C.R.AIPS 1

Comptes Rendus de l'Academie Internationale de Philosophie des Sciences

Tome I

Science's Voice of Reflection

Éditeurs
Gerhard Heinzmann
Benedikt Löwe

Cooperation and conflict between philosophers of science and scientists

Alberto Cordero

Philosophy, Graduate Center of the City University of New York, 365 Fifth Avenue, New York, NY, 10016, United States of America

Philosophy, Queens College, City University of New York, 65-30 Kissena Boulevard, Queens, NY, 11367-1597, United States of America

E-mail: `alberto.cordero@qc.cuny.edu, acordelec@outlook.com`

Abstract. Much good science has been done without explicit help from philosophers. However, judging by past and recent interactions, philosophers of science can and do help clarify and advance ongoing scientific projects and facilitate the critical reception of scientific proposals. I consider three significant channels of interaction—two associated with collaborative projects and one with confrontation. They involve, respectively: (1) direct epistemological and ontological influences of philosophers of science qua philosophers in scientific endeavors and vice versa, (2) ethical calls to examine lines of research deemed potentially dangerous to individuals or society, and (3) efforts by senior scientists to protect students from exposure to critiques and "fruitless distractions."

1 Philosophy and science

As the empirical sciences began to break away from philosophy in the 19th century, many working scientists maintained strong intellectual links with the old discipline. Here are some examples.

(a) In the 1840s, Charles Darwin articulated his Natural Selection theory, taking guidance from William Whewell's philosophy of the inductive sciences (1847, 1858).

(b) Albert Einstein's relativity theories incorporated insights from 19th-century work on empiricism and realism (see, e.g., Galison, 2004).

(c) In the 1910s and 20s, John B. Watson sought to improve psychology's objectivity by embedding its discourse in a positivist framework. His rejection of introspection in psychology was furthered a few decades later by B. F. Skinner (Skinner, 1976).

(d) Niels Bohr's ideas about the role of measuring devices and the boundaries of theoretical domains drew from Kant's philosophy and positivism (Bohr 1934).

(e) Heisenberg's quantum mechanics expressed a robust version of empiricism. Later his interpretation of the theory shifted towards Kantian insights (Heisenberg 1939, 1952, 1961).

Science's Voice of Reflection, edited by G. Heinzmann & B. Löwe.
Comptes Rendus de l'Académie de Philosophie des Sciences I (2022), pp. 25–39.

(f) In more recent times, Bell's investigations into the foundations of quantum mechanics explicitly revived interest in metaphysical epistemological themes in physics (see, e.g., Bell et al. 2001).

(g) In contemporary philosophy, many naturalist approaches see their goal as making science self-aware of the strengths and limitations of its findings, theories, and methods (see, e.g., Dudley Shapere 1984, Daniel Dennett 1995). More radical naturalists emphasize the growing continuity of science and philosophy of science, arguing that philosophy is not different in critical eagerness and style of argumentation from science or common knowledge (Alexander 2012, Sytsma & Livengood 2015). Biopsychology and bio-anthropology projects draw from analytic metaphysics, epistemology, and ethics (see, e.g., Dennett 1995, Baron-Cohen 2003).

Intellectual interactions such as these operate in varied and complex ways. However, one common trend is that philosophers of science generally seek to contribute results that can help scientists articulate new hypotheses—improving their internal coherence, plausibility, and compatibility with received scientific and philosophical information. Accordingly, they raise questions about the scope and limits of ongoing scientific approaches, scientific standards of evidence, motivation, and underpinning values (epistemic and non-epistemic). The resulting analyses by philosophers often gain recognition from scientists, connecting with their technical work. In recent times this is apparent in many fields, notably post-Bell physics (as reported in, e.g., Bell et al. 2001, Cordero 2019), evolutionary biology, and experimental psychology (see, e.g., Dennett 1995, Sterelny 1999, Sober & Wilson 1998, Baron-Cohen 2003), to mention just some cases.

On the other hand, some scientists consider all the noted philosophical efforts irrelevant to their practice. Limited receptiveness and even hostility to suggestions from philosophy are widespread, especially among leading physicists. Recall, for instance, the quick way Richard Feynman and his circle dealt with the interpretive problems posed by infinite integrals in perturbation theory (renormalization) in the 1940s. This neglect is also apparent in the idea that the electron can go temporally backward, among many other proposals. (Mathematically speaking, an antiparticle traveling forwards in time is indistinguishable from the corresponding particle traveling backward). It took time for philosophers of science to develop analytic projects in tune with these and other radical metaphysical proposals from quantum theory. They did it, however. In the 1970s, philosophers of physics began to offer increasingly coherent explications of the locality principle in modern physics theories, quantum non-separability, the many-worlds interpretation, multiple-times, the block universe, space-time point reality, to name a few developments. Since then, the intellectual and methodological contributions of the philosophy of science are on view in a plethora of transformative works.[1]

Nonetheless, many scientists in foundational fields don't care much about professional philosophers' insights, preferring their philosophical intuitions. Some believe that philosophy is dead—an idea Stephen Hawking endorsed in some of his final writings and public appearances (e.g., Hawking 2010). Such neglect, however, often results in ideas that, it seems, would benefit from more significant interaction with contemporary philosophers of science. Consider, for instance, the central thesis proposed in the generally delightful book *The Mathematical Universe* by Max Tegmark (2014). In it, Tegmark argues that the Universe is a "Multiverse." A Multiverse is a multi-level entity utterly big and strange, with levels described first by the standard mathematical physics, then by physics under variations of the "constants of nature", and thirdly by many-worlds quantum mechanics. Provocatively, the book claims that *all mathematical structures exist*. Exemplifying one of the contemporary roles of philosophers of science, Jeremy Butterfield has taken Tegmark's Platonist intimations to task. In a paper titled "Our Mathematical Universe?" Butterfield argues that even if one agrees that there is a mathematical multiverse, we still need to distinguish between applied mathematics (theoretical physics) and pure mathematics—the Multiverse is an *applied mathematical structure*. The claim 'There is a mathematical multiverse' holds for pure mathematics, Butterfield notes—i.e., all possible mathematical structures are equally real. However, he adds, this Platonist stance about pure mathematics has nothing to do with a physical multiverse. From the premises that (1) 'nature is an applied mathematical structure' and (2) 'there are a plethora of pure mathematical structures,' one cannot infer that 'nature is one of many equally real structures.' Tegmark, that is, commits the fallacy of equivocation. In propositions (1) and (2), 'mathematical structure' is equivocal between applied and pure structures. One can be a Platonist about pure mathematics (and believe in ever so many pure mathematical structures) and accept all this without believing that the physical Multiverse is a purely mathematical structure.

Similar interactions between philosophy of science and science are readily on view across the sciences. The point to highlight here is that philosophy is far from "dead." Contemporary philosophers of science make logical, epistemological, and ontological contributions. Furthermore, the latter seemingly help scientific investigations—and vice versa. The next section considers a complementary channel of interactions, focused on a different angle: the ethical scrutiny of scientific projects.

[1] Instances in point include, e.g., Albert (2003, 2013), Wallace (2012), Maudlin (2012, 2019), Lewis (2016), to mention a few contributions from just philosophers of fundamental physics.

2 On free inquiry

The ethical side of research comprises far more than justifying the allocation of resources out of finite public means available. Social and ethical issues arise when a line of inquiry touches topics of expected impact on individuals or society.

In this section, I consider the ethical scrutiny of scientific projects by philosophers of science. To make the topic manageable within the space available, I will focus on critical evaluations that oppose the conduction of specific lines of research on ethical grounds. The issue at stake is the idea of freedom of scientific investigation. I will examine several responses and suggest how arguments in progress attest to the lively engagement of philosophers of science in current debates. My focus will be on proposals that seek to articulate and clarify ethical critiques levied against specific scientific projects, also help dialogue between scientists and their critics (and the society at large).

Consider the following case of current interest. Recent evolutionary psychology theories propose that differences in cognitive performance between males and females shown by current surveys do not seem to come exclusively from cultural factors but also partly from *biological* differences (nativist explanations). To some critics, entertaining this kind of hypothesis is ethically problematic, given the possible uses and abuses that even preliminary results might have. Nativist theories about the existence of cognitive sexual differences could exacerbate ongoing injustice on specific groups—e.g., by supporting repugnant social policies and pre-existing prejudices, as has occurred repeatedly in the past. This possibility is no small fear. Human groups (particularly women and some ethnic groups) have been grossly discriminated against numerous times based on "biological" arguments that subsequently proved either seriously invalid or unsound.

So, are cognitive differences between human groups a taboo topic in enlightened society? How are research choices on the matter to be made, and by whom? Philosophers of science play a role here. One distinguished and controversial participant is Philip Kitcher, who invites us to decide in terms of the collective good that inquiry should promote in a democratic society (Kitcher 2001). His social-minded approach is especially critical of recent projects in evolutionary psychology to study alleged cognitive differences between average male and female performances. Alleged Darwinist hypotheses on such differences prompt bitter clashes (intellectual and legal) in liberal societies. The standard accounts of average academic performance variations focus strongly on local environmental factors, particularly *cultural* ones (nurturist explanations). In the social sciences, the common view is that we have become "creatures of culture" to such an extent that our evolutionary origins can tell little, if anything, about present cognitive differences between human groups. The issues at stake are numerous and deeply felt; approaching them thus calls for caution. Perhaps the most promising way to do so is to tackle calls for research censorship in this area is on a case-by-case basis.

The reactions to nativist projects in psychology open fronts of interaction between philosophers and scientists. These can be cooperative or negative. The epistemological and methodological difficulties faced by hypotheses about psycho-biological predisposition are numerous. For example, distinguishing between inheritance and learning from experience can be exceedingly hard—inherited traits often have "maturation" periods of many years. Nevertheless, it seems that progress in handling these difficulties has been made in the last half-century (see, e.g., Baron-Cohen 2003, also, Sterelny and Griffiths 1999, Part V). More difficult to approach are the ethical difficulties associated with nativist research. Many current projects raise concerns about ethical damage that even the very act of making inquiries explicit might cause (some thinkers claiming that even discussing certain nativist hypotheses leads to effective discrimination).

Consider, e.g., the question of why, despite so much egalitarian investment in education since the 1960s, still most top young mathematicians and theoretical physicists continue to be males. The empirical correlations between gender and certain analytic skills may all be the result of cultural inertial forces from the past. Or the cause may be something else. One working hypothesis proposes that, because of natural selection in Paleolithic environments, males are on average genetically both better disposed and more inclined to analytic thinking than females, particularly at the highest end of the achievement distribution. If this is correct, the found differences are part of our *Darwinian* nature. Working along these lines, evolutionary psychologist Simon Baron-Cohen (2003) argues that, overall, the female brain is more hard-wired for empathy intelligence, while the male brain is more hard-wired for analytic understanding and system building. I.e., Darwinian evolution developed men's and women's brains differently. To nurturist critics, Baron-Cohen stresses the role that evolution and genes could play in determining men's and women's brain types while playing down social and cultural influences. Nonetheless, his theory articulates various consilient Whewellian-Darwinian inductions from animal studies, evolutionary biology, endocrinology, brain studies, and genetics. Baron-Cohen and his collaborators at Cambridge further propose that people with autism and Asperger's syndrome have an extreme version of the male brain, along with startling novel predictions regarding prospective findings of genes that control empathizing and systemizing.

Baron-Cohen's nativist project has crucial gaps to fill; it is a work in progress. It remains unclear, e.g., whether decoding the human genome will pinpoint genes that control empathizing and systemizing, as Baron-Cohen claims. Baron-Cohen's group is aware of the hurdles and moves carefully regarding empirical correlations and their interpretations on the methodological and epistemological fronts.

Critics object to this and similar projects, especially on technical and—more inflexibly—ethical grounds. A major focus of technical objections to nativist projects centers on purported causal interpretations of experimental correlations. These seem potentially damaging enough to call for permanent vigilance. Still, Darwinian psychologists and anthropologists claim to have some ways of assessing the objectivity of psycho-biological claims in crucial areas.[2] Ethical considerations can be more difficult. From the 1970s on, the whole genre to which Baron-Cohen's project belongs has been the subject of scathing objections from major scientists and philosophers, conspicuously Richard Lewontin (1975), the late S. J. Gould (1980/1989, 1981), and Philip Kitcher (1997, 2001). Their critiques are fair regarding many specific proposals. Time and again, in the last century, the general public was rushed into believing that biological investigations had revealed all sorts of "unpleasant truths" about the existence of natural differences between some human groups. The allegations were subsequently found to have been wrong—though not before doing significant damage. Thus, there are reasons to be wary of certain nativist inquiries.

The question is how far those arguments apply to nativist inquiry in general. Calls for casting moral opprobrium on nativist research inquiries have received a boost from a general consequentialist argument articulated by Kitcher. In his view, there can be no right to free inquiry in problematic fields because the prevailing social context provides enough grounds for ethically condemning the highlighted nativist inquiries very broadly (2001, Chapter 8). By the argument's terms, Baron-Cohen's project would seem to come out as unacceptable, despite its methodological and epistemological caution.

Kitcher's consequentialist argument proceeds from the following four premises regarding a human group G. Suppose that:

The low standard of living of people in group G originates, to a significant extent, from a view C erroneously held in the past as dogma. (K1)

Even though C is now officially rejected, it lingers dangerously in society, because of a strong tendency to inflate evidential support in favor of C (epistemological asymmetry). (K2)

[2] See, e.g., Baron-Cohen 2003, chapters 4, 6, 8, and 10.

The society in question is politically biased toward C (e.g., news of results contrary to C would not lead to any social action in favor of G). In contrast, the slightest rumor favorable to C would raise C's popular and official credibility, with damaging consequences for G (political asymmetry). (K3)

Conclusion 1. In situations where free inquiry would increase the burden on G, there can be no right to free inquiry.

Research into the truth of nativist hypotheses regarding any possible superiority in cognitive faculties between men and women is virtually guaranteed to increase the current burden on women. (K4)

Conclusion 2. There can be no right to free inquiry into the truth of such hypotheses.

Corollary. The inquiries in question deserve moral opprobrium because far less controversial than any duty to seek the truth is the duty to care for those whose lives already go less well and to protect them against foreseeable occurrences that would further damage them (K1).

The argument just presented calls for ethical constraints on scientific research. Its assumptions are controversial and invite cooperative scrutiny from philosophers of science. Several intertwining lines of considerations call for clarification. First, are the premises compelling? Do the intended conclusions follow? Do our current social realities provide reasonable grounds for deeming the said evolutionary inquiries ethically condemnable?

Secondly, some considerations overlooked by practicing scientists and philosophers need to be made salient (Cordero 2005):

(a) Are the terrible consequences envisaged in Kitcher's consequentialist argument a likely outcome in contemporary liberal democracies? It is not in question that political agendas can co-opt scientific debates and inquiries. Prime exemplifications abound in the form of persistent discrimination against women, 'mob racism', and the phenomenon of 'Scientific Creationism', to mention a few varieties. However, as Kitcher appreciates, cases like these also attest to civil society's actual power to efficiently limit the impact of mob epistemology through legal containment. The situation is different in authoritarian societies, but there the dangers of rational forms of inquiry to human groups are negligible compared with those posed by the state.

(b) All research into the human condition is difficult and dangerous. However, it is far from clear that trying to learn about human nature from a Darwinist perspective is *more difficult or dangerous* than trying to

> learn about human individuals or human groups from a sociological or any other perspective. Furthermore, it seems dubious that the disadvantaged among us would benefit from discouraging any kind of *serious* research, especially in societies marred by political and epistemic asymmetries.

Kitcher's consequentialist argument depends heavily on specific context. In Darwinian conjectures about natural differences in social and psychological dispositions between men and women, two observations come to mind. First, the structure and motivation of the noted nativist hypothesis are rooted in current evolutionary biology. As such, not just any conjecture will do as a working hypothesis. There is no room for genetic determinism since biology accepts that phenotypes are shaped jointly by genes and the environment.

Furthermore, evolutionary claims about complex phenotypes are primarily about *tendencies*, and so they are compatible with virtually any given single case outcome imaginable within the relevant total range of performance. For instance, the Darwinian suggestion that members of some group G might be, on average, less naturally gifted than non-members for original thinking in mathematics or theoretical physics is fully compatible with the most accomplished individual in those fields being a member of group G. Even strong believers in a Darwinian suggestion about the male brain do not consider the outstanding mathematical talent of Amalie Emmy Noether as a counterexample to their belief. Relevantly, in Darwinist conjectures, the reference to natural tendencies is characteristically indirect in at least two ways: (1) The germane probabilities are second-order, in that they correspond to averages over probabilistic trends at the individual level; and (2) at the individual level, tendencies operate against the backdrop provided by the environment and past experience on the one hand, and the effects of most individual organisms' *ability to learn* new behaviors—to acclimatize to a new stressor (see, e.g., Dennett 1995, Chapter 3).

Even if the scientific news turned out to be very bad for some given human group, there is a solid reason to expect the findings in question to come with an array of biological and genetic pointers of theoretical and practical significance. Suppose, e.g., that it became unreasonable to scientifically deny that members of some group G are, on average, less naturally gifted than non-members for some celebrated aspect of human excellence. Some might hastily conclude that members of G should henceforth be regarded as hopeless in the specified respect, regardless of training and education. Yet, we already know this conclusion to be false. It is a *fact* that proper training can bring practically all human beings to master basic college-level mathematics and such. Nor would it be correct to conclude that individuals cannot reach high in any significant area where they rank low as a *group*, for we also know this to be false. And something else is incorrect as well, namely the intimation that our distinctly human traits are simple, one-dimensional features—they are not. These clarifications are, however, only part of the story. In contemporary natural science, beliefs are not isolated but develop in entangled clusters. As with research into oncogenes, no matter how distressing a research result might prove to be for some people, there is reason to expect that it will also point to the design of correctives— chemical, genetic, educational—to be made available to interested individuals. The debate over the above points remains alive. My discussion here aims to suggest how philosophers of science are trying to clarify and better articulate theories like Baron-Cohen's. The suggestions above focus on the texture of theoretical belief in the contemporary natural sciences and the role of inquiry in fallibilist contexts.

The considerations outlined also seem helpful to society at large. We live immersed in scientific ideas and products like never in history, yet the average scientific literacy keeps falling in most contemporary societies. As a result, public understanding of the scope and weakness of mainstream ideas tends to be shaped more by ideology and propaganda than critical reflection. Philosophers of science can help citizens better understand the promises, limitations (both epistemic and ethical), and prospective ethical impact of scientific proposals.

In this section, I have presented an ongoing debate on the epistemology and ethics of nativist hypotheses as exemplifying an opportunity for fruitful interaction between philosophy of science, science, and contemporary liberal society. If the suggested considerations are on target, there is ample room for mutually beneficial interaction between philosophers of science and scientists. However, some scientists reject advice from philosophers on methodological grounds. The following section considers some reactions of this sort.

3 Help not always welcome

As entwined as the philosophy of science and science are, their expectations diverge at multiple levels—enough, according to some, to limit fruitful interaction between them. On one school of thought, exposure to history and philosophy of science (HPS) can be even *unhelpful* to the practice of science. In the heyday of anti-positivist critique, Thomas Kuhn (1959, 1962) and Paul Feyerabend (1974) suggested that HPS can be detrimental to working scientists because of the revisionary claims historians and philosophers often make about science. These thinkers compared theorists working at the cutting edge with athletes competing in Olympic Games, not to be bothered with subtle critical elucidations of their practice while running, especially about how their outcomes fall short of avowed ideals of thought and behavior. The most genial minds of science, they noted, routinely trespass the received categories of understanding. Moreover, scientists do this often as if in a state of rapture, proposing deviant, sometimes initially incoherent, approaches through which they proceed fruitfully, on the whole, oblivious of challenges posed by historical, epistemological, or metaphysical doubters—let alone philosophers.

Two suggestions in this negativistic view of the interface between HPS and science are worth highlighting.

(a) Scientists, it is noted, draw strength from a progressive picture of science and the scientist as rational, open-minded participants. On this ideal picture, scientists always proceed methodically, grounded indisputably in the outcome of controlled experiments, seeking objectively for the truth, ready to let the chips fall where they may. Contemporary historians and philosophers of science challenge this pragmatically fruitful professional ideal and public image. So, the argument goes, to the extent that HPS propounds ideas at odds with the progressive view, supervisors should shield scientists at the start of their careers from HPS.

(b) It is further claimed that writings on HPS are usually not acceptable for publication in leading scientific journals. So, science majors and graduates will likely waste their time doing work on those issues (Kuhn 1959: 344).

The relevant point here is the claim that learning about what philosophers and historians say regarding scientists' existing standards and behavior can be "demoralizing" for aspiring students. The image under attack presents scientists as exemplary rational, open-minded investigators. Evidence, however, suggests that scientists operate in considerably more subjective ways. Experimental verification is often of secondary importance compared to non-standard scientific arguments (e.g., from metaphysics and religion), at least during some of the significant conceptual changes in science. For example, while Ptolemaic astronomers faced numerous refuting instances, for at least a century, the Copernican theorists faced arguably even more extreme refuting cases, compounded by severe conceptual conundrums. According to Kuhn (1962) and other critics, heliocentrism, favored on quasi-mystical grounds, gained strength in influential circles between the 1540s and 1640s. Its challenges were rendered ineffective by ad hoc hypotheses and clever techniques of persuasion.

Numerous other examples of debunking cast similarly "negative" light on scientific discoveries. Cases in point include Copernicus, Galileo, Lavoisier, Dalton, Mendel, and Robert A. Millikan, to mention a few. In Feyerabend's view (*Against Method*, 1974), the slogan "anything goes" summarizes the history of science. Science, he claimed, is wonderful but does not deserve any special status because it is "just" another human project among many, closer to myth than scientistic philosophy is prepared to admit. Science, Feyerabend urged, is one of the many forms of thought that our species have developed, and not necessarily the best. Like the above from the 1960s, abrasive charges continue strong to this day outside mainstream philosophy of science (notably in some "postmodernist" projects).

Most analytic philosophers rejected early on this pessimistic view of scientific education. As Israel Scheffler admonished at the start of the anti-objectivist turn, the relativist narratives purport to establish some acid anti-objectivist claims, in particular these. (a) Scientific theory "is not controlled by data, but that data is manufactured by theory. (b) We cannot evaluate rival hypotheses rationally, there being no neutral court of observational appeal nor any shared stock of meanings. (c) Scientific change is a product not of evidential appraisal and logical judgment but intuition, persuasion, and conversion. (d) Reality does not constrain the thought of the scientist but is rather itself a projection of that thought. To this Scheffler (1967) responded: "Unless the concept of responsible scientific endeavor is to be given up as a huge illusion, the challenge of this alternative must, clearly, be met."[3] Since then, further doubts have been raised against anti-objectivist, Neo-romantic approaches over the last decades. Detailed critiques by Shapere (1964, 1980, 1984), Stephen Toulmin (1972), and numerous others have challenged the historical cases invoked by Kuhn and Feyerabend—e.g., regarding the rise of Copernican astronomy and the ousting of Newtonian theory by Einstein. The traditional idea of cognitive progress was over optimistic, but it is not as naïve as Kuhn and his followers claim.

According to objectivist critics, historical and philosophical studies might challenge the brightest science students, but that can be a *good* thing, not at all counter motivational. As Stephen G. Brush argued in seminal writings on the history of science after Kuhn (e.g., Brush 1974), historians must do more than document the application of objectivity to scientific problems. They must be prepared to analyze the philosophical, psychological, and sociological aspects of scientific work, explain how specific issues came to be considered "scientific," and how particular standards happened to evaluate solutions to those problems. The historian may also have to account for scientific change in terms other than linear progress from error toward truth. Most importantly—as far as education is concerned—learning about the historical and philosophical adventure surrounding current science can be enlightening to science students. It ostensibly was to Charles Darwin, Henri

[3] *Science and Subjectivity.* Indianapolis: Bobbs Merrill (1967): v-vi.

Poincaré, Albert Einstein, Louis de Broglie, Niels Bohr, Erwin Schrodinger, Werner Heisenberg, and Paul Ehrenfest, to mention a few notable cases.

Still, the pragmatic objections to exposing science students to HPS may seem to stand. Too many scientists seemingly derive strength from sanguine ideas about truth and progress that—history and cold reasoning suggest—are better left unexamined critically to do good science. HPS, which seeks to enhance self-awareness, freedom, and responsibility, may not be good for everyone. The debate on this matter continues. On their part, contemporary objectivists offer an increasingly rich and nuanced view of the relationships between science and society. In recent decades, a representative of the objectivist shift is a family of projects that now go by the label "Selective Realism."[4] On this family of approaches, the most successful scientific representations of the world are not completely correct. Still, they are not totally wrong either: successful scientific theories generally contain parts that make them "approximately correct" rather than "True, Pure, and Simple."

Selective realists respond to the problems posed by post-Kuhnian antirealists. They do so particularly regarding the empirical underdetermination of theories, the availability of skeptical readings of the history of science, and postmodernist skepticism and relativism. The principal selectivist move is to drop the more extreme claims of earlier realists. According to selectivists, empirically successful theories generally turn out to be only partly correct: taxonomies of natural types have mushy boundaries; standards for assessing scientific results change along with science, fundamental ontologies can be seriously off the mark. The natural philosophies of Galileo, Newton, Darwin, Einstein, Bohr, and numerous others strongly suggest that there are no meta-scientific criteria for accepting and rejecting scientific proposals. Humans, not nature, confer scientific significance (be it of observations, test results or an entire research line). So, over time, scientific facts—what Kitcher calls 'subversive truths'—undermine deep old beliefs and value systems and allow us to operate more effectively in the world (Kitcher 1993, 2001). Having purged Scientific Realism of excessive optimism, the next selectivist task is to show how to articulate a robust and substantive realist stance about scientific theories well-grounded in exacting scientific evidence. Kitcher is optimistic about the challenge: '[T]here is no basis for believing that value judgments inevitably enter into our appraisal of which of a set of rival hypotheses (if any) is approximately correct' (Science, Truth, and Democracy, p. 41). Notably, the noted departures from traditional scientific Realism seem to allow for a strong stance (Cordero 2017). Selective

[4]Started in recent times by John Worrall (1989) with a structuralist focus and by Philip Kitcher (1993) with a focus on content. The general strategy was then variously developed by (Stathis Psillos (1999), Juha Saatsi (2005, 2011), Ioannis Votsis (2011), Peter Vickers (2013), Mario Alai, and Alberto Cordero (2016), among others).

Realism is still a work in progress. Still, the point here is that the negativistic tensions between science and philosophy have been (and continue to be) addressed by philosophers of science, with some promising outcomes on the horizon. For the moment, at least, the "death of publicly relevant philosophy" does not seem in sight.

4 Concluding remarks

I have explored some of the roles that contemporary philosophers of science can and do play in science and the public discussion of science. Section 1 considered representative cases where practicing scientists explicitly resort to philosophy in their work. § 2 considered the ethical scrutiny of an ongoing scientific project and its impact on freedom of research. I focused on a particular case from evolutionary psychology, trying to display roles played fruitfully by philosophers of science in the current debates. Section 3 considered ideas propounded in the 1960s and 1970s, still embraced in some quarters, to the effect that history and philosophy of science can be *unhelpful* to the practice of science.

My overall conclusion of the above considerations is that there are numerous channels of interaction between philosophers of science and scientists. Although I have considered only three, the tracks highlighted seemingly illustrate how the philosophers of science can play a fruitful part in the scientific endeavor today.

References

Albert, David Z. (1993): Quantum Mechanics and Experience. Cambridge, MA: Harvard University Press.

Albert, David Z. (2003). Time and Chance. Cambridge, MA: Harvard University Press.

Alexander, J. (2012): Experimental Philosophy: An Introduction. Malden, MA: Polity Press, 2012

Baron-Cohen, Simon (2003): The Essential Difference. Men, Women and the Extreme Male Brain. London: Allen Lane.

Bell, John S. et al. (2001): John S. Bell on the Foundations of Quantum Mechanics. Singapore: World Scientific Pub. Co. Inc.

Bitbol, Michele & Stefano Osnagh (2013): "Bohr's Complementarity and Kant's Epistemology." Bohr, 1913-2013, Séminaire Poincaré XVII: 145 – 166.

Bohr, Niels (1934/2011): "The Atomic Theory and the Fundamental Principles underlying the Description of Nature," in Atomic Theory and the Description of Nature: 102-138. Cambridge: Cambridge University Press.

Brush, Stephen G. (1974): "Should the History of Science Be Rated X?" Science (Vol. 183): 1164-1172

Brush, Stephen G. (1976). The Kind of Motion We Call Heat – A History of the Kinetic Theory of Gases in the 19th Century. North Holland, 2 volumes

Chiesa, Mecca (1994). Radical Behaviorism: The Philosophy and the Science. Cambridge, MA: Center for Behavioral Studies.

Cordero, Alberto (2005): "Contemporary Nativism, Scientific Texture, and the Limits of Free Inquiry." Philosophy of Science (72): 1220-1231.

Cordero, Alberto (2011): "Scientific Realism and the Divide and Conquer Strategy." Philosophy of Science, (78): 1120–1130.

Cordero, Alberto (2019): Philosophers Look at Quantum Mechanics. Springer Nature Switzerland: Synthese Library.

Dennett, Daniel C. (1995): Darwin's Dangerous Idea. New York: Simon & Schuster. Galison, P. (2004): Einstein's Clocks, Poincare's Maps: Empires of Time. New York: W.W. Norton & Co.

Gould, Stephen J. (1980/1989): "Sociobiology and the Theory of Natural Selection". Reprint in Michel Ruse (ed.), Philosophy of Biology. London: Macmillan, 253–263.

Gould, Stephen J. (1981), The Mismeasure of Man. New York: Norton.

Hawking, Stephen (2010): The Grand Design. New York: Bantam.

Heelan, Patrick (1965): "Reality in Heisenberg's Philosophy" - Chapter Eight of Quantum Mechanics and Objectivity.

Heisenberg, Werner (1930/1943): The Physical Principles of the Quantum Theory (Translators Eckart, Carl; Hoyt, F.C.). Dover.

Heisenberg, Werner (1952/1966). Philosophic Problems of Nuclear Science. Translated by F. C. Hayes. London: Faber & Faber.

Heisenberg, Werner (1961): "Problems of Modern Physics" by W. Heisenberg in On Modern Physics, C.N. Potter London: Orion Press.

Kuhn, Thomas S. (1959/1963). "The essential tension: Tradition and innovation in scientific research," reprinted in Scientific Creativity, Its Recognition and Development, C. W. Taylor and F. Barron. Eds. New York: Wiley.

Kuhn, Thomas S. (1962/1970): The Structure of Scientific Revolutions, Chicago: University of Chicago Press (1970, 2nd edition, with postscript).

Kitcher, Philip (1993): The Advancement of Science. New York: Oxford University Press.

Kitcher, Philip (1997): The Lives to Come. New York: Touchstone.

Kitcher, Philip (2001), Science, Truth, and Democracy. New York: Oxford University Press.

Lewis, Peter J. (2016): Quantum Ontology. Oxford: Oxford University Press.

Lewontin, Richard C. (1975), "Genetic Aspects of Intelligence", Annual Review of Genetics (9): 387–405.

Maudlin, Tim (2012): Philosophy of Physics: Space and Time. Princeton, NJ: Princeton University Press.

Maudlin, Tim (2019): Philosophy of Physics: Quantum Theory. Princeton, NJ: Princeton University Press.

Psillos, Stathis (1999): Scientific Realism. London: Routledge.

Shapere, Dudley (1964): "The Structure of Scientific Revolutions," Philosophical Review (73): 383–94.

Shapere, Dudley (1980): "The Character of Scientific Change," in T. Nickles (ed.), Scientific Discovery, Logic, and Rationality. Dordrecht: D. Reidel (Boston Studies in the Philosophy of Science 56): 61-102.

Saatsi, Juha (2005). "Reconsidering the Fresnel-Maxwell Case Study." Studies in History and Philosophy of Science 36 (3): 509–38.

Saatsi, Juha and Peter Vickers (2011): "Miraculous Success? Inconsistency and Untruth in Kirchhoff's Diffraction Theory." British Journal for the Philosophy of Science 62: 29–46.

Shapere, Dudley (1984): "Objectivity, rationality, and Scientific Change," Proceedings of the Biennial Meeting of the Philosophy of Science Association 1984: 637-663.

Skinner, B.F. (1976). About Behaviorism. New York: Random House.

Sober E., Wilson D.S. (1998): Unto Others: The Evolution and Psychology of Unselfish Behavior. Cambridge, MA: Harvard Univ Press.

Sterelny, Kim, and Paul E. Griffiths (1999), Sex and Death. Chicago: University of Chicago Press.

Sytsma, J. and J. Livengood (2015): "The Theory and Practice of Experimental Philosophy." Peterborough: Broadview Press.

Tegmark, Max (2014): Our Mathematical Universe. New York: Random House.

Vickers, Peter (2013): "A Confrontation of Convergent Realism." Philosophy of Science 80: 189-211.

Votsis, Ioannis (2011): "Saving the Intuitions: Polylithic Reference." Synthese 180 (2): 121–37.

Wallace, David (2012): The Emergent Multiverse: Quantum Theory According to the Everett Interpretation. Oxford: Oxford University Press.

Whewell, William (1847): The Philosophy of the Inductive Sciences, Volume II, Section III—Tests of Hypothesis. London: John W. Parker.

Whewell, William (1858/1968): Novum Organon Renovatum, being the Second part of the Philosophy of the Inductive Sciences, 3rd Edition, London. In Robert E. Butts (ed.) William Whewell's Theory of Scientific Method. Pittsburgh: University of Pittsburgh Press: 103-249.

Worrall, J. (1989): "Structural Realism: The Best of Both Worlds?" Dialectica (43): 99-124.