A&M University, 7-44.

[48] Kyle Donald G, 1991, E. Norman Gardiner: historian of the ancient world, *The International Journal of the History of Sport* 8 (1): 28-55.

[49] Kyle Donald G, 2007, *Sport and spectacle in the Ancient World*, Malden (Ma): Blackwell, 403 p.

[50] Lee Hugh M, 2007, The *halma*: a running or standing jump, in Schaus Gerald P; Wenn Stephen R (eds), *Onwards to the Olympics: historical perspectives of the Olympic Games*, Waterloo: Wilfrid Laurier University Press, 153-165.

[51] Lenoir Matthieu, 1991, *De antieke Griekse vertesprong met halters* (Licentiate thesis Physical Education, University of Ghent), 111 p.

[52] Lenoir Matthieu, De Clercq Dirk, Laporte Willy, 2005, The "how" and "why" of the ancient Greek long jump with weights: a five-fold symmetric jump in a row? *Journal of Sport Sciences* 23 (10): 1033-1043.

[53] Lenoir Matthieu, De Clercq Dirk, Laporte Willy, 2006, Mythe of ingenieuze techniek? *Sportimonium* 26 (1-2): 4-10.

[54] Luppe Wolfgang, 2001, Joachim Ebert, *Gnom* 73 (2): 184-186.

[55] Manas Alfonso; Rodriguez Juan, 2010, Some light on the jumping event in the ancient Olympic Games: suggestions provided by the Purchena Games of 1569, *International Journal of the History of Sport* 27 (13): 2288-2310.

[56] Marrou Henri Irénée, 1964, *Histoire de l'éducation dans l'Antiquité: Nouvelle édition,* Paris: Seuil, 646 p.

[57] McIntosh Peter C, 1984, Hieronymus Mercurialis 'De Arte Gymnastica': classification and dogma in physical education in the sixteenth century, *The International Journal of the History of Sport* 1 (1): 73-84.

[58] Mckenzie Chloe; Brughelli Matt; Gamble Paul; Whatman Chris, 2014, Enhancing jump performance with handheld loading, *Strength and Conditioning Journal* 36 (2): 32-38.

[59] Mercurialis Hieronymus, 1601, *De arte gymnastica*, Venetiis: apud Juntas, 308 p. + index.

[60] Miller Stephen G, 1991, *Arete: Greek sports from ancient sources,* Berkeley: University of California Press, 227 p. (Reedition 2004, 235 p.)

[61] Miller Stephen G, 2004, *Ancient Greek athletics*, New Haven (Ct): Yale University Press, 288 p.

[62] Minetti Alberto E; Ardigo Luca P, 2002, Biomechanics: halteres used in ancient Olympic long jump, *Nature* 420: 141-142.

[63] Mezö Franz, 1930, *Geschichte der Olympische Spiele,* München: Knorr & Hirth, 279 p.

[64] Mouratidis John, 2012*, On the jump of the ancient pentathlon* (Nikephoros Beihefte 20), Hildesheim: Weidmann, 219 p.

[65] Muybridge Eadweard, 1907, *The human figure in motion: an electro-photographic investigation of consecutive phases of muscular actions*, London: Chapman & Hall, 146 p.

[66] Palaeologos Kleanthis, 1976/1982, Jumping, 176-187, in Yalouris Nicolaos (ed), *The Olympic Games in ancient Greece*, Athens: Ekdotike Athenon, 303 p.

[67] Patrucco Roberto, 1972, *Lo sport nella Grecia antica*, Firenze: Olschki, 436 p.

[68] Pfeijffer Ilja Leonard, 1999, *Three Aeginetan odes of Pindar: a commentary on Nemean V, Nemean III & Pythian VIII*, Leiden; Brill, 724 p.

[69] Popplow Ulrich**, (**1959) 1967, *Leibesübungen und Leibeserziehung in der griechischen Antike* (Beiträge zur Lehre und Forschung der Leibeserziehung 2), Schorndorf bei Stuttgart: Karl Hofmann, 201 p.

[70] Pouelmans Edgard, 1948, *Het pentathlon in Oud-Griekenland* (Licentiate thesis Physical Education; supervisor J. Gessler), Leuven: KU Leuven, 212 p.

[71] Renson Roland, 1980/1992, *Geschiedenis van de sport in de oudheid*, Leuven: Acco, 208 p.

[72] Renson Roland, 2019, *The enigma of the halma: an iconographic approach to unravel the technique of the ancient Greek long jump* (Power point presentation 9 may 2019; Sportimonium, Zemst; Sarton Medal Lecture Ghent University).

[73] Robinson Rachel Sargent, 1927 (1955), *Sources for the history of Greek athletics in English translation*, Chicago: Ares, 289 p.

[74] Saurbier Bruno**,** 1955 (1969: 22-23), *Geschichte der Leibesübungen*, Frankfurt/Main, Limpert, 232 p.

[75] Schmid Harald, 1997, *Zur Technik des Weitsprungs (halma) in der griechischen Antike* (Doctoral dissertation Sport Sciences), Mainz: Johannes Gutenberg University, Vol. 1: 217 p.; Vol. 2: 215 p.

[76] Schöbel Heinz, 2000, *Olympia und seine Spiele*, Berlin: Sportverlag, 175 p.

[77] Schröder Bruno, 1927, *Der Sport im Altertum*, Berlin: Schoetz, 196 p.

[78] Sinn Ulrich, 2000, *Olympia: cult, sport, and ancient festival*, Princeton: Markus Wiener, 149 p.

[79] Sinn Ulrich, 2004, *Das Antike Olympia: Götter, Spiel und Kuns*t, München: Beck, 276 p.

[80] Spaak Bob**,** 1964, *Goden in het stadion: De Olympische spelen in de oudheid* (Geïllustreerde salamander 169), Amsterdam: Querido, 224 p.

[81] Spivey Nigel Jonathan, 2005, *The Ancient Olympics*, Oxford: Oxford University Press, 273 p.

[82] Sturzebecker Russell L, 1985, *Photo atlas athletic-cultural archaeological sites in the Greco-Roman world: Europe – North Africa – Middle East*, West Chester (Pa): R L Sturzebecker, 514 p.

[83] Swaddling Judith, 1980/2008, *The ancient Olympic Games*, London: British Museum Press, 120 p.

[84] Sweet Waldo E., 1987, *Sport and recreation in ancient Greece*, Oxford: Oxford University Press, 281 p.

[85] Van Hove D; Laporte W; Bultiauw P; Raepsaet G; Hemelrijk J, 1992a, De sport-takken, in Van Hove D (ed) *Sport in Hellas: van spel tot* competitie (Exhibition 1992), Brussel: Paleis voor Schone Kunsten, 99-123.

[86] Van Hove D; Laporte W; Bultiauw P; Raepsaet G; Hemelrijk J, 1992b, Les disciplines sportives, in Van Hove D (ed), *Le sport dans la Grèce antique: du jeu à la compétition* (Exhibition 1992), Bruxelles: Palais des Beaux Arts, 99-123.

[87] Van Hove D (ed), 1993a, *L'Olympisme dans l'Antiquité*, Lausanne: Musée Olympique, 163 p.

[88] Van Hove D, 1993b, Les disciplines sportives, in Van Hove D (ed), *L'Olympisme dans l'Antiquité*, Lausanne: Musée Olympique, 44-49.

[89] Waller Richard, 2008, An early eighteenth-century view of Greek athletics: Pierre-Jean Burette (1665-1747), *Journal for Eighteenth-Century Studies* 5 (1): 105-115.

[90] Wassmannsdorf Karl, 1885, *Monatsschrift für das Turnwesen* 4: 270-275.

[91] West Gilbert, 1749, *Odes to Pindar. With several other pieces in prose and verse, translated from the Greek. To which is prefixed A Dissertation on the Olympick Games*, London: R. Dodley, 315 p.

[92] Young David C, 1984, *The Olympic myth of Greek amateur athletics*, Chicago: Ares, 202 p.

[93] Young David C, 2004, *A brief history of the Olympic Games*, Malden (Ma): Blackwell, 200 p.

[94] Zarnowski Frank, 2013, *The pentathlon of the ancient world*, Jefferson (NC): McFarland, 208 p.

[95] Zschietzschmann W, 1960, *Wettkampf- und Übungsstätten in Griechenland: I. Das Stadion* (Beiträge zur Lehre und Forschung der Leibeserziehung 5), Schorndorf bei Stuttgart: Karl Hofmann, 73 p.

[96] Zschietzschmann W, 1961, *Wettkampf- und Übungsstätten in Griechenland: II. Palästra – Gymnasion* (Beiträge zur Lehre und Forschung der Leibeserziehung 8), Schorndorf bei Stuttgart: Karl Hofmann, 134 p.

[97] Zebrowski Martha K, 2012, Gilbert West's Dissertation on the Olympick Games (1749): 'Established on great political views', *Journal for Eighteenth Century Studies* 35 (2): 239-247.