What could the 'longue durée' mean for the history of modern sciences?

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The text

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Abstract

Fernand Braudel's concept of the longue durée is easy at hands when historians of science take into view extended periods of time. But what is exactly meant when we speak of a longue durée history of an object, instrument, concept or research field? Here, a revised meaning of the concept is proposed, which takes into account the historical observer and the background, which in the case of recent science is provided mostly by developmental narratives. Thus, a perceived longue durée could refer to historical episodes marked by continuity in the sense of a "contemporary of the non-contemporary" (Gleichzeitigkeit des Ungleichzeitigen, R. Koselleck). In the light of a temporal understanding of the longue durée, the problem is distinguished from that of micro- versus macrohistories. Prospective examples of longue durée histories are presented (e.g. simple techniques, instruments or concepts) as well as some reasons why telling such stories might benefit not only the history of science, but the general perception of the sciences.

Keywords

longue durée, history of science, life sciences, historiography

Que pourrait signifier la 'longue durée' dans l'histoire des sciences modernes ?

Résumé

Le concept de la longue durée selon Fernand Braudel vient à l'esprit si des historiens de science considèrent des périodes étendues. Mais que signifie exactement une histoire de la longue durée d'un objet, instrument, concept ou d'un champ de recherche?

En prenant en compte l'observateur et l'arrière-plan historiques, ce dernier étant caractérisé notamment par des narrations 'développementales', nous proposons une réevaluation du concept de longue durée. Ainsi, la perception d'une longue durée pourrait se référer à des épisodes historiques marqués d'une continuité dans le sens du 'contemporain du non-contemporain' (Gleichzeitigkeit des Ungleichzeitigen, R. Koselleck). De plus, nous différencions le problème de la longue durée de celui des micro- versus macrohistoires.

Nous présentons des exemples d'histoires en longue durée (p.e. techniques simples, instruments, concepts) et nous esquissons pourquoi ce sujet pourrait bénéficier non seulement à l'histoire des sciences, mais à la perception de la science en général.

Mots-clefs

longue durée, histoire des sciences, sciences de la vie, historiographie

Sommaire

When studying past science, observers (historians or not) may perceive certain developments –such as uses of instruments, techniques, concepts or topics, straddling extended periods of time or even reappearing. In such cases, Fernand Braudel's concept of the *longue durée* is easy at hands. But what is exactly meant when an author speaks of a *longue durée* history of research fields, or even the *longue durée* of a certain object, e.g. a manuscript?

This working paper pursues these questions. Therefore, I will first present an analysis of the *longue durée* as used for science by Braudel and other authors. In a second part, a somewhat revised meaning of the concept is proposed, which is based on reflections on observations of the past as well as the historiographical work of Reinhart Koselleck. A *longue durée* in this sense fits well with the specific complex of problems encountered when studying recent science. It appears that perceiving historical developments as extended in time requires reflecting on historical background against which stories unfold. Moreover, the impression of such developments being out of joint with time, or in the words of Koselleck the "contemporary of the non-contemporary" (*Gleichzeitigkeit des Ungleichzeitigen*) seems central when applying this concept to modern science. The problem of *longue durée* will also be disentangled from that of micro- versus macro-histories or 'big pictures'.

Finally, some prospective fields of where to look for *longue durée* episodes are presented (e.g. simple techniques, instruments or concepts), as well as some arguments of why telling such stories might benefit not only the history of science as an academic activity, but the perception of science more generally.

In the framework of a discussion paper, the objective cannot be more than to hint at possible ways to work on a topic that has repeatedly popped up in different contexts in the history of science (for references see below). Moreover, these reflections remain bound to how the problem of the *longue durée* has appeared in the history of the 20th century experimental sciences, and the examples chosen are certainly skewed by the author's area of expertise, the life sciences. Yet, these reflections may hopefully provide a stimulus for further discussions among historians and historians of science of all sorts.

What is the *longue durée*, and how has it been related to the history of science?

Shortly before he deceased in 2004, historian of science Frederick L. Holmes made a plea for *longue durée* approaches in the history of science, referring of course to Fernand Braudel, who had coined this concept another half century ago (Holmes F., 2003). Braudel, who belonged to the second generation of the *Annales* school of history, had employed the *longue durée* to develop a historical perspective that focuses on how almost immobile natural conditions, or more generally stable *structures* have shaped human action over extended periods of time. Famously, Braudel's work has shown how the geography of the Mediterranean region with its interspersed mountains ranges and stretches of sea has influenced the formation of e.g. trade or political relations. In the same vein, Braudel underlined the impact of annual rhythms of climatic changes on the customs and life forms of e.g. farmers, coast dwellers or the development of settlements. Braudel differentiated this "quasi-immobile history, that of man in relation to the milieu that surrounds him" from two shorter historical times. These are a "slowly rhythmic" social history, centring on the actions of groups, and lastly the *histoire evenementielle* – the traditional history of events and the individual, which he characterized as a "history of brief, rapid, nervous oscillations" (Braudel F., 1966: 16 [foreword of 1946]; Braudel F., 1977: 56).

In a programmatic text that tried to relate the historical and the social sciences in the environment of the 1960s, Braudel briefly hinted at possible uses of the *longue durée* in the history of science. He mentioned the "constructed universes" that had an impact on human life for centuries despite their explanatory insufficiencies – the "Aristotelian universe" that lasted until Galilei, Descartes and Newton, or the "geometrically represented universe", which itself only collapsed following the "Einsteinian revolutions" (Braudel F., 1977: 57).

Frederic Holmes, renowned for numerous in depth historical studies of the chemical and the life sciences (mostly centred on individual scientists such as Lavoisier, Claude Bernard, biochemist Hans A. Krebs or molecular biologists Meselsohn and Stahl), then asked whether it

would not be possible to adapt the *longue durée* to the history of science less by such "thought structures that have prevailed in the past than by organizational assemblages, techniques, deeper mental frameworks, spiritual constraints, or economic systems that have provided the support for and the limits that science could not transgress for long periods of time [?]" (Holmes F., 2003: 465) That is, his vision of a renewed *longue durée* aimed at a reconciliation of the insights and methods that the history of science had gained following the 1980s through often local, practice-centered case histories with the "grandeur of the 'origins of modern science' that the older stories [in the history of science, MG/MM] celebrated". (Holmes F. 2003: 463)[1].

Holmes then sketched a few lines along which such narratives could be imagined. He identified "one succession of enduring structures that commanded the flow of biological history" on the level of techniques, more specifically methods of observation (*l.c.*: 468). From unaided vision to achromatic and finally electron microscopy, these methods each related biological organization on various levels (organs, microscopic structures such as tubules, cells and their organelles) to function.[2] Holmes then outlined similar broad stages in the history of chemistry since the early modern period, the scope and the limit of which were set "by the apparatus, instruments, and the repertoire of substances with which chemists could perform their operations." (*l.c.*).

Holmes' argument, published posthumously from a manuscript of a presentation is inspiring in this attempt to reconcile recent methods and topics of the history of science with a concept usually associated with narratives from another era. Yet, it leaves many questions open. What does it exactly mean to perceive a development as long? Do we not inevitably over-generalize when bringing early modern anatomy and molecular biology into one perspective, thereby losing the achievements of microhistory? What is the relation between the *longue durée* and so-called 'big picture accounts'?

The *longue durée* as a form of observation and its historiographical implications

It may appear relatively straightforward to use the concept of *longue durée* in a simple "chronological" or "calendarial" sense to the history of science, e.g. to describe the observation that certain conceptual frameworks have remained stable over extended times, or for a long-term history of a method, an instrument or a model. I assume that many uses of the *longue durée* concept have been made quite casually in this sense, without questioning too much what an analogy to Braudel's concept may exactly refer to, or reflecting on the historiographical framework in which it emerged. A *longue durée* in this sense may appear unproblematic when referring to periods distant from the present or time spans we usually conceive of as "extended" (i.e. several centuries), in particular when the focus of analysis straddles customary periodizations of historiography, such as the early modern period and the 20th century. It seems that Staffan Müller-Wille and Hans-Jörg Rheinberger use the *longue durée* in this way in the introduction to their history of heredity, which studies the phenomenon broadly from *c.* 1500 -1870 (Müller-Wille S. and Rheinberger H.-J. 2007: ix, 6-7). Yet, also in this case, one could ask questions similar to the ones discussed here for more recent times and shorter periods. First, the question of temporal reference points is obvious – where is the chronological limit for a *longue durée*? There can be no doubt that answers to this question depend on the specific case, the period we are looking at, and what specific background knowledge we have on that period. In the case of heredity, the reason to speak of a *longue durée* is based on the fact that the work looks back into natural history and legal or administrative discourse long before the heredity rose to prominence in 19th and 20th century biology. In a sense, things are brought into connection, which are usually conceived of as belonging to different historical periods.

1. Even if most of Holmes' books focus on the work of individual scientists, he discussed the problem of a „long-term construction of scientific knowledge" before, when he conceived of a history of intermediary metabolism from 19th century physiological chemistry to mid-20th century biochemical pathways. He suspected that the „short-term social processes upon which Latour and Woolgar, Shapin, Collins, and others rely to explain scientific judgements and the achievement of consensus do not seem adequate to encompass problem situations that require several scientific generations to resolve." (Holmes F. 1992: 46 ff., 50)

2. The question of whether the relationship of form to function has remained constant throughout remains open here and would require deeper scrutiny.

Moreover, and this point becomes especially relevant when we follow Holmes and use the *longue durée* not only for broad mental frameworks or trends, but for e.g. concepts, instruments, practices or objects, one has to ask what the link or the continuity we assume actually refers to. Is it justified to speak of the *longue durée* of a concept if its meaning has significantly changed? Does it make sense to talk of the *longue durée* of a text if it has been transferred to another scientific community where it has found a different usage? Hence, the question is to determine what actually remains in place over a given time span, and in what way we conceive of continuity with respect to our background understanding of the past.

The need to circumscribe in more detail the meaning and the historiographical function of the *longue durée* adapted to science's history is obvious for recent times, and most of all for the 20th century with its acceleration of scientific development. Clearly, years do not tell us much here about continuity or breaks, and the question of who perceives which time span as lasting long against what background cannot be avoided if we want to make a meaningful use of this concept. In order to shed some light on these issues, let us return to some of the examples mentioned by Braudel, Holmes and others for *longue durée* approaches in the history of science, and then sketch a refined framework that deals better with these questions.

A proposal for a revised meaning of the longue durée

Continuity - From 'mental prisons' to layers of time (Zeitschichten)

Braudel explains the continuity observed in history by the existence of structures that shape and constrain human action over extended periods and that are themselves "little worn off by time and carried on for very long" (Braudel F., 1977: 55).[3] Geographical and climatic factors range among the most prominent in Braudel's work, epitomized already in the title of "*La Méditerranée et le monde méditerranéan dans l'époque de Philippe II*". Thus, it is presumably no surprise that an early attempt to adapt a *longue durée* approach to the history of medicine, Mirko Grmek's "*Préliminaires d'une etude historique des maladies*" (1969), has proposed an intertwining of the *natural* history of diseases (referring to e.g. the invasion of species such as rats, the spread of certain germs or the immunization of populations) with an history of civilization. This approach to transfer the *longue durée* into the history of medicine seems to make immediate sense, although it becomes problematic when we consider the historically changing conceptions of diseases, and our bind to historical sources (primarily textual) when studying natural phenomena of this episode. The reliance of history on constraints provided by other sciences becomes even more problematic when we move into the realm of economy (here, Braudel mentions e.g. the "limits of productivity" as constraining development at a given time), let alone when he talks about "thought structures" such as *the* Aristotelian or *the* Newtonian universe. In a frequently quoted expression, these latter representing represented for Braudel "mental prisons of the *longue durée*" (*l.c.*). We have to take into account that concepts such as the Aristotelian universe are themselves results of historical work, and in many cases, detailed analyses have rendered them much more nuanced and stratified than it may appear on the first sight (see the critique of Holmes by Huisman F., 2005). Some of Holmes' cases for a *longue durée* in the history of science also display such problems. When he states, for example, that "[a]mong the structures that supported and constrained biological investigation for many centuries was that of the human or animal body itself", one may readily object that the history of science has revealed to us how different bodies have been perceived and studied very differently over the course of centuries (Holmes F., 2003: 466).

For both Braudel and Homes, one needs to ask what continuity actually means and what it refers to. Clearly, there is a tension between the

3. One of Braudel's interests in this text was to differentiate his usage of the term "structure" from that of the social sciences - he referenced Claude Lévi-Strauss' *Anthropologie structurale* (1959). This debate on 'structures' in between historians and social scientists of the 1960s is itself already part of the history of the human sciences and cannot be recounted here. Michel Foucault's "*L'archeologie du savoir*" (1969: 3 ff.) provides a critical position aiming at Braudel and the *Annales* historians. Foucault has countered the idea of structures by highlighting the significance of discontinuities in the history of ideas, science or philosophy. More recently, historians of space and environment have addressed Braudel's line of argument from a different angle – by historicizing what appears to him as the most evident example of the invariable, that is, geographical and climatic conditions (on the historization of physical space see *e.g.* Koselleck R., 2000: 96).

supposition of continuity provided by long-term frameworks constraining science and the recent methodology and results of historical research that focus on individual events and situate these in specific, local contexts. Following the German historian Reinhart Koselleck, one could say that the mode of investigation of our own discipline turns metahistorical categories, such as that of a 'structure' stabilizing human action, into historical statements.[4]

Is Holmes' plea for a *longue durée* that takes into account histories of particulars thus aporetic? I do not think so. A first step to illustrate this would be to specify Holmes' example of the body constraining biological investigation. One may assume that not a fixed biological reality has provided continuity but certain forms of human activity, *e.g.* the fact that human and animal bodies have been *compared* in anatomy over a long period of time. In that case, the issue looks more subtle and cannot be so easily dismissed. The question remains, however, on what grounds we perceive the continued comparison as long. Addressing this problem obviously requires an analysis of our understanding of historical time.

The two formulations of the problem of a *longue durée* of the body address different modes of continuity, or more generally of temporality. If we take the body as a biological reality in analogy to geographical structures in Braudel's work, we refer to a different type of continuity, or duration, than when stating that a method has been repeatedly used by scientists over a long time. The work of Reinhart Koselleck, who has fathomed the *longue durée* problem for conceptual history (*Begriffsgeschichte*), can help to clarify this distinction.

Koselleck has distinguished different forms of duration that the *longue durée* could refer to. First, we may think of geographical and climatic preconditions of human action, which (even if influenced to a degree) humans do not completely control – here the climate, the mountain ranges and seascapes that have become synonymous with Braudel's work come to mind. "Natural preconditions [of our existence as mammalian organisms] that allow our specific anthropological experience of time" fall in the same category – circadian rhythms, sequences of generations framed by birth, growth, reproduction and death, etc. (Koselleck R., 2000: 12).[5] From these basic conditions of humankind's existence in time, Koselleck differentiates "structures of repetition" (*Wiederholungsstrukturen*), that human beings "consciously adopt, ritualize, culturally enrich and level to a degree of consistency that helps to stabilize a certain society" (*l.c.*: 12/13). Death, or so he illustrates, can be culturally shaped as political killing, and feeding as asceticism or culinary art.

Distinguishing these two "layers of time" (*Zeitschichten*), as Koselleck put it, may help to make sense of the concept of *longue durée* for the history of science. For although scientific activity can certainly not be disconnected from natural preconditions of human existence, repeated actions of humans establishing continuity seem a far more promising way to conceptualize such phenomena in science. Moreover, this way of thinking frees the *longue durée* from a naturalistic or structuralist discourse on history that one can see in the examples of Braudel. This latter appears particularly inappropriate when assuming that science is after all a social and cultural activity, and that its historical development needs to be described as a highly individualized process. This reformulation, that is, should make the *longue durée* more palatable for contemporary historians of science.

That is, rather than analogizing *e.g.* a long-lasting background assumption informing science or a continued use of a method to the effects of geography or climate, I propose to understand such phenomena of continuity as *repeated human activities*. For example, a problem may be addressed by different generations of scientists, an instrument may be used year after year, a method taught to students over many semesters. Also, different patterns of repetition are conceivable. Certain phenomena may exist continuously or be interspersed by breaks; repetition may be desired, forced by external constraints or simply exist as a result of inertia. A spectrum of recurrent actions in historical time not only goes beyond static or generalizing approaches to historiography, but also allows to reconcile the *longue durée* of a certain historical object reconciled with different motives of actors. Thinking of continuity in science as instances

4. "*Alle metahistorischen Kategorien schlagen im Zuge der Forschung um in historische Aussagen.*" Koselleck R., 2000: 301.

5. I am aware that current science and technology has an increasing impact on many of these conditions – from reproductive medicine to mundane things such as artificial lighting or greenhouses. Yet, these gradual changes to still very tight patterns (think of a night without sleep, or life beyond the confines of Western metropolitan areas) do not justify abandoning Koselleck's distinction completely.

of recurring phenomena (*Rekurrenzphänomene*, Koselleck R., 2000: 20) allows us to accommodate the highly individualized situations encountered in historical research with a differentiated concept of continuity. Each historical moment may comprise different layers of time (*Zeitschichten*) at once, and that moment may be conceived of as a singular instantiation of a broader trend.[6]

What is more, a conception of continuity based on repetitions allows for variation and gradual change. It is interesting to note that in this respect, recurrent phenomena fit well with conceptions that have been brought forward to explain the dynamics of scientific *change*, such as the notion of "tinkering" advocated by François Jacob, or the differential reproduction of experimental systems in the work of Hans-Jörg Rheinberger (Jacob F., 1977; Rheinberger H.-J., 1997; Morange M., 2013a). Repeated, gradually changing phenomena exemplify two sides of the same coin. Whereas the prevailing interest has been to understand gradual scientific change, the same phenomenon also secures stability or permanence in science.[7]

The longue durée as an historical experience - the contemporary of the non-contemporary (Gleichzeitigkeit des Ungleichzeitigen)

Let us now try to understand to what historical experience this modified concept of a *longue durée* beyond a mere chronological sense might refer to when we observe past science. Even if the history of science has been purged of progressivist understandings, it is probably true (at least for the 20th century experimental sciences) that most of its narratives are informed by *change* as science's primary mode of existence. Most histories simply centre on a *development* – *e.g.* from classical to molecular genetics, the introduction of the electron microscope etc. Generally, historical analyses are far less interested in what has remained in place, that is, in continuity, longevity or re-appearances, let alone in things falling out of fashion and fading out. All of these phenomena may be expected when we study *e.g.* instruments, patterns of explanation, research fields or styles of doing science – or do they? Is it not all too obvious that science constantly changes, brings forth the new etc? Can't we read in the newspaper every morning that a new theory or model for a natural phenomenon has been found, that a new generation of instruments will advance science, don't we see every year that prizes are awarded for novel discoveries (or at least those from years ago that are considered for their novelty today)?

Certainly, development in the sense of innovation is what characterizes modern science and it seems intrinsic to the entire endeavour, but different forms of continuities as well as disappearances are almost as certain (for examples below) – it is more the question of whether we consider them worth looking at. But let us accept the 'developmental narrative' of science as prevalent for a moment, as it provides the background against which to understand the experience connected to a longue durée phenomenon in the abovementioned sense.

Imagine that in the course of a studying historical material, we come across a form of continuity, say the repeated use of an instrument that appears outmoded at the time, or an explanation that has been aired decades ago already. We will probably be surprised of this happening *so late*, in fact *too late* with respect to an historical background informed by development. Of course, the respective phenomenon needs to be of relevance for the scientific pursuit at that time to cause that impression – if a scientist strikes a match in 1980 as others may have done in 1900 that does not count.

By contrast, the reappearance of an "*old*" question may even shock us, as may the continued use of an outmoded instrument (a 'dinosaur') or method that we connect with other times. To illustrate the experience of an event going in this sense 'against the tide of time', imagine seeing a 1920s car driving through the concrete environment of a post-War European suburb, and then imagine to find out that these sorts of cars were actually still relevant for certain purposes.[8]

6. With the so-defined concept of temporal layers (*Zeitschichten*), Koselleck aimed at overcoming the parallel arrangement of *longue* and *courte durée* as well as *histoire evenementielle* in Braudel's work.

7. Heiko Stoff (2009) has proposed to use the concept of „assemblage" (*Gefüge*) to describe ontological aspects pertaining to structures, continuity and change. Stoff has taken up this concept from *e.g.* Paul Rabinow or Hans-Jörg Rheinberger and Staffan Müller-Wille as a way to conceive of "constellations of actors, things and institutions" that allow to represent both change and continuity without having to employ make use of "totalizing categories" (Stoff H., 2009: 152; Rheinberger H.-J.and Müller-Wille S., 2009: 237). The assemblage, however, does not address the issues of temporality in science's historiography that is at stake here.

8. For real-world examples of such situations regarding technologies, see D. Edgerton (2008), *The Shock of the Old.*

In the course of his conceptual reflection on history and its writing, Reinhart Koselleck has called an event that appears temporally out of joint with an assumed line of development the "contemporary of the non-contemporary" (*Gleichzeitigkeit des Ungleichzeitigen*, Koselleck R., 1989: 132). Presumably, the *longue durée* often refers to this type of historical experience. Some examples will help to illustrate what this means. The continued relevance of simple techniques such as chemical indication reactions or Robert Koch's microbiological culture plates in an age we normally associate with spectroscopy, electron microscopy or genomics may be called a *longue durée* of these practices. Similarly, reappearing motives of explanining biological ageing by intoxication from the early 20th in the early 21st century could be called a *longue durée* (see below for details on these examples).

In these cases, we experience a contrast between one sequence of events characterized by continuity in the sense of repetition and a received background of science's development. We have encountered what we were not expecting, maybe not even looking for. Obviously, this experience of perceiving something against the course of time is connected to our very own horizon of expectation (*Erwartungshorizont*) as observers of the past. That is, such cases will probably appear as somewhat anticlimactic, potentially marginal and certainly "below the surface" against a background characterized by change, be it through the actions of individuals, technological innovation or social factors.

Even if the meaning of the *longue durée* sketched here has departed from Braudel in many respects, there is common ground. In the case of human history shaped by geography, climate or larger economic trends as in the present conception, the influence of *longue durée* factors run counter to the expectations and the preconceived notion of history as more or less permanent change driven by the actions of individuals, or *histoire evenementielle*. Braudel conceded that he had begun his dissertation on the Mediterranean politics of Philippe II in the framework of diplomatic history, being indifferent to geographical conquests, economy and social problems, and ignorant of the "big facts of civilization" (Braudel, Préface 1969, 3). He became aware of these broader factors influencing history that were not present in prior historiography on the subject only in the course of his work. Later, he designated the *histoire evenmentielle* (metaphorically the ripples on an ocean's surface) as the most capricious, illusory of all lapses of time" (Braudel F., 1966: 16 [foreword of 1946]; Braudel F., 1977: 52; see also Holmes F., 2003: 464). That is, here as in the cases described above, a *longue durée* seems to address a layer of time and a level of impact that is not immediately visible to observers, an that may come as a surprise to many.

A thought experiment might help to illustrate the bind of the *longue durée* experience to the expectations of an observer, and thus to the background understanding of history exposed here. Imagine we discover the continuous relevance of an "old" method in scientific practice, which first of all surprises us. Imagine then that in the course of further inquiry, we find out that this phenomenon has been more of a rule than an exception. We would probably cease to speak of a *longue durée* at a certain point and revise our background understanding of the historical development, even though the chronology of events remains the same. Such a *longue durée*, characterized as a form of historical experience, cannot be separated from our horizon of expectation as observers of the past, it is informed by the background narratives with which we are accustomed, and hence prone to revision in the course of historical work.

Longue durée versus 'big picture accounts'

Before sketching some examples of methods, explanations or scientific fields that appear to run against the received narrative of scientific development, the problem of the *longue durée* as framed above should be disentangled from that of "big picture accounts", or micro- versus macrohistory. Again, Holmes' paper will serve as a starting point. He introduced his plea for the *longue durée* against his impression that a history of science dominated by microhistorical case studies had lost a certain "*grandeur*" proper to earlier narratives, such as the "origins of modern science" stories (Holmes, F. 2003: 463). Holmes was not alone with that impression. Reflections on how big pictures or "generalist approaches" could be brought back to counter a fractured landscape in the history of science dominated by microstudies can be traced back to the early 1990s.[9]

9. Secord J. A., 1993, de Chadarevian, S., 2005; see also the focus section "The Generalist Vision in the History of Science", *Isis* 96 (2), 2005.

But what exactly is a big picture? In a special issue of the *British Journal for the History of Science*, John R. R. Christie characterized big pictures by first, a general embracing subject (*i.e.* the Chemical Revolution, biology), second, an epochal spatio-temporality (*i.e.* covering either a chronologically long period, or a global space) and third, an ultimate significance (Christie, J.R.R., 1993). Even if the latter category is of course highly subjective, Christie describes big pictures from the 20th century history of science as usually based on "philosophical emplotments". That is, the narratives of these works were inspired and borne by epistemological concerns of certain directions in the philosophy of science. Christie and Soraya de Chadarevian (2005) seem to agree on the fact that big pictures of this type have waned in parallel with the rise of a more sociologically than philosophically inspired history of science that centres on practices and local knowledge rather than theories and general subjects – with Christie pinpointing the watershed in the development away from big pictures to Thomas Kuhn's "Structure of scientific revolutions" (1962).[10]

Thus, on the one side, we have the problem of generality, significance and unity of accounts in the landscape of recent history of science as a methodically refined and differentiated research field.[11] On the other side, we have the question of how science should be understood as unfolding in time, and how we accommodate stories highlighting forms of continuity such as persistence or repetition in relation to accounts interested in and focusing on change.

Braudel's mentioning of the Aristotelian or the mechanic universes as *longue durée* structures in the history of science may have effected a conflation of the two problems, as we tend to associate especially these classical topics with "big picture history of science". We should be aware that the background against which these two problems need to be differentiated nowadays is the recent development of our own field, as contrasted to Braudel's horizon.

Big pictures, however they may be delineated, could provide one favourable terrain to describe *longue durée* phenomena (e.g. if they straddle delimitations of historical epochs, research fields etc., e.g. by centring general practices such as observation). However, one may easily imagine *longue durée* histories that would not qualify as big pictures – such as a history of the continued impact of Robert Koch's plate technique in microbiology since the 1880s over more than a century, notwithstanding all dramatic innovations and changes this field has undergone since then.[12]

Highlighting such a form of continuity often looks anachronistic in relation to established narratives of history of science. This makes such stories appear marginal on the first glance, and thereby diametrically opposed to big pictures.[13] The demand to look at such historical episodes as well (least of all to gauge their relevance) resonates with some aspects of what David H.E. Edgerton has outlined for the history of technology in his "*The Shock of the Old*". Edgerton advocated a shift of focus away from innovation and high-tech to "technologies in use" and their differing modes of existence, such as maintenance, re-uses or adaptations (Edgerton D., 2008).

It should be stressed again that this is not a call for a revisionist historiography that denies the obvious, namely change in 20th century science, nor is this a plea for some sort of structuralist or even naturalizing historiography based on a monolithic idea of continuity. However, balancing narratives of development and change with those stressing forms of continuity (such as repetition, re-uptake or persistence) would be desirable in

10. It could be added that Foucault's "*Archéologie du savoir*", which proposes to focus on discontinuities and transformations in history rather than on continuity, dates to the same decade; see Footnote 3.

11. This problem also pertains to problems such as the external reception of work done in the history of science, how the academic education in this field should be structured etc.

12. Also, one should note that chronologically extended works exist in the history of science that one would probably classify as neither a big picture, nor *longue durée* in the sense proposed here, such as the German „*Geschichte der Biologie*", a multi-author volume edited by Ilse Jahn, covering the life sciences from Greek natural philosophy to molecular biology (Jahn I., (Ed.) 2000). In a similar vein, John R.R. Christie, one of the editors of the "Cambridge Companion to the History of Modern Science" (ed. 1990) qualified the work as "not a unitarily conceived work with a chronological structure devoted to a whole civilizational epoch", but "essentially a heterogeneous collection of small and middle-sized pictures" (Christie J.R.R., 1993, 394).

13. The notion of what one could call an „observational anachronism" that we are referring to here, i.e. an historical experience that does not match with the context, would need to be distinguished from anachronisms as a methodical problem in the history of science, that is, the various ways of retroprojecting today's state of science to understand earlier stages of its development.

order to draw a more adequate picture of science. This also does not mean that such episodes have not yet been described, as will become clear below. Yet they remain scarce and scattered, and it looks as if there is little focus and interest to describe phenomena of continuity. Even if the background assumption of progress as 20th century science's mode of existence might have vanished today, that of development seems to inform and frame what is being studied. After providing some examples for a *longue durée* in this sense, I shall explain why a broader discussion of *longue durée* phenomena as things seemingly going against the tide of time might be worthwile.

Forms of continuity - examples for the *longue durée* in the history of the 20th century life sciences

In the following, some interesting examples for conceiving of *longue durée* histories are presented. These cases pertain to different levels of scientific practice – from methods and instruments to explanations, research fields or ideals of rationality. Due to the author's expertise, the focus is mostly on experimental and conceptual aspects of the 20th century life sciences. This selection does not imply that the concept of *longue durée* could not prove fruitful regarding social, political or institutional issues; Quite to the contrary - under the heading "[o]n contemporary uses of *longue durée* in the history of science", Heiko Stoff has analyzed the question of (dis-) continuities in German science that straddle the political ruptures of 1914, 1918, 1933 and 1945 (*l.c.*, 144). Paraphrasing Stoff, the impression of a *longue durée* resulted in these cases from a perceived or supposed asynchronicity between a potentially continuous development of science and the background narrative provided by political history. Stoff also reported that in this context, the *longue durée* was not employed to address the reintroduction of big picture narratives, but referred to the problem of continuity and discontinuity (Stoff H., 2009: 146). In line with the suggestion made here, this suggests to understand the *longue durée* essentially as a problem of temporality and thus perceptions and comparisons of historical developments.

The suggestions that follow are certainly eclectic and heterogeneous. Presumably, this results not only from the tentative character of this paper, but from the fact that continuity, as described above, is not a monolithic phenomenon, but that it can be brought about in different ways. However, these cases at least display a sort of family resemblance in the sense that they refer to episodes in science's history going against a purely developmental narrative.

Banal but influential – Robert Koch's culture plates or the longue durée of simple methods and techniques

One does not have to be a microbiologist to gauge the relevance of the plate technique to grow, inspect and distribute microbes under standardized, aseptic conditions over more than a century of biological, medical and technological practice. Usually this technique is connected to the name of German bacteriologist Robert Koch, but the story is more complicated than that.

Research on microorganisms before the 1880s mostly relied on liquid culture media, which however made it difficult to grow clonal colonies, i.e. those stemming from one cell only. Mycologists traditionally used solid organic materials, such as gelatine or the surfaces of potatoes sterilized by cooking to cultivate fungi, and it was in the laboratory of German botanist/bacteriologist Ferdinand Cohn that such substrates were adapted for bacteria (Schlegel H.-G., 2004; see also Gradmann C., 2005: 60). Schlegel traces Robert Koch's 1881 approach to use gelatine plates for cultivation back to his experience in using gelatine for photographic plates and his acquaintance with mycologists' methods. The smooth, transparent surfaces of gelatine rapidly became the gold standard to cultivate all sorts of microbes, related to diseases or not. In fact, Koch's plates provided a veritable "display" for microbes – their cultures could be inspected in a transparent glass or plastic container from all sides, they could be transported and stored on the plates with out contamination. However, gelatine had the disadvantage of being liquefied by the enzymatic activity of many bacteria (which destroyed the fixed arrangement of the colonies). The introduction of a substance called agar-agar proved a better alternative. Prepared from algae and used as a jellying agent in food preparation, agar-agar was reportedly proposed by Fanny Hesse, the wife of Koch's collaborator Walther Hesse (Schlegel, H.-G., 2004).

Moreover, in 1887, R.J. Petri introduced a double glass dish as the default container of agar plates. The "Petri dish" has become an almost proverbial expression for the site of an experiment ever since.

The smooth surface of sterilized agar (which had the further technical advantage of solidifying at a lower temperature than gelatine, and thus being easier to pour into the plates when liquid) allowed distributing microbes in a Petri dish, either by pouring in liquid or by streaking a solid sample with a wire-based spatula. As a comparison of methodical texts shows, many of the simple, but crucial tools and manipulations to obtain such clonal or "pure" colonies of bacteria on plates seem to have been practiced in the early 20th century in a way very similar to that taught to undergraduate microbiology or biotechnology students around the globe today (see e.g. Kißkalt K. and Hartmann M., 1909; Madigan M., *et al.* 2012). Apart from the fact that Petri dishes are nowadays made from plastic, and innumerable variations of standardized media, sizes etc. exist, the general design of culture plates *à la* Koch, as well as the techniques and the rationale connected to them seem to have remained the same– in detail, this hypothesis would of course have to stand the test of a thorough historical analysis.

The concept of bacteria or later prokaryotes has been revised fundamentally since the late 19th century (remember, for example, that the problem of microbial heredity remained controversial until the post-war time). Moreover, microbiology has seen innumerable technological innovations and changes in work style over the last century – from the introduction of biochemistry to recombinant DNA or genomics. And yet, Koch's plate technique has formed a stable and influential framework for microbiological practice – from simple demonstrative experiments taught to students to routine diagnostics in medicine or sophisticated genetic and biotechnological interventions (Madigan M. *et al.* 2012; the use of such culture plates is central e.g. to basic routines of genetic engineering such as molecular cloning). It is presumably no exaggeration to say that culture plates have continuously shaped what microbes were and what they have been conceived of in the last century. Without this simple tool to grow, separate, manipulate, transport and stock these organisms, much of today's high-tech research and diagnostics would be hard to imagine. Culture plates have also contributed to the standardization of microbes in the laboratory. In fact, microbiologists over the last century have repeatedly critiqued culture plates for producing artificial life forms that would not be encountered in environmental samples (Doolittle W.F., 2013).[14]

The fact that Koch's plates remain relatively unchanged among the most important tools of microbiology in an age mostly linked to gene sequencing, synthetic biology etc. justifies to present them as a prime example to conceive of a *longue durée* of a simple scientific tool and technique.

Vintage science? The relevance of "old" instrumentation and techniques

The form of continuity presented by Koch's culture plates can be characterized *cum grano salis* as "timeless" for the simplicity of technique, the fact that the rationale behind has been materially realized in different forms (for example by using Petri dishes made from plastic instead of glass) and their ongoing significance. However, one could also look for examples of a *longue durée* of methods and instruments in which what one my call their 'systematic age' is more significant. The term systematic age, adopted from the work of historian of art George Kubler, refers here to the temporal relation of one series of artefacts to another (Kubler G., 2008: 35). Kubler discussed the ongoing presence of an artistic style in a period usually associated with a later style, but the case of a vintage car on a contemporary street might again be illustrative. Here, systematic age refers to a hiatus in terms of function, design or rationale, which is not necessarily congruent with chronological age.

To provide an example of what this could mean in scientific practice, we shall briefly sketch an episode from biochemical research in Germany, which is the continued use of the so-called Warburg apparatus after 1970. This case is certainly in many aspects singular, but further examples of ongoing uses of a technology that had been cutting-edge decades before, thus "instrumental oldtimers" and their phasing out would be worth analyzing.

14. Standardizations of scientific or medical routines, products and objects provide another broad theme that could be conceived of under the framework of *longue durée*, see e.g. Clarke/Fujimura (1992), Gaudilliere/Hess, 2008.

In the course of a historical study of the 1970s life sciences, the author was struck to find that in a time normally connected to the rise of molecular biotechnologies and recombinant DNA, developmental or neurobiology, a pre-War instrument, the so-called Warburg apparatus, was still actively used in research. In fact, the apparatus supplied important data on a very new research project (Grote M., 2013).

This instrument, named after the German cell physiologist and biochemist Otto H. Warburg (1883-1970), was based on manometry, that is, the measurement of pressure changes resulting from the production or consumption of gases in a closed reaction vessel. These are taken as parameters of biochemical reactions. Practically, substrates such as tissue samples or unicellular organisms were incubated in a glass flask connected to the so-called manometer. During incubations of several hours, and under varying environmental conditions (such as illumination, the presence of reactants in the liquid etc.), the pressure in the vessel was manually read out and the data on gas exchange were used for calculations based on chemical reaction kinetics.

The apparatus stands for cell physiology, photosynthesis and cancer research as carried out in Warburg's institute since the 1920s, or the study of intermediary metabolism by his former student Hans A. Krebs.[15] After the second world war, the Kaiser Wilhelm Institute for Cell Physiology in Berlin was re-integrated into the Max Planck Society. Warburg continued to work after retirement age until his death in 1970 with his established methods, encompassing also e.g. custom-made optical instruments, lenses etc. This experimental continuity was secured by technicians specifically trained in instrument-making and measuring. In 1972, the institute was dissolved and the Max Planck Society distributed its instrumental and human resources. One technician and a number of apparatuses and optical gear were transferred to the newly opened Max Planck Institute of Biochemistry close to Munich. Warburg's technician continued to work there until his retirement in 1991, and the Warburg apparatus was used for research purposes until the early 1980s. Since the 1970s, oxygen electrodes, a new device to measure gas exchanges were available, and generally research had taken new directions, such as bioenergetics, structural biology or recombinant DNA. Even if the Warburg apparatus was one technology among many others, it supplied important data, ironically for a research project that was considered 'cutting-edge' around 1970.

Beyond the local case related to the transfer of a technician and instruments within the Max Planck Society, there are some indications that the Warburg apparatus, a "classic" and very instructive biochemical instrument, has played a role in academic education when it finally became outmoded for research purposes – a German company produced the instrument until the 1980s (Grote M., 2013).

Different explanations could be tried for the continued use of an instrument of 1920-1930s biochemistry in the age of recombinant DNA, which of course lost its role as a 'modern' technique to that of an established and later on a phasing-out model. In the present case, the instruments and a skilled technician were simply available, and any economic or pragmatic consideration would suggest using them unless there were serious disadvantages. Also, the fact that these instruments were custom-made for specific uses could have enhanced their value, and even if Warburg's methods were highly disputed in the post-War time, the young biochemist then leading the project stressed the accuracy of the method in the hands of a long-time technician. Moreover, the adoption of an established technique could make it easier to compare data to previous results and thereby to establish reliability. For the mentioned case, one could also suspect that at least among the German community, where the Warburg tradition still emblematized an ideal of scientific rank and productivity, there may have been social reasons for adopting this old line of research into a new research project carried out by a new generation.[16]

It is interesting to note that the incorportation of the Warburg apparatus as an "instrumental dinosaur" in the 1970s did not mean that the new project was entirely relying on established methods or concepts. To the contrary, publications and laboratory notebooks reveal that many contemporary methods were used (such as protein sequencing, gel electrophoresis etc.) and the actual research

15. On the development of manometry, see the article by F. Holmes in Clarke A. and Fujimura J. (Ed.) 1992.

16. However, this latter point does not hold internationally, as Warburg had a bad reputation following the post-war controversy on the quantum requirement of photosynthesis (Nickelsen K. and Govindjee 2011)

object (a protein called bacteriorhodopsin) rapidly advanced to a new, hot topic in the context of bioenergetics and structural biology. Even if these latter aspects are mostly highlighted in recollections of bacteriorhodopsin research, the case illustrates how a project can encompass different layers of scientific time at once, and how instruments, approaches and objects of differing systematic ages can be combined. It is also notable that this form of experimental continuity was bound not only to the instrument itself, but in combination to a laboratory technician, an often neglected figure of science much less linked to innovation than to maintenance and routine (on technicians, see Shapin S., 2008)

Recurrent explanatory patterns or embracing concepts in the life sciences

Historians and philosophers of science have frequently debated whether modern science constantly provides new, refined or differing explanations of phenomena, whether these explanations are necessarily congruent, reducible to one another or not. What is less discussed and maybe perceived, or so it appears, is that and why certain motives of explanations remain around for extended times or that they reappear. One may readily object that if one looks into the details of an explanation, let alone its context, an historical analogy will quickly dissolve with respect to the encountered differences. There is no doubt that such differences exist, that they have sometimes been neglected by e.g. scientists in order to construct certain narratives (such as the infamous precursor), and that it is an important function of the history of science to reveal false continuities. Yet, would it not be possible to retain these insights and still to try to examine different forms of continuities in scientific explanations? Or, does it make sense to try to think of more generally embracing concepts ("conceptual brackets") in order to understand the development of science in time, the relationships of different projects, disciplines etc., as well as the problems and difficulties that research repeatedly encounters? Here, I will first provide an example of a case in which motives of an explanation in the life sciences have been repeatedly used to understand the biological phenomenon of ageing. Second, I will refer to an attempt to shape general categories to understand practice in the life and chemical sciences beyond the usual divisions provided by disciplines, research fields etc.

The general motive of explaining ageing by an intoxication of the body through a substance not present in its youth even predates modern science. Its first expression under the latter regime, however, was put forward by the Russian-French immunologist Ilya Mechnikov (Morange M., 2011). Metchnikov studied the process of ageing in various animals and tried to correlate both their anatomies as well as life styles to their different life spans. He rejected the assumption that ageing was a consequence of the decreasing proliferative power of cells (ageing by loss of function) in favour of the intoxication hypothesis on several grounds. Among these were the detection of specific substances in ageing tissues at the time, visualized as granules, and the observation by French cell biologist Alexis Carrel that cell proliferation *in vitro* required an exchange of the medium even if the nutrients had not been fully used up – thus one could suspect the accumulation of a toxic substance.

In the contemporary life sciences, a model exists to explain neurodegenerative diseases such as Parkinson or Alzheimer through the formation of so-called proteotoxic aggregates (*l.c.*). This model is supported by experimental data such as that mutations which prolong the life span of experimental animals also delay the formation of protein aggregates, or that certain drugs which prevent the formation of aggregates also delay ageing. Although protein aggregation has recently been termed a "paradigm for ageing" (*l.c.*: 2), the exact mechanism of how this process works in different cases has not yet been resolved, and neither has the effect of the proteotoxic aggregates been understood.

In spite of all differences in detail, Mechnikov's and the present model of ageing by intoxication appear similar, with the important difference that now a toxic entity has been physically identified (proteotoxic aggregates), and that for Mechnikov, the toxins partly originated externally to the organism. Interestingly, also the shortcomings of both explanations appear similar, namely the problem to reconcile them with an evolutionary understanding of ageing (i.e. to explain why natural selection has not countered the intoxication process).

The *longue durée* of ageing by intoxication, which comprises more than the two instances

mentioned above, becomes even more interesting if one contrasts the intoxication approach to a counterpart – the explanation of ageing by loss of function, e.g. through decreases of the cellular division, tissue regeneration and generally the capacity of organisms to react to environmental influences. Whereas 'negative' models of ageing, such as through loss, are more easily squared with evolutionary theory, 'positive' models of ageing are more likely to conceive of medical interventions, e.g. through inhibition of the intoxicating processes. Thus, a *longue durée* history of explications of ageing (which would straddle a century of bio-medical research, the upsurge of genetics and molecular biology, drastic instrumental changes etc.) might not only help to understand the debates on current models better, but potentially also to sketch the larger contexts and ramifications of these explanations – within and beyond science.

In the previous case, the repeated appearance of explanations of ageing has spanned times of science that we usually conceive of as far apart. In line with the *longue durée* conception sketched above, the similarity of the explanatory pattern contrasts with the assumed background narrative of scientific development. We imagine that this similarity will be surprising to many readers – at least it is to the author – and to a degree even conspicuous. How far does the analogy hold, and what is behind it? The same questions apply to a deliberate attempt to use embracing concepts (such as intoxication) to conceive of histories beyond the temporal divisions provided by disciplines, research projects, general history etc. When developing a historical "background panorama" for a collected volume on the history of biochemistry, Joseph Needham sketched such an approach (see Introduction of Hill R. and Needham J. (Ed.) 1970). His interest was to use general concepts such as "ferment" or "element" as a "perspective glass to show that their subjects [i.e., modern physiological chemistry or biochemistry] did not suddenly come into being like a set of extraordinary mutations, but rather that they grew like plants with roots coming up a very long way from the sub-soil of antiquity", not only in Europe of course (*l.c.*: viii). Needham then announced a number of concepts "almost as if they were entries in a kind of Voltairean '*Dictionnaire philosophique*'" (*l.c.*: ix) – in addition to those mentioned *pneuma*, humour, *krasis*, quintessence, elixir and conjunction.

Again, one may throw one's hands up in horror of over-generalizations and anachronisms - at least most readers of today will be surprised. There certainly is a feeling of alienation, or of the "contemporary of the non-contemporary" when connecting the ancient concept of *pneuma* with the modern biochemical assumption of certain gases serving as evidence for a potential emergence of life – up to the point where we perceive these similarities as so superficial to be ridiculous. Apropos the indelible traces of the "pneumatic heritage" on our thinking and language, Needham noted not without irony: "As I sat down to write these words, builder's tradesmen were loudly singing 'in high spirits' on the other side of the garden wall, and in the newspaper I had read of the evidence transmitted back by Mariner 7 that there is methane and ammonia – therefore perhaps *life* – on the plant Mars".

And yet, does Needham's suggestion not deserve a second look when trying to understand modern biochemistry as part of a larger stream of human activity taking organisms apart, describing their components and using them to act upon life's processes? Take quintessence or elixir: The extraction or preparation of active principles from biological materials and their usage in medicine remain central to biochemical and biomedical practice and form a bridge to scientific and mundane practices of earlier times. The danger of linking episodes that do not have much more in common than the use of a similar term is obvious. But there is also the possible benefit of learning from the *longue durée* "historical baggage" of concepts, the designated phenomena and the associated practices. *Virus* or *ferment*, for example, which sound far less alchemically tainted than Needham's categories, have remained in the 20th century's scientific vocabulary - could a *longue durée* history of such concepts tell us more about the growth of biochemistry beyond its disciplinary consolidation? In how far could such attempts be differentiated from the "thematic prejudices" or "themata" that Gerald Holton (1984) has suggested for a history of ideas in physics?

A recent history of vitamins, enzymes and hormones under the title of "Wirkstoffe. Eine Wissenschaftsgeschichte der Vitamine, Hormone und Enzyme" (*Active principles. A history of science of vitamins, hormones and enzymes*; Stoff H., 2012) has taken a step in the direction of shaping generic terms referring to both biological and

social phenomena to provide a novel framework within which to understand biochemistry, pharmacy and medicine far beyond a mere history of its ideas. *Topoi* such as "activation", "regulation" or "precarization" provide the structure of this history, thereby enabling the narrative to transcend both received periodizations as well as the nature-nurture divide. Such embracing categories would allow thinking of *longue durée* histories than run counter the preconceived time spans of disciplines, scientific frameworks, projects etc. These histories could reveal unexpected continuities and synchronicities within science, but also between science and the history of other human activities.

Larger scales: Fields, trends and styles and ideals of science

The examples provided above have incrementally moved from the concrete and material towards more conceptual aspects of science. Whereas the *longue durée* of Koch's plate technique or the Warburg apparatus would be very particular histories, patterns of explication or more embracing categories to describe the activity of certain research fields take a more general perspective, illustrating the contingent relationship between the *longue durée* and big pictures. I will conclude on an even more general level, that of research fields and finally what one could call ideals or styles of science. This last aspect brings us close to "big pictures", or at least generalist approaches, yet the focus is again on the temporal aspects in these stories.

Biological classification is an intrinsically conservative branch of science. One aim of classification has been at each point of time to establish an order of nature perceived as "scientific" by current methods of observation or experimentation. Yet, another goal, or at least a necessity for most researchers, has been the stability of their categories in the light of changes in science. That is, what is today conceived of as species A should only if absolutely necessary be conceived of as species B tomorrow, and what was yesterday species C should be congruent with today's species C. Especially in the 20th century, a whole range of tools, conventions and institutions have served the purpose of regulating classification and negotiating new data and requirements with the received nomenclature and categories. Even if one would presumably encounter similar situations regarding botany or zoology, I shall limit my sketch to microbial classification. Here, individual or institutionalized culture collections have maintained and distributed the living "type strains" of microbes (the analogue to botanical type specimens) for comparison and later analyses, "codes of nomenclature" have stipulated rules for legitimate naming, groups of experts have decided on contested issues and edited the relevant schemes for identification and classification.[17]

Classification seems an especially favourable terrain for *longue durée* phenomena. Type specimen, plant materials collected serving as material references to species, could be even said to embody the phenomenon at stake here. These dried, brittle specimen often from times of science long bygone, still play an important role have in order to decide on conflicts in classification in spite of all their material shortcomings.

A recent study reanalyzed type specimen of what are nowadays known as cyanobacteria (formerly classified as blue-green algae among the plant kingdom) by molecular methods in order to clarify the relationships of certain species. Practically, small samples of the dry cell powder in herbaria collected a century ago were taken to a laboratory, DNA was prepared and sequenced (Palinska K. *et al.* 2006). I argue not only that the role of type specimen, and the knowledge and practices connected in these, should be understood as a *longue durée* phenomenon in action, but that classification generally displays a temporal structure favourable for synchronicities of the "old" and the "new". To cite just one more example, a manual for experimental methods used in microbial classification from 1969 lists original protocols of the pre-War time, and one dating back as far as 1819 - long before Pasteur and Koch, and the modern concept of microbes (Skerman V., 1969). If today's researchers wanted to compare their isolate with these first descriptions of certain organisms, they would have to follow precisely these old protocols, thereby bringing these latter back into contemporary scientific practice.

There would be many more examples showing that the historical development of classificatory practice follows a different pace than the history of microbiology in the 20th century. Not only is there a time lag in the adoption of novel methods and concepts into classification, but there is a

17. See e.g. Daston L., 2004 (on type specimen), Sapp J., 2005, on Bergey's manual, see Murray R. and Holt J., 2005 – a history of microbial classification is work in progress.

special relationship to the own field's past as well. Classification is a privileged field to look for forms of continuity in science. The field seems to display different temporal patterns with respect to assumed narratives in the history of the life sciences (such as the impact and arrival of molecular analyses, or the relevance of phylogenetic, *i.e.* evolutionary classification). Classification displays an intrinsic *longue durée* structure as for the continuous reference to and re-negotiation of historical sources – original descriptions and nomenclature, data, methods or materials belonging to different layers of scientific time.

To conclude, I shall briefly note an interesting observation of the contemporary of the non-contemporary in a recent work dealing with the development of ideals or styles of rationality in science. In their book "Objectivity", Lorraine Daston and Peter Galison sketch the historical unfolding of what they call "epistemic virtues" since the 18th century that "re-write and re-image the guides that divide nature into its fundamental objects" (Daston L. and Galison P. 2010). The first of these virtues is "truth to nature", which favours representations of natural objects, e.g. in drawings perceived as characteristic. Through the expertise of a specific scientific author or illustrator, these were meant to reveal something about their objects' essence. In the 19th century, "truth to nature" was superseded by "mechanical objectivity", an often mechanical style of representation in which the subjective influences of the image's creator were to be avoided at all costs. Yet, "truth to nature" did not disappear completely – from a dominant virtue it turned to a particular, but retained its influence in specific fields (*e.g.* in botanical drawings) much longer than in other branches of science. This persistence of "truth to nature" seems a good case for a *longue durée* of a scientific style. Daston and Galison also report that those advocating the earlier virtue "too late" were often in a defensive position with respect to the community (*l.c.*: 43). It is intriguing to observe the place of this *longue durée* phenomenon in a work that, as the authors state in the introduction, "is about the creation of a new epistemic virtue", namely objectivity (*l.c.*: 16). In their words, the rise of objectivity has advanced through innovation and proliferation rather than a "monarchic succession" of epistemic virtues – that is, the "new forms did not abolish the old ones", but as new stars appearing next to old ones, they change the geography of the heavens. Clearly, the phenomenon or observation of a *longue durée* is present in this beautiful metaphor and it is not at the same time – as the new stars usually seem to shine brighter than the old ones, and the latter are rarely looked at.

And why look at *longue durée* phenomena?

It seems inevitable in the history of 20th century science to focus on development and change. Even if the notion of "revolutions" may have come of age, is it not obvious to study how new fields of science have formed, what impacts novel instruments, institutional structures or political frameworks have had etc.? Certainly, it is to a degree and, as stated above, I do not want to deny the obvious. Also, I am aware of the danger of over-generalizations and anachronisms – these have to be carefully scrutinized in every study again. So, why look at the contemporary of the non-contemporary, why study forms of duration such as persistences of the "old" and potentially residual, at re-appearances or continuities?

First of all, and here I have to again underline the provisional character of this paper, to describe such as episodes, contour different forms of continuity, thereby estimating their potential impact on science. It seems that e.g. in the history of the 20th century life science, the question of continuities on the experimental and conceptual level has rarely been asked, so there is no danger of overstating it. There are of course exceptions, such as the issue of standardizing in experimental and biomedical practice (see above). Yet, it seems that histories of the molecular life sciences, for example, are mostly structured by the new, by innovations and change accelerating the further one moves towards the present. There may be different reasons for this – from the simple fact the scientist's accounts have naturally privileged changes to more sophisticated ones. Possible continuities are often harder to detect in sources than innovations, as they may go unmentioned, and there may be fear of methodical anachronisms when telling such stories. Finally, there is the basic fact that histories need a narrative, and narratives are mostly based on development and differentiation.

Let me finally provide three connected arguments of why *longue durée* approaches in the temporal sense of developments seemingly going against the tide of time, by highlighting repetition, maybe even stasis or decline, should be of

interest. First, looking into the contemporary of the non-contemporary may help to establish more balanced narratives by getting less spectacular, development-centred episodes of science into the picture. Second, this would allow us to ask why certain patterns of explication or frameworks remain around or come back, and third, to think of what one could term "sustainability" in scientific practice.

The call for balanced narratives does not need much explanation. Without presuming the extent to which continuity has had an impact on science, it is simply a suggestion to take this phenomenon into consideration when conceiving of a project or working with sources, to note and to estimate the relevance of aspects such as the impact of an established technique or the return of an explanatory pattern. There might be several terrains to look for other forms of duration. One strategy could for example be to move away from the centres of research, from the hotspots to the periphery, or from principal investigators to technicians, and to gauge the impact of novelty at a given time. Also, the role of academic teaching could be studied and compared to the development of research.

Many stories thus told would result in rather mundane, but potentially surprising accounts of science – the role of simple *coup de mains*, of an omnipresent material or a technician versus the image of science driven forward by technological innovation and creative individuals. What has become of the ultra-centrifuge after it turned into a routine technique? What do today's models of ultracentrifuges have to do with their predecessors, where are they used and for what purposes? Here again, we move close to what David Edgerton (2008) has demanded for the history of technology, that is, to study technologies in use rather than focusing on certain moments of innovation.

The second point is related to the persistence or re-appearance of patterns in scientific explanations – what does this tell us about science? In how far is what looks similar really similar, where exactly lie the differences? Are continuities noted by scientists, do they refer to a mode of operation of scientific explanation and practice or to factors beyond the lab bench? What can one learn from the fact that reappearing explanations face similar problems, e.g. about the larger conceptual framework or functioning of science? For a history of science that addresses not only historians of science, but also philosophers and scientists, and maybe even broader audiences, such cases might be not only be enlightening, but well-needed. One could argue that in order to counter progressivist or 'developmentalist' narratives, a nuanced analysis of eventual continuities might be a better remedy than stating that what looks the same is actually not at all related and can be explained by totally different factors (on the fear of anachronisms and presentism, see Oreskes N., 2013).

Let me finish on a somewhat speculative suggestion. Not only the public perception, but also the historical picture of the 20th century life sciences is characterized by continuous and even accelerated development, by novelty and change on all levels. As many readers have presumably experienced themselves, there is a demand for a continuous acceleration regarding scientific work rhythms and productivity, with an increasing focus on "newness" and "innovation" (there are certainly various reasons for this development, which cannot be discussed here). Yet, as everyone who has ever worked in a laboratory knows, to do successful research (let alone academic education) it is not possible to rely on innovation alone, but continuity of technologies, themes, skills and personnel are also important. A good example is provided by the long-term experiment of evolutionary biologist Richard Lenski, which has been continuously running for 25 years since 1988 and which furnished important data on the process of speciation in bacteria (Pennisi E., 2013). Lenski's experiment has transcended the generational life spans of scientists and straddled important developments of the life sciences such as the introduction of genome sequencing. Thus, it is in itself a fascinating *longue durée* phenomenon in that the conditions and scientific background under which the study had been set up changed in the face of an ongoing experiment – turning the latter into an instance of the contemporary of the non-contemporary. Moreover, even if this experiment appears almost trivial from a technical point of view – involving "only" the cultivation, separation and banking of bacterial cultures, it has required a permanency of skilled staff, a fixed methodical canon and a well-sorted and kept archive of materials and prior data on time scales that most academic contracts and working conditions do not provide.

To borrow a term from current ecology and economics, one could speak of a "sustainability of research" that is needed not only to carry out

Lenski's experiment, and that seems to get out of sight in the way science is implemented and practiced today. Could not a focus on *longue durée* histories be productive as a means to correct the detrimental image of change, even breathless acceleration as science's sole mode of existence?

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