Hanne Andersen (Copenhagen)

The Changing Roles of Method

The idea of a special scientific method has figured prominently in past as well as contemporary discourses on a range of topics related to science, including the content of science education, the use of science in the courtroom, preferences of funding agencies, and definitions of scientific misconduct and research integrity. In this talk, I will present an overview of these areas of discourse and their development, and discuss how they relate to philosophical debates on scientific method.

Evan Arnet (Bloomington)

Methodological Dictums in the History of Science: Lloyd Morgan’s Canon

Morgan’s canon stating, “In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale” is perhaps the best-known sentence in psychology. According to disciplinary lore, assiduous adherence to the canon led to the dominance of deflationary accounts of mind in animals throughout the 20th century. I trace the canon through the history of comparative psychology, examining how it was understood, described, and employed. I argue that the discourse around methodological dictums like Morgan’s canon is fertile ground for the philosophical and historical exploration of methodological thinking. The creation of such a dictum, almost by nature, involves the compression of a lengthy and sophisticated justification into a terse few sentences. The relationship between this justification and specific employments of the canon can be, to put it mildly, complex. But by attending to this relationship, I contend we can gain insight as to the movement and development of methodology within science over time. Methodological dictums ultimately remind of us both the relevance of abstract thought and reasoning to science, and the messy way in which methodological thinking becomes scientific practice.

Uljana Feest (Hannover)

Operational Analysis and the Investigative Process in Twentieth Century Experimental Psychology

When we look at methodological writings in psychology from the 1940s-1960s, there are some striking parallels to debates and proposals that we find in philosophy of science some decades later. In particular, the multi-trait-multi-method approach by Campbell & Fiske (1959) notably anticipates the notion of robustness as it was formulated by William Wimsatt (1981). This is no coincidence, as Wimsatt’s paper originally appeared in a volume in honor of Donald Campbell. The notion of robustness, as applied to experimental reasoning, is typically taken to amount to the idea that measurement procedures mutually validate one another if they can be shown triangulate on the same object of measurement, which in turn also boosts our beliefs in/about the object in question. This analysis takes a rather mechanical view of the validation process, and moreover presupposes that concepts and measures are already in place. It therefore does not address the materiality of the process whereby concepts are formed and measures are constructed in the research process. As recently argued by Schupbach (2018), it also does not provide us with the analytical tools to understand the specifics of historical case studies, such as the famous case of Perrin. Schupbach argues that robustness analysis is
best understood as a process whereby scientists gradually converge on theoretical posits by systematically ruling out competing explanations of empirical data. In my talk I will argue that this analysis can fruitfully be adopted by a philosophy of experimental practice that focuses on analyzing investigative processes. Moreover, I will show that a very similar normative proposal was already put forward in psychological methodology in 1956 by Garner, Hake, and Erikson, who drew attention to the importance of probing the specific conceptual assumptions that go into research designs. In this vein, I hope to contribute to, and highlight the importance of, the dialogue between general philosophy of science and methodological reflections within science.

REFERENCES


Becca Jackson (Bloomington)


In Alvan Feinstein’s seminal Clinimetrics, he outlines why some of the criteria we typically look for in our evaluation of metrics (such as objectivity, preservability, interval scale) are not always necessary in the clinical context, and can even be detrimental (Feinstein 1987). He proposes a substitute for clinimetricians to look toward for evaluating the “hardness” of measured data: consistency (often referred to as “reliability,” in the psychometric context). He defines a “consistent” metric as one whose procedure which always leads to similar results, and claims we can evaluate consistency without needing to take into account what we are measuring. In this paper, I dethrone even consistency (as defined above) from being an a priori desiderata for evaluating the appropriateness of a measure, without regarding the relevant features of what we are measuring and our purposes for doing so. I argue that a phenomenon which has a distribution of different legitimate states (not just “noise”), which is measured well, will not be measured “consistently,” in Feinstein’s conception of the word. For these phenomena, no consistent measure could be valid. Ultimately, I argue that, in the same way that Feinstein says we need some “gold standard” for evaluating validity, we also must have some “gold standard” for evaluating consistency. That is, we need an idea of what distribution of results we should expect our measure to be consistent with, rather than assume that our gold standard of consistency is always a uniform distribution (or even a normal distribution).

**Jared Neumann (Bloomington)**

*The Interconnected Progress of Language, Mind, and Science*

In reaction to the theories of language offered by Descartes and Locke -- that ideas precede the signs that denote them, and those signs are essentially arbitrary -- several eighteenth-century philosophers argued that language was more historically dynamic and epistemologically active. Thus Étienne Bonnot, Abbé de Condillac (1714-1780), David Hartley (1705-1757), Adam Smith (1723-1790), and others interested in the origins of human knowledge traced the history of language to its role in capturing primal sensations and needs. As human knowledge became ever more general and abstract, so too did language. Signs were first used to pick out particular dangers, then particular objects; and when multiple objects elicited the same ideas, the signs were extended to denote whole classes. The history of language was the history of the progress of the human mind as it synthesized new experiences to fill out the complex lexica we enjoy today. And, as the classes denoted fluctuated with changes in knowledge, their corresponding signs required reform. In some cases, these philosophers even argued that not only did natural reasoning influence language, but language itself influenced the mind, going so far as to conclude that thought itself is impossible without it. It is well known that Antoine Lavoisier and his fellow chemists involved in the reform of chemical nomenclature were indebted to Condillac, specifically, for his theory of language. For, if classes are ill-defined, any corresponding science is prone to errors. However, in this paper I will show that this was not the only influence of eighteenth-century theories of language on science. While the development of new nomenclatures, technical terms, and definitions in the nineteenth-century are typically understood as signaling the formation and professionalization of various scientific disciplines, they were also consciously grounded by a belief in the close interconnection of the progress of knowledge and the progress of language toward ever greater levels of abstraction. I will focus specifically on the British context in which the analytical power of language as an instrument of discovery, clarity, and education was embraced by William Whewell (1794-1866), Samuel Coleridge (1772-1834), Dugald Stewart (1753-1828), Richard Whately (1787-1863), and others in a critical continuation of eighteenth-century ideas.

**Kärin Nickelsen (Munich)**

*Methodological Innovation and Scientific Change: How Manometry Reconfigured Twentieth-Century Photosynthesis Research*

How does a change in method affect a research field? In my paper, I will address this large question at the case of photosynthesis studies in the first decades of the twentieth century. Photosynthesis was then conceived as the conversion of carbon dioxide to sugar and oxygen in green plant cells under the influence of light. The main questions were, first, how the carbon dioxide was chemically reduced, and, second, how units of sugar were formed from the small one-carbon elements. The methods available were limited: input-output procedures combined with chemical analysis in order to identify the compounds, and analogical inferences on reaction paths from in-vitro observation.

In 1919, the cell physiologist Otto H. Warburg entered the field and introduced a new experimental system to study photosynthesis: a specific manometry performed on suspensions of unicellular green algae of the genus Chlorella. Warburg no longer tried to chemically identify the intermediates of the path but measured the process’s rate under different conditions; and from these data tried to infer the characteristics of the underlying mechanism. The introduction proved extremely successful and soon became the standard technique of the field. In my paper, I will look at the details of
the process: why the new methods were so quickly adopted, how they re-directed the focus of attention, and how, in effect, they moved the disciplinary center of photosynthesis research away from chemistry; and how this change of method even paved the path for a completely new concept of photosynthesis.

Evan Ragland (South Bend)

**How Should We Think Historically about the Rise of Early Modern Experimentation?**

Just about every historian writing on the subject of the “rise” or “development” or “origin” of scientific experimentation takes the early modern period in Europe (ca. 1500-1750) as a major—if not the—crucial period of change. There are certainly earlier exemplars of experimental methods (Galen’s anatomy, medieval optics and alchemy, natural magic), but in this period experimentation became widespread and, for some commentators, characteristically “modern.” In recent decades, historians have offered increasingly varied and even divergent descriptions of important threads in this long history, including contrasting and contradictory explanations. I plan to offer a brief, selective tour of some representative and important works in the secondary literature, and some thoughts on how historians write about early modern experimentation. While embracing pluralism, I hope to offer some suggestions for consolidation by attending to a richer notion of scientific “practice.” Mostly, I’m looking forward to hearing any and all ideas about how to make sense of all this, and where to go from here.

Sarah Reynolds (Indianapolis)

**Learning to Think, Motivated to Do: Method in the Origins of Laboratory Coursework**

In the nineteenth century, the educational use of the experiment changed significantly as it moved from being a means of expert demonstration to directly engaging general students in the processes of science. By the end of the century, American scientists and educators were promoting the “laboratory method of teaching” as particularly developing students’ ability to actively and independently pursue sound knowledge. While training in technical research methods was an essential element of such education, the discourse surrounding the new laboratory classes, which I examine through textbooks and pedagogical articles of the day, ultimately focused on cultivating a scientific method of thought within its practitioners. Consequently, the criteria for the success of such education and its embedded demarcation between scientific and non-scientific involved a deep interweaving of epistemological and methodological concerns. Furthermore, the historical discourse surrounding the challenges of mentally as well as physically engaging students within the laboratory reveals a tension between methodology and routine that persisted throughout the entirety of the pedagogical shift. While scientific methodology was enabling for the student researcher willing to think deeply about what he or she was doing, even while following established procedural standards, educators emphasized that when method became mere routine its knowledge-producing power and efficacy were destroyed or gravely diminished. By examining the educational context, we thus reveal a fragility to methodology that often goes unspoken in the standard research context yet can serve to moderate methodology’s impact as well as drive the pursuit of innovation and improvement.
Raphael Scholl (Cambridge / Munich)

Unwarranted Assumptions: Claude Bernard and the Growth of the vera causa Standard

The physiologist Claude Bernard was an important nineteenth-century methodologist of the life sciences. Here I place his thought in the context of the history of the *vera causa* standard, arguably the dominant epistemology of science in the eighteenth and early nineteenth centuries. Its proponents held that in order for a cause to be legitimately invoked in a scientific explanation, it must be shown by direct evidence to exist and to be competent to produce the effects ascribed to it. Historians of scientific method have argued that in the course of the nineteenth century the *vera causa* standard was superseded by a more powerful consequentialist epistemology, which also admitted *indirect* evidence for the existence and competence of causes. The prime example of this is the luminiferous ether, which was widely accepted, in the absence of direct evidence, because it entailed verified observational consequences and, in particular, successful novel predictions. According to the received view, the *vera causa* standard's demand for direct evidence of existence and competence came to be seen as an impracticable and needless restriction on the scope of legitimate inquiry into the fine structure of nature. The Mill-Whewell debate has been taken to exemplify this shift in scientific epistemology, with Whewell's consequentialism prevailing over Mill's defense of the older standard. However, Bernard's reflections on biological practice challenge the received view. His methodology marked a significant extension of the *vera causa* standard that made it both powerful and practicable. In particular, Bernard emphasized the importance of detection procedures in establishing the existence of unobservable entities. Moreover, his sophisticated notion of controlled experimentation permitted inferences about competence even in complex biological systems. In the life sciences, the *vera causa* standard began to flourish precisely around the time of its alleged abandonment.

Jackie Sullivan (London, ON)

Assessing the Dynamics of Novel Tool Development:
The Rodent Operant Touchscreen Chamber as a Case Study

In areas of neuroscience that investigate cognition, the development of innovative and reliable cognitive testing tools is equally as important as the development of tools for successfully intervening in, visualizing and decoding brain activity. In this talk, I briefly describe and evaluate the evolution of one such state-of-the-art cognitive testing tool: the Bussey-Saksida Rodent Operant Touchscreen Chamber, which was first developed in the 1990s and is now used in 300 laboratories worldwide. I argue that the eventual uptake of this tool and its on-going successful use in behavioural neuroscience is attributable to a dynamic set of epistemic strategies implemented by its designers. I end by briefly addressing the question of whether there is something unique about this particular experimental tool or whether the same strategies are more or less at play or ought to be with respect to other experimental tools in neuroscience in particular and science more generally (e.g., interventionist tools).
Dana Tulodziecki (Lafayette)

*Evaluating Past Methodologies*

Snow’s famous South London water study (1853—1854) is widely hailed as a paradigm of experimental methodology. Re-examining Snow, I show that, in fact, Snow’s “natural experiment” had many methodological flaws. As a result, I will argue, we should be wary of locating the epistemological strengths of successful theories in the methodological arguments that (allegedly) supported them, even if those theories turn out to be right. And while the default in philosophy of science is often to focus on the methodologies of ‘winning’ theories, I argue that this focus is misguided and might lead us to neglect much of what is actually methodologically valuable, especially during periods of scientific controversy.