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Integrated History & Philosophy of Science

June 26-28, 2014

Fifth Conference

Abstracts

Integrated History & Philosophy of Science

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Invited speakers

Jane Maienschein
(Arizona State University)
Looking at Cells around 1900: Seeing Complex Systems

Jürgen Renn
(Max Planck Institute for the History of Science Berlin)
On the Evolution of Knowledge: From Cooperative Action to Science

Jean Gayon
(Sorbonne)
Natural Selection vs. Descent with Modification: What Comes First?
Reflections on Darwin and Sober

Introspection and the Problem of the Stimulus-Error: Historical and Contemporary Debates

Mazviita Chirimuuta (University of Pittsburgh)
Uljana Feest (Max Planck Institute for Human Development)
Gary Hatfield (University of Pennsylvania)

General Abstract
A basic method of perceptual research is that of presenting subjects with stimuli and obtaining responses about the resulting perceptual experiences. But what exactly is the nature and status of such responses? Are they introspective reports about one's own experiences, or are the reports about the objects of experience? Should they be relied on as accurate? Can this be decided at all; and if so, by what standards? And what assumptions need to be in place to regard such reports as providing data about perception? These questions were debated fiercely amongst philosophers and experimental psychologists in the 50 years following Fechner's *Elements of Psychophysics* (Fechner 1860). Debates revolved around an issue that Edward Titchener (1905) termed the "R-error," though it subsequently became better known as the "stimulus error." Roughly, this expression referred to the problem of mistaking reports about the stimulus for reports about a subjective sensation. However, it is not entirely clear (a) what the error is, precisely, (b) whether it is really an error, and (c) what methodological steps should be taken to avoid it (see Boring, 1921, for an early discussion).

We argue that the conceptual, philosophical, and methodological problems encapsulated in the concept of the *stimulus error* are still highly relevant not only to contemporary research in the science and philosophy of perception, but also to the epistemology of experimentation (e.g., Hon 1989). Building on previous work (Chirimuuta in press; Feest in press, Hatfield in press), the contributors to this panel will take a new look at the old debates in the light of recent philosophical interest in the use of introspective data, and we will analyze recent questions and debates in the light of our analyses of the historical material.

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Mazviita Chirimuuta

The Stimulus-Error, "Equivocal Correlation" and Perceptual Constancy

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Boring (1921, 451) writes that, "[w]e commit the stimulus-error if we base our psychological reports upon objects rather than upon the mental material itself, or if, in the psycho-physical experiment, we make judgments of the stimulus and not judgments of sensation." Titchener (1910) and Boring (1921) both argue that the stimulus-error is indeed a serious methodological pit-fall. While some of the theoretical suppositions motivating their arguments—a rigid separation of sensation from perception, and their characterization of psychology as the measurement of purely mental phenomena—are currently unfashionable, one aspect of the stimulus-error debate is of perennial importance to psychophysics and the psychology of perception. This is the idea that the stimulus-error is a source of unwanted variability in subjects' responses, but one which can be controlled for by careful training of subjects and judicious use of experimental instructions. In this paper I propose first to discuss Boring's presentation of the problem of unwanted variability ("equivocal correlation") in haptic perception and then to examine the issue in relation to recent experiments on lightness and colour constancy. Discussion of the stimulus-error sheds light on the ongoing debate about how best to measure constancy phenomena and reveals some of the conceptual fault-lines within perceptual psychology past and present.

In the concluding section of his 1921 article, Boring makes the case that the stimulus-error is not exclusively the concern of adherents to the "psychology of datum" (i.e. those using introspectionist methods), but is also of concern to the "psychology of capacity" (i.e. behaviourism). He writes that:

the effect of the "stimulus-error,"s from the point of view of a psychology of capacity, is … to render the correlations between stimulus and response equivocal and thus to jeopardize the rigor of conclusion that science demands. (Boring 1921, 465-6)

His primary example is the measurement of the tactile two-point threshold—the measurement of the minimum distance between two pressure points on the skin which reliably gives the impression of two separate stimuli. In such an experiment the psychologist of capacity is only concerned with the stimulus and verbal report. However, Boring observes, the relationship or "correlation" between stimulus and report is variable ("equivocal") due to differences in intermediate factors of attention and "attitude" or criterion, i.e., whether the subject's report reflects her sensory state or her judgment of the stimulus (see Figure 1). As Boring (1921, 470) writes, "the failure to control the attitudinal factor…results perforce in an equivocal determination of these responses, which is nothing more nor less than a 'stimulus-error'".

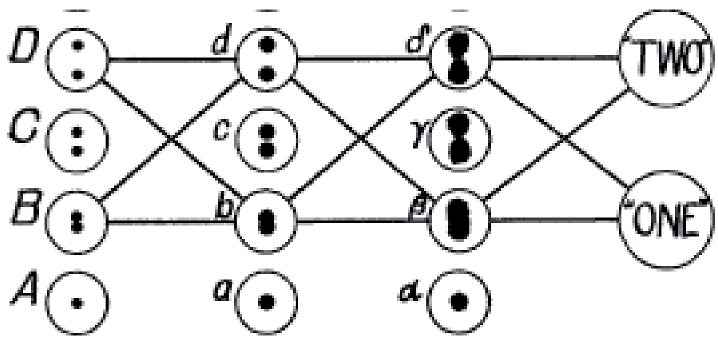

Figure 1 (from Boring 1921, 466)

Boring's concerns about the effects of shifting response criteria were in some respects met by the development of *signal detection theory*, a set of techniques used by psychophysicists to estimate the discriminability of stimuli regardless of the subject's response bias. On the other hand, some well known experiments on colour constancy have exploited, to good effect, response variability due to the difference between stimulus and sensation reports.

Colour constancy is often characterised as the stability of colour *appearances* (the hue and saturation that objects appear to have) despite changes in ambient illumination. However, changes in illumination do cause noticeable changes in colour appearances so it is open to debate whether colour constancy is better characterised as the ability to match coloured *stimuli* across changing illumination. Arend and Reeves

(1986, 1743) criticised an earlier study by McCann et al. (1976) on the basis that the task performed by their subjects was open to either interpretation. In their own study, Arend and Reeves gave their subjects two different kinds of instructions: either to match hue and saturation or to match stimuli so that they looked as if made from the same colour paper. Arend and Reeves report that in the first task (sensation reports) subjects showed little colour constancy, whereas for the second task (stimulus reports), subjects showed approximate colour constancy.

One might conclude that while Boring and Titchener were right to draw attention to the stimulus-"error" as a source of response variability, their strictures against stimulus reports are unfounded; indeed, it may be the case that certain perceptual phenomena, like colour constancy, are *better* measured through stimulus reports. However, that would be to ignore an on-going controversy about whether all such stimulus reports are genuinely *visual* and not, rather, judgements or inferences made by subjects about the likely source of stimulation.

For example, Robilotto and Zaidi (2004) performed a series of experiments on lightness constancy in which subjects were required to determine which out of four stimuli presented under different illumination conditions (see Figure 2a) was the odd one out due to a different surface lightness (see Figure 2b for correct answer).

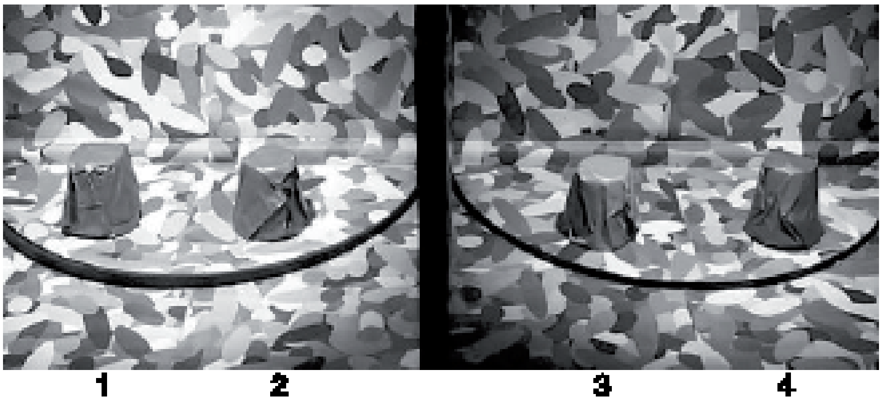

Figure 2a

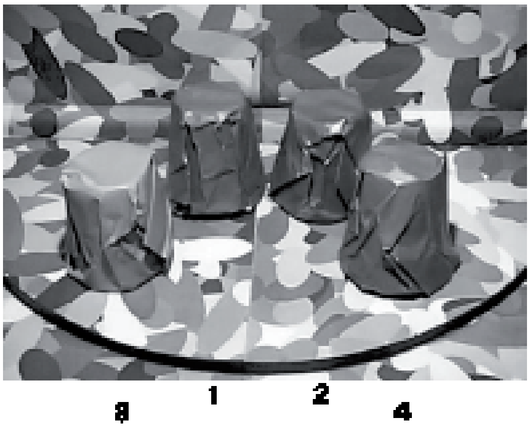

Figure 2b (from Robilotto and Zaidi 2004)

Now even though Robilotto and Zaidi's task instructions were such as to prompt a stimulus report, significant response variability was still observed. The majority of subjects' data was consistent with them using a strategy based on sensation matching, whereas two subjects' data suggested that their responses were based on their inferences about the likely stimuli rather than perceptual experience *per se*. Robilotto and Zaidi (2004, 792) write that, "[i]ndividual differences thus are likely to be due to attempts to infer a non-sensory quality, rather than due to the particular task or instruction." What is striking is that Robilotto and Zaidi's analysis rests on a robust sensation-perception distinction, and a suspicion regarding non-sensory reports, that is controversial amongst constancy researchers (Chirimuuta 2008, 578). Thus, I will argue, in this modern iteration of the stimulus-error debate we come full circle back to Boring and Titchener's initial concern to demarcate the appropriate phenomena for the psychology of vision.

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Stimulus Error and the Red Herring of Introspection

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Prior to the early 20th century, a lot of empirical research in psychology concerned itself with descriptions of consciousness, and it was commonly assumed that one could arrive at such descriptions by relying on subjects' reports about the experiences they had when exposed to particular stimuli. In this vein, the tradition of psychophysics in the psychology of perception aimed to formulate laws that would capture the relationship between physical stimuli and the ways in which they were experienced (e.g., Fechner 1860). This raised important methodological concerns, however: On the one hand, one needed independent measures of both stimuli and experience in order to formulate the functional relationship between them. On the other hand, experiences of stimuli could only be accessed by presenting subjects with stimuli, raising the question of whether the description of the experience was potentially contaminated by that of the stimulus. It is this worry that Edward Titchener (1905) addressed when coining the expression "stimulus error." By this expression he meant both (a) the error (on the part of experimental subjects) to mistake descriptions of experienced objects for descriptions of the experience itself and (b) the error (on the part of the experimenter) to treat their experimental subjects as reliable reporters of their own experiences, hence introducing a particular kind of measurement error into psychophysical experiments (see Chirimuuta's contribution to this panel). His proposed solution to this problem was to provide subjects with instructions that would minimize the stimulus error by maximizing the veridicality of their introspective reports (Titchener 1905; Schwitzgebel 2011, ch.5. For the notion of instruction see Hatfield's contribution to this panel).

In my talk I will argue that while the notion of a stimulus error continues to pose intriguing philosophical puzzles, Titchener's attempt to address it by means of a training manual for experimental introspection has produced something of a historical and philosophical red herring insofar as it has created the impression that the problem of the stimulus error is related to introspection per se. On the historical side, this assumption has obscured the recognition that (contrary to behaviorist rhetoric) the main point of contention with regard to Titchener's approach was not his introspectionism, but his structuralist conception of psychology (see also Hatfield 2005; Beenfeldt 2013). On the philosophical side, it has obscured the significance of this issue to at least two topics in current philosophy of science, concerning the role errors play in investigative contexts (see Hon et al. 2009; Alchins 2001; Mayo 1996) and the relevance of the stimulus error to areas of psychological research other than perception or consciousness.

While Titchener may have coined the term "stimulus error," the worry that we read features of stimuli into the experience was articulated by others as well. Two versions of this worry were the following: First, in response to Fechner's psychophysical program, many pointed out that the measurability of the intensity of a stimulus does not imply the measurability of the intensity of an experience (Boring, 1921). Second, Gestalt psychologists argued vehemently that one should not assume a one-to-one correspondence between elements of stimuli and elements of experiences (Gestalt theorists referred to this assumption as the "mosaic hypothesis"). I argue that both of these points express a concern about committing a stimulus error. However, the Gestalt psychological articulation did not call for more accurate introspection and clearly pulled into an entirely different direction from Titchener's articulation, to the point that Titchener and the Gestalt psychologists would effectively accuse each other of committing a stimulus error. This shows, I will argue, that the disagreement lay much deeper and could not be fixed by providing adequate training for experimental subjects. The real issue was what were appropriate types of stimuli, isolated elements or holistic configurations? This was a theoretical disagreement, which determined the actors' views about experimental methods.

I will argue that my historical analysis affords us insights into (a) the very notion of a measurement error, and (b) the problematic of making inferences from features of experimental tasks to features of the mind. In elaborating on the first point, I will draw on existing literature about the role of errors in experimental science. Deborah Mayo (1996), for example, has argued that scientific knowledge generation consists not only in theory-testing, but also in probing for errors, which can be deeply engrained in some of the very conceptual and material assumptions required in order to run an experiment (see Alchins 2001). My historical case study gives some indication of how difficult this can be. In elaborating on the second point, I will argue that there is a structural similarity between the worries about stimulus error we find in the 19th and early 20th century and more recent considerations (both in psychology and philosophy of psychology) of the question that while it is important to analyze the tasks required of experimental subjects, this does not imply that there is a mental module that is specifically designed for this kind of task (e.g., Bechtel 2008). I will argue that this is a modern-day version of the concern about stimulus errors.

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Gary Hatfield

The Stimulus Error and Experimental Design: The Manipulation of Perceptual "Set"

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The psychologist E. B. Titchener is credited with introducing the notion of "stimulus error" into experimental psychology. As discussed by Boring (1921), the term has several applications. Its primary meaning, as in Titchener (1905, xxvi), characterizes the "error" that occurs when subjects in experiments directed toward sensations use their beliefs about the physical stimulus in making their responses, rather than reporting the phenomenal attributes of sensation:

We are constantly confusing sensations with their stimuli, with their objects, with their meanings. Or rather – since the sensation of psychology has no object or meaning – we are constantly confusing logical abstraction with psychological analysis; we abstract a certain aspect of an object or meaning, and then treat this aspect as if it were a simple mental process, an element in the mental representation of the object or meaning. (ibid.)

The "error" results when, instead of holding object-perception and meaning in abeyance, the subject abstracts an object-content from perception and reports that content. Titchener gives examples from auditory, gustatory, and haptic perception but also alludes to the tendency in visual spatial perception to overlook sensations (which correspond to "peripheral cues") in favor of objects arrayed in space (1909, 314). According to him, the elemental sensations of vision are bidimensional and we acquire perception of the third dimension (1909, 303–6). Boring (1921, 462–3) describes an instance from size perception, deriving from Martius (1889), of the need to direct experimental subjects to respond to the *apparent* sizes of rods at various distances rather than their *actual* sizes (to which subjects normally attend).

Viewed in one way, the notion of a stimulus error belongs to an outdated viewpoint that draws a hard-and-fast distinction between sensation and perception. Accordingly, sensations are pure states of sensory effect, devoid of interpretation and meaning. In vision, they correspond to the retinal image. Philosophically, they may be equated with now discredited "sense data." Such sensations are mistakenly posited as what we find by "introspecting" or "turning inward"; but, in fact, we find nothing by looking inward. In reflecting on seeing, we only find the world out there. Subjects who are directed to introspect are right to report only on the object, because there is nothing else available.

This response accords with present-day philosophical accounts known as naive direct realism (or plain "disjunctivism") and content physicalism (or the pure informational, intentional, or representational theory). Such positions deny subjective intermediaries in vision and point to the "transparency" of perception, its world-presenting character, as a refutation of the older view of introspection ascribed to Wundt and Titchener (as "structuralists").

As it happens, these more recent responses are not well-attuned to the actual practices and debates that surrounded the phenomenon of stimulus error. Moreover, the aspects of these recent positions that would discredit the notion of subject-dependent aspects of perception are heavily theory-dependent: they rely on contentious analyses of the relation between perception and its objects and make too easy an inference from phenomenal "transparency." In this way, they partake of a feature of the earlier discussions that I want to highlight: the interplay between experimental design and theory. Other theorists besides Titchener found differences when subjects were asked to report on phenomenal aspects of experience as opposed to actual object properties (perhaps without finding "error" in the latter or treating the former as elemental sensations). By comparing Titchener's notion of sensation with other outlooks, I show how different theoretical stances yield different conclusions about the legitimacy of experimental protocols aimed at uncovering subject-dependent aspects of perception.

Although Titchener believed that sensations are the primitive (unanalyzable) elements of mental life, for him even the seasoned introspector does not experience unvarnished sensations. Rather, we discover the properties of sensations by establishing conditions for isolating them and then reporting introspectively on their attributes, such as quality, intensity, or duration, all of which cannot be attended at once or made the subject of a single report. From his point of view, if one succeeds in focusing on the pitch of an upper partial tone in a musical note, one has noticed an attribute of an element that was present in the tone all along. Still, the notion that there are primitive sensations that compose complex experiences comes from theory (Titchener 1915).

Various investigators who were sympathetic to phenomenal reports, from James (1890) through Gibson (1950), accepted that one begins from unitary phenomenal experiences that are as of a scene or sound in the world (phenomenal "transparency"). They then applied diverse procedures in studying aspects or attributes of such experiences. How they conceived the experimentally determined attributes depended on their theoretical outlooks. Some theorists, including the Gestalt psychologists and Gibson, held that in vision the experience of a three-dimensional visual world of objects is not only phenomenally immediate by psychologically primitive, as is an ordinary tone. Accordingly, one experiences the upper partial tone, or a bidimensional visual field, by adopting a special attitude that does not uncover a pre-existing element but produces a new, secondary sort of experience in place of normal experience. Nonetheless, such theorists allowed that experimental investigations can be conducted by attending to phenomenal attributes or dimensions of normal experience, in abstraction from other attributes and meaning. Thus, one might attend to sizes, distances, or shapes as attributes within visual experience.

This paper explores the interplay between experimental protocols and theoretical outlooks in relation to "stimulus error." From the time of Martius (1889), experimenters used instructions to invoke specific perceptual attitudes in subjects. Subjects might be asked to attend to "apparent" size or to judge the "objective" physical size of objects. The latter task does not produce an "error" but simply a different perceptual response. By examining the use of instructional protocols by Fernberger, Brunswik, Boring, and others in the investigation of size and shape perception in vision, I seek to determine whether they see the differing responses under differing instructions as resulting from (1) changes in phenomenal experience due to a change in task; (2) access to different aspects of a unitary phenomenal experience; or (3) access to distinct phenomenal and conceptual dimensions of experience. The answers can be related to differing philosophical analyses of the perception-object relation, including naive realism and content physicalism as above, but also critical direct realism and appearance theories, in which objects are presented via subject-dependent aspects of experience.

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Scientific Discovery: Historical and Philosophical Dimensions

Thomas Nickles (University of Nevada)
Samuel Schindler (Aarhus University)

Scientific Discoveries are a generic part of the "telling" of science, but discoveries themselves are fundamentally difficult to individuate as events. Their identification, demarcation, and delimiting involves historical and philosophical considerations, making discovery a prime topic for this conference. Moreover, it is controversial whether history or philosophy is the appropriate domain of study of 'discovery'.
In the proposed symposium presenters will re-examine cases and critique standard analyses of discovery, aiming for an integrated understanding.

Thomas Nickles
Scientific Discovery and the End-of-History Fallacy

Strong conceptions of scientific discovery (in the broad sense of creative work at the frontiers of research) are linked to strong conceptions of historical change. Contrariwise, impoverished conceptions of both scientific discovery and the history of science yield conservative accounts of scientific work. One form of historical impoverishment, of which even strong historicists can be guilty, is a truncated conception of history that fails to include future history. The difficulty, nay impossibility, of concretely visualizing future historical change leads even sophisticated thinkers to commit what I term "the end-of-history fallacy," analogous to the mistake made by deeply historical thinkers such as Hegel, Marx, and Fukuyama. Traditional history of science made us realize that the development of the sciences until now has been a highly dynamic enterprise. But traditional history ends at the present, and we need means to make the unrealized future come more alive for its creative, hence dynamical possibilities. A better appreciation for the nature of creative work at the frontiers of research – a better understanding of what we might call frontier epistemology – suggests that even the supposedly mature sciences may experience a long- term, highly dynamic future. Such a view has implications for the scientific realism debate as well as for science policy and the public understanding of science.
Section 1 of the paper is plea to take history of science seriously once again, indeed, even more seriously than in the 1960s and '70s, when 'history of science' usually meant 'the past of science' rather than considering that past as only the possibly raw beginnings of time series of developments that may last for many millennia beyond the present. Although it sounds oxymoronic, I shall include the history of the future as well.

Following original work on the history of mechanics by Koyré, Butterfield famously contended that each modern science began with a founding revolution. In The Structure of Scientific Revolutions Kuhn went further to argue that there have been later revolutions as well, re-foundings in a sense, and that in the mature, hard sciences, later revolutions without end are almost inevitable. (Others have since pointed out other kinds of transformative spurts than the Kuhnian variety.) Kuhn is one of the few analysts to project such a dramatic future dynamic of science. Given the expansion of scientific domains, the tightening of linkages, and the nonlinearity of the internal dynamics of science (that even a seemingly normal result can eventuate in a transformation), future Kuhnian revolutions might even become larger rather than smaller.

Section 2 distinguishes several different concepts of mature science and points out crucial tensions be- tween retrospective and prospective accounts of maturity. By contrast with Kuhn some strong realists hold that mature sciences are both highly creative yet not likely to undergo significant transformation, or at most a series of ever-smaller ones that converge on the truth. To argue, as some realists do, that today's sophistication can easily handle the research problems of past frontiers, overlooks the fact that living sciences constantly generate new frontiers that are at least as difficult as the old ones. At these frontiers the big questions usually involve decision-making under extreme uncertainty rather than merely under risk.

Section 3 relaxes the assumption that a significant future dynamic must be revolutionary. Kuhnian revolutions and other sorts of rapid spurts are not necessary to imagine that future mature science may well transform itself almost beyond recognition. After all, given enough time, gradual evolution can achieve transformations as radical as you please. Further, as in the case of biological evolution, it is arguable that, over a plausible range of conditions, the future evolution of science is inevitable – and will be much faster. The usual historical-cultural time-scale begins to look rather arbitrary (even presentist in a sense) when we consider the future as extending out to, say, 40,000 years of creative scientific research, as compared with the 400 years since the beginning of the Scientific Revolution. On the strong realist view (which also cannot be proven wrong), these first few centuries will, centuries hence, be known as The Age of Scientific Discovery, a project essentially completed.Section 4 briefly sums up my "deep history" and "deep discovery" positions in terms of a set of interpre- tations of Mary Hesse's "principle of no historical privilege" and some reminders about changing human interests, goals, and human creativity.

In Section 5 I claim that many analysts, including philosophers of science, commit an "end-of-history fallacy," deriving from the difficulty, nay impossibility, of envisioning a distant future of science. The fallacy often involves a cluster of questionable assumptions, including a conflation of different senses of 'mature science' and an insufficiently prospective analysis deriving from our limited horizons of imagi- nation. Insofar as maturity implies that the main period of discovery is over, it would seem that maturity claims announce the end of the History of science ('History' meaning the universal sense of 'history'). The fallacy is committed by people who assume, without adequate argument, that the future will be rel- atively "flat," i.e., not dynamically interesting, not highly nonlinear, that the future expansion of mature science will consist mostly of routine specialization and "translational" work, a sort of normal science "flatline." Often this assumption is a default assumption that remains implicit, by an author's simply failing to consider seriously the possibility that the future may be interestingly creative and dynamic.

Section 6 briefly rejects basic objections to the above, namely, that I am a global antirealist whose use of the end-of-history fallacy marks me as a global skeptic in matters scientific and that my own views on heuristic appraisal undermines my position. Heuristic appraisal is evaluation of the future fertility of anything, and sometimes can legitimately judge a given specialty area of be essential finished and hence sterile of further significant discoveries. Hence the objection.

Section 7 concludes the paper by briefly pointing out some implications for public understanding of science and for policy, including the way granting agencies are run. Highly optimistic philosophies of science claiming that mature science has nearly reached its ultimate goal (whether strongly realist or not) can discourage investment in long-term, potentially transformative projects. End-of-history fallacies may contribute to the conservative granting policies that currently plague institutions such as the U.S. National Science Foundation. The overall message of the paper is that, despite our limited horizons regarding the future, we philosophers must be more prospective in our thinking.

Samuel Schindler

Scientific discovery: that-what's and what-that's

What is a scientific discovery? T.S. Kuhn (1962b, 1962a) claimed that a discovery always involves not only a discovery-that (the observation of the discovered object) but also a discovery-what (the correct conceptualization of the discovered object); one without the other is insufficient for a discovery. Kuhn also distinguished between two broad classes of discovery: discoveries in which the discovery-that is being made before the discovery-what (one may refer to those discoveries as that-what discoveries), and vice versa, discoveries in which the discovery- what is being made before the discovery-that (what-that discoveries).

Each class of these discoveries comes with distinctive features, whereby the former Kuhn considered the more interesting ones.

This paper will defend Kuhn's distinction between the two types of discovery and their characteristics against alternative accounts of discovery proposed by Achinstein (2001), Hudson (2001), and McArthur (2011). It will be argued that these alternative accounts are inappropriate, in large part, because they have fallen behind Kuhn's insights. Yet there some aspects in Kuhn's account of discovery that are vague. This paper will seek to make these aspects more precise.

For T.S. Kuhn, "discovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both that something is and what it is" (Kuhn 1996, 55). It would be a mistake, according to Kuhn, to "assimilate" discoveries in science to the (naively construed) act of seeing or to s other sense perceptions (ibid.). Rather a discovery, for Kuhn, not only involves the observation of an ob- ject, but also the correct conceptualisation of that object. Kuhn's main example for illustrating this point is the discovery of oxygen. Although Joseph Priestley was arguably the first to have isolated oxygen, he did not conceptualise it correctly. Rather, working within the theoretical framework of the phlogiston theory, Priestley thought that he had discovered dephlogisticated air, i.e., air that depleted of phlogis- ton. Lavoisier, according to Kuhn, can not be said to have discovered oxygen either, because also his conception of oxygen was mistaken: he believed that oxygen gas was a combination of oxygen (i.e., the 'principle' of acidity) combined with caloric, the (non-existent) matter of heat. On the other hand, without the requirement of the correct conceptualisation of the thing discovered, we would have to say that oxygen was discovered by anybody who ever bottled impure oxygen since Priestley himself did not manage to isolate a pure sample of oxygen (54). All we can say then, according to Kuhn, is that oxygen was discovered sometime in the period of 1774 until 1777. More generally, discoveries are "not isolated events, but extended episodes" where it is largely arbitrary to identify any one scientist as the discoverer of a scientific object (ibid., 52).

Interestingly, in a paper published in Science in 1962 (reprinted in 1977), which formed the basis for chapter six of The Structure of Scientific Revolutions (published in the same year), Kuhn made a distinc- tion between two basic kinds of discoveries (which he no longer makes explicitly in The Structure). In one kind of discovery, the conceptualisation is carried out before the object in question is being observed: these are classic cases of prediction, such as the discovery of missing elements in the periodic table, the neutrino, and radio waves (1977, 166-7). But given that that these discoveries were anticipated (usually, but not always, on theoretical grounds), they are "an occasion only for congratulations, not for surprise"; they are thus prime examples for normal science activity, which does not aim for surprising novelties (Kuhn 1996, 58). In contrast, in discoveries of the second kind, the conceptualisation of the thing dis- covered usually follows the observation of the thing discovered. Those discoveries may be referred to as that-what discoveries. It is those discoveries that Kuhn considered "troublesome" and which he made the main focus of chapter six in The Structure.

According to Kuhn, that-what discoveries have sharply distinct characteristics from what-that discoveries. Whereas what-that discoveries can be instantaneous with regard to the incidence of the discovery-that (1977, 171), only rarely give rise to priority debates (166-7), and where, accordingly, "only a paucity of data can prevent the historian from ascribing [discoveries] to a particular time and place" (167), the contrary is the case in that-what discoveries. In that-what discoveries (such as in the discovery of oxygen), there necessarily is a time-dimension to discoveries, for it simply takes time to conceptualise a thing for which one had no, or only an inapppropriate, conception at the time of observation (1996, 55). The necessary time dimension of that-what discoveries, regularly involving several individuals, is therefore a major reason for why an attribution of a discovery to any one individual is "often impossible" and to a moment in time is "always imposssible" (55). In that-what discoveries there are thus "no benchmarks to inform either the scientist or the historian when the job of a discovery has been done" (1977, 167). Although not all discoveries may fall neatly in either the what-that or the that-what category (ibid., fn on 167), Kuhn clearly thinks that most cases do. Whether that is the case or not will not be decided in this paper. What this paper will confirm, though, is that there are important cases of discovery that are well-captured by Kuhn's account and in fact much better than by alternative accounts of scientific discovery.

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Sensing the Unknown: Historicising the Discoverability of the Olfactory Receptors within the Life on an Experimental System

The notion of scientific discovery is traditionally associated with the introduction of novelty into a scientific discourse. It has been central to the philosophy of science, especially in relation to the concept of evidence and the context of justification. The topic of this paper is a discovery that has not been dealt within philosophical debate and that poses an interesting case to formulate another notion of discovery as pertaining to historicity and procedures of epistemic iteration integral to scientific practice, rather than novelty. The case with which I am concerned with is the discovery of the olfactory receptors and its role in the life of an emerging experimental system surrounding the molecular basis of smell perception.

A considerable debate in olfaction theory surrounds the yet unknown mechanism of primary odour recognition. It is hoped that this mechanism will explain why a particular molecule has a particular smell. For almost the entire 20th century any hypothesis about the molecular basis of odour perception remained speculative, simply because the receptors were unidentified. Nonetheless, being considered as part of a wider group of ligand binding processes such as digestion, metabolism and immune responses, primary smell perception was assumed to act according to a shape-sensitive mechanism. Demonstrating the adequacy of this hypothesis appeared to be a local scientific problem and subject to further advancements in technology and measurement. With the discovery of the olfactory receptors (ORs) by Linda Buck and Richard Axel in 1991, the key element for research on the olfactory mechanism was identified at last. Knowing what kind of protein is associated with olfactory responses, it was believed, should enable us to identify what kind of perception mechanism is at work. Fast forward to the present day, however, and insight into the details of the recognition process has not improved greatly. The problem is the experimental inaccessibility of the OR binding site. Studies of transmembrane proteins are notoriously difficult and only very few breakthroughs in elucidating the structure of their binding sites have been made. ORs present a particularly difficult case as standard methods of crystallisation, an essential requirement for protein modelling, have so far been unsuccessful.

Nonetheless, this discovery had important implications for further olfactory research, because it identified smell receptors as a class of 7 transmembrane G-coupled proteins, which strongly suggested that molecules (causing a particular odour) dock on a specific primary receptor according to some kind of shape-sensitive mechanism. It was the background of advancements in genetics and growing experimental evidence for an involvement of a G-coupled protein that paved the way for this groundbreaking discovery. Previous studies on olfactory responses already indicated the presence of cAMP (cyclic AMP), a messenger molecule that activates ion channels when a cell is activated. Because of its function of stimulating the formation of cAMP, the involvement of a G-coupled protein was considered to be likely before its ultimate discovery. Although G-coupled proteins take part in a variety of physiological processes, ranging from vision to the regulation of behavioural and immune responses to digestion, those proteins active in chemical ligand binding were all considered to act according to a shape-sensitive mechanism. For this reason, the theoretical implications of Buck and Axel's discovery were not a complete surprise but, rather, reflected orthodox opinion about primary smell perception, which had always taken aspects of molecular shape to be the key feature responsible for odour detection.

It is against the background of the trajectory of olfaction theory that I will analyse the role of this discovery within the life of an emerging experimental system. To understand how this discovery was made and, moreover, to further show how it relates to the past and future course of olfaction theory, I will trace the reasoning that governed the methods and interpretations and that fostered a laboratory culture most integral to turn previously dispersed olfactory studies into an organised modelling context. Drawing on Hans-Jörg Rheinberger's notion of an "experimental system" and Hasok Chang's concept of "epistemic iteration", I will trace the gradual entrenchment of conceptual assumptions and experimental strategies underlying Buck and Axel's search for the olfactory receptors. The thereby outlined reasoning resonates with the concept of "discoverability" as introduced by Thomas Nickles. Discoverability describes a process of generative justification, meaning a rational reconstruction of the strategies involved in the path of discovery. In contrast to "discovery" as the original generation of, for instance, theories or hypotheses, the concept of discoverability is also related to the context of justification. Whereas discovery is understood historically as a particular event that resists methodological generalisation, discoverability reflects its post hoc rationalisation that needs not to coincide with the original actions undertaken.

The aim of this paper is to present an argument why not only the event of discovery but also its epistemic reconstruction within justification strategies needs to be historicised. The question by which I am going to address the historicity of discoverability is as follows: *what* is it *for which* a discovery is reconstructed in terms of its discoverability? Rather than primarily justifying a theoretical framework, I claim, the impact of the olfactory receptor discovery lies in its historical and changeable role within the life of an experimental system. Discoverability as a rational reconstruction implies the question for what exactly it is supposed to provide a generative justification. The purpose of such a narrative, however, is dependent on the stage of scientific discourse within which it is placed and, given the growth of knowledge, subject to revision as well. The presented genealogy of a discovery as related to the live of an experimental system will aid me to further explore what a historicised perspective on discoverability implies for philosophical analysis of scientific discoveries.

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Mathematical Theory, Natural Experiments and Ovarian Dissections. The Epistemology of Hamilton's work on Tropical Social Wasps (1963–1968)

W. D. Hamilton's *The Genetical Evolution of Social Behavior I and II* published in 1964 are the two founding papers of Sociobiology. In these works, Hamilton famously exposed his theory of Inclusive Fitness about the origins of altruistic behavior. Part I outlines the mathematical features of the theory; whereas Part II shows how the theory applies to concrete biological cases, mostly social insects. In recent years, historians and philosophers have focused on the theoretical significance of Inclusive Fitness as well as on Hamilton's theoretical/mathematical approach to social evolution. However, before and right after the 1964 publications, Hamilton engaged in extensive naturalistic observations as well as in experimental manipulations of insect colonies, especially wasps. Existing narratives assume that Hamilton was just trying to test his theoretical results. In my paper, I ask: is this true? What do the data he collected in his naturalistic and experimental observations bear upon his theory of inclusive fitness? And, what does this tell us about the origins of Sociobiology and Behavioral Ecology more generally?

In my talk, I question the assumption that Hamilton, in his 'naturalistic meanderings', as David Hughes calls them, was just trying to test his theory. I argue that Hamilton was actually trying to reconstruct the evolutionary pathway that took solitary species to cross the threshold of sociality and develop complex cooperative and altruistic behaviors. To do so, he focused on primitively eusocial wasps and bees. Given to their primitively social features, these taxa are good material for the investigation of the transition from solitary to highly eusocial life. To understand how social life actually evolved, Hamilton used naturalistic, experimental and theoretical/mathematical tools. He felt the need to integrate different approaches, from highly theoretical ones to experimental and observational ones. This is recorded in his Notebooks from 1963 to 1968, his memoirs and his correspondence with important entomologists and naturalists of the time, among others Mary Jane West Eberhard, Warwick Kerr, Robert Richards and Charles Michener.

It is well documented that, after the rejection of a bigger paper by *Nature*, Hamilton split the paper into two and submitted them to the Journal of Theoretical Biology in May 1963. After revisions, he resubmitted the two papers in February 1964. Before the resubmission, and for a few months afterward, Hamilton spent time in the lab of the famous entomologist W. Kerr in Rio Claro, Brazil. He also travelled around Brazil looking for wasp and bee nests. The observations he made during these trips informed both Part II of *The Genetical Evolution of Social Behavior* and his later works on sex ratios and the evolutionary origins of altruistic behavior. In his observations during his trip to Brazil, Hamilton was trying to understand how social wasps, especially the tropical species Polistes canadensis and Polistes versicolor, managed to keep their colonies cohesive and cooperating in spite of highly multiple egg-laying queens. He wanted to detail how many queens and how many workers were on each nest, as well as the genetic relationships among them. These observations had direct implications for the theory of inclusive fitness. They aimed to understand how coefficient of relationship, mechanisms of sex determination and social mechanisms play out in the evolution of social behaviors.

From the Notebooks and letters, it emerges that Hamilton in his naturalistic and observational studies, tried to reconstruct the steps towards the evolution of social life by integrating different kinds of investigations within the mathematical framework of his new theory of Inclusive Fitness. Hamilton performed behavioral observations of Polistes wasps upon nests in their natural environment. Also, he staged encounters between queens taken into the laboratory or caged in their natural environment, trying to manipulate their social systems. He performed wing-clipping experiments to determine the effect this had upon dominance order and tried to transplant dominant wasps from nest to another to observe what that implied for the functioning of the colony as a whole. While in Brazil, Hamilton also learned by Warwick Kerr how to perform dissection of wasps and bees internal bodies, such as ovaries and fat bodies. After his observations and behavioral experiments, he would dissect the insects and look into the development of their internal bodies to find out how many of the wasps on a nest were queens (the ones with developed ovaries), how many were inseminated and what state of development had the fat bodies attained.

In order to organize his data, Hamilton started an index card system. On these cards, Hamilton recorded the results of his dissections as well as observations on their meaning and significance for his theory of inclusive fitness. Hamilton collected in this way a huge amount of data that he processed and interpreted over the course of the following decades. This body of new data informed the further development of his ideas about the evolution of altruistic behaviors. By interpreting Hamilton's work as a constant interplay of theoretical and naturalistic/experimental investigations, in my talk, I flesh out some interesting epistemic and epistemological features of the the work of the intellectual father of Sociobiology. I finally show that Hamilton's approach at the intersection of many disciplinary fields, and with the tendency to integrate different investigative approaches for the understanding of evolutionary phenomena, can be exemplar even today in the age of molecular and systems biology.

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A Case Study of Case Studies: Scientific Realism and Integrated HPS

Case studies of past and present science, whether focused on the interpretation of specific theories, or on the nature of theory change over time, are often presented as evidence for or against the viability of scientific realism. One use to which such evidence is put is in evaluating the viability of forms of *selective* realism: forms that advocate belief in certain "components" of theories, as opposed to their entire descriptive content. The motivation for selective realism often stems from hopes of responding to the so-called pessimistic induction on the history of the sciences, which problematizes realism in the present by pointing to a history of discontinuities in theoretical beliefs in the past. Selective realists (such as French, Hacking, and Worrall, to name just a few) are inclined to invoke components of theories that putatively survive these discontinuous shifts, thus putatively vindicating realism. Historical case studies are used not merely to illustrate such contentions, but as arguments for them. Conversely, cases are also used by critics of these views to suggest that the interpretations of past science suggested are untenable.

In this paper I consider the question of how probative historical case study evidence can be in testing forms of selective realism, focusing on three prominent versions of the selective approach (each of which admits of finer-grained variations in the literature): explanationism; entity realism; and structural realism. In each case I suggest that while case studies do serve as a precondition of philosophical analysis, they are not decisive in the way that many participants to debates about selective realism think they are (cf., in this connection, more general considerations concerning the use of historical case study evidence suggested by Pitt, and contested by Grandy). I offer three arguments to support this thesis, each targeting disputes regarding the viability of one of the three selective realist strategies mentioned above.

The first argument concerns debates about the prospects of explanationism: the attempt to ground selective realism in those components of scientific theories that describe what is "responsible for" – that which is indispensible or essential to explaining – the empirical success of a given theory.

Discussions here commonly focus on the question of whether it is possible, by means of considerations of scientific attitudes, methodologies, and practices, to demarcate what is genuinely explanatory (for example, in the caloric theory of heat, as argued by Psillos and contested by authors such as Chang and Stanford). I argue that several worries (familiar, I think, from other contexts of debate) concerning the nature of truth in historical narrative, historiographical objectivity, and unanimity of ontological commitment within scientific communities, problematize the role of case studies in thinking about explanationism. In this section of the paper I draw parallels with and extend arguments of a more general nature found in recent work by Schickore.

A second argument focuses on entity-realist-type strategies for selective realism (as presented by Cartwright, Giere, and Hacking, and developed by a great many since, including Massimi) and their critics. The problematic here is generally framed very specifically in terms of versions of the pessimistic induction and responses thereto. I argue that while historical cases furnish the initial subject matter of investigation, arguments on either side are quickly and naturally transformed into disputes about how best to strike an appropriate balance between emphasizing sense or reference in accounts of the meanings of theoretical terms, and the credibility of forms of meaning holism or contextualism and causal theories of reference and meaning in different contexts of scientific knowledge. Here, disagreements about semantic considerations function as a proxy war for disputes between selective realists and antirealists, and it is my contention that, very plausibly, it is the commitments to realist and antirealist stances that various interlocutors bring to their historical case studies that drive their semantic commitments, and not the other way around.

A final argument targets structural-realist-type strategies for selective realism and their detractors. The problematic here varies according to the form of structuralist hypothesis at issue, but in each case, I maintain, the tenability of the selective realist proposal rests not, as some authors would suggest, on the historical cases that serve as subject matters for analysis, but rather on a number of logical, conceptual, and metaphysical issues concerning the definitions of the term 'structure' on which different variants of structuralism rest. Through a brief tour of the range of these definitions (from the Ramsey-sentence structuralism of authors including Papineau and Worrall, to the ontic versions of structuralism advocated by authors including Ladyman and French, and including my own favored approach to structuralism incorporating a dispositional analysis of properties of scientific interest), we see that historical cases are a substrate on which these forms of selective realism are imposed. The case studies themselves, however, and contrary to what is sometimes suggested, cannot hope to decide, by means of the historical narratives they present, which if any of these forms of selective realism is tenable.

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Qualitative novelty and the scientific revolution: The emergence of the concept of pressure

In this paper I interpret the mechanical philosophy, not as an attempt to replace one, Aristotelian, account of the ultimate structure of the material world by another, mechanical, one but rather as an attempt to extend knowledge of such things as levers and clockworks that were archetypical mechanisms in the common sense of the term. At the dawn of the scientific revolution knowledge of that kind had been securely established and mathematically theorized in the area of statics, yielding a unified theory of such mechanisms as balances, levers and pulleys. The question I address is the extent to which extension of mechanical knowledge was capable of yielding in the seventeenth century the kind of novelty that might warrant the term 'revolution'. More specifically, I focus on the extension of statics to include hydrostatics via the introduction of the concept of pressure.

The fact that the issue calls for some finely-tuned historical and philosophical analysis is bought out by highlighting puzzling features of the relationship between two early versions of hydrostatics, *The Elements of Hydrostatics* published by Simon Stevin in 1586 and the treatise *On the Equilibrium of Liquids* composed by Blaise Pascal around 1653 and published posthumously in 1664. The former reads as a text modeled on Euclidean geometry. Theorems are derived from postulates with the aid of many geometrical diagrams. Applications of the theory to novel situations are treated by Stevin not as providing evidence for, but as applications of, it. By contrast, Pascal explains a range of hydrostatic phenomena, both novel and already familiar, in a way that is justified by an appeal to experiment. There is no explicit appeal to mathematics and there are no geometrical diagrams. Pascal's text would not be out of place in an introductory course on hydrostatics today. But the significance of these striking differences can be countered. All of the consequences of Pascal's theory are in fact consequences of Stevin's, or some modest extension of it. Many of the experiments appealed to by Pascal are modified versions of those described by Stevin (under the guise of practical applications). What is more, for all his emphasis on the experimental basis for his theory, there are reasons for doubting that Pascal actually performed the most significant of the experiments he describes! These latter points need to be dealt with if we are to read Pascal's hydrostatics as a significant early move in the revolutionary transformation of science in the seventeenth century.

Anyone wishing to develop a theory of hydrostatics late in the sixteenth century could take a mathematical science of weight for granted. They could also take for granted a common-sense distinction between solids and liquids, including some puzzling phenomena such as the balancing of unequal volumes of liquid communicating via a common vessel. Solids and liquids are alike insofar as they possess weight. What was needed was a characterization of the distinguishing feature of liquids that differentiates them from solids and which could be added to weight to yield foundations for a science of hydrostatics. The fact that that move was far from obvious is apparent from the shortcomings of hydrostatics as formulated by such able thinkers as Galileo and Descartes.

Stevin's hydrostatics can be challenged on the grounds that it appealed to questionable principles, such as his version of the impossibility of perpetual motion, and to arguments involving thought-experiments that lacked deductive rigor. But even if his derivations are conceded, there are some telling objections remaining. The additions to weight that need to be made to yield a hydrostatics were made by Stevin, not by way of an explicit and succinct characterization of the distinguishing feature of liquids, but by drawing on common sense knowledge of properties of liquids (such as the fact that they flow) in an opportunist and ad hoc way. Further, the reductio character of the arguments that he employed had the consequence that he failed to reveal the causality lying behind the phenomena described by his theorems, including novel phenomena described by Stevin as practical applications. If a mechanical explanation involves a grasp of the mechanism that links cause and effect, paradigmatically involved in the understanding of how clocks work, then Stevin did not supply a mechanical explanation of hydrostatic phenomena. (When Beeckman and Descartes evaluated Stevin's hydrostatics in 1618 they explicitly raised the latter objection, with Beeckman complaining that Stevin 'was too devoted to mathematics and dealt too rarely with physics'.)

In the first half of the seventeenth century figures such as Galileo and Descartes sought to identify fundamental principles on which to base their hydrostatics. These included the inverse proportionality principle, exhibited, for instance, by the movements about an equilibrium position of the weights on an unequal-armed balance, and the principle that a system moves spontaneously under gravity only if that motion involves a lowering of the centre of gravity of the system. These principles are restricted to the action of weights and the displacements involved are in a vertical direction only. For that reason attempts to extend application of the principles to the isotropic forces involved in hydrostatics were problematic and met with very partial success.

It is in Pascal's Treatise that we find the above deficiencies overcome. Pascal makes it clear that by virtue of their 'continuity and fluidity' liquids transform forces applied to them, whether stemming from their own weight or applied externally, into isotropic ones that are transmitted throughout the liquid in such a way that the force per unit area is conserved. In short, Pascal introduced the notion of pressure as a cause of hydrostatic phenomena in addition to weight. The adequacy of the theory was to be borne out by experiments, a number of which were identified by Pascal.

But what are we to make of the fact that Pascal may not have bothered to carry out those experiments? Here I appeal to a notion of theory confirmation that was mentioned by Descartes and which I believe can be taken as representing views that were intuitively held at the time. According to that view a claim is confirmed to the extent that it can be successfully applied to a diverse range of cases in a natural, rather than contrived, way. On this view, it makes no difference whether or not knowledge of the cases precedes or postdates knowledge of the claim and it also makes degree of confirmation a matter of degree. Adopting this view, Pascal's hydrostatics was significantly confirmed by virtue of the natural way that it could explain a wide range of phenomena, including puzzling phenomena, that had been known for many decades. That is why Pascal could be confident that the experiments he described would conform to his predictions. When

Robert Boyle performed his own versions of Pascal's experiments he did in effect extend the degree to which Pascal's theory was confirmed, but in a way that would have come as no surprise to Pascal. What is more, Boyle's success shows that Pascal's theory was supportable by experiment in just the way he claimed it was.

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Mechanical molecular models and haptic reasoning

Up to the 1960s, biochemistry and molecular biology were profoundly influenced by the deployment and diversity of a peculiar kind of *research tool*: mechanical molecular models (Francoeur, 1997, 2000). Although such physical models of molecular structures have been replaced by simulated or virtual models in modeling tasks (Francoeur & Segal, 2004), the building of scale models of molecular structures from tangible components was once an important part of the practice of biologically oriented chemists. This model-building strategy directly served the scientist's research interests. The construction and manipulation of plastic, wooden and metallic models of possible molecular structures played a central part in an informed trial-and-error research strategy that proved especially useful in elucidating complex molecular structures such as those of organic macromolecules. In the late 1940s and early 1950s, chemist and biochemist Linus Pauling pioneered the *systematic* use of these "tinker toys" for the structural determination of compounds (e.g. Corey & Pauling, 1953), a research strategy historian of science Lily Kay has characterized as a "molecular architecture epistemology" (Kay, 1993, p. 262). This pervasive research strategy led to new scientific knowledge, including major breakthroughs, by stimulating the scientist's imagination and pointing to new research avenues (Laszlo, 1993, 2000). Certainly the most famous example of such successful use of molecular models is James Watson's and Francis Crick's use of this research strategy in discovering the basic structure of the DNA molecule. The metallic model they used was not simply a dramatic display in an extravagant showcase: it was itself a tool for research and served as a locus for discovery. And this case is no anomaly—the use of such models for complex structural determination was typical rather than exceptional (Francoeur, 1997).

Although nowadays these tangible models are seldom encountered outside undergraduate biochemistry courses, their historical importance as research tools guarantees their place in the standard iconography of science, making them familiar even to the layman. The role these physical models have played in scientific discovery has received little attention, with most scholarly focus being aimed to their supporting roles as pedagogical devices manipulated for better learning/memorization of a selected molecule's structure or used as visual support in classrooms (e.g. Coll, 2006). Though the historian Robert Olby (1974) does mention here and there the use of such models, and describes in detail Pauling's paper-made alpha-helix model (Olby, 1974, p. 208-201), there is no systematic discussion of the importance and role these physical models played—even though they did play a central role in paving the way for contemporary molecular biology. Philosophers have paid even less attention to these models but, when attended, the discussion centers on the more general aspects of representing and modeling (e.g. Giere, 2012), such as the scientists' struggle with visualizing three-dimensionality by a two dimensional media (e.g. Francoeur, 1997; de Chadarevian & Hopwood, 2004; Gooding, 2006)). More generally, in the SEP entry on *Models in Science*, Frigg and Hartmann write about physical models that they "[…] do not give rise to any ontological difficulties over and above the well-known quibbles in connection with objects, which metaphysicians deal with." (Frigg and Hartmann, 2012, section 2.1). From this latter perspective, there seems to be little more to physical models of molecular structures than a means to represent more accessibly the three-dimensional structure of a given molecule.

The trouble with this view is that it does not account for the actual practice of using physical models as research tools nor for the manner by which new scientific knowledge is produced when doing so. When a modeler aims to solve a molecule's structure with the aid of such physical models, she explores different combinations of model parts, makes measurements, often disassembling and reassembling the models built. These interactive manipulations exploit the mechanical properties of the models, properties which are not reducible to matters of visualization. Moreover, the use of these physical models has often replaced the deployment of mathematical calculations in molecular modeling contexts. This introduces a pragmatic dimension that cannot be ignored: when compared with geometrical drawings and mathematical calculations, physical models appear to be costly (in time, energy and money) and cumbersome replacements for clean paper work. I will argue here that mechanical molecular models are not just static representations, equivalent to flat geometrical and mathematical calculations with a three-dimensional twist; they have something more that makes them non-trivially different from their inscriptional counterparts.

The main claim of this paper is that mechanical molecular models (henceforth, '*M-models*') were used as research tools that, through their mechanical properties, augment and extend the modeler's cognitive capacities and performances. . Instead of relying on mathematical tools or solely on internal cognitive capacities, the architect manipulates these material props in such a way as to become more efficient in her problem solving. This is done by replacing extensive mathematical calculations by a different set of cognitive capacities such as visuospatial and haptic reasoning. Moreover, exploitation of the materiality of the M-model by informed manipulations on the part of the architect allows the M-model to take an active part in the cognitive work required to solve a complex molecule's structure. M-models thus facilitate and extend the architects performances by integrating the inherent causality of the M-models materiality to serve as part of the cognitive process required to solve molecular modeling tasks.

Moreover, I will argue that their component parts were *designed, built and used* to serve such cognitive functions. By an intelligent use of M-Models' material properties, the molecular architect's manipulations of the model parts enhanced her cognitive performances by facilitating the modeling task. This shows that, contrary to Frigg and Hartmann's summary, there is more to the ontology of physical models than what has been led to believe by current philosophical investigations. Recasting M-models as cognitive augmentations opens the way for a new horizon of research in the philosophy of science about the scientific uses of physical models.

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Jean Perrin and the Philosophers' Stories: A Case Study on the Role of Case Studies in &HPS

The French physicist Jean Baptiste Perrin (1870–1942) is generally credited with providing the conclusive argument for atomism in the beginning of the 20th century (Brush 1968, Nye 1972, Chalmers 2009).

Perrin's argument was based on the existence of thirteen different experimental procedures for determining Avogadro's number (N), including his own determinations based on the height distribution, mean displacement, and mean rotation of Brownian particles (Perrin 1909, 1916).

Being so successful in ending the 19th century atomic debates Perrin's argument has been the focus of much philosophical interest. We can discern two relatively independent strands of philosophical treatment in the literature. On the one hand, Perrin's case is often cited in philosophical discussions on experimental multi-determination. In this context, Perrin's offering of thirteen different methods for determining N is referred to as a classic example of experimental multi-determination (or experimental robustness). Other than this, however, not much analysis is devoted to the role that the multiple determinations of N actually played in Perrin's argument or in convincing the scientific community.

On the other hand, Perrin has been the object of detailed case studies aiming to capture the reasoning behind his successful argument for atomism. Strangely enough, the philosophers who have paid attention to Perrin's argument tend to downplay the role that the multiple determinations of N play in it. The reasoning behind Perrin's experimental strategy was initially interpreted as a case of a *no miracles argument* or as an *inference to the best explanation* (Harman 1965). Clark Glymour used it as an exemplification of his account of *bootstrapping confirmation* (1980). In a detailed analysis of the structure of Perrin's argument, Wesley Salmon presented it as a case of a *common cause argument*. Salmon's interpretation was one of the few that put a lot of emphasis on the role played by the concordance of independently established facts (Salmon 1978, 1984). Nancy Cartwright drew on Salmon's interpretation of the case, but because of her distrust of the theoretical laws in physics, presented it as an *inference to the most probable cause* (Cartwright 1983). Cartwright's account diminished importantly the role that multiple determination plays in actual experimental practice. The role of the multiple determinations of N was downplayed even further both in Deborah Mayo's interpretation of Perrin's experimental work as *a severe test* for the kinetic theory of gases and in Peter Achinstein reconstruction of Perrin's argument as supporting his account of evidence (Mayo 1986, 1995, Achinstein 2001). These different assessments of what stood behind the success of Perrin's argumentative strategy are to be expected insofar as the various interpretations, rather than 'disinterested' reconstructions of the rationale underlying Perrin's reasoning, seem more like efforts to present Perrin's case as a confirmatory instance for general theories of confirmation. Nevertheless, the situation seems to exemplify some of the most difficult problems scholars cite when discussing a fruitful collaboration of history of science with philosophy of science (see for instance, Brooke 1981 for some of the problems faced by the case-study approach).

I claim that this situation should not considered as an obstacle for a fruitful combination of historical and philosophical perspectives as long as we distinguish between idealized and empirical or historically driven case studies. Whereas idealized case studies tend to use the historical material in order to elucidate and evaluate general reasoning patterns, historically driven case studies examine the significance of methodological arguments as scientists make them in concrete historical situations; they focus on aspects of scientific practice that may or may not be generalizable to other historical episodes (Schickore and Coko 2013). Moreover, I argue that a combination of historical and philosophical perspectives is needed in order to understand the role that the multiple determination played in Perrin's argument for atomism and in convincing his contemporaries.

I argue that in order to understand the persuasive force of Perrin's argument we need to pay attention not only to the (a)historical 'slice' comprising the scientific work and the wider intellectual context of the early 20th century, but also to the larger temporal dimension. For instance, contrary to what most scholars seem to admit, there was an extensive and sophisticated experimental work done on the phenomenon of Brownian movement during the course of the 19th century. Although these experimental investigations contributed more in excluding possible causes of the phenomenon rather than establishing a positive causal explanation, by the end of the 19th century, they had left the molecular-kinetic hypothesis as the most plausible explanation of Brownian movement. A sensitivity to the temporal dimension of scientific work is also needed in order to distinguish between two different kinds of independence involved in the various determinations of N: some of the procedures for determining N were not only theoretically, but also historically (or 'genetically') independent.

A closer look at the details of Perrin's experimental work reveals that the strategy of using multiple means of determination is involved at many levels; not only in the determinations of N, but also in the determinations of the various parameters and theoretical assumptions required for the calculation of N. Another relevant distinction in Perrin's case is that between quantitative and qualitative multi- determination. Arguments aiming to connect Brownian movement with the molecular hypothesis based on the qualitative characteristics of the phenomenon were common in 19th century experimental investigations, but it was the quantitative agreement between the numerical values obtained for N by independent procedures that played a crucial role in convincing the scientific community.

Finally, Perrin's case serves to highlight a difference between two concepts often not distinguished in the philosophical literature; namely, between 'robust' and 'multiple-determined'. Whereas the first is more fitting for experimental results or phenomena that remain invariant despite changes in the experimental setting, the second is more appropriate for results obtained by independent experimental procedures. Arguments from robustness rely on a notion of *invariance*, whereas arguments from multiple determination rely on a notion of *concordance*. The two kinds of argument played different roles in connecting the movement of Brownian particles with molecular motion.

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The Unity of Science: Two Hundred Years of Controversy

Contemporary philosophers and historians of science are often tempted to read the unity of science movement of the 1920s and 30s as insisting that all of the sciences should be "just like physics" in all details of method, ontology, and concepts. This reading locates the issue as pitting what we call the natural sciences narrowly conceived against what we call the social sciences. This caricature misses the point. A better reading of the unity of science movement understands it as responding to specific challenges to science as a whole. This challenge comes from writers who see themselves as altogether outside what we usually take to be science, that is, from broadly humanistic writers. So conceived, the unity of science movement is but one stage in a controversy that runs for at least two hundred years from the rise of romantic idealism in the early nineteenth century to today's faculty meetings throughout the academy. This paper will explore the specific historical traditions to which the unity of science movement was responding. It will also show that this nuanced picture of that movement was doing is more illuminating than the standard caricature could ever be about enduring philosophical questions concerning the relations among the sciences and between science and other human intellectual endeavors.

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Kelvin's dictum revived: the intelligibility of mechanisms

Lord Kelvin (1824–1907), the epitome of nineteenth-century physics, famously declared: "It seems to me that the test of 'Do we or not understand a particular subject in physics?' is, 'Can we make a mechanical model of it?'." Kelvin's dictum implicitly assumes that understanding is an important aim of science and explicitly states that this aim can only be achieved by devising mechanical models. Kelvin's dictum was widely supported in the nineteenth century but proved to be untenable in the light of later developments in physics. It was in particular the advent of quantum theory that has refuted the universal applicability of mechanical modeling as a road to understanding. To be sure, present-day physicists still talk about mechanisms but often this talk is only metaphorical (esp. in the case of fundamental particle physics, the so-called Higgs mechanism being a prominent example). For this reason Salmon's causal-mechanical model of explanation (Salmon 1984) cannot be a *universal* theory of scientific explanation: the ontology it presupposes doesn't square with the basic ontology of modern physics.

But the inapplicability of mechanical models in fundamental physics does not imply that mechanistic understanding has to be rejected in other fields and disciplines as well. In other subfields of physics and in other scientific disciplines it may still be useful. Indeed, recent years have witnessed the rise of the 'new mechanists': philosophers of science who have developed new mechanistic models of explanation that are inspired by the contemporary practices of the life sciences (e.g. Machamer, Darden and Craver 2000; Glennan 2002; Craver 2007). The core of these models is an analysis of mechanisms as organized wholes that by virtue of the interaction of their parts produce specific behavior or perform a particular function.

How do such mechanisms provide understanding? An important feature of mechanistic explanations is that they are typically not purely linguistic but contain visual (pictorial or diagrammatic) representations. Mechanisms are visualizable, and most scientists prefer visualizations to linguistic descriptions because of their pragmatic advantages: visualizations directly convey the spatial organization of complex mechanisms (and temporal change can be represented visually too). Thus, they are more tractable than linguistic representations. Moreover, visual reasoning can be facilitated by simulation tools such as scale models or computer models.

But is tractability the same as intelligibility? Does tractability lead to understanding, and if so, how? The answers to these questions depend on one's conception of scientific understanding. I will argue that understanding lies in the ability to use a model or theory to generate predictions of the target system's behavior. In the case of mechanistic explanations, one has achieved understanding if one is able to see how (functional) behavior is *produced* by the (hypothesized) mechanism. In other words, mechanistic explanations render phenomena intelligible by specifying productive relations. The new mechanists have as yet merely defined explanation as description of mechanisms, without specifying why such descriptions provide understanding. I will defend a view of scientific understanding and intelligibility (in terms of recognition of qualitative consequences of theories and models) that makes sense of the claim that (mental models of) mechanisms provide understanding by allowing the modeler to see how the system produces particular behavior. I will show that visualization can be an effective tool to achieve such understanding.

My pragmatic account of scientific understanding (with its emphasis on abilities, tractability, and use) leads to the question of whether the mechanisms that provide explanatory understanding should be regarded as real or as (merely) representational. In other words, what is the ontological status of mechanisms and mechanistic explanations? Are mechanisms realities out there, or are they (merely) our mental representations of the observable phenomena? The new mechanists seem to be divided over this issue. I will argue that explanatory understanding does not require scientific realism: it is perfectly possible to achieve understanding of phenomena via theories or models independently of whether they are true representations of a reality underlying the phenomena. My claim contradicts the traditional association between anti-realism and descriptive aims on the one hand and realism and explanatory aims on the other. However, I will argue that such an association has to be rejected.

This view has a precursor in the epistemology of Ludwig Boltzmann, who, at the end of the nineteenth century, defended his *Bildtheorie* (picture theory) of scientific knowledge, a sophisticated epistemological position which stated that scientific theories and models are mental pictures having at best a partial similarity to reality. While Boltzmann (1899) admitted that mechanical models could no longer be regarded as realistic representations of physical reality (developments in physics led to the collapse of the mechanical world-picture in the 1890s), he argued that such models could still be employed to achieve understanding of the phenomena. My analysis of scientific understanding follows Boltzmann's approach, implying that mechanical models can provide understanding even if they defy realistic interpretation. In this way, Kelvin's dictum can still be relevant for twenty-first-century science.

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Reconsidering Priestley's Defense of Phlogiston

Building on John Norton's (2003) "material theory of induction", I contend that studies of the use of analogy in experiment can provide historical and philosophical insight into the process of discovery and invention. In particular, this case study focuses on Joseph Priestley's experiments and interpretation of combustion. A stage in-between what Friedrich Steinle (2002, 2005) calls "exploratory experimentation" and robust theory, I argue that analogy encouraged research to substantiate why the likenesses should outweigh the differences (or vice versa) when evaluating results and designing experiments. I contend that this methodological approach is especially evident in Joseph Priestley's research and helps to resolve a longstanding tension in our understanding of his chemistry, namely his dogged adherence to phlogiston theory.

Benjamin Franklin (1751), Priestley's mentor in electrical research, probably made the most famous analogical argument in the history of physics, namely that clouds produce lightning in the same manner as an electrostatic generator or, as Franklin would have called it, a friction machine. He was not alone in arguing that there existed likenesses between natural and laboratory-created systems, analogs that not only promoted scientific research, but also helped to underpin the epistemic value of experiment. As Alan Shapiro (1995, 43) has shown: "The belief in the 'analogy of nature', or that 'nature is ever consonant to herself', served as a guiding maxim throughout [Isaac] Newton's career." The Comte de Buffon, an influential French naturalist and director of the Royal Gardens, argued "that if 'experience is the foundation of all our physical and moral knowledge, analogy is its first instrument.'" (Riskin 2002, 95) Thomas Young (1971, 15), in a series of physics lectures, also emphasized its importance: "That like causes produce like effects, or, that in similar circumstances similar causes ensue, is the most general and important law of nature; it is the foundation of all analogical reasoning, and is collected from constant experience."

Like his contemporaries, Joseph Priestley also stressed the importance of analogical reasoning in the design and evaluation of experimental stratagems in electrical and chemical research. Although his publication—*History and Present State of Electricity* (1767)—was widely read and cited, few historians take Priestley's electrical research seriously or consider how it affected his chemical studies. A detailed analysis of his electrical and chemical research, however, shows that his electrical work informed his chemical studies in significant ways. Because heat and electricity produced common effects – e.g. exposing air to a spark or flame caused similar changes in its composition, volume, and toxicity – Priestley argued that electricity and heat were related substances. I argue that his identification of phlogiston with electrical fluid affected how he received his contemporaries' chemical studies, especially Antoine Lavoisier's theory of heat.

Accounts of the chemical revolution are diverse and sometimes at odds with one another. Key elements of this transformation include, but are not limited to, the overthrow of phlogiston theory, a common eighteenth-century explanation for inflammability, and new emphasis on measurement, especially the weight and volume of chemical reactants and products. As John Heilbron (2000) notes, there are three main lines of argumentation: the external— Lavoisier and others importing quantitative methods from physics construct a revolution in chemistry; the internal—chemists' methods underwent a dramatic shift in a short period of time, changing the theory and practice of chemistry from within; and the revisionist– there was no revolution per se, but rather through a series of slow and successive modifications chemistry was transformed.

Within this literature, Priestley has a mixed legacy. He was (and is) celebrated for having isolated vital air or dephlogisticated air (oxygen) and for showing it was necessary to animal respiration. Yet, because he vocally opposed French chemists' attempts to develop a new chemical nomenclature and Lavoisier's theory of heat, he was (and is) disparaged for defending phlogiston and for not "seeing" the theoretical implications of his own chemical research, i.e. oxygen was absorbed during combustion. There is an incongruity between these different depictions of Priestley. Roughly put, it is difficult to reconcile how a creative experimentalist could also be, as his biographer Robert Schofield (2004, 193) writes, "a bumbler".

Although this presentation deals with Priestley's chemistry, I make no attempt to enter into this larger debate of whether or not there was a chemical revolution. Rather, I am interested in reassessing Priestley's defense of phlogiston in light of his electrical research and emphasis on analogical reasoning. It is a modest attempt to restore Priestley's work to the fold. I argue that his dogged defense of phlogiston—his explanation for thermal and, as I will show, electrical phenomena—stemmed from both his chemical and electrical research, which were inextricable. The questions he was trying to answer about chemical properties and behavior differed significantly from his contemporaries because he was considering a broader range of phenomena that included not only thermal effects, but also electrical phenomena. Analyzing his chemical experiments in light of his electrical research and commitment to analogy helps to clarify some of his seemingly contradictory statements regarding chemical processes; therefore, providing a better understanding of his theory and practice. By taking an integrated historical and philosophical approach to his research, I demonstrate that Priestley was less a foil to the progressive French chemistry than a would-be synthesizer in a time of increasing specialization.

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Checks-and-balances: orbital symmetry and quantitative methods in late twentieth century quantum chemistry

Since the early twentieth century chemists have sought to account for a number of organic reactions of crucial importance to synthetic chemistry, ranging from the relatively simple reaction between two ethylene molecules to the Diels-Alder reaction, and reactions within the fundamental carbon skeleton such as the Cope and Claisen rearrangements. By the mid 1960's, developments in quantum chemical theories of chemical bonding resulted in the application the molecular orbital theory to these important organic reactions in the form of a relatively simple, qualitative modelling technique. R.B. Woodward and Roald Hoffmann's orbital symmetry approach proved a remarkably successful technique to predict broad trends in chemical data of particular use in synthetic chemistry. The fundamental property governing the course of "pericyclic reactions", as Woodward and Hoffmann called them, was the relative phase symmetry of the molecular orbitals representing the bonds that contributed most to a reaction. When the symmetry of the molecular orbitals is conserved in the transition from reactants to products, a reaction is "allowed" because it requires less energy. Woodward and Hoffmann's relatively simple, qualitative quantum chemical approach had great utility not only because it worked well as a predictive tool, but also because of its intelligibility to theoretically inclined experimentalists who lacked the requisite knowledge of quantum mechanics to engage in the computationally daunting task of applying fundamental physical theory to quantitative studies of molecules and their reactions.

Given the importance of the orbital symmetry approach to modern chemistry and its potential historical and philosophical interest, it is remarkable that it has received so little attention. Although the orbital symmetry approach is mentioned by Brush (1999), for example, a dedicated investigation of its role in forging a contemporary understanding of organic reactions seems to be absent in the historical and philosophical literature. Moreover, there appears to be very little cognisance of the broader epistemic impact, methodological repercussions, and controversies that followed the introduction of Woodward and Hoffmann's ideas. Applying quantum mechanics to the study of organic reactions is challenging because of the complexity of the target systems. But it is also challenging for a more subtle reason. One might suppose that whatever method of approximation is employed, the quantitative results might differ in terms of their precision but not in terms of their consistency. However, different approximation methods give different answers, and this led experimental chemists to infer that the numerical results were merely artefacts of the approximation methods. It is no over-statement to say that this controversy of quantitative methods has proved to be one of the most important and divisive issues in modern chemistry although like orbital symmetry itself there appears to be very little awareness of it outside of chemistry.

This paper investigates the historical and philosophical significance of orbital symmetry, and probes its epistemic status and function within the context of a controversy of immense importance to contemporary chemistry. One central issue concerns how models are evaluated in their historical context, and how these situated criteria of assessment mesh with philosophical analyses of model evaluation. For example, orbital symmetry is renowned for its predictive abilities. The "Woodward-Hoffmann rules" are selections rules that have enormous utility because they provide experimental chemists with the means to anticipate the stereochemical course of the appropriate class of organic reactions in spite of a degree of imprecision in their results. Perhaps this is typical in chemistry. For example Slater (2002) has dubbed Woodward and Hoffmann's approach a "rule-based theory". One might cash-out such an idea by appeal to the practice of "trade-offs" in science. Drawing from Hoffmann's ideas on idealization in chemistry, Weisberg (2004) argues that while highly idealized ("qualitative") models in chemistry may not be as accurate as quantitative predictions, precision can be traded off against generality. One of us has argued that trade-offs between manageability and accuracy are a notable feature in the development of computational methods in twentieth century quantum chemistry (Park 2009). A finer-grained analysis of trade-offs can be advanced by looking to the specific historical development of orbital symmetry, by considering the various kinds of trade- offs employed by practioners working within the frameworks of quantitative theory, qualitative modelling and experimentation, and the motivations and justifications for these trade-offs.

This paper also considers the broader context of the application of quantum mechanics to the study of organic reaction mechanisms: the interplay of models and computational methods in the controversies surrounding legitimate applications of fundamental theory. The orbital symmetry approach and ab initio computational methods used to study organic reactions tend to agree in their independently derived results. The convergence of orbital symmetry predictions and the more precise ab initio methods calls for an investigation of the use of robustness analysis in quantum chemistry. Robustness analysis was developed in population biology (Levins 1966), and its applicability to the assessment of models making different idealizations and/or approximations in chemistry has been pursued by Weisberg (2008). This paper looks to the specific moves made by the historical actors engaged with the controversy of methods and the significance of robustness analysis in settling methodological disputes regarding standards of rigour in quantum chemistry. In a delicate process of epistemological checks-and-balances, qualitative models and quantitative methods emerged as a means to coordinate the difficult task of extending quantum mechanics from the study of molecular structure to molecular dynamics.

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Styles of Reasoning in Biology: The Case of Models in Membrane and Cell Biology

Changing styles of reasoning in the life sciences have for some time attracted attention from scholars in the history and philosophy of science. For example, it has been argued (Rheinberger 2000) that, whereas early molecular biology aimed at 'creating the technical means of an extracellular representation of intracellular configurations' – exemplified perhaps most iconically in Watson and Crick's stick-and-ball model of the DNA double helix – the advent of recombinant DNA technologies has led to an inversion of this direction of fit. It is now the explicit rewriting of life according 'extracellular projects' (e.g., the demands of industrial or medical application) that shapes much of contemporary biomedical research. Similar shifts in focus – from the 'neutral' representation of naturally occuring phenomena to the 'application-driven' construction of phenomena that blur the line between nature and artifact – have been proposed under the label of 'techno-scientific research' for other disciplines as well. This calls for an analysis of the interplay between representational projects in science and changes in instrumentation, experimental practice, and available technical infrastructure. The present paper analyzes one such example from cell biology: research into the structure of the cell's membrane. The first experiments that probed cell membrane structure were performed by Charles Overton in 1895, which led him to believe that cell membranes and lipids bear certain similarities, and that non-polar molecules pass through the membrane by 'dissolving' in the membrane's 'lipid interior'. Later analysis of the remnants of red blood cells revealed a lipid presence in the membranes themselves, followed by the realization (in the 1930s) of a protein presence alongside the dominant lipids. For several decades to follow, the lipid-protein Davson-Danielli model dominated representations of the cell membrane in the life sciences. Yet, as closer historical and philosophical analysis reveals, none of the preceding discoveries necessitated the particular configuration of protein molecules proposed by the Davson-Danielli model. When technological changes – notably, the advent of electron microscopy – appeared to show a trilaminar structure of the cell membrane, this was taken as a clearcut case of additional confirmation of the protein-lipid-protein structure of the Davson- Danielli model. What contributed to the long-lived attractiveness of the Davson-Danielli model? For one, the model promised a unified account of membrane structure – making it the 'unit membrane model' – and thus exhibited what has often been deemed a core theoretical virtue in science: unification. However, we argue that much of the appeal of the model is, in fact, owed to the prestige of the new technology – electron microscopy – that was employed from the 1950s onwards. The great successes of electron microscopy in material sciences and physics, and the tangible materiality of the new technological infrastructure, served as a source of credibility for what, by hindsight, must be considered a theoretical model that was on rather shaky grounds from the start. Not only did the initial model lack sufficient motivation (as well as precedents in other relevant areas), but it also exhibited significant inconsistencies in the way it was used to explain experimental data. Eventually, in the early 1970s new preparation methods for electron microscopy brought out the inconsistencies in a way that could no longer be ignored, giving instead rise to what is still the accepted view of the cell membrane (with only minor modifications) today: namely, the fluid-mosaic view of the model (which postulates proteins being scattered throughout, and bobbing in and out of, a fluid lipid bilayer). The present paper tells the story of this theoretical shift in our understanding of the cell membrane as one that is marked by the (discontinuous) interplay between experimental data, theoretical models, and technological practices.

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Experiments and concepts in Gilbert's *De magnete*

Gilbert's *De magnete* (1600) instituted a new domain of natural-philosophical research: the new "physiologia" of magnetic matter. Given his dissatisfaction with the conceptual apparatus that permeated previous accounts of magnetism, and accounts of how magnetic effects can be explained, a central part of Gilbert's treatise is the articulation of a new conceptual apparatus to deal with both those magnetic effects that were already known and those that were freshly discovered. Gilbert introduced (or, at times, re-conceptualized) an entire vocabulary to describe and make sense of magnetic phenomena. He introduced or re-conceptualized notions such as "verticity", "orb of virtue", "magnetic coition", and "magnetic inclination" in order to make his findings intelligible, and in order to set out an operational vocabulary for his "magnetical philosophy". Such notions are thus essential parts of Gilbert's natural- philosophical program.

Gilbert's appeal to experiments has not been contested in the scholarship. In fact, studies of the experimental dimension of *De magnete* have focused on identifying the origins and influences of Gilbert's experimental practice (e.g., Zilsel, 1941; Henry, 2001), rather than on establishing what kinds of experiments Gilbert appealed to, or what their epistemic aims were, what kind of results they yielded, how they were reported, etc. The literature has confined Gilbert's experimental practice to data- and proof-gathering, and to testing and illustrating pre-established theories. In this light, the dominant narrative so far has been that Gilbert appealed to experiments to prove his theoretical commitments to the Earth's magnetism and the belief that a magnetic earth could substantiate Copernicus' astronomical model. (e.g. Pumfrey, 2002) No doubt some evidence can be marshaled in favor of a theory-driven experimental practice in Gilbert's case: that a large number of Gilbert's experiments use a spherical loadstone, the "terrella" ("little Earth")—which was both difficult to use and to acquire—as a model of the Earth itself, that for Gilbert iron and loadstone are "one in kind" and differ from all the other substances, and so on. However, this "theory-driven" reading presupposes an ineludible gap between theory and experiment. The scholarship has presumed experiments to be instrumental to Gilbert's theoretical agenda. Given this, little work has been done to prove that this is in fact so.

However, it is precisely this insistence on opposing the experimental program to the theoretical agenda that obscures Gilbert's conception of what experimentation does for knowledge production. On my reading, the role of experimentation is shifted away from the questions of how experiments prove or justify. In this way, my analysis of series of experiments deviates from Marcum's (2007, 2009) account of serial experimentation as justificatory. Instead, my concern is with what and how serial experimentation produces in the first place. Here, I argue that, in the context of experimental series, conceptual articulation is one such product. The claim I make is not that the experiments compel Gilbert's conceptual apparatus, but rather that the way the concepts end up being defined (and understood) is context sensitive—and the context to which it is sensitive is the serial experimental one. The cumulative findings of the experimental series create the conditions for articulating concepts, and gradually establishing what the concept stands for.

Rouse (e.g. 2007, 2011) has extensively argued that a central part of science is the practice of conceptual articulation ("or how concepts acquire content in their relation to experience" Rouse, 2011, p. 244) through experiments, or more precisely in the context of experimental microworlds. He takes an experimental microworld to be "reproduced arrangements of some aspects of the world" (Rouse, 2011, p. 245). In this paper I build on Rouse's account of experimentally-articulated concepts to treat the conceptual innovations that Gilbert proposes in *De Magnete.* I however shift the argument from experimental microworlds as the vehicle of conceptual articulation to experimental series. I thus argue that the concepts Gilbert introduced (or re- conceptualized) were articulated (at least partially) within a serial experimental context. I show that serial experimentation should be treated as an integral part of Gilbert's means for articulating his conceptual apparatus insofar as what each concept signifies (or stands for) depends on the connections given between experiments. For example, Gilbert's concept of magnetic inclination (or magnetic dip) as a rotation, rather than as a deviation from a place, makes better sense when considered in the context of the series of experiments with versoria (a (ferrous) metal needle suspended on a fulcrum so as to move freely on the horizontal axis and/or the vertical axis) and those with magnetic needles suspended in air (esp. Book 5 of *De magnete)*, rather than seeing it as what would otherwise seem an ad-hoc commitment. There are two interconnected parts to this thesis: 1) that the concepts are articulated experimentally, and 2) that the particular experimental practice in which the concepts are articulated should be treated as serial. I argue for the former thesis by way of example: I show how Gilbert came about articulating the concept of "magnetic coition" as the mutual action of magnetic bodies, whose strength depends on their relative positions and masses. I defend the latter claim by showing that what a given concept (e.g. magnetic coition) signifies can be understood only against a group of experiments that end up being connected precisely because what a concept signifies is prompted by the cumulative results of such experiments. A concept's signification is never exhausted by a single experiment, but rather is specified through the experimental series. At the same time, what holds the experiments together as a series is precisely the fact that they contribute to the articulation of a conceptual space. By challenging the series (or part of it), or the further development of the series, the concept's significance can also be challenged (and, at times, replaced).

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William Henry Bragg and the Nature of X-Rays

In 1928, William Henry Bragg remarked in his Presidential Address to the British Association for the Advancement of Science, in regards to the wave-particle duality of light: "On Monday, Wednesdays and Fridays we adopt the one hypothesis, on Tuesdays, Thursdays, and Saturdays we adopt the other" (p. 222). This comment has been often repeated in describing the way physicists treated the emerging quantum theory of the 1920s. This famous quote, which is often attributed to Bragg in 1928, is a statement Bragg used during a lecture in 1921. Back then, the dual nature of light was still being debated and Bragg most keenly felt the difficulties in reconciling the two theories. He had spent most of his career working on X-rays and, for many years in the early decades, was one of the foremost defenders of a corpuscular interpretation: the neutral pair hypothesis. Bragg, however, will always be most well known for providing the strongest proof for the wave nature of X-rays. For his work in the analysis of crystal structure by X-ray diffraction, Bragg received the Nobel Prize in Physics in 1915, an honor that was shared with his son William Lawrence Bragg. How did the man who once so vehemently advocated the particle view also become the man whose name will forever be associated with the work that conclusively demonstrated the wave nature of X-rays? This question is the starting point of this paper.

Since the discovery of X-rays in 1896, the strange behaviors of these new rays have perplexed physicists. In the early decades, the relationship between light and X-rays was unclear and whether X-rays were waves or particles was up for debate. When Einstein wrote his 1905 paper on the photoelectric effect, he was concerned with ultraviolet light, not X-rays, and most of the X-ray researchers, especially outside of Germany, were skeptical of the light quantum; many did not believe light and X-rays to be the same phenomena. After Laue devised his 1912 experiment demonstrating X-ray diffraction by crystals, X-rays were then understood as a kind of light. But the other perplexing properties of X-rays remained and caused physicists to reconsider the nature of all electromagnetic waves; these problems were not fully explained until the 1920s. Bragg played an important role in this early history. His debate with Charles Glover Barkla in 1907–1908 over the nature of X-rays was the first wave-particle controversy of the century. Bragg's insights into the behavior of X-rays were very prescient of wave-particle duality. While Bragg had no contact with Einstein, he still became one of the first advocates of a "quasi wave-particle" theory. This paper tells the history of how one scientist struggled with one of the most significant conceptual changes in physics and how the controversy he precipitated was received by and changed a scientific community.

Many authors (Ewald, 1962; Caroe, 1978; Hunter, 2004; Schmahl and Steurer, 2012; Eckert, 2012) have presented Bragg as holding a straightforward corpuscular theory before 1912. By this popular account, Bragg was convinced of the wave nature of X-ray by Laue's diffraction experiments and abandoned his theory for a wave interpretation. The popular account is false; Stuewer (1971, 1975) and Wheaton (1983) offer much more nuanced histories that acknowledge the evolving nature of Bragg's theory in response to new evidence and show that Bragg did not completely give up on his corpuscular theory post-1912. The previous historiography missed one of the most important aspects of Bragg's thought: Bragg viewed his theory as a working model, that is, he held an instrumentalist view of the corpuscular theory. In this paper, I will argue that this way of understanding Bragg's commitment to the corpuscular theory explains both how he was able to defend his theory prior to 1912 and also explains why he continued to hold the view after 1912.

By looking through Bragg's published papers, as well as, his private letters and manuscripts held at the Royal Institution of Great Britain and the University of Cambridge, I argue that at the very conception of his neutral pair hypothesis, Bragg held a *physical* model that contained both wave and particle elements. I argue that over the course of his debate with Barkla and as new X-ray phenomena surfaced over 1908–1911, Bragg had changed his view to advocate a *working model*, essentially giving up claims to the earlier physical picture. It is in this sense that I mean instrumentalist: a working model that can be exploited for constructing hypotheses and experiments, but does not claim to be physically true. I also argue that Bragg was surprisingly consistent in his view of the instrumental importance of the particle theory even after Laue's experiments. He continued to believe that the corpuscular model captured something that the accepted wave model was missing.

This episode in the history of science brings out interesting philosophical issues regarding theory and experiments. Bragg and Barkla's debate over their experimental results shows how their assumptions and theoretical commitments about the nature of the X-rays shaped their interpretation. Bragg's attitude towards his own corpuscular theory, as more contradictory evidence emerged, shows how committed Bragg was to finding a theory to account for *all* the results. Throughout the controversy, Bragg found himself hitting the boundaries of the explanatory power of the present physical theories. I think Bragg's experience highlights some interesting methodological problems when interpreting experiments with incomplete, competing theories. How do we choose a theory when there is conflicting evidence? Bragg's solution is to become an instrumentalist. In the last section of paper, I will argue that Bragg's position is a rational one in face of scientific controversy. Bragg's emphasis on pragmatic concerns— especially how fruitful the theory is to the development of future research—is a rational criteria to hold. While his contemporaries harshly criticized Bragg for holding on to his corpuscular theory, I argue that ultimately Bragg was being a "good" experimenter in maintaining the conviction in his results and holding his theory and the wave theory to a higher explanatory standard.

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Narrative and evidence: on the role of historical case studies in the philosophy of science

It is relatively uncontroversial that philosophical ideas about scientific knowledge and practice need to be adequate to the historical record. While in the "marriage debates" (Giere, Burian, McMullin) that followed the publication of Kuhn's *Structure*, clarifying the relations between the history and the philosophy of science was deemed of utmost importance to the fate of both disciplines, at present there exists much less explicit discussion on how the historical adequacy of the philosophy of science should be established. One dominant method for warranting the historical adequacy of philosophical doctrines is by relying on historical case studies. But it is not clear how exactly historical case studies provide evidence for philosophical claims and arguments.

Recently, Joseph Pitt (2001) noted a dilemma in the use of case studies: If the case is picked with a specific philosophical view in mind, this invites the charge of cherry picking the evidence. But if the case is chosen independently of a philosophical doctrine, it is unclear what exactly it constitutes evidence for. Jutta Schickore (2011) answered to this dilemma in the context of a broader criticism of what she called the "confrontational model" of HPS. The model thinks of the history of science as providing evidence to test philosophical doctrines. Pitt's dilemma, Schickore argues, does not emerge, if we abandon the "confrontational model" and acknowledge the interpretative character of historiography. Schickore's solution, however, creates another problem: if historical reconstruction is interpretative in the sense of being informed by prior philosophical assumptions, how do we deal with historiographical pluralism – the possibility that one and the same historical episode may be consistently interpreted from different philosophical viewpoints? This paper deals with the question of how historical case studies can provide evidence for the philosophy of science given the interpretative and pluralistic character of historiography. It has three parts.

In the first part, I deal in more detail with the "confrontational model". Going beyond Schickore's critique, I argue that the model rests on two interrelated misconceptions. First, it renders invisible the methodological efforts needed when reconstructing the historical development of the sciences. Ironically, in relying on the "confrontational model", precisely those philosophers who reflect on the methodological subtleties of the natural sciences remain ignorant of comparable problems arising in the discipline of history. Second, the "confrontational model" oversimplifies the relations between historical evidence and philosophical theories. With debates over underdetermination, confirmational holism, the theory-ladenness of observation, etc. the relations between theory and evidence in the natural sciences have come to appear complex and problematic. We should expect the relations between philosophical theory and historical evidence to be no less intricate.

In the second part of my talk I present my own account of the historiography of science and its interpretative character. I argue that historiography is essentially narrative, selective and theory-laden. Taking inspiration from Hayden White's narratological account of historical discourse, I argue that historical representation has an irreducible narrative dimension. Rather than simply relating historical events in the chronological order of their happening, historical narratives insert events into meaningful plot-structures that lead the reader through dynamic cadences. Narratives provide explanations of the reported events by identifying causal dependencies between them, and by familiarizing historical developments to our cultural repertoire of pre-existing plot-patterns. I further argue that (quite like scientific models) historical narratives make aim-dependent selections regarding which aspects of historical reality they represent. Finally, historical narratives are theory-laden since theoretical and methodological assumptions guide the selection of historical sources and because inferring facts from available sources involves complex interpretative maneuvers that implicitly or explicitly rely on theoretical background assumptions. These background assumptions often are related to philosophical issues.

The remaining question is how, given the narrative, selective and theory-laden character of historical reconstruction, historical case studies can provide evidence to philosophical theses. I address this question in the third part of my talk. First, I argue that some degree of pluralism needs to be acknowledged in the historiography of science, as there exist multiple possibilities for narratively recounting the same series of historical events that each come with their own selections and theory-laden interpretations. Second, if historical reconstructions are theory-laden and if the theoretical assumptions that enter the construction of historical data are at least partly identical to the philosophical claims a specific historical account is supposed to support, the possibility of two philosophical positions arriving at different accounts of the same historical episode emerges. I argue, however, that this does not imply that case studies cannot provide philosophical arguments with evidential support.

Aiming for a more differentiated picture of evidential support, I distinguish between four related but different evidential functions historical case studies may fulfill: (a) New knowledge: Can we learn something new from historical reconstructions? (b) Belief revision: Can a historical reconstruction force us to revise our beliefs? (c) Confirmation: Can a historical case study confirm a general philosophical doctrine? (d) Decision: Can case studies decide all philosophical conflicts?

The first two questions I answer affirmatively. I argue that while there may be plural theory-laden reconstructions of historical episodes, each of these can tell us something new about the historical world (a). Neither does my account preclude that theory-laden historical facts can turn out to be sufficiently at odds with the philosophical positions that guide their construction to force us to revise our beliefs (b). The third question (c), I answer with a qualified yes. I suggest that while generalizing from particular historical episodes to general philosophical claims is always potentially problematic, we should nevertheless prefer historically plausible philosophical claims to historically implausible ones. Only the last question (d), I give a negative answer to: In some cases, historical evidence is less than a neutral arbiter in philosophical conflicts, as different philosophical positions can produce competing theory-laden accounts of the same historical episodes. While historical case studies do provide evidence for philosophical arguments, this evidence is not always decisive of philosophical conflicts.

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Scientific Inference and the Earth's Interior: Harold Jeffreys and Dorothy Wrinch at Cambridge

Between 1919 and 1923, Harold Jeffreys and Dorothy Wrinch, who had both been educated at Cambridge in the 1910's, co-wrote a series of papers on a wide range of topics, from scientific inference to seismology. These papers were deeply influenced by discussions about epistemology, inference, and probability that were taking place among philosophers at Cambridge at the time. The work in these papers formed the basis for Jeffreys's later views on scientific inference and probability, and his development of seismological techniques for extracting information about the deep interior of the earth from seismic wave observations. The aim of this paper is to examine how the views of Wrinch and Jeffreys about scientific inference emerged in response to the work of the Cambridge philosophers W. E. Johnson and C. D. Broad, and then how the views of Jeffreys on inference developed in relation to the epistemological needs of the emerging field of seismology.

Harold Jeffreys is widely regarded as one of the founders of modern geophysics, a master in the application of physics to the extraction of knowledge about the interior of the earth. He and his student Keith Bullen developed the first detailed models of the interior of the earth based on observations of travel times of seismic waves. Jeffreys is also known to philosophers of probability for his objective Bayesian approach to the foundations of probability, and his debates with R. A. Fisher over frequentism. Dorothy Wrinch is not as well-known a figure as Harold Jeffreys, but her contributions to several different fields in the early twentieth century are underappreciated. Wrinch was a student of mathematics at Cambridge who later switched to philosophy after having heard Russell lecture on epistemology. She studied logic after 1916 under Russell, and spent some time as Russell's assistant. She is now most well-known for work she did after the 1930's in the application of mathematics to molecular biology, and research on protein structure in particular.

Between 1919 and 1923, Wrinch and Jeffreys co-wrote a series of papers that appeared in *Philosophical Magazine* and *Nature*, in which they cover a wide range of topics, including the testing of Einstein's theory of relativity, the theory of probability, scientific inference, and seismology. They are all connected by a concern for the application of ideas about scientific inference, arising from Cambridge philosophers such as W. E. Johnson and C. D. Broad, to open scientific problems of the day. Jeffreys was clearly encouraged by the potential that these ideas had, but they could not be straightforwardly applied to the sciences in which he was most interested. His interests were in astronomy and geology, having previously written a paper on the origins of the solar system, and another on the issue of testing the theory of relativity. These are sciences where the aim is to determine facts about particular objects—e.g., what are the mechanical properties of the material 1000 km under the surface of the earth? On the other hand, most of the discussions about inductive inference at Cambridge centered on enumerative induction. In enumerative induction, one generalizes from particular objects of some type to particular objects of the same type, whereas the kind of inference Jeffreys wanted to do involved inferring facts about particular objects (the interior of the earth) from facts about other parts of those particular objects (the surface of the earth). Wrinch and Jeffreys thus took ideas arising in Cambridge philosophical circles and developed them so that they could be applied to the scientific problems in which they were interested.

Jeffreys developed his views in response to the epistemological needs of his own research, which increasingly centered on seismology in the 1920's and later. Seismology is the science of trying to determine facts about the deep interior of the earth, given observations of seismic waves at its surface. There are two problems that are immediately obvious when one considers the epistemology of seismology. First, there is a worry about underdetermination. Observations can only be done at the surface of the earth, so radically different models of the interior of the earth could potentially be consistent with all observations. Second, there is a worry about idealization. In order to extract information about the interior of the earth, one must inevitably make certain idealizations, such as isotropy of the medium in the deep interior of the earth.

The views of Jeffreys about scientific inference often strike philosophers as ad hoc, but part of the reason for this is that Jeffreys develops his views in response to epistemological needs arising from scientific practice, particularly his work in geophysics and seismology. The epistemological standards of scientific practice might well diverge from those of philosophers, although it is open to question whether they ought to do so. This paper is not so much concerned with the philosophical viability of the views developed by Wrinch and Jeffreys, on which there is already an existing literature. Rather, we have here an interesting case where a scientist takes ideas that originally arose in philosophical debates, and attempts to apply them in actual scientific practice, and then these ideas are further developed in response to the needs of scientific practice. This paper thus focuses on the following questions: What did Jeffreys think was needed for a theory of inference and probability that would provide a sufficient foundation for his work in geophysics, and seismology in particular? How did the needs of geophysical research influence the way that Jeffreys thought about scientific inference? How, on the other hand, did ideas arising from philosophical discussions help Jeffreys to develop the methods of seismology?

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Neither Logical Empiricism nor Vitalism, but Organicism: What the Philosophy of Biology Was

Like a slow-burning story of triumph, the canonical narrative of the history of contemporary philosophy of biology tells the tale of a subfield emerging out of the smoldering ashes of logical empiricist philosophy of science, and the wreckage of an equally futile vitalistic program that preceded it. Most logical empiricists scoffed at the life sciences, and those who did deem it worthwhile to explore the biological realm produced nothing of value. The logical empiricists failed because their project was a prescriptive enterprise whose primary mandate was to bring increased rigour to biology by importing methodological protocols from the physical sciences. Vitalists of the early twentieth century were not stricken with physics-envy, but the animating forces and other metaphysical phantasms they conjured into existence to ward off the threat of reductionism were at least as ill-conceived as anything produced by the logical empiricists. Practitioners associated with the aforementioned schools failed to seriously engage with the science that allegedly inspired their musings, and as a consequence, the philosophy of biology languished in a state of futility for much of the twentieth century.

Things began to change sometime in the late 1960s and early 1970s, when the textbooks by Michael Ruse (1973) and David Hull (1974), together with a series of articles by Ken Schaffner (1967; 1969a; 1969b) and Bill Wimsatt (1970; 1972a; 1972b), found their way into print. These efforts are regularly identified as the first significant contributions to modern philosophy of biology. Unlike the logical empiricists and vitalists of previous decades, these thinkers focused on problems internal to biology, and it is this unapologetic emphasis on contemporary science which facilitated their success. Over time, replies were published, new topics were examined, and the prejudices the philosophical community formerly harboured against the life sciences faded away as the philosophy of biology grew into the recognized field of specialization that it is today.

For over three decades, the above account of the discipline's history has circulated within the community (see, e.g., Sober 1984: 6-7, Brandon 1996: xii-xiii, Kitcher 2003: xii, Matthen and Stephens 2007: xi-xii). Although details occasionally vary, it is widely agreed that the philosophy of biology as a discipline was born in the last third of the twentieth century, following years of neglect and a host of misinformed false-starts. No one has done as much to popularize this account as two of the story's lead characters: Michael Ruse and David Hull. Over the years, Ruse has been particularly vocal about the pivotal role that he and Hull played in the establishment of the discipline, noting that 'David Hull is the father of modern studies of biology from a philosophical viewpoint' (2008: 4), and crediting himself on repeated occasions as "one of the founders of contemporary philosophy of biology" (2006: 37). (See also Ruse 1997: 120; Hull and Ruse 2007: xix-xx; Takacs and Ruse 2013: 5-6)

Hull and Ruse have also done a great deal to spread the idea that philosophical work on biology prior to the 1970s was completely devoid of value. Consider, for instance, the following passage from Ruse's *Philosophy of Biology Today*:

> [I]n this century particularly, the philosophy of science has become almost a subdiscipline in itself. But this does not include the philosophy of biology—at least, it did not until very recently [...] [P]hilosophers of science in the twentieth century have focused mainly on the physical sciences, and any spare effort has tended to be directed toward the social sciences. What little attention has been paid to biology has been generally directed to one extreme or another. At one end of the spectrum we have those who were overly impressed by the turn-of-the-century formalisms of the logicians and mathematicians, and who wanted to do likewise for biology. Since they—especially their leader J. H. Woodger—were simultaneously empiricists of the most naively dogmatic kind, their efforts tended to go unread. At the other end of the spectrum we have those who feared and loathed materialism, and who were determined to prove that an understanding of organisms demands reference to vital forces or spirits—*elans vitaux* or entelechies—forever beyond the grasp of conventional science. (Ruse 1988: 1-2)

The purpose of this paper is to set the record straight about the history of the field. Through a combination of historical and philosophical analysis, we argue that the current account of what the philosophy of biology was prior to the 1970s, as exemplified by Ruse in the above quote, is almost entirely false. We do this by suggesting that the most important tradition within early philosophy of biology—the organicist school that flourished in both Europe and the United States in the interwar period—had no direct connection to either logical empiricism or vitalism. We also demonstrate the continuity of the organicist literature with the contemporary debates in order to cast doubt on the claim that nothing of value was produced during the first half of the twentieth century.

To be clear, this is not simply a priority dispute about what deserves to be credited as 'philosophy of biology'. It is an attempt to make contemporary philosophers of biology aware of a huge body of literature containing insights from philosophically-minded biologists and biologically-minded philosophers whose contributions have been almost completely neglected for nearly a century. Contemporary philosophers of biology should be standing on the shoulders of these giants, not their faces.

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The taxonomical and the morphological concepts of type: back to Aristotle

Although organismal form (i.e. the geometrical and topological properties of biological entities at the anatomical level) played a privileged role in biology until the end of the 19th century, the significance of morphology progressively weakened until its practical disappearance in evolutionary biology. From a philosophical perspective, the absence of Form in the Modern Synthesis was justified on the basis of its associa-

tion with typological thinking (Mayr 1959), and more generally with essentialism (Hull 1965), i.e. the definition of each species according to intrinsic, necessary, and sufficient properties. Typological thinking was claimed to be at odds with the population thinking introduced by Darwin, whose emphasis in individual variation became the theoretical pillar of the synthetic view of evolution.
However, since the late 1970s morphology has experienced a renaissance in evolutionary biology which has entailed the return of typological concepts such as 'type', 'Bauplan', or 'homology' (Amundson 1998; Brigandt 2007; Love 2009). This renaissance of morphology has challenged the received view on typological thinking in both the historical and the philosophical fronts. The progress in the historiography of pre-Darwinist biology has led to the revision of "history of essentialism" (Winsor 2003; Amundson 2005). In philosophy of biology, evo-devo's mechanistic interpretation of types has led to a vindication of essentialism where biological entities are seen as homeostatic properties clusters (Wagner 1996; Rieppel 2005).

In this presentation I will argue that many of the Modern Synthesis' historical and philosophical misunderstandings regarding typological thinking, derive from the conflation of the type concepts used in the two biological disciplines in charge of organizing organismal diversity, namely taxonomy and morphology. I claim that the epistemological goals of taxonomy (i.e. the classification of species) and morphology (i.e. the definition of organismal form) imply radically different type concepts.

In particular, I will focus on the historical origin of the conflation between the taxonomical and the morphological concepts of type (Farber 1976), which I trace back to different interpretations of Aristotle and particularly of his work on the *History of the animals*. Firstly, I will show how the Scholastic interpretation of the method of division led to see Aristotle as the founder of "taxonomical essentialism". Secondly, I will argue that the Aristotelian study of biological diversity can be considered as a morphological (not taxonomical) project. As shown by the contemporary studies of Aristotle's biology (Lennox and Gotthelf 1987), the epistemological goal of the *History of the animals* is not to classify but to define animals, and the method used to achieve this goal is not the Scholastic 'dichotomous division' of taxa, but the 'definitional division' of animal parts. I will conclude that this first attempt to understanding the logics of animal form is akin to contemporary theoretical morphology (Thom 1990) and rests on an idea of type which is not logically incompatible with evolution.

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"Control(led) experiments" in historical and philosophical perspective

Arguably, the introduction of controls is a key methodological tool in scientific experimentation. Yet there are surprisingly few historical and philosophical studies of the concept of experimental control, and what little there is does not form a coherent picture. There is some work specifically on the emergence and career of randomized controlled trials, focusing on 20th century psychological and medical research (Hacking 1988, Keating and Cambrosio 2012). Often R. A. Fisher's agricultural experiments from the early 20th century are presented as a milestone in the discussion (e.g. Hall 2007) Some historians have hinted at a connection between controlled experiments and the process of industrialization and have argued that the concept of experimental control emerged in the mid- or late 19th century (Figlio 1977, Pauly 1987). Other scholars have suggested that controlled experiments were already performed in the late 18th century (Dunn 1997); yet others date their origin back to the Middle Ages (Crombie 1952) and even to Antiquity (Knoefel 1988, Stigler 1974).

The historiographical conundrum has not been tackled; and broader systematic analyses of the concept, the epistemological significance of the practice of controlling, or the conditions of the emergence of the methodological idea behind experimental controls do not exist. In this paper, I seek to prepare the ground for such a broader analysis. I offer a historical and philosophical interpretation of control(led) experiments in the biomedical sciences, focusing on the second half of the 19th century. I disentangle different strands of the history of control(led) experiments, draw some crucial conceptual distinctions among different meanings of the concept of control, and identify a number of questions that a historical and philosophical analysis of control experiments need to answer.

First of all, it is obviously important to distinguish between the emergence of the terms "control experiment," "experimental control", etc. and the history of the methods or strategies of experimentation that these terms refer to. Based on this distinction, a simple solution to the historiographical conundrum suggests itself: Perhaps the experimental strategies that came to be called "controls" had been applied long before the introduction of the term – maybe already in Antiquity – even though the methodological terms "controlling", "control experiment," "(experimental) control", etc. emerged in the second half of the 19th century? However, it seems to me that if we adopt this solution, we overstate the similarities between experimental strategies, and we downplay differences in the different contexts and historical settings in which these strategies were applied and in the significance that past experimenters attached to them.

I begin my presentation with a survey of concepts of control in late 19th-century bacteriology, immunology, and experimental embryology. I pay particular attention to the works of influential and methodologically reflective investigators, especially William Henry Welch, Paul Ehrlich and his co-workers, and Jacques Loeb. The concept of control plays an important role in all of these works. But it is used in at least three ways: to refer to a strategy that "controls for" the impact of specific factors on the outcome of experiments, to refer to a practice that corrects for unknown variables in the experiment, and to refer to the calculated design of new forms of organic life.

In the second part of my paper, I consider several 19th-century methodologies of experimentation that had an impact on methodological thought in late 19th-century biomedicine, namely the methodologies advocated by the French clinician Pierre Louis, John Stuart Mill, Claude Bernard, and the German embryologist Wilhelm Roux. While none of these methodologies mentioned the concept of control, each of them introduced strategies of securing experimental results that involved elements of comparison. But there are significant differences with regard to what was compared and for what purposes. According to Louis, experiments could be made more secure by comparing two populations, one of which receives treatment. According to Mill and Roux, causal factors can best be identified if two experimental situations are compared in which all conditions are held constant except the one under study. According to Bernard, experiments could be made more secure if a specimen is compared to a second, which is subjected to the same treatment except for a change in the variable under study.

In the third part of my paper, I bring the first two parts together and draw out a number of implications for a historically and philosophically informed account of control(led) experiments. Obviously, "the" history of experimental controls does not exist. Rather, we need to distinguish at least two traditions in the discussion about controls, the comparison of populations and the comparison of individual experiments. The works of Louis and Fisher are part of the first tradition, but during the 19th century there was little discussion about the problem of comparing populations (Coleman 1987). The second tradition – the most relevant for methodological thought in late 19th-century biomedicine – includes works by Mill, Bernard, Roux, Welch, Ehrlich, and others. In this tradition, the introduction of the term "control" came together with a loss of trust in the practical applicability of Mill's method of difference. We find criticisms of Mill's method in the writings of both Bernard and Roux. The concept of control came to be used after these experimenters had advanced the view that Mill's methodology of experimentation expressed an unattainable ideal, and that Mill's method could not address the most pressing problems of scientific experimentation in the life sciences – the complexity of living things. Finally, Jacques Loeb's notion of "control" is the only concept that can be traced to an engineering context (Pauly 1987). But if we read Loeb's work against the background of contemporaneous methodologies, it becomes immediately clear that he did not use the term "control" in a methodological sense.

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Why the dilemma of case studies misses the point: Towards an explicit methodology for integrated history and philosophy of science

One of the challenges for an integrated history and philosophy of science is "the dilemma of case studies": the argument that neither a "top-down" nor a "bottom-up" approach is obviously fruitful (see Joseph Pitt, 2001, Perspectives on Science). On the one hand, if we start with philosophical theses and proceed "downward" to historical cases, we must always suspect that the cases were chosen so as to fit our philosophical preconceptions. In other words, cases can never give real support to philosophical theses because of the possibility of selection bias. On the other hand, if we start with history of science and proceed "upward" to philosophy, then we do not have any obvious warrant for generalizations: Proper support for a philosophical thesis cannot derive from its applicability to one, two or even several cases.

Instead of accepting these challenges as refutations of the integrated approach, the dilemma of case studies should be taken as an opportunity: It points towards the need for an explicit methodology for the practice of integrated history and philosophy of science.

Where skeptics worry about selection bias, we argue that robust criteria for the choice of historical cases are required. Among the categories we propose are paradigm cases and hard cases. Paradigm cases are historical episodes which are already considered to be typical of particular aspects of science (say, confirmation) – and which thus may be used to make new points particularly effectively. Hard cases are chosen in order to make confirmation bias unlikely: They are structured such that they challenge rather than illustrate the philosophical thesis under consideration. The difficult question, of course, is what makes a case "hard".

Where skeptics argue that generalizations from historical cases are unwarranted in principle, we prefer to formulate fruitful procedures for dealing with either a match or a mismatch between philosophical theses and historical cases. For instance, instead of rejecting a philosophical thesis based on one or two counterexamples, counterexamples may indicate that a domain cannot be subsumed under a single philosophical category (e.g. several categories of explanation may exist, each of which finds counterexamples in the others). By contrast, when historical data matches a philosophical thesis, this should be understood not as straightforward "support" but instead as the beginning of an exploration of the range of applicability of the thesis.

We will illustrate each of our theses using cases from our own research in HPS. These include, among others, Semmelweis's discovery of the cause of childbed fever, the development of photosynthesis research in the late 19th and early 20th century, Volterra's predator-prey model and Mitchell's chemiosmotic theory.

This is a synthetic presentation of a number arguments and conclusions presented at a recent workshop titled "The philosophy of historical case studies", held at the University of Bern on November 21-22, 2013 (http://hpsbern2013.wordpress.com).

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Heuristic Reevaluation of the Bacterial Hypothesis of Peptic Ulcer Disease in the 1950s

Many historical accounts of the research on peptic ulcer disease (in short, PUD) roughly distinguish three phases separated by two landmark studies. In the first phase (from the second half of the 19th century to 1954) two main hy- potheses were investigated in parallel: on the one hand, the acidity hypothesis according to which the cause of PUD was gastric acid, and on the other hand, the bacterial hypothesis according to which the cause of PUD were bacteria. Neither hypothesis gained a decisive break-through in terms of theory confirmation, nor suffered from severe refutations. The situation changed in 1954 with the publication of a large-scale study by Palmer (1954) which challenged the bacterial hypothesis with serious refutatory counter-evidence. According to (Kidd & Modlin, 1998, p. 10), Palmer's study "may be credited with the envious distinction of setting back gastric bacterial research by a further 30 years". Similarly, Fukuda et al. (2002) suggest that Palmer's study "established the dogma that bacteria could not live in the human stomach, and as a result, investigation of gastric bacteria attracted little attention for the next 20 years" (p. 20).

Only in the 1980s the bacterial hypothesis had its come-back, culminating in a study by Warren and Marshall (1983; 1984), who managed to identify one of the main causes of PUD in Helyobacter Pylori. After their results have been confirmed by other scientists, the bacterial hypothesis was accepted (Thagard (2000), Solomon (2001)), and in 2005 Warren and Marshall were awarded the Nobel Prize in Physiology or Medicine for this discovery.

In this paper we investigate the status of the bacterial hypothesis after the publication of Palmer's study. We focus on the question whether the bacterial hypothesis was still worthy of further pursuit at this time. According to some scholars, Palmer's study had an impact of a crucial experiment, which clearly refuted the bacterial hypothesis. For instance, (Zollman, 2010, p. 21) writes that after Palmer's results "everything was 'done by the book' " and that "one can hardly criticize their [the researcher's] behavior" when abandoning the bacterial hypothesis until the new study of Warren and Marshall turned the tables. Hence should Zollman's assessment be adequate, our question regarding the pursuit worthiness of the bacterial hypothesis would have to be answered with a decisive "no".

We argue for the following two theses:

The perceived refutatory impact of Palmer's study is disproportionate to its methodological rigor. This undermines its perceived status as a crucial experiment against the bacterial hypothesis. Of special interest for our research question is the staining method used for detecting bacteria in Palmer's study. We begin our inquiry by asking what Warren and Marshal did differently 30 years later that allowed them to demonstrate the bacteria. The key difference in their method is the type of staining which the latter authors applied to the specimens under examination. In contrast to Palmer who used hematoxylin and eosin (H&E) impregnation, which is an excellent stain for displaying tissue morphology, Warren and Marshall applied silver staining, which showed Helicobacter well enough (Marshall & Warren, 1984). The question which immediately comes up is why Palmer did not use silver staining, or more precisely: Was the method of silver staining already well known by 1954 (the year when Palmer's article was published)?

In case the method of silver staining was indeed well known by this time, were there good epistemic reasons available already at that time, that silver staining should have been considered a significant method for the detection of spirochetes in gastric mucosa?

Were the shortcomings of the H&E method known by 1954, especially in the context in which it was used in Palmer's study? In other words, were there good reasons available at the time, that the H&E method should have been considered possibly problematic for the detection of certain spirochetes in gastric mucosa?

By answering these questions we are able to evaluate the reliability of Palmer's results.

In view of this and other considerations we argue that the bacterial hy- pothesis was worthy of pursuit in the 1950s. The question of the pursuit worthiness is best answered by means of a heuristic appraisal. Two concerns are of importance to this end:

The question whether there was enough of a protective belt for the bacterial hypothesis to give researchers – in Lakatos' wording – a "rational scope for dogmatic adherence to [their] programme in face of prima facie 'refutations' " (Lakatos, 1978, p. XX) such as Palmer's study. This concerns the question of negative heuristics.

The question whether the bacterial hypothesis was not stuck in terms of available research options. This concerns the question of positive heuristics which opens research venues and hence gives researchers problems or puzzles to work on and, in turn, options to refine and improve on their previous models.

The philosophical message to take home from this case is a message of the potential fruitfulness of methodological critical scrutiny for the practicing re- searchers on the one hand, and the fruitfulness of a close reading of the historical material for the philosopher interested in case studies on the other hand. Moreover, Zollman (2010) uses this case study to illustrate why scientific progress would benefit from a restricted information flow among scientists. Our results suggested the opposite: that the information flow among scientists was subop- timal in this particular case. We close the paper by mentioning a number of sociological and other factors that require further examination for this thesis to be substantiated.

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Retreading the Path of Science: the case of independent motions

The transformation of the subject of motion from pre-classical mechanics to the Newtonian world is an important part of the scholarship that integrates history and philosophy of science. In this paper, I bring to the fore the topic of conceptualizing the independence of motions and its empirical grounds. First, while Galileo and Descartes worked with rather different underlying conceptual assumptions of what makes two motions independent, in their common examples they rely on a similar inference-guiding rule of discerning between motions. Secondly, I show that several assumptions that they used are unmotivated by the conceptual tools that are available to both of them. Finally, I show how some of Newton's own struggles in trying to come up with a robust mathematical rule for composing motions were a reply to challenge of identifying a reliable notion of independence for motions.

There are at least two different ways of thinking of independence, depending on how motions are identified in the first place: the kinematical approach(dealing mainly with velocities) and the dynamical one (regarding forces). As I show, from a philosophical point of view, the role of diagrams and the geometrization of motion played a crucial role in how non-interference of motions is represented. This paper argues that Galileo's conceptualization of independent motions comes from his experiments and is represented in his diagrams, where the latter are constructed with the purpose of being accurate representations of natural motions (motions that would be empirically observable). On the other hand, Descartes's conception of independent motions is connected to the geometrical descriptions of curves and now the difficult problem is to find the corresponding motions within an orthogonal system of coordinates.

The structure of my paper is the following.

1) I begin by presenting how motions are identified and composed in pedagogical examples: a boat crossing a river, the motion of a projectile, etc. Quite often, students find the consequences of the mathematical (vectorial) counterintuitive. On the other hand, they find the mathematical (vectorial) representation almost trivial. I argue that the same reactions are recognizable, albeit in a rather different form, in Galileo's treatment of projectile motion in his Dialogue on Two New Sciences.

2) In the second part of my paper I develop the Galilean answer to the question: What are the component motions and what makes them independent?. I show that the mathematical representation by means of a diagram is necessarily part of the answer. In the case of Galileo's projectile's path, the motions that are independent are also on perpendicular directions. While this happens to be accurate for the particular motions that Galileo looked at, we could ask whether velocities that are perpendicular are also the representation of independent motions in general. Galileo understands this question in a particular way: Is this an empirically adequate representation of all projectile motions? For this reason his answer in the Dialogue on Two New Sciences only addresses the constraints under which his diagram is useful, but does not aim at a deeper understanding.

3) In his Principles, Descartes gives several examples where motions are decomposed on orthogonal directions. As I show, in some cases we seem to be forced in identifying one component (when the stone in a sling is released), in others the choice of motions seems arbitrary (the point on a circumference on a wheel). On the other hand, Descartes' geometrical work reveals a different conceptualization of independence of motions under which certain curves are not considered geometrical. While Descartes recognizes the need to use independent motions in generations of some curves, he doesn't have the tools to give a determinate definition of this geometrical independence. Moreover, this failure forces him in part to exclude mechanical curves from the subject of geometry.

I argue that the Galilean and the Cartesian ways of understanding independence are in tension with each other and the paper concludes that a way of reading some of Newton's works is to follow his attempt at reconciliation. My answer delineates the philosophical challenges we meet when trying to find a sharper definition of independence of motions. If motions are described by velocities, then the velocities that are represented as orthogonal are independent in the following sense:

Adding one motion to the other does not affect the first.

I would note in passing that this formulation also includes a symmetry condition (i.e. it doesn't matter which motion is added) which the empirical Galilean and the Cartesian examples do not satisfy. However, the empirical problem is that in most cases we cannot separate/add de facto one motion from/to the other. Then, the empirical rule used to identify and describe independent motions is the following principle:

Any change that is designed to affect only one of the motions will not change the other.

My paper shows that, while they used the second rule, Galileo and Descartes have insufficient conceptual resources to theoretically motivate and isolate such changes in a predictive and consistent fashion. Why are independent motions represented by an orthogonal system? On their own, the rules above give no justification for why certain identified motions also happen to satisfy the perpendicularity condition. In the case of Galileo and Descartes, their diagrams are not mathematical explanations, but are generalizations from observed cases. Once imported in representation, the perpendicularity became a useful and fruitful assumption that is not justified by or inferred in any way from their respective conceptual frameworks. Finally, I show how this conclusion was part of the conceptual development of Newton's own works.

Read as a contribution to the history and philosophy of science, my paper shows that scientific understanding is achieved by retreading the path of certain problems while emphasizing the historical constraints- both conceptual and practical- under which the answers are sought for.

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"Beyond the conventional boundaries of physics": On relating Ernst Mach's philosophy to his teaching and research in the 1870s and 80s

Ernst Mach's most well known critiques of mechanics concerning mass, inertia and space and time were conceptually motivated by the aim of avoiding unnecessary assumptions and basing the concepts of physics upon measured relations, and they were first published in the years around 1870 (for mass and inertia) and in his well known 1883 book Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt. Philosophical discussion of them has reflected these conceptual concerns, and related Mach's critique to his emphasis on the economy of thought. Yet manuscript records of Mach's teaching in the 1870s shows that his approach was animated also by the concerns of psychophysics and the relations between inner and outer worlds, and his publications attest to these broader interests also. In the 1870s, for example, Mach developed physiological studies of the sense of motion, and soon after his critical history of mechanics was published in 1883, his 1886 Beiträge zur Analyse der Empfindungen was intimately concerned with the relations between physiology and psychology. By investigating Mach's research across subject matter that has usually been treated separately, and seeking to integrate his teaching and research also, this paper aims at offering a study of Mach's philosophy as it is revealed in practice. Indeed, Mach offers a highly unusual example whose primary aim was to reform his own discipline of physics through the concerns of other disciplines, something he alluded to in 1886 when stating that he expected the next great enlightenments of the foundations of physics to come at the hands of biology.

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The Modelling Attitude and its Roots in 19th Century Science

Abstract:
I locate the origins of the contemporary model-based scientific methodology in the 'modelling attitude' of philosophically minded scientists in the second half of the 19th century. I distinguish an English speaking modelling school (identified with William Thomson, James Clerk Maxwell, and their followers in Victorian British physics), and a German-speaking modelling school (identified with Hermann Von Helmholtz and his Berlin school, as well as Heinrich Hertz and Ludwig Boltzmann). I argue that both schools share a commitment to the 'relativity' of knowledge, and a consequent emphasis on reasoning via models as the main method for the acquisition of knowledge about the natural world.

Extended Abstract:
In this talk I shall defend three interrelated claims: i) The historical roots of the 'modelling attitude' that is at present dominant in the physical sciences is to be found in late 19th century science; ii) this genealogical foundation suggests some philosophical foundations of the modelling attitude in what I shall refer to as the 'relativity of knowledge' thesis; and iii) this thesis in turn both grounds and lends credibility to current deflationary accounts of scientific representation.

The birth of science as a social institution in the 19th century coincided with the heyday of what I call the modelling attitude. Of course natural philosophers had employed models before, and had reflected upon the nature of those models. In fact, before science and philosophy parted ways, decisively at some point in the late 19th century, the building of the model and the philosophical reflection upon its nature often went hand-in-hand. But it is only, I argue, in the 19th century that a 'modelling attitude' emerges, as a systematic attempt to articulate and defend model building as the appropriate methodology for science.

The 19th century modellers introduce at least two novel elements. There is first a self-conscious emphasis on the hypothetical and even fictitious nature of the models; and, second, modelling became very sophisticated mathematically, particularly in the hands of what I call the German speaking school – spreading from 3-d geometry into complex versions of the calculus, and the algebra of equations. None of these elements are present in pre-19th century modelling, yet they are all central to contemporary modelling practice. Thus systematic reflection upon models begins in earnest with the electro-dynamical models of the ether, and there are in particular two sources or schools.

There is first an 'English speaking' school led by James Clerk Maxwell and William Thomson (Lord Kelvin), but encompassing also many of the other celebrated British physicists of the 19th century, such as Oliver Lodge, George F. Fitzgerald, Oliver Heaviside, John Poyinting or Joseph Larmor. Maxwell and Kelvin advanced a number of methodological considerations in their many attempts over the years to model the ether as a concrete physical medium of vortexes. These considerations mainly bear on the importance of the modelling attitude to their ongoing development and understanding of electric and magnetic phenomena. There is then a somewhat later 'German speaking' school represented mainly by Heinrich Hertz and Ludwig Boltzmann and heavily indebted to Helmholtz's methodology of physics. Partly under the influence of the English speaking school, but mainly as a result of an ongoing process that begins with Helmholtz's research into the nature of perception, the German theoretical physicists gradually develop a more abstract and theoretical sort of modelling and provide a cogent philosophical defence for it. The defenders of the bildtheorie have a philosophical agenda – nuanced and sophisticated, even in contemporary terms (an agenda that in Botzmann's case, at least, was linked to a defence of the tenability of the atomic hypothesis). Thus the modelling attitude is born in Britain but it grows of age and acquires the mature form that launches it into the 20th century – and that in essence endures to the present day – in the hands of the skilful 'German speaking' school.

My second claim is that the origin of the modelling attitude is historically and conceptually linked to a thesis that I refer to as 'the relativity of knowledge', and which has origins in the Scottish enlightenment and philosophy of common sense. According to this thesis scientific knowledge is never atomistic in the sense that it is never absolutely and exclusively of its own object. On the contrary, knowledge can only emerge out of a comparison of the object with something else.

Comparison, likeness, resemblance and analogy are therefore all means to achieve knowledge, and in fact the only means through which genuine empirical knowledge of the world can possibly come about. This 'relativity of knowledge' thesis (not to be confused with any form of contemporary 'relativism') is in turn the result of applying to the objects of empirical science the method of abstraction that had been developed in connection with mathematical knowledge by distinguished Scottish mathematicians (such as Simson and McLaurin) as early as the first half of the 18th century. For Simson, for instance, 'surface' is an abstract concept that results of a comparison of a real solid with an imaginary model of the solid split in two perfect halves, none of which can possibly contain the intermediate surface on pain of contradiction (since if the surface was contained in one of the halves it would the necessarily be missing in the other half, contrary to what is the case in the real solid once one half is in fact removed).

Finally, I sketch the argument that takes from the 'relativity of knowledge' to deflationary conceptions of representation. The claim is not that deflationary theories are a consequence of the relativity of knowledge thesis, but rather the opposite: deflationary conceptions of representation entail that analogical (or, more generally, surrogative) inference is essential to scientific representation. This makes the 'relativity of knowledge' thesis plausible, since it is a natural corollary of surrogative or analogical inference that all knowledge is comparative in the way required by the thesis. By contrast, substantive theories of representation do not support the relativity of knowledge thesis, but instead render it a mystery that modelling should essentially depend on comparative knowledge. On these views, instead, the comparison between a real and an imaginary case can only provide knowledge to the extent that it rides upon some pre-existent relation between two real entities or objects, and there is nothing in the process of abstraction per se that yields additional knowledge. Thus, to the extent that the modelling attitude is historically dependant upon the relativity of knowledge thesis, scientific representation via models makes is likely to be a deflationary concept.

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Values, Facts and Methodologies: A Case Study in Philosophy of Economics

Attempts to distinguish facts and values in social science and theorizing about social science are met with considerable skepticism nowadays. My point will be to urge caution with regard to such global skepticism by showing by example that even in a debate in an area that is particularly vulnerable to value bias it is sometimes possible to assess arguments on a value-neutral basis.

My case study concerns a notorious episode in twentieth century political economy, the socialist calculation debate between Ludwig von Mises and Otto Neurath (itself but one episode of a both older and longer lasting dispute). I will show that it can be argued that, despite the prima facie plausible claim by Mises to have become the victim of ideological prejudice, it is possible to adjudicate the dispute on more objective, namely epistemological and methodological, in short "procedural" grounds.

Today, of course, political economy is widely recognized as a normative discipline, so one might not expect a resolution of such an argument at all. But just in this seeming incongruity lies the interest of the case at issue. Given that in Mises's hands the debate pertains to a putative impossibility result, there is a factual element to be assessed: it cannot be argued that it is all a matter of perspective. Yet at the same time, the impossibility result had policy implications so naturally its presuppositions are under particular scrutiny. So we are here presented with a case where the policy conclusions that can be legitimately drawn from a scientific investigation depend for their force on whether the methdodological framework employed by that discipline is acceptable—and this raises the spectre of scientific methodologies being chosen or rejected for political reasons. My resolution of this quandary—in this instance—turns on the often overlooked fact that this episode of the socialist calculation debate does not only illustrate the problem of the possibility of objective social science, but also involves the doctrine of the separation of the natural sciences from the Geisteswissenschaften.

The talk will develop three hypotheses. The first hypothesis is that the separation of the distinctive methodologies and/or ontologies of the natural and social sciences has equally important roots—besides the work of Dilthey and Windelband—in the Methodenstreit, the methodological dispute between the so-called German Historical School headed by Gustav Schmoller and the Austrian School of Exact Economics led by Carl Menger. The second hypothesis is that the self-segregation of some economists as Geisteswissenschaftler plays a central part in how arguments in the debate between Mises and Neurath were—and are to be—evaluated. The third hypothesis is that the issue was resolved—in so far as it was resolved at all—only for the price of a significant change in how the anti-socialist calculation argument was put, namely by removing its dependence on its geisteswissenschaftlich foundations.

What did Mises' allegiance to Geisteswissenschaft amount to in this debate? Mises' claim in 1920 was that "rational economics" was impossible under the conditions of marketless or even market-restrained socialism. Essential to his argument was a particular conception of the rationality of economic agents and that conception in turn was derived from what he deemed purely a priori determinations of principles of rationality. With this argument in place, Mises denounced the opposition of willful rejection of his a priorist methodology on nothing but an ideological basis. Now Neurath did indeed reject Mises' methodology and as a later logical positivist was happy to accuse any recourse to Geisteswissenschaft as reactionary ideology under a methodological guise. Thus the stage was set for the kind of a seemingly irresolvable clash of values that we nowadays regards as definitory of political economy.

Nowadays, however, it is also held that the socialist calculation debate—at least as far as it concerned the marketless socialism argued for by Neurath in the German revolution of 1918/19 (there are other versions on offer nowadays where the case is by no means as clear)—was resolved and that it was so on factual grounds. Suppose that is so. I will show that what made it possible to move the argument outside of the contested ambit of politically motivated methodologies vs. methodologically motivated politics was a change in argumentative strategy on the part of anti-socialists. That resolution was achieved by Friedrich August von Hayek's later refashioning of Mises' calculation argument in 1938-45. Even though Hayek's argument is in many respects continuous with von Mises', it differs in precisely this respect that reliance on geisteswissenschaftliche methodology was no longer essential and that its logic was readily intelligible to thinkers of more empiriccal orientations.

In sum: here an instance of the perennial problem of social science-vs.-ideology was "resolved" (albeit only to the debatable extent of what was resolved: just how much socialism has been shown to be "impossible" remains an issue of contention) not by appeal to a universal vademecum, say a more or less crude application of Max Weber's demand for the value neutrality of social science (which in his hands though was not at all unsophisticated) by ruling out all value judgements in science, nor by a blanket acquiescence into value-ladenness and a whistful abandonment of the idea of objectivity in social science, but notably by careful attention to particulars of the case and the procedures of establishing intersubjectively valid truth claims.

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Negotiating a causal-historical theory of reference: the emergence of the 'type method' in 19th century biological taxonomy

The Kripke-Putnam causal-historical theory of reference (Kripke, 1980; Putnam, 1975) has been strongly criticized as a general theory of reference about theoretical terms in science. Yet, as David Hull (1982) already noted, it appears that the causal-historical theory describes correctly how species names refer. For each newfound taxon, biological taxonomists lay down a 'type specimen' that carries with it the name of the taxon it belongs to. This 'type method' enables any two taxonomists to agree on the correct name of a given taxon, regardless of any disagreement they might have about what the true taxon boundaries are, and independently of future changes in taxonomic knowledge. In other words, in contemporary taxonomy a type specimen fixes the reference of a taxon without defining it.

In a fascinating article, Lorraine Daston (2004) has retraced how the type method came to be. Using a backdrop of 'epistemic virtues' and 'regimens of representation' that structure her well-known work on the history of objectivity with Peter Galison (Daston, 1999; Daston & Galison, 1992; 2007), Daston reconstructs how, in the late 19th century,

William Whewell's 'Method of Type' gradually evolved into the modern 'type method'. This process, she argues, was one of 'metaphysics in action' or 'applied metaphysics', since the taxonomist who "eventually laid down the type method for preserving the stability of names, were primarily concerned with practices, not philosophy. Yet it was precisely their gradual articulation of a set of practices (publishing, labeling, traveling, referencing, compiling) centered on a collection of objects (type specimens), that is, an art of transmission, that turned [the type method] into a remarkable act of applied metaphysics, or so I shall argue." (Daston, 2004, p. 157).

In this paper, I will argue that although Daston is right to direct attention to the 'metaphysics in action' of nineteenth century biological taxonomy, she misunderstands the nature of the metaphysics that was being negotiated. She fails to see that type method shows the causal theory at work. I will show that error has important repercussions, not only for Daston's account of the history of the type method, but also for her broader account of the history of objectivity.

The history of the type method

Daston's basic error resides in her assumption that type specimens not only function as reference-fixers of taxon names, but also serve to represent, describe and define the taxa they are part of. Starting from this false premise, Daston sets out to retrace how Whewell's 'Method of Type' (Whewell, 1840), on which type specimens did serve as representative standards of comparison for their encompassing species, evolved into the modern type method through addition of the function of name-bearing. In reality, the notion of a type specimen underwent a more radical change in meaning. 'Type specimen' lost its old connotation of a typical standard of comparison, and came to refer to a standard of reference. Today it is true that no matter how atypical a type specimen is judged to be, it can still serve its role on the modern type method. On Whewell's Method of Type, on the other hand, an 'atypical type' would have been a conceptual impossibility.

By delving further into mid-19th century debates on naming in taxonomy than Daston has done, I will show this fundamental change in meaning of the type specimen came about in a surprisingly gradual process. I will show how extensive debates and negotiations between professional and amateur taxonomists, and between those working in the peripheries versus at established museums, slowly altered what was understood by a 'type specimen'. Where in the 1840s a type specimen was still universally understood to be a specimen that was deemed typical for its taxon according to the trained taxonomist's judgment, the end of the 19th century had brought communal agreement about a type specimen being 'fixed as typical' by the first taxonomist who deemed it typical. From a philosophical vantage point, this meant that a causal theory of meaning was substituted for a descriptivist theory of meaning. The determination of 'types' no longer relied on (subjective) judgment, but on (objective, communally recognized) stipulation.

The history of 'objectivity'

Because Daston fails to understand what the type method amounts to, she also fails to see that the framework that structures her account of the history of objectivity does not apply. As soon as one realizes that the type method is a method of naming taxa, and not of representing them, it becomes clear that Daston and Galison's account of shifting 'regimens of representation' won't deliver any insight about the present case. What is more, the actual history of the type specimen shows that in an important sense Daston and Galison's framework is too narrow, since it does not take into account how objectivity about reference standards was created in the 19th century. Thus, the actual history of the type method shows that their categories of 'mechanical objectivity' and 'structural objectivity' leave an important aspect of the history of objectivity unaccounted for.

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