SARTONIANA

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Sarton Chair of History of Science
Ghent University

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Introduction

Robert Rubens

The 25th volume of Sartonia represents a hallmark in this series. More than a quarter of a century ago Ghent University, remembering its alumnus George Sarton founded the chair, carrying his name and devoted to the history of sciences. The volume you are starting to read contains the transcripts of the lectures held during the past academic year.

Prof Sheila Jasanoff,the lecture holder for 2011-12, coming from Harvard University as the Pforzheimer Professor of Science and Technology Studies gave a new field to the interdisciplinarity so common in the creed of Sarton. Furthermore coming from a department imbued with the ideas of Sarton she has synthesized and introduced sociology in the large field of the history of science.

The historical review of the ideas of Burke as explained by Prof. Sean Dolan situates Edmund Burke in his time frame. By his careful studies of the original writings of Burke he furthermore could more precisely circumscribe the reforming ideas of Burke.

The painstakingly detailed collection of Belgian names in petrology as kept by Prof. De Paepe not only has been published in a large volume but for the scientist who needs an introduction we are glad that he could deliver a medal lecture devoted to the subject.

The last couple of years saw a new interest building in the history of veterinary science. Dr Luc Devriese not only worked years in bacteriology but also founded a museum devoted to the history of veterinary science. He was the medalist for the faculty of veterinary medicine and gave an overview of the genesis of the scientifically trained veterinary surgeon.

The whole of our monetary system is now in a turmoil. Prof. Michel De Vroey who had during many years the chair of the history of economic sciences and an international acclaimed author in this field lectured us about the two main lines in macroecomics.

Prof.Raphaël Suy was during years the leading cardiovascular surgeon in the medical faculty of Leuven. After his retirement he became an expert in the history of medical thought and medical philosophy. Referring numerous authors he will explain the quest of the urinary membrane.

In a volume devoted to the history of science Prof.Guicciardini describes the importance and the algebraïcal rules proposed by Newton in his writings.

Finally the paper by Trachtenberg gives a nice overview of architecture in premodern times. In his review the details of the different architectural structures of that period are carefully described.

Sarton Chair Lectures

Laudatio Sheila Jasanoff (1)

Raf Vanderstraeten

Ladies and gentlemen,

In 1913, George Sarton published in Ghent the first issue of the journal *Isis*, which was itself the first journal explicitly devoted to the history of science. On the front cover of this issue, Sarton listed *Isis'* patrons, among whom the famous French sociologist Emile Durkheim. In his opening essay, Sarton put forward his view on the identity of a yet-to-be-established scientific specialization. He defined his specialization as a "psycho-sociological investigation". A former Ph.D. student of Sarton at Harvard University, named Robert King Merton, became Associate Editor of *Isis* in the late-1930s, first responsible for what was called "the social aspects of science" and, as of 1942, for "sociology". And in 1952, only a few years before his death, George Sarton, who was by then generally respected as the dean among the historians of science, could still refer to "my sociology of science" (Sarton, 1952, p. 94).

Despite the interdisciplinary orientation of Sarton's original project, it is not difficult to see how the forces of specialization and disciplinarization have shaped today's scientific field. Before the early-1960s, there was no obvious single place for historians of science in the departmental organization of universities. Historians of science could be working in departments of history, but they might as well be members of other university departments, such as physics or medicine (see Kuhn 1984, p. 29-30). But in the late-1950s and the 1960s, that situation started to change. History of science was not only a clear beneficiary of the university expansion. The multiplicity of formal academic affiliations of historians of science was

narrowed down, too. In an article entitled "The Isis Crises and the Coming of Age of the History of Science Society", the Harvard professor Bernard Cohen (who was not only a Ph.D. student and protégé of Sarton, but who also succeeded Sarton as editor of *Isis*) wrote in 1999: "The very fact that there are now so many departments of history of science in U.S. colleges and universities is perhaps the best sign of the coming of age of our discipline" (1999, p. S42). Since the last decades of the twentieth century, history of science has increasingly developed its own research agenda. It has closed in on itself; it has a strong intra-disciplinary orientation. Contemporary historians of science mainly build their argument on, and refer to publications of other historians of science. They have their own specialized professional associations and visit their own specialized scientific conferences.

It is nevertheless also evident that the forces of specialization and discipline-formation have been and are being countered by interdisciplinary developments. Already in the middle of the twentieth century, scientists such as Thomas Kuhn and Robert Merton began to lay the groundwork for the integration of a sociological perspective into the history of science. In the last decades of the twentieth century, there have also emerged other types of 'science studies', such as sociology of science, social studies of science, or science and technology studies. These newcomers are characterized by an interdisciplinary orientation. The interchanges between these and neighboring specializations are often much stronger. But its practitioners also decry the asymmetries in the relationship with history of science. In the year 2000, for example, the then President of the Society for Social Studies of Science addressed the historians of science gathered to celebrate the 75th anniversary of the History of Science Society with the following words: "I was struck by a kind of asymmetry, or at least an imbalance, in our perceptions of each other. 4S [an acronym which stands for: Society for Social Studies of Science] has recognized from its very foundation that history of science has to be part and parcel of any meaningful attempt to study science and technology as human, social institutions. (…) On the HSS side [History of Science Society], the attitude toward 4S's intellectual programme bespeaks, in my view, considerably greater wariness" (Jasanoff, 2000, p. 622). These words were spoken by Harvard Professor Sheila Jasanoff.

It is a great honor and a great pleasure for me to welcome Prof. Sheila Jasanoff here as the beneficiary of the George Sarton Chair 2011/12 of Ghent University. This chair was instituted about a quarter of a century ago, at the centenary of Sarton's birthday. The first award of the Sarton committee was received by a famous sociologist and former student of the historian of science Sarton. I have already mentioned his name: Robert King Merton. In the academic year 1986/87, Merton delivered his inaugural lecture here on the Matthew effect in science. Today, twenty-five years later, we again pay tribute to someone who has in many regards followed in the footsteps of George Sarton. At Harvard University, as the Pforzheimer Professor of Science and Technology Studies, she is literally one of the successors of Sarton. Like Sarton, she has also contributed greatly to developing an open, interdisciplinary orientation to science studies. Over the last decades, Sheila Jasanoff has analyzed in much historical detail the role of science and technology in the politics of modern democracies. In her recent research, she has, in particular, been able to demonstrate the coproduction of national identities and science policies: her research highlights the myriad ways in which knowledge about the world both conditions and is conditioned by choices about how people wish to live in the world. With many case-studies, she has shown how nations try to govern scientific innovations and use science policies to establish or affirm their own national identities. I should also mention that Prof. Jasanoff has at Harvard University created a hospitable environment for an interdisciplinary approach to science studies, in which year after year new generations of young researchers and fellows – both from the USA and abroad – are stimulated to develop their research interests along lines which, without any doubt, would have received the full approval of George Sarton.

I would now like to give the floor to Prof. Jasanoff. But I cannot do this without heartily thanking the university and in particular the members of the Sarton Committee for their very positive response to my proposal to award the George Sarton Chair 2011/12 to Prof. Sheila Jasanoff.

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Laudatio Sheila Jasanoff (2)

Frederic Vandermoere

Dear colleagues, distinguished guests,

I would like to start this *laudatio* with a very short introduction to some of the "fact sheets" of Sheila Jasanoff. Professor Sheila Jasanoff holds an AB in Mathematics from Radcliffe College/Harvard, an MA in Linguistics from the University of Bonn, a PhD in Linguistics from Harvard University, and a JD from the Harvard Law School. Since 2002 Sheila Jasanoff is Pforzheimer Professor of Science and Technology Studies at the Harvard Kennedy School. She has also been a visiting professor at numerous universities including Oxford, Cambridge (UK), Yale, and MIT. Prof. Jasanoff served on the Board of Directors of the AAAS (the American Association for the Advancement of Science) and as President of 4S (the Society for Social Studies of Science).

Some other facts concern the following: A few weeks ago, when I was writing my *laudatio*, I did a search in our, much-debated, Web of Science. After a few seconds the system generated more than 150 publications with Jasanoff as author name. Unfortunately the Web of Science does not include data at the household level. And I'm referring here to her husband Professor Jay Jasanoff, who is also a prominent scholar at Harvard (in the linguistics department), but also to their son and daughter, who are assistant professors at MIT and Harvard respectively. Now, after a few minutes of "data mining and cleaning" the Web of Science generated 98 records with Sheila Jasanoff as author. Next to the fact that this is an underestimation I would like to share with you the following: In addition to a dozen of books, about a dozen of her writings are published in the journals *Nature* and

Science; another dozen are published in *Social Studies of Science*, the leading journal in the field of S&TS. And also George Sarton would be very satisfied, as Sheila has also published more than a dozen of other publications in history of science journals such as *Isis* and *Osiris*. In each of these cases Prof. Jasanoff developed new concepts and perspectives on topics such as gene technology, climate change, the control of chemicals, and the use and misuse of science and technology in lawsuits.

In other words, even this short introduction to some of the fact sheets of Sheila Jasanoff is very impressive. Of course. However, one of the main reasons for the success of Sheila Jasanoff cannot be captured by her CV or fact sheets alone. Besides, and although she also holds an AB in mathematics, it definitely does not relate to statistics. It relates rather to her consistent critiques on linear thinking, it relates to the so-called tacit rather than the normative structure of science. And it relates to the questioning of words such as normative, structure and science altogether. But it also relates, I think, to the ways in which she embodies her talk. Whereas social scientists of different sorts may *talk* about speech-acts, or some others may *talk* about the gap between action and cognition, Sheila Jasanoff is *doing* co-production.

George Sarton cultivated the virgin soil of history of science, he is the founding father of this (sub)discipline. On the other hand, Sheila Jasanoff is one of the founding mothers of science and technology studies. Today, at Harvard's STS Circle, historians of science can meet STS scholars on a weekly basis. Also on many other occasions such as the annual meeting of the Science and Democracy Network, she makes significant efforts to "break the waves" and to bridge "the island of the what is" and "the island of the how it works". Moreover, by stimulating comparative and social-historical research she brings the reconstructions of our past closer and closer to the constructions of the present.

I would like to end this *laudatio* with a final, so-called, fact: Behind the intellectual, behind the scholar, behind the Professor Jasanoff, stands a great and warm person named Sheila. With her charisma and vision she has trained and "assembled" many young fellows, her fellows, to STS. What used to be a fallow became a cultivated land without borders, populated by scholars from different disciplinary backgrounds, with people from different countries, ranging from the United States to New Zealand and

Australia, from Brazil or Nigeria to England, from Italy and Austria to France and the Netherlands, and indeed also from Gent to Cambridge Massachusetts.

It is a great honor and pleasure for me to introduce Prof. Dr. Sheila Jasanoff as the Sarton Medalist of the faculty of political and social sciences 2011-2012 at Ghent University. The title of her lecture is: “Fields and Fallows: The Interdisciplinarity of Science and Technology Studies”. Prof. Jasanoff, on behalf of our faculty and the department of sociology, once again, a very warm welcome to Gent and many congratulations.

The Publics of Public Reason

Sheila Jasanoff
Harvard University

Connections

It is conventional to begin a lecture like this one, in these splendid surroundings, on a festival occasion, before an elite audience of one's academic peers, by acknowledging one's deep sense of being honored – and I want to begin by making that simple acknowledgment in a voice that I hope is unique and personal. It is a high honor to be standing before you today as the recipient of the Sarton Chair for the academic year 2011-12, in the unseen but felt company of all the distinguished chair-holders of the past 25 years. I would like to thank the University, the Sarton Committee, and the participating faculties, especially the Faculty of Political and Social Sciences, for making this moment possible; and to offer a special word of thanks to Raf Vanderstraeten for his generous laudatio. This recognition means a great deal to me personally, not just for myself and my small family of professors, who (though unable to be here) know how to value such awards, but for all the far-flung students and colleagues in science and technology studies whose work and inspiration have made my own achievements possible and led me here today to represent much more than myself.

An occasion like this not only honors the person for whom a chair is named and the person on whom the chair is bestowed. It is also a ritual of remembrance. We remember George Sarton, the accomplished son of Ghent who went on to build new worlds on the other side of a wide ocean, at a university where I myself, another traveler from afar, now hold a professorship in

the area of study pioneered by Sarton. Through the recurrent naming of chair-holders, as regular as the passage of the seasons, we remember that scholarship itself is a living chain, a transmission of knowledge and ideas along with an enduring faith in the value of making knowledge, a chain that connects human generations to one another as surely as do blood relationships or the imagined ties of ethnicity and nationhood. In my home university of Harvard, where Sarton lectured over much of the first half of the 20th century, recipients of the doctoral degree are welcomed each year "to the ancient and universal company of scholars." As a deracinated intellectual, at home under many flags yet not fully encompassed by any, I have always felt those words to be the closest ones I know to the benediction of homecoming. It is the ancientry and the universality of scholarship that we mark here today, and I am happy to play a modest role in this rite of memory.

As Harvard celebrates its 375th year, however, it is important to remember that universality is no static thing; nor indeed are universities or the disciplines they nurture. Fields of human concern change and develop, as do our ways of approaching unanswered questions with new modes of disciplined inquiry. The agenda for historians of science today is not, could not be, and should not be the same as that which motivated George Sarton when he received his own doctorate from this university 100 years ago, in 1911; or when he founded *Isis*, one of the field's flagship journals, and lovingly edited it from 1913 to 1952. Indeed, when the University of Ghent established this manner of recognizing Sarton 25 years ago, the first holder of the chair was Robert Merton, an eminent student of Sarton's to be sure, but in 1986 a man more affiliated with the sociology of science, and of scientists, than with the idealistic, almost utopian history of science that Sarton espoused – a preoccupation of which Sarton once wrote: "The history of science is the history of mankind's unity, of its sublime purpose, of its gradual redemption."[1] (p. 32) Merton already heralded what has since come to be accepted as fact by many: the humanistic aspirations of Sarton's history of science could not be fulfilled by keeping the study of science rigidly bound to the past; past and present would have to be brought into conversation. As a student of knowledge change and as a humanist of broad vision, I think Sarton would have appreciated this transformation in the field that he himself did so much to establish.

Inevitably, the study of science today looks very different from the discipline that Sarton sought to nurture through his beloved journal, an increasingly rigorous set of scholarly practices, and a stream of students. Although most of us who study science believe that human history, especially in the last 400 years, can be effectively written by following the tracks of science, much has happened to complicate that project. It is widely recognized, for example, that one cannot make sense of science without a coupled study of technology, that science itself is but a convenient shorthand for technoscientific practices too diverse to number, and that human "redemption," whatever that concept means, will not come from the production of science alone.[2]

What is more, we now recognize that understanding science is itself no encapsulated enterprise, a mere matter of taking a period in time and a body of specialist knowledge and excavating its evolution, or narrating its paradigm shifts, over decades or centuries. Science, in modern accounts, has become if anything more human than scholars a half-century ago thought it to be, but at once less unifying, less sublime, more messy, and more densely interwoven with what is good and what is deplorable in human societies. Most important for me, any attempt to reflect critically on the human condition today requires us to turn a critical eye on our relations with science and technology, and that reflection has to connect the history of science with the concerns of the present in ways that I would like to explore with you today.

Past and Present

A few words to start with about why I see the history, philosophy, and social studies of science (and technology as well, of course) – together constituting the enterprise of science and technology studies – as part of a seamless project of critical reflection, a project nourishing and being nourished by what we do with, and what we know about, science and technology in all their rich variation. The relationship between history and contemporaneity in particular has occupied not only historians but more importantly some of the profoundest philosophical minds for generations. There are those who would like to segregate the study of the scientific past in a self-contained bubble, like antiquarians carefully hoarding the relics of

a bygone world, hoping thereby to safeguard their role as official archaeologists, custodians, and interpreters of times and places inaccessible to common folk. Others, though, have long seen the past not simply as another country, shut off from the present through linguistic and cultural strangeness, but as a place or a condition whose investigation is intimately tied to an ethical obligation to think through our position in the world, whenever or wherever we stand in undertaking that inquiry. As my historian colleague Peter Dear and I recently wrote in *Isis*, "But history is also about understanding the continuities and processes of change that connect the past to the present. After all, we look to the past chiefly to answer questions and address concerns that arise in the present."[3] We moderns have our own things to say.

There is, then, a mutual ethical bond between those who inquire into the murk of history and those whose gaze is fixed on making sense of the equally recalcitrant problematics of the here and now. Writing a riposte to Kant's famous 1784 essay, "What Is Enlightenment?" [4] – precisely two hundred years after its publication and shortly before his own death – Michel Foucault couched that ethical duty in terms that simultaneously link past to present and history of ideas to the social scientist's burden of critique:

> … criticism [Foucault said] is no longer going to be practiced in the search for formal structures with universal value, but rather as a historical investigation into the events that have led us to constitute ourselves and to recognize ourselves as subjects of what we are doing, thinking, saying. In that sense, this criticism is not transcendental, and its goal is not that of making a metaphysics possible: it is genealogical in its design and archaeological in its method. [5] (p. 46)

Criticism is "archaeological – and not transcendental," Foucault continued, "in the sense that it will not seek to identify the universal structures of all knowledge or of all possible moral action, but will seek to treat the instances of discourse that articulate what we think, say, and do as so many historical events." And it is "genealogical in the sense that it will not deduce from the form of what we are what it is impossible for us to do and to know; but it will separate out, from the contingency that has made us what we are, the possibility of no longer being, doing, or thinking what we are, do, or think." Thus Foucault on genealogy, contingency, and the role

of history in unlocking the possibilities for forms of life other than the ones we actually inhabit and have made our own.

All this of course is music to the contemporary student of science and technology, who approaches the making of scientific knowledge with the same modest and skeptical attitude that Foucault brought to claimed historical universals: a situated gaze on particular events and transformations, a focus on discourses and practices, an emphasis on what people actually do and think rather than on grand theoretical claims. But Foucault's vision of critique as history – at once archaeological and genealogical – has in turn been criticized for not providing its own offsetting positive norms, a "how to" manual for breaking out of the debilitating constraints of modernity. Does science and technology studies, which extends the project of the history of science into the present, offer any useful normative vantage points, or modes of thinking; and, if so, can STS suggest ways of making more effective uses of the histories of science and knowledge? Does STS, a skeptical field, not fall into its own traps of relativism and negativity? What guidance can our stories provide for refashioning the world if things are always contingent, always complex and over-determined, offering no single, blinding moment of absolute Enlightenment? Foucault's essay provided powerful justification for critics of the contemporary world to revisit their histories. Does the study of the present hold similar ethical meaning for those who prefer to tell stories about the past, untouched by today's worries? These are some of the questions and concerns that inform my choice today to focus on public reason, or more specifically on the publics involved in constructing reason in the public sphere.

As a quick preview, I would like to put forward the thesis that STS turns around the past-present question as conventionally stated. Instead of asking how critics of the present or of modernity should excavate and deploy the past, STS scholars are asking how the structures of the present might lead one to make better sense of the dynamics of history. Beside the generative concepts of archaeology and genealogy, both practices of history, concepts that I want to put forward from STS's inquiries into the present are *architecture* and *design*, terms that – when applied to public reason – enable us to make sense of the built and constructed nature of the public sphere, including the invisible norms that hold it together, making political edifices that last.

Public Reason, or the Accountability of Power

Political philosophers from Plato through Foucault, Habermas, and beyond have dealt with certain recurring questions about the purposes and processes of governance: for example, how do we make better worlds; whence do we derive our senses of the good; are there transcendental norms; how can freedom coexist with authority; and is collective action possible without disenfranchising some voices and silencing some positions, maybe permanently, in the interests of achieving a higher justice or a superior rationality? To these common concerns studies of science and technology, or STS, have added a particular twist, both descriptive and normative. This is to bring the constructedness of knowledge more explicitly into accounts of public norms-making, asking both how we collectively know and why it matters how we know. Descriptively, STS demonstrates how new knowledge is gained, how it is certified, in what ways it is incorporated into, and how it sustains, particular forms of social order; and how, as knowledge is translated into material, technological forms, technoscience helps configure the social world into particular ways of seeing, moving, sensing, and thinking.[6] Normatively, STS confronts many of the ancient philosophical questions with issues of knowledge more securely built in. Do growing knowledge and increased power to control material things grant us more maturity to govern ourselves and our societies, or to achieve *Mündigkeit* in Kant's formulation; do science and technology provide a firmer basis for fairness and justice, as Rawls implicitly assumed? Or, by eternally gesturing toward their own contingency through processes such as Ulrich Beck's reflexive modernization,[7] do science and technology undermine the possibility of providing useful critiques of power, as Foucault's genealogical project has been accused of doing?

Public Reason as a Field of Inquiry for STS

Over thirty years of scholarship, I have found no richer vein to mine in addressing these kinds of questions than public reason. Let me first say a few words about what this concept means for me. It is not a set of abstract principles, or meta-principles, for determining how arguments should be framed in order to achieve political legitimacy. It is not an exogenous set of criteria by which we can determine whether a particular set of argu-

ments should be seen as more reasonable or better than others. I too eschew that sort of transcendental ambition. Rather, public reason is constituted from below in my brand of political investigation and theorizing, through the practical moves and strategies that people make as they rationalize how they govern themselves and consider how they might do it better. Public reason thus includes the technical discourses of public policy, from risk assessment to cost-benefit analysis and from game theory to bioethics; it includes legal and political practices, such as administrative law, rules governing the use of expert committees, and the new modes of public engagement that are springing up in many European countries. Public reason, in this broad definition – one that I think Foucault would have approved – encompasses its own technological instruments, such as public inquiries, consensus conferences, polls like the Eurobarometer, or surveys of the public understanding of science (PUS). Public reason spawns and keeps reshaping its own supporting disciplines, especially in the social sciences, as "enlightened" states seek objective, neutral, scientific foundations on which to build potentially controversial policies. A full-blown understanding of public reason therefore requires us to comprehend in depth the institutions that generate the knowledge of the reasoners, and the nature of the experts who disseminate reason to states and their subjects.

For me, then, the study of public reason is at once *reflexive*, meaning that part of the study is our own role as academics and scientists in producing the prototypes of reasoning; *constructive*, in the sense that it is the construction of reason that deserves our full attention; and *normative*, in that, through studies of public reason, we expose the values that we share as collectives, and thereby gain the critical distance to ask if these are indeed the values with which we wish to govern ourselves. I do not think that the sort of situated, critical inquiry into public reason that I advocate is neutral with respect to values. For one thing, this way of approaching reason admits that there is a value to the act of reasoning itself: an act that forces those in power to engage in processes of self-justification, and by inviting in the critical energy of the spectators, allows for a flowering of creativity, of liberated *Mündigkeit*. It becomes possible for oppressed groups to find in the edifices of reason uninhabited nooks and crannies in which they can insert alternative visions, and shake at their foundations the imaginations

of the mighty. Consider in this light the emancipatory discourses of the nineteenth and twentieth centuries – from liberty and equality to independence to women's suffrage, human rights, or rights for the disabled – that have changed forever who has standing to express and represent the self in the public sphere. Consider too, as a concrete example, the mode of self-expression that is unfolding before our very eyes the "Occupy Wall Street" demonstrations, an as yet inchoate practice of public reasoning that is creating international networks, producing new forms of collective action and communication, and appropriating physical urban space in an effort to overthrow the hegemonic economic imaginations of those who have traditionally governed the rules of global financial markets, free from popular accountability.

Observing the "Occupy" protests in action puts the spotlight on architecture and design, terms I want to add to our conceptual repertoire along with archaeology and genealogy. As I was crossing the ocean, I happened to see an article about the "Occupy London" protests. Headlines proclaimed that St. Paul's Cathedral was being forced to close its doors for the first time since World War II in reaction to the occupation. The decision was being taken, the Dean of St. Paul's announced, for reasons of "health and safety." Various officials spoke of fire hazards, dangers to visitors, and even unmanageable rodents. Now, many micro-histories could no doubt be written about the technical aspects of creating tent cities and encampments in areas without the basic amenities (food, shelter, running water and sanitation) that make life livable in modern cities. But the interest of the St. Paul's story from the standpoint of public reason lies in the way it made the designs of modern statecraft visible and accessible for critique. For here in miniature were replayed some key legitimating moves of British political culture, particularly its presumptions of expertise, that we know from other contexts and other studies [8]: the mutual reliance of church and state; the deep-rooted self-image of the British state as protector of public health and safety; the use of empiricist discourses of common sense in delineating risks to social order; and the invocation of a historically sanctioned concern with fire as an always-already present risk to safety – especially at the beating heart of London's symbolic and spiritual city center.

This little case also illuminates the larger architecture of modern public reason in ways that we are primed to investigate, even though the Occupy

movement is in its infancy and may pass into history as a mere blip, accomplishing nothing immediately. We see important lines to which STS studies have made us attentive: that politics is performative, and that new modes of reasoning can arise in the spaces of theatricality offered by political processes; that the boundary line between public reason and private action is not given in advance, nor fixed for all time, but can be shifted through new forms of resistance and self-assertion; that spatial and material structures channel power (especially in cities), and their maps can be redrawn in the process of making power accountable; that established discourses of reason are hard to bypass or overthrow because they are engrained into institutions and identities, from a nation's highest bishops to its most humble police and fire forces; and that there is a deep structure of connection among space, materiality, and dominant forms of reasoning that needs to be tackled in the round in creating the preconditions for significant or revolutionary change.

Expertise and Publics

This set of reflections on a small contemporary event illustrates, concretely and empirically, some of the ways in which I think a critical commentary on the present needs to stay in animated, and animating, conversation with history and philosophy. For the remainder of this lecture, I would like to return to a more abstract consideration of the critical project of public reason, and how we might theorize its architecture further. I want to do this by sketching some lines of thought around a dyad that defines in my view the critical axis for all contemporary analyses of public reason: experts and the public. I would like to reflect on the ways in which modernity has constructed expertise, how that condition, or state, of specialized authority is bounded off from lay modes of knowing, and how publics both make themselves and are made so as to function as political forces in contemporary democracies of knowledge and expertise.

I recently attended a workshop on democracy and scientific authority that brought together political theorists and science studies scholars in an effort to address those issues. The organizers had identified four ways in which science, technology and expertise have become problematic in recent years: moral politicization; politicization through risk; politicization

through challenges to expert authority; and politicization through corruption. Though each of these pathways is significant for politics, I felt that they ought not to be seen as "distinct." All four, at bottom, have to do with questions about who has authority to deploy the power of science and technology to govern people's lives; all four thus revolve around the place of experts in a democratic political order. Each mode can be associated with a question about the legitimacy of expertise: is expertise being deployed consistently with a society's moral values; is it keeping society safe from harm (i.e., fulfilling government's basic mandate to secure the lives of citizens); is it sufficiently answerable to the public; is it unbiased and not captive to special interests?

The answers that scholars have given to these sorts of questions together constitute an emerging political theory of government by experts. Developing that theory is one goal that STS should pursue more systematically through its work of political analysis and synthesis. Even my oeuvre, after all, can be seen as more episodic and issue-focused than coherent or systematic. Thus, I have looked at the normative foundations of decision-making by expert committees[9], the role of the courts in creating rules of expert accountability (and thus the relationship between law and politics)[10], the basis for differences in national approaches to assessing risk, and the emerging practices of expertise in global governance – among other issues. Others in STS have written about the nature and efficacy of specific types of expert organizations, methods for securing public participation, the role of discourse in framing scientific or technological problems and building collectives, and the value of lay expertise, citizen science, or other forms of epistemic resistance. All of this collective learning and thinking can be consolidated under the rubric of "legitimating expertise," though it is important to ask where there is consensus and where there are divisions. A few years ago I might not have seen the divisions as worth noting, but lately conflict has developed even among STS analysts of expertise over how far societies should go in making expert processes transparent and participatory. At stake here is a new figure – at least one relatively new to STS – and that is the *public* in whose interest policy is made and whose acquiescence is needed in order to make policies democratic.

Though I've contributed volumes (literal and figurative) of writing to the topic of legitimating expertise, the line I would like to pursue today is, in

my terms, more expressly co-productionist. Let us accept at the outset that creating any kind of epistemological settlement – whether at the level of a single fact or an entire cosmology – entails solving or resolving associated normative questions. How then does the bounding off of science and technology as domains of specific types of expert authority co-produce the kinds of worlds in which such power has merit, makes sense, and is entitled to rule? More particularly, what roles do science and technology play in constructing *publics*, those amorphous entities for whose benefit expert logics are deployed in the (democratic) public sphere? In order to justify rule by experts in democratic societies, how is the "demos" itself theorized and studied? I want to ask what (express or tacit) assumptions are made about publics concurrent with (express or tacit) assumptions about experts. What tools have science and technology provided to examine and evaluate publics, and indeed to define how a democratic society should think about itself and its self-governing capabilities? Relatedly, to whom are the particular types of experts who make knowledge about publics and their natures accountable, and how? Is there an ethical obligation of "informed consent" before anyone presumes to characterize human nature and its collective dimensions; and how should such an ethical imperative be articulated in a democracy?

Needless to say, I am sketching here a *big* analytic project for STS, one wholly beyond the scope of the short and necessarily schematic conversation opener of a public lecture. This is a programmatic undertaking for a field. To keep my reflections here within bounds, I will restrict my observations to a few issues and ideas about publics that have emerged from ongoing work in science and technology studies.

STS has a long and rich history of reflecting on the politics of expertise, though not an equally long history of synthesizing its own contributions to this subject, especially as regards the nature of publics. Back in the 1970s, in the heyday of the new social movements (anti-war, environmental, feminist, consumer), "public participation" was the issue that people considered most interesting at the so-called interface of science, technology and society. Participation by citizens was seen as the antidote to wrong turns taken by advanced industrial nations through destructive warfare, environmentally unsustainable development, and unequal distribution of technological risks. Today, "public participation" has largely been replaced by

"public engagement" as the term of interest in STS. In conventional political science terms, we might say the field's agenda has shifted from public participation to public engagement. On the surface, the difference is negligible: only one word. Yet, in my view, there has been a profound shift in thought and inquiry marked not so much by the word that has changed but by the one that has not. For the "public" of the newly consolidated discipline of science and technology studies is not the same as the "public" of the more loosely structured field that was once called (and in some places still is called) science, technology and society.

So what has changed? It has become fashionable these days for STS scholars to refer to the Dewey-Lippmann debates of the 1920s in discussions of public engagement, and to point out (correctly, I think) that STS's sense of the public corresponds to the Deweyan ideal of an educable polity that forms around what Bruno Latour has called "matters of concern."[11] The implication is that the older debate about the nature of the public has been forgotten and we need to remind ourselves of its main points. But a genealogical perspective reveals that, in the *practices of democracy* (a more capacious concept than public engagement), the 1920s debate never really went underground. Which understanding of the public ought to underlie policy initiatives, and what is the evidence that we are getting our characterization of the public right? Is the public in Kantian terms capable of being *mündig*, is it willfully *unmünidg*, or does it only need education to achieve *Mündigkeit*? There are radically different conceptions at play about whether the public has the capacity to know enough to govern itself well; and the human and social sciences are contributing to very different constructions of publics and their knowledges. This contested disciplinary terrain, where alternative publics are continually under construction, is perhaps the most interesting site at which to observe and analyze key elements of the architecture of public reason.

For me personally, it has been most useful to explore these ongoing conflicts from departure points in the law. An instructive historical starting point for American law is 1946, the year that the US government acknowledged for the first time that technical decisionmaking needs new forms of legitimation, by going public. The Administrative Procedure Act of 1946 changed the presumptions underlying the increasingly prevalent rule by experts in the New Deal. Crucial here was the acknowledgment that

unchecked power comes as much from presumptions of superior knowledge and reason as it does from imbalances in status, wealth, or weapons. Bravely, a generation of US lawmaking and legal interpretation set about to fix the imbalance, not only in access to knowledge as pure market enthusiasts have urged, but also (especially through the work of federal courts) in the state's stance of superior knowing-ness or expertise, a far more radical undertaking. The results were deeply consequential, indeed, quasi-constitutional according to some legal analysts[12]; the Administrative Procedure Act and successor statutes launched a struggle for authority between expert and public knowledge that has not diminished with time.

The APA exhorts US executive agencies to inform, guide, make documents available to, and solicit comments from the public. Beginning with that law and carrying right on through such statutes as the Emergency Planning and Community Right to Know Act of 1986 (passed after the tragic 1984 gas disaster in Bhopal, India), the US Congress continually acted on behalf of an imagined Deweyan public[13] – the intelligent, educable polity that Dewey advocated for when he took issue with Walter Lippmann's despairing characterization of the public as essentially a phantom, incapable of informed self-governance, and easily led astray by the distortions of the media. A chronically distracted, lazy, ignorant and *unmündig* public. Oblivious to that philosophical debate, Congressional enactments repeatedly affirmed the notion of what I have called a knowledge-*able* public, with broadly defined epistemic rights, comprising individuals capable of understanding, absorbing, and weighing information, and holding their government to high standards of reason. And, in a co-productionist turn of great consequence, Americans actually behaved as the law contemplated: the growing vitality of non-governmental organizations and social movements throughout the 1970s, around highly technical issues of health, environment, discrimination, or product safety, for example, offers ample evidence. It mattered, in other words, how the law imagined publics. The law functioned almost like a social science, producing "interactive kinds" who shaped their behaviors to fit the law's presumptions, as described by the philosopher Ian Hacking.[14]

But what sorts of publics did the law help mobilize? STS studies of technological controversies have convincingly shown that the contemporary democratic public is not a single, faceless, amorphous entity, but instead a

series of shifting collectives that form and reform around new scientific constructs such as the gene, new technological objects such as computers or the Internet, new epistemes such as disease entities, climate change or animal rights, and new sociotechnical projects and imaginaries, such as human enhancement, the "doubly green revolution," or the eradication of chronic or infectious disease. This seems empirically consistent with the presumptions of the Administrative Procedure Act, which also imagined shifting groups of interested and affected parties around changing issues, but there are important differences between that law's tacit assumptions and documented public behaviors in relation to science and technology.

The animating architectural model for the APA was arguably not the *political* agora but – consistent with American political culture – the agora's *commercial* counterpart, the marketplace. The participatory legislation of the postwar period adopted the conventional American game plan of leveling the playing field between experts and lay citizens through provisions for more open information, explanations, and reasons. Citizens were seen as stakeholders, with pre-formed interests, needing only the resource of open information to actualize their democratic desires. Subsequent research has "thickened" that understanding of lay publics as rational actors, needing only the "public good" of information to actuate their preferences. We have seen that collective preferences are not formed in advance of seeking information but are tied up with identities and self-understandings which may morph along with innovation in scientific concepts and technological objects. To take an obvious example, people may not feel that they "need" a health intervention till a new technique reveals that they are ill or potentially ill. We find no fixed stratification between, say, the "attentive" and "inattentive" publics whose "understanding of science" was assiduously measured by decades of survey research commissioned by the US National Science Foundation. We encounter instead a vast array of potential publics, ready to refashion their identities and engage in forms of collective action and self-definition when new knowledge catches at the fibers of their being.

What these fluid publics want is not always what governments think they ought to want. This has opened the way not only to disputes about policies such as reducing greenhouse gas emissions but to "scientific" conflicts about publics themselves. Contemporary media accounts of technical

controversies often harp on public ignorance and illiteracy, blaming resistance to state policies (for example, about climate change), Lippmann-like, on the public misunderstanding of science, fed by corrupt, industry-generated pseudo-science and distorted journalistic reporting. STS scholars have struck back, Dewey-like, calling this the "deficit model" of the public[15] – a model that infantilizes, deskills, and disempowers what I have called knowledge-able publics. STS case studies have repeatedly demonstrated that public questioning of expert authority derives from unresolved questions of value and trust, questions that should not be decided without democratic engagement and deliberation. What looks on the surface like second-guessing or denying an expert safety evaluation often has roots in legitimate ontological and ethical uncertainty. People wonder not only whether a thing is safe, but also if the issue of safety was framed right to start with, whether non-knowledge received as much attention as claimed knowledge, and who (against the backdrop of prior sad experience) will be responsible for failure or catastrophe if it occurs. For new and emerging technologies, is the project of innovation worth doing at all, or only with different aims, or after more experimentation, or on a more modest scale, or under different supervision? And will these technologies deliver on their often over-hyped promise?

Understood in this light, STS scholarship helps to define a middle ground between two dominant views of the public that political theorists often render in oppositional terms: the liberal or "economist" view of the public as an aggregate of individuals bound together by well-articulated interests and preferences; and the communitarian view of publics united by adherence to supra-arching shared norms derived from religion, nationalism, or other powerful cultural codes. Missing from those political theory debates is a serious engagement with the fact that today's publics are members of advanced industrial (even post-industrial) societies, with their wants, needs and possibilities ineluctably shaped by developments in science and technology. STS scholarship recognizes that we share our world with *modern* concepts, things, and entities that shape our sense of self and other, individual and community, citizen, polity, and nation. As a result, publics are in practice more grounded, actual, plugged-in, and "real" than the abstract subjects posited by political theory or the detached aggregates sampled by survey research on the public understanding of science. Publics, so seen,

are also more knowledgeable than the percentages of non-knowers measured by old-fashioned PUS studies.

The publics of concern to STS researchers, however, have tended to be those that form in relation to issues and projects that are already demarcated as scientific or technological, and STS studies focus by preference on controversies around things that are emerging or in production. Accordingly, and not surprisingly, much STS research emphasizes how publics act as researchers in the wild, citizen scientists, lay experts, patient advocates, and the like. These characterizations restore a Dewyean capability to publics, but they run the risk of missing deeper struggles for authority in the contemporary politics of science and technology. STS's public sphere looks in these analyses like a place for weighing alternative propositional claims at the expense of foregrounding accompanying conflicts about virtue, rights, power or justice. It yields too thin a picture of politics. In discussing the Occupy Wall Street movement earlier, I wanted to expand the narrowing tendencies within my own field, and to show how we can ourselves more fully occupy the territories of political and social analysis.

At the same time, a new spate of work has emerged from within as well as outside STS that warns against exaggerating the wisdom of the multitude. A colleague with whom I recently discussed these issues wrote: "From laetrile to global warming to creationism, there is enough evidence of publics taking rather problematic positions that we can't only be Deweyan about them. Somehow the trick has to be to make the space for public engagement, but not to assume that the public, or rather all the publics, will come to make good judgments, or at least judgments that we might want to concur in." This then is a criticism that comes from friends and foes of STS, It needs to be taken seriously in refining our conceptions of scientific authority and democratic politics. It is consistent with critiques of STS for being too relativistic and of Foucault for being, ultimately, too descriptive and not adequately normative.

In partial reply, I want to suggest that we have to broaden our concern with publics well beyond what publics know about specific issues to how the very idea of a public functions within the established designs of power. If we do this, then we necessarily have to take on board not only how publics arrive at matters of concern, but also how publics are constructed by those who govern them, and with what results for inclusion, deliberation, reason,

and policy. At this moment, for example, arguably the most radical rethinking of the US public sphere in a century is coalescing through developments in three domains: neuroimaging of the brain, experiments in social psychology and behavioral economics, and a school of policy thinking attuned to "correcting" for human biases and heuristics though powerful "nudges" that help steer people toward making rational choices.[16]

The most famous success story of the nudge theory to date is a urinal in the Amsterdam airport which, in polite language, simplified the lives of Dutch sanitation workers. We even have quantitative measures that tell us how effective the nudge was! The key element in the Amsterdam experiment was a tiny, etched image of a fly in the bowl. It disciplined men's bodies to behave better. When asked to explain the phenomenon, Richard Thaler, co-author of the nudge theory together with Cass Sunstein, said, "I'm sure there's an evolutionary explanation for this, if you give them *[he meant men]* a target, they will aim."[17] To spell out the implications more fully, a state that knows how people ought to behave, empowered by an evolutionary theory that suggests why people are flawed thinkers and actors, should feel enabled to condition behaviors, and bodies themselves, on the basis of its (tacit) theorizing of human nature. This is not just a matter of "seeing like a state," to borrow James C. Scott's famous title.[18] The painted fly, trivial in itself, is emblematic of an all new regime of governmentality.

The evolutionary logic has proved very attractive to policymakers, as I can attest from conversations at the Harvard Kennedy School. It makes great sense for experts to think for, and hence to govern, publics who appear biologically ill adapted to interpret evidence or to reason well. This imaged and experimentally validated biologizing of the public, so quickly taken up in policy discourse, is an astonishing development in democratic theory and practice. Yet, in all the welter of STS work on public engagement and the deficit model, we do not as yet find compelling accounts of the implications of nudge theory's particular construction of the human, let alone the constitutional implications of generalizing this way of thinking to all dealings between governments their publics. Should urinal studies be extended to elections for example, nudging people toward voting, and then perhaps toward correct voting…? One reason for this deficit within STS may be a dearth of creative partnerships between social scientific and historical

thinking, for a historian would instantly remind us that the human sciences have long been engaged in constructing human nature in ways that are in synergy with the power structures of the state. The challenge for us critics of the present lies in understanding what kinds of design shifts have been enabled or suppressed in today's partnerships between particular institutional legacies, particular technoscientific developments, and particular cultural modes of public reasoning.

Further, getting into the public sphere through technoscientfic controversies as STS scholars have tended to do may desensitize political theory to the normative presumptions that shape public reason and the deep structures of power and culture within which reason operates. For evidence and proof, logic and justification do not follow a single universal prescription. Indeed, as I have often argued, differences between the West and the West may be as striking as those between the West and the rest. At the same time, the naturalized workings of the market may condition both global and local constructions of publics in ways that situated, case-specific controversy studies cannot hope to excavate through sheer archaeology.

To revitalize democratic theory in the age of technoscience it seems clear that we have to engage with articulations of the public that are both empowering and consistent with STS insights. Yet the publics constructed by opinion surveys, focus groups, and deliberative polling – not to mention the minds "revealed" through trolley problems, brain imaging, and neuroeconomics – seem much more tractable to decisionmakers than the publics and modes of reasoning detailed through careful historical and social analysis. This poses to the field of STS questions about its own strategies of demonstration, since without the capacity to demonstrate, theories often lack power to persuade. How can we, as contemporary theorists of both scientific and political authority, establish the value of seeing potential publics in the ways that we do see them, complex in their thinking, open in their receptivity to new knowledge, and responsibly engaged in their own self-fashioning?

Conclusion

I would like to conclude by returning to the big questions of normativity I raised earlier in the lecture. I hope I have shown why the publics of public

reason should be a major subject of analysis in the historical, philosophical, and social studies of science and technology. I hope also to have made a plausible, if not wholly compelling, argument for why a more active, symmetrical partnership is needed between historical and contemporary studies of science and technology in modernity's ceaseless ethical quest to know and to understand itself. I hope I have also put forward a thesis worth considering for why denying a transcendental position on what is to be done is not inconsistent with having principles and acting on them in a heterogeneous global society.

The transcendental norms for me are not ones that directly govern policy or redefine the power of institutions from one day to the next. They are longer, more enduring norms about the pursuit of knowledge, the importance of maintaining safe spaces for inquiry and self-expression, the values of dissent and reflexivity, the need for epistemic charity and for efforts to understand one's colleagues and fellow citizens across tribal divides that sometimes seem unbridgeable in the extreme. Nor do I feel the need to attribute universality to the norms I cherish. Yet an occasion like today's, in this auditorium of memory, is a moment to savor because, however temporarily, it dissolves the differences among us. It gives life to the Kantian mandate *sapere aude*, and for a fledgling moment it makes it seem as though all things are thinkable, all conversations are possible, and redemption can be attained if we only keep trying.

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SARTON MEDAL LECTURES

Laudatio Seán Donlan

D. Heirbaut

The professors of the Ghent Legal History Institute have in the past suggested several names for the honor of being awarded a Sarton Medal. In that company Seán Donlan is exceptional, not because he has already achieved so much, as have his predecessors who received the Sarton Medal, but because he is still relatively young. Moreover, he did not begin as an academic, but once he began working at the university, he did so with a vengeance, taking on several subjects. Seán Donlan is an American, even more he is from the deep South, so one could easily imagine him focusing on his own country. In fact, he has published on American constitutional law and American constitutional history. However, he is a native of Louisiana, which is unique in the United States because its law is French and Spanish as well as English in origin. His position is even more particular, because his corner of present-day Louisiana combined Spanish laws and Anglo-American customs at the time of its addition to the state. In those circumstances it is easy to understand that Seán Donlan directed his study of law towards comparative law and comparative legal history. Hence, he teaches not only methodology of law, legal theory, and public law, but also comparative legal systems, comparative public law, and legal history.

As one of the leading advocates of comparative legal history, Seán Donlan easily moves from comparative law to legal history and back again. His legal history transcends national borders. It does so not by putting forward the idea of a *ius commune*, a common law of Europe, at the detriment of anything else, but by promoting the concepts of legal 'hybridity' and 'diffusion', which come much closer to the historical and current realities. There is no such thing as a pure and unadulterated Roman law, common

law or customary law. In reality, legal systems are always mixed, combining legal traditions with sometimes very different origins. Whereas the idea of mixed legal systems as such is not new, Seán Donlan has, from his study of today's combinations of continental and Anglo-American legal traditions, gone on to study legal mixtures and movements in the past. His publications, either in print or forthcoming, deal with a theoretical framework for analyzing legal hybridity in the past or present.

As an American of Irish descent working in Ireland, the problems of Anglo-Irish law have been of particular interest to Seán Donlan. He has edited several books on law in Ireland in the era 1689-1848. In fact, his particular subject of study has been Edmund Burke, a man who managed to reconcile characteristics which are now seen as opposing and excluding one another: conservatism and liberalism, Irishness and Englishness, etc.. Thus, Burke is not just an object of Seán Donlan's research, he is a personification of hybridity, as he was himself well aware of the hybrid character of England's laws. Because Burke and his work show so many different sides, he has been a constant source of inspiration for Seán Donlan's articles, which now can be seen as a small library on Burke. His talent as a biographer of Burke has been recognized by specialists of Burke and by biographers in general. Hence, he has also written almost thirty biographical articles on other remarkable eighteenth-century persons, including England's most famous lawyer of the period, William Blackstone.

It would be wrong to consider Seán Donlan's contribution to science only through his publications. He is also a very active participant in international conferences and a highly respected member of several academic bodies. In Ireland, he has been Editor of the *University of Limerick Law Review* and has served as Secretary of the *Eighteenth Century Ireland Society*. He is on the Executive Council of the *Irish Legal History Society* on one hand and General Secretary of the National Committee for Ireland with the *International Academy of Comparative Law*, of which he is also a member, on the other. Most of all, Seán Donlan is best known as the founder of new organisations. In Ireland, he founded the *Irish Society of Comparative Law* and serves as Vice President of that association. In 2009, he was the co-founder of two new international societies: *Juris Diversitas*, which focuses on legal traditions crossing national boundaries, and the *European Society for Comparative Legal History*. It is a tribute to his personality that he, as an

American, succeeded in doing what we Europeans could not do ourselves: establish the European equivalent of the *American Society for Legal History*. Seán Donlan still has a long and fruitful career ahead of him, but even if he were to leave the academic world today, his publications and these societies would ensure that he has an enduring legacy.

Whatever Seán Donlan does: teaching, writing, taking part in and presiding over meetings, or just greeting one of his many international friends and colleagues (in his case, the words are interchangeable), his enthusiasm always catches on. Meeting Seán Donlan is always a special experience, because one knows that before the meeting is over, one will cheerfully have consented to contribute to the next great and eagerly awaited project. Seán Donlan's role sometimes receives less recognition then it deserves, because he is a humble man who does not always want to take the credit for an initiative which without him would never have come about. Therefore, it is fitting that the Ghent Law Faculty has chosen to put his name forward for a Sarton Medal and that the Sarton Committee has decided to follow its advice. The following text on Edmund Burke amply proves that he is worthy of that honor.

‘The law touches us but here and there, and now and then’: Edmund Burke, law, and legal theory[1]

Seán Patrick Donlan
University of Limerick

Manners are of more importance than laws. Upon them, in a great measure, the laws depend. The law touches us but here and there, and now and then. Manners are what vex or soothe, corrupt or purify, exalt or debase, barbarize or refine us, by a constant, steady, uniform, insensible operation, like that of the air we breathe in. They give their whole form and colour to our lives. According to their quality, they aid morals, they supply them, or they totally destroy them.

E Burke, *Letters on a regicide peace,* Indianapolis, Liberty Fund Press, 1999 [1795-7], 129.

Edmund Burke’s training in, knowledge of, and appreciation for, law is generally recognised. Indeed, as RB McDowell has written, while Burke

> may, during short bouts of irritation, have impulsively expressed intense exasperation with lawyers, their practices, procedures and prejudices, [but he] nevertheless remained convinced that the law, with all its limitations, must be regarded with reverence and that lawyers, with all their faults, performed functions of the utmost value to the community.[2]

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1 This paper is an extended version of SP Donlan, ‘Burke on law and legal theory’ for C Insole and D Dwan (eds), *The Cambridge companion to Edmund Burke* (*forthcoming*, Cambridge University Press, 2012). Readers will find additional information in the notes there.

2 McDowell, R.B. ‘Edmund Burke and the law’ in D.S. Greer and N.M. Dawson (eds.), *Mysteries and solutions in Irish legal history: Irish Legal History Society and other papers, 1996-1999*, Dublin, Four Courts Press, 2001, 113.

But the Irishman's extensive use of legal language obscures how little he actually said or wrote about legal theory. As in other areas, the numerous volumes of Burke's correspondence, writings, and speeches, cannot provide us with a clear and rigorous theory. His comments on the law typically took place in specific and complex, rapidly changing, political controversies: eg, the exclusion of John Wilkes from Parliament, the American war, the impeachment of Warren Hastings, the regency crisis of 1788-9, or the revolution in France. These reflections are often little more than the *obiter dicta* of political and public debates. They should not be confused with more formal treatise or commentaries on the laws. Instead of proscribing our interpretations to narrow limits, Burke's texts have invited considerable special pleading. This cannot absolve commentators from reasonable evidentiary demands and coherence, but considerable interpretive liberties are inevitable. Burke's words, whether in private correspondence or public commentary, require difficult choices to be made between literal and liberal interpretations, between letter and spirit. There are inevitably lacunae to be filled and reconstructed. Interpretive prejudices may be unavoidable. As a result, 'Rescuing Burke' from early interpretations is an ongoing affair.

As with English nationalists and political conservatives, Anglo-American lawyers have been quick to claim Burke as their own and to employ him in present debates. If these readings may be valuable in themselves, whatever their historical accuracy, they reflect at least two related problems. The first is an historiographical failure to appreciate the circumstances in which Burke wrote. The second is the hermeneutic problem of interpreting words from these past circumstances for present purposes. This approach seems quite foreign to Burke's careful attention to context. It is, however, all too common for lawyers. Treated, insofar as is possible, in their own terms, Burke's texts suggest a picture that is often at odds with common assumptions about him and the law. The Irishman's opinion of English jurisprudence is, for example, complex and not wholly complimentary. Especially in his early pre-political writings, Burke's jurisprudential asides presented a challenge to 'vulgar whiggism' and insular English and common law histories. His parliamentary statements also suggest that he emphasised the centrality of the legislature rather than, as is often suggested, the courts of common law. Perhaps most importantly, Burke's frequent use of legal

terms – contract, partnership, prescription, rights – is largely rhetorical, built on his wider understanding of morals, manners, and history.

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As there was then no place for legal training in his native Ireland, Burke left to study law at London's Middle Temple at the goading of his father, himself a lawyer with the High Court of the Exchequer. The younger Burke did not take immediately to his studies. He flirted with the idea of a literary career before determining that he felt 'comfort that tho a middling Poet cannot be endured there is some quarter for a Middling Lawyer'.[3] As it turned out, Burke left his legal studies without entering the bar. He began instead a writing career. In 1756, he published *A vindication of natural society*, parodying Lord Bolingbroke, the 'country' tory historian and deist. 1757 saw publication of *A philosophical enquiry into the origins of our ideas of the sublime and beautiful*, an empiricist aesthetics providing, in effect, a 'natural' foundation for 'artificial' society. The same year, there appeared Burke's collaboration – with William Burke, a friend he met at the Middle Temple – on *An Account of the European settlements in America*, a comparative history and ethnography of the new world. Elements of Burke's thought are consistent with the 'common law mind', the corporate, cumulative development of law over time. But there were similar, equally important sources – the culture of politeness, latitudinarianism, civic thought, and comparative and philosophical histories – that Burke imbibed long before his legal studies. The progressive 'wisdom of the ages' was inherent in contemporary empiricism and what he called, in the *Enquiry*, a 'more extended and perfect induction'.[4] This resembles the adjudicative growth of common law, but has as much to do with the corporate growth of science. Here as elsewhere, abstract ideas and general principles played a guiding though falsifiable role, without which 'all reasonings … would be only a confused jumble of particular facts and details'.[5] This is not, however, the simple induction of principles from

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3 Burke, E. *The correspondence of Edmund Burke,* 10 vols., Cambridge, Cambridge University Press, 1981-, T.W. Copeland (gen. ed.), i.111.

4 Burke, E. *A philosophical enquiry into the origins of our ideas of the sublime and the beautiful,* Oxford, Oxford University Press, 1990 [1756], 4.

5 Burke, E. *The works and correspondence of the Right Honourable Edmund Burke*, 8 vols., London, Francis and John Rivington, 1852, vi.101.

particular cases. Such principles, broader than the rules or rights of law and more flexible to circumstance, were one of the defining features of European, especially continental, jurisprudence.

Burke developed deep reservations about the narrowness of the legal training of the day and the quality of the public men it produced. Legal education amounted then to little more than attendance in the courts of Westminister and dining with practicing attorneys. It was, he later wrote, a 'narrow and inglorious study'.[6] A graduate of the University of Dublin's Trinity College, Burke emphasized instead the importance of a liberal education for those entering the law. In this, at least, he appears to have been in agreement with William Blackstone, whose *A discourse on the study of the law* (1758) made a similar point. As editor and contributor to the *Annual Register*, Burke actually appears to have reviewed the *Discourse*. He thought it a 'solid judicious and elegant oration … for putting the study of [law] under proper regulations, and spirited persuasive to make that study so regulated, a considerable part of academic education'.[7] The *Discourse* served as Blackstone's introduction to his Oxford lectures in 1758, the first in the English common law, and his subsequent *Commentaries on the laws of England* (1765-9). For Burke, an enlightened jurisprudence had to go beyond law, both pedagogically and philosophically.

By the late 1750s, in addition to his successful publications, Burke had begun to develop a rich web of friendships with many of the leading intellects and artists of the age. This included, for example, both Samuel Johnson and James Boswell in the 'Literary Club'. The former secretly assisted Robert Chambers, Blackstone's successor at Oxford, with his law lectures; the latter was trained as a Scottish advocate. Far more important for understanding Burke's thoughts on law and legal theory, he wrote, but never published, two important works on history and law in the late 1750s and early 1760s. An *Abridgement of the English history* (c1757-62), completed through the Magna Charta, and a short fragment on English law (c1757) may be the most informative of his texts.[8] Both show him deeply

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6 Burke, E. *The writings and speeches of Edmund Burke* (10 vols), P Langford (gen. ed.), Oxford, Oxford University Press, 1997-, i.323. See Burke, *Correspondence* i.111.

7 *Annual Register*, 1758, 452, 452.

8 These are included as 'Fragment. – An essay towards an history of the law of England' and *An Essay towards an Abridgement of the English history* in *Writings and speeches,* i.322-31 and i.332-552.

critical of English exceptionalism and insularity. Whatever the virtues of early England, the Saxons were a 'rude and barbarous people' – a trope long in use with the Irish – whose 'liberty' was license and anarchy.[9] In a climate that glorified the insular and immemorial nature of English law, Burke wrote that

> the present system of our laws, like our language and our learning, is a very mixed and heterogeneous mass, in some respects on our own; in more borrowed from the policy of foreign nations, and compounded, altered, and variously modified, according to the various necessities which the manners, the religion, and the commerce of the people have at different times imposed …[10]

Against contemporary party histories, Burke highlighted English improvement through its social commerce or 'communication with the rest of Europe'.[11] It is important to note that Burke's opinion appears solidly whig, though not 'whiggish'. His view was firmly rooted in the establishment political whiggism of mid-century. These establishment whigs, like the Rockingham whigs, were moderns. They sought to undermine the potentially radical histories of tory writers who had themselves adopted the 'vulgar whiggism' of the 'ancient constitution'. The country tory Henry St John, lord Bolingbroke, the target of Burke's *Vindication*, was thus also among the most whiggish historians. Burke's emphasis, with seventeenth-century jurists like Robert Brady and John Selman, is instead on the degree to which English law was European. Burke identified the 'three capital sources' of legal influence as the 'ancient traditionary customs of the North', the 'Canons of the Church, and 'some parts of the Roman civil law'.[12] In this and other ways, he is, contrary to superficial analysis of his texts, at odds with Blackstone as well as the seventeenth-century English jurist Matthew Hale.

By contrast to the ancients, the liberty of the 'moderns' came from the increase of state powers, by the very distance of government from the governed. It eroded the power of local nobility, contributed to the modernisation of many feudal holdovers and an increased social mobility, particu-

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9 *Writings and speeches,* i.430.
10 Ibid., i.325.
11 Ibid., i.453.
12 Ibid., i.331.

larly as offered by greater levels of social and financial commerce. In jurisprudence, this is seen most clearly in the increasingly insistence on a distinction between 'perfect' juridical-political duties (or rights) backed by public sanctions and deemed indispensable for any constituent social order and the 'imperfect' demands and institutions left over, the organs of beneficence (or benevolence). This was a change of some significance to social thought. As an important source of self-understanding, the increased universalism of the state suggested a more subjective and autonomous, indeed legalistic, concept of the individual. By these developments, those social practices and institutions remaining outside the state were thrown into relief. Laws, with their attendant sanctions, were increasingly distinguished from manners and norms. The growing strength of commerce as a 'power' independent of the state only strengthened this tendency.

Where Blackstone, a 'whiggish' tory, blamed the Normans for the corruption of English liberty, Burke saw the conquest as joining England with the wider progress of society in Europe. The Irishman was specifically critical of Hale for failing to note 'the great changes and remarkable revolutions in the law' over time and for fostering the idea that it was simply 'formed and grew up among ourselves'.[13] Burke's account was also not a mere jurisprudential history. With the Scots, he maintained that 'the changes, … in the manners, opinions, and sciences of men … [are] as worthy of regard as the fortunes of wars, and the revolutions of kingdoms'.[14] Linked to European manners, Burke saw the development of English, and European, law as progressive. 'What can be more instructive', he wrote:

> than to search out the first obscure and scanty fountains of that jurisprudence which now waters and enriches whole nations with so abundant and copious a flood – to observe the first principles of RIGHT springing up, involved in superstition and polluted with violence, until at length of time and favourable circumstances it has worked itself into clearness. The Laws sometimes lost and trodden down in the confusion of wars and tumults, and sometimes overruled by the hand of power; then, victorious over tyranny, growing stronger, clearer, and more decisive by the violence they had suffered; enriched even by those foreign conquests, which threatened their entire destruction; softened and mellowed by peace and Religion; improved

[13] Ibid., i.323, 323.
[14] Ibid., i.358.

> and exalted by commerce, by social intercourse, and that great opener of the mind, ingenious science?[15]

Burke's modern, evolutionary and progressive view was very different from that of many of his contemporaries. Even Montesquieu, 'the greatest genius, which has enlightened this age', was not above criticism in the texts.[16] Burke suggested, too, that history and the historical method was an important element in a liberal education. The same idea was strongly advocated in Henry Home, lord Kames' *Historical law tracts* (1758) in this period. Burke was, in fact, familiar with a number of leading Scottish jurists and thinkers, including: Lord Hailes, Hume, James Mackintosh, John Millar, and Adam Smith. Between them, Burke and the Scots exemplified the most pressing debates and developments of the century.

Burke also spent time in Ireland in the early 1760s, leading to the production of additional texts that shed light on his thoughts on jurisprudence. Much of his time was spent, however, engaged with more practical and sectarian politics. Burke was active in quieting the reaction of the Dublin government, dominated by the established church, to the so-called 'Whiteboy disturbances'. These agrarian disturbances, mischaracterised as confessional, implicated his own catholic relatives. Burke was, in fact, nowhere more critical of the laws of Britain and Ireland than in his *Tracts relating to [the] popery laws* (c1765) written in the same period. There, he recognised the virtues of a more 'regular, consistent, and stable jurisprudence' were real, a mark of legal progress and foundation for social politeness.[17] But the abuse of Irish Catholics by means of law struck him as particularly perverse. These Irish experiences are important to understanding much of his thought. This experience of the dispersal and destruction of a traditional aristocracy, of confiscations based on cultural and religious status, was a pattern Burke later saw repeated in British India and the revolution in France. We can also see his concern for the implications of legal and constitutional change. Legal reform was not easy 'because laws, like houses, lean on one another, and the operation is delicate, and should be necessary'.[18] Still, echoing Montesqueiu, he wrote that

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15 Ibid., i.322.
16 Ibid., i.445.
17 Ibid., i.330.
18 Ibid., ix.453.

> The Legislature of Ireland, like all Legislatures, ought to frame its Laws to suit the people and the circumstances of the Country, and not any longer to make it their business to force the nature, the temper, and the inveterate habits of a Nation to a conformity to speculative systems concerning any kind of Laws. Ireland has an established Government, and a Religion legally established, which are to be preserved. It has a people who are to be preserved too, and to be led by reason, principle, sentiment, and interest to acquiesce in that Government.[19]

The *Tracts* also noted the necessity of an 'interior history of Ireland' which would show that Irish grievances were 'not produced by toleration, but by persecution' and 'from unparalleled oppression.'[20] Burke spent considerable time over the next four decades trying to ensure that such histories were written. Most of his allies in this were Irish catholic historians. It was often Irish manners, they all insisted, that carried the nation through the barbarous application of English law in Ireland. Burke also donated historical documents to Trinity and persuaded others to do the same. Francis Stoughton Sullivan, the first professor of common law at the University of Dublin, was one recipient. Sullivan, at Burke's urging, sought to translate ancient Brehon law texts. And while Burke shared Hume's skepticism towards England's 'ancient constitution', he saw the Scot uncritically repeating the more offensive and prejudicial portrayals of Ireland. Burke sought unsuccessfully, with Tobias Smollet and Irish catholic historians, to persuade Hume to reconsider and rewrite his account.

Without appreciating these early texts and contexts, as well as Burke's rich rhetoric, the meaning of his later works may be distorted. In the *Reflections*, for example, Burke wrote that English jurists from '[Lord Edward] Coke … to Blackstone, are industrious to prove the pedigree of our liberties….', adding that 'if the lawyers mistake the particulars, it proves my position still the more strongly; because it demonstrates the powerful prepossession towards antiquity'.[21] We can see, however, from these early writings that Burke believed Coke and Blackstone had, with Hale and others, mistaken the particulars. When he observed that the English insisted on the continuity of their institutions, Burke did not maintain the truth of those claims.

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19 Ibid., ix.650.
20 Ibid., ix.479, 479.
21 Ibid., viii.81-2.

The appeal to the past created a useful continuity necessary for the progress of society in Europe. But it still remained myth, a point the Irishman would not forget. At its best, the appeal to history was, he said in the *Reflections*, to be 'guided not by the superstition of antiquarians, but by the spirit of philosophical analogy'.[22] Aware of the virtues of the British constitution, a belief widespread in Europe, Burke was equally aware of its vices, especially in Ireland, America, and India. And, as will be discussed below, comments of this sort were not a defence of common law adjudication against legislation, but of the British constitution against revolutionary radicalism.

Attempts by critics to link Burke with the so-called 'Historical School' of jurisprudence of the nineteenth century are also problematic. His influence on German thought is genuine, but simplistically equating his eighteenth-century hostility towards radical revolution with the opposition, especially by Frederick von Savigny, to the nineteenth-century codification of laws is quite seriously misplaced. Like Burke, Savigny emphasised the importance of the past to his present. Both were deeply critical of philosophical rationalism. But Savigny replaced reason with the mystical *Volksgeist*, the 'spirit of the nation or people' linking law and the people. The 'Historical School' was, in fact, linked to the insular nationalism of the nineteenth century and the hope for the creation of a German state. With Montesquieu and others, Burke recognised general differences in national character and culture, but these were extraordinarily fluid. European progress, in both manners and laws, was the result of the 'communication' or interaction of cultures. In the end, the *Volksgeist* resembles nothing so much as the Saxon 'spirit' of vulgar English whiggism. The often rowdy amalgamation of English and Celts, protestants and catholics, whigs and tories, that Burke sought to harmonise as a legislator bore little resemblance to such images. This attempt to recruit Burke to later English hostility is, of course, only one of many anachronistic errors made in wrenching Burke's texts out of their contexts.

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[22] Ibid., viii.84.

Not long after returning to England in the 1760s, Burke was working as personal secretary for Charles Watson Wentworth, Lord Rockingham. Shortly afterwards, he entered Parliament himself. For nearly twenty years, Burke would serve as the chief ideological spokesman of the Rockingham whigs. The commercial humanism they sought to maintain was a serious attempt at joining public honour and private interest, balancing the stability and corporate experience of a hereditary aristocracy with the energy and ambition of a 'natural' aristocracy. They were especially critical of crown influence and remained anxious, with their whig predecessors, about colonialism and 'standing armies'. These long-standing civic languages of critique are very different from the legalist and liberal vocabulary that would come to dominant politics after the revolution in France. The latter have little application to Burke's parliamentary career. Indeed, the reactionary nature of much contemporary populism, perhaps particularly in anti-catholic riots in Britain, is especially important to understanding Burke's later responses to British radicalism and European revolution. At its best, Burke saw Parliament, as a body, independent of the vagaries of public opinion and the influence of the crown. He spent much of his career engaged in modest, meliorist reform. Best-known was his unsuccessful economic reforms designed to eliminate feudal holdovers and to reduce crown influence. But he championed religious tolerance, spoke against slavery, and was critical of many of the more Draconian aspects of contemporary criminal law. The 'true genius' of the British constitution, Burke once confided to Boswell, was 'Tory Language and Whigg [sic] measure'.[23]

Burke's views on the primacy of the legislature also appear to put him at odds with William Murray, lord Mansfield, with whom he is often associated. While Burke no doubt respected Mansfield's abilities, and the judge was related to Rockingham, the two disagreed on a number of public issues, not least the American war. Mansfield also jailed John Wilkes who was supported by the Rockingham whigs. Perhaps most damning for Burke, Mansfield was, like Blackstone, a tory and was linked to John Stuart, lord Bute. Without descending to the anti-Scottish tirades of fellow whigs, Burke criticised Bute's influence on the king (as well as the king's on parliament). For his part, Mansfield suspected Burke to be the author of

[23] Burke, *Correspondence*, v.35.

Junius' Letters (1768-72), critical of him and around which another debate on libel arose. Indeed, while the former is credited with creating significant change in English law through the courts, the latter saw legislators rather than judges as the proper agents of legal reform. Given his parliamentary career, this is hardly surprising.

When Mansfield was denying the jury a role in determining questions of law, Burke wrote:

> I have always understood, that a superintendence over the doctrines as well as the proceedings of the courts of justice, was a principal object of the constitution of this House; that you were to watch at once over the lawyer and the law; that there should be an orthodox faith as well as proper works: and I have always looked with a degree of reverence and admiration on this mode of superintendence. For being totally disengaged from the detail of juridical practice, we come something perhaps the better qualified, and certainly much the better disposed, to assert the genuine principle of the laws; in which we can, as a body, have no other than an enlarged and a public interest.[24]

The difficult duty of articulating the law – in light of general principles on one hand and the practical limits of local manners on the other – was, in large part, the responsibility of the corporate legislature. Parliament represented, at least 'virtually' and ideally, the public virtue of Britain in a way that the courts could not. In the jury debates, Burke stated

> Juries ought to take their Law from the Bench only; but it is *our* Business that they should hear nothing from the Bench but what is agreeable to the principles of the constitution. The Jury are to hear the Judge; the Judge is to hear the Law where it speaks plain, where it does not he is to hear the Legislator.[25]

Neither English courts nor parliament were alone responsible for institutions like the jury. A development 'so elaborate and artificial as the Jury was … brought to its present state by the *joint efforts of Legislative*

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[24] *Works*, ii. 137 (taken from the draft of the 'Speech on the Jury Bill' in Ibid., 137-46). Cf. *Writings and speeches,* ii.343-9, which contains only that part of the speech given, and from which those citations are taken.

[25] *Writings and speeches,* ii.347.

authority and judicial prudence.'[26] Burke would also continue to emphasise the civic commonplace that it was the rule of men, especially the public virtue of public men, on which justice depended.

Burke's best-known opinion at this stage of his career was his strong opposition to the American war. In debates of the period, he articulated his rejection of legalistic formalism and declarations of abstract right. His objections were both philosophical and political. During the American Revolution, Burke chastised the English Parliament's insistence on its formal, theoretical privileges. As with Ireland, what was essential for him were ties of mutual interest and affection. After Rockingham's death in 1782, radical whigs became more vocal in their demands for extensive constitutional innovations, particularly in representation. British radicals, including religious Dissenters, were also critical to the losses of the Rockingham whigs in the electoral debacle of 1784. The fourteen-year impeachment of Warren Hastings found Burke arguing against any simplistic imposition of British laws and manners on India. Instead, he defended native civilisation and institutions.[27] Burke also noted the 'growing Melioration of the Law' that sought justice beyond legal formalism. The close relationship of European culture and commerce 'opened a Communication more largely with other Countries, as the Law of Nature and Nations … came to be cultivated; … antique Rigour and over-done Severity gave Way to the Accommodation of Human Concerns, for which Rules were made, and not Human Concerns to bend to them'.[28]

The uncertainty of succession in the 'Regency crisis' further exposed the widening divisions over constitutional theory and history. The increasing inflexibility of radical demands in ostensibly 'natural' rights, piqued Burke's hostility. For him, 'abstract principles of natural right – which the dissenters rested on, as their strong hold – were the most idle, because the most useless and the most dangerous to resort to. They superceded society, and broke asunder all those bonds which had formed the happiness of mankind for ages'.[29] They threatened, as the revolutionaries would in France, civilisation itself. 'Am I to congratulate an highwayman and

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26 Ibid., ii.347.
27 Ibid., vii.168.
28 Ibid., vii.163.
29 Report of Mr Burke's speech on 2 March 1790, in the debates on the 'Test and corporation acts' of 1790' in The Parliamentary Register Vol. XXVII, (John Debrett, 1790), 178-187, 180.

murderer, who has broke prison, upon the recovery of his natural rights?'[30] His focus, he insisted, was instead on the 'civil social man', who in order to 'obtain justice … gives up his right of determining what it is in points the most essential to him'.[31] For Burke, natural law was expressed or instantiated, however imperfectly, throughout history and a variety of legal orders. But the clarity of claims of natural right were a 'confusion of judicial with civil principles'.[32] They were epistemologically unsound and ontologically denied the inherently communitarian nature of human association. Politically, they risked the progressive, precarious articulation of political rights in European history

Burke's doctrine of prescription, which may be seen descriptively as early as the *Abridgement*, became a progressively more important normative requirement in his attempt to pursue and maintain moderate reforms. If Burke's employment of 'prescription' is problematic, it is so because of its absence of rules for application. The legal analogy had little to do with any specific body of law. It may even be universal. Burke's presumption in favour of established rules and institutions was not uncritical. It was a prudential consideration rooted, in part, on the essentially communal and non-rational nature of human social life and the 'natural', ie naturalistic, basis of human presumption, habituation, etc. This is related, too, to Burke's critical view, with most of his contemporaries, of any strict doctrine of precedent. With his inherent philosophical-epistemological scepticism towards 'precepts' and 'rules', Burke was always wary of rigid legalism. He did not believe that legal precedent was a straightforward matter. Such rationalism runs contrary to the whole tenor of Burke's thought. This should not be surprising. Nor was precedent, political or legal, simply binding. The acceptance of modern *stare decisis*, in which a single decision of a superior court is binding on inferior courts, is a product of nineteenth-century positivism. Indeed, '*[p]recedents* merely as such cannot make Law', Burke wrote, 'because then the very frequency of Crimes would become an Argument of innocence.'[33] Past decisions were

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30 Burke, *Reflections on the revolution in France and on the proceedings of certain societies in London relative to that event*, London, Penguin Books, 1968 [1790], C.C. O'Brien (ed.), 90.
31 Ibid., 150.
32 *Works*, iv.485.
33 *Writings and speeches*, ix.502.

persuasive, as evidence of learned opinion of the law and a valuable source of legal stability, but they were not authoritative in themselves.

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It was in this context that Burke's response to events in France must be understood. As constitutional reform turned to cultural revolution, the novelty and proselytising spirit of the revolution became ever more apparent. Approached by Thomas Paine, Burke was taken aback by his enthusiasm for an expanding European revolution. More immediately, a published sermon of the Dissenter Richard Price to English sympathisers of the revolution seemed to confirm a real threat to Britain. The *Reflections on the Revolution in France and on the proceedings in certain societies in London relative to that event* (1790) was largely aimed at this native audience. Paine and Price also seemed to confirm Burke's belief in important political and philosophical links between British religious radicalism and French revolutionary thought. This revolutionary zeal appeared to him little different from the religious enthusiasm of the British, Irish and European wars of the previous century. It was not progress Burke dreaded, but the loss of the improvement that had already occurred in Europe over centuries. Even before the Terror in France he feared

> first of all the science of jurisprudence, the pride of the human intellect, which, with all its defects, redundancies, and errors, is the collected reason of the ages, combining the principles of original justice with the infinite variety of human concerns, as a heap of old exploded errors, would no longer be studied.[34]

Even manners might lose all anchor and the historical progress of society in Europe endangered

> No part of life would retain its acquisitions. Barbarism with regard to science and literature, unskilfulness with regard to arts and manufactures, would infallibly succeed to the want of a steady education and settled principle; and thus the commonwealth itself would, in a few generations, crumble away, be disconnected into the dust and powder of individuality, and at length dispersed to all the winds of heaven.[35]

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34 *Reflections,* 193-4.
35 Ibid., 193-4.

A defender of the modernity of the *ancien régimes*, including his own, Burke did not dread progress or change, but the loss of centuries of European civilisation and improvement. It was the slow, fragile development of European manners that ultimately supported both commerce and the laws. In the 'shade' of these manners, 'commerce, and trade, and manufacture, the gods of our oeconomical politicians, are themselves perhaps but creatures; are themselves but effects, which, as first causes, we choose to worship. … They too may decay with their natural protecting principles'.[36] Burke's essential concern was for the corporate, mediating, process by which individual and popular will was balance by public reason.

In what may be his most famous passage, Burke wrote that 'Society is, indeed, a contract'.[37] The passage, which follows shortly after that quoted above, continues:

> Subordinate contracts for objects of mere occasional interest may be dissolved at pleasure; but the state ought not to be considered as nothing better than a partnership agreement in a trade of pepper and coffee, calico or tobacco, or some other such low concern, to be taken up for a little temporary interest, and to be dissolved by the fancy of the parties. It is to be looked on with other reverence; because it is not a partnership in things subservient only to the gross animal existence of a temporary and perishable nature. It is a partnership in all science, a partnership in all art, a partnership in every virtue and in all perfection. As the ends of such a partnership cannot be obtained in many generations, it becomes a partnership not only between those who are living, but between those who are living, those whose are dead, and those who are to be born.[38]

But this extract shows, perhaps better than any, the mistake of applying to Burke any narrowly political or jurisprudential reading. It suggests the close connections between manners, history, and law. Burke's point is precisely to deny that the language of 'contract' is sufficient to understanding or articulating the complexities of human community and history. For Burke, 'society', the civil or civilised society, was an entity wider than state or nation. It was the felt sociability and lived associations of men,

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36 Ibid., 173-4.
37 Ibid., 194-5.
38 Ibid., 194-5.

plural and corporate, enveloping *all* social practices and institutions. While these were based in natural human dispositions, they were not insignificantly altered by culture and historical circumstance. There are few points more important in understanding Burkean jurisprudence than the recognition that he does not collapse 'civil society' into the state, but the state into civilised society. The social practices and manners of a people, not least their economic structures, influenced the character and content of their laws and institutions. Modern legislators must be concerned with manners, and the mediating orders and institutions that moderate them, precisely because he may do so little to alter them.

Burke saw manners as both the source of the laws and practical limits to their efficacy. And manners had, of course, 'natural' sources. For all of the uniqueness of its mechanisms and sanctions, law was ultimately 'beneficence acting by a rule'.[39] The modern and enlightened cultures of pre-revolutionary Europe were historically progressive, as Burke saw it, precisely because they balanced the inevitable development of relationships of status with those of choice, including contract. This emphasis on manners and beneficence, on nature and culture, is Burke's most serious challenge to the epistemological transparency and ontological subjectivism of the radical enlightenment, both secular and religious. It puts him closer to thinkers of the so-called 'Scottish Enlightenment' – and to Irish catholic historians – than to common lawyers in ultimately prioritizing manners (or culture) over law. Law was insufficient without beneficience, just as reason was without sentiment. The 'spirit of our Laws' were founded on 'our own dispositions, which are stronger than Laws'.[40] It is in this sense that

> Manners are of more importance than laws. Upon them, in a great measure, the laws depend. The law touches us but here and there, and now and then. Manners are what vex or soothe, corrupt or purify, exalt or debase, barbarize or refine us, by a constant, steady, uniform, insensible operation, like that of the air we breathe in. They give their whole form and colour to our lives. According to their quality, they aid morals, they supply them, or they totally destroy them.[41]

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39 Ibid., 149.
40 Burke, *Letters on a regicide peace,* Indianapolis, Librty Fund Press, 1999 [1795-7], 384.
41 Ibid., 126.

Burke's jurisprudence is imperfect, in both the general and eighteenth-century senses. For better and worse, he argued that manners and history continually reassert themselves in the face of a more perfect justice that neglected manners, other norms, and the mediating practices and institutions of society.

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For Burke, the relationship of law, history, and manners was rooted in a naturalism based in the dynamic empiricism of his age, the legacy of the 'culture of politeness', and religious latitudinarianism. What he called, in the *Reflections*, the 'moral constitution of the heart', the formation of individual character, was a sublime amalgamation of native predispositions and cultural influence.[42] It was on this 'constitution' that the history of European manners and progress was built. In articulating such a view, Burke was working in parallel, if not actually in partnership, with many of the most sophisticated historians and jurists of the day. A 'civil economy' of glory, he believed, continued to provide the social stability necessary for improvement and a link between private and public interest. A commercial humanism provided energy and ambition for social change and 'communication'. Here as elsewhere, the 'civic' traditions, ancient and modern, leavened Burke's faith in commerce and law. Finally, general principles of natural justice, with none of the clarity of revolutionary rights, continued to guide legislators. In the final analysis, law was itself only a highly formal, though critical, method of ensuring public virtue and private beneficence in light of manners and history.

Among the materials in the Burke archives is what appears to be a draft defence of his later, anti-revolutionary writings. There he wrote:

> For the future, I shall stick to my profession. We lawyers do not always make the best hand of a Metaphor. I have burned my fingers with them. In future, I shall avoid all metaphors – I shall stick to my precedent Book … & my special Pleading … Oh. *Si sic omnia*![43]

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42 *Reflections*, 176.

43 F (M) A.xiv.12a-d in the Fitzwilliam (Milton) Burke Collection at the Northampton Record Office.

Burke made considerable and colourful use of legal metaphor throughout his life. We must be careful not to burn our fingers. Burke was not, of course, always imprecise. His early comments on law very clearly show him critical of insular English and common law histories that failed to acknowledge their external debts to the wider progress of society in Europe. While we must also be careful with his parliamentary statements, issued in the midst of major public debates, he consistently insisted that Parliament rather than courts should be at the centre of legal change. More generally, reading his work as a whole suggests that, for Burke, positive law was the imperfect application of natural principles significantly altered by historical circumstance. His use – or misuse – of the language of law was a rhetorical strategy that served as a critique of the thin legalism of revolutionary sloganeering. He defended the modernity of the old regimes, with all of their imperfections, for fear of the loss of centuries of cumulative, corporate progress. Readers, or at least scholars, must be more attentive to these contexts and less determined to rescue Burke for contemporary causes.

Oh, si sic omnia!

Laudatio: Paul De Paepe

Peter Van den haute

I have the honor and pleasure to deliver the introduction to the communication that will be presented by honorary professor Paul De Paepe, at the occasion of the meeting that has been organized to award him the George Sarton medal. I do not consider this moment to be the right one for depicting an exhaustive overview of Paul's scientific career but I find it worthwhile to illustrate some of his major achievements, which I also regard to be appropriate for what I will call the "adornment" of the occasion.

As a young geologist (he was born on May 5th, 1939), Paul De Paepe started his scientific career in 1962 with the preparation of a doctoral thesis on the volcanic rocks of the Galapagos islands. In those years, such a project required quite some perseverance, not only because the journey forth and back to the islands, had to be made by ship but also because there was no senior geologist who could provide him with sufficiently competent scientific guidance, the whole department of Earth sciences of our university being mainly focused on soil science at that time. However, this did not prevent him from conducting his research in what must have been quite a characteristic self-reliant way and in June 1968 he successfully defended his PhD thesis. Although he always stays very unpretentious about his doctoral work, it proves that by the time that he had accomplished his thesis, he had become a fully qualified petrographer and petrologist of igneous rocks. This was confirmed later on by the further studies he made on volcanic and intrusive rocks from various parts of the world, including the Cap Verde Islands, Central Africa and Easter Island, which resulted in about fifty papers, many of them published in highly ranked journals.

Although, he had already done some occasional petrographical work for archaeologists before, it was in the mid-seventies that his field of interest greatly shifted away from pure igneous petrology to archaeological earthenware and stone artifacts. This new scientific journey was again, going to test his perseverance. Not only, part of his older colleague geologists, did consider such a work as a complete waste of time and regarded him as lost to Earth science, also our archaeologists needed to be convinced yet that any important information could be gained from the microscopic investigation of their finds, that moreover, had to be cut and destroyed before any relevant observation could be made and that hence, were considered to be rendered useless for the archaeological archives.

When he retired, about thirty years and one hundred and twenty publications later, Paul had obtained a solid international reputation as one of the very few, if not the only specialist in Belgium of provenance studies of archaeological pottery. When interrogated about his skills as an archaeo-petrographer, he usually answers with the phrase that in the country of the blind, the one-eyed is king. But, when I recall the moments that I saw him sitting behind his microscope inspecting a tiny rock fragment in some exotic potsherd with his meticulous look, I must admit that I often thought that actually he must have a third eye, that was specially made for this kind of work.

Another major accomplishment in the field of archaeometry that he has made is the exhaustive study on the provenance of the classical white marbles that were used in ancient Greece and in the Roman empire. This study, that he has carried out together with Luc Moens, our present vice-rector, who as a specialist in analytical chemistry investigated their trace element and isotopic composition, involved the collection of marble samples from all major quarries that existed in the Mediterranean region during antique times. After a considerable amount of painstaking research work, they were finally able to make a fingerprint of each type of marble and to determine its origin with sufficient certainty. As a major outcome of this study, Paul was invited by several museums all over the world for an expertise on their marble statues. Hence, it seems beyond doubt to me that this marble project must have been rewarding for all aspects of his life as a scientist, when considering the field campaigns that it brought about from Tuscany to the isle of Naxos and the free visits to museums from Geneva, Switzerland to Malibu, California.

His direct contribution to the history of Earth science and hence also the major reason why I presented him to the Sarton Committee for the 2011-2012 Medal, is the book that he has published with Academia Press, early 2010, together with Eddy Van Der Meersche and Georges Stoops. In this book entitled “Minerals with Belgian roots”, that he prepared entirely after his retirement, he discusses the etymology of mineral names that were discovered in Belgium and/or that refer to Belgian citizens. A problem that he encountered during the preparation of his book was, that for a number of these citizens, there existed some doubt about their Belgian nationality. So, it must certainly have required some patient and perseverant research again before these problems could be solved. Undoubtedly, his book provides Belgium’s mineralogical heritage with a unique reference work that can be consulted by the professional mineralogist, the interested amateur and even the complete non-mineralogist, not only as a source of information but also as a source of mere pleasure to the eye, due to the numerous marvelous illustrations and the photographs, shot by Eddy Van Der Meersche.

Time has come now to end this introduction and to hand the floor over to Paul. I understood that in his communication, he will provide us with some further details on the stories of “the people behind the minerals” who figure in his book and that he will throw some further light on persons that were attached to our university at the time. I am sure that all of us are eager to hear what he is going to tell.

Mineral Species named after Belgian Citizens and Localities

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1. How mineral naming works

A glance at the authoritative *Handbook of Mineralogy*[1] learns that many mineral species have names providing information on their chemical composition, colour, typical crystal form, or other physical properties. Selected minerals assigned to this category are chromite (named in allusion to its high chromium content), althupite (for ALuminium, THorium, Uranium and Phosphorus in the composition), hematite (from the Greek *aima*, blood, because of its resemblance to dried blood), triangulite (for the typical triangular habit of its crystals), garnet (from the Latin *granatum*, pomegranate, alluding to its resemblance to the seeds of that fruit, both in colour and shape), actinolite (from the Greek *aktis*, meaning "ray", in allusion to its fibrous nature), anhydrite (from the Greek for *without water*, in contrast to hydrous calcium sulphate minerals), pyrite (from the Greek for fire, as sparks may be struck from it), halite (from the Greek *halos*, meaning "salt", in allusion to its taste), barite (from the Greek *baryos*, meaning "heavy", the mineral having a high specific gravity), magnetite (in reference to its magnetic properties), oursinite (from the French *oursin*, sea urchin, alluding to its radial aggregates resembling spines of sea urchins) and fluorite (from the Latin *fluere*, to flow, because of its fluxing properties).

[1] Anthony, J.W., Bideaux, R.A., Bladh, K.W. & Nicols, M.C., 1990-2003. *Handbook of Mineralogy*, 5 vols, Mineral Data Publishing, Tucson, Arizona.

Other mineral names are silent upon the chemistry or physical characteristics of the mineral species they are linked with, and instead recall to mind either an individual, a group, an organization or an event, or they convey information on the country, region, geographic context, or locality where they were first found. Minerals of this second category are exceedingly numerous too.

Mineral names after individuals may commemorate e.g. the discoverer or first analyst of the mineral, a scientist or another person of noteworthy proportions, a ruler, a mineral collector, and even mythical creatures and deities. Some representative examples are rosenbuchite (in honour of the German geologist and mineralogist Carl Rosenbuch), becquerelite (for the French physicist and Nobel Prize winner Antoine Henri Becquerel who, along with Pierre and Marie Curie, discovered radioactivity), goethite (as a tribute to the German poet, dramatist and philosopher, Johann Wolfgang von Goethe), linnaeite (after the Swedish botanist Carl von Linné), stanleyite (in celebration of Sir Henry Morton Stanley, Welsh-American journalist and explorer), gagarinite (for the first cosmonaut, the Russian Yuri Alekseyevich Gagarin), willemite (for William I, King of The Netherlands) and the chrysoberyl variety called alexandrite (after Tsar Alexander II of Russia). Only in exceptional cases individuals have more than one completely unrelated mineral named after them. Marie Curie-Sklodowska, Polish-born French physicist and chemist, who discovered the element radium and won the Noble Prizes of Physics 1903 and Chemistry 1911, is immortalized by both curite (co-named with her husband Pierre Curie) and sklodowskite. Another example concerns Neil Alden Armstrong, the first man to set foot upon the Moon. In addition to armstrongite, the mineral armalcolite (an acronym deriving from the last names of Neil Alden ARMstrong, Edwin Eugene ALdrin and Michael COLlins, the Apollo 11 astronauts who collected the type samples) testifies to the exploit and celebrity of this American hero. With regard to mineral names reminding of an individual, it should be mentioned with great emphasis that, contrary to expectations, a mineralogist cannot name a mineral after himself, but, should the occasion arise, can name it after someone worthy of his choice.

Names such as brazilianite, senegalite and srilankite make it clear in which countries the first samples of these mineral species have been recognized.

This also fits the case for e.g. andalusite, balkanite, caledonite, eifelite and labradorite, minerals passing under a name indicative of the region where the very first specimens were unearthed. Mineral names derived from a locality or a geographical feature are common too and bagdadite, salzburgite, bauxite (after Les Baux, a village in southern France), kolwezite (for its occurrence at Kolwezi, Democratic Republic of the Congo), trabzonite (for the Trabzon Province, in north-eastern Turkey) and agate (for its occurrence at the Achates River, in south-western Sicily) are a few examples. Neptunite, tapiolite and quetzalcoatlite refer to Neptune (the Roman god of the sea), Tapio (a god of Finnish mythology) and Quetzalcoatl (an Aztec deity), respectively, whereas apachite and eskimoite pay tribute to the Apache Indians in Arizona (USA), and the Eskimos, known to be early settlers of Greenland.

In the past a rather limited number of mineral species got a name drawing attention to their use or the magical powers attributed to them. Nephrite and amethyst[2] are minerals belonging to this category. The former was believed to have healing properties and to fight kidney diseases. The name of the latter derives from the Greek *amethystos*, meaning "sober", and expresses the ancient popular belief that it protects against drunkenness and prevents from intoxication.

In all cases related above, the suffix "ite" comes from the Greek word *lithos*, meaning rock or stone. Mineral names having the suffixes "ine", "id", and other endings, are anything but common. Spessartine (for the locality near Spessart, Germany), almandine (for Alabanda in Caria, Turkey, a cutting centre in antiquity), and chloritoid (a name making allusion to its similarity to chlorite minerals) are three examples.

At the end of the 1950s over 20.000 mineral names were known to occur in literature. This proliferation of mineral names was deceptive as research performed in more recent times made it clear that many names published in the 19th and the first half of the 20th century had to be rejected. Thanks to new, sometimes highly sophisticated instrumental methods, it became gradually obvious indeed that a large number of mineral names that years ago felt quite at home in descriptive mineralogy, duplicated names already in use, or it was definitely settled that minerals published previously were

[2] Amethyst is a violet variety of quartz.

actually mixtures of already characterized mineral species. It was the merit of the International Mineralogical Association (IMA) that a Commission on New Minerals and Mineral Names (CNMMN) was founded in 1959. This commission aimed to obviate the inconstancies and problems of mineral naming and nomenclature that occurred before that date. For that purpose all mineral species described before 1959 were re-examined and, depending on the outcome of the findings of the qualified IMA commission, were classed either as approved (A), discredited (D), grandfathered (G) (pre-IMA), questionable (Q), redefined (Rd), or renamed (Rn). At the same time an official naming procedure to control and judge adequately the validity of new proposals was elaborated. For more details on that matter reference is made to Nickel, E.H. & Grice, J.D. (1998)[3]. Finally, it should be mentioned that a continually updated list of IMA-approved mineral names is available at the website of IMA.

Blackburn, W.H. & Dennen, W.H. (1997)[4] calculated that among the 3.700 mineral species known to science in 1997 and approved by IMA about 45 percent got a name in honour of a person, 23 percent after their discovery locality, 14 percent for their chemical composition and 8 percent for a distinctive physical property. Combinations of personal, geographical, chemical or physical terms were recognized in the names of another 8 percent, whereas about 2 percent was given a name deriving from none of the above-mentioned categories. In addition, it is also worth noting that some mineral species known from antiquity (quartz, etc.) have a name of uncertain or unknown origin.

2. Mineral names uncover history

Most mineral species used from ancient times until the late 18th century were given a name in relation to their appearance, or their physical properties. Abraham Gottlob Werner (1749-1817)[5], called 'the father of German geology', is thought to have been the very first to name a mineral after a

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3 Nickel, E.H. & Grice, J.D., 1998. The IMA Commission on New Minerals and Mineral Names: Procedures and guidelines on mineral nomenclature. *The Canadian Mineralogist*, **36,** 1-16.

4 Blackburn, W.H. & Dennen, W.H., 1997. *Encyclopedia of Mineral Names.* The Canadian Mineralogist*,* Special Publication, **1**, 360 p.

5 Abraham Gottlob Werner studied law and mining at Freiberg and Leipzig. He was a very influential geologist and published the first modern textbook on descriptive mineralogy.

person. This occurred in 1783 and the mineral at issue was prehnite, named after the Dutch army officer Hendrik van Prehn (1733-1785), who found it at the Cape of Good Hope, South Africa. This practice became soon very popular and with time more and more newly discovered mineral species were supplied with a name after an individual. Even today mineralogists make it a rule to name new mineral species after a deserving person. In the beginning new mineral names usually referred to the most meritorious predecessors of the scientist proposing the name but today it has become common usage to eternize the name of a colleague or collaborator of the discoverer through a mineral species.

The number of minerals named after discovery localities also strongly increased in the course of time. The study of mineral names may hence yield a unique insight into the historical development of the geological and mineralogical research worldwide, or in a specific country. A look at the names and the scientific curriculum of the scientists who proposed them or examined the newly discovered mineral species, also provides valuable information in that respect.

It appears that the number of mineral species discovered through time was subject to strong variation. From antiquity to about two hundred and fifty years ago, this number changed hardly, whereas since the middle of last century it increased in a very spectacular way. The reasons for that are obvious, the main one being the development of very efficient and accurate instrumental techniques which allow scientists to study with great precision the chemical composition, optical characteristics and other physical properties of very small crystals. Moreover, thanks to the improvement of the analytical methods only minimal quantities of material are required. The result is that for many years the total number of known mineral species has increased annually with about 60 to 80 units on average, and there are no signs that this situation will change drastically in the decades to come.

As will be demonstrated below, the study of mineral names in relation to a specific country makes it possible to trace which institutes or research centres were leading at any moment of history and to speculate about the origin of the shifts occurring in the scientific production of each of them. From time to time casual events may also play a prominent role in that matter.

3. Focus on Belgium: from halloysite (1826) to ernstburkeite (2011)

Van Der Meersche *et al.*[6] stated that in early 2010 no less than ninety-one mineral species occurring in literature had a name honouring either a Belgian citizen[7], or a geographical feature (locality, region, etc.) related to Belgium. The discovery of ernstburkeite[8] in Antarctica and its approval by IMA in February 2011 brought on that nowadays this number stands at ninety-two.

All mineral species complying with the criteria related above are listed in **Appendix 1**. 27 out of the 92 were discovered in Belgium. The others were found for the first time in 12 countries worldwide, the vast majority however in the present-day Democratic Republic of the Congo (DRC) (*table 1*). The names of 78 mineral species refer to a person, whereas 14 were given a name after a place, a natural region or water-course in Belgium, or after the country itself.

Table 1: Place of discovery of all mineral species named after a Belgian citizen and first found outside Belgium

Place of discovery	Number of mineral species
Antarctica (Queen Maud Land)	1
Canada	1
Democratic Republic of Congo	49
France	1
Germany	1
Italy	1
Mexico	1
Morocco	1
Russia	2
Switzerland	1
Tunisia	1
UK	1
USA	4
	65

[6] Van Der Meersche, E., De Paepe, P. & Stoops, G., 2010. *Minerals with Belgian Roots from hopeite (1824) to tazieffite (2009)*, 231 p., Academia Press, Ghent.

[7] Some of them got the Belgian nationality for a limited period of time only (Giuseppe Cesàro, Jacques Jedwab, Haroun Tazieff, etc.).

[8] Sakurai, T., Genceli Güner, F.E. & Hondoh, T., 2011. Ernstburkeite IMA No. 2010-059. CNMNC Newsletter **8**, April 2011, 292. *Mineralogical Magazine*, **75**, 289-294.

As shown in *table 2*, the IMA-status of the 92 mineral species is varied. Although several mineral names commemorating Belgian citizens and localities have been discredited by the IMA Commission on New Minerals and Mineral Names (21 in total), or have a questionable/doubtful status (5 in total), it appears that the majority of them are grandfathered or IMA-approved. As not only grandfathered and IMA-approved mineral names but also obsolete names and doubtful species are important for both a good knowledge of the mineralogical heritage of our country and a clear understanding of the evolution of the mineralogical research in Belgium, all of them are treated in the present paper.

Table 2: IMA-status of all mineral species named after Belgian citizens and places in Belgium

IMA-status	Number of species	Belgium	DRCongo	Other countries
Discredited	21	13	8	
Questionable	5	2	3	
Redefined	1	1		
Grandfathered	27	5	20	2
IMA-approved	38	6	18	14
	92	27	49	16

17 mineral species listed in Appendix 1 have been described before the end of the First World War (WW1), 19 during the interbellum, and the remainder (56 species) since the beginning of the hostilities of the Second World War (WW2) in 1940 (**Fig.1**).

It should be mentioned that the mineral descriptions and, with few exceptions also the biographies given below are very concise and do not relate to all mineral species presented in Appendix 1. For more details and information on the subject, the interested reader is recommended to consult the book '*Minerals with Belgian Roots from hopeite (1824) to tazieffite (2009)*' written by the author of the present paper in close collaboration with Eddy Van Der Meersche and Georges Stoops (see footnote 6).

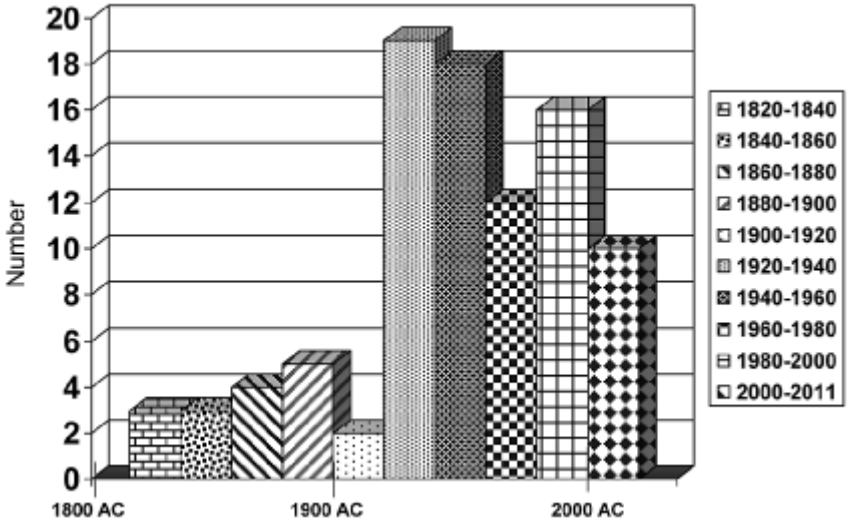

Fig.1. Number of mineral species named for Belgian citizens and localities since the early 19th century (92 in total)

3.1. The period before 1918

Fig.1 and Appendix 1 show clearly that the number of new mineral species published between 1826 and 1918 is rather limited (17 in total) as compared to what happened subsequently. In addition, it appears that only five of them are still considered valid species, the others either have been discredited (9), labeled as questionable (2), or ask for redefinition (1). There are many reasons for this. Several mineral species recognized in the early days of the mineralogical research in Belgium were described by field geologists and not by thoroughbred mineralogists. Moreover, we have to realize that in these earlier days in Belgium, no less than in other countries, the instrumental methods were far from being as effective and all-embracing as they are today and did not allow to analyse mineral samples at a microscopic level. This situation gradually changed for the better after WW1 and thanks to the IMA Commission for the Naming of Minerals and Mineral Nomenclature not a single mineral species listed in Appendix 1 has been declared invalid after 1959, date of the establishment of the IMA-CNMMN.

Halloysite was the first mineral species named after a Belgian citizen. It pays tribute to Jean-Baptiste Julien d'Omalius d'Halloy[9], statesman, a pioneer of modern geology and considered to be 'the father of Belgian geology'. The original halloysite was observed by Omalius d'Halloy at Angleur (Liège), an area with old zinc and iron mines and extensive outcrops of Carboniferous limestones with solution cavities and pockets filled with alteration products. Halloysite was first described in 1826 by Pierre Berthier (1782-1861), a French geologist and mining engineer who focused his research on geological materials relevant to industry. Berthier described also for the first time bauxite and nontronite. In 1816 Berthier became chief of the laboratory of the *Ecole des Mines* in Paris. We suppose that he made the acquaintance of Omalius when the latter, acting under the orders of Napoleon, stayed in Paris to prepare and to work at the first geological map of the French Empire and its neighbouring territories. After Napoleon's overthrow, Omalius was appointed governor of Namur, a position he owed to King William I of The Netherlands and he occupied until the Belgian Revolution of 1830. In 1848 he was elected as a member of the Belgian Senate before becoming its vice-president (1851). Omalius was made a member of the Academy of Sciences in Brussels and became its president in 1850. Thanks to its contribution to the geology of France, he had the great honour and privilege to serve the Geological Society of France as president in 1852.

Three years after Belgium obtained independence **calamine** was described by the pharmaceutical chemist Charles Joseph Davreux. Although the name of this mineral reminded of La Calamine (Kelmis in Dutch), presently a municipality in the German-speaking part of eastern Belgium, it most probably derives from the Latin term Cal(a)mis, meaning "a place with calaminarious stones" (zinc-bearing ore). For centuries La Calamine was home of an important zinc mine, known as *Vieille-Montagne* (in French) or *Altenberg* (in German). At the time Davreux discovered calamine La Calamine did not belong to Belgium but formed part of the territory of Neutral Moresnet. This mini-state, with a surface area of hardly 3.5 km^2, was called into existence after the fall of Napoleon (1815) when the borders of the United Kingdom of The Netherlands and the Kingdom of

[9] Dupont, E., 1876. Notice sur la vie et les travaux de Jean-Baptiste-Julien d'Omalius d'Halloy. *Annuaire de l'Académie royale des Sciences, des Lettres et des Beaux-Arts de Belgique*, **42**, 181-288.

Prussia were re-established. At Moresnet, because of the economic interest of the zinc ore deposits occurring in the subsoil of the Vieille-Montagne mine, both nations could not come to an agreement. As a result the municipality was divided into three parts. It was decided that the area where for centuries the zinc ore was quarried should become a neutral territory. At the end of WW1, the Treaty of Versailles stipulated that Neutral Moresnet was awarded to the Kingdom of Belgium. Today calamine is no longer considered a valid term because the material studied by Davreux was a mixture of (at least) two distinct minerals: zinc carbonate (smithsonite) and a zinc silicate (hemimorphite). Both mineral species are very similar in appearance and chemical analysis is required to differentiate them.

In the middle of the 1830s André-Hubert Dumont[10] was appointed professor of mineralogy and geology to the University of Liège. By order of the Belgian government, he devoted about twenty years of his very productive, though short academic career to the preparation of a nine-sheet geological map of Belgium at the 1:160.000 scale. Dumont seized this opportunity to explore almost every outcrop in Belgium, either on foot or on horseback, so that no single geological feature or fact of importance escaped from his attention. No wonder thus that during his countless trips and field observations he also found four mineral species which in his opinion were new to science: **davreuxite**, **delvauxite**, **bastonite**, and **salmite**. Three of them were described by himself, and one by E. Prost. Their names are reminiscent of two colleagues of A.-H. Dumont from the University of Liège (Charles-Joseph Davreux, Jean Charles Delvaux de Fenffe), a locality (Bastogne) and the Salm river, a tributary of the Amblève, which flows near the place where the fourth species was discovered.

As the field work carried out in prospect of the drawing of the geological map of Belgium strongly impaired Dumont's health, he was advised to travel to regions with mild climatic conditions. He decided to visit Germany, Austria, Turkey, Greece, Italy, Sicily, Spain and France. However, instead of taking rest he took occasion of his 10-months lasting travelling to compare the geological data collected in Belgium with what he observed in the countries he visited. Dumont took also this opportunity

[10] For a comprehensive biography of André-Hubert Dumont in English reference is made to Portlock, J.E., 1858. *Quarterly Journal of the Geological Society of London,* **14**, lxii-lxxi.

to participate in geological excursions organized by colleagues and did an attempt to establish on a large scale the geological correlation between the various regions of Europe he paid a visit. At the same time the idea occurred to him to make a geological map of Europe and to present it at the World Fair of Paris in 1855. This hazardous project was carried to a successful conclusion and in Paris he was crowned with honour. However, the labour and stress connected with this undertaking, in combination with his appointment to rector of the University of Liège were fatal to his health and André-Hubert Dumont died in February 1857, aged 48.

For a relatively short period of time, C.-J. Davreux[11] was on duty as *préparateur-adjoint* to the chair of chemistry held by J.C. Delvaux, and curator of the mineral collections of the University of Liège. However, in 1836 he was no longer interested in an academic career and left the university to devote himself to more practical professional activities in relation to industrial chemistry and toxicology. Graduated as a doctor in medicine, Jean Charles Delvaux de Fenffe[12] joined the Faculty of Sciences of the Imperial University of Liège in 1811, where he had to lecture physics and chemistry. In 1817, after the foundation of the University of Liège he became professor to the Faculty of Sciences of this institution and got the responsibility to teach experimental physics, general chemistry, chemistry applied to the arts, and metallurgy. As mentioned in Appendix 1, both bastonite and salmite were discredited by the IMA commission. Davreuxite is recognized as being grandfathered, whereas the IMA status of delvauxite is questionable.

After the unexpected death of Dumont, at first Gustave Dewalque and thereupon Giuseppe Cesàro became responsible for the classes of geology and mineralogy at the University of Liège. Although Dewalque[13] focused his research on geologic problems, and especially on the stratigraphy of the sedimentary formations of Belgium, Félix Pisani (1831-1920), French chemist, author and mineral dealer, judged it only fair to name a mineral species he discovered at Salmchâteau (Vielsalm) **dewalquite**, as homage for the excellent scientific work realized by Dewalque in Southern

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11 Dewalque, G., 1873. Charles-Joseph Davreux. *Biographie nationale*, **4**, col. 733-735.

12 Florkin, M., 1963. Jean Charles Delvaux de Fenffe. *Annuaire de l'Académie royale de Belgique*, **CXXIX**, 3-21.

13 Lohest, M., 1911. Gustave Dewalque. *Annales de la Société géologique de Belgique*, **38**, B77-B126.

Belgium. At about the same moment Arnold von Lasaulx, German mineralogist and petrographer to the University of Bonn (Germany), described the same mineral species but gave it the name **ardennite**. The specimen von Lasaulx analysed was thought to come from the surroundings of Ottré (Vielsalm), although the exact origin of the sample he described is still unknown. According to IMA the mineral species studied by Pisani and von Lasaulx were identical and it was decided that it should be named ardennite and not dewalquite as proposed by Pisani.

During his professional career, Pierre Joseph Destinez was employed at the department of geology and mineralogy of the University of Liège, where for many years he was *conservateur-préparateur* of the mineralogical and palaeontological collections. He was also the dutiful and skilled technician of Gustave Dewalque, whom he readily accompanied in the field. Although he did not have any university degree, Destinez was endowed with a strong observation power, both in the field and in the lab, and very soon he became a real expert of the fauna and flora of the Carboniferous limestone beds of Belgium. No wonder thus that, in recognition of its scientific merits and contribution to the study of the Carboniferous of our country, several fossils and even a mineral (**destinezite**) were named after him. The proposal made by Peacor *et al.* (1999)[14] to use the name destinezite for crystalline $(Fe^{3+})_2(PO_4)(SO_4)(OH).6H_20$ was accepted by IMA-CNMMN.

Giuseppe Cesàro[15] grew up under difficult financial circumstances in the vicinity of Naples, in the former Kingdom of the Two Sicilies but was university-trained in Belgium, where he was a pupil of Gustave Dewalque. Because of his in-depth knowledge of mineralogy and the quality of his writings, and despite the fact he did not have the required certificates of qualification, he became responsible for the classes of mineralogy and crystallography in 1891 when Dewalque decided to relinquish this part of his educational charges at the University of Liège. Unlike Dewalque, Giuseppe Cesàro was a mineralogist to the backbone. He published many articles on descriptive, theoretical and optical crystallography and discovered several new mineral species. Among the minerals treated in the

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[14] Peacor, D.R., Rouse, R.C., Coskren, T.D. & Essene, E.J., 1999. Destinezite ('diadochite'), $Fe_2(PO_4)(SO_4)(OH).6H_20$: its crystal structure and role as soil mineral at Alum Cave Bluff, Tennessee. *Clays and Clay Minerals*, 47, 1-11.

[15] Buttgenbach, H., 1942. Giuseppe Cesàro. *Annuaire de l'Académie royale de Belgique*, **CVIII**, 35-70.

present paper **koninckite**, **cornetite** and **fraipontite** are still standing as valid species, whereas **richellite** was considered by IMA as being questionable.

Laurent-Guillaume de Koninck[16] was a pupil of some of the most prominent chemists of his time (L.J. Thénard, J.L. Gay-Lussac, E. Mitscherlich and J. von Liebig). From 1836 onwards he lectured general chemistry, organic chemistry and elements of chemistry on behalf of students in Art at the University of Liège. In the middle of the 19th century de Koninck stopped his activities in chemistry and became a world specialist of the fossils of the Palaeozoic, and in particular of the faunas of the Carboniferous. de Koninck discovered more than 700 new fossil species in Carboniferous limestones of Belgium and many palaeontologists, not only in North- and South America, but also in India, China, Australia, etc., appealed to his knowledge. Moreover, many fossils were named after him.

At the time cornetite was discovered by Cesàro, Jules Cornet[17] lectured geology, mineralogy and palaeontology at the *Ecole des Mines* in Mons. Concurrently he was also on duty at the University of Ghent where he was in charge of the classes of physical geography at the Faculty of Sciences, and lectured history of commodities at the Special School of Commerce attached to the Faculty of Law. Jules Cornet earned national and international reputation thanks to the pioneering geological research he did in the ore-bearing territories of Katanga, at that time a part of the former Congo Free State. Cornet participated from 1891 to 1893 in a scientific mission organized by the *Compagnie du Katanga* and headed successively by Captain Lucien Bia and Emile Francqui. During the Bia-Francqui expedition Jules Cornet contrived, among other things, to draw the first geological map of the region he traversed and to evaluate the volumetric importance of the copper deposits in the subsoil. Therefore he is said to be the 'founder of the geology of the Democratic Republic of the Congo'.

In 1916, the mineral **belgite** was introduced in descriptive mineralogy by the Italian scientist Ruggero Panebianco. In Panebianco's opinion the name willemite was inappropriate to designate a zinc silicate the French miner-

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16 Fraipont, J., 1887. Laurent-Guillaume de Koninck. *Annales de la Société géologique de Belgique*, **14**, B189-B248.

17 Marlière, R., 1967. Jules Cornet. In *Florilège des Sciences en Belgique pendant le XIX^e siècle et le début du XX^e*, 453-469, Académie royale de Belgique, Classe des Sciences, Bruxelles.

alogist and crystallopgrapher Serve-Dieu Abailard (Armand) Lévy (1795-1841) discovered in the Vieille-Montagne mine in the late 1820s (see: calamine). According to IMA, Panebianco's suggestion to replace the species name willemite by belgite was unfounded and the CNMMN rehabilitated the name proposed by A. Lévy. At the time Lévy found the new mineral species he called willemite after William I, King of The Netherlands, he was a member of the academic staff of the University of Liège and in charge of the classes of mineralogy. He described several new minerals. The famous Scottish physicist David Brewster (1781-1868) named levyne after him.

Constantin Klement[18] was born in the former Austro-Hungarian Empire. After earning a doctor's degree in sciences in Vienna, where he studied chemistry under Ernst Ludwig (1842-1915) and mineralogy under Gustav Tschermak (1836-1927), and an assistantship to the laboratory of the K.K. Chemisch-Physiologische Versuchstation at Klosterneuburg (near Vienna), he took up the post of *aide-naturaliste* at the department of mineralogy and lithology of the Royal Belgian Museum of Natural History[19] in Brussels. There Klement assisted Alphonse Renard, head of department and personal friend of Tschermak, in quantitative chemical research. Late 1888 he even succeeded to Renard when the latter was appointed full professor to the University of Ghent. The chlorite mineral called **klementite** by G. Tschermak was for the first time analysed chemically by C. Klement and originated from Salmchâteau (Vielsalm). Being identical to chamosite, klementite was discredited in the middle of the 1970s. Klement took out letters of naturalization and became Belgian barely five years before he passed away.

With the exception of ottrelite, all mineral species with a name reminescent of a place of discovery in Belgium described in the pre-1918 period (ardennite, bastonite, belgite, calamine, ciplyite, moresnetite, richellite and salmite) are now considered being either discredited or questionable. Their names were proposed by German, Italian and Belgian scientists. Augustin Damour (1808-1902) and Alfred des Cloizeaux (1817-1897), who described first **ottrelite**, were French mineralogists. The former provide

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[18] Van Der Meersche, E., De Paepe, P. & Stoops, G., 2010. Constantin Klement. *Minerals with Belgian Roots. From hopeite (1824) to tazieffite (2009)*. 59, Academia Press, Ghent.

[19] This museum is presently known as the Royal Belgian Institute of Natural Sciences.

the very first reliable quantitative chemical analysis of more than 20 minerals. The latter made a reputation with a method for determining different kinds of feldspar.

In conclusion we can urge that between 1826 and the end of WW1 rather few new mineral species got a name honouring a Belgian citizen (7) or locality (6), or a geographical feature related with Belgium (3). In comparison with the situation in later periods new names reminiscent of a place of discovery or a geographic feature in Belgium were relatively abundant and only one new mineral species (cornetite) originated from outside Belgium. Individuals honoured with a mineral name were chiefly field geologists and mineralogists of the University of Liège. This is anything but a surprise as at that time geological and mineralogical research was flourishing in Southern Belgium, and especially in the Liège Province, thanks to demanding mining activities and booming industrialisation. The scientists who described the new mineral species were mainly members of the academic staff of the University of Liège, although the contribution of scientists from institutions in other West-European countries was far from being negligible.

3.2. The interbellum

Though the interbellum lasted hardly twenty-one years, it is a fact that during this time-span more new mineral species named after Belgians and Belgian localities were published than in the lasting pre-1918 period (Appendix 1). Seven of the nineteen mineral species newly described in the interbellum issued from research carried out by Alfred Schoep[20]. Schoep was dr. in geography and geology and became junior professor (*chargé de cours*) in mineralogy to the Faculty of Sciences and the School of Technology of the University of Ghent in 1919. In the first years of his professional career (1906-1910) he served as assistant to the Laboratory of Analytical and Toxicological Chemistry of the Faculty of Medicine in Ghent, joined the department of Mineralogy and Crystallography in 1910 and participated in a scientific mission to Katanga, led by the German geologist Constantin Guillemain, from 1910 to 1913.

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[20] De Leenheer, L., 1970. Memorial of Prof. Alfred Schoep. *The American Mineralogist*, **55**, 597-602.

All mineral species Alfred Schoep described for the first time are thought to originate from Katanga but were not collected by him during the Guillemain expedition of 1910-1913. How Schoep gained possession of them is a cock-and-bull story that, because of wartime conditions and economic secretiveness, was kept dark for many years and, to the best of our knowledge, never emerged in any front-rank scientific publication or conference report during his life. All the ins and outs of the story however were divulged during an interview given by Schoep to the responsible of editorship and management of the *Wetenschappelijke Tijdingen*[21], three years after he had retired from office.

The starting-point of the discovery of (at least part of) Alfred Schoep's new mineral species was a radioactive mineral sample given in the early 1920s to Jules Cornet by one of his pupils and said to come from an unknown locality in Katanga (former Belgian Congo). At that time Jules Cornet was professor at both the *Ecole des Mines* in Mons and the University of Ghent. Alfred Schoep was a respected colleague of him and lectured mineralogy and crystallography in the same institutions. When Cornet asked Schoep if during his stay in Katanga he ever perceived the mineral given to him by his pupil, the latter answered in the negative but agreed to go further into the matter. Both scientists were aware indeed of the interest of the problem as it was a public secret that uranium minerals were a major source of radium, and consequently very important for health care and industrial applications.

Soon after Schoep met Cornet, the former contacted his friend, mineralogist Leonard James Spencer[22]. At that time Spencer was Assistant Keeper of Minerals to the Mineral Department of the British Museum in London, an institution that houses today, as it did in the past, one of if not the finest mineral collection on earth. Schoep made an appointment and traveled, the mineral specimen entrusted by Cornet at hand, to London during the Easter-holidays of 1921. Spencer was put through his paces but, unfortunately, did not have by hand a ready solution to the problem. According to Spencer it was not unlikely that the material submitted by Schoep could originate from Katanga but he advised him to apply to Francis Henry

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21 Goossenaerts, J., 1954. Prof. Schoep en het Uraniumerts uit Kongo. *Wetenschappelijke Tijdingen*. Orgaan van de Vereeniging voor Wetenschap, v.z.w. Ghent, **14**/4, 131-138.

22 Tilley, C.E., 1960. Memorial of Leonard James Spencer, *The American Mineralogist*, 45, 403-406.

Butler, a businessman living at no.158, Brompton Road, London, S.W., a short distance away from the British Museum buildings in South Kensington.

Butler studied at the Royal School of Mines, London, next became a fully qualified physician but, at Richard Talling's death[23], decided to establish himself as a professional mineral dealer. Along with selling educational collections, Butler also handled in high-quality minerals and occasionally provided the British Museum with rare and very fine mineral specimens. At most one week before Alfred Schoep arrived in London to sound his colleague about the source and nature of Cornet's sample, Butler dropped in into Spencer's office with a box containing uranium minerals said to come from Katanga. However, as the place of discovery of the material was unknown to Butler, Spencer was not interested to purchase it.

Shortly after his visit to the British Museum, Schoep paid a visit to Brompton Road no. 158. The mineral specimens Spencer reported upon were kindly shown to him by Butler, who hinted that the stuff was probably uranium-bearing, originated from an unspecified place in the Belgian Congo and came into his possession during WWI through a Belgian officer. According to Butler only one sample of the whole collection was supplied with a label, but unfortunately the inscription on the label was hardly legible. Despite this handicap, the curator of the London Museum of Practical Geology[24] decided a few days before to buy the sample provided with a label, whereas the rest of the samples were returned to the salesman. This knowledge of facts obviously incited Schoep to set off for this single science museum in the hope of recovering the provenance of the minerals still in the possession of Butler. A painstaking visual analysis done on the spot by Schoep of the label attached to the sample purchased by the Museum of Practical Geology revealed an inscription looking like *Kabalo*, *Kabolo* or Kasolo, followed by *Belg. Cong*. After having taken due note of the supposed inscription, Schoep returned to Butler's shop and purchased the box with what was left of the minerals for the fair sum of one pound sterling. After closing sale, Butler unburdened that he already did some

[23] Richard Talling (1820-1883) is known to have been the greatest Cornish mineral dealer of all time. When he died he left his mineral collection and stock of minerals to Francis Henry Butler.

[24] This museum is presently known as The Geological Museum of London.

research on the samples and came to the conclusion that they contained a lot of lead and possibly also some uranium.

At his return to Belgium, Schoep consulted with the *Comité Spécial du Katanga* in Brussels about the possible locality names he thought to have recognized on the label he saw in the Museum of Practical Geology in London. His final conclusion was that the source of the mineral specimen was Kasolo, a hill located 2 km south-west of Shinkolobwe. It was at this locality that in 1915 one of the largest uraniferous deposits of the world was discovered by Major Robert Rich Sharp. Sharp was a British prospector who became employee of *Union Minière du Haut-Katanga* after its creation in 1906. For Schoep it was at once clear that, as to the origin of the mineralogical treasure he bought in London, everything was now fixed up and he started analysing it in the lab. The outcome was that from the end of 1921 until the middle of the 1920s Schoep was able to acquaint the international mineralogical community of the existence of whole set of minerals which so far were unknown to science. Some minerals were given a name after prominent foreign scientists (Henri Becquerel, Pierre Curie, Henri Julien, Arthur Parsons, Maria Sklodowska and Frederick Soddy). Others were supplied with a name honouring mineralogists, geologists and chemists on the home front (buttgenbachite, dewindtite, dumontite, renardite, stainierite, stasite and vandenbrandite). A third group reminded of a physical property (epiianthinite, ianthinite), or a locality in Katanga (kasolite).

When Giuseppe Cesàro retired in 1921 Henri Buttgenbach[25] became full professor in charge of crystallography, mineralogy and petrography of igneous rocks at the University of Liège. The mineralogical research and mining explorations the latter conducted in Upper Katanga started in 1902 and lasted almost half a century. At the establishment of the *Union Minière du Haut-Katanga* (October 1906), Buttgenbach was appointed member of the Board. He studied also the minerals in the subsoil of Belgium and his book on the *Minerals of Belgium and the Belgian Congo* is still today a valuable reference work. The mineral **buttgenbachite,** called into existence by Alfred Schoep, paid without doubt a tribute to one of the most qualified mineralogists of Belgium. For the sake of his pioneering work in the Congo Free State and the former Belgian Congo, he is also rightly named 'the father of the Congolese Mineralogy'.

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[25] Mélon, J., 1968. Memorial of Henri Buttgenbach. *The American Mineralogist*, **53**, 536-544.

Jan De Windt[26], son of a physician, was a pupil and assistant to Alphonse Renard. During his geological studies at the University of Ghent he attested to great social engagement, and as a member of the general committee of the Willemsfonds and secretary-treasurer of the local branch of this non-profit cultural organization in his home town Aalst, he founded two subdivisions of the Willemsfonds: '*Higher Education for the People*' and '*Self-Education*'. On the advice of Alphonse Renard, De Windt traveled in 1896-1897 to Vienna and Berlin to specialize in geomorphology under Albrecht Penck[27] and Ferdinand Paul Wilhelm, Baron von Richtenhofen[28]. During his stay in Berlin, he was asked to participate in an exploratory mission to Katanga organized by the Government of the Congo Free State and directed by lieutenant Charles Lemaire (1863-1926). In the first days of August 1898, Jan De Windt, the British gold prospector William Caisley and several natives transported equipment of the Lemaire expedition with pirogues to Moliro, a settlement at the southern extremity of Lake Tanganyika. During their travelling on the lake they were caught in a heavy storm and drowned. As Schoep commenced his university studies hardly three years after De Windt had perished, is it any wonder that he honoured this young and deserving geologist of his Alma Mater with a new mineral name (**dewindtite**).

Both Alphonse Renard and Xavier Stainier were professor at the Geological Institute in Ghent. Shortly after being ordained, Alphonse Renard[29] was appointed curator to the department of mineralogy and lithology of the Royal Belgian Museum of Natural History in Brussels. Next, he entered as full professor at the Faculty of Sciences of the University in Ghent, where for fifteen years (from 1888 until 1903) he taught crystallography, mineralogy, geology and palaeontology. One of his major achievements was to be the very first to introduce in Belgium the use of the polarizing microscope in the scientific study of minerals and rocks. He got conversant with

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26 Coosemans, M., 1952. Windt (De) (Jean-Charles-Louis), géologue. *Biographie coloniale belge*, **III**, col. 928-930.

27 Albrecht Penck (1859-1945) was a German geologist and geographer. He discovered the four major glaciations of the Alps and taught in München (1882-1885), Vienna (1885-1906), Berlin (1906-1927) and Prague (1927-?).

28 Ferdinand Paul Wilhelm, Baron von Richtenhofen (1833-1905) was a famous German explorer, geologist and geographer. He is the founder of geomorphology. He explored China and the term 'silkroad' was introduced by him.

29 Roekeloos, A., 1976. La vie mouvementée d'un savant: Alphonse Renard. *Annales du Cercle Historique et Archéologique de Renaix et du Tènement d'Inde*, **XXV**, 163-189.

this technique during scientific missions to Vienna and Leipzig, and became a real expert in this matter. Renard earned international reputation through his research on rocks from oceanic islands and deep-sea sediments collected by the scientific staff of the *Challenger Expedition* (1872-1876). The validity of the mineral species called **renardite** by Schoep is still questionable.

Stainierite, presently a discredited name, alludes to Xavier Stainier[30], former professor of geology at Ghent University, where in 1903 he succeeded to Alphonse Renard. Although Stainier was pre-eminently a field geologist and an authority on mines, having a great liking for the stratigraphy and tectonics of the Belgian coal-basins, he published in almost all research areas spanning geological sciences. In addition, he also wrote articles on archaeological, prehistoric and anthropological topics. Moreover, he was remarkably productive. His bibliography makes mention of hundreds of papers and reports, being equal to over 6.000 pages of original scientific work. Over his career he also collected an impressive amount of rock samples, the majority of them in relation to coal beds in Belgium.

Having been working four years at the Laboratory of Analytical and Toxicological Chemistry of the Faculty of Medicine in Ghent and being the son of a pharmacist, Alfred Schoep took always a lively interest in chemistry. Not surprising thus that one of the first minerals he discovered and named after a Belgian scientist was called **stasite**, in honour of Jean-Servais Stas. Stas initially aimed to become physician but switched to chemistry. From 1840 onwards he lectured at the Royal Military School in Brussels. His research work was concerned mainly with atomic weight determinations – including the atomic weights of oxygen and carbon – and in this field of research he was of worldwide fame. He is considered to be one of the most skilful chemical analysts of the nineteenth century and, in addition, to be a founder of modern toxicology and a pioneer of the study of industrial pollution. As stasite was later shown to be identical to dewindite, this mineral species has been removed from the list of IMA-approved minerals. A better fortune was granted to the mineral **vandenbrandeite** that was classified among the grandfathered minerals by the IMA-CMMN. However, the lot

[30] Stockmans, F., 1970. Stainier (Xavier-Philibert-Joseph), géologue et professeur. *Biographie nationale*, **XXXV**, col. 688-693.

that fell to agronomist Pierre Vanden Brande[31], who discovered the uranium mineral deposit of Kalongwe, Katanga, and spent a long professional career in the former Belgian Congo, was much less fortunate. During soil and vegetation mapping in our old colony he was attacked by a herd of elephants and succumbed to his injuries.

Lucien De Leenheer (1912-1984) was a pupil of Alfred Schoep. Though graduated as agronomist and pedologist, De Leenheer's academic career started at the laboratory of mineralogy of the University of Ghent, where he worked under the guidance of Schoep. During two years (1934-1935), he studied hydrated oxides of cobalt coming from mines in Katanga, and in particular samples collected at Mindigi and Shinkolobwe. This allowed him to recognize three new minerals species, and two of them (boodtite, and trieuite) were given a name after a Belgian. All were discredited about thirty years after their first description and they are considered now to be synonyms of heterogenite, a black oxyhydroxide of cobalt. The name **boodtite** refers to Anselmus Boetius de Boodt[32], who was born and passed away in Bruges. He is known to have been a mineralogist, court physician to Rudolf II in Vienna and Prague, and author of *Gemmarum et Lapidum Historia*, a famous treatise on gemology. **Trieuite** was named after Robert du Trieu de Terdonck[33], who graduated as mining engineer and engineering geologist from the Catholic University of Louvain. During his professional career du Trieu de Terdonck was very active in the field of mine prospection. For years, he was employed by the *Union Minière du Haut-Katanga*, at first in the former Belgian Congo and later in the central administration of this company in Brussels.

The names of the minerals Henri Buttgenbach first described in the interbellum (**bialite**, **cesàrolite**, **droogmansite**, **fourmarierite** and **thoreaulite**) are either reminiscent of colleagues from his own university (G. Cesàro, P. Fourmarier) or the University of Louvain (J. Thoreau), or they are linked with pioneers of the Congo Free State (L. Bia, H. Droogmans), who contributed largely to the economic development of this part of

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31 Walraet, M., 1968. Vanden Brande (Pierre). *Biographie belge d'Outre-Mer*, **VI**, col. 113-114.

32 Dewalque, G., 1873. de Boodt (Anselme Boèce) ou de Boodt. *Biographie Nationale*, **IV**, col. 814-816.

33 Lederer, A., 1998. Trieu de Terdonck (du) (Robert). *Biographie belge d'Outre-Mer*, **VIII**, col. 421-424.

Central Africa. As shown in Appendix 1, three of these mineral species are still standing as valid species.

As mentioned earlier, Captain Lucien Bia[34], who originated from Liège and joined the Belgian Army in 1870, headed the expedition Jules Cornet participated in from May 1891 to April 1893. Hubert Droogmans[35], who studied economy, inititialy worked as Secretary-General to the Finance Department of the Congo Free State, became chairman of the *Comité Spécial du Katanga* at the very beginning of 1900 and was appointed Secretary-General to the Ministry of Colonies when Belgium took possession of its colony. Paul Fourmarier[36] was appointed ordinary professor at the University of Liège when Max Lohest retired. His scientific research focused on general geology and structural geology inside and outside Belgium. A substantial part of his field activities took place in the former Belgian Congo, where he was very active in the field of geological mapping and stratigraphic research. He was a most productive geologist as shown by the nearly 600 treatises, books, papers, notes and reports he wrote during his professional career and his retirement. Jacques Thoreau[37] was a mining engineer and became full professor of geology, crystallography, mineralogy and petrography at the Catholic University of Louvain in 1920. Parallel to his academic commitments, he was also consultant for private colonial societies and public colonial organizations. His field work took him to mining areas throughout Europe and Africa, including the mineral-rich Katanga. Here he focused on problems related to hydrology, oil exploration and ore prospection.

From the foregoing we can deduce that in the interbellum, thanks to investigations on uranium minerals of Shinkolobwe, Kasolo and Kalongwe (Katanga) conducted by A. Schoep and his co-workers L. De Leenheer, V. Billiet and A. Vandendriessche, the University of Ghent earned a very solid international reputation in the field of mineralogical research. Nine on a total of 19 new mineral species were firstly described in Ghent. This was to some extent the result of a conjunction of favourable circumstances, but

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34 Buttgenbach, H., 1951. Bia (Lucien). *Biographie Coloniale belge*, **II**, col. 58-62.

35 Robert, M., 1955. Droogmans (Hubert). *Biographie Coloniale belge*, **IV**, col. 242-247.

36 Calembert, L., 1971. Paul Fourmarier. *Bulletin de la Société géologique de France*, 7me série, **13**, 210-218.

37 Denaeyer, M.-E., 1974. Jacques Thoreau. *Bulletin des Séances de l'Académie royale d'Outre-Mer*, **20**, 49-57.

above all it highlights the scientific skills, perseverance and team-building capacities of Schoep, who was the inspiring director of the Department of Mineralogy and Crystallography between WW1 and WW2. In concert with what occurred in the long period preceding WW1, the mineralogical research carried out in the interbellum at the University of Liège continued to be at very high level thanks to the excellent work done by Henri Buttgenbach in the former Belgian Congo, Belgium and other countries worldwide.

3.3. From 1940 until today

Of the 18 new mineral species discovered between the onset of WW2 and the very end of the 1950s, eight were first described by Johannes Vaes (1902-1978), a Dutch mining engineer. After Vaes served as assistant to mineralogist Jan A. Grutterink at the Technical Highschool in Delft (The Netherlands), he took up office with the *Union Minière du Haut-Katanga*. From 1926 to the late 1940s, in parallel with prospection of copper, cobalt, tin, gold and uranium in the former Belgian Congo, he did pioneering research on new or poorly known minerals. In this period he published following new mineral species: **billietite**, **diderichite**, **masuyite**, **vandendriesscheite**, **richetite**, **renierite**, **sengierite** (in co-authorship with the famous American mineralogist Paul Francis Kerr) and **cousinite**. A few years after WW2, J. Vaes applied for a docentship in mineralogy at the University of Ghent but the nomination failed for budgetary reasons. The mineral species reported above were not the first Vaes discovered. In the early 1930s he already described **saléeite**. This name was given in honour of canon Achille Salée (1883-1932)[38], professor of stratigraphical palaeontology and animal palaeontology at the Catholic University of Louvain, who in March 1932 was victim of a deadly car crash during a geological mission in the south of present-day Rwanda.

Seven new mineral species described by Vaes were originally found at Shinkolobwe, whereas one came from the Prince Leopold Mine (Kipushi) and another from Luiswishi, Upper Katanga. Three of them pay homage to

[38] Delhaye, F., 1933. Le Chanoine Achille Salée. *Bulletin des Séances de l'Institut royal colonial belge*, **4**, 28-37.

a victim of WW2. Valère Billiet (1903-1945)[39], who set up a laboratory of röntgen-analysis at the University of Ghent, died a few days before the end of the war, when the ocean-liner SS *Cap Arcona*, carrying thousands of prisoners of Nazi concentrations camps, was bombed by the Royal Air Force in the bay of Neustedt (Lübeck). Adrien Vandendriessche (1914-1940)[40] was killed in action at Ursel (Knesselare) during the 18 day campaign, whereas Gustave Masuy (1905-1945)[41], who served the colonial mining industry, most probably died as a result of maltreatment, physical sapping, illness and affliction, having been a concentration camp prisoner for a long period of time. The other minerals honour Norbert Diderich[42], Emile Richet[43], Edgar Sengier[44] and Jules Cousin[45] who spent the greater part of their professional career in Katanga and contributed much to the economic development of this part of Central Africa.

It appears that many new mineral species discovered between 1960 and 1980 were published in articles authored and co-authored by French mineralogists of the universities of Paris (F. Cesbron, B. Bachet) and Orléans (R. Pierrot), American and Canadian mineralogists (C. Christ, J. Clark, J. Francotte, G. Chao, P. Mainwaring, J. Baker), and mining engineers of the *Union Minière du Haut-Katanga* (R. Oosterbosch) and the *Union Minière Exploration and Mining Corporation Ltd*, Toronto, Canada (T. Verbeek). Five honour Belgian staff members of the UMHK (**demesmaekerite**, **marthozite**, **derriksite**, **briartite** and **oosterboschite**) and were discovered either in the oxidation zone of a Cu-Co deposit at Musonoi, near Kolwezi (Katanga), or in the Prince Leopold Mine, at Kipushi (Katanga).

From 1960 to 1980 relatively few Belgian researchers were involved in the description of new mineral species. The new technologies which in these years were used in mineral research were fairly expensive and not yet available in Belgium. This unfavourable condition changed however in the

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39 Hacquaert, A., 1960. Valère Billiet. Rijksuniversiteit te Gent. Liber memorialis 1913-1960, **II,** 324-326.

40 Van Der Meersche, E., De Paepe, P. & Stoops, G., 2010. Adrien Vandendriessche. *Minerals with Belgian Roots. From hopeite (1824) to tazieffite (2009)*. 212, Academia Press, Ghent.

41 Stevens, Ch., 1945. Gustave Masuy. *Bulletin de la Société belge de Géologie, de Paléontologie et d'Hydrologie*, **54**, 141.

42 Van Den Abeele, M., 1952. Diderrich (Norbert). *Biographie coloniale belge*, **III**, col. 239-244.

43 Van Der Meersche, E., De Paepe, P. & Stoops, G., 2010. Emile Richet, *Minerals with Belgian Roots from hopeite (1824) to tazieffite (2009)*, 185, Academia Press, Ghent.

44 E. Van der Straeten, 1973. Sengier (Edgar). *Biographie belge d'Outre-Mer*, **VII-A**, col. 429-437.

45 E. Roger, 1967. Cousin (Jules). *Biographie belge d'Outre-Mer*, **VI**, col. 241-246.

beginning of the 1980s and from then onward the centre of gravity of the descriptive mineralogical research in Belgium was to be found in the Royal Belgian Institute of Natural Sciences in Brussels, the Royal Museum for Central Africa in Tervuren and at the campus of the Catholic University of Louvain at Louvain-la-Neuve. Mineralogists and chemists of the universities of Liège, Leuven and Antwerp spent also a lot of time and efforts to this subject but in a much lesser degree than their colleagues at the Royal Belgian Institute of Natural Sciences (Brussels), the Royal Museum for Central Africa (Tervuren) and the Catholic University of Louvain (Louvain-la-Neuve).

The research in the field of systematic and Congolese mineralogy carried out since the late 1970s by geologist Michel Deliens and crystallographer Paul Piret can hardly be overestimated. The bibliography of Michel Deliens[46], who worked initially at the Royal Museum for Central Africa (Tervuren) and subsequently joined the department of mineralogy and petrography of the Royal Belgian Institute of Natural Sciences (Brussels), includes 36 articles presenting a total of 39 new mineral species, among them 10 named after a Belgian scientist: **blatonite**, **comblainite**, **françoisite-(Nd)**, **lepersonnite-(Gd)**, **metavanmeersscheite**, **moreauite**, **mundite**, **oswaldpeetersite**, **piretite** and **vanmeersscheite**. The majority of these mineral species were discovered in the Democratic Republic of the Congo (8 in total), while two came from the USA. Seven of the new mineral species listed above were published by Deliens in co-authorship with his colleague and friend Paul Piret.

Paul Piret (1932-1999)[47] studied chemistry and was much involved in crystallographic research during his professional career at the Catholic University of Louvain (Louvain-la-Neuve). He wrote many articles in collaboration with Maurice Van Meerssche, who was the director of the laboratory of physical chemistry at Louvain-la-Neuve, and described some thirty new mineral species in co-authorship with M. Deliens. Nearly all the new mineral species described in co-authorship by Piret and Deliens, were discovered during research devoted to copper-cobalt- and uranium-bearing secondary mineral associations in southern Katanga and uranium-bearing

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46 Van Der Meersche, E., De Paepe, P. & Stoops, G., 2010. Michel Deliens. *Minerals with Belgian Roots from hopeite (1824) to tazieffite (2009)*, 122, Academia Press, Ghent.

47 Van Der Meersche, E., De Paepe, P. & Stoops, G., 2010. Paul Piret. *Minerals with Belgian Roots from hopeite (1824) to tazieffite (2009)*, 176, Academia Press, Ghent.

mineralizations associated with the beryl- and columbite-bearing pegmatite of Kobokobo (Lusungu River district) in southern Kivu (Democratic Republic of the Congo).

Toward the end of the 20th century Deliens also collaborated occasionally with chemist Renaud Vochten[48], full professor to the University of Antwerp and visiting professor at the University of Ghent. Vochten co-authored the paper on blatonite, oswaldpeetersite and piretite, described **deliensite** and in turn was honoured with a new mineral species that, thanks to Pieter Cornelis Zwaan of the National Museum of Geology and Mineralogy in Leiden (The Netherlands) and Eddy De Grave of the department of physics and astronomy at the University of Ghent, got the name **vochtenite**.

Mineral species such as **andremeyerite**, **camermanite**, **drugmanite**, **fransoletite**, **graulichite**, **jedwabite**, **mélonjosephite**, **parafransoletite**, **vantasselite** and **viaenite**, together with those mentioned just now, remind us that in the post-interbellum period deserving mineralogical and geological research was carried out at several Belgian universities and museums. A whole series of researchers working at a certain moment at one of these institutions were immortalized indeed by a mineral name in recognition for the work they did in the field of mineralogy: André Meyer[49], Carl Camerman[50], Julien Drugman[51], André-Mathieu Fransolet[52], Jean-Marie Graulich[53], Jacques Jedwab [54], Joseph Mélon[55], René Van Tassel[56] and Willy Viaene[57].

The most recently discovered mineral species that was given a name after a Belgian citizen is **ernstburkeite**. This mineral was found in an ice core at Dome Fuji station, Queen Maud Land, Eastern Antarctica, and it was approved by the IMA Commission on New Minerals, Nomenclature ad Classification in February 2011 (IMA No. 2010-059). Ernst A.J. Burke

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48 Van Der Meersche, E., De Paepe, P. & Stoops, G., 2010. Renaud Vochten. *Minerals with Belgian Roots. From hopeite (1824) to tazieffite (2009)*. 221, Academia Press, Ghent.
49 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* André Meyer, 86.
50 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* Carl Camerman, 29-30.
51 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* Julien Drugman, 42.
52 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* André-Mathieu Fransolet, 151.
53 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* Jean-Marie Graulich, 53.
54 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* Jacques Jedwab, 153.
55 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* Joseph Mélon, 166.
56 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* René van Tassel, 75.
57 Van Der Meersche, E., De Paepe, P. & Stoops, G., *op. cit.* Willy Viaene, 78.

studied geology and mineralogy at the University of Leuven. He is a permanent resident of The Netherlands since 1 November 1965 but still has the Belgian nationality. He joined the *Vrije Universiteit Amsterdam* in 1965, where he was lecturer and senior lecturer (crystallography, mineralogy and ore microscopy) before becoming head of the Laboratory of Microanalysis (1991-2002). Ernst Burke was associate editor (1988-1995) and chief editor (1995-2001) of the *European Journal of Mineralogy* and from 2003 to 2008 Chairman of the Commission on New Minerals, Nomenclature and Classification of IMA.

As conclusion we can say that contrary to what happened during the interbellum and between 1940 and 1970, when the new mineral species originated overwhelmingly from the present-day Democratic Republic of the Congo (DRC), it is obvious that since 1970 the number of new mineral species coming from countries other than Belgium and the DRC increased in a significant and progressive way. Indeed, of the 32 mineral species described as new since 1970 and having a name derived from a Belgian citizen or locality, 13 were discovered outside the borders of Belgium and those of its former colony. These exotic mineral species originated indeed in Antarctica, Canada, France, Italy, Morocco, the Russian Federation, Switzerland, the United Kingdom and the United States of America, and we suppose that in the coming years this trend will persist.

4. Final conclusions

According to Philippe Roth[58] from the Swiss Federal Institute of Technology in Zürich (ETHZ), Belgium is amongst the most prolific countries in terms of density of new mineral species per surface unit. Using the density of new species per 1000 km^2, our country is preceded it is true by Switzerland, the Czech Republic, Italy, Germany and Austria, but it puts back several countries with great mineralogical tradition such as e.g. the United Kingdom, France, Russia and the USA.

The present study demonstrates that the geographical distribution of the type localities of all mineral species named after a geographical feature

[58] Roth, Ph., 2006. *Minerals first discovered in Switzerland and minerals named after Swiss individuals*, 239 p. Kristallografik Verlag, Achberg, Germany.

(locality, region, etc.) related to Belgium is small and points invariably to the Ardenne region. The oldest rocks of Belgium are exposed here and in many places ore deposits, some of them very useful in zinc production, were extracted for a long period of time. It shouldn't cause any surprise that scientists from the University of Liège were the first to bring to the open the mineralogical treasures of southeastern and eastern Belgium. The geological mapping they carried to a successful conclusion in the provinces of Liège and Luxembourg during the 19th and the early 20th century enabled them to study thoroughly the mineralogy of all the rocks of the basement, including the associated ore deposits, and led to the discovery of several new mineral species. Some got a name reminiscent of the place of discovery, while others were given a name honouring a pioneer of the geological research conducted in that area.

When the Congo Free State was annexed as a Belgian colony by the government of Belgium (1908) the geological and mineralogical research in Katanga, that initiated in the late 19th century, received great attention thanks to geologists and mining engineers serving private companies such as the *Union Minière du Haut-Katanga*. This explains why from 1912 onwards many new minerals species with Belgian roots were described in our former colony, the first of all being cornetite, named for Jules Cornet and discovered at the Star of the Congo Mine, southern Katanga. A major event in the history of the mineralogy of the present-day Democratic Republic of the Congo was of course the finding during the First World War of uranium bearing minerals at Shinkolobwe, Kasolo and Kalongwe. This occurrence supplied the mineralogical community soon with a whole series of new mineral species, many of them named after Belgian individuals who contributed in a substantial way to the exploration and economic development of this part of Central Africa.

This state of things didn't change substantially from the 1940s until about thirty year after the Congolese declaration of independence on June 30, 1960. From the early 1990s onwards however more and more new mineral species with Belgian roots were discovered outside Belgium and its former colony, and for evident reasons it is to be expected that this situation will not change in the years to come.

Acknowledgments

The author of the present paper would like to thank Prof. dr. Robert Rubens and all members of the Sarton Committee of the University of Ghent for awarding him the Sarton medal 2011-2012 from the Faculty of Sciences. He is also very grateful to Prof. dr. Peter Van den haute, his former colleague of the Laboratory of Mineralogy and Petrology, who submitted in due time his candidature for this award to the Faculty of Sciences. He also acknowledges the support of Prof. dr. Danny Segers and his team of the Museum of the History of Science and, last but not least, he expresses his great gratitude to lic. Eddy Van Der Meersche and Prof. dr. Georges Stoops for their major contribution in the realization of "*Minerals with Belgian Roots from hopeite (1824) to tazieffite (2009*) that was published in the spring of 2010 and gives more in-depth information and hundreds of full colour photographs about the topic treated in this paper.

APPENDIX 1: Chronological catalogue of mineral species named for Belgian citizens and localities

(after Van Der Meersche, E., De Paepe, P. & Stoops, G., 2010, updated)

Year	Species*	IMA-Status	Named after	Author(s)
1826	Halloysite (B)	G	Omalius d'Halloy, Jean-Baptiste (1783-1875)	Berthier, P.
1833	*Calamine* (B)	D	La Calamine (Moresnet)	Davreux, C.
1838	Delvauxite (B)	Q	Delvaux de Fenffe, Jean Charles (1782-1863)	Dumont, A.-H.
1842	*Ottrelite* (B)	G	Ottré (Vielsalm)	Damour, M. & des Cloizeaux, A.
1848	*Bastonite* (B)	D	Bastogne	Dumont, A.-H.
1852	Davreuxite (B)	G	Davreux, Charles (1800-1863)	Dumont, A.-H.
1865	*Moresnetite* (B)	D	Moresnet	Risse, H.
1872	Dewalquite (B)	D	Dewalque, Gustave (1828-1905)	Pisani, F.
1872	*Ardennite* (B)	D	Ardenne	von Lasaulx, A.
1879	Destinezite (B)	Rd	Destinez, Pierre (?-1911)	Forir, H.
1883	*Richellite* (B)	Q	Richelle	Cesàro, G. & Despret, G.
1883	Koninckite (B)	G	de Koninck, Laurent-Guillaume (1809-1887)	Cesàro, G.
1884	*Salmite* (B)	D	Salm river	Prost, E.
1888	*Ciplyite* (B)	D	Ciply (Mons)	Ortlieb, J.
1890	Klementite (B)	D	Klement, Constantin (1856-1902)	Tschermak, G.
1912	Cornetite (C)	G	Cornet, Jules (1865-1929)	Cesàro, G.
1916	*Belgite* (B)	D	Belgium	Panebianco, R.
1920	Cesarolite (T)	G	Cesàro, Giuseppe (1849-1939)	Buttgenbach, H. & Gillet, C.
1922	Stasite (C)	D	Stas, Jean (1813-1891)	Schoep, A.
1922	Dewindtite (C)	G	De Windt, Jan (1876-1898)	Schoep, A.
1923	Schoepite (C)	G	Schoep, Alfred (1881-1966)	Walker, T.
1923	Lohestite (B)	D	Lohest, Maximilien (Max) (1857-1926)	Anten, J.
1923	Fourmarierite (C)	G	Fourmarier, Paul (1877-1970)	Buttgenbach, H.
1924	Dumontite (C)	G	Dumont, André-Hubert (1809-1857)	Schoep, A.
1925	Buttgenbachite (C)	G	Buttgenbach, Henri (1874-1964)	Schoep, A.
1925	Droogmansite (C)	D	Droogmans, Hubert (1858-1938)	Buttgenbach, H.
1927	Bialite (C)	D	Bia, Lucien (1852-1892)	Buttgenbach, H.
1927	Fraipontite (B)	G	Fraipont, Julien (1857-1910) and de Fraipont, Charles (1883-1946)	Cesàro, G.
1928	Renardite (C)	Q	Renard, Alphonse (1842-1903)	Schoep, A.
1929	Stainierite (C)	D	Stainier, Xavier (1865-1943)	Schoep, A. & Cuvelier, V.
1932	Legrandite (ME)	G	Legrand, Charles (1873-?)	Drugman, J. & Hey, M.
1932	Vandenbrandeite (C)	G	Vanden Brande, Pierre (1896-1957)	Schoep, A.
1932	Saléeite (C)	G	Salée, Achille (1883-1932)	Thoreau, J. & Vaes, J.
1933	Thoreaulite (C)	G	Thoreau, Jacques (1886-1973)	Buttgenbach, H.
1935	Trieuite (C)	D	de Trieu de Terdonck, Robert (1889-1970)	De Leenheer, L.

Year	Species*	IMA-Status	Named after	Author(s)
1936	Boodtite (C)	D	Boodt, Anselmus Boetius de (1550-1632)	De Leenheer, L.
1942	*Franquenite* (B)	D	Franquenies (Court-Saint-Etienne)	Anthoine, R. & Antoine, P.
1942	*Viseite* (B)	D	Visé (Wezet)	Mélon, J.
1945	Cattierite (C)	G	Cattier, Félicien (1869-1946)	Kerr, P.
1947	Billietite (C)	G	Billiet, Valère (1903-1945)	Vaes, J.
1947	Diderichite (C)	D	Diderrich, Norbert (1867-1925)	Vaes, J.
1947	Masuyite (C)	G	Masuy, Gustave (1905-1945)	Vaes, J.
1947	Paraschoepite (C)	G	Schoep, Alfred (1881-1966)	Schoep, A. & Stradiot, S.
1947	Vandendriesscheite (C)	G	Vandendriessche, Adrien (1914-1940)	Vaes, J.
1947	Richetite (C)	G	Richet, Emile (1884-1938)	Vaes, J.
1947	Anthoinite (C)	1972-005	Anthoine, Raymond (1888-1971)	Varlamoff, N.
1947	Varlamoffite (C)	Q	Varlamoff, Nicolas (1910-1976)	Buttgenbach, H.
1948	Renierite (C)	G	Renier, Armand (1876-1951)	Vaes, J.
1949	Sengierite (C)	G	Sengier, Edgar (1879-1963)	Vaes, J. & Kerr, P.
1950	Metasaléeite (C)	G	Salée, Achille (1883-1932)	Mrose, M.
1952	Camermanite (B)	D	Camerman, Carl (1885-1958)	Denaeyer, M.-E. & Ledent, D.
1956	Borgniezite (C)	D	Borgniez, Georges (1901-1965)	de Bethune, P. & Meyer, A.
1958	Cousinite (C)	Q	Cousin, Jules (1884-1965)	Vaes, J.
1959	Delhayelite (C)	G	Delhaye Fernand (1880-1946)	Sahama, T. & Hytönen, K.
1960	Metavandendriesscheite (C)	A	Vandendriessche, Adrien (1914-1940)	Christ, C. & Clark, J.
1965	Briartite (C)	1965-018	Briart, Gaston (1897-1962)	Francotte, J. *et al.*
1965	Demesmaekerite (C)	1965-019	Demesmaeker, Gaston (1911-?)	Cesbron, F. *et al.*
1965	Metaschoepite (C)	G	Schoep, Alfred (1881-1966)	Christ, C.
1969	Marthozite (C)	1968-016	Marthoz, Aimé (1894-1962)	Cesbron, F. *et al.*
1970	Oosterboschite (C)	1970-016	Oosterbosch, Robert (1908-1992)	Johan, Z. *et al.*
1971	Derriksite (C)	1971-033	Derriks, Joseph (1912-1992)	Cesbron, F. *et al.*
1972	Eylettersite (C)	1969-035	Van Wambeke-Eyletters, Lea	Van Wambeke, L.
1973	Andremeyerite (C)	1972-005	Meyer, André (1920-1965)	Sahama, T. *et al.*
1973	Melonjosephite (MA)	1973-012	Mélon, Joseph (1898-1991)	Fransolet, A.-M.
1978	Donnayite-(Y) (CN)	1978-007	Donnay, Joseph (1902-1994) and Hamburger, Gabrielle (1920-1987)	Chao, G. *et al.*
1979	Drugmanite (B)	1978-081	Drugman, Julien (1875-1950)	Van Tassel, R. *et al.*
1980	Comblainite (C)	1978-009	Comblain, Gordon (1920-1996)	Piret, P. & Deliens, M.
1982	Lepersonnite-(Gd) (C)	1981-036	Lepersonne, Jacques (1909-1997)	Deliens, M. & Piret, P.
1982	Vanmeersscheite (C)	1981-009	Van Meerssche, Maurice (1923-1990)	Piret, P. & Deliens, M.
1982	Metavanmeersscheite (C)	1981-010	Van Meerssche, Maurice (1923-1990)	Piret, P. & Deliens, M.
1983	Fransoletite (US)	1982-096	Fransolet, André-Mathieu (b.1947)	Peacor, D. *et al.*
1985	Moreauite (C)	1984-010	Moreau, Jules (b.1931)	Deliens, M. & Piret, P.
1985	Mundite (C)	1980-075	Mund, Walter (1892-1956)	Deliens, M. & Piret, P.
1987	Vantasselite (B)	1986-016	Van Tassel, René (b.1916)	Fransolet, A.-M.
1988	Françoisite-(Nd) (C)	1987-041	François, Armand (1922-2012)	Piret, P. *et al.*
1989	Vochtenite (UK)	1987-047	Vochten, Renaud (1933-2012)	Zwaan, P. *et al.*
1992	Parafransoletite (US)	1989-049	Fransolet, André-Mathieu (b.1947)	Kampf, A. *et al.*
1996	Piretite (C)	1996-002	Piret, Paul (1932-1999)	Vochten, R. *et al.*
1996	Viaenite (B)	1993-051	Viaene, Willy (1940-2000)	Kucha, H. *et al.*

Year	Species*	IMA-Status	Named after	Author(s)
1997	Deliensite (F)	1996-013	Deliens, Michel (b.1939)	Vochten, R. *et al.*
1997	Jedwabite (RU)	1995-053	Jedwab, Jacques (b.1925)	Novgorodova, M. *et al.*
1998	Blatonite (US)	1997-025	Blaton, Norbert (b.1945)	Vochten, R. & Deliens, M.
2001	Oswaldpeetersite (US)	2000-034	Peeters, Oswald Maurice (b.1945)	Vochten, R. *et al.*
2002	Graulichite-(Ce) (B)	2002-001	Graulich, Jean-Marie (1920-2001)	Hatert, F. *et al.*
2002	Verbeekite (C)	2001-005	Verbeek, Theodoor (1927-1991)	Roberts, A. *et al.*
2005	*Stavelotite* (B)	2004-014	Stavelot	Bernhardt, H.J. *et al.*
2007	*Ardennite-(V)* (IT)	2005-037	Ardenne	Barresi, A. *et al.*
2007	*Ardennite-(As)* (B)	Rn	Ardenne	Barresi, A. *et al.*
2007	Françoisite-(Ce) (SW)	2004-029	François, Armand (1922-2012)	Meisser, N. *et al.*
2007	Arsenovanmeersscheite (G)	2006-018	Van Meerssche, Maurice (1923-1990)	Walenta, K. & Theye, T.
2009	Tazieffite (RU)	2008-012	Tazieff, Haroun (1914-1998)	Zelenski, M. *et al.*
2011	Ernstburkeite (AN)	2010-059	Burke, Ernst (b.1943)	Sakurai, T. *et al.*

* Places of discovery are given between brackets: AN = Antarctica; B = Belgium; C = Democratic Republic of Congo; CN = Canada; F = France; G = Germany; IT = Italy; MO = Morocco; ME = Mexico; RU = Russian Federation; SW = Switzerland; T = Tunisia; US = United States of America. Mineral species with a name referring to a locality or another geographical feature are in italic type.

Laudatio Dr. Luc Devriese

Frank Gasthuys
Dean of the faculty of veterinary medicine

Dr. Luc Devriese is born in 1941 as the eldest son of a family of farmers from Pittem near Tielt in the province of West Flanders of Belgium. He came to the university of Ghent to study veterinary medicine. He finished his studies as veterinary surgeon in 1967. Immediately after his studies he became a staff member in the then existing school of veterinary science, later to become the faculty of veterinary medicine.

Very soon in 1967 he became resident, chief resident in 1972 and junior lecturer in 1977. In 2003 he retired. His whole professional career was spent in the department of pathology, bacteriology and poultry diseases. He did do mainly research in the taxonomy and antibiotic resistence of animal pathogens and the gut and skin parasites of animals. Luc was also the last years of his career the editor of The Flemish Veterinary Journal (Vlaams Diergeneeskundig Tijdschrift).

After his retirement he helped to start the initiative of Prof. Hoorens: the Museum of veterinary medicine. As chairman of the Committee of the museum about the history of veterinary medicine he still is active until now. He is the editor of the articles “From the past” in the Flemish Veterinary Journal. He is making two yearly exhibitions in the atria of our lecture halls. Recently he made an exhibition in the dean’s building: “Veterinary past in the showcase”.

Meanwhile Dr. Devriese has more than 140 papers and reports in various journals about local history, about archeology to folklore (mainly about animal matters) most of them in Dutch.

Some titles: "*Schelleken boeboe, bezweringen bedwingen onwillige paarden, koe ter dood veroordeeld, trekhond opkomst, paarden sneuvelen of worden opgegeten, vinkensport 400 jaar geleden, rattenpakkers-gevechten, miraculeuze offerkippen, boterhonden en hondenpompen, kattenverbranding "* and finally "*Heilige Brigitta en Cornelius*".

The title of his lecture is: From mule, horse and livestock to companion animals. A linguistic-etymological approach to veterinary history, mirroring animal and (mainly) human welfare.

From mules, horses and livestock to companion animals. A linguistic-etymological approach to veterinary history, mirroring animal and (mainly) human welfare

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Abstract

In some languages, major changes in the veterinary profession are mirrored in the names of those engaged in this branch of medicine during different periods of history. These names were most often derived from the animal species that were of predominant importance in any given period. The terms *veterinarius*, *mulomedicus* (mule healer) and *hippiater* (horse doctor) reflect the major importance of these animals in Roman and Greek antiquity. Draft and pack animals (Latin: *veterina*) played a major role in the improvement of mankind's living conditions. Without their help, humans – both men and women – had to do all the heavy labor with the help only of primitive instruments, and they had to transport all burdens on their own body.

Horses became of paramount importance in warfare. Chivalry (*cheval* in French: horse) attained a high status in mediaeval society. This high esteem for horses, horse riding and everything associated with it continued even after the horse had lost its military significance. We see this in terms such as *maréchal* in French (meaning both 'shoeing smith' and 'field-marshal'), *marshal* in English, *maarschalk* in Dutch, derived from an old Germanic word for 'keeper of the horses' but originally meaning 'horse boy'. Similar

titles were *paardenmeester* for 'horse master' in Dutch, and *Rossarzt* or *Pferdarzt* in German.
In the 20th century, the words *veearts* in Dutch (from *vee*: farm animal stock, and *arts*: from the Greek *archos* and *iatros*: leading healer) and the parallel *Vieharzt* in German (pertaining to farm animal medicine), became popular. Once again, this reflected a major change in society and human welfare. Modern animal production methods have drastically reduced the high cost of meat and dairy products. The raising of animals for food production has been developed on an industrial scale.
The overall increase in the standard of living has made it possible for the common man to invest more care and money in companion animals. Attitudes towards animal welfare have changed considerably in recent times and are continuing to change. Concurrent with these developments, the term *dierenarts* (*dier*: animal in general) in Dutch has superseded the more restricted term *veearts*, thus reflecting the increasing importance of companion animal medicine in the last third of the previous century.
In contrast to this, the terms *veterinarian* and *vétérinaire,* which are generally used in English and French, do not differentiate between the species and types of animals involved. This term, derived from the learned Latin *medicus veterinarius*, was not created by the public, but rather was promoted by the early veterinary schools and professional organizations. Its supposedly general meaning was most probably a factor that guided this deliberate choice. Nobody alluded to its primary significance (etymology) involving the care of 'beasts of burden', and it is a pity that almost no one any longer is aware of this. The enormous role that these humble animals once played in the liberation of mankind from slavish labor, and from slavery itself, remains practically unknown. The term 'veterinary' has lost nothing of its forgotten original content. Knowledge about this may help to rehabilitate the humble donkeys, the mules and other beasts of burden who delivered mankind from slave labor … and became our slaves.

Introduction

Today's veterinarians are engaged in a wide range of tasks dealing with a steadily increasing number of different animal species. The subjects of their occupations, the patients, vary greatly. Domestication has considerably changed their characters and their 'looks'. Dog breeding has produced both the Chiwawa and the Great Dane, all derived from the Wolf. Not only breeds, but also species of companion animals, animals accompanying man, or more accurately defined: animals used in this way or to that end by humans, has varied considerably.

In this contribution we take a look into written history, starting about two thousand years ago, in order to detect major trends in veterinary occupations. We will see that the changes in animal species involved in veterinary work reflect changes in human welfare. We witness an evolution from the slavery of many, if not the majority of individual humans and families in ancient 'civilized' populations down to the present day situation in which many, if not most humans living in prosperous countries can afford to pay much attention to their own pet animals, to engage themselves in 'animal rights' movements, and … to pay considerable veterinary fees for the health care of their beloved pets. Facing the complexity of the topic, we have restricted our approach mainly to linguistic facets of the issue. One inherent limitation of the present work lies in the limited number of languages with which the (native Dutch speaking) author is more or less familiar.

Veterinarius: 'pertaining to beasts of burden'

The term *veterinarian* is commonly used in English to designate professionals who take care of animal health. The word originated not as a noun, but rather was derived as an adjective from *medicus veterinarius*. This is easily recognized in the French expression *médecin vétérinaire*, which is still in use along with *vétérinaire.*

Veterinarian is of Latin descent. Country life was highly esteemed by the Romans. Treatises on agricultural topics were popular. It is therefore perhaps not surprising that *veterinarius* was mentioned in the important compilation work of Columella (about 60 AD), 'De re rustica' (On rural matters). It was not intended to indicate, however, individuals entirely or mostly occupied with animal health care, but rather it designated a person who handled draft and pack animals. *Veterinarius* is derived from *veterinus* (veterina, veterinum), veterinorum (veterinarum) pertaining to 'beast of burden'. *Veterinus*, possibly stems from *vehere*, meaning 'to carry' (Littré, 1877). More recent dictionaries link it to *vetus*: old, as in veteran. This may mean: pack animals too old for military service or races (Dictionnaire historique, 1992) or: old and experienced enough to carry or to pull loads, or even more specifically, 'one year old, hence strong enough to carry burdens' (Chambers Dictionary of Etymology, 1988; Klein's Comprehen-

sive Etymological Dictionary, 1971). In any case, the relationship with 'beast of burden' is evident.

Medical connotations soon became apparent in other Latin texts and in inscriptions. The *medicus veterinarius* appeared on the scene, but this term also still meant a person who heals (or tries to heal) draft animals. *Medicus iumentarius* was a synonym: a *iumentum* was a draft animal. Similar terms were *medicus pecuarius* (pecus: cattle) and *medicus equarius* (equus: horse), but in Mediterranean regions undoubtedly the most meaningful term was *mulomedicus*: a medical person who takes care of mules.

The humble mule was indeed the most frequently used 'beast of burden' in the Roman Empire. The story of Hannibal crossing the Alps not only with (a few) elephants but also (and mainly) with mules, to destroy the Romans, is well known. It can be taken for sure that donkeys and especially the mules played an important role in this episode. Large donkey jacks were crossed with good mares to produce strong, though normally infertile mule offspring. With the help of these animals, it was possible to carry heavy loads over long distances and along difficult tracts. Even in those days logistics were of great importance in warfare. This was acknowledged and highly appreciated by the well organized Roman troops, as well as in civil life (Dunlop and Williams, 1996). Not long ago, during the Second World War, mules were very valuable during military operations in mountainous regions, as was the case, for example, in the terrible battle of Monte Cassino in Italy. Mules contributed … and paid a high tribute.

The role of beasts of burden in the evolution of human welfare remains largely unrecognized, and is greatly underestimated at best. In the old days, after the sedentary way of living became the rule, humans, man and woman, had to do all labour by means of their own physical strength, using primitive instruments. They carried all sorts of burdens on their own shoulders, backs or heads, and under their own arms. This was the slave work to which they were condemned after being expelled from Eden, as the Bible tells us. It can be stated that humans were delivered (redeemed, saved in biblical terms) from this primitive state by beasts of burden. The donkey carried the heavy sacs, the ox and the mule drew the plough. With the poor small tenants of our great grandparent's days, it was often their single cow who did this. Those who were somewhat better off could afford an ox,

which was only fattened (if possible) for slaughter when maybe ten year old. Only the rich farmers had a horse, perhaps two or three.

Serfdom was common in Western Europe in the first millennium, and in many ways down through the 19th century. Humans were used, were 'held' as slaves by their rich owners. The owners did this in a way not unlike the way they held their *domestic* animals. Slaves as well as *domestic* animals were part of the *mancipium*, in later times called the *dominium*. Note that *domestics* or *domestic workers* (Latin *domestici*, belonging to the *domus*, the home, the domain, *domestiques* in French) are household servants.

A popular historical theory links the disappearance of the slave system in our regions to the fact that heavy labor, slave work, was increasingly done by domestic animals. The French military historian Lefebvre des Noëttes published in 1931 a curious work on the history of hitches and hitching in which he went as far as to link the gradual disappearance of slavery in Western Europe from the 10th century onwards to the introduction of the padded horse collar, which rapidly replaced the earlier throat-and-girth collar systems. Although this assumption is certainly an oversimplification (Spruytte, 1977; Raepsaet, 1982; Amouretti, 1991), beasts of burden certainly played a role in this process.

Mules were the most important beasts of burden, and in view of this fact, it is no surprise that the first entirely veterinary text book was titled 'Digestorum artis mulomedicinae libri'. These books (libri) containing a digest of the art of mule medicine were written by Vegetius (Publius Flavius Renatus)in the fourth or fifth century AD. The interest in mules of Vegetius, a widely read author on warfare techniques, was inspired by their military function. Translations in German (1532), Italian (1543), French (1563) and English (1748) of this work and of 'De re rustica' by Palladius have greatly influenced authors of other veterinary texts (Dunlop and Williams, 1996; von den Driesch and Peters, 2003).

Vétérinaire (art vétérinaire) was introduced in the 16th century from these Latin sources into French, possibly through Italian mediation (Dictionnaire historique, 1992), and *veterinarian* appeared in an English text for the first time in 1643 (Chambers Dictionary of Etymology, 1988). In Great Britain the term *veterinary* was promoted in the first decades of the 19th century because it sounded learned and it distinguished the new class of scientifically trained diploma holders from farriers and other animal healers

(Woods and Matthews, 2010). These farriers, men of some standing, were usually literate, but they had no notions of Latin. The founding of the first veterinary school in London (1791), headed by a Frenchman, Charles Benoit Vial de St Bel, gave an impetus to this new scientific trend. The first edition of the most important work of Claude Bourgelat, a lawyer by education and founder of the very first veterinary schools (Lyon, 1762 and Alfort, 1765), carried the title 'Elements d'hippiatrique …' (Elements of Horsemanship), while the following editions were titled 'Elemens de l'art vétérinaire …'. The general meaning of the term *veterinary*, supposed to involve all animal species, may have been a factor contributing to this choice. Nobody at that time seemed to have been aware of its etymology, as explained above.

Veterinary s*urgeon* is a popular title in the Anglo-Saxon world. It is derived from the medieval category of *master surgeons*, craftsmen that were reputed for their skills. This word is derived from the Greek *kheirourgia*: 'working by hands' or 'done by hand', from *kheir* 'hand' and *ergon* 'work'. Several (human medical) surgeons attended the courses of the first veterinary schools, graduated and contributed to their success (Gray, 1957). Note that *medicate* has the same etymology as *meditate* …

In conclusion, it can be said that human welfare profited greatly from the introduction of the domestic animal. The truth of this statement turned the other way round is less clear. Most owners and attendants (the *veterinarii*…!) probably tried to give the best possible care to their precious draft animals. Even though medical science was embryonic and many medicines and interventions look barbarous in our eyes, the supposed healing properties of every (im)possible remedy was tried out, including white magic. Usually experienced herdsmen or 'wise' old men, healers and shamans, were charged with this. Many of them most certainly acquired considerable skill in gynecological interventions and surgery, starting with castration.

When the animals grew old and became useless, feelings of compassion appeared. A folk tale about famous emperors who tried to get rid of their old donkey appears in Persian, as well as in Byzantine (the blind emperor Theodosius) and in Western European traditions (the emperors Charlemagne, 8-9th century AD and the Habsburgean Charles Quint, 16th century). The discarded animals managed somehow to attract the attention of their former masters, after which they received a nice 'pension' for the

rest of their days (Lox, 1999). This type of tale is related to the famous Grimm story of the rebellion of 'the Bremen town musicians', four household animals, a donkey, a dog, a cat and a rooster, against their cruel master.

Hippiater, maréchal paardenmeester

Mules belong to the family of *Equidae*, but that was not the reason why the title of the (second) translation into French of Vegetius' work by Saboureux de la Bonnetterie (1783) referring to mules, changed into 'L'art vétérinaire ou l'hippiatrique de Vegetius Renatus'. The horse had become of paramount importance, and this brings us to the Greek *hippiater* (*hippos* – horse, iatrein – to heal: horse healer). This term is older and was much more widely used than its Latin counterpart medicus *equarius*. Compilations of knowledge on animal medicine appeared in which the horse dominated. This can be inferred from the title *Hippiatrica*, the most comprehensive Byzantine compilation (9th or 10th century AD) of Greek texts on animal medicine that had survived until that time.

Horses were held in high esteem among the Greek and Hellenic peoples, as evidenced in numerous and splendid pieces of art. Chariot races were enormously popular. It was however only in the first centuries of the second millennium AD that horses and horse riders came to prominence in Western Europe. This was largely due to changes in warfare techniques. The invention of the bridle and stirrup was instrumental in this development. Stirrups were adopted from Central Asian nomadic peoples by the Byzantines, but it took several centuries before they reached the rest of Europe. They allowed the fierce warriors to carry heavy armor on their great horses and they lent support to their bodies, thus allowing the riders to stay in the saddle in violent battle clashes.

Soldiers moving and fighting on foot were of low stature (not only literally), even though their bows were often deadly weapons. These troops were called 'infantry', a term surviving until the present day. Infantry stems from *infant*, but although these soldiers often may have been very young, they differed from the present day 'child soldiers' of certain African warlords. During the Middle Ages, the word 'child' meant any person unable to earn a living, to survive on his or her own. Fourteenth century

songs in Flemish composed in aristocratic circles during uprisings of the poor against the *ruters* (riders) are full of insults against these uncivilized *kerels*, the rogue peasant chaps (Heeroma, 1966).

Opposed to the humble infantry man was the noble who could afford to take part in battles, highly seated on the back of his horse: the chevalier (cheval: horse, in English to be found in *chivalry*) and *der Ritter* (in German) or *ridder* (derived from *ruter* or *ruiter* in Dutch), meaning rider. Horse riding has an aristocratic military origin in Western Europe. Cavalry was unrivaled in our Middle Ages, and until the days of Cervantes' Don Quixote with his poor horse Rosinante (1605 and 1615), chivalry was often associated with ideals of magnitude, honor and courtly love. The 'noble' horse and horse riding remained in high esteem long after their military role had disappeared.

All this explains why horse medicine and those involved in horse care ranked high. An almost incredible example of this is the word *marshal* (French *maréchal*) and its relationship to veterinary medicine. This noun is composed of *mare* (meaning horses in general) and *scalc*, meaning servant, boy or child (in the mediaeval sense): a person attending horses. It is needless to say that this term had a dazzling career, rising to become the name of the highest military rank in several armies. In the earliest Dutch-Latin dictionary, published by Kilianus (Cornelis Abts van Kiel) in 1599, we find *maerscalck* translated as (1) a person attending horses, especially hoofs, manes and tails: *minister equorum, qui ferreos calceos* (iron shoes) *adfigit* (fixes) *et jubas* (manes) *et caudam* (tail) *comit* (referring to grooming); (2) as a farrier, *faber ferrarius* and (3) as *medicus equinus*, *veterinarius.* Hence, it follows that in that period the difference between a groom, a farrier and a 'veterinarian' was not evident.

Again, the high standing of horses inevitably led to great love and care, sometimes reaching almost absurd proportions as in certain grooming habits and even in 'esthetic surgery'. Veterinary text writers paid almost exclusive attention to horses. Typically they added a dozen pages on diseases of cattle – almost 'for show' – at the end of their books and booklets, followed by one or two pages on pig, sheep and goat diseases, and very rarely a few words on poultry. Dogs, and especially hunting dogs, were better off. Their splendor is illustrated in Jean Froissart's chapters on Gaston Phoebus, Count of Foix (Pau, Southern France), excelling in the

three ‘special delights’ of his life (arms, love and hunting), who composed a famous ‘Livre de Chasse’ (Book of the Hunt, 1387-1388). Similarly treated were the hunting falcons described by the famous and learned Hohenstaufen Emperor Frederick II of Sicily (1194-1260) in his ‘De ars venandi cum avibus’ (On the Art of Hunting with Birds).

All this happened during a period named in German literature the ‘Stallmeisterzeit’, to be translated as ‘Stable Master Era’, somewhat arbitrarily placed in the period from about 1250 to 1762, (von den Driesch and Peters, 2003). The year 1762 is marked by the founding of the first veterinary school in Lyon, and 1250 was taken as a starting point because it is the publication year of the first mediaeval Western European veterinary text book by Jordanus Ruffus (Ruffo or Rosso), *Imperialis marescallus major* in Frederick’s court. This work in Latin, known as *De medicina equorum*, in fact originally had no title, though its opening line stated significantly: *Incipit liber maescalcie, marestalle ...ipatorie.* These texts were often copied, and its prescriptions were introduced into folk medicine. Veterinary ‘art’ borrowed extensively from Ruffus until the 18th century (Dunlop and Williams, 1996).

Similar texts carrying analogous titles appeared until 1568, when *Quod veterinaria medicina* … was printed in Venice. This innovative work, authored by Giovanni Philippo Ingrassia, philosopher and physician, was the first publication highlighting the term *veterinary medicine*. To be followed by many others… However, until the end of the 18th century the term *maréchal* held strong, especially in France. Major publications on horse science and medicine included ‘Le Parfait Maréchal’ (1664, Jacques de Solleysel) and ‘Le nouveau parfait Maréchal’ (1741, François de Garsault).

The fame of the *maréchal* continued even after veterinary education on a regular school basis started (1762). In France, a system was introduced in the early 19th century to allow individuals lacking academic education to work as ‘veterinarians’ after succeeding in examinations involving minimal theoretical knowledge, for which they were awarded the diploma of *maréchal vétérinaire*. In other countries, similar systems were introduced. Again, the association of veterinary medicine with horses was evident. In fact, many of those performing horse medicine were farriers or farrier’s sons (farrier in French is *maréchal ferrant* and ‘fer’ means ‘iron’).

This 'second rate' diploma was soon abolished in France (Leclainche, 1936), but in other countries it persisted much longer. In Belgium, after the first graduates had returned from the newly founded veterinary schools in France at about Napoleon's time the *maréchal vétérinaire* was allowed to continue his profession. This did not change later on when veterinary education was organized by the local governments, first in the Netherlands (Utrecht, 1821) and from 1836 on at Cureghem near Brussels in the newly founded Belgian state, The situation was regulated in 1852, when those participating and succeeding in examinations were allowed to continue their profession, but later on no new diplomas of that kind were awarded (Mammerickx, 1997).

In Dutch, these men, as well as the veterinarians graduated from veterinary schools, were called *paardenmeester* (horse master). This is still the case with veterinarians in rural areas, although most of them do not dare to take care of ailing horses. The designation goes back to at least the 17th century and is related to the notion of 'master' in medieval craft and artisan organization. *Meester* and *master* are contractions of *magister,* derived from *magis* (more, greater). Records of 17th century sorcery trials relate details of certain individuals acting as *paardenmeester* (Momballyu, 2003). These 'horse masters' are not to be confounded with the high ranking titles once existing in several countries signifying *Magister equitum*. Such is the Master of the Horse in the United Kingdom**,** once an important official of the royal household, the third dignitary of the court. Nor are they to be mistaken with the ill-famed *poester* (Dutch-Flemish), *Pfuscher* (German) or *maquignon* (French), especially skilled in 'preparing' ailing horses to be sold, hiding weak spots and worse.

In German a similar name, *Pferdarzt,* was used; the term *Ross-Arzt* apparently had a lower standing in the 19th century, indicating men exercising the profession without any diploma.

Farm animal practitioner, vétérinaire de campagne, Vieharzt (German), veearts (Dutch)

Hardly a century ago, meat, dairy products and eggs were not cheap, and before that time these animal products were expensive. This is illustrated in an astonishing way in plain English. The farmers preserved the use of the

Anglo-Saxon (Germanic language) words for their animals, but names of different types of meat are derived from French, the (adopted) language of their Norman invaders and new masters, led by William the Conqueror (Battle of Hastings, 1066). Farmers raise animals and gentry eat meat:

– *Beef* (French: boeuf) meat from *cows, ox's* and *steers*
– *Veal* (French: veau) meat from *calves*
– *Mutton* (French: mouton) from *sheep*
– *Porc* (French: same) from *swine, pigs*

Dairy products were also very expensive.

One Flemish song deals with four little weavers (*vier weverkins*) who went to the butter market, but ended up buying only one pound for the four of them, because they did not have enough money to get four separate portions. The most prestigious open area in the center of Ghent, facing the magnificent town hall, has been called the Botermarkt (Butter Market) since the 17th century..

The (in our eyes) incredibly high cost of quality food was indisputably due to low productivity and poor agricultural and animal rearing practices in which innumerable endemic (enzootic) and epidemic (epizootic) diseases played a large role. The great agricultural revolution starting in the USA from the last third of the 19th century onward changed this situation. In the Dutch language this revolution was preceded by the introduction of the term *veearts* (*vee*: farm animal stock; arts: from the Greek *archiatros,* a title in use at the Byzantine and Carolingian imperial courts composed from *archos* and *iatrein*: 'healer-in-chief'). However, this term could not rival with the *paardenmeester* (*horse master, Pferdarzt*) until well into the second half of the previous century. Farmers asked for the 'horse master' to deliver their cow. Many veterinarians remained reluctant to take care of sick bovines, or else they considered this as a job of secondary importance. In any case, *veearts* became the Dutch counterpart of veterinarian. The veterinary faculties of Utrecht and Ghent started as *veeartsenijschool*. The term *veeartsenijkunde* (veterinary science) appeared in print for the first time in 1798-1804 (Woordenboek der Nederlandsche Taal, 1954), coinciding with the introduction of the veterinary school system.

In German the analogous term *Vieharzt* (*Vieh* and *Arzt*) was apparently not as widespread as in Dutch, at least not in the more or less official language, as evidenced by the fact that the term *Vieharzneyschule* (veterinary school

referring to farm animals) was superseded by the designation *Tierarzneyschule* already in the late 18th century founding period (von den Driesch and Peters, 2003). It is possible that the latter term was preferred because horses were not considered to be *Vieh*, the ordinary farm animal stock, as becomes clear from book titles referring to *Pferd- und Vieharzt*. Also, the low status of the pre-existing *Vieharzt* and the popular use of 'Vieh' as an insult presumably prevented the founders of the new schools from using this term. *Veterinär* was and still is a synonym for *Tierarzt* in German.

Even in earlier times, the health care of farm animals other than horses was reflected in the professional names. Some herdsmen were designated as *koei meester* (cow master) in the 17th century sorcery trials referred to above. But these remained of low standing, and were often feared because of their supposed secret skills. In regions which were to become part of Germany, the *Kue* (cow) *arzt* (Fröhner, 1929) was also known. In England, the *cow leech* (from old-English *læce*: healer, *lǽcnian*: to heal), sometimes perhaps ironically called *cow-doctor*, and the *castrator* played a similar role (Lane, 1991; Hill Curth, 2002).

A rather strange sounding professional name of veterinarians in some Flemish and Walloon villages was *artist(e).* Most probably, this was borrowed from the designation given to the first graduates of the newly created veterinary schools in France. They received a 'brévet de privilégié du Roi en l'*art* vétérinaire'. Artist is derived from artis (Latin Genitivum of *ars*), in this case *artis veterinariae,* meaning 'of or in veterinary art': a person possessing knowledge of veterinary 'art', (art in the sense of science). After the French revolution and during the first decades of the 19th century, veterinary graduates were officially titled *artiste vétérinaire* in France, Belgium and the Netherlands (Remacle, 1839; Mammerickx, 1967; van der Vliet, 2003), possibly because they refrained (or were prevented?) from using the designation *médecin*. These *artistes vétérinaires* were accredited by the governments. Only later on in the same century did the *artistes véterinaires* change into *médecins vétérinaires*. Both titles separated them from farriers and other 'laymen' exercising the profession. Some may regret that these 'artists' disappeared from the vernacular long ago, although in a few villages the 'title' survived until somewhere halfway into the 20th century.

Another popular professional title in Flanders in the same time period was *expert*. This term most likely originates from the duties of graduated veterinarians as official experts in legal cases, often concerning so-called redhibitory vices and defects (actio redhibitoria: hidden impediments annulling sale transactions). In a similar way, the state government conferred upon them official tasks in the prevention and eradication of plagues. Responsibilities of this type were denied to the farriers and uneducated healers, considered to be more noxious to animal health than the infectious diseases themselves (Leclainche, 1936). The *maréchal-expert*, a title indicating a higher grade of farriers, created by the Napoleonic administration to cope with the shortage of veterinary professionals (Leclainche, 1936) is a less likely source. In certain rural areas, *expert* became synonymous with veterinarian and the name is still in use as such.

Successful officially organized eradication campaigns of rinderpest (18th century), bovine pleuropneumonia (19th century), tuberculosis and brucellosis (20th century), each of them mainly affecting cattle, increased both the need for fully qualified veterinary graduates and the esteem in which they were held. Moreover, the later introduction of therapeutics with scientifically proven effects (antibiotics, antiparasitic products, etc.) had an enormous impact and further contributed to the success of veterinarians and veterinary science. Today's vets dispose of a wide array of products with proven effectiveness. Cattle, and in some countries or regions also sheep, have received great attention. Swine and poultry had to await the advent of production on industrial scale before veterinarians and, most importantly, veterinary science would deal adequately with them. Effective disease control programs, including the administration of vaccines, have made this evolution possible. The massive accumulation of, most often, young animals, highly susceptible to infectious diseases, as is the case in modern production units, is not possible without the help of such programs. Note that the term vaccine is derived from *vacca* (Latin: cow). 'Blossom', the legendary cow of Jenner's milkmaid, is at the origin of the prevention, the control and finally the disappearance of the dreadful smallpox disease in humans, one of the greatest achievements of medical science and organization.

This evolution towards the massification and industrialization of farm animal keeping, part of what is sometimes called the second agricultural

revolution, started not much more than half a century ago. Changes in the attitudes of humans to animals have coincided with and are more or less related to this very recent evolution in farm animal rearing. Is industrial farm animal keeping acceptable from a (human) moral point of view? Moral philosophers explore the notion 'sentient being' introduced by Jeremy Bentham (1748-1832), and (human) psychologists endeavor to provide insight into human-animal relationships (Herzog, 2010). This brings us to our final chapter.

Companion animal practitioner, Tierarzt, dierenarts

Until about the last quarter of the previous century, veterinary medicine remained almost synonymous with farm animal practice. But, especially from the 1970's onward, companion animal medicine rapidly gained importance. Horse medicine revived, as part of this trend, and *equine practitioners* have become highly specialized professionals taking care exclusively of horses kept for riding, for racing, as companions of owners and their children, or even almost as 'ornaments'. The same thing has happened with the small animal vets, most of whom take care of dogs and cats. Several types of specialization, usually according to animal species, emerged, and they have continued to show rapid development. Appropriate names indicating subdivision, specialisms and specialists have appeared spontaneously: *avian medicine, poultry veterinarian, Kleintierarzt*, etc …

The numbers of small animal practitioners have been increasing steadily, and this trend is running parallel with the 'feminization' of the veterinary profession. Again, this change is mirrored in the names given to those exercising the profession. *Dierenarts* (animal vet) has replaced *veearts* (farm animal veterinarian) in Dutch. Although *dierenarts* is older and was promoted during the first half of the 20th century by veterinary organizations and university faculties, it was only after the 'explosion' of companion animal medicine that it became the standard designation of vets in the Netherlands and in Flanders (Belgium). In Germany, as indicated above, this substitution happened much earlier.

It may seem a paradox, but the inhumane massive production of cheap meat, eggs and dairy products, also necessitating large-scale monocultures of soy and other feed ingredients, contributes to our affluent way of life in

which we can afford to spend great love (and much money) on our own cherished animal companions. The *veearts*, the *farm animal vet*, has helped to provide means of life to the *dierenarts*, the *companion animal vet*.

Looking back at the end of this story to the starting point, the 'beasts of burden' serving mankind, which lent their name to the *veterinarius*, one can ask whether or not our present day companion animals are really different from the mules of the *mulomedicus*. In the very essence of their existence, they are not. Their lives are entirely in the hands of their masters, whether or not these are 'animal rights' activists, lords of agricultural industry, brutes, or just common people loving their housemates and taking good care of them. The term 'veterinary' has lost nothing of its forgotten original content. Knowledge about this may help to rehabilitate the humble donkeys, the mules and other beasts of burden who delivered mankind from slave labor … and became our slaves.

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Laudatio prof. dr. Raphael Maurits Ernest Suy-Verburg

Frank Vermassen

It's an honor for me to introduce prof.dr. R. Suy as holder of the Sarton medal for the academic year 2011-2012 at the Faculty of Medicine and Health Sciences.

Prof.dr. Suy can be considered as one of the founding fathers of vascular surgery, not only in Belgium but also in Europe and even worldwide. Although he spent most of his career at the University of Louvain, he always remained to have strong ties with the city of Ghent where he was born in 1934. He was the third child in a row of 5 and having a psychiatrist and a musician as parents, at first sight, this did not seem the ideal breeding ground for a later career as a surgeon. The ground proved to be fruitful though as two of his brothers also became famous as an international diplomat and a well known psychiatrist. With them he spent most of his youth at the country side, a period he has good remembrance of, even during the war, and still loves to remind to. After the second world war, he started secondary school, which ended after visiting 3 different schools, according to some because his father always looked for the best one, according to others because he was not always the most exemplary student.

He started his studies in medicine at the Ghent University in 1951 but moved to Louvain after 2 years because of health reasons. His main interest during his medical studies were in anatomy, histology and embryology. He always succeeded with great distinction.

During his last year as co-assistant, he became tempted by the profession of surgery.

He started his training in surgery in 1960 in Heerlen, the Netherlands, a period he always likes to refer to and where the basics for his great career was laid.

He married in 1961 with Nadia Hauman and got 2 children, of which one unfortunately died at the age of 17 in a car accident, an event that took him profoundly.

In 1968 he was appointed head of clinic at the department of cardiovascular and thoracic surgery in Louvain (chair prof. Stalpaert) with the specific task to start and build out vascular surgical interventions. He followed a specific training in that field with dr. Volmar in Heidelberg.

As these were also the early days of coronary surgery, he was sent to the USA in 1972 to work with dr. Dudley Johnson (Milwaukee, Wisconsin). Afterwards, he started the coronary program in Louvain. In total he did more than 4000 coronary bypass operations.

In 1990 ther department in the University Hospital In Leuven was split and he became chief of the department of vascular surgery. His specific interests were in carotid and aortic surgery. He became a well known expert and reference person in these fields. At scientific meetings his opinion was always regarded with much respect and the witness of his great experience.

He trained more than 30 surgeons in coronary and vascular surgery, so that his impact on surgical practice even today cannot be overrated. All remind him as a “big” surgeon, not only literally, and in formal and informal meetings among vascular surgeons, still today, the name “Suy” often appears to quote some of his famous statements or tell one of the many anecdotes that happened during his career.

Prof. Suy was member of several scientific societies and one of the founding members of the Belgian Society of Vascular Surgery of which he also became a president. He was also the first foreign president of the Société de Chirurgie Vasculaire de langue Française for which he organised a well attended congress in Brugge. He also organised an annual international symposium in Louvain.

His scientific work comprises more than 130 international papers, many of which can be considered as landmarks, especially in the field of monitoring during carotid surgery and treatment of obstructive as well as aneurysmal aortic diseases.

His second passion was the history of medicine. Already during his youth he collected stamps on that subject and when he became professor emeritus in 1999, he finally had the time to perform research in that subject. He wrote several articles on the history of cardiovascular surgery in Belgium and a book “A history of arterial aneurysm” for which he did a lot of research and which can now be considered a very well illustrated reference work on that topic, giving an overview of the treatment of aortic aneurysms from ancient time till today. His subject of today lies on the crossroad of two of his passions in addition to surgery: history of medicine and embryology.

The quest for the elusive human urinary membrane

R. Suy

Historia peculiari digna est haec tunica, tum quod de ejus existentia hactenus controversum est, tum quod in variis animalibus insigniter varietur.[1]

Gualtore Needham 1667

A. The anatomy of the fetal membranes as now accepted

All vertebrates, except fish and amphibians, form reduplicated embryonic membranes around the embryo in a very early stage of the embryogenesis[2]: an innermost called *amnion* from the Greek word αμνιον for 'lamb', an outermost called *chorion* from the Greek word χώρειν for 'enclose', and a middle called *allantois* because, in ruminants like the goat, the primary experimental animal in ancient times, it has the form of a sausage or ἄλλαs in Greek. This middle membrane plays an important role in the formation of the placenta, and will also become the receptacle of the foetal urine.

The presence or absence of an allantois in the human conceptus was disputed by anatomists up to the second half of the 18th century. Finally, at the beginning of the 19th century, it was agreed that the human foetus has no urinary membrane. There is even no space for it between the amnion and the chorion. **(Fig. 1)** The reason was demonstrated a century later. This

[1] This membrane deserves its own story, since its existence is still controversial and since it varies according to species. (Needham W. *Disquisitio anatomica de formato foetu*. London: R. Needham. 1667, p. 65).

[2] Embryo is the primitive form of life (in human up to the eight week).

article on the search for the 'elusive human allantois'as it was termed by A.W. Meyer,[3] tells the story of anatomy before embryology was promoted to being a fully fledged speciality in the 19th century. For further clarification, I shall provide a brief exposition of the morphology and function of the embryonic membranes, the yolk sac or umbilical vesicle, the umbilical cord and the placenta, which together are called the embryonic appendages.

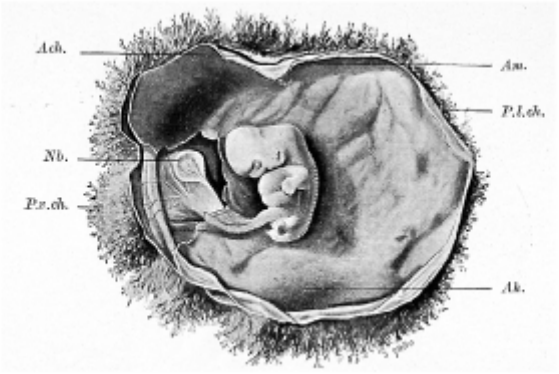

Fig. 1: A six week old human conceptus. The amnion which is partially removed to show the umbilical vesicle (from Grosser, 1909, fig. 81, p. 86)

The embryonic appendages[4]

The conceptus is formed by ectodermal, mesodermal and entodermal layers.[5] The amnion is derived from the ectodermal germ layer and surrounds the entire embryo. The chorion, which is derived from trophobastic cells (i.e. the external layer of the morula) covered by a mesodermal layer, spreads over the entire germ, and later becomes a part of the placenta

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3 Meyer AW. The elusive human allantois in the older literature. In: *Science, Medicine and History. Essais written in honour of Charles Singer*. London: Oxford University Press. 1953; Vol I, 511-520.

4 I thank Prof. em. Dr. Leo De Ridder from the University of Ghent and Prof. Dr. vet. A. Weyns from the University of Antwerp for their support.

5 In human, after about three days, the fertilised cell (zygote) forms a mass of cells (morula) which changes into a hollow blastocyst, consisting of an outer layer (the trophoblast), and an inner cell mass (the embryoblast). The ectoderm, mesoderm and endoderm are the three primordial germ layers within the embryo.The ectodermal cells differentiate into skin cells and neurons. The mesoderm cells differentiate into muscle cells, tubular kidney cells, red blood cells etc.. The endodermal cells differentiate into lungs and digestive system. Endodermal cells form the epithelial lining of the whole digestive tract, except for part of the mouth and pharynx, and the terminal part of the rectum, which are lined by ectodermal cells.

in mammals. The pouches of the chorionic part of the placenta penetrate the endometrium in eutherians. Between these two membranes (chorion and mesodermal layer) there are two extra-embryonic extensions; a proximal one originating from the midpart of the primitive gut, and a distal one originating from the primitive hindgut. The distal extension will form the allantois, the proximal will form the yolk sac which will ultimately shrink to form the umbilical vesicle.[6] **(Fig. 2)**

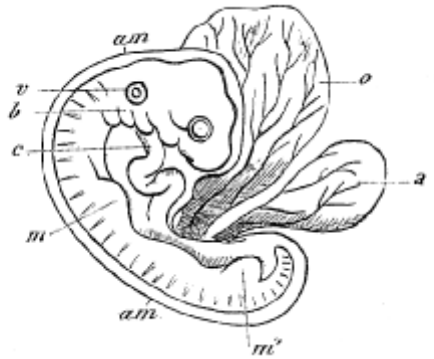

Fig. 2: A four weeks old human embryo within its amnion (am), with on its ventral side two extra-embryonic extensions: one to the umbilical vesicle (o) and one to the allantois(a)(From Debierre, 1890, fig. 481, p. 933)

The middle membrane or allantois as urinary membrane

The open canal from the fetal urinary bladder, accompanied by side branches of the fetal hypogastric arteries, runs into the umbilical cord as a duct. This open canal, termed 'urachus' (between the fetal bladder and the umbilicus) and 'allantoic duct' (within the umbilical cord), is the easiest exit for the fetal urine which is collected within the saccular allantois, also called the "urinary membrane", located between the amnion and chorion. In ruminants, the sausage-shaped urinary membrane is situated on the ventral side of the foetus. In most viviparous animals and in oviparous animals, the allantoic duct grows to form a sac that surrounds the amnion like an envelope. The inner blade of this envelope adheres to the amnion to form the

[6] Debierre Ch. Développement des annexes embryonniares ou foetales. In:*Traité éléméntaire d'anatomie de l'homme*. Paris: F. Alcan. 1890; Tome II, pp. 933-954.

allanto-amnion, whereas the peripheral or visceral blade of the envelope adheres to the chorion to form the allantochorion. **(Fig. 3)** Because of these adhesions, one can have the impression, in animals with an envelope-like urinary membrane (allantois), that the fetal urine is contained in the exocoele, which is the space between the chorion and the amnion.

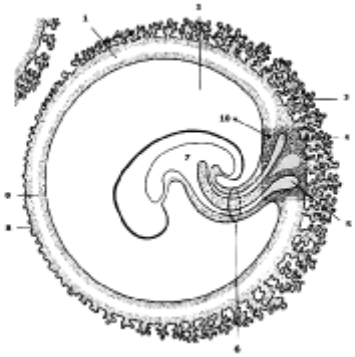

Fig. 3: Schematic drawing of the formation of embryonic appendages in man. (1) chorionic cavity, (2) amniotic cavity,(3) chorionic villi, (4) allantoic duct, (5) umbilical vesicle, (6) umbilical cord, (7) primitive gut, (8) chorion, (9) peripheral blade of the amnion, (10) body stalk. From K. Dierickx, 1969

In anthropoid apes and humans, the allantoic duct does not exceed the contact zone of the body stalk with the chorion.[7] Premature occlusion occurs through apoptosis of the inner endodermal cells.[8] In humans, the allantoic duct is completely blocked in the eighth week of gestation, and the urachus shrivels around the sixth month of gestation into a ligament from the top of the bladder to the umbilicus. The human foetus voids through its urethra into its amniotic fluid.

The middle membrane (allantois) as a base for the fetal part of the placenta

A placenta formed by the chorion and vascularised by the allantoic vessels (an allantochorionic placenta) is the definitive fetal part of the placenta.

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7 Dierickx K. Embryologie van mens en eutheria. Gent: Story-Scientia. 1969; 22.

8 Apoptosis is the term for genetically programmed cell death.

End branches of the allantoic vessels, surrounded by some mesodermal connective tissue, penetrate into the chorionic pouches to form villi for the exchange of gases, nutrients and electrolytes. At the macro level, there are four types of placenta, **(Fig. 4)** namely (1) the diffuse placenta type, seen in horses and pigs, where almost the entire surface of the allantochorion is involved in the formation of the placenta, (2) the cotyledonary type,[9] observed in ruminants, with integration of patches of the allantochorion with the inner side of the womb, (3) the zonar type, seen in carnivores like dogs and cats, where the placenta takes the form of a band of tissue surrounding the chorion, and (4) the single discoid type, seen in humans, anthropoid apes, rodents, rabbits, and bats.[10]

Fig. 4: Four types of placenta. From left to right: the diffuse type, the cotelydonary type, the zonar type, and the discoid type. From De Groef, Fig. 1.11

The umbilical cord is clad with amniotic ectoderm and built up of chorionic mesoderm (the jelly of Wharton) in which run the allantoic and vitelline ducts with two concomittant arteries and two concomittant veins. This cord grows as a solid mass known as the 'body stalk' which fuses with the chorion. In eutherians, the vitelline duct and its vessels occlude and shrivel within a few weeks of gestation. In primates, there is only one wide umbilical vein which splits into two branches in the foetus; one to the inferior caval vein (ductus venosus Arantii), and one to the portal vein.

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9 In this placenta-type, the foetal placentulae have the appearance of the vinegar-cups of the ancients, and hence are called cotyledons or acetubula. It was from observations of sheep and goats that the term 'cotyledons' in foetal anatomy took its origin.

10 De Groef B. Placentotrofie en de dracht bij zoogdieren. In: *De biologie van de seks*. Leuven: Acco, 2009; 29-38.

B. Historical review

1. Antiquity

Hippocrates (460-370 BC)

The ancient anatomists described fetal anatomy on the basis of their findings in animals. In the Hippocratic writings, the author advised studying a chicken's egg each day over the entire brooding period in order to better understand the formation of a human child. He also described an early human abortion as a red, round peeled raw egg with fluid within an inner membrane, and with clots on an outer membrane [the villous chorion?]. He was most surprised by a projection in the middle of the membranes through which the embryo seemed to breathe. The amnion, and the chorion with its sanguineous folds, were accurately described without making mention of a urinary membrane; so we may assume that he analysed a human conceptus.[11]

Aristotle (384-322 BC)

Aristotle emphasised the equal development of all animals with, as the most important difference, the connection of the umbilicus with the shell, or with the womb, or with both, depending on the species. He most accurately described the development of the chick after three, ten and twenty days of incubation – the day on which it discharges residuum [urates of the allantoic fluid?] in the direction of the afterbirth. He gave a fair description of the membranes, but makes no distinction between allantois and chorion. He observed embryos of many mammals, including man, for he said that the human embryo, on the fortieth day of gestation, is as big as a large ant. His report on the umbilical cord with four vessels and an allantoic duct, and on the cotylodenary placenta, is concise and correct. About the membranes, he wrote that, between the uterus and the embryo, there is a chorion and 'other membranes'. He was probably describing the envelope-like urinary membrane in a carnivore (dog or cat), as he said that the animal develops within the innermost envelope, and that another membrane containing fluid and mostly attached to the womb, appears around the previous one. In

[11] The Seed and the Nature of the Child. In: G. Lloyd (ed.) '*Hippocratic writings'*. (Translated into English by JM Lonie), London: Pinguin books,1983; 317-341.

between [these two membranes] is a watery or sanguineous fluid, which the women folk called 'the forewaters'.[12,13]

Galen (129-ca.200)

Almost five hundred years passed before another leading figure appeared in the person of **Galen the Pergamite**. There are also some grains of embryological facts in the works of Rufus (ca. 50-100) and of Soranus (ca. 130-200) of Ephesus. Rufus states that the embryo is surrounded by an amnion and a chorion, and that urine is drained through the urachus towards the chorion, which is exactly as found in animals with an envelope-like urinary membrane such as pigs, horses and carnivores.[14] Soranus describes the [human foetal] urachus in his treatise on gynaecology as the fifth vessel in the umbilical cord to drain the urine towards the chorion, a description based on his findings in animals.[15]

Galen extensively discusses the embryonic membranes in his treatises *de Usu partium* and *de Uteri Dissectione*. His major failing is his confusing habit of tacitly applying to man observations made on animals, either from a carnivore such as a dog with a zonar placenta and an envelope-like urinary membrane, or from a ruminant such as a goat with a cotylodenary placenta and an sausage-like (allantoic) urinary membrane. He is the first to report the manoeuvre to compress the fetal urinary bladder to push the urine into the allantois. He also states that voiding urine is a voluntary act of which a foetus is not capable.[16] His descriptions of the fetal membranes, and particularly of the chorion, are not consistent: in his treatise *de Uteri Dissectione,*[17] he implies that the chorion is not a membrane but a mesh, composed of many closely interwoven arteries and veins adhering to the uterus through the connection of the uterine and chorionic vessels, which

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12 Aristotle. 'Historia Animalium' in: Smith JA, Ross WD (eds.),Vol IV of *The Works of Aristotle* (translated into English and annotated by d'Arcy Wentworth Thompson),1962; 562 & 586[a,b].

13 Aristotle. 'De Generatione Animalium' in: Smith JA, Ross WD (eds.), Vol. V of *The Works of Aristotle* (translated into English and annotated by Oglie W.), 1949; 745[b]-746[a].

14 Daremberg Ch, Ruelle Ch.E. '*Oeuvres de Rufus d'Ephèse'*. Paris: Imprimerie nationale, 1879;.166-167.

15 Soranus, '*Gynecology*' Translated into English by Owsei Temkin. Baltimore: John Hopkins Press, 1991; Book I, 58-61.

16 Galen. '*De usu partium*' Translated by Margareth Tallmadge May as 'On the Usefulness of the Parts of the Body'. Ithaca (N.Y.): Cornell University Press, 1968; 655-669;

17 Galen*, De Uteri Dissectione*. In: Opera Omnia, Translated by C.G. Kühn. Leipzig: C. Cobloch, 1821; p. 902

is inconsistent with the description in his treatise *de Usu partium*[18] in which he describes the chorion as a double membrane lining the entire inside of the uterus.

2. Medieval times

Galen's views misled anatomists, including Vesalius, for many centuries, partly because, until the 16th century, nobody dared to ask whether or not he might have been wrong. In the 12th century, the medical knowledge of embryology, rooted in the Hippocratic writings and in the works of Aristotle and Galen, reached the Latin West via Arabic translations, commentaries and compendia. Most prominent amongst the latter were Avicenna's *Canon* and *De Animalibus.* In the 13th century, **Albert the Great** and **Giles of Rome** (Aegidius Romanus) investigated generation and embryonic morphology.[19,20] Their works contain no new points of view concerning the fetal appendages, but did herald the beginning of the period of developing humanism, naturalism and scientific renaissance which saw Leonardo da Vinci as the universal man.

3. The Anatomical Renaissance

At the Universities of Padua and Bologna, from the beginning of the 16th century, all of the anatomists were interested in human embryogenesis, but had to rely primarily on the ancient anatomists, since a short supply of appropriate specimens remained the most serious problem, making observations on the human foetus extremely rare. **Berengario da Carpi** (1465-1530, anatomist at the University of Bologna) remarked on how difficult it was to get to see human abortuses, and tells how he bribed midwives to show him what he wanted to see.[21]

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18 Galen, *De usu partium* (translation by May), 1968; p. 665.

19 Stadler H. *Albert the Great. De animalibus libri XXVI nach Kölner Urschrift*. Munchen: Aachersdorf, 1916; Book IX, pp. 725-726.

20 Hewson MA. '*Giles of Rome and the medieval conception. A study of the 'de formatione corporis humani in utero*', London: Athlone Press, 1975; p. 154.

21 Adelmann HB. "*Marcello Malpighi and the evolution of embryology'*. Ithaca (N.Y.): Cornell University Press. 1966; Vol II, p. 753.

Fortunately, there was still the chicken's egg. The Hippocratic advice to observe and record systematically the daily development of the chicken's egg, was followed for the first time by **Ulysses Aldrovandus** (1522-1603) and his brilliant student, the Frisian **Volcher Coiter** (ca. 1534-1567?), anatomists and zoologists at the University of Bologna in the latter half of the sixteenth century. Coiter's descriptions of the development of the chicken, published in 1572, are far superior to the meagre statements of Aristotle, but were still unhelpful towards a better understanding of the morphology of the human fetal membranes.[22]

One hundred years later, in 1672, daily observations by microscope on the development of the chick were reported by **Marcello Malpighi** (1628-1689), professor at the University of Bologna and Pisa. After three days had passed, he found, near the point of emergence of the umbilical vessels [the omphalomesenteric trunks], a 'vesicle well supplied with blood vessels' which he thought to be the fleshy stomach [the gizzard], but which is in fact the rudiment of the distal intestinal loop [allantoic duct]. At a later stage of incubation, he referred to what is now known as the allantois, as 'surrounding the thin albumen'. **(Fig. 5)**

Fig. 5: Drawing of a microscopic view of a chick at the tenth day. (A) The amnion containing the embryo, (G) the "albumen" or allantois, (H)the area vasculosa(yolk sac) and vitteline vessels. From Malpighi: De Formatio Ovi et Pulli 1672, Plate III, Fig. XIX

[22] Coiter V.'*De ovorum gallinaceorum generationis primo exordio progressuque, et pulli gallinacei creationis ordine'*. Nüremberg, 1572. Translated into English and edited with notes and introduction by Howard B. Adelmann, in Annals of Medical History. 1933; **5**: 327-341 and 444-457.

Malpighi remarked that, 'being unable to detect the first origins [of the organs], one was forced to await the manifestations of the parts as they successively came into view.'[23] Unfortunately, as Sarton said, 'most of Malpighi's investigations were unprofitable and certainly premature and soon thrown into the backbround by unprofitable discussions.'[24] The summit of these unprofitable discussions was the endless and pointless Haller-Wolff debate on preformation and epigenesis,[25] in the second half of the eighteenth century.[26]

Meanwhile, human foetuses and even a few pregnant women had been anatomised, mostly by anatomists of the universities of Padua and Bologna. The first beautiful drawings of the human foetus in utero are from the hand of **Leonardo Da Vinci,** (1452-1519) and were probably intended for an anatomical atlas, never published, of Mercantonio della Torre (1481-1511), an anatomist in Padua. Leonardo applied embryonic observations made on ruminants to man, with an illustration of a human foetus of about four months in an opened cotyledonary uterus. He wrote that the foetus is surrounded by an *animus* (amnion), by an outer membrane which he called *Secundina* or chorion, and by a third membrane or *allanchoidea* which is hidden between both legs, but described as passing between the hands and knees of the child as it lies curled up.[27]

The already mentioned **Berengario** was the first anatomist to report on the human foetus and its appendages. He reported his attempts to test Galen's assertion that by pressing on the bladder in human foetuses, urine can be forced into an allantois by way of a urachus, and erroneously concluded that a connection between the bladder and the allantois exists in the human

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23 Malpighi M. 'De Formatione Pulli in Ovo, and De Ovo Incubato' Letters to the Great Royal Society of England. 1672. Translated into English by H.B.Adelmann. In: *Marcello Malpighi and the Evolution of Embryology* Ithaca, New York: Cornell University Press, 1966; Vol II, 934-1013.

24 Sarton G. 'The Discovery of the Mammalian Egg and the Foundation of Modern Embryology'. *Isis* 1931; **16**: 315-316.

25 The preformationists believe that the embryo pre-exists in some form in either the maternal egg or the male spermatozoon. Most also thought that all embryos had been formed by God at the creation and were encased within one another to await their future appointed time of development. Epigenesis, on the other hand, argued that each embryo is newly produced through gradual development from unorganised material. (Roe, 1981,p. 1.)

26 Roe SA. 'Matter, Life and Generation. Eighteenth-century embryology and the Haller-Wolff debate' Cambridge: Cambridge University Press, 1981; 52-53.

27 McMurrich JP. '*Leonardo da Vinci, the anatomist (1452-1519)*' Baltimore: Williams & Wilkins, 1930; 68-69 & 230-232.

foetus, as he had based his conclusions on the presence of such a connection observed in canine foetuses.[28]

Andreas Vesalius (Andries van Wesel, 1514-1564) is generally recognised as the founder of modern anatomy with his *De Humani Corporis Fabrica*, first issued in 1543. It would have been better if he had not written the chapter on fetal human membranes, for he misled the reader with his description of canine embryonic appendages showing a human foetus hidden within the amnion![29] **(Fig. 6)** Vesalius called the outermost wrapping *secundina* instead of chorion as in Galen's writings. This *secundina* is described as an annular mesh into which the vessels of the uterus pass and are taken up, and whose outer surface is contiguous with and attached to the inner surface of the uterus. This outermost wrapping is in fact a zonar canine placenta. According to Vesalius, the middle wrapping, which he called allantois*,* collects the urine all over its surface through a channel from the top of the fetal bladder. The allantois, which is obviously the chorion, is described as an avascular membrane whose outer surface, except for the part that is attached to the outermost wrapping, touches the uterus, but is not attached to it.

Fig. 6: A Human foetus in canine embryonic membranes. The zonar placenta (fig. 30, 1) is described as the "outermost wrapping". From Vesalius 1543 fig 30. (1 to 4) of Book V, p. 382

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28 Berengario da Carpi. Commentaria cum amplissimis additionibus super anatomia Mundini. Bologna: H. de Benedictis. 1521; ccxlvii-viii & cclx

29 Vesalius A. De involucris foetum in utero. In*:'De Humani Corporis Fabrica Libri septem*' Basel: J. Oporinus, 1543; Book V, chapter XVII, 540-542. Translated into English by WF Richardson and JB Carman as '*On the Fabric of the Human Body'*. Novato (California): Norman Publishing, 2007; Tome IV, 195-201.

Referring to Galen's writings on the allantois, Vesalius stated that

> 'Galen writes that the second wrapping of the foetus contains urine, and says that it does not surround the whole foetus but only the buttocks and extremities. On this point I request your judgement, and I also beg that, when you are dissecting a human foetus (or failing that, at least a canine), that as soon as you have withdrawn the wrapping that I have described as the outermost wrapping of the foetus from the womb, and are holding the foetus and its other wrappings in your hand, you should set the foetus upright, and observe how the urine trickles towards the region of the feet, and collects there; then turn it head downwards, and notice that the urine is contained in the wrapping that I have just described as membranous, and as standing next to the uterus (except where the outer wrapping intervenes), and that this wrapping is not a chorion since it has no veins or arteries, apart from those which are supplied to it, as to other membranes for the purpose of nutrition, and that these vessels do not come directly from the uterus, but from the ones that weave into the thick and spleen-like wrapping of the foetus … If you examine these points carefully, and use your dissection to compare my teachings with Galen's, you will be able to judge very rapidly to what extent the rest of Galen's teaching in *On the Formation of the Foetuses* and *on Semen* is consistent with nature.'

The Fabrica was immediately plagiarised, by, amongst others, Thomas Geminus (Thomas Lambert (?) ca. 1510-1562, of Lixhe near Visé, Belgium) who made unauthorised copies of the plates on the female reproductive organs, and on the fetal membranes for the second edition of *the Birth of Mankind* (1545), the English translation of *Der Swangern Frauwen und Hebammen Rosegarten* (1413) from Eucharius Rösslin. *Rosegarten* was translated into Latin, and into all main European languages. In the English version, reprinted until 1654, the human fetal membranes were discussed according to Vesalius, including the errors in his writings.[30]

In the meantime, Vesalius himself, admittedly without naming him, was accused of plagiarism by **Carolus Stephanus** (Charles Estienne, 1504-1564), who, in the foreword of his *De dissectione partium corporis*

[30] Hobby E. 'Illustrations in the birth of mankind' Introduction to '*The Birth of Mankind: Otherwise Named The Woman's Book*' Farnham: Ashgate.2009; xxvii-xxx.

humani, issued in 1545, insinuated that his work had been plagiarised.[31] Estienne, like Vesalius, was a student of Jacobus Sylvius, (Jacques Dubois, 1478-1555, a convinced Galenist and anatomist at the University of Paris). Vesalius had most likely seen Estienne's drawings during his stay in Paris from 1533 to 1536. However, it is hardly likely that Vesalius had taken any of Estienne's illustrations, which have, according to Saunders and O'Malley, the distinction of being without doubt the most hideous ever published.[32] Estienne used drawings from contemporary artists, removing small sections of their illustrations of superficial tissues to replace them by his own anatomical insets of the corresponding parts. Estienne's presentation of the female reproductive organs and open gravid uterus with a curtain-like allantois, **(Fig. 7)** is inspired by the myth of Venus and Cupid as drawn by Perino del Vaga and engraved by Jacob Caraglio in 1527.

Fig. 7: The goddess Venus anatomized to demonstrate the allantois in the pregnant uterus. From Estienne; 1545

In 1546, Vesalius wrote a defence against the unjustifiable plagiarism of his illustrations in the *Birth of Mankind*, and against the sharp comments of Sylvius. Concerning the fetal membranes, he admitted that, at the dissection of a heavily pregnant woman, he was in such a hurry [could it have

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31 .Estienne Ch. *'De Dissectione Partium Corporis Humani'* Paris:Simon de Colines, 1545. Introduction.

32 Saunders JB de CM, O' Malley ChD. 'The illustrations from the works of Andreas Vesalius of Brussels'. New York: Dover publications, 1973; 24.

been a snatched body?] that he was not aware of the difference between the embryonic appendages of a canine foetus and those of a human foetus. He added that, in any case, it is clear enough that Galen's description of the outer membrane deviated very far from the truth, and therefore could be considered to be anything but perfect.[33]

In the second edition of the *Fabrica* (1555), Vesalius, in the chapter on the fetal membranes, introduced important changes. The canine zonary placenta is replaced by a human discoid placenta, and there are but two membranes, the chorion [which is still called allantois] and the amnion. (**Fig. 8**) He stated that the fetal urine is contained between the amnion and the chorion which he called allantois, yet he goes on to say that the urine is not always so contained, but that the urachus sometimes ends in a large, membranous sac as shown in an additional illustration.[34] **(Fig. 9)**

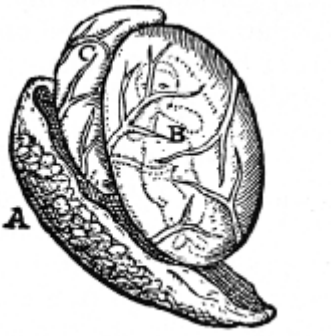

Fig. 8 (L): The human foetus and its membranes in the 1555 edition of the Fabrica

Fig. 9 (R): Human embryonic membranes in the 1555 edition of the Fabrica
(A) The Chorion, (B) amnios, (C) Allantois

Vesalius' successors in Padua were not happy with this. **Realdus Columbus** (Realdo Colombo ca. 1516-1564) primarily criticised Vesalius' presentation of the canine fetal appendages. He retained three fetal membranes whereby he described the placenta as the 'outer membrane',

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33 Vesalius A. 'Epistola rationem modumque propinandi radicis Chynae decocti pertractans'. Basel: J. Oporinus, 1546; 207-208. Translated into Dutch by H. Pinkhof in '*Opuscula selecta Neerlandica de Arte medica*' Amsterdam: F. van Rossen, 1915; **3**, 137.

34 Vesalius A. 'De involucris foetus in utero' In: '*De Humani Corporis Fabrica Libri septem'*. Basel: J. Oporinus, 1555; Book V; Chaper XVII, 671-676.

and the chorion as the 'membranous membrane'[allantois].[35] **Gabrielis Falloppius** (Gabriele Falloppio, 1523-1562, professor in Padua after Colombo's departure), wrote to his master and friend Vesalius that he was disturbed, because his friend always wanted to correct Galen, appropriately or not, and he added that human fetal urine was not drained into an allantois, but into the chorion.[36] Vesalius responded in detail to Fallopius' letter including the following about the human allantois he had depicted:

> '(furthermore) because I had observed a third membrane in cows that was situated between two covering membranes [the amnion and the chorion] that does not enclose the calf (which is why it may not be called an enclosure of the foetus) and that like a bladder, takes up urine, and because Sylvius (for whom, moreover, I have always had much respect) in his writings (in which he contended, without reason, that Galen had anatomised women, and not goats and calves) swears by all the saints that he encountered this piece of meat in women, I felt compelled (or better I wanted to), because I had not had the opportunity to anatomise a pregnant woman, to add a digression in the second printing (by Oporinus) of my book *De Humani Corporis Fabrica,* in which I said that the membrane, in which the urine gathers separately, sometimes appears in the dissection, and is compared with a sausage and named analogously [allantois], and that the external enclosure is called the chorion, whilst the innermost always retains its own name [amnion]'.[37]

From that time forward, most anatomists will report on the chorion as a urinary receptacle, except for **Adrianus Spigelius** Bruxellensis (Adriaan van den Spiegel from Brussels, 1578-1625), who, in his *de Formato Foetu*, posthumously published in 1626, referred to the 1555 edition of his fellow countryman Vesalius.[38] It is a strange fact that in the accompanying illustrations of pregnant wombs, no allantois is pictured between the human fetal membranes. This is because all of the plates were intended for the anatomical atlas of his deceased predecessor Julius Casserius (Julio Casserio, 1551-

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35 Columbus R. De re anatomica Libri XV. Venetië: N. Bevilacqua, 1559; 248.

36 Falloppius G. 'De iis quae ad foetum pertinent' In: *Observationes anatomicae in libros quinque digestae*. Venetië: M.A. Ulmus, 1562; chapter XIIII, 176-184.

37 Vesalius A. Anatomicarum Gabrielis Falloppii Observationum Examen. Venetië: de Fransiscis; 1564, 823. Translated into Dutch by L. Blanckaert et al. Brussel: Kon. Vl. Ac. Geneeskunde 1994; 133-134.

38 Spigelius A.'*De formato foetu*' Pavia: B. de Martinis and L. Pasquatus, 1626; 8-10.

1616), who obviously agreed with his own predecessor Fabricius that the sausage-like urinary membrane was not present in humans.[39]

Hieronymus Fabricius ab Aquapendente, (1537-1619, anatomist at the University of Padua between 1565 and 1604) is known as the father of (comparative) embryology with his works *de Formato Foetu* (1600)[40]and *de Formatione Ovi, et Pulli* (posthumously issued in 1621),[41]. The text of this beautifully illustrated monograph on the development of the chick does not measure up to the high quality of the illustrations, since Fabricius was still too much under the influence of medievalism to record a straightforward account of his observations.[42]

Remarkably correct and beautifully illustrated is his still used classification of placentas. He also described an external membrane covering the chorion as a 'certain membranous placenta material thicker than the other membranes and attached to the uterus', a structure that would later be termed, the 'decidua reflexa.' Fabricius stated that voiding fetal urine in the normal way is 'most inconsistent with Galen and with reason because the motive faculty is entirely inoperative in the foetus, neither does any muscle act, nor would nature employ a means of discharging urine from the human foetus so different from that in other animals.' According to Fabricius, the human foetus swims in its own sweat, and filters its urine through minute filaments in the umbilical cord towards the chorion.[43] With this, Fabricius went against **Arantius**, his opponent in Bologna, because as he said, 'it is safer to give credence to the ancients and to all later authorities rather than to only one man'.

Julius Arantius (Julio Aranzio, ca. 1529-1589), a scholar of Vesalius, was the first physiologist who ventured to call into question the dogmata of Galen. He was correct with his findings that a foetus voids urine into the

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39 Thiery M, Houtzager H. *Der vrouwen vrouwlijcheit*. Rotterdam: Erasmus Publishing, 1997;. 87.

40 Hieronymus Fabricius ab Aquapendenta. De formato Foetu' Venetie, Bolzetta. 1600. Translated into English, annotated and commented by HB. Adelmann as 'The formed foetus' in: '*The Embryological Treatises of Hieronymus Fabricius*' Ithaca (N.Y.), Cornell University Press, 1967.

41 Hieronymus Fabricius. 'De formatione Ovi, et Pulli'. Padua: A.Bencij, 1621.(Translated into English, annotated and commented by Howard B. Adelmann as 'The formation ofthe egg and of the chick' in '*The Embryological Treatises of Hieronymus Fabricius'* Ithaca (N.Y.): Cornell University Press. 1967.

42 Adelmann HB. Analysis of the Embryological Treatises of Fabricius. In: *The Embryological Treatises of Hieronymus Frabricius of Aquapendente*. Ithaca (N.Y) Cornell University Press, 1967; Vol. I, 87-99.

43 Fabricius (1600), 14-16, 23, 121-128 (Adelmann, 1967. Vol. I, 267-269, 299, 306-313).

amnion, since no urinary passage is found at the bottom of the bladder as in animals, and since the human urachus is just a ligament which binds the neck of the bladder to the peritoneum, something that, as he said, '*the dissector of the pregnant uterus, with a little care, can easily see'*.[44]

Also **Ambroise Paré** (ca. 1510-1590), a French anatomist, military surgeon and obstetrician, believed the human foetus voids into its amniotic fluid, initially via the open urachus that is blocked a few months before delivery, after which the human foetus urinates in the normal way. Paré advised the scholars [the *doctores medicinae*] to anatomise a woman in the last stages of pregnancy in order 'to look upon the truth insofar as they will open their eyes'.[45] The regents of the University of Paris found him to be most shameless, ignorant, and audacious and demanded, fortunately in vain, that his works be burned.[46]

Johannes Veslingius (Jan Vesling, 1598-1649), professor of anatomy at the University of Padua from 1632 to 1649, noted in his manual of anatomy, first issued in 1641, that the human foetus voids urine through the umbilicus, 'but as soon as the infant is born, the remains of the umbilical cord are left to the body after it is cut off; its former use ceasing, it is turned into a ligament.'[47]

Voiding urine in the amniotic fluid remained, in spite of the convincing work of Arantius, very controversial, for it was contrary to the principle of analogy between man and animal. For this reason, **Vopiscus Fortunatus Plempius** (1601-1671), professor at the University of Louvain, wrote in his *Fudamenta medicinae*, edited in 1644, that 'some anatomists doubt the existence of the human urinary membrane but I think that it does exist in analogy between man and animal, but that it is so thin-walled and fragile that it is often missed at dissection.'[48]

Aristotle's doctrines on causes and on the soul, on epigenesis and on differentiation in general were still prominent, but were already being hotly debated. In 1620, **Thomas Fienus** (Thomas Feyns, 1567-1631), a pupil of

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44 Arantius J. 'De Membranis Foetum Obvolventibus & de Uraco' In *'De Humano Foetu opusculum breve sed utile'* Bologna, 1564, 31-39.

45 Paré A. 'De la Génération de l'homme, receuilly des Anciens & Modernes' In: '*Les Oeuvres Complètes d'Ambroise Paré'*. Paris, 1575, Book XXIV, chapters VII and XII.

46 www.Medarus.org/médecins/Paré.

47 Vesling JV.'Syntagma anatomicum' Padua: P. Frambotti, 1641. Translated into Eglish by N. Culpeper as '*The anatomy of the body of man'*. London: P. Cole, 1653; 32.

48 Plempius VP. Fundamentae medicinae. Leuven: J. Zegers, 1644; 209.

Arantius and professor in anatomy at the University of Louvain from 1616 until his death in 1631, edited a widely discussed book on the formation of the foetus. The unique topic was the moment of implantation of the rational soul on the third day after conception.[49] According to Jan Papy, the present professor of Latin literature at the university of Louvain, the choice of this topic illustrates the attitude towards Aristotle's biological thoughts just before Cartesianism entered the stage, and incorporates in its fullest sense the tensions between authoritative and innovative thinking.[50] The Enlightenment was nearby.

4. The Enlightenment

During the course of the second half of the 17th and the first half of the 18th century, many theories were propounded on the existence of a human urinary membrane. One's own research became the primary requirement, and new insights were always successful. References to the ancient anatomists were avoided. *Aude Sapere*[51] had become the motto of the scientists.

William Harvey (1578-1657), who was at the medical school in Padua when Fabricius' *de Formato Foetu* was published, took considerable pains to refute everything that had previously been claimed for the human urachus and allantois, especially by Fabricius, since he believed that Fabricius mentioned it 'rather in justification of the doctrine of the Ancients, than that he himself found any such thing, or thought it useful to any attention'. Harvey's *de Generatione Animalium* issued in 1651, reprinted twice within one year, and translated into English in 1653, met with European-wide success. Harvey wrote that 'he was involved in the same errors as Arantius since he was sure that, if one compresses the bladder of a large-grown foetus, be it a human foetus or any other animal, the urine will start out at the privities.' He also stated that he 'never saw a urachus nor could even observe that upon compression of the bladder, urine would gush out into the secundines.' According to Harvey, there was indeed a certain kind

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49 Fienus Th. 'De formatrice foetus liber inquo ostenditur animam rationalem infundi tertia die'. Antwerpen: G. à Tongris. 1620.

50 Papy J. 'The Attitude towards Aristotelian Biological Thought in the Louvain Medical Treatises during the Sixteenth and early Seventeenth Century: the Case of Embryology' in: C. Steel – G. Guldentops – P. Bullezns (eds), *Aristotle's Animals in the Middle Ages and Renaissance*, Mediaevalia Lovaniensia, I/27 (Leuven: Leuven University Press 1999), pp. 317-337.

51 Dare to know.

of bladder in the umbilical cord of sheep and deer, but never an urachus as described by Fabricius,[52] which was a very strange statement from a brilliant scientist of his calibre, as it was known from Galen's time that the urachus does exist in ruminants.

Also, in the most used anatomy and obstetric textbooks of the time, it was stated that the human foetus has no urinary membrane. **Jean Riolan** the Younger,(1577-1657) well known as a convinced Galenist, after having anatomised a woman in late pregnancy, stated that an allantois does indeed not exist in a human conceptus.[53.] The Danish anatomist **Thomas Bartholinus**, (1616-1680) the most illustrious pupil of Veslingius in Padua, wrote 'that a child voids urine in the womb by its yard into the membrane amnios (which makes it full of liquor) and that a great part of urine is also retained in the bladder, which is the cause that newborn children, for the first are in a manner of continually pissing',[54] whereas the French obstetricians **François Mauriceau** (1637-1709) and **Pierre Dionis** (1643-1718) thought that a human child starts to void urine at birth.[55,56]

It looked as though the anatomists had definitely opted for the absence of a human fetal urinary membrane until **Walter (Gualtore) Needham** (ca. 1630-1691), a respected member of the Royal Society of London, in his book entitled *Disquiqitio anatomica de formato foetu,* issued in 1667*,* contended that the chorion can be divided into an outer porous, spongy, heavily vascularised membrane, and an inner sturdy transparent urinary membrane of a totally different structure. According to Needham, this urinary membrane was present not only in horses, his experimental animal, (**Fig. 10**) but also in men, cats, rabbits, and other animals with a placenta, despite far advanced differences. He also said that he could never find any sign of an open human fetal urachus, yet he was of the opinion that, by blowing air into the fetal bladder, the air might be forced through the

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52 Harvey W.'Excercitationes de generatione animalium' Amsterdam: J. Ravensteyn, 1651. Translation into English as '*The anatomical excercitations concerning the generation of living creatures'*. London: O. Pulleyn, 1653; columns 510-522.

53 Riolan J. l'Histoire du foetus humain. Book VI p. 899 In: *Les oeuvres anatomiques de M. Jean Riolan*. Paris: D. Moreau 1629; Book VI, 899.

54 Bartholinus G. Anatomy made from the precepts of his father, and from the observations of all modern anatomists, together with his own. London: P. Cole, 1668; Book I, Chapter 37, 84.

55 Mauriceau Fr. Des maladies des femmes grosses et accouchées. Paris: J. Henault, 1668; Book II, 215.

56 Dionis P. Traité général des accouchements, qui instruit tout ce qu'il faut faire pour être un habile accoucheur. Paris: Ch-M. d' Houvry, 1724; 106.

human urachus, as easily as he had often done through that of a whelp.[57] These statements lack any coherent meaning: the allantochorion of an horse, which might be compared to a carpet, composed of pile (the villi) and backing (the peripheral blade of the allantois), cannot be split, [58] whereas the human embryo has no urinary membrane at all. Within a few years, Needham did become an authority on the generation of the human foetus despite the contention that he had simply transferred his findings in animals to people.[59]

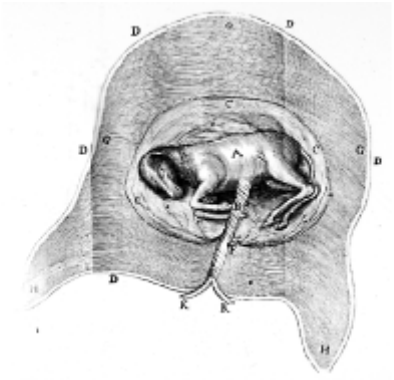

Fig. 10: A horse foetus(A) in its amnion. The external membrane or chorion (D) is fused with the urinary membrane (G). Fetal urine is drained through a side opening(F) in the umbilical cord (B) (Needham, 1667, Fig 1b)

In 1701, a strange paper appeared in the Transactions of the Royal Society of London. In this treatise, entitled 'the full discovery of the human allantois', the author, a certain **Richard Hale,** contended that 'neither Needham nor any other has taken the right method of finding the human allantois entirely'. For Hale, it was most important to inflate the allantois through the opening whence the urine of the human foetus issued, in order to see the true shape of the allantois, its fundus and cervix, its relation to the other membranes, and the insertion of the urachus.[60] Hale's illustration of the

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57 Needham W. 'Disquiqitio anatomica de formato foetu. London: R. Needham'1667; 55-67.

58 I thank Professor A. Weyns from the department of Zoology of the university of Antwerp for his efforts in this field.

59 Eloy N. *Dictionnaire historique de la médecine*. Mons: H.Hoyois, 1778;Vol III, 380.

60 Hale R. 'The Human Allantois fully discovered' in '*Philosophical Transactions of the Royal Society of London*. 1701;N° 271, 835 (Reprinted in Vol. IV of the Philosophical Transactions from 1694 to 1702, London 1809, 577-586.)

human embryonic membranes from an early abortion with an intact allantois as innermost wrapping [!] is a complete failure which clearly shows that in those days, peer review committees did not yet exist, not even for the Royal Society of London despite its motto *nullius in verba*.[61]

Anyway, for decades, scholars, probably without personal experience, referred to Needham's and Hale's findings to justify their belief in the presence of a urinary membrane in the human conceptus.

Nicolaus Hoboken (1632-1678), professor at the University of Harderwijk, reported on the human fetal membranes in 1669 and 1675.[62,63] It is clear from the exhaustive text and accompanying illustrations (44 in the 1675 edition!) that Hoboken did not split the chorion. He called it 'the middle (urinary) membrane' since, as he wrote, it collects urine via the internal urachus, and further on via the texture [the jelly of Wharton] of the umbilical cord. The inner part of the uterine wall was called 'the chorion' but is in fact the decidua. (**Fig. 11**)

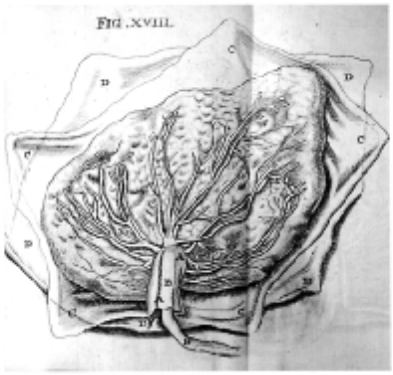

Fig. 11: The three human fetal membranes after splitting the chorion. (A) some remains of the amnion, (B) umbilical cord, (C) middle urinary membrane, (D) chorion (outer membrane). From Hoboken, 1675, Fig. XVIII, p. 506

61 Take nobody's word for it.

62 Hoboken N. Anatomia secundinae humanae, quindecim figuris ad vivum propria authoris manu delineatis illustrata.' Utrecht: J. Rubben, 1669; Chapter IV, 20-28 and Chapter X, 42-48.

63 Hoboken N. 'Anatomiae secundinae humanae repetita, aucta, reborata, qaudraginta figuris propria Authoris namu delineatis insuper illustrata.' Utrecht: Rubben, 1675; Book I, Chapter III, 112-130 and Book II, Chapter II, 470-480.

Reinier de Graaf (1641-1673), the famous discoverer of the significance of the ovarian follicles, illustrated his technique of splitting the human fetal chorion by inflating air between the chorion and the amnion,[64] (**Figure 12**) and in 1672, **Ysenbrand Van Diemerbroek** (1609-1674), professor at the university of Utrecht, wrote that

> 'all doubts concerning the allantois of women and the place where the urine of the embryo is contained and preserved, were most splendidly removed by the invention of Needham and the confirmation by Hoboken …. This Membrane, when others also saw, they took it for the inner part of the *Chorion*, and so asserted the *Chorion* to consist of a double membrane, to which opinion many other anatomists gave their consent.'[65]

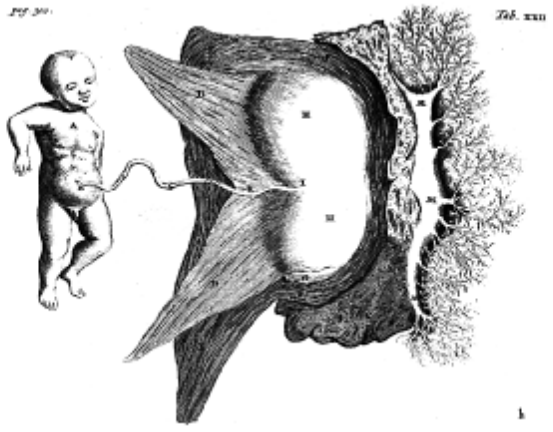

Fig. 12: Drawing of a three month (?) old human foetus after air insufflation between the amnion (D) and the chorion (E) to show the allantois (H). From De Graaf, 1705, plate XXII, 300

In 1679, the human allantois was included as 'the urinary receptacle' in **Blankaart**'s medical encyclopaedia,[66] and in 1685, the famous anatomist **Govert Bidloo** (1639-1713) from Amsterdam, edited his splendid volume *Anatomia Humani Corporis* including eleven plates on the gravid uterus

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64 De Graaf R. 'De Iis quae Ovo in Utero Accidunt'. In: *De Mulierum Organis Generationi'*. Leiden: Hacknania, 1672. (in *'Opera omnia'* Amsterdam: Wetsstebiana, 1705; 278-299.)

65 Diemerbroek Y. van, 'Anatome Corporis Humani'. Utrecht: 1672. Translated into English by Salmon W. as 'The Anatomy of Human Bodies'. Little-Britain: W. Whitwood, 1685; Book I, chapter XXXI, column 246.

66 Blankaart St. 'Lexicon Medicum Graeco-Latinum.' Amsterdam: J. ten Hoorn, 1679; 11.

and on the embryonic appendages (Plates LIII to LXIII).[67] Bidloo described the jelly of Wharton in the umbilical cord as small tubes for drainage of juice, one large vein and two umbilical arteries with a twisted shrivelled urachus as a small membrane in between. Most remarkable is the illustration of the chorion [which is undoubtly the decidua reflexa], separated from the urinary membrane. **(Fig. 13)**. Bidloo was an arrogant surgeon without any knowledge of, or interest in physiology, which explains why, having read the accompanying text, one still has to guess whether or not urine is contained in this fetal urinary membrane.

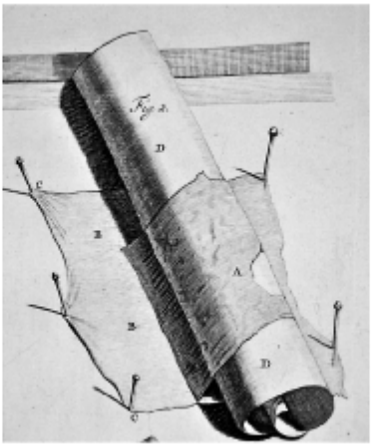

Fig. 13: Dissection of the human fetal membrane in the chorion (A) and the urinary membrane (B). From Bidloo 1685, Plate 61, fig. 2

But there were also convinced disbelievers in the human urinary membrane, such as:

Charles Drelincourt (1633-1697), professor of anatomy at the famous university of Leiden, who, in 1685, ridiculed the believers in the human urinary membrane in his treatise *De humani foetus membranis hypomnemata.*[68]

Frederik Ruysch (1638-1731) from Amsterdam, the most experienced anatomist of the Golden Age of Dutch anatomy, who taught that the

[67] Bidloo G. 'Anatomia Corporis Humani.' Amsterdam: J. van Someren, 1685; text opposite to plates 53 and 63.

[68] Drelincourt Ch. 'De humani foetus membranis hypomnemata.' Leiden: C. Boutateyn, 1685; 104-107.

chorion was occasionally double-layered. Ruysch told his pupil, and later friend Philip Verheyen, professor in anatomy at the University of Louvain from 1693 to 1710, that in 50 human afterbirths, he had never once encountered a urachus or an allantois.[69] He described the inner layer of the double-layered chorion as very thin and transparent, and called it the 'pseudo-allantois', with which he indicated that the allantois, as a urinary receptacle, does not exist in humans. The outer part he described as non-transparent, thick, very fragile, and spread over the entire upper layer of the placenta. According to Ruysch, this part of the outermost membrane could still be called the 'chorion' with the understanding that the deviating part over the placenta would be called the 'villous membrane', because there, the nutritive fluids proceed from the ends of the maternal arteries to the foetus.[70] **[**This villous membrane is undoubtedly the decidua reflexa.]

Philip Verheyen (1648-1710) for whom the human foetus had no urinary membrane (which was called *pisvlies* in the *Lingua Belgica*).[71] He had given a public lecture on the urinary membrane of the calf while studying in Leiden,[72] **(Fig. 14)** and repeatedly asked Bidloo, his opponent at the University of Leiden, to give an appropriate demonstration of his dissection technique of his presumed human urinary membrane.[73] This bold question led to a sharp dispute and to insults, which were, in those days, apparently tolerated in Dutch academic circles, and which only came to an end when Bidloo died in 1713.[74]

Willem Noortwijk (ca 1713-ca 1777), an obstetrician from Leiden, who, in 1743 wrote a book about the anatomy of the pregnant womb based on his findings at the post-mortem of his wife who had died when she was six months pregnant.[75,76] He added a very critical study of the literature on the

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69 Verheyen Ph. p. 716 in: 'Betreffende de Voort-teelinge van den Mensch en des zelfs Voltrekkinge tot sijne Geboorte ' pp. 641-798, in: *Anatomie oft Ontleed-kundige beschrijvinge van het Menschen Lichaem'.* Vertaling door Sassenus A. Brussel: t' Sertstevens; 1711; 716.

70 .Ruysch Fr.'Het vijfde en het zesde kabinet.' in '*Alle de ontleed-, genees- en heelkundige werken'* Amsterdam: Janssoons van Waesberge. 1744; pp. 636, 642, 653, 682.

71 Verheyen Ph. 'Betreffende de Voort-teelinge van den mensch en des zelfs voltrekkinge tot sijne Geboorte'. In: *Anatomie oft Ontleed-kundige beschrijvingen van het Menschen Lichaem*. Brussel: t' Serstevens, 1711; Book VII; 641-798..

72 Verheyen Ph. 'Supplementum anatomicum sive anatomiae corporis humani'. Liber secundus. Brussel: t' Serstevens, 1710. 334, Plate V.

73 Verheyen Ph., 1711; Book VII, 718.

74 Fourneau I, J. Papy, R. Suy. De polemiek tussen Govert Bidloo en Philip Verheyen over het menselijk pisvlies. *Geschiedenis der Geneeskunde.* Antwerpen/Appeldoorn: Garant. 2011; **4**: 236-247.

75 Noortwyk W. 'De uteri humani gravidi anatome et historia' Leiden: J. & H. Verbeek. 1743.

76 Lindeboom GA. Dutch Medical Biography. Amsterdam: Rodopi. 1984; column 1437.

Fig. 14: Illustration of the afterbirth of a human foetus (1&2) and of a calf foetus (3 to 5). From Verheyen 1710 Liber II, Plate V, p. 344

human allantois which he concluded as follows: 'I, like Harvey, will not stubbornly deny that there is an [human] allantois, but, insofar as someone can have confidence in himself, I, with appropriate modesty, will not be ashamed to openly assure that I, in so many ways, have become so convinced that I do not doubt that a urachus and an allantois, of whatever nature, are lacking in man.'[77]

The doubt about the existence of a human foetal allantois was clearly articulated by **Alexander Monro Primus** (1697-1767), the father of academic medicine in Scotland, who in 1744 wrote that:

> "The existence of this membrane [the allantois] in women has been warmly disputed on both sides. Those who are against its existence deny they could ever find it; and allowing if it were so, allege that since the urachus is impervious, as appears by our not being able to throw liquors from the bladder into it, or *vice versa,* it cannot serve the use that is agreed by all it does in beasts, and therefore in the human body there is no such thing. But when I considered on the other hand, first, that there seems to be the same necessity for such a reservoir in man as in animals; secondly, that we actually find urine contained in the bladder of the human foetus; thirdly, that urine has been evacuated at the navel when the urethra was stopped, which urine

77 Noortwyk W, 201.

without this conduit would have fallen into the cavity of the abdomen; fourthly, that midwives have pretented to remark two different sorts of water come away at the time of birth; and lastly, that Dr. Littre and Dr. Hale have given in this membrane of an human subject with all the other secundines curiously prepared, the one at the royal academy at Paris, the other to the royal society at London, by which societies their respective accounts are attested; not to mention Verheyen, Heister, Keil, &c. who affirm their having seen it; and Mr. Albinus, that famous anatomist, professor at Leyden, shows, as I am told, to his college every year a preparation of it:[78] On all these accounts I must own, that it seemed not improbable to me there was such a membrane in the human body. But in the four bodies I purposedly dissected, wherein I was assisted by a very accurate anatomist, Dr. Sinclair, I could not observe any such thing. However my want in skill will more be doubted, than the truth of relations supported by such autenthic vouchers called in question.'[79]

5. The end of the myth of the human urinary membrane in the second half of the 18th century

The definite breakthrough came in the second half of the 18th century with the most prolific physiologist **Albrecht von Haller** (1708-1777), and with **William Hunter** (1718-1783), the most celebrated teacher of anatomy in his day in Britain.

In 1739, Haller was, as a young professor at the new University of Göttingen, the promoter of two inaugural dissertations on post-mortem findings in pregnant women. Both of the *promovendi* described the allantois as a sturdy membrane that could easily be released from the soft chorionic villi on the inside of the uterine wall, but without any connection whatever with the urinary bladder, since the urachus was just a ligament.[80,81] In 1744, Haller called this structure the 'middle membrane'. He described the chorion as villous, fleshy, full of pores and small vessels, of a reticular

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78 Monro is misinformed, since Verheyen and Heister were real disbelievers, whereas Albinus demonstrated an umbilical vesicle or an hydatid cyst in the umbilical cord.

79 Monro A. primus**, '**An Essay on comparative Anatomy'. London: J. Nourse. 1744; 70-72.

80 Haller A, Meyer J. Historia nuperae dissectionis feminae gravidae. Göttingen: A. Vandenhoeck. 1739; 1-19.

81 Haller A, Lehman I. De morbo hypochondriaco. Göttingen: A. Vandenhoeck, 1739; 1-12.

fabric, easily lacerable, so as to resemble a fine placenta, connected to the flocculent surface of the uterus by vessels smaller that those of the placenta, but manifestly inosculated from the chorion into the vessels of the uterus [the inner layer of the decidua]. To the question 'whether there is an allantois?' he answered that 'any proper receptacle continuous with the hollow urachus, either has not yet been observed with sufficient certainty, or else the experiment has not yet been repeated often enough to become general in the human species, …. but that it may be not improbable that some portion of the fetal urine is conveyed into the umbilical cord, and that therefore of all animals, man has the longest umbilical cord, because he has no allantois.' And he added that 'those eminent anatomists who have observed a fourth kind of vessel to be continued along the umbilical rope into its proper vesicle, will not allow the vessel to be called urachus and very lately have referred it to the omphalomesenteric genus [the umbilical duct and vesicle].'[82]

'Those eminent anatomists' were undoubtly **Herman Boerhave** (1668-1738) and **Bernhard Siegfried Albinus** (1697-1770), Haller's teachers at the University of Leiden. Boerhaave had, referring to Hale and to Spigelius [why not to Galen?], described an oval bladder on top of the placenta between the chorion and the amnion for the reception of the ever-increasing amount of fetal urine,[83] whereas Albinus in his anatomical notes, reported a vesicle at the end of the umbilical cord. This small organ, which apparently was shown to the students as an allantois,[84] was either a hydatid cyst, or an umbilical vesicle as it can indeed be found at the third month of pregnancy as a shrunken oval organ between the fused amnion and chorion.[85]

William Hunter was an obstetrician with a special interest in the anatomy of the afterbirth and placenta. Apparently, he also had dissected mammals late in gestation in order to enlarge his knowledge of the subject. He denied the existence of an allantois in the human species, and found all previous

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82 Haller A.von, 'Primae Linguae Physiologiae' Göttingen: Vandenhoeck, 1747. Translated into English by W. Cullen as '*First Lines of Physiology'*. Edinburgh: Ch. Elliot, 1779; 463-466.

83 .Boerhaave H. '*Institutiones medicae in usus annuae exercitationes domesticos*' Leiden: J. vander Linden, 1713. § 684, 275-276. Translated into French by de la Mettrie as '*Institutions de médecine'*. Paris: Huart et Bresson, 1740; Vol. I, 464-465.

84 Neufville L. de, 'Dissertatio medica inauguralis allantoide humana'. Leiden: H. & J. Verbeek. 1736; 40.

85 Albinus SB. 'Academicarum Annotationum' Leiden: J.& H. Verbeek. 1754-1756; Chapter XIX, 74-75.

descriptions either erroneous or wholly imaginary. Hunter, who was able to secure 'with the assistance of many friends' [by body-snatching?] thirteen bodies of women in late gestation, exhibited his findings in figures on 34 plates accompanied by a descriptive text in English and Latin in his *Anatomia uteri humani gravidi*, first issued in 1774 and re-edited in 1815.[86] Hunter described the inner layer of the endometrium as a thick, spongy and brittle membrane which in the early months aligns loosely with the uterus, and which divides at the edges of the placenta into two strata, one between the placenta and the uterus, and the other reflected over the placenta, and therefore termed by him '*decidua reflexa'*.[87] In his lectures on the gravid uterus and midwifery, Hunter said: 'We will now, gentlemen, for once try what new language will do. I will say there are three membranes, the 'amnion,' seen on the inside of these two commonly called 'true' and 'false' chorions, and the external one of all 'decidua' or 'caduca', which is a thick spongy and brittle membrane. This decidua, we shall see is a layer of the uterus.'[88] William Hunter was also the first to describe and picture precisely an umbilical vesicle **(Fig. 15),** but he was not the first to describe the inner part of the endometrium as a membrane around the conceptus. As already metioned, it was already reported and illustrated in 1600 by Fabricius, but it was on Hunter's authority that eventually, one hundred years after Needham's notorious book, the myth of the human fetal urinary membrane or allantois was definitely dispelled.

6. The mini-allantois, a consolation prize for the believers

At the beginning of the 19th century, embryology became a fully-fledged specialty. Nearly all embryologists virtually agreed that there was no human urinary membrane until reports were published about an additional

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86 Hunter G. 'Anatomia uteri humani gravidis'. London: E. Cox, 1815.

87 The actual description of the changes in the endometrium within the first weeks of pregnancy is as follows: "During the transit of the fertilised egg through the Fallopian tube, the uterine mucosa has been prepared for the reception of the fertilised egg. It is thicker, velvety, soft, spongy, vascularised, and its glands are full of clear secretion [sugar]. The human blastocyst buries itself in the endometrium and expands within the mucosa, growing laterally beneath a considerable area of superficial mucosa which becomes the decidua reflexa. The portion of the mucosa on which the ovum rests is called the decidua capsularis; all the rest of the lining of the uterus is called the decidua vera." (from: De Lee JB, Greenhill JP. The Development of the Ovum. In: *Principles and Practice of Obstetrics*', Philadelphia and London: W.B. Saunders Co. 1948; 15-16).

88 Teacher JH. 'The contents of the pregnant uterus' in the '*Catalogue of the anatomical and pathological preparations of Dr. William Hunter'* Glasgow: MacLehose. 1900; Vol. I, 697-703.

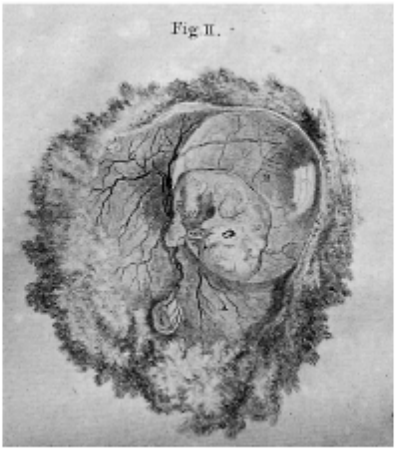

Fig. 15: Drawing of a four weeks old human conceptus, showing the umbilical vesicle (C), distended with a fluid. From Hunter 1815, Plate XXXV, fig. 2

embryonic appendage that, during the first weeks of pregnancy, was situated between the amnion and the chorion, and which not could be mistaken for the umbilical vesicle.'

In 1817, **Johann Friedrich Meckel** the Younger (1781-1833), professor at the University of Halle, reported observing a small, very thin-walled, flaccid sac, filled with clear fluid, and located between the chorion and the amnion in a four week old human embryo.[89] (**Fig. 16**) In his textbook on anatomy, issued in 1820, he reported on several similar cases in which he succeeded to follow the hollow urachus up to the placenta, but not as far as the space between the chorion and the amnion. He thought, much to the great annoyance of Velpeau, his opponent in France, that this sac was the shrivelled allantois.[90].

Alfred-Armand Velpeau (1795-1867) surgeon and obstetrician at the Pitié Hospital in Paris, was also occupied in research on the structures and connection of fetal appendages. He had dissected more than 200 human conceptuses, and succeeded in isolating a distinct reticular mass in the space between the amnion and the chorion in several embryos within the

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89 Meckel JF. von. 'Deutsches Archiv für die Physiologie'. 1817; Vol. 3, part 1. Plate I, figure 2.

90 Meckel JF. von. 'Besondere Eingeweidlehre und Geschrifte der Fötus' in '*Handbuch der menschlichen Anatomie'.* Halle: Hallischen Waisenhaus, 1820: Vol IV. Translated into French as *Manuel d' anatomie générale, descriptive et pathologique*. Paris: J.B. Baillière. 1825; Tome III, 768-772.

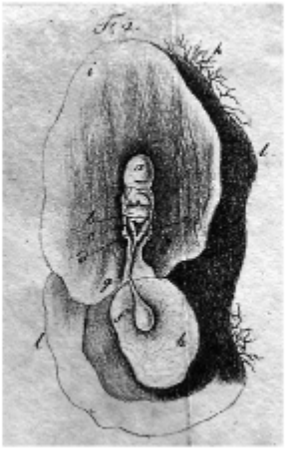

Fig. 16: Drawing of a four week old human conceptus showing the umbilical vesicle (f) and a flaccid membranous allantois (h). From Meckel, 1814, Table I, figure 2

first five weeks of gestation. (**Fig. 17**) This structure was filled with a creamy vitriform liquid but a connection with the fetal bladder could not be found. Nevertheless, Velpeau called this structure 'allantois', but he thought that it was not primarily a urinary sac, but a nutritive organ, and the initial stage of another not yet well-determined organ.[91] Velpeau had also a keen interest in the decidua, which he called *la membrane anhiste* because this inner part of the uterine wall, which was in his opinion avascular, contained much fluid [sugars], but had no well-defined structure.[92] This erroneous conclusion was drawn probably because he virtually never used a microscope, an instrument that, as he wrote, everybody knows how many illusions it has generated in the sciences for which has been used.[93]

In 1837, **Karl Ernst von Baer** (1792-1876), a Baltic German biologist and professor at the Königsberg (Kaliningrad) University, stated with conviction that Velpeau's reticular organ was a soft protein mass that formed threads upon contact with water or alcohol *(Weingeist*). He added that this mass, which is situated between the chorion and the amnion, occurs in

[91] Velpeau A.'Embryologie ou Ovologie humaine, contenant l'histoire descriptive et iconographique de l'oeuf humain' Bruxelles: H. Dumont. 1834; 30-37.
[92] Velpeau A., 1834; o.c. 1-7.
[93] Velpeau A., 1834, o.c.Preface, p. 1.

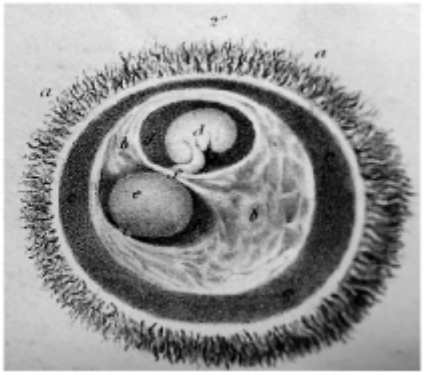

Fig. 17: Drawing of a human conceptus of twelve days showing an embryo (d) within its amnion(b) and chorion(a) and inbetween an umbilical vesicle (e) and a mass considerd by Velpeau to be the allantois. From Velpeau, 1834, Table I, Fig. 2

many mammals in an early embyonic stage, and presses the umbilical vessels against the chorion [to form the allantochorion].[94]

Meanwhile, **Christian Heinrich Pander,** (1794-1865) also a Baltic German biologist and a good friend of von Baer, had started the microscopic study of the development of chickens' eggs during the first five days of incubation at the University of Würzburg. He used a total of 2.000 incubated eggs; one was opened every quarter of an hour! Pander described three distinct components which give rise to the development of all tissues. He called them the serous layer [ectoderm], the mucous layer [endoderm], and the vascular membrane [mesoderm].[95] He thought that the vascular membrane, which follows the meandering serous layer, forms the chorion or allantois [he preferred to use the term chorion instead of allantois]. His report on the further development of the chickens' allantois (during the first five days of incubation) is somewhat sketchy.[96]

His work was continued by his friend von Baer, who saw, in 1826, mammalian fetal eggs appear in the wall of Graafian vesicles, followed them along the oviduct into the womb, where he watched their further

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94 Von Baer KE. Über Entwickelungsgeschichte der Thiere. Beobachtung und Reflexen. Königsberg: Bornträger, 1837: 383.

95 See Note 2.

96 Pander CH. 'Beitrage zur Entwickelungsgeschichte des Hühchens im Eye'. Würzburg: H.L. Brönner, 1817. Translated into French and commented by Stéphane Schmitt as: *Les textes embryologiques de Christian Heinrich Pander, (1794-1865).* Turnhout: Brepols. 2003; 155-189

development.[97] Von Baer developed Pander's theory of the formation of tissues, laying the foundations of modern (comparative) embryology in his main work entitled *Über Entwickelungsgeschichte der Thiere* of which the first part appeared in 1828, and the second part in 1837. All tissues of several human embryos within a few weeks of gestation had been microscopically examined with Pander's germ layer theory in mind. Nearly always, a small umbilical vesicle and a very small allantois were observed in the umbilical cord as far as the body stalk. Von Baer widely discussed the development of the human urinary membrane (*Harnsack*), which was, according to him, built up from the vascular membrane (*Gefässblatt*) [mesoderm] and from the mucous layer [endoderm]. According to von Baer, the allantois deserved its nickname, because it originates from the cloaca, and also because it carries the umbilical arteries towards the chorion. [Could it be that von Baer was unaware of the etymology of the term allantois?] He also stated that the allantoic duct arrives at its destination within six weeks of pregnancy, followed by its obstruction by connective tissue.[98]

Von Baer's contribution was the definitive end of the quest for the once elusive human urinary membrane or allantois. At that moment, the question remained why man, in stark contrast to most other eutherians, does not have a urinary membrane.

7. Why does the human foetus not have a urinary membrane?

From the early 1700s on, some disbelievers (in the presence of a urinary membrane in the human foetus), thought about this question. It was more to do with speculation than with scientific knowledge and evidence. As already mentioned, some scientists thought that the human foetus retained the urine in its bladder, and started to void only after birth. Other sceptics, such as Harvey and Haller, thought that a small quantity of urine was contained within the very long umbilical cord. The most amazing explanation for the absence of a human allantois was to be found in the well-known *Dictionnaire raisonnée des sciences, des arts et des métiers* first issued in

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97 Von Baer KE, Sarton G. The Discovery of the Mammalian Egg and the Foundation of Modern Embryology' in *Isis*: Chicago: Chicago University Press, 1931; Vol 16, 315-377.

98 Von Baer KE. 1837. o.c.p381-383.

1750. The authors stated that the large human foetal head, because of its excessive demands, takes nearly all of the available nutritive blood, at the expense of the lower part of the body [including the kidneys]. In humans,a reduced amount of urine could easily be contained in the very long umbilical cord.[99]

The previously mentioned Philip Verheyen from Louvain postulated that voiding urine by the foetus into the womb would be a very uncomfortable situation for bipedal eutherian females because of the possibly large weight and quantity of the additional liquid. He thought that, in the placenta, the extremities of the fetal arteries might link perfectly to the extremities of the maternal arteries, and that therefore the serous ("*weiachtige"*) fetal liquors can be easily transferred to the mother.[100] This statement is, as far as we know, the first reference to the optimal contact between the human maternal and fetal circulation, which is indeed the major reason for the absence of a human fetal urinary membrane, as will be briefly discussed in the next paragraph. A foetus of an animal species with an optimal contact between the maternal and foetal circulaton may eventually void into its amniotic fluid without any risk of damaging its delicate skin. Such foetuses have either a very small urinary membrane or none at all.[101]

The placenta barrier

Before the formation of the placenta, there are three fetal components (the endothelium of the allantoic capillaries, the connective tissue in the villi, and the chorionepithelium) opposed to three maternal components (the epithelium of the endometrium, the connective tissue of the endometrium, and the endothelium of the endometrial capillaries). Advanced microscopic examination of the placenta, and especially of the contact zone between the maternal and the fetal blood, became possible in the second half of the 19th century. Pioneering in this field was **Mathias Duval** (1844-1907), professor at the university of Strasbourg. He reported between 1880 and 1889, sinuses, which he called *lacunes*, in the fetal part of the placenta of rabbits, rodents and bats, filled up with maternal blood and situated within

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99 Encyclopédie, ou Dictionnaire raisonnée des Sciences, des Arts et des Métiers. Diderot et d'Alembert (eds.). Berne/Lausanne, 1781; Tome II, 146.

100 Verheyen Ph. 1711; o.c. p745.

101 Bats, rabbits and rodents have a very small, sometimes nearly detectable urinary membrane located on top of the placenta. Anthropoid apes and man have no urinary membrane at all

an ectodermal embryonic mass. He also widely mentioned the merits of **Edouard van Beneden** (1847-1910), at that time professor in zoology at the université of Liège, who, in 1888, had stated that such a placental structure is also present in man.[102]

Finally, in 1909, **Otto Grosser** from Prague proposed the widely used classification based on the nature of the maternal tissue with which the chorion makes contact, with preservation of the six components in the 'epitheliochorial type' (in animals with a diffuse placenta such as pigs and horses), the loss of the endometrial epithelium in the 'syndesmochorial type' (in animals with a cotyledonary placenta such as ruminants), the loss of endometrial epithelium and connective tissue in the 'endotheliochorial type' (in animals with a zonary placenta such as carnivores), and finally, the 'haemochorial' type with direct contact between the chorionic epihelium and the maternal blood in animals with a discoid placenta such as bats, rodents, rabbits, anthropoid apes, and man.[103] In the 1927-edition of his monumental work, Grosser concluded his extensive chapter on placentation with this remarkable statement: 'As for the restriction of the individual lifetime, we also find in the formation of the placenta indications of the limited lifetime of mankind'.[104] The presence of an intact chorionic epithelium seems to be essential for the survival of the human foetus. In 1926, it was stated that rabbits and rodents have a haemo-endothelial placenta type, which was later disproved by electron microscopic examinations.[105]

Epilogue

A descriptive science such as embryology is based on accurate quantitative observations, precise prediction, and rigorous methods of testing hypoth-

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[102] Duval M. 'La Placenta des Rongeurs' in: G.Pouchet & M. Duval (eds,) '*Journal de l' anatomie et de la physiologie normales et pathologiques de l'homme et des animaux'*. Paris: F. Alcan, 1889; 1, 309 – 342.

[103] Grosser O. Schlussübersicht über die Placentation der Säugetiere. In: *Vergeleichende Anatomie und Entwickelungsgeschichte der Eihäute unde der Placanta mit besonderer Berücksichtigung des Menschen.* Wien: Braumüller, 1909; 290 – 294.

[104] Grosser O.Schlussbemerkungen. In: Frühentwicklung Eihautbildung und Placentation des Menschen und der Säugtiere. München: J.F.Bergmann, 1927; 402 (*So wir für die Begrenzung des Lebens des Inviduums, finden wir in Placentarbau auch Hinweise auf die durch den Abschluss einer Entwickelungsreihe gebrenzte Lebensdauer der Art, der Menscheit im ganzen)*.

[105] Stevens DH. (ed.) Anatomy of the Placenta Barrier, Chapter 2, p. 25-57, in*: Comparative placentation. Essays in structure and function.* London: Academic Press, 1975.

eses. 'It's all-in the numbers' (*meten is weten*), is the only dogma that counts.

The presence or absence of an allantois in the human conceptus was disputed by anatomists up to the second half of the eighteenth century. The principal reasons for this have been demonstrated in this albeit incomplete historical review. They are, (1) shortage of appropriate anatomical specimens, (2) presumed analogy between man and animals, (3) respect for ancient anatomists and mentors, (4) urge to philosophise about unknown or mysterious concepts such as the then so-called epigenesis and transformation, (5) and most importantly, lack of technology to go deeper into the structure of tissues.

Laudatio Niccolò Guicciardini

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Prof. Dr. Niccolò Guicciardini, a scion of an illustrious family of historians, is a historian of the exact sciences in the modern period at the University of Bergamo. He has been a Professeur invité at the Université Paris 7 Denis Diderot and Mellon Visiting Professor at the California Institute of Technology. In addition to being awarded a Sarton Medal at Ghent University (March 2012), Prof. Guicciardini has been awarded the prestigious Fernando Gil International Prize for the Philosophy of Science as well as *Selezione Giuria Scientifica del Premio Letterario Galileo per la Divulgazione Scientifica* during the past year. These prizes testify not just to the wide esteem with which Prof. Guicciardini is held inside and outside the professional environment in which he labors, but in their diversity also provide a sense of the significance of Prof. Guicciardini's work. In particular, while Prof. Guicciardini's research focuses on the history of mathematics and physics in the age of Newton his impact is felt in the philosophy of mathematics, especially in questions surrounding the application of mathematics. Moreover, he is also a brilliant disseminator of the history and philosophy of science to a wide audience. Within the academic world he has not shirked performing (more than) his fair share of editorial and professional duties. I have come to know him as a generous correspondent, a splendid mentor to younger scholars, and warm academic host.

Prof. Guicciardini's academic reputation rests primarily on three intersecting books that have appeared, like a perfect isochronous pendulum, at the end of each decade. The first to be published was *The Development of*

Newtonian Calculus in Britain, 1700-1800 (1989; this was based on his second degree supervised by Ivor Grattan-Guinness at Middlesex Polytechnic); the second was published in 1999: *Reading the Principia: the Debate on Newton's Mathematical Methods for Natural Philosophy from 1687 to 1736*; and the third, *Isaac Newton on Mathematical Certainty and Method*, followed in 2009. In addition he has written (among other books) a textbook on quantum-mechanics in Italian and several popular biographies on Newton, including one, *Newton: un Filosofo della Natura e il Sistema del Mondo*, that was translated widely, including Dutch. The titles of these books do not give an accurate representation of the diversity of Prof. Guicciardini's contributions: he has written classic articles in the historiography of the history of mathematics as well as on Johann Bernoulli, John Keill, Jakob Hermann, and Thomas Reid among others.

In what follows I call attention to four significant aspects of Prof. Guicciardini's work. My first point concerns his writing-style; the second and third points focus on some of his particular contributions to our historical understanding; the final point returns to style, in particular, Newton's style.

First, I praise the clarity of Prof. Guicciardini's English prose. It is easily forgotten when reading his books, but he is writing about a mathematical world that is nearly completely alien to contemporary readers; presuppositions, notations, and techniques (among other things) have shifted dramatically since (say) around 1700. Rather than copying his words and saying, "see," I'll give a sense of his achievement from an autobiographical perspective. Prof. Guicciardini's researches involve the most heavily studied episodes in the history of mathematics and physics. The history of a science as a professional discipline grew up around Newton studies. I have spent most of my adult life (nearly twenty years) on the materials presented and analyzed by Prof. Guicciardini, as well as on much of the secondary literature that he has assimilated. The materials involve the development of complex interaction among mathematics, physics, and philosophy in their evolving historical and biographical contexts. It is very easy to focus on one aspect and lose sight of the bigger picture; it is very difficult to be expert on the (metaphorical) trees and still present the forest. I studied with two of the other leading Newton experts (George Smith and Howard Stein) and the world's leading Descartes expert (Dan Garber). In his 2009 book there is no doubt that a discerning eye will recognize, for

example, Prof. Guicciardini's substantial debts to (say) Henk Bos' brilliant 2001 work on Descartes or D.T. Whiteside's monumental scholarship on Newton's mathematical papers. (Prof. Guicciardini is very generous in his acknowledging debts.) But it is only *after* reading Guicciardini that I could go back to some of the crucial passages in Bos and Whiteside and feel confident that I really would understand what is at stake in their arguments!

Second, one of Professor Guicciardini's fundamental scholarly contributions involves undoing a persistent myth. Now, I think all scholars that work on well documented historical episodes sometimes fantasize about showing that everybody else got it wrong before. Most of us have to settle for relatively minor changes in emphasis on the significance or meaning of the past. In general, at our best we make precise what was previously discerned confusedly. But Professor Guicciardini's first two books show we were really all wrong.

Let me explain. The rapid development of the calculus and rational mechanics after the initial inventions by Newton and Leibniz is commonly associated with Hermann, the Bernoullis, Euler, D'Alembert, Clairaut, etc. Prior to Professor Guicciardini's 1989 and 1999 books, scholars believed that with one or two exceptions British mathematicians were being completely outclassed by their Continental peers – the myth (presumably developed in the nineteenth century) told us that the Brits were stuck with a terrible, impracticable notation and too reverential of Newton's authority. Professor Guicciardini's books show in remarkable and penetrating detail that the British mathematicians were not just in an ongoing conversation of equals with developments in Basel, Paris, and Berlin; they were also proving exciting results and competing successfully for prizes in the main Academy competitions. While there was a drop-off in the quality of British mathematics eventually, it came much later for reasons that have become urgent research matter in the wake of Professor Guicciardini's path-breaking research.

Third, I call attention to the remarkable fifth chapter of Prof. Guicciardini's 1999 book. The title, "Huygens: The *Principia* and proportion theory," captures the content but not its significance. The episode is familiar: Newton explicitly modeled his *Principia* on Huygens' (1673) *Horologium*, which he repeatedly praised. Both books deal with physics in geometric fashion; both hide some of the advanced mathematical techniques that they

deployed in discovery phase. Newton must have been very disappointed by Huygens' critical response; Huygens rejected Newton's account of universal gravity on methodological and empirical grounds (Schliesser & Smith). By relying on manuscript evidence from Huygens and by focusing on their very different treatments of proportions, Guicciardini has found a very original way to make clear in vivid fashion the enormous mathematical and conceptual gulf separating the two leading seventeenth century mathematical philosophers. Doing so reveals (among other things) that in Newton proportions are "understood as equations, that is, objects not acceptable to a purist Euclidian." (Guicciardini 1999, 134) Huygens adheres to standard proportion theory, while Newton uses the language of proportion. But while Huygens "studies variation in time of physical magnitudes by comparing changes acquired by two related magnitudes after equal infinitesimal intervals …. Newton … writes relations between a greater number of magnitudes (leaving them all to vary) and evaluates, through limit arguments, rates of change." (Ibid) In the chapter leading up to these concise lines, Prof. Guicciardini teaches us how to understand with precision how differences in mathematical technique are connected to physical and metaphysical commitments. The chapter is a major contribution to the comparative study of Huygens and Newton!

Fourth, I return to matters of style. Style is often thought to be a discredited concept in the study of art history and mathematics (Cf. Panofsky with Bourdieu). The word "style" does not, in fact, occur in the (otherwise) helpful index of Prof. Guicciardini's 2009 book. Even so the word is deployed throughout the argument, and one brief section (8.7) is officially devoted to style. In particular, the book explores the contrasting mathematical styles of Descartes, Barrow, Leibniz, and Newton in depth. These are not merely rooted in biographical and social factors (publishing conventions, academic norms), but also in conflicting philosophical commitments. Now, Professor Guicciardini's strategy is to operationalize "style" by way of an exact and careful analysis of the different and evolving preferences for various mathematical techniques deployed by Newton (let's focus on him). Prof. Guicciardini then shows that Newton's preferences get articulated and theorized in a complex, unstable philosophy of mathematics. Professor Guicciardini's treatment is sympathetic, but by no means uncritical (e.g., "Newton's position is ultimately contradictory," (308; see

also 383 and, especially, 385ff)). In my view Professor Guicciardini shows with his masterful book that Newton was in some sense fundamentally at odds with the intellectual world, which values the intellectual division of labor and the decomposition of complex problems into manageable technical problems, that, within constraints, Newton shaped enduringly. Guicciardini's "help to Newtonian scholarship" (2009: 387) is a monument to the exciting and fallible human journey of intellectual achievement.

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"Specious algebra is fit enough to find out, but entirely unfit to consign to writing and commit to posterity": Newton's publication strategies as a mathematical author

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In my lecture I will be advocating an approach to the history of mathematics inspired by the method followed by intellectual historians. In the first part, I will recall some of the desiderata for intellectual history proposed long ago by Quentin Skinner. I take Skinner's rejection of what he called the "mythologies" of the history of ideas as a proposition that can still be inspirational for historians of mathematics. In the second part, which is devoted to Newton's publication and authorial strategies, I will show how a kind of humanistic historicism inspired by the work of intellectual historians can be pursued in writing, in an historically informed way, about one of the great giants of early modern mathematics. In the third part, I will attempt to draw some conclusions.

First, I will state that I do not wish to downgrade, or reject, internalistic approaches to the history of mathematics, which indeed have been, and will continue to be, of central importance. Second, I will make clear that my talk is not meant to be a defence of Skinner's dense philosophical ideas on textual interpretation, which are inspired by Wittgenstein and Austin. My approach is much more philosophically naive, and should rather be taken as a methodological reflection on the craft of the history of mathematics, carried out by one of its practitioners.

1.

Historians of philosophy have often considered the contrast and balance between two approaches: one focussed on textual analysis, the other on context. "According to one school of theorists, the philosophically important aspect of a text is the text itself, which, it is maintained, is logically independent of, and intellectually autonomous from, any historical context. All that is relevant to the understanding of any philosophical text is timelessly in the text itself".[1] Contrasting with this assumption of a permanence of meaning that is outside of time, and a-historically locked in "atoms" of text, is an opposing theoretical school according to which terms and arguments in the history of philosophy must be interpreted within the special framework of concepts and distinctions specific to the thinker's cultural context.[2]

One might recall here that in the 1960s Quentin Skinner took position against approaching a text as a self-sufficient object of enquiry. This formidable historian of political thought maintained that texts are best understood as linguistic actions embodying what Austin termed the author's "illocutionary intentions," and advocated the need of situating historical utterances in their intellectual and linguistic context, in order to establish the purpose their authors had in mind when advancing their arguments.[3] It is an absurdity, he continued, to regard the contributions of past actors to problems unavailable to them. Too often as historians we ask past actors our questions and see what their answers are. Such "mythology of doctrines" debases history to a "pack of tricks played on the dead," as Voltaire would say.[4] Skinner writes: "The most persistent mythology has been created by historians working with the expectation that each classic

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1 A. W. Levi, *Philosophy as Social Expression* (Chicago and London: The University of Chicago Press, 1974), p. 1.

2 James E. Force, William Whiston, Honest Newtonian (Cambridge: Cambridge University Press, 1985), p. 1.

3 See Quentin Skinner, *Visions of Politics, Volume 1: Regarding Method* (Cambridge: Cambridge University Press, 2002), and J. L. Austin, *How to Do Things with Words*, ed. J. O. Urmson and Marina Sbisà, 2nd ed. with corrections (Oxford: Oxford University Press, 1980).

4 "J'ai vu un temps où vous n'aimiez guère l'histoire. Ce n'est, après tout, qu'un ramas de tracasseries qu'on fait aux morts." Voltaire to Pierre Robert Le Cornier de Cideville (9 February 1757) in *Voltaire, Correspondence*, ed. Theodore Besterman (Geneva, Institut et Musée Voltaire, 1953-1965), vol. 31, pp. 47-8.

writer … will be found to enunciate some doctrine on each of the topics regarded [today] as constitutive of the subject."[5]

These considerations, applicable as they are to the history of political theory, as Peter Laslett's study of Locke's *Two Treatises* showed long ago, at first sight might seem extraneous to the history of mathematics.[6] Considering a text as a self-sufficient object of enquiry that can be evaluated in terms of the consistency of the demonstrations enunciated in it, or dealing with the development of a specific doctrine over a long period of time, do not appear too sinful after all, in the case of mathematics. We have seen informed histories of the theory of uniform convergence in Weierstrass, or of the development of the integral calculus from Archimedes to Lebesgue, for example. As Mark Bevir observed, and his words seem to fit particularly well in the case of the history of the exact sciences, an historian "may legitimately be more interested in texts as expressions of meaning embodying beliefs than as linguistic actions embodying illocutionary intentions."[7]

I make no claim to methodological exclusiveness in my paper. In advocating a humanist historicism inspired by Skinner, I keep distance from any attempt to impose one single method in our discipline, the history of mathematics, which thrives because of the fact that it is practiced by scholars equipped with a variety of backgrounds and who adopt different approaches. The logic of a mathematical proof, or the techniques for the solution of a geometrical problem, can be studied as independent of the cultural context in which they were conceived, because mathematics possesses a notable stability through time and robustness when communicated between different cultures. Philosophers, since Plato's times, have

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5 Quentin Skinner, 'Meaning and Understanding in the History of Ideas', in *Visions of Politics*, pp. 57-89 (on p. 59) [originally published in *History and Theory*, 8 (1969), pp. 3-53]. In his own idiosyncratic way, Skinner of course was repeating themes that often recur in the methodological debate over the aims of historical research. Indeed, in 1754 Samuel Johnson was administering very much the same lesson to Thomas Warton in his writing: "The Reason why the authors which are yet read of the Sixteenth Century are so little understood is that they are read alone, and no help is borrowed from those who lived with them or before them". Samuel Johnson to Thomas Warton, July 16 1754, in *The Letters of Samuel Johnson*, ed. B. Redford (Princeton: Princeton University Press, 1992-4), vol. 1, p. 81.

6 John Locke, *Two Treatises of Government*, a critical edition with an introduction and apparatus criticus by Peter Laslett (Cambridge: Cambridge University Press, 1970).

7 This is how Sami Syrjämäki defines Bevir's position in 'Mark Bevir on Skinner and the Myth of Coherence,' *Intellectual History Review* 21.1 (2011), pp. 15-26 (on p. 17).

found it difficult to explain the source of this feature of mathematical thought. It is this feature – let us call it the "portability" of mathematics – that makes the technical debate on the cogency of proofs developed even millennia ago legitimate, informative, and fascinating. Yet, it is a different approach that I will be advocating today.

Let me briefly expand on Skinner's methodology. I shall refer to his seminal paper of 1969 entitled "Meaning and Understanding in the History of Ideas."[8] Here we find a sustained attack addressed against the mythology of doctrines that we have just considered – i.e., the anachronistic search for a doctrine, a systematic reply to our questions, in each of the great classics. We also find a scathing critique of other mythologies, among which the mythology of coherence and the mythology of *prolepsis* are of particular interest for us.[9] All are historiographic diseases, according to Skinner, which infect the history of political thought. All, we may add, are natural assumptions in the historiography – sometimes a high-quality historiography – of the exact sciences.

The mythology of coherence consists in conceiving historical criticism as an attempt to "supply the texts [written by classic writers] with the coherence they may appear to lack," when it is often the case that classic writers, such as Richard Hooker in *Laws*, are "not altogether consistent, or even fail to give any systematic account of their belief" (p. 67). The temptation to impose an "inner coherence" which it is the duty of the interpreter to reveal (p. 70), is "exacerbated" by lack of interest for the "proper emphasis and tone of a work," that is for what I would like to call the "voice" of an author, the style in which he writes: something which can be extremely revealing of the author's intentions in writing and communicating, provided the linguistic conventions of his or her age are properly understood.

The third historiographic delusion, after the mythologies of doctrines and of coherence, is the mythology of *prolepsis*, which "we are prone to generate when we are more interested in the retrospective significance of a given episode than in its meaning for the agent at the time" (p. 73). Such mythology comes in spades in the history of mathematics. When, for example, we talk about Descartes' discovery of "analytic geometry" or

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8 Quentin Skinner, 'Meaning and Understanding in the History of Ideas', in *Visions of Politics*, pp. 57-89 [originally published in *History and Theory*, 8 (1969), pp. 3-53].

9 I will not discuss the mythology of parochialism.

Newton's invention of "the calculus," aren't we projecting on the mathematical practices of these two giants of the seventeenth century interests that inform our teaching and research practices as 21st-century mathematicians? Such *prolepsis* is immediately revealed by our usage of terms such as "analytic geometry" and "calculus" that were never employed by Descartes and Newton, whose discoveries were referred to by their contemporaries as "common analysis" and "methods of series and fluxions" respectively.

I am aware that my attempt to relate my own work to Skinner's views on historical method cannot be taken in a very strict and philosophically engaged sense. The history of mathematics has its own requirements, dictated by the discipline it studies; as I said above, mathematics produces linguistic utterances that show a remarkable stability over time and that can travel almost intact through different cultures. This said, I think that Skinner's pronouncements can be inspirational for historians of mathematics: they can inspire new research projects and point to questions that historians of mathematics have ignored or underestimated. My paper, indeed, should be understood as a philosophically low-tech reflection, inspired by Skinner, on the craft of the history of mathematics carried out by one of its practitioners.

It is my conviction that Skinner's lesson, which so profoundly transformed the history of political thought, and had a notable influence on the history of science as well, can still be fruitfully approached by historians of mathematics. The case study I will propose to you is that of Isaac Newton. In the second part of my talk I will focus on the publishing practices for mathematical discoveries that Newton adopted in the 1670s and early1680s. I hope I will be able to convince you that it is fruitful to regard his mathematical work, even his masterpiece of 1687, the *Principia*, as lacking coherence, as fractured and stratified, and thus as revealing tensions that tell us something fascinating about Newton's intentions, his agenda and his anxieties as a mathematical author. In order to perceive Newton's own voice, the "proper emphasis and tone of [his] work," we need to place his mathematical practice into its linguistic context: we need to avoid projecting on Newton's mathematical texts – through what we might call *prolepsis* – disciplinary boundaries, questions, and terminologies that are our own. We need to raise the question of what Newton was "doing in saying what he

said" (p. 85), since his actions vis-à-vis his contemporaries reveal the relationship he wished to establish between himself as a mathematical author and his readers and acolytes. Indeed, again following Skinner, in order to have access to the author's intentions we need to pay attention to "what he may have been doing in presenting his doctrine in the precise form in which he chose to present it." (p. 83)

2.

Newton began his career as a creative mathematician in the winter of 1664 when, drawing inspiration from Wallis's *Arithmetica infinitorum*, he achieved, by sheer guesswork, the binomial series for fractional exponents. As historians of mathematics appreciate very well, this fundamental result was not proved until much later: Newton's path of discovery was based on the unproven assumption that a general form of the coefficients of the binomial expansion valid for positive integer exponents could be extrapolated for negative exponents and interpolated for fractional ones. A lucky guess. During his *annus mirabilis*, 1665, he further delved into unknown territory. One of his main objectives was to develop a method for studying the properties of mechanical – we would say "transcendental" – curves that he conceived of as traced by motion. In this field he often defined curves as generated by tracing-instruments (*organa*), but he proved also to be a master in representing curves by means of algebra. Infinite series allowed him to represent transcendental curves, such as the logarithmic curve, but also to calculate curvilinear areas, for example the area of a circle sector, or the arch length of a curve, for example that of the ellipse. What proved crucial in this research was the use of infinitely small magnitudes, which a few years later Newton dubbed "moments of fluent magnitudes," meaning that a geometrical magnitude varying continuously in time, a "fluent," will acquire a "moment" in an infinitesimal interval of time, an infinitesimal variation. Mathematicians in the 1660s had not yet learned how to justify the use of infinite series, sums with an infinite number of terms, and infinitesimals. The idea that such techniques could be reframed in terms of the method of exhaustion attributed to Archimedes, and that they were therefore safe, was widely held, but practitioners of the infinite and the infinitesimal – as in the case of Wallis with Fermat – had to withstand the criticism

of those who despised the cavalier methods of the moderns and praised the beauty and certainty of those of the ancients.[10]

The first who came to know about the work of the young Newton was the Lucasian Professor Isaac Barrow, an outstanding mathematician who was researching into the properties of curves, which he conceived as generated by continuous motion. It is through Barrow's intermediation that a short manuscript tract entitled *De analysi per aequationes numero terminorum infinitas* was dispatched to London. The addressee was John Collins, a mathematical dilettante who made a living, albeit a modest one, out of his entrepreneurial activities in the field of mathematical book publishing. This sector was somewhat in crisis because of the depression in the print business caused by the Great Fire, but Collins managed to supervise the printing of several books, mostly related to algebra. In this discipline there was great need for updating what was available on the English market. For Newton, getting in touch with Collins meant having free access to a network of mathematical correspondents, both British and Continental, and to the bustling world of printers and booksellers active in the capital. Newton could not have been offered the option to print his method of series and fluxions in a more conspicuous and attractive way, yet nothing came out if it.[11]

The extant correspondence between Newton and Collins reveals much of Newton's changing approach to publishing mathematics in the period from 1669, the year in which he was elected Lucasian Professor in succession to Barrow, to late 1670. Collins had several proposals for Newton: for example, the *De analysi* could be printed together with some of Barrow's works whose publication he was attending.[12] While waiting for Newton's permission, Collins made copies of the *De analysi*, and we have good reasons to think that he circulated information about this youthful work by correspondence with British and Continental mathematicians. Reading the epistolary exchange between Newton and Collins leads the historian to follow a zigzag path, whereby Newton at first seems close to accepting

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10 For an overview, I refer the reader to my *Isaac Newton on Mathematical Certainty and Method* (Cambridge (Mass.): MIT Press, 2009), pp. 3-17.

11 *Ibid.,* pp. 339-342.

12 Collins to Newton (12 October 1678) in Isaac Newton, *The Correspondence of Isaac Newton* (Cambridge: Cambridge University Press, 1959-77), vol. 2, pp. 286-7. See Mordechai Feingold, *Before Newton: the Life and Times of Isaac Barrow* (Cambridge: Cambridge University Press), p. 109, p. 167n.

Collins' invitations to print the *De analysi*, or even to dispatch him a fresh, more extensive treatise in which he had "newly methodized" his discoveries; but then – in matter of weeks – we find him withdrawing his promise, much to Collins' frustration. As Collins wrote to James Gregory in June 1675: "Mr Newton intends not to publish anything, as he affirmed to me, but intends to give in his lectures yearly to the publick library."[13] In this case Newton's deposited lectures could be accessed by members of the university.[14] But even this less ambitious project seemed to remain stillborn, as Collins learned from Newton in September 1676: "though about 5 years agoe I wrote a discourse in wch I explained ye doctrine of infinite aequations, yet I have not hitherto read it but keep it by me."[15] Much to Collins' frustration, as time went by, Newton again and again stated his reluctance to print his mathematics. The passage from another letter to Gregory is famous: "both he [Newton] and Dr Barrow – wrote an alarmed Collins – [are] beginning to thinke mathematicall Speculations to grow at least nice and dry, if not somewhat barren."[16] By the mid-1670s, Newton was quite adamant in not allowing his mathematical jewels to escape from his hands. To the few lucky ones who had corresponded with him on mathematical subjects and who had had access to his manuscripts he ordered silence and secrecy. In October 1676 he wrote to Henry Oldenburg, the secretary of the Royal Society who, after Collins, enjoyed Newton's overtures on mathematics: "Pray let none of my mathematical papers be printed without my special licence."[17] Which were the origins of Newton's anxieties in printing his mathematical discoveries?[18]

A good place to start from is the only publication project suggested by Collins and Barrow that Newton began to undertake – apparently in earnest – in 1670, before putting it aside as an unfinished draft: the edition of Gerard Kinckhuysen's *Algebra*, a Latin translation of a Dutch treatise carried out by Nicolaus Mercator. Newton accepted to provide annotations

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13 Collins to J. Gregory (29 June 1675) in *David Gregory, Isaac Newton and Their Circle: Extracts from David Gregory's Memoranda 1677-1708* (Oxford: Printed for the editor, 1937), p. 310.

14 Collins to J. Gregory (24 December 1670): "Mr Barrow told me the Mathematick Lecturer there is obliged either to print or put 9 Lectures yearly in Manuscript into the publick Library, whence Coppies of them might be transcribed." Newton, *Correspondence*, vol. 1, p. 54.

15 Newton to Collins (5 September 1676) in Newton, *Correspondence*, vol. 2, p. 95.

16 Collins to J. Gregory (19 October 1675) in Newton, *Correspondence*, vol. 2, p. 356.

17 Newton to Oldenburg (26 October 1676) in Newton, *Correspondence*, vol. 2, p. 163.

18 As his edition of Varenius's *Geographia* (1672) testifies, Newton at the time was not averse to entering into the print business.

that would improve and update Kinckhuysen's work. As we read Newton's annotations to Kinckhuysen, we immediately perceive the hand of the great algebraist at work.[19] But at the turn of the 1670s, Newton was beginning to ponder the relationships between algebra and geometry.[20]

On this issue, Huygens's *Horologium*, which landed as a presentation copy on Newton's table in 1673, exerted a lasting influence. There are good reasons to believe that, until his last days, Newton never ceased to recommend Huygens's mathematical method as a model. The Dutch polymath showed him how one could carry out cutting-edge research in pure and mixed mathematics by means that were purely geometrical, without any help of equations, infinite series, or infinitesimals. The cycloid, a daunting transcendental curve for mid-17th-century mathematicians, was tamed with elegance in the *Horologium*, and put to good use in the study of pendulum motion.

By the mid-1670s Newton, having set aside the sloppy treatise by Kinckhuysen, turned to the distant past of mathematical development. He began reading the works of Greek geometers, especially the late compilation of the Alexandrian mathematician Pappus. His ruminations on the comparison between algebra and geometry, occasioned by the project of annotating Kinckhuysen, developed into admiration for the geometrical way. Newton reached the conclusion that the mathematical methods of the Ancients were superior, as far as beauty, elegance and conciseness were concerned, to modern ones. Over time, this belief was to take stronger and stronger roots in his mind.

Newton's admiration for the Ancients went hand in hand with his anti-Cartesianism. Newton believed that Descartes, whose mechanistic philosophy he abhorred, had introduced a cumbersome symbolic method into mathematics; and this is what had caused him "nausea." Newton contrasted the "tediousness" of algebra to the elegance and beauty of Greek geometry, as revealed in the works of Euclid and Apollonius. He had come to share the then common view that the Ancients possessed a hidden geometrical method of discovery superior to the algebraic one. This line of research

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19 See Newton's text and D. T. Whiteside's commentary in Isaac Newton, *The Mathematical Papers of Isaac Newton*, edited by D. T. Whiteside, 8 vols. (Cambridge: Cambridge University Press, 1967-1981), vol. 2, pp. 277-447.

20 See, for example, the Addendum to *De Methodis* in Newton, *Mathematical Papers*, 3, pp. 328-53.

enabled Newton to come up with interesting results in projective geometry, in the footsteps of Desargues, Pascal and de la Hire.[21] Newton also attempted to reformulate his youthful method of series and fluxions in geometric terms in order to render it more compatible with the methods of the Ancients. A theory of limits that has been read as an anticipation of the ideas introduced by Cauchy and Riemann in the nineteenth century was developed by Newton in this context.[22]

It is interesting to note that Newton's fascination with Greek geometry and his disparaging attitude towards modern algebra resonate with the conviction endorsed at least from the mid-1680s that the Ancients possessed a superior knowledge of Nature and God. Newton contrasted this with the philosophy and religion of "recent men," as epitomized by Descartes, the modern philosopher *par excellence*, who had dared to cast a hyperbolic doubt on all past knowledge in order to formulate a new philosophy on completely novel bases. Similarly, Descartes the mathematician had dared to state in the *Géométrie* that the ancient geometers did not possess a proper method, otherwise – he surmised – they would have written treatises much shorter than Pappus's *Collectiones*. Newton aimed to profile himself as an heir to the geometrical school of Alexandria, not as a continuator of the Cartesian methods taught in Leiden.

Like many of his contemporaries, Newton believed that the history of mankind was that of a regress from perfection to corruption, and that it was necessary to restore a pristine, lost knowledge. Newton the theologian and philosopher gazed at the past with as much admiration as Newton the mathematician. Yet, in his youthful studies Newton had relied upon innovative symbolic and algebraic methods such as those of Descartes and Wallis. By the mid-1670s the tension between modern mathematical practice and ancient exemplars had engendered a feeling of anxiety in Newton's mind which might partly explain his refusal to print his method of series and fluxions.

From the mid-1670s down to the early 1690s, Newton tended to use the register of print publication for demonstrative geometry, and that of scribal publication (manuscript circulation and correspondence) for heuristic algebra. As he himself stated, according to David Gregory's *memorandum*

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21 Guicciardini, *Isaac Newton on Mathematical Certainty*, pp.79-107.

22 *Ibid.,* pp. 213-32. Of course, such a reading is an example of prolepsis.

of 1694: "algebra is fit enough to find out, but entirely unfit to consign to writing and commit to posterity."[23] Newton was proud of his symbolic mathematical discoveries: the binomial expansion, the method of infinite series, the algorithm for calculating tangents and curvatures, and especially the quadrature techniques that allowed him to calculate the area subtended by any known curve, as he proudly announced to Collins. He could circulate these results, in a controlled way, via manuscript exchanges and correspondence, as he did in 1676 with two well-studied, in part elliptical but nonetheless highly informative letters addressed to Leibniz via Oldenburg.

Yet, printing those heuristic methods would have aligned Newton with modern mathematicians who had to withstand criticism from the defenders of the rigor and certainty of classic geometry. Such a polemic would have been lethal for Newton, who wished to be identified as an heir to the classical tradition rather than a follower of the innovators. Were not his acolytes in the early eighteenth century referring to him as "our great discoverer and restorer"? Further, let us also take into consideration the fact that in the 1670s Newton engaged in a fierce polemic with some fellows of the Royal Society, and especially Robert Hooke, on the new theory of light. This polemic had also to do with the role of mathematics in natural philosophy. Newton claimed that his natural philosophy was certain because it was mathematical. But in order to profile himself as the philosopher who could surpass the kind of mitigated probabilism of experimental science defended in texts such as Hooke's *Micrographia* via the use of mathematics, Newton had to avoid becoming embroiled in a polemic concerning the certainty of mathematical methods, as had happened to Wallis, whose methods stood so much at the root of Newton's use of infinite series.[24]

When Newton composed the *Principia* he wrote his opus magnum in geometrical style, yet only with difficulty could he conceal the use of symbolical methods. Geometry was not powerful enough to tackle the complex mathematical issues of gravitation theory. The historian of mathematics should view the *Principia* as a text that reveals tensions and contradictions between the methodological declarations in favour of geometry

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[23] "Algebram nostram speciosam esse ad inveniendum aptam satis at literis posterisque consignandum prorsus ineptam." University Library Edinburgh MS Gregory C42, translated by D. T. Whiteside in Newton, *Mathematical Papers*, vol. 7, p. 196-7. See also *Correspondence*, vol. 3, p. 385.

[24] Guicciardini, *Isaac Newton on Mathematical Certainty*, pp. 19-29.

and against the use of infinitesimals with which it begins (in the Preface to the author and in Section 1) and the mathematical practice that in many advanced propositions is essentially symbolic and based on infinitesimals.[25] The complex, stratified nature of the *Philosophiae Naturalis Principia Mathematica* is in part explained by these anxieties of Newton's over method and by his desire to write a work reminiscent of ancient exemplars, patterned on Huygens's model, anti-Cartesian in form as much as in content.

With time, Newton was to redefine his publication strategy to meet the challenges posed by other mathematicians. He disliked printing symbolic language, but at the same time was proud of the results obtained in his youth. Since other mathematicians, both in Britain (David Gregory and John Craig) and on the Continent (Tschirnhaus, Leibniz, Jacob and Johann Bernoulli), were advancing techniques equivalent to his, Newton had to allow more and more of his methods to become accessible to others.

Studying Newton's publishing strategies is a challenging task. It might be too much to say that the above-mentioned extra-mathematical factors *caused* Newton to reject Collins's publication proposals in the 1670s or to avoid the explicit use of algebraic methods in the *Principia*. I would also like to recall that the extra-mathematical factors I referred to above are anti-Cartesianism, admiration for the ancients, and a desire to overturn the mitigated skepticism in vogue within the Royal Society. Rather, one might say that it was a number of philosophical ideas and political concerns that propelled Newton's polemical reading of Descartes and the "modern philosophers"; that these concerns together with Newton's tense dealings with the Royal Society – like force vectors – *pointed* his mind *away* from the prospect of committing to print his symbolical, rather Cartesian, modern and uncertain mathematical discoveries. An important element in my defence of a kind of humanist historicism inspired by Skinner is the rejection of any attempt to define Newton's thought and behavior as

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[25] Such stylistic choice did not help Newton's readers, who often complained about the lacunae and excessive conciseness of Newton's *opus magnum*. In order to tackle the mathematization of the System of the World, Newton had indeed to deploy quadrature techniques (i.e. integrations), but in the printed text he only provided a geometrical construction of the results he obtained by the squaring of curves, not the techniques themselves. The latter he revealed only to his acolytes via oral or epistolary exchange. See Niccolò Guicciardini, *Reading the Principia: The Debate on Newton's Mathematical Methods for Natural Philosophy from 1687 to 1736* (Cambridge: Cambridge University Press, 1999).

governed by causal laws.[26] Newton's approach to publication is far from being coherent, and the historical record does not afford us any simplistic description. Yet, his dealings with Collins, Oldenburg and later Halley during the publication stages of the *Principia* – as well as with Wallis, when he decided to let some of his method of fluxions to be printed in the latter's *Opera* – reveal something of his authorial strategies, of the way in which he wished to profile himself vis-à-vis his contemporaries, of the role he attributed to himself as a rediscoverer of ancient exemplars and the defender of an anti-Baconian way of envisaging the relationship between mathematics and natural philosophy. But, ultimately, we must accept that the late publication of Newton's early mathematical writings is also the result of contingencies such as the depression in printing caused by the Great Fire of London (1666), Collins' death (1683), and the publication conventions in England under the Restoration, an age in which scribal publication flourished.

My objective as a historian has been to reveal the voice of a mathematician for whom mathematics was a discipline that played a considerably different role from the one we are accustomed to attribute to it nowadays. This is an author whose questions – concerning the elegance and antiquity of methods – are specific to his cultural context, and who framed replies to these questions that sometimes lack systematicity and coherence, or even often stand in contradiction to his mathematical practices. In my work I have described the style of Newton's published and unpublished mathematical writings, and his publication and authorial strategies, as the expression of philosophical, religious and political agendas. I have sought to study Newton's mathematical texts as revealing – even in their more technical details at times – Newton's intentions *in writing* mathematics, as revealing his response to anxieties that concern the politics and philosophy of his own times, rather than as anticipating our own versions of Newtonian dynamics and calculus. Indeed, in order to achieve my aim I have had to pay attention to Newton's activities, to "what he was doing in saying," to his anti-Cartesian philosophical agendas and his polemical engagement with the ideology in vogue in the Royal Society in the 1670s. In doing so, I might dare to say that I have sought to write the kind of cultural history delineated long ago by Skinner.

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[26] See Timothy Stanton, "Logic, Language and Legitimation in the History of Ideas: a Brief Survey of Bevir and Skinner," *Intellectual History Review* 21(1) 2011, pp. 71-84 (on p. 75).

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Skinner's approach may be described as one which "takes the historical character of texts as fundamental and understands these, in the last instance, as actions whose meaning is to be sought in the intentions of their authors in performing them."[27]

Two objections can be raised against such approach to history. The first is specific to the history of mathematics, and focuses on the importance of developments internal to mathematics itself that can be accounted for quite independently from the context I have been dealing with throughout this talk. The second concerns the opacity of the author's intentions. Historical records alone would not allow us to acquire any reliable information about the intentional dimension of past actors' actions and writings. Newton's voice would be irremediably lost, it would no longer be audible.

It is easy, I dare say, to reply to the first objection. I am very much in favour of internalist readings of the history of mathematics and have tried myself to contribute to this research area. Indeed, a technical, internalist reading of Newton's philosophy, physics, theology, alchemy and mathematics is vital for the agenda I have set myself as a historian. Newton's "voice" can be reconstructed only if we take into consideration what was so important for him: the solution of the daunting problems he faced in these diverse areas. And those who have taken the technicalities of Newton's natural philosophy, mathematics, and theology seriously have made a contribution to the historical understanding of Newton's thought that cannot be overestimated. Historians of Newtonian science are devoting their attention to the works of an historical actor for whom topics such as the following were of paramount importance: the efficiency of a measuring device, the reading and analysis of numerical data, the resolution of problems via algebraic equations, the understanding of the role and limitations of transduction, the concepts of space and time, the nature of causality and gravitation, the apocryphal status of specific passages of Holy Scripture.[28] We will never understand Newton's "illocutionary intentions" if we do not commit

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[27] J. Dunn, 'The History of Political Theory,' in *The History of Political Theory and Other Essays* (Cambridge: Cambridge University Press, 1996), 11-38 (on p. 19) and Stanton, 'Logic, Language and Legitimation in the History of Ideas,' *Intellectual History Review* 21.1 (2011), pp. 71-84 (on p. 71).

[28] This list is incomplete, of course.

ourselves to facing the technicalities implied by these problems, and that compelled Newton to think about them so intensely and so compellingly.[29]

As Brendan Larvor puts it (perhaps attributing to mathematical development a somewhat excessive progressive inner force):

> Mathematical development may be distorted by ideological interference, stymied by academic rivalries or halted by the fall of empires. Nevertheless, […] the direction of mathematical development and the response of mathematics to external stimuli are both best explained by factors proper to mathematics itself.[30]

You cannot trisect an angle by straightedge and compass: advances in the study of conic sections were also searched for because these curves could be put to good use to solve that problem. You cannot square the circle via the use of polynomial equations in Cartesian coordinates, the algebraic tool of Descartes' *Géométrie*. Torricelli and Wallis, who certainly lived in quite different cultural contexts – the former being an Italian with a Jesuit education and the secretary of a high Catholic prelate, the latter an erudite English professor who served as a member of the Westminster Assembly – proposed similar methods based on infinitesimals and infinite summations or products enabling the quadrature of curvilinear surfaces. I need no convincing of the fact that any good historian of mathematics must delve into technical details and give proper attention to difficulties, advances and failures that can be accounted for in terms to a large extent independent of any cultural context, because mathematical methods possess what I called "portability": for some philosophers mathematical truths can even be said to be "eternal." The square root of 2 is not a rational number, no matter whether you are an adept of a Pythagorean gnostic sect in 2nd-century AD southern Italy, or a contributor to the French *Encyclopédie*. I have a great admiration for historians of science, mathematics and philosophy who set

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[29] An internalistic study of the mathematical problems and solutions devised by historical actors can be very fruitful in the context of an approach to the history of mathematics that adopts intellectual history as a paradigm. This is because the agency of historically situated mathematicians was very much driven by those problems and solutions. Problems and solutions that often markedly differ from the ones considered by mathematicians today. My favourite example is Bos' study of Descartes' *Géométrie* in *Redefining Geometrical Exactness: Descartes' Transformation of the Early Modern Concept of Construction* (Springer, 2001).

[30] B. Larvor, 'What is a dialectical philosophy of mathematics?' *Philos. Math.* (3) 9 (2) (2001), pp. 212–29 (on p. 215).

themselves the task of searching for coherence in Newton's textual pronouncements. This is a very important, indeed vital task for our discipline. I would even say that the pursuing of this task is the backbone of the history of philosophy and the history of science.

Such a task, however, cannot be pursued beyond certain limits. There is a moment in which an historian in the rational reconstruction of the meaning of a text has to recognize that there are fractures, tensions, contradictions that it is fruitful to recognize; and such recognition can be a fruitful act, since it might be the beginning of a reading of the text that reveals the historical actor's intentions. An excessive concern with coherence, an attitude that emerges especially in the study of the "great classics" of science, leads one to depart from the intended illocutionary force of past actors' uttrances.[31] On the other hand, Mark Bevir, one of the sharpest among Skinner's critics, is correct in claiming that "a focus on illocutionary intentions has the effect of undermining a concern with the coherence of an author's work," because the "coherence constraints on illocutionary intentions are weaker than the coherence constraints on beliefs."[32]

But I have still to face the second, much more daunting objection: aren't we, in our attempt to eschew Skinner's mythologies, led into the hermeneutic trap of "intentional fallacy"? To put it bluntly, it might be claimed that motives and intentions are simply impossible to recover: they are private entities to which no one can gain access, as Wimsatt and Beardsley claimed in the 1950s.[33] I will not even attempt to face the intimidating philosophical problems raised by "New Criticism" and its critics.[34] Neither will

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31 As Syrjämäki puts it: "According to Skinner, in many, or even in most, cases, it would be a good idea to take contradictions as contradictions, or at least to consider it possible that the author is being inconsistent and not to ask automatically: how is the (apparent) contradiction(s) to be explained so that it would be provide a better understanding of the coherent theory?" Syrjämäki, 'Mark Bevir on Skinner,' p. 21.

32 M. Bevir, 'Mind and Method in the History of Ideas,' *History and Theory*, 36.2 (1997), pp. 167-89 (on p. 168). Syrjämäki, 'Mark Bevir on Skinner,' p. 16.

33 Wimsatt, William K. and Monroe C. Beardsley, 'The Intentional Fallacy,' *Sewanee Review*, 54 (1946), pp. 468-88. Revised and republished in *The Verbal Icon: Studies in the Meaning of Poetry* (University of Kentucky Press, 1954), pp. 3-18.

34 For Skinner's reply to Wimsatt and Beardsley, see Q. Skinner, 'Motives, intentions and interpretation,' in *Visions of Politics, Volume 1: Regarding Method*, pp. 90-102. Skinner's reference to the second Wittgenstein and Austin is important here. The author's motives and intentionality Skinner is referring to are not private and individual: the mind's private dimension might be inaccessible, especially that of a past actor. They are made accessible and public by the author's utterances and actions that can be interpreted when one is aware of the conventions shared by the author and her contemporaries.

I broach the debate concerning weak intentionalism that was carried out by Vivienne Brown a decade ago.[35] And I will ignore the distinctions between substantive and formal intentionalism.[36] After all: in the opening of my lecture I promised a low-tech philosophical reflection! To say that the intentions of past actors are utterly inaccessible seems, by instinct, excessive to me. I will not push my confidence in accessing past actors' intentions as far as Collingwood goes, claiming that historical knowledge consists in the "re-enactment in the historian's mind of the thought whose history he is studying."[37] However, to state that the Athenians and Plateans at Marathon were fighting in defence of the peculiar freedom guaranteed by their *poleis* menaced by Persian absolutism, and that Herodotus' writings embody such ideals, does not sound too far from the truth to me, even though at school this example played a rhetorical role in a deliberate attempt at political indoctrination in support of the Eurocentric values of parliamentary republican democracy, values alien to the mindset of a Miltiades. These values were imposed on me and my schoolmates through a proleptic and propagandistic reading of Greek history – a mythology, if you like. A mythology, for sure, less dangerous and nasty than the one suffered by my parents before the war in Mussolini's Italy, but equally misleading from a historical point of view.

When we probe past agents' motivations we rely upon the material traces they have left: pictures, letters, alchemical laboratory notes, the memoranda of diarists and acolytes, marginalia, legal documents, and so on and so forth. Traces that inform, but also distort, our image of the past, since they have been selected deliberately, or by chance: papers may have been interpolated by copyists, burnt by fire, or used by monks for packing goods or over-writing prayers books. Those who are in search of a voice coming from the past will try to find it in these scant traces by placing historical sources in the context constituted by the actions and interactions of past agents. It is not an impossible task, but, admittedly, it is a desperately diffi-

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35 Vivienne Brown, 'On some Problems with Weak Intentionalism for Intellectual History,' *History and Theory,* 41 (2002), pp. 198-208; 'Historical Interpretation, Intentionalism and Philosophy of Mind,' *Journal of the Philosophy of History*, 1 (2007), pp. 25-62.

36 A good place to start from in order to appreciate the dense philosophical debate originated by Skinner's methodological writings is *Meaning and Context: Quentin Skinner and His Critics*, edited and introduced by James Tully (Polity Press, 1988).

37 The historian "must be able to think over again for himself the thought whose expression he is trying to interpret." Robin G. Collingwood, *An Autobiography* (Oxford University Press, 1939), p. 111-12.

cult one. I am convinced, however, that notwithstanding the high probability of failure, this task cannot be ignored: it is a battle worth fighting even if defeat is the most probable outcome. After all, so much of culture as I mean and cherish it depends upon the fact that this task is tackled. To elaborate on Jonathan Clark's metaphor in his splendid book on *English Society 1660-1832*, we might say that we look at past actors through the glass of our anachronisms, thus rendering their actions anticipations of the present. Our glass cannot but be dirtied by the impurities of our own preconceptions. We see the shadowy figures of our predecessors, we turn and tilt the glass, we try to eliminate impurities that distort the image. The glass of our doctrines and prolepses will be always there: we are 21st-century actors ourselves, and we cannot but use the optical instruments provided by our own times and by the hermeneutic tradition we belong to. The important thing is that, by manipulating the glass of historical research too clumsily, we do not "turn that glass into a mirror", and end up seeing our face reflected in it.[38]

[38] J.C.D. Clark, *English society 1660–1832* (Cambridge: Cambridge University Press, 2000), p. 13.

Laudatio for Prof. dr. Michel De Vroey

Freddy Heylen
Faculty of Economics and Business Administration

Michel De Vroey is a highly respected historian of economic thought, both in Belgium and abroad, with a major focus on the history of macroeconomics. His permanent place of work is the research institute IRES at the Université catholique de Louvain (UCL). He has frequently, however, been a visiting professor in many European and North American universities (see the box below).

Work

Michel De Vroey wrote about 50 to 60 articles in national and most so international journals or edited books. Most of these articles are separate pieces of reflection, developments of particular ideas, very much to the point. All, however, contributed to his lifetime goal, which is to write the history of macroeconomics. Two great books are the result of all these separate pieces of reflection. A third forthcoming book, to be published by Cambridge University Press, will be his lifetime achievement.

Macroeconomics began in the 1930s when the belief in the self-adjusting, self-correcting capacity of the market economy was seriously shaken. So was the belief in the economic theory of those days. Macroeconomics began with Keynes. A major element in Keynes's programme was to demonstrate the existence of mass involuntary unemployment, not due to excessive wages, but due to causes outside the labor market, and to justify that demand stimulus by the government could be a remedy. After Keynes many authors with Keynesian inspiration have tried to realize this programme. Hicks and Modigliani constructed and recasted the IS-LM model. Clower,

Benassy and Malinvaud among others developed the so-called disequilibrium models. Much later came so-called new Keynesian economists who gave strong micro foundations to different types of market failure and involuntary unemployment (or underemployment) with the implicit contract theory, the insider-outsider model, the efficiency wage model, etc. In between there was the anti-Keynesian attack led by very influential people like Milton Friedman (monetarism) and Robert Lucas (new-classical macro), economists who took (take) it for granted that the market system functions well and who called 'involuntary unemployment' a theoretical construct of Keynes, but not a fact where economist should spend time trying to explain. In his first book "Involuntary Unemployment" (Routledge, 2004) Michel De Vroey describes the development of macroeconomics as centered around the concept of involuntary unemployment.

His second book (Dalloz, 2009) and a forthcoming third book (Cambridge University Press) focus on Keynes and Lucas, and their impact on the whole development of macroeconomics. Both books discuss the history of macroeconomics, since Keynes until today, be it in full awareness of the state of economics before Keynes. Here I refer to Michel De Vroey's contributions on Walras and Marshall.

Quality

Reading Michel De Vroey's work one can only be struck by its very high quality, which makes the Sarton Medal so very well deserved. Allow me to emphasize a number of points, and in between a lesson that I would draw from them.

A first important quality is its conciseness in statements, always to the point. As a reader of Michel De Vroey you never wonder 'Why does the author make all these detours?' Among historians, one does not find this frequently. Notwithstanding its conciseness, Michel's work is very rich. He describes the history of macroeconomics along more axes than most people do. One axis is the ideological position that most macroeconomists have. Do they defend the free market, or rather emphasize the need for stabilisation policies by the government? Another is the conceptual apparatus that macroeconomists apply, be it either Marshallian or Walrasian. The latter distinction is related to whether economists rather work in partial equilibrium (Marshall) or in general equilibrium (Walras). It is also about the

purpose of economic theory: should it be focused on actual problems and facts to be understood (Marshall), or should that not be the case and should economists rather be interested in matters of principle (Walras)? Related to this is the role that people assign to models and mathematics in economics. Should it be limited (Marshall), or should mathematical economics rather be the future of the discipline (Walras)? Reading Michel's work, you learn macroeconomics, you learn about how from time to time macroeconomics was challenged, how new roads were taken, how it developed… You learn its strengths and weaknesses, you get a broad and critical perspective.

Work like this is of crucial importance. Seeing today's very bright young macroeconomists, masters in building and estimating so-called dynamic stochastic general equilibrium (DSGE) models, one may wonder… Do they still know where all this comes from? Do they know the choices that macroeconomists have made in the past? Do they have this broad historical and critical perspective? I am not sure. In the age of widespread division of labour, with everyone being a specialist in narrow fields, historians of economics therefore have a very important role to play. That role is to bring a broad and critical perspective to the theorists. Training in the history of economic thought is crucial. To use one of Michel De Vroey's metaphors: we do not only need soldiers for the regiment, very well trained, masters of their weapons, physically strong, ready to walk… we hope that these soldiers also know why they walk in a particular direction, and that they are ready to go back or to reconsider their direction if necessary… This is important especially now. We again face in macroeconomics the situation where belief in current theory is shaken, belief in current DSGE models which basically exclude the possibility of malfunctioning market systems. We are again in a situation of rising mass unemployment in many countries. In that respect we are back in the situation that economists faced in the days of Keynes. Where do we go now? There could not be a better time for this Sarton Medal than today.

Let me emphasize another quality of Michel De Vroey's work, his neutrality. Macroeconomics is a politically-laden field. As I mentioned, some key players believe in free-markets and argue in favour of full economic liberalism (Friedman, Lucas). Others see market failure, and emphasize the need for an active role of the government to remedy (Keynesians). Most economists hold a firm standpoint in this divide. Michel

observes, but takes no side. He is like the art critic. He is not painting, but he is putting the painting by others in perspective. That – according to him – is the role of historians of economics. The artists can be ideologically driven, and actually often are, but the critic cannot.

There is a final point that I want to emphasize, given that we have so many students in the audience. This final point is the way in which professor De Vroey combines what we call the key duties of an academic. You do not find too many who are researchers with an excellent international reputation, but who are also great teachers. Michel De Vroey is. Most students are impressed by the high quality of his lectures, insightful and eloquent. They are impressed by the way in which he pushes students also to read the original work. It is clear that students benefit from his strong knowledge of macroeconomics and of the history of economics in general. PhD students value very highly his open-mindedness, his willingness to listen. Michel De Vroey is said to be tough to PhD students if necessary, but always careful and encouraging enough not to let him or her depress. Excellent.

Michel De Vroey – Short CV

Education

• 1965 B.A. Philosophy, Université catholique de Louvain
• 1973PhD Economics, Université catholique de Louvain
• 1970-71Graduate Fellow, University of California, Berkeley

Past and present positions

• 1969-77 Researcher Fonds de la Recherche Scientific (FNRS)
• 1973-83 Lecturer, Université catholique de Louvain
• 1983-...Professor, Université catholique de Louvain

Fields of interest

History of economic thought, in particular history of macroeconomics

Visiting positions: Université de Montréal, Université d'Orléans, Université de Paris XIII, Université de Nice, University of Massachusetts, Amherst, Université de Paris I-Panthéon-Sorbonne, Duke University, Université Paul Cézanne, Aix-en-Provence, Facultés Universitaires Saint-Louis, Brussels, Clemson University, University of British Columbia.

Main publications (published books)

- Involuntary Unemployment. The Elusive Quest for a Theory, Routledge, 2004.
- Keynes, Lucas, d'une macroéconomie à l'autre, Editions Dalloz, 2009.
- Twentieth Century Macroeconomic Theory: From Keynes to Lucas and Beyond, Cambridge University Press, forthcoming.

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What can civil society expect from theoretical macro?

Michel De Vroey [1]

Introduction

The question I wish to address in this lecture surely makes sense in the context of the time we are living in. For the last four years our economies are experiencing a deep recession, and, right or wrong, this experience has spilled over on the general judgment made about macroeconomics. In a nutshell, its reputation is at low ebb. Is this judgment justified? On a broader level, what can civil society expect from macroeconomic theory? Is the discussion triggered by this last question ridden with ambiguities and misunderstandings, and, if yes, of which nature? These are the questions that I want tackle. To do the job, I shall follow a historical thread starting with the rise of macroeconomics to end up with real business cycle macroeconomics.

I. The rise of macroeconomics

The sub-discipline of macroeconomics studies aggregate economic variables such as employment, output, the general price level, the interest rate, etc. It saw the light of day in the wake of WW II. Actually, it did not arise from scratch. Before, it existed under the name of monetary theory, and its concern was the study of how money, in particular the supply of money, had an impact on 'real' economic outcomes as studied by pure economics.

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The piece that, rightly enough, is considered its starting point is John Maynard Keynes's 1936 book, *The General Theory of Employment, Money and Interest*, in short the *General Theory*. Before writing it, Keynes was already a towering figure in the economic profession and a widely recognized expert on monetary matters. His main concern was practical policy until the outburst of the Great Depression with its mass unemployment, peaking above 20% in several countries. This dramatic event triggered Keynes to become concerned with high theory as he felt that the economic theory of the time, mainly Marshallian neoclassical theory, proved unable to come to grips with it. The 1930s were also a time where Russia was witnessing strong economic results to the effect that a possible electoral victory of parties leaning towards communism (or their taking power in more unorthodox ways) was a possibility that could not be discarded. In short, capitalism was in peril, both economically and politically, and Keynes realized that its survival implied important changes in its functioning. While these elements, beautifully expressed in the concluding chapter of the *General Theory*, were looming in the back of Keynes's mind, his endeavor was mainly theoretical. To him, the economic theory of the time was wanting and the policy conclusions that they reached were exactly the opposite of what should be done. This was the state of affairs he wanted to change. As aptly noted by Skidelsky, Keynes's biographer, this venture intertwined theory and persuasion:

> "Keynes understood that his theory had to be usable for politicians and administrators: easily applied, offering political dividends. But he also understood that, before he could win the political argument, he had to win the intellectual argument" (Skidelsky 1992, p. 344).

The main diagnosis about the crisis available to economists at the time was of 'Austrian' inspiration. The crisis, the story run, signaled a situation of overinvestment and misallocation of resources, a state of affairs that required a process of 'liquidation' for its solution, a real wage deflation on the one hand, and elimination of the firms that had engaged in wrong investment decisions on the other. Flexibility was thus the motto. The more flexible prices and wages were, the faster the liquidation process coming to an end and conditions for prosperity being re-established. However, when the depression kept its course without wages deflation failing to exert its proclaimed effect, economists started to waver about the virtues of laissez

faire and to wonder whether, this doctrine to the contrary notwithstanding, the state should engage more actively in the economy. Thus, economists were torn between the policy conclusions following from accepted theory and their guts feeling that another policy should be taken. Keynes's aim was to remove this contradiction by providing a theoretical argument in favor of the guts feeling. He did not mean a total overhaul of standard economic theory, but rather its emendation. The task he set himself was to demonstrate that the economy could be stuck in a state of equilibrium while also featuring the presence of involuntary unemployment, a notion that had no place in the lexicon of accepted economic theory. In a nutshell, his explanation was that involuntary unemployment resulted from a deficiency in aggregate demand, itself the result of insufficient investment. To chase the economy away from its involuntary unemployment equilibrium a policy of autonomous demand activation was needed.

Keynes's book got an enthusiastic reception, especially from young economists. Dissatisfied with the existing situation, they were crying for a new theory that would justify the abandonment of the laissez-faire doctrine. In this respect, Keynes's work delivered beautifully. As Axel Leijonhufvud said, it was received as a "liberating revelation" (1968, p. 31). Nevertheless, confusion over the central message of Keynes's book was great, even amongst his admirers. In effect, The *General Theory* was a complex book, hard to read and intertwining different types of arguments, developed at distinct levels of abstraction the compatibility of which was hardly obvious.

Progress came when Hicks succeeded in transforming Keynes's cryptic analysis into a simple system of three simultaneous equations. This is the renowned IS-LM model, the starting point of what can be called Keynesian macro, a paradigm that reigned over the profession for the twenty five years that followed the end of WWII.

The IS-LM started as an abstract model but under the stewardship of another great Keynesian economists, Lawrence Klein, it became transformed into an empirically testable model. The Klen-Goldberger model (1955) marked the start of a long chain of macroeconometric models to be used for making predictions as well as for assessing alternative policies.

These are the three stages – the *General Theory*, the IS-LM model and the birth of Keynesian econometric models – through which macroeconomics

came into existence as a new sub-discipline of economics. It soon thrived. The offspring of the Great Depression, its prominent aim was to highlight market failures and to vindicate that they could be remedied upon by state interventions. So, from the onset, it had a reformist flavor. From the 1950s onwards, Keynesian macroeconomics established itself as a new sub-discipline of economics. It was taken up both in universities and public institutions such as central banks.

II. Characterizing Keynesian macroeconomics

Let me now try to describe the main features of Keynesian macroeconomics in its theoretical rather than its empirical dimension. To this end, I shall use the following seven benchmarks: (1) macroeconomics overarching *explanandum;* (2) the place and role of the notions of equilibrium and disequilibrium; (3) the relative importance of supply and demand; (4) the main methodological rule; (5) the micro/macro relationship; (6) theory's accessibility to laymen; (7) the underpinning ideology.

1. The *overarching object of study* of Keynesian macro was departures of the economy from full employment. Although this was not the line that Keynes had favored, underemployment was viewed as the result of some sluggishness in the adjustment of wages. State interventions aiming at activating demand through fiscal policy or monetary activation was regarded as the remedy to such occurrences.
2. Keynesian theory mixed the notion of *equilibrium and disequilibrium*. The result that was strives at was one where the economy was in equilibrium, in the state of rest sense of the term, this going along with excess supply of labor, i.e. involuntary unemployment. That is, some agents are in a state of individual disequilibrium – they are unable to make their optimizing plan come through.
3. S*upply of and the demand for labor*. According to the Keynesian approach, variations in employment resulted from changes in aggregate demand. The underlying picture was that labor suppliers are passive, employment decisions being made unilaterally by firms. Moreover, this approach tended to consider the supply of labor and the labor force as the same thing, supposedly a fixed magnitude

4. *The central methodological principle* of Keynesian macro was external consistency. That is, theory was defined as consisting of propositions aiming at explaining reality. Models are good if they realistic. The prevailing intellectual mood was pragmatism. That several of the basic notions – involuntary unemployment, full employment, rigidity and sluggishness – were defined in a loose way, that the analysis focused on the short period cut off from the long period, that expectations received little attention and were conceived of as backwards-looking, all this was hardly considered harmful. Empirical models, the construction of which was often due to engineers rather than economists, were more data- than theory-constrained.

5. *Relationship between micro and macro*. Little attention was given to the process through which agents make their decisions. It is not that Keynesian economists were against what is now called microfoundations, i.e. the need to start the analysis of aggregate magnitudes from individual agents' optimal choices. Rather, they treaded Marshall's footsteps by considering that goal-oriented behavior as a matter of intention, not of performance, and by seeing no harm in starting the analysis from market functions without explicitly deriving these from individual decision-making. To paint with broad strokes, macroeconomists were lacking behind microeconomists in terms of the rigor of their analysis.

   As far as the relationship between the scientific communities of microeconomists and macroeconomists was concerned, it took the form of a partition of territory. Each of these two scientific communities had their own field of research and the good practice was not to look in the neighbor's garden. Keynesian theory was concerned with the study of the economy in the short period, where it supposedly featured market non-clearing and individual disequilibrium. In turn, the study of the long period, where the economy was supposed to experiment market clearing and individual equilibrium, was the domain of neoclassical theory. This consensus was called the *neoclassical synthesis* (i.e. the Keynesian/neoclassical synthesis) – actually, a rather inappropriate appellation since it designates the view that it is better to have the two fields developing separately rather than to try to integrate them.

At the time, neither microeconomists nor macroeconomists complained about this division within economics broadly understood. Two reasons may explain. First, it is true that Keynesian macro was premised on the view that the market system could be prone to market failures, and hence that laissez faire was the wrong policy to be taken. But this view was congruent with the general mood of the time about the role of government in society, a mood probably shared by microeconomists and due to the lasting impact of the Great Depression on people's minds. The second factor was that that the fine-tuning recipe associated with Keynesian theory seemed to be working and to have played an important role in the unprecedented economic prosperity of the time.

6. *Accessibility to laymen*. The result of this pragmatic attitude is that Keynesian macro theory was and remains simple to understand even by non-economists. Its level of technicity was low. It used a terminology close to that of newspapers and political discourses – for example, the idea that employment is decided unilaterally by firms that excess supply of labor is a directly observable occurrence.

7. *Associated ideology*. Macroeconomics is normative because its concern is the ideal way of organizing the economy in terms of efficiency and welfare. This is a subject about which people often hold prior judgments. Therefore, the field bears an ideological dimension, the 'ideology' term not being taken in a pejorative way. It just designates a given vision about economic governance. The vision most congenial to Keynesian macro can be branded 'mitigated liberalism'.[2] It defends the market system as being superior to a planning system without going to the full extent of advocating laissez faire.

Such are in my mind the central features of Keynesian macro. As I said, it reigned for a quarter of century after which it lost its grip over the profession. First, some macroeconomists ceased to adhere to the earlier partition

[2] It is intentionally that I have spoken of congeniality. A theory is a conceptual apparatus or, to use a metaphor, a language, that is, a set of syntax rules. In so far as one respects these, one can say whatever one wants. Because of the premises upon which it is built, Keynesian macroeconomics may well bent towards mitigated liberalism but this association is not automatic. The language can be forced away from it: models using the Keynesian apparatus can be constructed that end up vindicating laissez faire. The best example of this is Milton Friedman's work.

of territory between micro and macro. Taking micro principles more in earnest than their predecessors, they started to critically investigate the foundations of macro to discover that they were shaky. Second, the arising of stagflation in the 1970s was a phenomenon absent from the Keynesian radar. Many took it as the sign that demand activation failed to work; instead, it was argued, it generated a cumulative inflationary process. Third, as the remembrance of the Great Depression faded away, the defenders of a higher pitch of liberalism found themselves under more favorable winds.

III. The new classical revolution

Milton Friedman was an important character in the transformation of macro. When thinking of critics of Keynesian theory, it is his name that comes first to the mind. However, while it is true that he blazed the trail, the direction that was eventually taken was different from what he envisaged. In effect, Friedman's dislike of Keynesian theory was more a matter of disagreement about policy conclusions than methodology. In contrast, the economists treading his footsteps, Robert Lucas, Thomas Sargent and Robert Barro, were after a more radical breach from Keynesian macro. As Lucas was the leading figure in this move, I shall be concerned with him only.

Lucas and his companions started their career as applied macroeconomists. Their microeconomic background was stronger than that of most of their macroeconomists colleagues, what led them to become aware of the several weaknesses of the Keynesian paradigm. It also led them to refuse the neoclassical synthesis, the view that macro theory could be dispensed from abiding by the microfoundations requirement that prevailed in microeconomics. In a surprisingly short span of time, they were able to formulate and impose a new conception about how engaging in macroeconomic research. They called it 'new classical macro' to honor the tradition that Keynes wanted to dismiss.

This transition deserves to be called a 'scientific revolution' in the sense proposed by Thomas Kuhn referring to episodes in the history of a discipline where a series of unsolved puzzles pile up disturbing its normal development. This situation of unrest, of dissatisfaction with existing theory then triggers a drive to change the agenda, the conceptual toolbox

and the research methods in radical ways. All this is often accompanied by thundering declarations of war, a confrontation between younger and older generations of researchers, the rise of new stars in the profession, and the eclipse of the previous ones.

Like all scientific revolution, the new classical transformation was two-legged, with a criticism of the existing paradigm, on the one hand, and the construction of a new one, on the other. Lacking time to enter into the critical aspect, let me just say that Lucas argued (a) that Keynes did no deserve the 'great economist' stature usually ascribed to him; his main contribution, he claimed, was ideological and political and consisted of having helped to preserve capitalist economies from the socialist temptation; as far as theory was concerned, his contributions were minor; (b) that the central concepts presented by Keynes in the *General Theory*, involuntary unemployment and full employment in particular, were hollow; (c) that adaptive expectations were an inadequate way of representing expectations; (d) that, the result of their lack of microfoundations, Keynesian econometric models were of no help for comparing alternative policy measures (the famous 'Lucas critique').

I shall come to Lucas's positive program presently. However, since one of the distinctive feature of the new classical revolution is that consisted in a shift from the Marshallian towards the Walrasian approach, a preliminary task is to clarify the content of the Marshall-Walras divide.

IV. The Marshall-Walras divide

Alfred Marshall and Léon Walras are the two towering historical figures of neoclassical theory. Most economists, though aware that these authors differed in purpose and methodology, think that these differences are small beer compared to what they have in common. I, for one, believe that on the contrary these differences are sufficiently important to warrant the conclusion that the Marshallian and the Walrasian approaches are alternative, not complementary, research programs within the broader neoclassical family.

Marshall wanted economic theory to solve practical and well-defined issues – e.g. what is the impact of a change in the demand for fish on its price and quantity traded? To him, the economy was such a complex reality that studying it as whole was a desperate enterprise. Hence his partial equi-

librium research strategy of studying problems under the *ceteris paribus* clause. It was also important for him to start the analysis of any problem well armed with observed facts. In his mind, definitions and distinctions should not be fixed all and for all. Rather, they should be dictated by the specific problem one is dealing with. Theory had to espouse the contours of reality as much as possible. In view of the complex character of reality, Marshall was skeptical about building formal models. Although trained as a mathematician, he was of the opinion that mathematical considerations should not take a dominant role in economics. Their proper place was in the appendices of theoretical works. An additional factor for this viewpoint was that he intended to be read by a large audience and knew that most of his readers would be repelled by formal arguments.

In contrast, Walras was interested in matters of principle, in questions of a more philosophical nature – in particular, the issue of the logical existence and the efficiency of the equilibrium of a decentralized economy, a query that can be traced back to Adam Smith's attempt to elucidate the mechanism behind the invisible hand metaphor: can a decentralized economy with no authority in charge be an efficient system of resource allocation? However, Walras addressed this issue at an incomparably higher level of abstraction than Smith. Although, unlike Marshall, he had little mathematical training, he was convinced that economic theory needed to be mathematical and built on the model of physics. Of course, he was also aware that an economy was a complex reality, but his strategy for tackling this problem was to start the examination with the study of the most conceivable simple economy (a two-good exchange economy) instead of foregoing the study the economy as a whole. Thus, in his *Elements of Pure Economics*, he began with assessing the mechanism of equilibrium determination in this simplified framework. This done, he extended the results there obtained to more complex economies. To him, internal consistency was the primary criterion for good theoretical construction, which he viewed as a step-by-step process leaving no conceptual issue unsettled. He was fully aware that his theory was about fictive construction. The role of theory *vis-à-vis* reality was to be a foil, possibly an ideal to be attained, not a description of reality. He once wrote, "pure theory requires no confirmation from reality". To him, doing theory amounted to constructing a conceptual benchmark helping to reflect upon the market system and its

functioning. Therefore, Walras's work can be conceived as belonging to political philosophy, for that matter analytical political philosophy, doing about the economic sphere what Rawls would later be doing about justice. Small wonder then that Walras was poles apart from Marshall as far as the targeted audience was concerned. In his eyes, the price to be paid for rigorous thinking was that theory could no longer be understandable to non-initiated.

I cannot enter into the description of Walrasian theory content-wise. What ought to be noticed, however, is that the trade technology, the institutional set up making up for the formation of equilibrium, is devised in such a way that equilibrium is always achieved, equilibrium being understood as a result where all agents' optimizing plans have been made compatible or, in other words, were all agents are in a position of individual equilibrium. Thus, the idea that agents might find themselves non-participating in a given exchange against their will (the gist of the involuntary unemployment notion), that firms might find themselves with unsold good or non-used production capacities against their will, has no room in this framework. What at first sight could be viewed as excess supply is nothing else than the manifestation of the optimal decision of not participating in exchange, prices being what they are.

To summarize, Marshallian theory focuses on external consistency, Walrasian on internal consistency. When Marshall pronounces a theoretical proposition, it pertains to reality. In contrast, Walras's propositions pertain to the fictive economy that he has created, not to reality. Moreover, while a Marshallian proposition is matter-of-factly, a Walrasian proposition is the result of a mathematical demonstration.

At first, everybody will favor the Marshallian methodology strategy striving at a direct explanation of observed phenomena even if bears the price of some sloppiness and of explaining behavior in terms of rules of thumb rather than optimal choice. However, the Walrasian strategy, consisting of admitting no loose ends in the reasoning, should not be discarded too fast in so far as it cannot be excluded that, for all its outward benign character, the Marshallian lack of rigor may end up blocking cumulative theoretical development, in which case the Walrasian strategy would prove to be more productive eventually.

The background clarified, I now return to the main thread of my argumentation to expose the distinctive features of new classical macro. To this end, I shall use the same benchmarks I used for describing Keynesian macro.

V. New classical macro

1. *Overarching aim*. The first point to be stressed is the change in the research agenda that occurred. The central object of study of Keynesian macroeconomics was unemployment – and, in a wider sense, the search for the malfunctioning of markets. In the span of a few years, the unemployment theme ceased to be an important preoccupation of macroeconomists and was sent back to labor economists. The issue of explaining business cycle has taken its place at the top of the agenda. Of course, variations in economic activity (and hence in employment) are a central item in the study of economic fluctuations, but in the new paradigm they are accounted for in terms of hours worked without consideration of the split between the employed and the unemployed. Keynes had been unable to construct a theory of the business cycle, contenting himself with addressing the task of trying to demonstrate the existence of involuntary unemployment at one point in time, a task that in itself was already daunting. Lucas's opinion was that the profession was now ready to address the topic that Keynes had left aside. He also wanted its study to be part of the Walrasian approach. We have seen that Walrasian theory is an equilibrium theory. Hence the 'equilibrium theory of the business cycle' label. Such an attempt was audacious because, before, it was believed that constructing a mathematical model of business fluctuations inspired by Walras was an impossible task. The merit of Lucas, and later of Kydland and Prescott and Plosser and Long, is to have invalidated this opinion.
2. *Equilibrium/disequilibrium*. We are in a Walrasian world. Thus the discussion bears on a fictive model economy. It is constructed in such a way as to be always in equilibrium in the Walrasian sense of the term. Lucas uses the expression of 'equilibrium discipline' understood as a rule that one imposes on oneself to achieve a certain activity. Perhaps some of you known of the French writer Georges

Perec. Among other things, he wrote a book entitled 'La disparition'. It tells a normal story except that the letter *e* is absent from the whole book. This was the discipline Perec imposed himself, in his case a purely gratuitous one. As far as macro is concerned, the exercise is made on the presumption that it can be theoretically productive to picture agents and the economy in this extreme way.

The result is that a totally different picture of the business cycle emerges. Earlier, it was viewed as the disequilibrium phenomenon *par excellence*, the manifestation of a market failure. The mere assertion of its existence was seen as an invitation for the state to take steps to make it disappear. In the new approach, the business cycle expresses the optimizing reactions of agents to outside shocks affecting the economy. In other words, business fluctuations are no longer viewed as market failures, and governments should refrain from trying to prevent their occurrence.

3. *Supply and demand*. Lucas's hunch (first presented in a paper co-authored with Rapping) was that changes in the supply of labor, viewed as a result of optimizing decision-making, play a central role in explaining fluctuations. This insight was further developed in a paper published in 1972, entitled "Expectations and the Neutrality of Money", in which Lucas aimed to give a stronger foundation to Friedman's natural rate of unemployment notion. Market clearing, rational expectations (the assumption that agents' subjective expectations about any coming event coincide with the model-builder's objective expectations) were the three cornerstones of the paper. Lucas's take in the paper, borrowed from capital theory, was that the decision to participate in the labor market or to produce on a self-employed basis is a matter of allocating leisure (and hence labor) both within a given period of time and over time. Economic agents ought to be depicted as comparing the wage rate at one point in time with the wage rate they expect to prevail later in time, say today and tomorrow. If the former is higher than the latter, they will decide to work more today and less tomorrow.

   This intertemporal substitution phenomenon, Lucas contended, is decisive in explaining variations in the level of activity over time. On this insight, he constructed a model of the business cycle where vari-

ations in activity over time are due to two factors: exogenous monetary shocks, on the one hand, and agents' imperfect information, on the other. In this model, agents receive one signal incorporating two distinct pieces of information. Taken separately, these two pieces of information would trigger opposite reactions, changing or not changing the total hours worked. Needing to engage in signal extracting, the optimal solution agents will adopt is to mix the two opposite reactions in some weighted way. Hence the hours worked departs from what they would have been with perfect information. Here, Lucas claimed, rests the explanation of the variations in hours worked over the business cycle. Monetary shocks have real effects but, as argued by Friedman, the government cannot exploit them since they occur only when the changes in money supply are unanticipated.

4. *Main methodological principle*. Lucas wants macro to abide by the Walrasian methodological principles that I have evoked earlier. That is, internal consistency is the alpha and omega of theoretical construction.

5. *Micro/Macro relation.* A stated, new classicists want to get rid of the neoclassical synthesis. The neo-Walrasian conception of equilibrium adopted, there is no longer any reason for trying to build a synthesis between Keynesian disequilibrium theory and Walrasian equilibrium theory because the latter can perfectly take on board that part of the *explanandum* that earlier on was assigned to Keynesian theory. This marks the end of the Keynesian exception. In Lucas's words:

> The most interesting recent developments in macroeconomic theory seems to me describable as the reincorporation of aggregative problems such as inflation and the business cycle within the general framework of 'microeconomic' theory. If these developments succeed, the term 'macroeconomic' will simply disappear from use and the modifier 'micro' will become superfluous. We will simply speak, as did Smith, Ricardo, Marshall and Walras of *economic* theory (Lucas 1986, p. 107).

6. *Accessibility to laymen*.
A dynamic study of the economy requires using new tools, resorting to new difficult mathematical techniques such as optimal control, dynamic programming, etc. As a result, the level of mathematics used

in new classical macro bears no comparison to what was the case earlier. A high barrier to entry needs to be overcome for having just a mere understanding of what is going on.

7. *Ideology*. Keynesian theory was geared toward bringing out market dyfonctioning. Here we have the opposite, a Panglossian view of the economy, to use Keynes's nice reference to Voltaire's *Candid* in the *General Theory*.

> "The celebrated *optimism* of traditional economic theory, which has led to economics being looked upon as Candides, who, having, left this word for the cultivation of their gardens, teach that all is for the best in the best of all possible words provided we will let go along …" (Keynes 1936, p. 33).

Fluctuations in employment reflect rational and optimal reactions by economic agents to changing conditions. If there is no pathology, there is also no need for the state to intervene.

Table 1 summarizes the result of my confrontation between Keynesian and new classical macro. Two points stand out. The first is that these visions of macroeconomics are poles apart. The second is their respective roots ought to be traced back to Marshall and Walras respectively.

Table 1. A confrontation of Keynesian and new classical macro

Criteria:	Keynesian macro:	New classical macro:
1. Overarching *explanandum*:	underemployment	business fluctations
2. Equilibrium/disequilibrium:	disequilibrium	equilibrium
3. Labor supply/demand:	focus on demand	focus on supply
4. Main method. criterion:	external consistency	internal consistency
5. Micro/macro relationship:	neoclassical synthesis	hegemony of micro
6. Accessibility to laymen:	easy	high barrier to entry
7. Underpinning ideology:	mitigated liberalism	laissez faire

V. RBC macro

The 'new classical macroeconomics' term applies only to the works of Lucas and his fellow travellers. Soon, the paradigm that they had inaugurated underwent an inner evolution led by Kydland and Prescott that resulted in the emergence of real business cycle (RBC) modelling. A

further transformation that I shall not discuss led to the emergence of dynamic stochastic general equilibrium (DSGE) modeling. These three modelling strategies should be considered as phases within the same research program the main features of which were present from the beginning.

From the start, Lucas had expressed the view that the task ahead in business cycle theory was to write a FORTRAN program, "a fully articulate artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies" (Lucas [1977] 1981, p. 219). He himself, however, did not contribute much to the implementation of this program. Those who did it were Kydland and Prescott whose 1982 paper, "Time to Build and Aggregate Fluctuations" paper started RBC modeling.

Kydland and Prescott's model is neo-Walrasian, like Lucas's. The equilibrium discipline, rational expectations, a dynamic-stochastic environment, and intertemporal substitution are its basic ingredients, as of Lucas's model. But there are also striking differences. First, Kydland and Prescott abandoned Lucas's insight that the shocks triggering the business cycle were monetary. Instead their hunch was that technology shocks were the right suspect. Second, they abandoned the imperfect information line of research. Third and finally, Kydland and Prescott's work was quantitative while Lucas's model was qualitative. In short, they brought macro to the computer.

The aim of Kydland and Prescott's 1982 model, like that of Lucas, was to show that economic fluctuations could be explained as a consequence of economic agents' optimizing adjustment to exogenous technological shocks. To the outside observer, what is striking in their endeavor is the contrast between the model they use – a stochastic version of the Ramsey model, close to a Robinson Crusoe economy – and its purpose, to shed light on the development of the US economy from 1950 to 1975. The task that Kydland and Prescott set themselves was colossal. Several steps needed to be taken for such a research, each of which involved solving a lot of tricky mathematical, computational and empirical work. The empirical validation of the model proceeded through comparing the volatility, correlation and auto-correlation of output, investment, consumption, hours worked and productivity, as characterizing the US economy, with the equivalent moments from the model economy, the success of the model consisting of

having its simulation mimicking the empirical observations. Kydland and Prescott's model achieved this aim to a rather large extent.

While their paper was first met with skepticism, it gradually became the accepted view that it marked a methodological breakthrough. Several ways were proposed to overcome the objections that were leveled against the inaugural model, such as conceiving other shocks. In effect, from the mid-80 to the mid-90s, RBC modeling witnessed a tremendous internal progress, to an extent that few would have predicted at the time Kydland and Prescott published their paper.

For lack of time, I cannot enter into more details. Nor can I recount the different ways in which economists with a Keynesian inclination endeavored to retort to Lucas's attack against Keynesian theory. Nor have I the time to speak of how, starting in the mid-90s and somewhat surprisingly, a consensus arose between so-called new Keynesian economists and RBC economists on using a model borrowing its basic elements from the two approaches, sometimes labeled the 'new neoclassical synthesis'. Fortunately, for my purpose of answering the question that makes the title of this lecture, it is unnecessary to delve in these developments.

Let me thus go at once to the assessment part of my discussion. How should we judge the developments that took place in macro, namely the dethroning of Keynesian macro and its replacement by new classical/ real business cycle macro (or to use another label DSGE macro)? Is it progress or regress?

VI. An assessment

The question in everybody's mind is which of the two approaches I have described (the 'old' Keynesian macro or the new non-Keynesian macro) is the best one. Lipsey, an old-Keynesian, wrote that what happened was the "replacement of messy truth by precise error" (Lipsey 2000, p. 76). His point is that Keynesian theory has a strong truth-value; it constitutes a good representation of reality even if the argumentation was loose. The opposite would be the case for new classical theory.

Lipsey's standpoint is clever and there are good chances that it may seduce many. In my eyes, however, it is wanting. Why would Keynesian theory

have a higher truth-value than new classical theory? Both are fictive construction differing just because they start from different premises. It is just that one looks more realistic than the other. Keynesian theory, I claim, is based on propositions that seem obvious – for example, that there exist two types of unemployment, frictional and involuntary unemployment, or that the existence of something called 'excess supply' can be assessed right away by observing reality or real-world statistics – but cease to be so upon closer scrutiny. The blind spot of Keynesian economists is that they are unaware that the Marshallian supply and demand apparatus is almost as much inadequate as the Walrasian one for tackling the unemployment phenomenon. Neither of them is useful for such a task – and until the arising of search theory in the 1980s no adequate apparatus existed. The difference between the Marshallian and the Walrasian framework is that, since in the former the reasoning is looser, it is easier to force an outwardly fine (but at bottom defective) explanation of unemployment into it while in the Walrasian framework the contrived character of such an import is at once blatant. If this view is accepted, the conclusion must be drawn that Lucas did a good job in bringing out the conceptual defects of Keynesian macro. In terms of logical consistency, the replacement of Keynesian macro with Walrasian macro was certainly a progress.

Does this mean that NC/RBC macro is without defect? Surely not. A first reason is that Lucas, Kydland and Prescott may well proudly declare themselves neo-Walrasian economists, but most great names of neo-Walrasian theory have qualms considering them part of the family. The reason is that traditional neo-Walrasian economists accept the limitations of their approach while thinking that Lucas and co violate them in their attempt of bringing Walrasian theory to the empirical test. By doing so, their judgment runs, they try to marry two incompatible bedfellows, Lausanne and Chicago. Let me illustrate the point with two quotations.

> Lucas was in the Chicago tradition and was very concerned about empirical testing – whatever the hell that means – something that I have little sympathy for and very little interest in, to be perfectly honest. … I am still of the opinion that theory is more a way of organizing your thoughts, how you would think about the world. And it's strongest in providing counterexamples when people confidently claim that something is true in general (Cass 1988, p. 546).

To Hahn, the Arrow-Debreu model, the emblematic neo-Walrasian model role is to provide a benchmark.

> The Arrow-Debreu model serves a function similar to that which a perfectly healthy body might serve a clinical diagnostician when he looks at an actual body. Now one of the mysteries which future historians of thought will surely wish to unravel is how it came about that the Arrow-Debreu model came to be taken descriptively; that is, as sufficient in itself for the study and perhaps control of actual economies. … If ever a theory was straightforwardly falsified it is the theory of the American economy in Arrow-Debreu equilibrium. But it was never meant to be so obviously falsified; it was designed as both a reference point and a starting point (Hahn [1882] 1984, p. 308).

A second critical observation is that, in equilibrium models of the BC, the policy conclusions are embedded in the premises of the models (this is also the case for Keynesian macro but there the premises are less spelled out). As a result, theorists should refrain from peddling them to politicians. The conclusions of the models cannot be exploited politically. It must be put on Lucas's credit that this is something he has been aware of:

> There is something wrong, and necessarily transient, with this easy translation of a technical contribution to economic theory into a platform for economic policy. … There can be no simple connection between what appears on the scratch pads of professional economists, however original, and important conclusions about the way our society ought to operate (Lucas, various, Box 23, Barro folder).

Unfortunately, it is far from sure that all modern macroeconomists share Lucas's lucidity.

My third and final remark is about the equilibrium discipline, the glasses through which Walras, new classical and RBC economists decide to look at reality. I have already underscored its ideological underpinning. But here I want to draw the attention on another drawback, again one that Lucas was clever enough to perceive without being followed by Prescott. At stake is whether RBC modeling should limit itself to the explanation of mild, normal business fluctuations while admitting its inability to address more dramatic episodes such as the Great Depression (or for that matter, the 2008

recession). Lucas is of the opinion that RBC modeling is valid only for phases of plain sailing.

> One may thus think of the model not as a positive theory suited to all historical time periods but as a normative benchmark providing a good approximation to events when monetary policy is conducted well and a bad approximation when it is not (Lucas 1994, p. 13).

Lucas is right, I believe, but the consequences are more far reaching that what be aware of, as an another quotation, this time from Obstfed and Rogoff, makes clear:

> "A theory of business cycles that has nothing to say about the Great Depression is like a theory of earthquakes that explains only small tremors" (Obstfeld and Rogoff 1996, p. 627).

These remarks bring me to my conclusions. Although I have much sympathy for Keynes and Keynesian economists, I am of the opinion – *horresco referens* – that the dismissal of Keynesian macro has been a theoretical progress. Moreover, the evolution that took place has brought to the forefront a tension that is proper to the macro field. As stated, the latter is supposedly about policy and models end up with policy conclusions.

A second conclusive remark is the judgment made about present day macro cannot be based only on its theoretical content. The meta-theoretical comments made about the theory matter also. In this respect, economists engaged in the same type of theorizing may fare very differently. To wit, as seen, Lucas is well aware of the methodological limits of the theory he inaugurated while, in contrast, Prescott seem to take pleasure in crossing these thresholds, in my eyes, to the disservice of his theoretical contribution.

My final remark is that the path taken by macroeconomics, which in itself has much going on for it, has also led to widen the gap between the production of economic science and economic policy as an art. With macro becoming more like hard science, academic economist loose the ability that earlier economists – such as for example Keynes – enjoyed of going back and forth between the roles of a good scientist and a good statesman. Therefore, to return to my basic question, the progress of macro hardly allows civil society to expect more from it on the matter of choosing the right

policy into which to engage. The public should not have wrong expectations about what present-day macro theory can deliver and macroeconomists should avoid falling in the pitfall of believing that they have an edge on policy matters.

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Laudatio Prof. dr. Marvin Trachtenberg

G. Châtel

Marvin Trachtenberg is the Edith Kitzmiller Professor of the History of Fine Arts at New York University since 1990. He has recently been elected to the American Academy of Arts and Sciences. He is a distinguished scholar on Italian Gothic and Early Renaissance architecture, still enough a generalist to have co-authored, with Isabelle Hyman, *Architecture from Prehistory to Post-modernity* (1986/2002). This comprehensive book remains a mainstay reference work on the western architectural tradition, and I can mention that it is compulsory reading for the students trained in architecture at Ghent University.

Marvin Trachtenberg studied architectural history under such illustrious figures as Wolfgang Lotz and Richard Krautheimer. He authored many groundbreaking articles in the field. A number of his books were awarded important prizes: for *The Campanile of Florence Cathedral: "Giotto's Tower"*, the adaptation of his doctoral dissertation published as a book in 1971, he was awarded the *Alice Davis Hitchcock Prize* by the Society of Architectural Historians; with *Dominion of the Eye: Urbanism, Art, and Power in Early Modern Florence* published in 1997, he won a second Hitchcock Prize, and in 1999 he was awarded the *Charles Rufus Morey Prize* by the College Art Association. Moreover, his outstanding career was supported by numerous grants and fellowships.

The *Dictionary of Art Historians* declares that 'as a scholar Trachtenberg built a career on reinterpretation and rethinking the commonplaces of art history.' Indeed, his work often challenges the current notions of historiography. In opposition to the traditional bias towards a clear periodization of history, he appears to be convinced that the elements of continuity are at

least as revealing as instances of rupture. Almost provocatively he speaks about a 'medieval-Renaissance' architecture – both terms being hyphenated for the occasion. In 1991 he contended that Italian 'Gothic' is a building tradition profoundly refractory to the current classifications of art history. I quote (JSAH, Vol. 50, nr. 1, p. 37):

> Quite simply, […] the shift to the Renaissance should not be viewed as a replacement of "Gothic" by "classical," but rather as the opposition of a newly purified historicist taste, admitting only the classical, to a long medieval era in Italy of voracious eclecticism that omnivorously had consumed everything needed by its complex metabolism, Gothic and classical alike.

His work brilliantly demonstrates how insight in duration and in the persistence of currents or undercurrents deepens our understanding of history. This was already implied by Manfredo Tafuri in his *Theories and History of Architecture* (1968). Though Tafuri set out his argument by pointing at the rift occurring in the 1420's, he argued further on that (I quote, p. 208):

> The problem of the passage from late-Gothic to Brunelleschian Humanism, […], shows clearly that the ideal revolution of methods, of the conception of artistic production and of its social meaning, depended on the re-organization and restructuring of linguistic material and methodological instruments that were already extensively present in the figurative tendencies of fourteenth-century Tuscany.

A case in point is the trecento city planning. In *Dominion of the Eye: Urbanism, Art and Power in Early Modern Florence* Marvin Trachtenberg established that the late-medieval Florentine piazzas (such as the Piazza della Signoria or the public space demarcated around the cathedral) were in fact consciously shaped, formally ordered and esthetically informed 'works of art.' They were realized over a considerable length of time by the agency of numerous parties, often cut out of existing medieval fabric. Nonetheless, they were outlined according to a well thought-out, coherent and flexible set of geometric and visual rules, akin to contemporary developments in the practical sciences and the arts. Though Camillo Sitte already somehow recognized such a mechanism in his comments on the subject (*Der Städte-Bau nach seinen Künstlerischen Grundsätzen*, 1889),

its very conception is at variance with most other scholarly accounts. According to the prevailing art historical views, ideals of regularity, proportion and visual coherence were indeed the exclusive apanage of Renaissance planning.

The question of how these communal 'works of art' could ever have been created collectively over the course of time obviously also applied to the large civic and religious monuments to which those 'squares' were intimately related. No less than the piazzas, these monuments were built over long periods that frequently exceeded the life span of the architects involved. Which kind of practice was capable of producing the monumental quality that we recognize in these buildings? This is the central question to which Marvin Trachtenberg's latest book *Building-in-Time, from Giotto to Alberti and Modern Oblivion* (2010) is devoted. I quote (p.70):

> […] Buiding-in-Time, which […] has remained virtually invisible and nameless in modern consciousness or at best has been perceived as something irrational and strange, even abject, was in fact an intensely rational, highly ordered, and deeply grounded mode of material praxis.

The book thus aims at rediscovering the rational substructure, and at retracing the *modi operandi* of this forgotten building practice. The reader is brought to measure the distance separating a consent to provisional achievement posited by the praxis of 'Building-in-Time', and a state of perfection considered by Leon Battista Alberti as 'so that nothing may be added, taken away, or altered, but for the worse' (*De re aedificatoria/On the Art of Building in Ten Books*, 1485/1988, VI-2, p. 156). Alberti appears to have written *De re aedificatoria* in opposition to the architectural praxis of his time. Though his views on an essential integrity of the project soon deeply penetrated the domain of ideas, they hardly had any influence on the actual building practice. The praxis of 'Building-in-Time', remained predominant for a long time, in fact until technological innovations and the industrial organization of the building enterprise considerably accelerated the building process.

Building-in-Time, from Giotto to Alberti and Modern Oblivion considers the tortuous way by which modernity established itself in history. The narration coalesces with a polemical argument about historiographical

method, and an acute reflection about intentionality, authorship, and more generally, about the epistemological basis of architectural practice. It thereby points to temporality as a key notion in the constitution of an architectural culture.

Architectural culture is a field where practice, theory, and visions of history encounter and interact. Yet, in this configuration the interrelations are all but steady. Alan Colquhoun noted that architecture is a form of knowledge profoundly determined by experience (in *Three Kinds of Historicism*, 1983, p. 17). However, in *Il territorio dell'architettura*, a reflection on the epistemological condition of architecture, Vittorio Gregotti remarked that 'history presents itself as a curious instrument: its knowledge seems indispensable, but once acquired, it cannot be used; it is a sort of passageway one has to walk through, but that doesn't teach us anything about the art of walking' (*Le territoire de l'architecture*, 1966/1982, p. 87).

As a productive discipline or as a practical art, architecture partakes in the formation of reality. It thereby envisions the future while considering its past. Nevertheless, this does not imply any simple temporal continuity nor some inevitable collusion between the practice of architecture and that of history. Both practices can be considered as projects, yet of a fundamentally different nature. Architecture may stir interest because every project attests of a speculative attitude in the face of reality, but such an attitude would impede sound historical research. While the study of history pursuits truth, the endeavor of architecture merely aims at truthfulness, and history avails to practical use only when deformed by ideology. Critical history exists as an aspiration to free itself from the grip of false consciousness, and as an antithesis to the praxis of architecture.

Dear Professor Trachtenberg, the presentation of the *Sarton Medal for the History of Sciences* simply expresses our esteem and gratitude. As architects we are grateful to a critical historian – of course not for teaching us how to walk or where to go – but for helping us to comprehend what we do while we walk.

Grand Canyon, Saqqara, and Brunelleschi's Dome: Thinking and Making Architecture in the Premodern Era

Marvin Trachtenberg

In the modern world architecture lives in a problematic relationship with time and temporality, which it seeks to distance from its glossy surfaces. In its intensity this conflict is a recent phenomenon, best approached not directly but via the portal of the distant past. Strange as it may at first seem, an expedient entry is through landscape and geological history, ultimately by way of certain ecological concepts, broadly construed. Among the most informative sites of this domain is the great scar on the face of the America earth wrought by time, so tremendous as to be visible from space to the naked eye, yet culturally invisible in its ecological implications.

Learning from Grand Canyon

Visitors to the Grand Canyon tend to experience it in silence, grasping its proportions as a landscape of the immeasurable, violent sublime. The land opens up without warning, dug out a mile deep through scores of miles, an immeasurable vertiginous vista (Fig. 1, p. 224). One finds oneself looking down into the bowels of the earth. The sublime assumes a shocking, implacable beauty that challenges one's power to assimilate sensory experience.

Alternatively, for the late modern consumer of sensation there is always the possibility of destroying the uncanny silence and asserting one's dominion over nature by taking a deafening helicopter ride through the Canyon, or

testing one's nerve with white-water rafting in the Colorado River. At the very least one sneaks up to the edge as close as one dares in places without railings, sometimes too close.

Grand Canyon seems to have become an experience of spectacle and sensation for its hordes of visitors. But there are other modes of apprehension, in particular one that I employed during a recent visit when I stood at the edge with my eyes riveted on the scene. What I mainly was watching was the river, the living heart of the site (Fig. 2, p. 224). It was of course the river that created the canyon. I saw that even in the dead of winter, before the snow melt in the mountains that are the main source of the river's water, it was strongly flowing. The water was obviously saturated with sediment scraped from the canyon's bottom and crumbling banks. It was visibly carving the Canyon further out, and further down. I sensed the erosive power of the river as it slides along the bottom of the canyon, eating away at the banks that, for the time being, still channel its path.

Thinking about the gigantic vista and its coming into being, I came to realize that the driving power was not the river alone. It was not simply the river that carved the canyon down from the flat plains as it snaked imperceptibly back and forth in varying paths grinding the earth. The primary sculptural force was not flowing water but time itself – sheer duration – vast quantities of it. The river was only serving as the agent of time, not as durational measure but as the primary agent of change in the world. The canyon exists not only in space – as space – but as a vertiginous vista into oceans of time past.

The 17 million years that it took time and the river to carve out the canyon seemed overwhelmingly long until I recalled that the formation of the multilayered crust of the earth from which the Canyon was carved took even longer, at least 600 million years according to cross sections of its staggering geology (Fig. 3, p. 225). The Canyon resulted not only from the time it took the river to cut through the earth but also the time to form the land, the 600 million years first required for the earth to be built up, only to be then cut down, sediment by sediment.

As one ponders this vast and involuted geological history, time emerges vividly as both creator and destroyer, a double role that was fully recognized at the founding of time philosophy by Aristotle. This Janus-faced agency is particularly evident at the Canyon: time creates layer upon layer

of the earth's crust, then slowly destroys it, yet in this destruction creates what for modern human experience is a sublime spectacle as well as a world-class site of entertainment.

Architecture Against Time, Time Against Architecture

These observations concerning the indomitable forces of time lead back to the question of architecture and time. In western thought one tends to imagine architecture, at least in theory, as an absolute tectonic entity, immutably existing outside time, in a primary mode of absolute being rather than becoming. Yet the tectonics of human-built architecture is no less saturated with the forces of time than are the "tectonics" of the earth's crust, as that branch of geology is sometimes called. As specialist in French literature Denis Hollier brilliantly explains in his book, *Against Architecture*, conceptually architecture exists against time, whose corrosive, implacable agency threatens it with entropy and oblivion.[1] Certainly what we think of as "monumental architecture" – the most significant buildings that societies erect – tend to project an aura of resistance to time, at least in the Western World (Fig. 4, p. 225). Its works are designed to resist time's corrosive forces – to have *inertia* – and above all they are made to *look as if* they might defeat architecture's nemesis and to triumph over time. This illusionism is not limited to what Alois Riegl called "intentional monuments" such as the pyramids.[2] Historically it has affected virtually all ambitious works. Often this involves not only a look of imperturbable strength but of "timeless" form. Thereby, time is self-deceptively factored into architecture by theoretically being removed from it. Time is ironically present in its apparent absence.

In the end this strategy of deception always fails (Fig. 5, p. 226). One way or another, whether by the forces of nature or at the hand of man, most monumental buildings end in ruins, for they exist not just as ideal tectonic concepts but materially in the world. Even the pyramids are coming slowly undone, and ultimately, probably in far less than 17 million years, they will be ground down and literally buried in the sands of time (Fig. 6, p. 226).

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1 Denis Hollier, *Against Architecture: The Writings of George Bataille* (Cambridge, 1992).

2 Alois Riegl, "The Modern Cult of Monuments: Its Character and Its Origin," *Oppositions* 25 (Fall 1982), 23-51.

Architecture and Time

Be that as it may, I am concerned primarily with the role of time in building architecture up – the positive, creative agency of time. Here I am not treating time as history, or as the sensory kinetic experience of the beholder, but rather time in its primal, generative role. Time is not a subject much considered in modern architecture culture, which chronophobically has tended to exclude temporality from theoretical discourse, but it was a primary factor in the thinking and making of premodern architecture. The earliest monumental project in Egypt offers a paradigmatic example (Fig 7, p. 227). Zoser's funerary monument at Saqqara, the first of the pyramids, did not come into being as an immaculate design, but through a two-decade process. The initial building of a traditional, if oversized, mastaba was repeatedly enlarged, then radically transformed into a gigantic stepped-pyramid, which itself was finally enlarged into the ultimate version that would become the prototype of the giant old kingdom pyramids in its pure, unstepped pyramidal form. As with the Grand Canyon, the destructive as well as creative agency of time was at work here, albeit on a smaller scale, in piling layers above the earth's surface, digging into it, destroying old forms and making them into new ones. Conceptually the essential difference to the Canyon is the presence of human intelligence as opposed to nature's blind agency. It was not time and the river that were at work at Saqqara, but time and human cognition, volition, and action.

Thinking and making architecture in time in the premodern world is an extremely complex topic, and my recent book on the subject is only an initial study of what deserves a wider field of investigation.[3] Here, I offer not a synopsis of the book but some cases concerning the thinking and making of architecture in time in the medieval-Renaissance period, primarily in Italy.

Making Architecture through Long Durations

Although all buildings come into being in time, the time required varies greatly not only between buildings but more among architectural epochs. Even the largest modern buildings tend to rise so quickly that no sooner are

3 *Building-in-Time from Giotto to Alberti and Modern Oblivion* (London:Yale University Press, 2010).

foundations poured than the completed structure seems to stand before our eyes. In works such as the Empire State Building, an iconic instance of extreme speed, erected in less than fourteen months, the dimension of time is collapsed into a brief spasm of comprehensively financed, programmed, and instrumentalized activity (Fig. 8, p. 227). Certain premodern time-spaces, notably Imperial Rome which built the Colosseum in less than ten years, the Baths of Caracalla in four (Fig. 9, p. 228), often approximated the modern capacity to produce monumental architecture with extreme expedition. But others did not.

Protracted construction of large buildings was the norm in the European middle ages and Renaissance. Canterbury Cathedral, for example, required a series of projects from the 11th to the 14th century to attain its full measure of extremely disparate parts (Fig. 10, p. 228). This was in sharp distinction to the four years of construction of a highly unified scheme in the immense Roman bath complex of Caracalla into whose central suite of rooms the entire English church would fit. At Lincoln cathedral, the facade alone comprises elements from the 11th century portal zone to the 13th century flanks and finally the 14th century towers (Fig. 11, p. 229). Much as at Saqqara the design of this facade evolved through time, here a great deal of time, three centuries rather than the two decades of King Zoser's reign. And here the composite result was proudly displayed rather than buried within a dense, inscrutable mass of masonry. But the linkage of time and change is just as direct.

Lincoln and Canterbury might give the impression that long durational construction necessarily produced extremely heterogeneous final results, but this was not necessarily the case – as we saw already at Saqqara. The desire for apparent formal unity of older and newer parts is a factor that varies in time and place. Although English cathedrals tend to manifest extreme formal disparity between successive parts, French cathedrals are generally far more unified, even when evolving over similar time spans. Notre Dame in Paris, for example, came into being over nearly two centuries of evolution during which unity was tightly maintained (Fig. 12, p. 229). The French might have regarded the English buildings as unbearably discordant, and vice versa. Opposing desires regarding temporal effects were at work, in one case the intent to smooth them over, in the other to highlight the way time produces change.

Regardless of such questions of formal modality, many cathedrals were under construction for so long that they constituted not a building but a perpetual building site. In Florence, for example, construction of the Cupola substructure went on for so many decades that the surrounding street became known as the "Via dei Fondamenti," a name which lasted until the 18th century.

That the monument in an enduring state of becoming was a familiar feature of many cities is apparent in the church seen in the background of the 15th-century Pietà by Konrad Witz in the Frick collection (Fig. 13, p. 230). In it the new choir is completed, the facade is under construction, but the small old nave is still standing between them. Also to note in this picture is not only the creative agency of time – slow and discontinuous though it may have been – but its destructive force, which is evident in the fortress to the right falling slowly into ruins.

Building Histories in Italy

The examples of the practice of Building-in-Time in northern Europe could easily fill this entire essay. But I would like to turn to the historical site commonly known as medieval and Renaissance Italy, with which my work has been mainly concerned. Examining the well-known Bigallo view of 1342 of late medieval Florence or one of the many cinquecento *vedute* of the new St. Peter's in Rome, one finds that the process of construction embodies the same temporal modality that we have seen in the northern European examples. In the Florentine view, the duomo, campanile, and S. Croce are seen in their actual unfinished state as of 1342 (Fig. 14, p. 230). S. Croce appears on the extreme right, depicted with its open, half finished nave. To the right of the baptistery, the old cathedral of S. Reparata has been truncated by the new, half finished duomo facade, and next to it only the lowest stories of the campanile are standing, as was the case in 1342. These works rise on the pictorial surface as images of the building process and its incompleteness, their points of initiation receding decades into the past and their termination stretching towards an unknowable moment in the future. Similarly, by the 1530s St. Peter's has attained a massive presence after a generation of construction, but closure of the vast, glacially-moving project was more a matter of faith and hope than any expectation that the

viewer might live to see it (Fig. 15, p. 231). In fact the Florentine campanile took two decades more to complete, S. Croce a half century, and the duomo and St. Peter's each an additional century or more.

Such "building histories" – a term invented to designate such extended construction – are all but inconceivable in modern practice. The rare exceptions, notably St. John the Divine (New York) and Gaudi's Sagrada Familia (Barcelona), constitute strange neomedieval frankensteins, architectural barnacles rising in our sleek modern cities. But to the premodern observer, such works (or the Bigallo and St. Peter's views) would have presented nothing out of the ordinary. Large-scale, long-incomplete buildings were a familiar sight in the cities of medieval and Renaissance Italy. At any point between 1100 and 1600, and beyond, buildings such as San Marco and the Palazzo Ducale in Venice as well as cathedrals throughout Tuscany and Lombardy could be seen in the midst of ongoing construction spanning generations and sometimes centuries. Some of these sites were old projects winding their way slowly to completion – sometimes never attained, or only in the 19th century like Milan cathedral. Others represented modification of older fabric once regarded as complete. Construction included not only contiguous extension but also satellite buildings, such as baptisteries or bell towers. Moreover, the space opened around the edifice, the piazza, served the monument and was generally considered an extension of its solid into the void, and an integral part of its history.

Fluidity of the Project in Building-in-Time

An essential point in studying extended duration as a primary ground of this monumental architecture concerns the instability associated with time. That is to say, the way time inexorably produces change. Since the origins of philosophy, change has been at the core of the construction of time: to think "time" is to imagine "change." In Aristotle's words, "Time cannot be disconnected from change."[4] This means that the underlying condition of the architecture under consideration was not so much duration but change itself: architecture embodied an inherent tendency to mutation. Practically no ambitious building of the age was completed in the form initially intended, and this is true also of many lesser buildings.

4 *Physics,* IV, 11, 218b.

The phrase "form initially intended" raises a second issue: time produced change, but change from what? What was it that changed? If we were to imagine an image of one complete design being replaced by another, we would in most cases be badly misconstruing the process. Initially the formal intentions regarding any project were incomplete, and this condition produced a multiplier effect on the inherent mutability of the structure slowly emerging in time. A comprehensive design did not exist at the beginning any more than did the building itself. The intentions of the architect were not "complete" even to himself; a hard line between designing and building did not yet exist. What came into being in time was not only the evolving physical structure but quite literally *its design*, as an integral part of the slow process of facture, *in* the realization of the building itself.

The "change" produced by time thus did not necessarily mean change from one theoretically complete state of design to another. Rather it entailed an evolution in *formal status*, from the relatively schematic to a slowly emerging formal completeness. This obviously made the design/build process quite complex, as both the ever-incomplete design and the ever-evolving fabric were continuously changing in tandem, along with the formal status of the totality. Moreover, such changes typically did not emerge from a single mind, but were made by successive architects of a work, with each repeatedly revising his own revisions through time.

Changing Social and Ideological Conditions

One underlying factor for this rather curious state of affairs is the differing social basis of architecture. As is well known, premodern social life was deeply communal. Before the 15th and 16th centuries, monumental architecture usually was not the product of individual patrons and architects but of the community as a whole or its various institutions, and even with the rise of "individualism" in the Renaissance this practice continued. Whether as patron or architect one built for the community of which one was a part rather than narrowly for oneself. The basis of architecture was transsocial and transgenerational. One's strictly personal investment in a given design was thus limited, at least comparatively speaking. This tended to mitigate personal antagonism to design changes, and to soften and even preclude resistance to one project modifying and overtaking another.

Things changed when the individual patron emerged and the concept of the individual architectural author was invented. For this neo-Petrarchan architect, working in the climate of a new temporal consciousness driven by the mechanical clock and humanism itself, time and change were radically problematized. Rather than enabling his project, they became a mortal threat to his system of desire, existence, and life's work. This shift did not happen spontaneously. As I explain in the book, the new architect-author was one of the great inventions of Alberti, who advocated what I call "Building-outside-Time." In this theoretical program all design activity is restricted to the preconstruction phase. Afterward, all change is forbidden, as if time and change did not exist. Only in this way, Alberti rather desperately imagined, would the design of the would-be architectural author survive and be realized in a monument that would grant him enduring honor and fame. For Alberti, this was the raison d'être of architectural practice.[5] During the Renaissance Alberti's temporal program was more an ideal than a reality, serving mainly as a current of resistance to the normative architectural temporality of what I term Building-in-Time. Building-outside-Time only came to dominate architecture culture in the modern period. That is, Alberti's famous definition of "beauty" as "that reasoned harmony of all the parts within a body, so that nothing may be added, taken away, or altered, but for the worse" only became a primary architectural directive in our own time, along with his strict separation of designing and building, thinking and making architecture.[6]

Planning Principles of Building-in-Time

This radical new code raises the question of just how the pre-Albertian regime of Building-in-Time actually managed to produce coherence rather than the disorder that Alberti feared. Coherence and order resulted not simply from a spontaneous ad hoc solving of various design problems according to the randomly distributed abilities of architects. Rather, a set of four operative principles was at work. It constituted an *administrative system* that directed planning and enabled the creative and effective management of the forces of change.

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5 On this theme, cf. Marvin Trachtenberg, "Ayn Rand, Alberti, and the Authorial Figure of the Architect," *California Italian Studies*, 2 (1), 2011, http://escholarship.org/uc/item/6ff2m22p

6 *De re aedificatoria*, VI, 2; echoed at least eight times in his treatise (cf. Trachtenberg 2010, 70).

By "principles" I mean not any codified set of rules in the manner of Vitruvius, Alberti, Serlio or Palladio, but rather aspects of a *virtual* system embedded in practice. This methodology comprised a number of interlocking conceptual and practical paradigms that describe the essential conditions, techniques, and directives that structured and guided the complex design/build process. These mediating "principles" formed a code that was the genetic basis of medieval/Renaissance architecture. As such they provide a key to the informed and effective logic that underlay the seemingly tangled building histories of the age.

I have given these principles names, which are, in a certain logical order, continuous redesign, myopic progression, concatenate planning, and retrosynthesis. In practice, there often occurs an inevitable elision and imbrication between them, as is inherent in any such quasi-structuralist system, but the four are distinct protocols nevertheless.[7] As the reader will note, just as was the case with so many "medieval" cultural phenomena, in certain important respects all four principles seem to have had identifiable ancient roots (including Ovid and Vitruvius). In general, alien as these concepts might at first seem, study reveals that they were deeply embedded in the period *mentalité*.

One: Continuous Redesign

The first and most basic of the principles, *continuous redesign*, concerns an underlying condition of the architectural regime, that is, the essential, pervasive phenomenon of change. In a sense it is just another way of describing change as caused by or that accompanies time. But this would naturalize the presence of continuous redesign, and there was far more to it. For one thing, it may have been yet another instance involving the pervasive influence of Ovid in the period (being, in any case, the "Ovidian" principle). More critically, specific historical conditions of society at large seemed to impel architectural metamorphosis. The complexity and volatility of the independent city states of Italy, for example, were powerful factors in architectural production. Despite certain shared sociopolitical structures and tendencies, the cities were diverse in all aspects of the public

7 For a fuller explanation of the four principles, Trachtenberg 2010, 130-43.

realm, and such diversity was intensified by an inter-city and inter-institutional competitiveness. Each city, moreover, was politically unstable, rapidly evolving as a whole and in its various institutions. In architecture, the resulting need to satisfy an ever-changing range of practical needs and to fulfill the desire to establish signs of ever-shifting terms of identity and difference meant that no single architectural style – Gothic, classical, or whatever – or set of typologies could possibly attain hegemonic status. Especially before the quattrocento, formal idealism was conspicuously absent from Italian planning.

Instead, a high degree of *eclecticism* was prevalent in every aspect of architectural design, including iconography, morphology and materials. For example, Parma baptistery combines Gothic portals with classical galleries, and inside the building Gothic ribs coexist with classical columns and niches (Fig. 16, p. 231 & Fig. 17, p. 232). Every important building tended to constitute a new formal assemblage dedicated to the individuated realization of specific practical and symbolic needs, that is, a motivated *change* from previous works of its genre such as cathedral, town hall or baptistery. Not only was the presence of change thus inherent to the initial design, but the formal assemblage that constituted this design was inherently unstable because of its eclecticism. In the process of Building-in-Time the eclectic package of the "initial" design was liable to unravel and undergo revision, particularly given the volatility of the social and economic environments of architectural production.

Seen from a slightly different perspective, eclecticism also fed continuous redesign in its ability to incorporate an unlimited range of forms, images, and symbols. An eclectic assemblage had the tendency not only to unravel, but also to draw ever-new elements into the package as configured at a given moment. It was able to do so because for Italy eclecticism entailed an implicit rejection not only of formal idealism but also of unichronic purism and the exclusionary notion of the anachronistic. Eclecticism thus allowed a polychronic dimension in the sense of what Georges Didi Huberman[8] and more recently Alexander Nagel and Christopher Wood have said regarding painting,[9] that is, the incorporation of more than one

8 Georges Didi-Huberman, *Devant le lemps: histoire de l'art et anachronisme des images* (Paris, 2000).

9 Alexander Nagel and Chirstopher S Wood, *Anachronic Renaissance* (New York 2010).

layer of time in an artifact. Eclecticism meant that Building-in-Time literally was *building with time* in incorporating diverse temporalities, and this served to increase the volatility of planning. In multiple ways, therefore, the underlying eclectic methodology of Italian practice was in close alignment with the principle of continuous redesign.

Two: Myopic Progression

Given the pervasive tendency toward continuous redesign, it would have been pointless at the outset of a project to establish a detailed, comprehensive design for actual construction. Certainly the technical means to do so in the way of plans, elevations, models, templates, and prototype details were not lacking. These were the means eventually used in the period to produce the final building**,** and they theoretically all could have been exploited at the outset. But buildings were not pre-designed in this manner. Instead, the general rule was that initially only the spatio-structural scheme was defined, with other aspects resolved to lesser degrees of accuracy, few of them in directly buildable detail. Various formal levels were only finalized as the process of construction neared actual fabrication, and ultimately in the act of fabrication itself in the case of intricately carved details such as tracery, capitals, leafwork, intarsia patterns, and so on. Translating a spatial into a temporal metaphor, I term this gradualist design method *myopic progression*, in analogy to the way the details of an object come into focus to a near-sighted person only as she or he approaches it, with progressive resolution from larger outlines gradually down to fine details.

To more fully understand this principle, we must be reminded of how fluid Building-in-Time was, and how it relied on a feedback of processes that today are kept distinct and unidirectional. As mentioned earlier, morphogenesis was not cleanly separated into thinking and making, fashioning and fabricating. Builders constantly were fine-tuning and resolving problems not readily apparent initially but only manifesting themselves in the fabrication process. That buildings were not fully pre-designed – as they are in today's Albertianist practice – meant that what came into being in time was the evolving physical structure and *also* quite literally its comprehensive design, in the realization of the building itself.

One thus grasps the crucial role of myopic progression in the planning system. It was the principle that regulated and limited the extreme fluidity of the design/build process. Myopic progression funneled communal desire and workshop energies into alignment with the temporal and material conditions of practice. If Continuous Change was fueled by unrelenting desire, Myopic Progression was driven by a relentless logic of facture, which protected its integrity.

The operation of this guidance system was highly varied. It included not only the planning of space-containing form and external massing and facades – that is, "pure" formal design – but also other aspects of the ever-developing project. Among these was the very structure of the building. Especially in very large vaulted buildings – strange as it may seem to us – structural issues were not always settled, or even seriously raised, until construction was well advanced. This was the case famously at the Florentine Cupola, which was structurally resolved only half a century after its formal shape was fixed (Fig. 18, p. 232). This procedure was evidenced even more drastically in the failure of the Duomo Nuovo in Siena, where serious structural analysis came too late, resulting in condemnation of the entire project (Fig. 19, p. 233). Siena presents an unfortunate example of the overextension of myopic progression, whereby key details were left too long unresolved. Most projects evidently did not end in such disaster. The strange procedure usually worked, and it is possible for us to grasp the epistemology and temporal logic of treating structure as an aspect of myopic progression.

For all their collective experience and knowledge of traditionally "correct" material and structural "norms," the builders seem to have realized that their structural knowledge was limited. They were not in the position of modern engineers, who generally can attain absolute, mathematical certainty regarding the exact load and stresses posed by any new project as well as the precise strength of building materials and structural forms used to meet those demands. There was never any such advance certainty in premodern structural design, especially regarding very large, vaulted construction, but only guesswork and approximation, despite the occasional claims of architects to the contrary. At the beginning, the outcome was veiled in uncertainty. Structural certitude, the true validity of a structural formula, only came into being gradually, in the act of building,

which was thereby performative not only materially but epistemologically.

One thereby built not only the design, but structural knowledge: the validity as well as the full definition of its structural properties. Neither "theoretical" rules nor experience-based judgment provided certainty, but only the material act of building itself. Just as the problematics of formal details slowly became clearer, so too did the various problems of structure come to manifest themselves in time. Once again, current architectural doctrine was in accord with still-fashionable ancient doctrine, here the pervasive dictum *veritas filia temporis*, or "truth is the daughter of time" (a topos traceable to a phrase in the Roman writer, Aulus Gellius).[10] Time demonstrated not only what had been planned well, but what seemed to be going wrong, and continuous change and myopic progression usually allowed that such problems be corrected ad hoc. Thus, the extensive written program of 1420 to build the Florentine Cupola, for example, explicitly required that changes be made in the project should periodic reviews of construction reveal problems.

In this age, unlike our own, builders presumed that structures could always *theoretically fail,* and occasionally, as with Siena Duomo, they did. There the architectural knowledge that was built was not of the correctness of planning but the certitude of error. In this respect, perhaps the paradigmatic building of this regime was the Leaning Tower of Pisa, where structural "knowledge" literally and visibly unfolded in time. As the tower gradually leaned due to unknown flaws in the subsoil (an underground river), its incline was repeatedly "corrected" by building successive groups of stories plumb, resulting in a bent axis or "spine" that enables it to stand up to this day (with some assistance of modern reinforcement of the soil).

Three: Concatenate Planning

The funnel-like movement of myopic progression – its trajectory from the abstract to the concrete, whole to part, and large to small – was interwoven with another protocol of Building-in-Time. It concerned the general

[10] Fritz Saxl, "Veritas filia temporis," in Raymond Klibansky and H.J. Paton, eds., *Philosophy and History: Essays Presented to Ernst Cassirer*, New York 1963, 197-222.

problem of morphogenesis rather than its "myopic" devolution from the schematic to the detailing. I term this principle *concatenate planning*. As the term implies, it involved a chain-like linkage of planning steps. More precisely, it required that every design move be linked to a previous move, or series of such moves, which meant that as a whole the building formed a continuous chain of design steps. Virtually nothing in the process could be considered arbitrary. Everything was grounded in a totalizing system of cross-referencing, often to numerical values, geometric figuration, and a canon of proportional modalities and relationships. Ultimately this doctrine would seem to derive from the Vitruvian principle of *symmetria* or commensurability (I. 2.4; III.1): the rule that every part be related to every other part, and the parts to the whole. This notion was taken up widely in medieval practice just as later it would also be reflected in rigidified form in the Renaissance theory of Alberti, Palladio and others, as studied by Wittkower.[11] What distinguishes concatenation from these related ancient and neo-antique doctrines is above all its dynamic quality and its participation in the fluid processes of Building-in-Time.

The concatenate process was only metaphorically chain-like. It rarely formed a linear, single strand of links. Rather it tended to incorporate branching and other complications, resulting in a complex net- or web-like structure (Fig. 20, p. 233). Like a true chain, on the other hand, the *catena* was not a closed-structure but theoretically endless. No matter how complete-looking, the *catena* could always be added to, so that it became a fragment of a potentially more extensive chain. Moreover, such complication or revision could occur at any time. Nor was there any inherent distinction between complications effected during the production of the "initial" design of a building and new or altered links subsequently forged by revision during construction or even long after the nominal "completion" of the fabric. Design and redesign were conceptually indistinguishable at the level of concatenation in the regime of Building-in-Time.

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[11] Rudolf Wittkower, *Architectural Principles in the Age of Humanism*, New York, 1971.

Four: Retrosynthesis

Thus far I have described three principles that governed the practice of Building-in-Time:

continuous redesign, or the engine of persistent change; myopic progression, or the devolution of design from the whole to the parts over time; and concatenation, or the accretionist technique of chaining architectural forms large and small, solid and void into netlike assemblages of continuous interrelationships. The three principles provided respectively for design mobility, practical logic of design attention, and relationship between parts. But what of the effect of the "final" whole? The fact that the work was consistently concatenated did not mean that the resulting totality would necessarily be harmonious or unified, attaining that elusive presence and authenticity which were vested in coherent formal properties in the final analysis. Again, planning needed to be in accord with Vitruvius, in this case his important rule of comprehensive harmony, *eurhythmia*, I. 2; VI. 2.

Such unity and associated properties of identity in the regime of Building-in-Time resulted from the fourth principle, which I term *retrosynthesis*. It required that planning always be synoptic: no matter how great the passage of time between design changes or their number and scale, a form of overall unity must always obtain. Such unity was imposed retroactively, in a process of retrofitting the new to the old, recasting the effect of the old in the light of the new, beginning with the insertion of the initial project into the (always preexisting) building site. Through retrosynthesis – in effect, *eurhythmia* in dynamic operation– the building history of a work becomes the history of a series of coherent projects, each one integral in itself, yet subsequently assimilated into a new unified whole. This process may continue through any number of cycles, theoretically unlimited although inevitably terminated by historical contingencies.

Retrosynthesis was essential to the formal integrity of the realized architectural product. It was the limiting principle that proscribed freewheeling change and suppressed the possibility that openended concatenation might degenerate into formless metastasis. Retrosynthesis often leaped across huge gaps in time in a Kubleresque way as at San Miniato al Monte, where Michelozzo's 1440s tabernacle harmoniously transforms the 11th century interior. Similarly, in the cinquecento at San Lorenzo in Milan, Martino

Bassi powerfully reconfigured the still-recognizable late antique shell (Fig. 21, p. 234 & Fig. 22, p. 234). As realized with unsurpassed vision and skill through four centuries of near-continuous re/planning at the Duomo complex at Pisa, retrosynthesis contained the will-to-change within an envelope of desired unity and harmony (Figs. 23-25, p. 235).

Durational Aesthetics versus the Modern and Postmodern

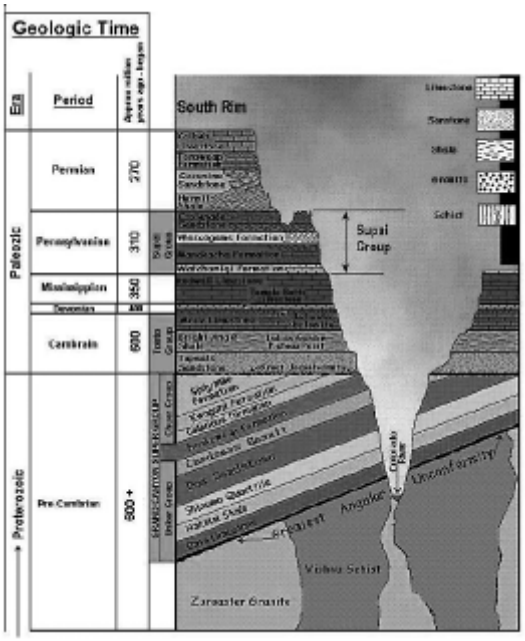
Retrosynthesis thus directly (if inadvertently) addressed the dilemma of Greek thought regarding the problematic agency of time as well as the fusion of change and identity in the same evolving thing. It was in fact a key to what I would call "durational aesthetics." This premodern model presents an alternative to both modernist and so-called postmodernist aesthetic doctrines. Time becomes problematic in both latter cases, particularly in architecture. In Alberti's absolutely perfected form, time is excluded. In postmodernist thought, time overwhelms the building and blocks the goal of a perfected object, as in George Steiner's statement that "form is not perfected act but process and incessant revision,"[12] By contrast, in the premodern model of durational aesthetics retrosynthesis was the final means of *enabling the production of perfected form in a process of incessant revision.* Thereby time, facture, change, and the goal of perfection coexisted in a dynamic, salutary state of becoming and being. Without this model, neither the Pisa complex, nor San Marco in Venice, nor the Florentine cupola, nor new St. Peter's would have been remotely conceivable, let alone realized. Whether this temporal model is relevant to our own architecture culture is another question.

12 George Steiner, *On Difficulty and other Essays (*Oxford, 1978).

Illustrations

1. Grand Canyon (author)

2. Colorado River, Grand Canyon (author)

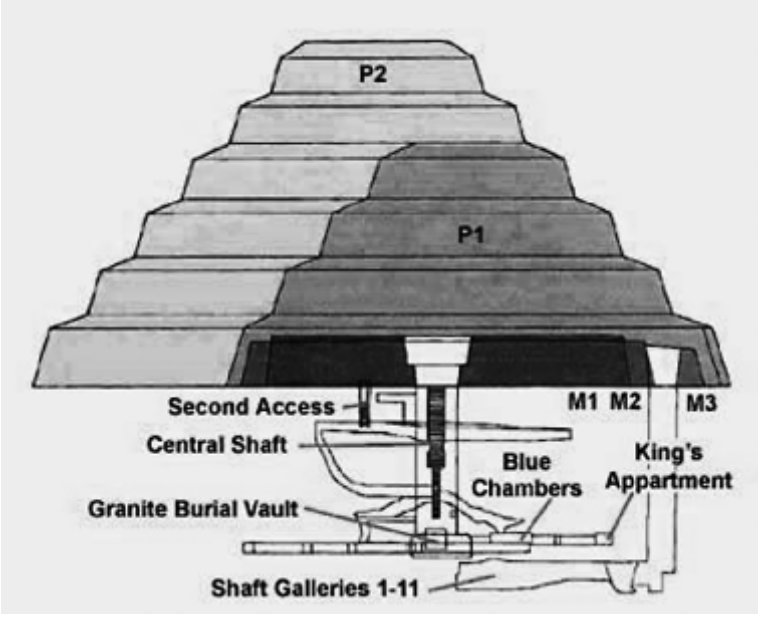


3. Grand Canyon, geological cross section (Wiki commons)

4. Temple of Hera I, Paestum (author)

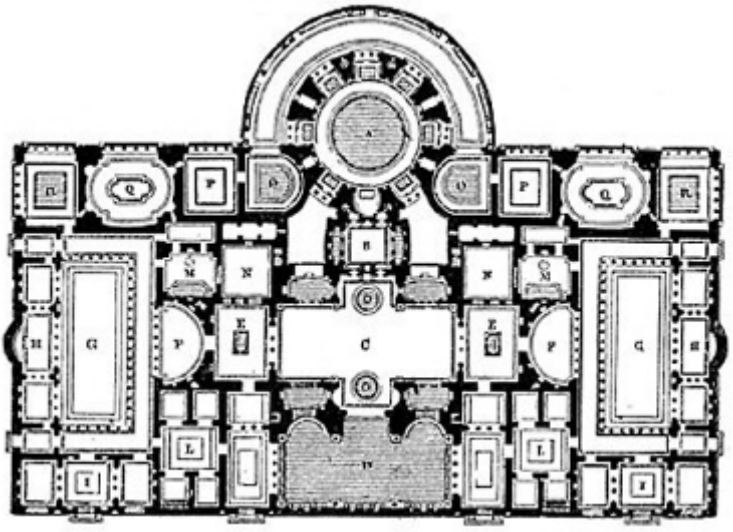
5. Ruins of the World Trade Center, September 2001

6. Bent Pyramid, Dashur (author)

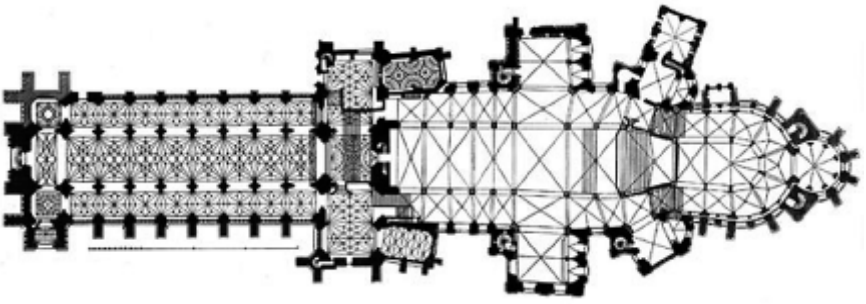


7. Cross-section of Zoser's Stepped Pyramid at Saqqara (Oklahoma State University)

8. Empire State Building, built in 410 days, March 17, 1930 to May 1, 1931 (New York Public Library)

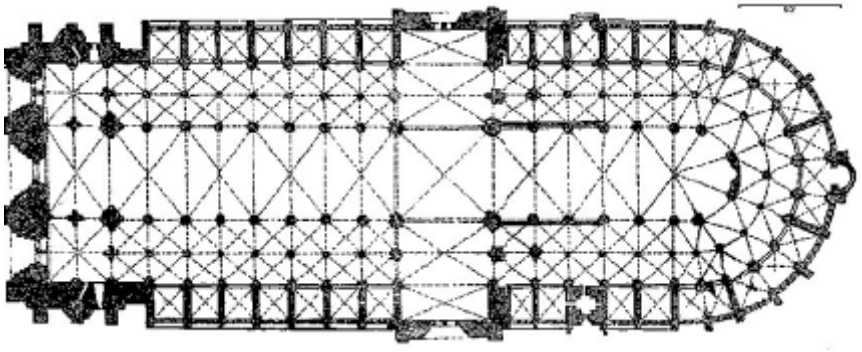


9. Baths of Caracalla (W. Lübke: Grundriß der Kunstgeschichte, 1901)



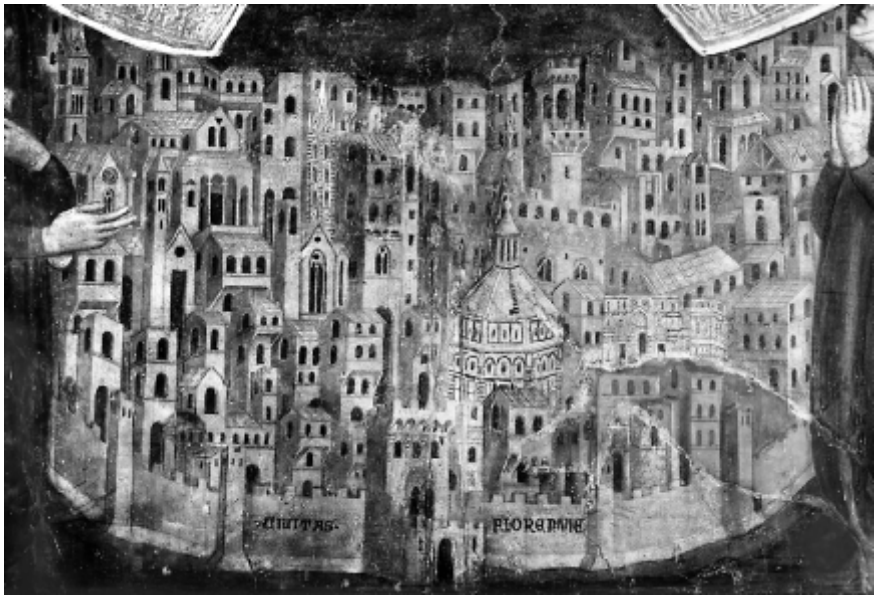
10. Canterbury Cathedral (G. Dehio, 1901)

11. Lincoln Cathedral facade, 11th-14th centuries (author)

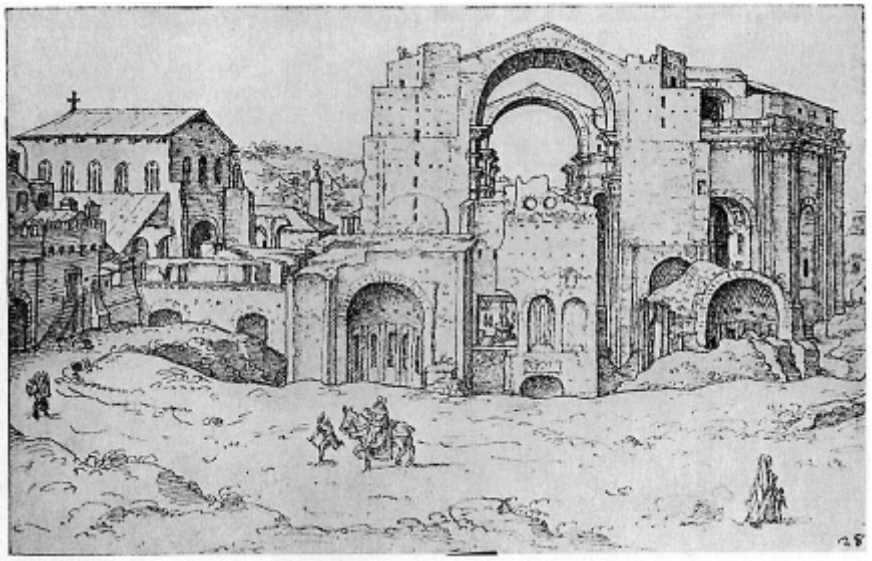


12. Notre Dame cathedral, Paris (Wiki commons)

13. Konrad Witz, detail of Pieta (Frick Collection)



14. Bigallo fresco, Florence, 1342 (Alinari)



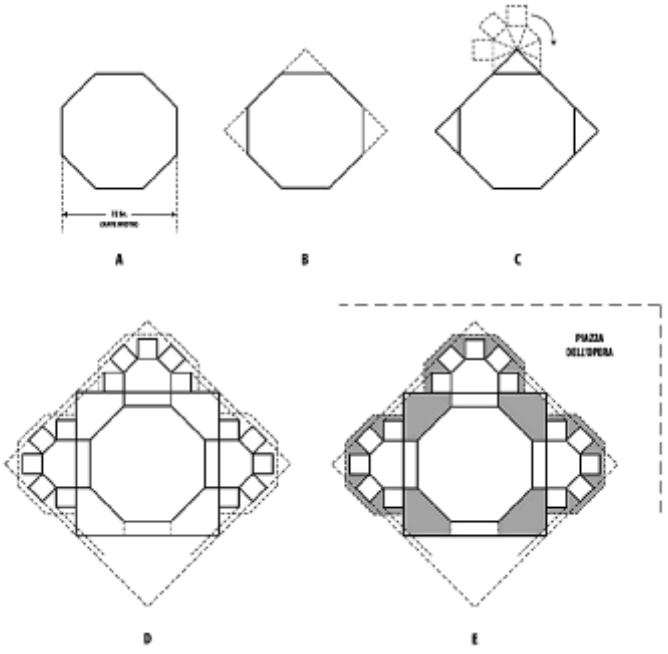
15. Marteen van Heemskerck, St. Peter's *vedutra*, sketchbook 1, f.15r., ca. 1532/6, Kupferstichkabinett, Berlin

16. Parma Baptistery (Emanuele Lugli)

17. Parma Baptistery, niche and vaulting (Emanuele Lugli)

18. Cupola, Florence cathedral (author)

19. Siena, ruins of Duomo Nuovo (author)



20. Florence cathedral, planning of east end, 1360s (author)

21. S. Miniato al Monte, 11th century, with Cappella del Crocefisso of 1448 (author)

22. S. Lorenzo, Milan, late antique shell with remodeling of 1573 by Martino Bassi (author)

23-25. Pisa duomo complex, 11th to 14th century (author)