Philosophy of the Social Sciences OnlineFirst, published on December 30, 2009 as doi:10.1177/0048393109352877

Review Essay

Philosophy of the Social Sciences
XX(X) 1–17

DOI: 10.1177/0048393109352877
http://pos.sagepub.com
SAGE

Current Philosophy of Science

Joseph Agassi[1]

Abstract

This *Companion* to the philosophy of science reflects fairly well the gloomy state of affairs in this subfield at its best—concerns, problems, prejudices, and all. The field is still stuck with the problem of justification of science, refusing to admit that there is neither need nor possibility to justify science and forbid dissent from it.

Keywords

Philosophy of science, induction, dogmatism, agreement, sociology of science, Vienna Circle

Stathis Psillos and Martin Curd, eds.
The Routledge Companion to Philosophy of Science. London: Routledge, 2008. xxvii + 604 pp.

The Value of Accord

Why do we strive at agreement? Descartes, Leibniz, Laplace, and many other occupants of our hall of fame said that agreement brings peace. Relativists go for less, for peaceful coexistence. They nonetheless disturb the peace, as their view precludes peaceful negotiations. Negotiation, not agreement, is what brings democratic cooperation, said John Watkins (1957-1958). As the fourth and final part of this volume handles the specific sciences, I hoped to find there some discussion of the value of agreement and of negotiations. That part

Received 28 September 2008
[1]Tel Aviv University, Israel, and York University, Toronto, Canada

Corresponding Author:
Joseph Agassi, 37, Levi Eshkol Street, Herzliya 46745 ISRAEL
Email: agass@post.tau.ac.il

includes eight chapters, four "hard" and four "human": cognitive science, economics, psychology, and social science.

Cognitive science is in part "hard"—computation theory, neurobiology, and such; it also has some psychology and anthropology. The author Paul Thagard centers on the controversy between reductionists and their opponents. He proves his opponents' view implausible and he dismisses it (p. 535). He exaggerates, of course, but he has a point: for now, opponents who wish to join the action may have to join. They do not have to agree: they can use common sense (Agassi, 1977). Thagard declares the "logical" positivists his allies (pp. 539-41). They, however, viewed the opponents thesis as confused, not implausible.

Economics is "a controversial discipline" (p. 543), says Uskali Mäki. He offers a historical list of dissenters, overlooking Marx and Keynes. (The latter appears two pages later.) He discusses the controversy between Popper and Lakatos (p. 544). The latter offered a "modified version" of the former that survived only 15 years (p. 545). Both were not clear, says Mäki, about what theory was being tested "and what kind of performance it is tested for" (p. 546). He does not explain. He refers to the tradition of Mill as one that deemed the basic assumption of economics "more-or-less true" (p. 541). The "Popperian-Laktosian episode . . . ignored" that tradition, but it "has been upheld—but not in one choir—by Daniel Hausman (1992), Nancy Cartwright (1989), and myself (1992), of whom the last two have also been influenced by the Poznań school on idealization and the Aristotelian tradition" (p. 546). Again, no explanation.[a] Mäki then snubs those who complain that the models of economics are abstract, declaring that "the Greek economy is a Walrasian system" true-or-false (p. 547). Of course: every claim that an idealization has an instance is true or false. Now since idealizations have no instances, how do they advance science that seeks true explanations? Mäki does not say. Switching to applied science, he explains how false hypotheses can be useful. This holds for Galileo's hypothesis too, as Milton Friedman stressed. Mäki criticizes Friedman briefly (*loc. cit.*), throwing no light on the value of accord or discord.

Psychology may be a place for discussing the ambivalence about debates. Richard Samuels chose to discuss briefly other items. Are we computers? To what extent are we modular? (That is, do we have some self-contained units of thought captured by inborn fixed neural structure?) And, what mental structure is innate? What (rightly) troubles Samuels is that reductionism precludes originality.

[a]Musgrave notices (1995, 151) that Cartwright deems laws vacuously true, since they are idealizations.

Discussing the social sciences, Harold Kinkaid comments on "the role of idealized models, the place of individual behavior in social explanation, the status of teleological and evolutionary explanations, and the role of values" (p. 594). The role of models engages most authors of this *Companion*. Following Friedman, he presents idealizations as belonging to the instrumentalist philosophy of science. As the paradigm case of idealization is that of Galileo, the formidable enemy of instrumentalism, this invites explanation. Thus far, no one has offered any, except for Larry Boland, whom only Mäki mentions and merely as a follower of Popper. Kinkaid contrasts individualism not with collectivism, as tradition has it, but with mechanism, whose great advocates, beginning with Julien Offray de La Mettrie, were traditionally sworn individualists. If any whiff of collectivism enters his essay, it is under the rubric of evolutionism: "Marx thought that the state exists in order to defend the interests of the ruling class. Durkheim claimed that the division of labor exists in order to promote social solidarity" (p. 599). Under the same rubric enter the functionalists and the school that favors replacing explanation in social studies with understanding. He dismisses them all as they make "illicit biological analogies" (p. 600). Kinkaid does not say what would legitimize their views. Finally, he claims that science is value-free and dismisses the view of Gunnar Myrdal that science is value laden, and Quine's claim that there is no sharp line dividing fact from value: they too have not legitimized their views (p. 602).

This is strange. The *Companion* is divided interestingly into four parts: the fourth presents specific sciences; the third discusses "concepts," that is, specific technical issues, including causation, evidence, idealization, measurement, and probability; the first offers a general and historical background; and the second is titled "Debates." Proof kills debates, observed the great Bacon. How then is disagreement possible? What role do debates play in learning? No answer here, nor in the editors' seven-page introduction to this volume that is largely historical. They say of part 3 that the debates it discusses are important. Why? As debates come to eliminate error, why is here so little about scientific error? One discussion here concerns scientific failure; it ascribes to Larry Laudan the idea that as we deem past science failures, so will future commentators deem our science. He thus deems all error failure (p. 232). To show him in error, consider a set of suspects and an equal set of investigators who shadow them; at most only one investigator will find the suspicion correct, yet the success is of the group as a whole—if they are lucky. Fallibilism finds error or at least possible one of the most valuable products of the human mind. This *Companion* lamely reconciles fallibilism with the theory of rational belief (pp. 92, 98, 479). Discussion of falsificationism should refer to scientific errors. Alas, here it does not. Its author, Gürol Irzik, reports what seems to him its fatal philosophical error, namely, its inability to claim certitude for its thesis that science approximates the truth (p. 62). Why he finds certitude necessary, I cannot say. As fallibilism rules out all claims for certitude, its adherents wait to be told what merit certitude has today, after it has ceased to guarantee the truth.

The Expression of Accord Here

The present book seems defensive. This is a pity, as its task is not to defend but to help potential users. Perhaps I am expressing here my bias. Perhaps an outsider like me should not review handbooks, as these are bound to represent establishments that outsiders decline to join. If they do, they may feel obliged to rehash familiar criticism of received views. Attempts at fair presentations facilitate this task. To my delight, this book presents both established and fringe views, both critical realism (to which my output belongs) and subjectivism-relativism (which is my pet aversion). Admittedly, establishment presentations of fringe ideas may be inaccurate, but this comes with the territory.

The least allowable distortion here appears in the few discussions of probability, especially the axiom system for it (p. 118). The famous system of Kolmogorov (1933) is better. Alfred Rényi offered a mathematically better system (1955). Popper's system (1955) goes further (see below). He showed repeatedly that the probability of a hypothesis does not follow the axioms of probability.[b] Thus, the degree of confirmation of any tautology is minimal and its degree of probability is maximal. Moreover, proper probabilities are positive; the probability of a hypothesis is all too often negative.[c] Popper called "corroboration" the confirmation that follows his theory. As he noted, to formalize corroboration fully is impossible, as the data it considers are results of tests, and tests are intensional. Moreover, confirmations in science and in technology differ (Agassi 1985, 33-37).

The best comments on Popper's formal axiom system for the calculus of probability are due to Hughes Leblanc, here conspicuous by his absence. However, its novelty is obvious even without commentary. It is the first presentation of probability theory that follows the presentation of group theory: a

[b]Those who find this odd may notice that likewise the market does not abide by the laws of the free market, that some acids are sweet, and that the English horn is no horn (but an oboe).

[c]It is easy to transform a function whose values are between zero and one to a function whose values are between minus one and plus one. The resultant function, however, does not follow the calculus of probability.

formal system postpones discussing the question, What are the objects to which the theory applies? This signifies philosophically as it allows for as many answers to the postponed question as possible, namely, for as many interpretations as possible of a group or of probability, this leading to easy tests for the adequacy of each of them. The nearest to a reference to all this is an opaque denial of it in an implied contrast that Maria Carla Galavotti makes between Popper's new propensity interpretation of probability that "gains increasing popularity, but also elicited several objections" and "the pluralistic approach . . . [that] is gaining ground" *tout court* (p. 423). Galavotti omits to mention the name of the author of "the pluralistic approach"; it is Popper: his fully formal axiom system for probability embodies it.[d]

The interpretation of the calculus of probability as the degree of rational belief in hypotheses permeates the literature on the philosophy of science, in disregard of Popper's suggestion. Rational belief theory serves no purpose, he said, yet the least objectionable version of it is popular among scientific researchers, not among philosophers of science (Popper 1935/1959, §62, note *1). It abides not by the axioms of probability but by Popper's theory of corroboration that ignores all information other than results of tests. These are efforts at refutation.

This *Companion* esteems rational belief as rational agreement. By default, writers here identify it with faith in science. Some philosophers will disagree. Some of them it disregards as hostile to science. (Heidegger does make a token appearance here, not his opinions.) Others belong to the commonsense school of philosophy that takes common sense as prior to science.[e] Theirs is the realist commonsense version of the sense data theory. Russell advocated it as the hypothesis that he repeatedly tried to render a logically adequate foundation of scientific realism—adding that if it fails in this, we should relinquish it (Schilpp 1944, 718). Regrettably, this *Companion* overlooks him. The paper of Joanne Waugh and Roger Ariew about "The History of Philosophy and the Philosophy of Science" has no reference to him. They thus insult a this great light who was the initiator of the process that they describe—of rendering the field both more rigorous and more historical. They ascribe it to Carnap whose philosophy was eminently a-historical.

[d]Popper's enrichment of the logical interpretation of probability by deducing Boolean algebra from its axioms is sorely missing here, as is Leblanc's proof that Popper's system has new models (Leblanc, 1989).

[e]This was the dominant view in England in the middle of the 20th century, when I was a student there. It puzzled me very much, as I knew that in most places common sense is steeped in magic, and even in places where science runs supreme magic is still rampant.

The Praise of Accord

The book is reasonably comprehensive—it includes over 600 pages of about 450 words per page (about 900 of more ordinary pages). How can it be useful? How can practitioners use it? What can handbooks do better than encyclopedias? Practitioners can see which way the wind is blowing. Those who have no wish to trim their sails to the wind, then, will ignore handbooks. These are the familiar two kinds of practitioners (the normal and the leading, to use Kuhn's terminology). A new breed is growing, the meta-philosophers (Kuhn himself?). They do not speculate; they observe. This is pleasing, as observation is less open to controversy than speculation. (Even Popper, Kuhn's most formidable adversary, admitted Kuhn's observation that the normal is ubiquitous.) What do they observe? This is a bothersome question. Meta-philosophers may stave it off by limiting their observations to philosophy departments. The answer will not work: not all philosophers dwell there: Locke and Sartre were not academics, and Frege was a mathematician. Meta-philosophers may then limit their observations to philosophical matters like alienation or science. What is alienation? The question is welcome—unlike the parallel question regarding science. This is an observed fact. The reason for it is obvious: controversy is annoying and staving it off is pleasing; and, of the two questions, only the latter invites controversy.

Why does controversy annoy? How can we limit it? Efforts to proscribe it have failed, as have incentives for its avoidance such as, the promise of scientific status on the ground that science spells unanimity. Valuing unanimity, we may tolerate some received dogma, of course. Kuhn advocated this, knowing that allegiance to science did not save him from the advocacy of a measure of dogmatism. He noted that scientists are often dogmatic, and that there are worse dogmas than science. His point is unnerving: what if, as Popper said in response, science as a whole is deteriorating and loses its edge? Kuhn answered this too. He used Popper's own contribution to the philosophy of science: as the problem of induction is insoluble and as science progresses nonetheless, clearly it can do without induction. Hence, added Kuhn, research rests on some baseless suppositions. These are dogmas; so Kuhn observed that science avoids stagnation by replacing its dogmas under the pressure of accumulating experience. This *Companion* treats him harshly, as the wind has ceased to blow his way. It addresses the problem, What distinguishes science from dogma?

Tradition contrasts the dogmatic adherence to opinion with its rational justification. By this means the problem of demarcation of science becomes the problem of inductive justification: how does experience justify theory? This is odd. We learn from experience; science is learning from experience; we cannot say how. Meta-philosophers come to the rescue: rather than struggle with the problem they observe philosophers struggling with it. Thus, almost all of the 55 essays in this *Companion* touch upon this question, and more than half concentrate on it. Who should be a meta-philosopher then? Compilers of handbooks and their authors. The editors of this *Companion* testify that their contributors excel as philosophers too (p. xxvi).

Agreeably, meta-philosophers need not arbitrate controversies. Bayesians and critical rationalists hardly appreciate each other's output. The editors of this *Companion* allow them both to have their say. It thus records the chief ideas and standard clichés of different schools. The *Companion* also offers major arguments of one school against the other, but not fully, thus minimizing controversy. This is well within tradition, of course.

Here is a one-paragraph report on five examples of this kind of treatment of dissent here. (1) The editors admit, "Popper put forward a different conception of scientific method" from "the logical positivists"—different, not conflicting—yet he "shared" with them "hostility to psychologism"[f] and the view that "the philosophy of science is . . . normative" (p. xxiii). (2) In an essay on "logical" empiricism, Thomas Uebel notices (p. 79) that the "correct formulation" of its meaning criterion "proved controversial and elusive." "It is not clear whether the entire logical empiricist project is derailed by this," he adds. In conclusion Uebel says (p. 87), "It is not easy to separate sharply the logical empiricist philosophy of science from all approaches that dissent" from it. (3) Collin Wilson's essay on Bayesianism goes further and posits a general "convergence of opinions" (p. 112): Bayesian philosophy allows for all divergence of opinions, since empirical evidence leads to their quick convergence (p. 111).[g] (4) Alan Hájek and James M. Joyce discuss diverse theories of confirmation. They assert (p. 115) that Popper was "unfriendly to confirmation theory."[h] Hájek and Joyce conclude (p. 127) by siding with Carnap against Hume and Popper in insisting that confirmation is important for science.[i] (5) Robert Nola, on the social studies of science, tries to equate as much as he can the views of Robert K. Merton and of Karl Marx (p. 262).

[f]This is the received opinion. Carnap wrote a book (1928/1998) that employed a thoroughly psychologistic idea the he called "methodological solipsism," and he never gave it up (Creath and Friedman 2007, 161). It is erroneous. No text of any member of the Vienna Circle rejected psychologism as definitely as Popper did in §2 of his *The Logic of Scientific Discovery*.

[g]Divergent prejudices refuse to yield to evidence and thus preclude the convergence that Bayesians promise.

[h]Popper explained the great import of confirmation. He was unfriendly to theories of confirmation that do not confine it to severe tests. Those who may object that the difference is too abstract to matter should note that it was not too abstract for the U.S. Supreme Court to notice (Edmond and Mercer 2004, 199).

[i]The boot is on the other foot. Carnap's book on induction ends with the admission of failure and the promise to try again to overcome Hume's criticism (Carnap 1950, 365); Popper's theory strengthens Hume's criticism and describes scientific progress as in accord with it.

The Editors' Agenda

Agreement is valued everywhere. The peculiarity to Western philosophy is the demand that it should be rational and thus transcend the parochial. The modern version of this idea is due to Francis Bacon. He demanded the rejection of all metaphysics as preconceived opinions that distort perceptions and disturb the peace by destroying unanimity. He considered progress vital for science; he praised humility and small contributions to science, promising that no matter how small these are, they stay secure in escrow to yield great profit. He promised that (unlike theology) science would leave nothing hidden, so that the neglect of metaphysics now will later on advance metaphysics that will rest on science, thus achieving scientific status.[j] He deemed rational only proven belief and had no theory of proof. His influence today in the philosophy of science survives as the lead item on its agenda. It is the question, What belief is rational? The same holds for his categorical opposition to all daring thinking and to all controversy as rooted in personal ambition. After the debates between Einstein and Bohr about quantum mechanics won so much acclaim, this is hard to maintain. Thus, Bacon's influence persists for no good reason. Science does not command unanimity. It prevails because it is as easy to agree as to disagree, and agreement sounds friendlier. Active agreement is still easy, as it invites mere repetition, whereas active disagreement is criticism, and criticism is creative. Thus, philosophers of science who today often praise Einstein with little or no knowledge of his ideas. Thus, Laudan says (1983), the problem of the demarcation of science is that of the credibility of the scientists (*cp*. pp. 257, 282, 309). Is this true? This *Companion* dodges the question, even in its meager discussion of the sociology of science. The ideas that this *Companion* enlarges on boost ideas of Bacon.

Kant found scandalous the disagreement about the basis of agreement in science. He claimed to have resolved the scandal by his view of science as synthetic a priori knowledge. Einstein deemed this idea harmful. Under Einstein's influence, Schlick and Reichenbach relinquished certitude for a while. They then assumed that scientific theories are synthetic a priori putative truths. They later switched to a Wittgenstein-style verifiability criterion of meaning, soon to bump into the hard fact that no theory is open to verification. They then refused to give up verification. Pity. Here we find a mere echo of their older view unexplained (pp. 81-82).

---

[j]All proselytizing religions share the idea that we should surrender what little we have in return for a promise of great treasures in the near future. Comte viewed science as the religion that rejects speculative metaphysics.

The major task of the philosophy of science, say the editors in their opening kick (p. xix), is "to understand science as a cognitive activity that is uniquely capable of yielding justified beliefs about the world." This clashes with the discussion here of critical rationalism that explicitly rejects all justification (p. 59). Science has its agenda, and only rational debates should change it. How can we do that?[k] By received opinion, rational belief is at the top of the agenda of the philosophy of science. A proper *vademecum* should discuss the agenda. The editors here have missed their call. They adjudicate rather than explain.

Philosophers of science take rational belief to be all and only belief in (contemporary) science.[l] What they look for is not an alternative to this idea or for an improvement of it but the process that leads to it. They lack philosophical justification, yet if they must have it, they should prefer it scientific.[m]

The problem of induction concerns the birth, confirmation, and employment of scientific theory. Bacon said well-born theories require no confirmation. Whewell disagreed: the discovery of a theory must precede its test and tests seldom lead to confirmation. Duhem said that scientific theory has a limited domain of application; kept within these limits it is true. The critical rationalist view of the situation is much simpler. The proverbial "all swans are white" is a generalized observation that rests on the hypothesis[n] that any subspecies of birds are likely to share colors. The presence of black swans need not overturn that generalization, then, since they may belong to a different subspecies.

---

[k]The current jargon term for the change of agenda is "paradigm shift." Kuhn, its author, said the leaders are in charge of the agenda. He did not say who these are and how they achieve their status. He suggested that they gain it by exhibiting their intellectual abilities. This idea is empirically refuted.

[l]Thus Quine, a confirmed nonjustificationist, committed himself faithfully to the ontology that science advocates.

[m]The idea that any justification is better scientific than metaphysical is Popper's last word. See the last appendices to the latest German edition of his *The Logic of Scientific Discovery*. Indeed: every justification leads to infinite regress. The philosophical one is barren; the scientific one is fruitful.

[n]This hypothesis is questionable: it behooves researchers to ask, of what subspecies it holds and why, and test the answer to it. Thus, science is forever tentative. People who want scientists to be infallible hate this. They cannot annul fallibility, however. This is Popper's older and better version of Putnam's meta-induction cited here (p. 232) that dodges the conclusion that science is fallible (p. 520). The dodge rests on a hypothesis regarding natural kinds. Discussion of natural kinds takes much space here and is of no use as all its versions are hypothetical. The only interesting versions of natural kinds are those that we find in given cultures. Their dependence on cultures makes them all a priori unscientific.

This makes us more critically minded about generalizations. Inductivists want the order reversed: they want claims about a subspecies to rest on observations. This does not work.[o]

Belief and disbelief[p] come in a context that specifies whether they may stay or should undergo tests that may lead to their rejection or modification.[q] Justifications of belief too depend on context; they are thus qualified. The demand to have them unqualified is the hallmark of classical modern philosophy that demands starting afresh.[r] Hume's criticism of this demand is today uncontested: learning without presuppositions is impossible. Husserl tried to outdo Descartes; even his disciples agree that he failed.[s] And so today discussion of justification often focuses on its context: it is at times problematic, as legal test cases and appeals amply testify. Efforts to wriggle out of this situation take a major portion of this *Companion*.

If belief should stay on the agenda of the philosophy of science, it should appear together with disbelief. Bacon's demand to justify every belief had a rationale: it rested on his doctrine of prejudice that says that people never correct their errors: they always dismiss criticism with some excuse. The only viable policy for researchers, he said, is to avoid error. Whewell witnessed the switch from the particle theory of light to wave theory and took this as a refutation of Bacon's doctrine.

---

[o]The search for the universal is for freedom from constraints of contexts. The conditional with the context of an observation as its antecedent and the observation as its consequent is context-free. The tacit program of the Vienna Circle was to create such context-free observations. It was explicit in the artificial intelligence program. Its advocates admitted failure: we do not know the context; we much less observe it.

[p]Bacon held the quaint idea that belief requires justification but disbelief does not. This was his concession to skepticism. The law speaks differently: it requires that doubt be reasonable. Robert Boyle and John Locke declared court procedures as the model for scientific testimony. They did not refer to reasonable doubt, much less to laws against witchcraft. (They lived in the midst of a legal witch-hunt.) See also next note.

[q]Surprisingly the idea is often admitted without debate that scientists cling to refuted hypotheses for want of better alternatives to them. Among its advocates were Lenin, Hempel, Kuhn, Feyerabend, and Lakatos. It holds not in science but in courts, where dissenters from a default opinion may have to defend their dissent.

[r]The idea of starting afresh is radicalism, the idea of uprooting traditional ideas (radix = root). It began in political thinking (Popper 1945, I, 9). Bacon (who was politically a conservative) introduced it in science; his followers applied it to politics. That received philosophy of science is radical need not make its practitioners radicals: they are simply unaware of their radicalism or else unable to free themselves of it. Thus, some of them deny that all observation-reports are true, yet they use them as

The original designers of the 20th-century theory of rational belief did not ignore contexts. Sir Harold Jeffreys did not specify any context; J. M. Keynes offered a single context for all induction, his principle of limited variation. So did Russell, who reluctantly introduced the principle of induction as a synthetic a priori valid truth. The most influential thinker is Bruno de Finetti, the father of Bayesianism.[t] He made a complex set of psychological hypotheses that look untestable. This *Companion* seems to deny this (p. 105, first paragraph; p. 115, final two lines; p. 119; p. 124, first two lines; pp. 134-35; p. 250).[u] Perhaps not: context-less justification, we learn, must exist, or else skepticism is true (p. 115). This is a tacit transcendental proof.

On the first page of this *vademecum*, speaking on the philosophy of science in general, the editors introduce four problems from that field:

1. What characteristic is specific to science?
2. Are scientific theories true descriptions of reality?
3. What is cause, explanation, confirmation, etc.?
4. What rules and what values (if any) govern theory-change?

Answers to questions 1, 2, and 4 enjoy a broad consensus, I daresay, although the agreed answers leave much for further discussions that pertain to possible answers to question 3. They are as follows.

1. Science comprises empirically testable explanations.
2. Scientific theories are true-or-false; they are series of approximations to the truth.
4. A refuted or an otherwise unsatisfactory theory is a challenge to researchers to try to devise a better alternative to it.

---

given truths in their formulas of (mock) probability of hypotheses. Yet the situation is problematic: is it wise to declare a theory false when it conflicts with an observation? This is Boyle's problem. It disturbed him particularly since he declared only repeatable observations scientific, and that makes them all hypothetical. Russell has offered a lovely solution to this problem: theory and observations serve to correct each other. But in this volume, the treatment of Russell is worse than that of Popper.

[s]Husserl is absent from this volume; Heidegger appears here only as the target of some famous criticism.

[t]This *Companion* uses systematically the popular misnomer for his theory: Bayesian probability. It is probability only metaphorically. Bayes's law is provable. Were its use justified the way Bayesians suggest, induction would be valid and Hume's critique answered. The assertion (p. 105) that "its applicability is quite general" is unwise.

[u]The feminist essay similarly notes that traditional philosophy tacitly considers philosophizing masculine (p. 183).

Observing the tremendous growth that the philosophies of the specific sciences have undergone over the last few decades, the editors insist: as science is one, the general conception of it precedes its detailed manifestations. They choose the Vienna Circle's "conception of philosophy of science" as an example. It "was strongly challenged by three important and influential thinkers," Quine, Sellars, and Kuhn, they report. This is inappropriate. Kuhn explicitly expressed agreement with them. Quine repeatedly criticized Carnap, never the Vienna Circle. And the most recent book on Sellars (Rosenberg 2007) mentions them once, apropos of an influential collection of essays of his. Still, the editors are right in observing the ubiquity of debates. Should we examine valuable historical cases? The editors end their introduction with a historical three pages that introduce 20th-century ideas. They mention Russell as a logician, overlook Wittgenstein, and ascribe his message—metaphysics is meaningless—to Schlick and his Circle (p. xxii). They laud it and hint (p. xxiii) that Popper shared it.

The editors' report on Quine's criticism of the views of the Vienna Circle confuses the naturalism of the Vienna Circle that Popper refuted with Quine's naturalism that Popper had announced earlier.[v] Let me quote the *Stanford Internet Encyclopedia*,[w] on this point:

> Following Quine, naturalism is usually taken to be the philosophical doctrine that there is no first philosophy and that the philosophical enterprise is continuous with the scientific enterprise. . . . [S]cience, thus construed . . . , is . . . the complete story of the world.

(Popper's naturalism goes further.[x]) On method, Quine said, "slogans aside," he was "in substantial agreement" with Popper (Hahn and Schilpp 1986, 621).

The editors declare the idea of the unity of science as "the dominant dogma . . . favored by the logical empiricists" (p. xxiv), overlooking the ubiquity of this idea. Moreover, as it is metaphysical, the logical empiricists

[v] Popper (1935/1959, concluding section). Hume's naturalism, incidentally, is still different (Agassi 1986).

[w] This quote is from the essay "Indispensability Arguments in the Philosophy of Mathematics." That argument says, since sets are indispensable for science, they are real. The majority deny this; a quote from Putnam there (*loc. cit.*) accuses them of "intellectual dishonesty." This is odd: it is not clear whether Putnam admits the existence electrons (p. 232), let alone sets. The situation is thus a real mess that this *Companion* does not clean up.

[x] Popper (1963, chap. 5: "Back to the Presocratics") goes further than Quine, as he adds myth to the blend that includes science and metaphysics.

rejected it. They replaced it with physicalism,[y] the claim that the language of all science is the language of physics. No matter; the point of the editors is that in the 80s the tide changed and the disunity of science became fashionable. They ignore my "Unity and Diversity in Science" of 1969, where I say that efforts at explanation, when successful, unify; criticism, when successful, diversifies.[z]

In this *Companion* I found names of people who dropped out of the literature decades ago, and I missed names of active ones who undergo much exposure to date. All my efforts to find a method here failed. I read with interest reports on debates and wanted to see the current situation regarding them and the recommendations authors make about them. Particularly debates on the theory of confirmation here reported are murky. Carnap criticized Hempel for his having "conflated" different concepts of confirmation (p. 116). No explanation, no mention of the defense of Hempel, and no mention of Popper's concept of confirmation even though he proved that the same conflation in Carnap's work leads to inconsistency. I would love to know how things stand these days. This *Companion* should help me. It does not. A *Companion* has an advantage over an anthology in that the editors can participate in the production of its parts; it has an advantage over an encyclopedia in that it can be more boldly future-oriented. The editors of this *Companion* did not make sufficient use of these advantages. Perhaps the next *Companion* will be more useful and herald a healthy return to Russell and accent on his followers, Popper, Quine, Bunge, Gellner, Lancelot Law Whyte, and others whom I missed as I examined this *Companion*. Perhaps my slogan should be, back to Russell's *Problems of Philosophy* (1912).

After this cursory review of the editors' preface, let me skip to the final part and report on the four "hard" items on "individual sciences."

Alexander Rosenberg discusses biology. Since Darwinism relies on random variations, it defies natural laws, he observes. Given a terrain and random motion of rocks, eventually they will end up situated in valleys: randomness leads to stability. Since species are reasonably stable, biologists try to explain them as points of stability. Efforts to aid research by analyzing the concepts of "levels and units of selection" have opened new avenues of research (p. 516), and this, it seems, Rosenberg welcomes.

Robin Findlay Hendry discusses chemistry. Its building blocks are elements, usually but erroneously identified with the chemical atoms: the assumption behind it, that atoms are indivisible, is false. Hendry uses big

[y]Neurath gave an example of a protocol sentence allegedly in the physicalist language. It is grotesque.

[z]See Agassi (1975, 404-68: "Unity and Diversity in Science").

cannons, Kripke and Putnam, needlessly. Consider the assumption behind hydrodynamics, that fluids are continuous. It is worse than atomism, and the big cannons will not help here. Discussing chemistry as a part of physics, though an ancient idea, Hendry brings new cannons to back it up. He would have been more helpful had he reported diverse successes as well as the diverse extant problems, not to mention the surprising ability of the noble gases to combine with other elements in refutation of Pauli's beautiful explanation of their nobility.

Peter Clark discusses mathematics. It is hard for me to comment on it, partly for want of background knowledge. He says he wants his study to be useful for the philosophy of science, and this eludes me. His discussion of Dedekind's pioneering study of numbers (p. 560) is so incomplete that it falls between the stools of the popular and the accurate. He expresses dissatisfaction with the ad hoc proscription of paradoxical sets. This is a basic problem in the foundations of mathematics, but it does not signify for the philosophy of science. He says that classical abstract set theory "could hardly be philosophically satisfying, for . . . we are left entirely in the dark as to what sort of structures they are" (p. 561). This is surprising. The great development of modern mathematics began with Lagrange's discussion of dimensions in the abstract and with the rise of group theory that discusses some aspects of structures without asking what sort of structures they are. Boole's extensionalism then allowed for all sorts of crazy classes and thus opened the road to formalization and thus to modern logic (Bar-Am 2008). Hilbert found formalism great just because it leaves unsaid so much about the objects under study. This has direct application to the philosophy of science, not only because the techniques of Lagrange appear in physics and in economics, but also because it is easier to make abstract testable hypotheses than to speak concretely. Thus, discourse about democracy in the abstract may be more testable than about some definite democratic structures (Agassi 1989). I cannot guess why it seems to Clark so unpleasant that we have so many ways of understanding natural numbers. But perhaps this is my error: Clark accepts Hilbert's view that this characteristic is inherent in mathematics (p. 562). Also, he moves to the "Quine-Putnam indispensability arguments and the key argument in defense of naturalism," where he loses me. The question here is, Do mathematical entities exist, and if so, in what sense? Quine told me he gave up hope of ever receiving an intelligent comment on his publications because commentators refused to believe that he meant what he said when he said that in his view numbers exist just like tables and chairs. Is this the indispensability thesis? I doubt it, and I cannot see how it matters to the philosophy of science.

Simon Saunders discusses physics. He opens with a difficulty that he ascribes to Heisenberg. Russell portrayed it well when he said (Russell 1940, 15),

> Science seems to be at war with itself. . . . Naïve realism leads to physics, and physics, if true, shows that naïve realism is false.

Saunders ignores him. Eddington presents the matter as a discrepancy between his two desks, one made of tactile and smooth wood and the other almost empty and atomic (Eddington 1928, Preface). Saunders ignores him too. At least he takes the wave-particle duality seriously. Unfortunately, he misrepresents Einstein (p. 568). In 1905, presenting a new particle theory of light, Einstein said, Maxwell's wave theory approximates the new particle theory for strong fields. In 1913 Bohr came up with a different idea: a particle behaves according to Maxwell's theory when far from nuclei and over long distances. The two criteria for approximation clash for a quantum particles in a weak field traveling along great distances: they should display characteristics of quantum particle according to Einstein and wave characteristics according to Bohr. The famous two-slit experiment complies with this setup. In it, radiation displays wave characteristics along its path and quantum particle characteristics at its target. For researchers in physics, the paper here is too sketchy, inaccurate, and scarcely new. For what audience then is it written, and to what end? And how does it fit in this *vademecum*?

How can one prepare a better guide for the philosophy of science? Back to Russell's *Problems of Philosophy*. Perhaps also his *The Scientific Outlook* that presents science as a Promethean madness.

Declaration of Conflicting Interest

The author declared no conflicts of interest with respect to the authorship and/or publication of this article.

Funding

The author received no financial support for the research and/or authorship of this article.

References

Agassi, Joseph. 1975. *Science in flux*. Dordrecht, the Netherlands: Reidel.

Agassi, Joseph. 1977. *Towards a rational philosophical anthropology*. The Hague, the Netherlands: Nijhoff.

Agassi, Joseph. 1985. *Technology: Philosophical and social aspects*. Dordrecht, the Netherlands: Kluwer.

Agassi, Joseph. 1986. A note on Smith's term "Naturalism." *Hume Studies* 12:92-98. (Reprinted in Stanley Twyman, ed., *David Hume: Critical Assessments*, vol. 3, London: Routledge, 1994)

Agassi, Joseph. 1989. The logic of consensus and of extremes. In *Freedom and rationality: Essays in honour of John Watkins*, edited by F. D'Agostino and I. C. Jarvie, 3-21. Boston Studies in the Philosophy of Science, vol. 117. Dordrecht, the Netherlands: Reidel.

Bar-Am, Nimrod. 2008. *Extensionalism: The revolution in logic*. Berlin: Springer.

Carnap, Rudolf. 1928/1998. *Die Logische Aufbau der Welt*. Hamburg, Germany: Felix Meiner Verlag.

Carnap, Rudolf, 1950. Logical Foundations of Probability, Chicago: Chicago university Press.

Creath, Richard, and Michael Friedman. 2007. *The Cambridge companion to Carnap*. Cambridge: Cambridge University Press.

Eddington, Arthur Stanley. 1928. *The nature of the physical world*. Cambridge: Cambridge University Press.

Edmond, Gary, and David Mercer. 2004. The Invisible branch: The authority of science studies in expert evidence jurisprudence. In *Expertise in regulation and law*, edited by Gary Edmond, 197-242. Farnham, Surrey, UK: Ashgate.

Hahn, L. E., and P. A. Schilpp. 1986. *The philosophy of W. V. Quine*. Peru, IL: Open Court.

Kolmogoroff, Andrey, 1933. *Grundbegriffe der Wahrscheinlichkeitsrechnung*. Berlin: Julius Springer; 1956. Foundations of the Theory of Probability, 2nd ed. New York: Chelsea.

Laudan, Larry. 1983. The demise of the demarcation problem. In *Physics, philosophy and psychoanalysis: Essays in honor of Adolf Grünbaum*, edited by Robert S. Cohen and Larry Laudan, 111-27. Boston Studies in the Philosophy of Science, vol. 7. Dordrecht, the Netherlands: Reidel. (Reprinted in Michael Ruse, *But Is It Science? The Philosophical Question in the Creation/Evolution Controversy*, Buffalo, NY: Prometheus Books, 1988; and Larry Laudan, *Beyond Positivism and Relativism: Theory, Method, and Evidence*, Boulder, CO: Westview, 1996)

Leblanc, Hugues 1989. The Autonomy of Probability Theory (Notes on Kolmogorov, Rényi, and Popper). *British Journal for the Philosophy of Science* 40 (2):167-181.

Musgrave, Alan. 1995. Realism and idealization (metaphysical objections to scientific realism). In *The problem of rationality in science and its philosophy: on Popper vs. Polanyi, the Polish conferences, 1988-89*, edited by Józef Misiek, 143-66. Boston Studies in the Philosophy of Science, vol. 160. Dordrecht, the Netherlands: Reidel.

Popper, Karl R. 1935/1959. *The logic of scientific discovery*. London: Hutchinson.

Popper, Karl R. 1945. *The open society and its enemies*. London: Routledge.

Popper, Karl R., 1955. Two Autonomous Axiom Systems for the Calculus of Probabilities. *British Journal for the Philosophy of Science* 6 (21):51-57.

Popper, Karl R. 1963. *Conjectures and refutations*. London: Routledge.

Rényi, Alfréd, 1955. On a new axiomatic theory of probability. *Acta Mathematica Academiae Scientiarum Hungaricae* 6, 285-335.

Rosenberg, Jay F. 2007. *Wilfrid Sellars: Fusing the images*. Oxford: Oxford University Press.

Russell, Bertrand. 1912. *The problems of philosophy*. London: Williams and Norgate and many later editions.

Russell, Bertrand. 1940. *An Inquiry into meaning and truth*. London: Allen & Unwin. (Pelican ed., 1962)

Schilpp, Paul Arthur. 1944. *The philosophy of Bertrand Russell*. LaSalle, IL: Open Court.

Watkins, John. 1957-1958. Epistemology and politics. *Proceedings of the Aristotelian Society* 58:144-55.

Bio

Joseph Agassi, FRSC, is professor emeritus in Tel Aviv University and in York University, Toronto. He is the author of about 20 books and editor of a few, as well as of over 400 contributions to the learned press.