On Theories: Logical Empiricism and the Methodology of Modern Physics, by William Demopoulos. Cambridge, MA: Harvard University Press, 2022. Pp. xxiv + 247.

Everyone will find something interesting in this book, and many will find something or other that they completely disagree with. William Demopoulos was no fan of "isms", and he was no builder of systems. As a result, this book — written during Demopoulos' decade-long battle with terminal illness — does not provide a sustained defense of realism, or antirealism, or pragmatism, or whatever-ism. Similarly, one would be hard pressed to identify some key motive behind Demopoulos' work, such as, e.g., the naturalism that served as the polestar for the work of Quine and Lewis. Nonetheless, Demopoulos spent his entire career developing well-informed stances on most of the issues that occupy philosophers of science (especially of the physics orientation), and his considered opinions are unique, and sometimes in stark opposition to those of other philosophers in the field. So this book will, or at least should, provoke many new discussions and debates.

In keeping with Demopoulos' style, I will not attempt to put his diverse ideas into a simple nutshell. But I will take a few of the points he makes in this book, and explain why they are so interesting, important, and worthy of our attention.

1 Overview

Demopoulos' book includes both a foreword and an afterword by Michael Friedman. The practical reason for these supplements is, unfortunately, Demopoulos' untimely death. The result, however, is quite brilliant, as Friedman's commentary provides an ideal contextualization of Demopoulos' work — and not just the work in this book. In fact, Friedman's afterword explains the entire trajectory of Demopoulos' philosophical work, from the 1970s until his death in 2017. Similarly, Friedman's foreword provides, among other things, a historical and systematic framework for the following chapters.

Demopoulos' own text is contained in five chapters: an introduction, and then four numbered chapters. Chapter 1 takes up the general issue of "theoretical terms" that so bedevilled the logical empiricists. Chapter 2 turns to a specific episode in the history of science: Perrin's explanation of Brownian motion in terms of the molecular structure of matter. Chapter 3 takes up Henri Poincaré's philosophy of physics, and Chapter 4 takes up the interpretation of quantum mechanics.

Glancing at the titles of Chapters 1 through 4, one might guess that these chapters have little to do with each other. However, Demopoulos' introduction and Friedman's foreword go a long way toward triangulating on a central motif. While both Demopoulos and Friedman flag the motif of "theory mediated measurement", it would present an overly narrow view of Demopoulos' contribution to call that *the* driving idea of the book. In fact, Demopoulos' interest in theory mediated measurement is just one manifestation of a thoroughly empiricist attitude that pervades his work.

First, Demopoulos is constantly asking "*how* do we gain epistemic access?" — even if he believes, contrary to the typical empiricist position, that humans *can* gain epistemic access to unobservable reality. Second, when it comes to his own methodology, Demopoulos also shows his empiricist cards, in particular with his emphasis on how things actually work in scientific practice. (For example, Demopoulos rejects the method of hypothesis because it was not actually convincing to scientists in the nineteenth century.) So, although Demopoulos might not announce his position as empiricist, he can safely be placed alongside Locke, Hume, and Kant in maintaining the centrality of experience in any account of scientific knowledge.

2 The method of hypothesis

If someone asked me whether Demopoulos is a scientific realist, then I would be hard pressed to answer. While he does speak frequently of epistemic access to unobservables, he resists the terms in which this realism debate has traditionally been cast. In particular, most scientific realists accept some version of inference to the best explanation (IBE). However, Demopoulos rejects IBE, along with all other versions of the method of hypothesis. What's more, and more interesting, is that Demopoulos rejects this method not on the basis of a general skepticism, but apparently because it misses the interesting parts of how scientists actually do get epistemic access to unobservable reality.

Roughly speaking, the method of hypothesis says that if a hypothesis H bears some quasi-logical relation to empirical claims $E_1, \dots, E_n$, and if these empirical claims are established, then support accrues to H. (I say "quasi-logical" in order to include relations such as "is the best explanation of".) Various versions of the method of hypothesis came into vogue with the fall of logical empiricism and the rise of scientific realism. This resurgence led, in turn, to certain skeptical arguments against IBE, such as van Fraassen's (see Psillos, 1996). Thus, the debate about scientific realism turns to a great extent on the question of whether the method of hypothesis is rationally compulsory or even warranted.

Where does Demopoulos come down on this debate? That's an interesting question, because Demopoulos is just as critical of antirealism as he is of the method of hypothesis. Demopoulos announces his stance in the introduction, where he calls for a return to theory mediated measurement, and he provides further support for this approach in Chapters 1 and 2. I found the discussion in Chapter 2 particularly engaging and illuminating. Here Demopoulos takes up the infamous case of the molecular hypothesis (i.e. "physical matter is composed of molecules") and how it was — at least in the minds of scientists such as Poincaré — confirmed by Perrin's work with Brownian motion. Demopoulos points out that molecular-kinetic theory had been on the table for decades, and by the standards of the method of hypothesis, it had quite a bit of support. But many were not convinced. It was only with Perrin's application of theory mediated measurement that skepticism was exorcized.

Demopoulos' reader should now be intrigued, for it seems that "theory mediated measurement" is a holy grail for scientific practice. But what exactly is it? For better or worse, the answer we get in Demopoulos' book is primarily historical: theory mediated measurement is what Newton proposed in the *Principia*. Recall, in particular, that "force equals mass times acceleration" links a theoretical quantity (force) in an quasi-definitional way with an observable quantity (acceleration). But does this offer us a new way of looking at theoretical terms that differs in an essential way from, say, Carnap's reductive definitions? That is an interesting question, and there are many ways to develop a response. One way to respond is to say that there is some fancy formal reconstruction of Newton's method that makes it look better than Carnap's. However, Demopoulos shows not the slightest inclination toward a formal reconstruction of Newton's or Perrin's methods. Indeed, "it would be premature to conclude that Perrin's argument is adequately represented by a probabilistic reconstruction" (p 72). Given how general and flexible probability theory is, it seems clear that Demopoulos does not hold out hope for any formal account of theory mediated measurement.

3 Putnam's model theoretic argument

It's a common theme in Demopoulos' work that he is responsive to data. Sometimes that data is a historical episode, such as Perrin's work on Brownian motion. And sometimes that data is a mathematical theorem, such as Bell's theorem or Putnam's model theoretic argument. In all of these cases, Demopoulos asks "what really is the take-away point?"

In the case of Putnam's model theoretic argument, Demopoulos identifies several take-away points, and he also argues strongly against other claims about what the take-away point is. Recall that Putnam argues that if a theory T is consistent, then there is no reason not to consider T as true in our world. Of course, most other philosophers besides Putnam have considered the result to be, in some sense, a reductio ad absurdum. That is, something false must have been assumed, because it would be crazy to think that truth is nothing more than consistency.

The most famous response to Putnam is the realist retrenchment of David Lewis (1984). According to Lewis, the false assumption in Putnam's argument is that every subset of objects in our world could be the extension of a predicate. In contrast to this assumption, Lewis suggests that:

1. (Natural properties) Only certain subsets of objects correspond to *natural properties*, i.e. properties that cut nature at the joints.

2. (Reference magnetism) We should interpret other people's utterances so that their predicates refer to natural properties.

The conclusion, says Lewis, is that Putnam's trick — of assigning things the extension they to have for T to be true — does not show that T is actually true.

While some philosophers, such as Van Fraassen (1997), see Putnam's argument as trading on some confusion, metaphysicians tend to see it as supporting the doctrine of natural properties. But here Demopoulos, once again, teaches us that there are not just two approaches to these issues: while he agrees with the metaphysicians that Putnam's argument has a significant philosophical moral, he draws completely different morals than the metaphysicians do.

The first moral that Demopoulos draws is that the logical empiricist reconstruction of theories is inadequate as an account of our epistemic access to the unobservable world. Recall that the logical empiricists adopted a doctrine of "partial interpretation" where the vocabulary of a theory is divided into observational and theoretical terms, and where the two kinds of terms of linked via correspondence rules. Seen in this light, Putnam's argument — and a similar result by John Winnie — shows that the reference of theoretical terms is never fixed by the correspondence rules. We could then respond to that fact in the spirit of Carnap, by taking scientists to be agnostic about what theoretical terms refer to. However, Demopoulos balks at this suggestion. For him, this outcome would be tantamount to renouncing the idea that theories *enable* the scientist to get a better epistemic grip on the unobservable world.

What then is the upshot? One might guess that Demopoulos would blame the syntactic view of theories, and so turn to the semantic view for a solution to the problems. However, he argues that the semantic view is also subject to Putnam's reductio. In fact, Demopoulos claims that it is a fatal flaw of constructive empiricism that it lacks the resources to distinguish between a theory being empirically adequate and a theory being true. Here one is left to wonder whether Demopoulos has not made the same mistake as so many other readers of van Fraassen, of taking constructive empiricism to be a normative doctrine about what one should or should not believe. As van Fraassen has repeatedly clarified, constructive empiricism is first and foremost a claim about the goal of science: that the goal is to produce empirically adequate theories. But in this case, how is Putnam's paradox a problem for constructive empiricism?

4 Bell's theorem

In his early career, Demopoulos was one of the pioneering explorers of the philosophical consequences of the Kochen-Specker no hidden variables theorem for quantum mechanics. What we find in Chapter 4, however, is that Demopoulos has come to see Bell's theorem as the more interesting result. Interestingly, Demopoulos' conclusion here agrees with the view of certain "quantum reformers", i.e. those who believe that the statistical algorithm of QM ought to be supplemented by an underlying theory (such as Bohmian mechanics or the Ghirardi-Rimini-Weber theory) that is broadly classical, in the sense that it is a theory of matter in motion. Nonetheless, the agreement ends at "Bell's theorem has an interesting lesson"; for Demopoulos draws a completely different kind of conclusion from Bell's theorem than the quantum reformers do.

It is easy to guess why the quantum reformers would make little of the KS theorem and correspondingly much of Bell's theorem. The KS theorem shows that, if quantities are in one-to-one correspondence with a certain kind of mathematical object (viz. self-adjoint operators), then not all quantities can have a determinate value at the same time. However, there is a theory, viz. Bohmian mechanics, for which all quantities are determinate, and which reproduces the statistics of QM. So the KS theorem may show something about *quantum theory*, i.e. that not all its operators correspond to quantities, but it does not yet tell us anything about the *world itself*.

While the KS theorem rules out assigning determinate values to all quantum-mechanical observables, Bell's theorem was the result of his trying to figure out how a determinate theory, viz. Bohmian mechanics, actually manages — despite such no-hidden variables theorems — to reproduce the predictions of QM. Bell's answer is that Bohm's theory can do this precisely because of its non-local nature. Indeed, the stronger takeaway, according to contemporary Bohmians is that, "if certain predictions of quantum theory are correct then our world is non-local" and, indeed, "in the light of Bell's theorem, the experiments thus establish that our world is non-local" (Goldstein et al., 2011). Note the decisive shift to the material mode: Bell's theorem does not merely yield a lesson about *quantum theory*, but about the *world itself*.

Based on his pronouncements in this book, it is clear that Demopoulos' reading of Bell's theorem is rather different than that of the quantum reformers; and this disagreement manifests itself clearly in the distinct ways that they understand the relationship between Bell's theorem and the EPR argument. Recall that EPR's 1935 argument claims that if locality holds then QM is incomplete, i.e. it omits some "elements of reality". And what does Bell's theorem say about EPR's challenge to QM? According to Demopoulos, Bell's theorem undermines the EPR argument: "...by far the most important objection to the [EPR] argument is the one raised by Bell's theorem" (p 155). However, the contrapositive of EPR's claim is that if QM is complete then non-locality, and this is precisely what the QM reformers insist is the lesson of Bell's theorem: that the world is non-local whether or not QM is complete.

A partial solution to this puzzle is provided by reflecting on the history of the interpretation of Bell's theorem. Famously, Jon Jarrett argued that the probabilistic assumption of Bell's theorem is a conjunction of realism and locality. The result is that the experimental violation of the inequality shows that either not-realism (i.e. no hidden variables) or non-locality. But the quantum reformers protest this analysis. They maintain that locality is sufficient to derive the inequality, and that "violations of Bell's inequality show us that the world is not causally local" (Maudlin, 1994a, p 98).

In the meantime, work by Fine, Pitowsky, and others shows that if all of the probabilities involved were classical, then locality would come for free (see Fine, 1982; Baez, 1987; Raggio, 1988; Pitowsky, 1989). But then one might read the violation of Bell's inequality as showing the failure of classical probability theory, rather than dynamical non-locality. And it seems, at times, if Demopoulos is still attracted to this reading:

> Bell's theorem shows that in the case of the EPR state, the totality of these theoretically derived probability assignments cannot be interpreted as ratios of classical truth-value assignments. (p 169)

And yet, Demopoulos is not one of those interpreters of QM who insists that it *is* a local theory. In fact, he claims that QM is a "local theory, albeit a local theory of *nonlocal* correlations" (p 183).

The careful reader is now likely to be quite confused about what exactly the lesson of Bell's theorem is supposed to be. At first it seemed that Demopoulos might side with the quantum reformers in claiming that the violation of Bell's inequality shows that world is non-local. Then it seemed that Demopoulos sides instead with the quantum logicians who claim that the violation of Bell's inequality shows the failure of classical probability theory. It seems then that this is another case where Demopoulos simply won't take the bait to provide an answer to what he takes to be an oversimplified question.

5 Principle theories

I have already pointed out a couple of cases where Demopoulos gives a epistemological (or methodological) gloss on an issue that others read ontologically. First, while metaphysicians see Putnam's paradox as confirming the doctrine of natural properties, Demopoulos sees it as disconfirming a certain picture of the structure of scientific theories (most especially the logical empiricist reconstruction of theories). Second, while quantum reformers take Bell's theorem to tell us that the world is non-local, Demopoulos takes it to show that certain framework assumptions of classical physics cannot be maintained. But the most decisive case of Demopoulos' defection from the ontological point of view is his stance on relativity and quantum mechanics as "principle theories".

Roughly speaking, a principle theory describes laws that ordinary things — i.e. things found in the "manifest image" — must obey. A classic example here is thermodynamics, which says that a perpetual motion machine cannot be built. Similarly, the young Einstein seems to have arrived at special relativity through principle-theoretic thinking, and in particular, the principles that no physical object can travel faster than light and that the laws of physics are equivalent in all reference frames. In contrast to principle theories, constructive theories say what there is and how it changes over time. So, a classic example of a constructive theory is Newtonian particle mechanics.

Now, one might hope and expect that a constructive theory would ground a corresponding principle theory. (In fact, isn't this grounding what we see in the relationship between classical statistical mechanics and thermodynamics?) However, the converse idea seems problematic, for how could one derive an ontology from the assumption that certain principles are universally valid? Wouldn't that method be completely backwards? What's more, there is a sense among many philosophers (and some physicists) that a principle theory without an underlying constructive theory would be explanatorily unsatisfactory.

At this point the situation becomes dialectically charged. Some philosophers and physicists claim that special relativity was a good principle theory, and that quantum mechanics becomes intelligible when we see it as a principle theory. Besides Demopoulos himself, one of the more distinguished proponents of this stance is Carlo Rovelli. On the opposite side from Demopoulos, we find Harvey Brown, who argues explicitly against this way of thinking (see Brown, 2005; Brown and Timpson, 2006), as well as a host of quantum reformers, who argue that QM is broken precisely because it does not have the features of a good constructive theory.

Unfortunately, Demopoulos devotes little space in this book to analyzing this debate, or to arguing against the constructive-theoretic view. However, he states the principle-theoretic view in unequivocal terms.

> Just as special relativity understands the phenomena of dilation and contraction in terms of features of Minkowski space-time, rather than the assumed effect of the electromagnetic character of the constitution of matter, the probabilities exhibited by the EPR correlations are understood as a consequence of the Hilbert space structure of the properties of physical systems, rather than the effect of unknown local causes. (p 183)

This claim is doubly controversial. First, it already controversial whether relativity does understand dilation and contraction in terms of features of Minkowski spacetime. Certainly that view has its proponents (such as Graham Nerlich and John Norton), but it also has its opponents (such as Harvey Brown). But it is even more controversial, and I will say implausible, to say that "the probabilities ... are a consequence of the Hilbert space structure". The problem here is that while Minkowski spacetime is an ambiguous creature (is it a physical or mathematical object?), it seems rather clear that Hilbert space is not a physical object and does not directly represent one. But how could physical facts be a consequence of a mathematical structure? Of course, there will be some realist types who will double down by reifying Hilbert space. However, that is not the direction that Demopoulos would want to go. I would suggest, instead, that Demopoulos and his fellow principle theorists ought to be more steadfast in resisting the "dogmatic" call to explain all principles in terms of fundamental ontology. In particular, a principle theorist should not say that Minkowski spacetime exists and grounds the Lorentz transformations, but that the Lorentz transformations are presuppositions for any intelligible talk about ontology. That is, descriptive claims are meaningful only if they transform the right way under the relevant symmetries.

6 Completeness of quantum theory

As mentioned previously, Demopoulos reads Bell's theorem in a very different way than the quantum reformers do: he sees it as supporting the completeness of QM against the charge of EPR, whereas they see it as demonstrating worldly nonlocality. Of course Demopoulos is not the first to say that QM is complete. However, he adds an interesting twist to this discussion by providing a positive definition of "complete".

The key to understanding Demopoulos' sense of completeness is his invocation of Gleason's theorem, which can be stated as follows: let $\mathcal{B}$ be the collection of all Boolean sublattices of the lattice of projection operators on a Hilbert space (of dimension greater than two). Now let ω be a function from pairs $\langle A, a\rangle$, where $A \in \mathcal{B}$ and $a \in A$, to the unit interval $[0,1]$. We say that ω is a *generalized probability measure* (or just "measure" for short) if it behaves as such for each fixed $A \in \mathcal{B}$. We say that ω is *non-contextual* if $\omega(A,a) = \omega(B,a)$ whenever $a \in A \cap B$. Finally, Gleason's theorem shows that if ω is a non-contextual measure, then there is a density operator ρ such that $\omega(A,a) = \mathrm{Tr}(\rho a)$ for all $A \in \mathcal{B}$ and $a \in A$. Since the density operators are just the (countably additive) quantum states, Gleason's theorem shows that non-contextual probability measures correspond one-to-one with quantum states.

Gleason's theorem has the following corollary: for any Boolean sublattice $A \in \mathcal{B}$, and for any classical probability measure μ on A, there is a quantum state ρ such that $\mu(a) = \mathrm{Tr}(\rho a)$ for all $a \in A$. In particular, for any "classical context", corresponding to a Boolean sublattice A, any hidden variable is already represented by some quantum state! It is in this sense that Demopoulos sees Gleason's theorem as establishing the completeness of quantum mechanics.

Demopoulos' claim that QM is complete is unlikely to convince quantum reformers, who agree with John Bell that "either the wave-function, as given by the Schrödinger equation, is not everything, or it is not right," that is, either QM is incomplete or QM is incorrect. Nonetheless, the burden of proof is now on these self-proclaimed reformers to explain what they mean by QM being incomplete. If I might help them to an answer, then I suspect it is contained in Maudlin's (1994) critique of van Fraassen's modal interpretation (a critique that applies with equal force to several other no-collapse interpretations of QM): even if the hidden variables (i.e. value states) are represented by vectors in the Hilbert space, QM does not provide a dynamical law for evolution of these value states. However, *pace* Maudlin, for some such interpretations, there are in principle reasons against dynamical laws for the value states. For example, for a Bohrian complementarity interpretation, the classical context of description is specified by the describer, and not by the world. Hence, the application of dynamical laws should be expected to apply only within such a context, and not across contexts. (Furthermore, Bohr himself thinks that the context of spacetime coordination is strictly incompatible with the application of those conservation laws that are needed for the definition of dynamical concepts.) Similarly, for the Everett interpretation, measurement outcomes are merely appearances, and one would not expect physics to supply dynamical laws for the appearances.

It is not clear from the text how Demopoulos might respond to this challenge from the quantum reformers. However, he does mention another fact that would likely figure in his response. Recall that Gleason's theorem shows that every *non-contextual* measure on the partial Boolean algebra is represented by a quantum state. If we turn that around, we have the following obvious fact: quantum probabilities are non-contextual. Friedman connects this fact with the traditional concept of objectivity in the following fashion:

> ... the fact that dynamical properties are relative to particular measurement contexts seems incompatible with their *objectivity*, insofar as invariance over different measurement contexts (different perspectives) is a criterion of objectivity in general. So we cannot have both determinacy and objectivity for the dynamical properties of particles.
> ... [and yet] we have objectivity for all probabilities defined over this totality due to the noncontextuality of quantum probability. (p 194)

The picture that emerges here is, I must admit, quite intriguing. Could it be that the magic of quantum theory is in giving us a tool to create objective (non-contextual) probabilistic descriptions on the basis of the subjective (contextual) data of experience?

7 Bohr and the shifty split

The preceding points already put Demopoulos at odds with the views of many philosophers of science — both of the realist and antirealist persuasions. However, where Demopoulos really shows himself as contrarian is in his defense of Niels Bohr's understanding of quantum mechanics. Most philosophers these days don't even consider Bohr a worthy opponent; and I fear that Demopoulos' charitable reading of Bohr might meet with a corresponding silence. However, Demopoulos brings a genuinely novel angle by showing Bohr to be a Newtonian empiricist rather than a crass instrumentalist.

I would propose that one key to understanding Demopoulos' approach is to see him as a non-naturalist about epistemology. That is, Demopoulos does not believe that epistemological claims (such as "E is evidence for H") will follow straightforwardly from primitive ontological claims (e.g. "there is a particle subject to an inverse square gravitational force."). To be clear, Demopoulos never says that he is a non-naturalist; but I think it can be inferred from his claim that evidentiary concepts can float free from underlying theoretical concepts. For example: "Bohr's thesis is not about the primacy of classical concepts in the *theoretical* claims of any system of any system of future physics. Their primacy is *evidentiary*." (p 120).

Demopoulos' idea of an evidentiary framework deserves study in its own right. Upon a first reading, it seems that an evidentiary framework is supposed to be a sort of folk theory that humans use to understand themselves, the data they collect, and the significance of that data for their theoretical commitments.

> A framework within which experiments are designed and their results reported and assessed must contain standards that enable agreement regarding the cogency and intended significance of experimental results. (p 122)

Note the use of the normative words "standards" and "significance", showing that an evidentiary framework is supposed to play roles that no fundamental physical theory can plan.

So now to turn back to Bohr. Two of Bohr's frequently dismissed claims are:

1. In applying quantum theory, we need to split the world into two parts — a subject and an object.

2. Classical concepts are indispensible for our description of the world.

Recall that John Bell labelled the first claim "the shifty split", suggesting that there is something problematic with its arbitrariness. What Bell did not see, and that Demopoulos does, is that the split is not intended ontologically. Indeed, the subject is not describing himself, but is using his own evidential framework to interpret the quantum mechanical formalism.

What Bohr — helped by Demopoulos — says here should not be considered to be so controversial. We know well that understanding new theoretical vocabulary (or how mathematical structures apply to physical reality) requires that we are able to articulate their meaning in our old vocabulary, i.e. in our common evidentiary framework. Some philosophers dream of kicking this ladder away, i.e. of finding a theory that explains its own meaning. Demopoulos does not argue that such a theory is impossible, but he is clearly not animated by that dream.

8 Conclusion

The relatively short length of this book conceals an astounding breadth in the topics it covers. It does indeed provide us with much material to think and debate about. I would suggest, in particular, that we ought to take a closer look at the notion of theory mediated measurement, and whether it can be elucidated by formal methods, and whether it might provide a middle way between realism and antirealism. I would also suggest that Demopoulos' statements about Bell's theorem and about principal theories should catalyze new discussions of these issues.

While I am not convinced by everything that Demopoulos says in this book, I am struck with overwhelming admiration for his rigor, style, and honesty as a thinker. It is books like this one that demonstrate the value of philosophy of science.

Hans Halvorson
Princeton University, USA
hhalvors@princeton.edu

References

Baez, John (1987). "Bell's inequality for C^*-algebras". In: *Letters in Mathematical Physics* 13.2, pp. 135–136.

Brown, Harvey R (2005). *Physical relativity: Space-time structure from a dynamical perspective*. Oxford University Press.

Brown, Harvey R and Christopher G Timpson (2006). "Why special relativity should not be a template for a fundamental reformulation of quantum mechanics". In: *Physical theory and its interpretation*. Springer, pp. 29–42.

Fine, Arthur (1982). "Hidden variables, joint probability, and the Bell inequalities". In: *Physical Review Letters* 48.5, p. 291.

Goldstein, Sheldon et al. (2011). "Bell's theorem". In: *Scholarpedia* 6, p. 8378. DOI: 10.4249/scholarpedia.8378.

Lewis, David (1984). "Putnam's paradox". In: *Australasian journal of philosophy* 62.3, pp. 221–236.

Maudlin, Tim (1994a). *Quantum non-locality and relativity: Metaphysical intimations of modern physics*. John Wiley & Sons.

— (1994b). "The unbuttoned empiricist: Van Fraassen speculates about the quantum world". In: *Philosophical Books* 35.2, pp. 94–101.

Pitowsky, Itamar (1989). *Quantum probability-quantum logic*. Springer.

Psillos, Stathis (1996). "On van Fraassen's critique of abductive reasoning". In: *The Philosophical Quarterly* 46.182, pp. 31–47.

Raggio, Guido A (1988). "A remark on Bell's inequality and decomposable normal states". In: *Letters in Mathematical Physics* 15.1, pp. 27–29.

Van Fraassen, Bas C (1997). "Putnam's paradox: Metaphysical realism revamped and evaded". In: *Philosophical Perspectives* 11, pp. 17–42.