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INTERVISTE

Conversation with John P. Burgess

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John P. Burgess is the John N. Woodhull Professor of Philosophy at Princeton University. He obtained his Ph.D. from the Logic and Methodology program at the University of California at Berkeley under the supervision of Jack H. Silver with a thesis on descriptive set theory. He is a very distinguished and influential philosopher of mathematics. He has written several books: A Subject with No Object *(with G. Rosen, Oxford University Press, 1997),* Computability and Logic *(with G. Boolos and R. Jeffrey, 5th ed., Cambridge University Press, 2007),* Fixing Frege *(Princeton University Press, 2005),* Mathematics, Models, and Modality *(Cambridge University Press, 2007),* Philosophical Logic *(Princeton University Press, 2009),* Truth *(with A. G. Burgess, Princeton University Press, 2011),* Saul Kripke: Puzzles & Mysteries *(Polity Press, 2012),* Rigor

& Structure *(Oxford University Press, 2015), and* Set Theory *(Cambridge Elements, Forthcoming). In this interview, Professor Burgess talks about how his interests in mathematics and philosophy developed and relate to each other. He then answers questions about specific themes of his philosophical work, with a focus on issues pertaining to philosophy of mathematics.*

1. Dear John, thank you very much for accepting to be interviewed for APhEx! Let me start by asking some biographical questions. You started your education in mainstream mathematics; when did you decide to focus on mathematical logic (set theory)?

JPB: Thank you, Silvia, for the invitation to be interviewed. Here is how I came from mathematics more specifically to mathematical logic: my grandfather, an immigrant groundskeeper, taught me basic arithmetic before I started kindergarten, and I remained ahead of my classmates in mathematics right through secondary school. Fortunately my parents and public school teachers in Cleveland, Ohio were very good about directing me towards resources allowing me to advance at my own pace. I first became curious about mathematical *logic* at age twelve, when my parents bought me for Christmas the anthology *The World of Mathematics*, where I was immediately drawn to the more "foundational" essays. But for me, as for Stewart Shapiro, whom *APhEx* has also interviewed (Moruzzi and Sereni 2013), the most important special resource was the summer mathematics program for high-school students at Ohio State in Columbus, run by Arnold Ross, emphasizing number theory and algebra but including a course on logic by Ivo Thomas of Notre Dame. That was my first opportunity to *study,* and not just read *about*, mathematical logic. Father Thomas taught mainly classical and intuitionistic sentential logic, but a baby version of Gödel's incompleteness theorem was included. I majored in mathematics in college at Princeton, where my course work was mostly in core mathematics, especially algebra and algebraic number theory (with Goro Shimura) and algebraic topology (with Ralph Fox), but at Princeton undergraduates also do "independent work" in addition to course work, and mine was in logic, with Simon Kochen. I worked partly on applications of model theory to algebra, his specialty, and partly on topics in modal logic I had learned from Thomas as a Ross program counselor during the summers between college academic years. My senior thesis became my first publication, "Probability Logic" (1969).

And here is how I came from mathematical logic more specifically to set theory: I was accepted into the interdepartmental *Group in Logic* at Berkeley, founded by Alfred Tarski, for graduate study, but had to delay entry for a year, which I spent back at Ohio State, where there were no logicians, and so I did a master's degree in core mathematics, specifically algebraic topology. I became serious about set theory upon my belated arrival in Berkeley, where I took the beautifully-taught basic course in the subject from Robert Solovay, and was soon introduced to more advanced topics by Ronald Jensen, Robert Vaught, and my eventual dissertation supervisor Jack Silver. I put the sort of work in logic I had done as an undergraduate wholly aside. My thesis was primarily on descriptive set theory, secondarily on combinatorial set theory and the applications of those two branches of set theory to the model theory of infinitary and generalized-quantifier logics. Using high-powered results and methods of Silver I was able to solve two problems from a pre-publication version of Harvey Friedman's list of ninety-odd questions in mathematical logic. After completing my PhD I took up a two-year post-doc at the University of Wisconsin, in one of the few mathematics departments that had a heavy investment in logic (S. C. Kleene, J. H. Keisler, K. J. Barwise, Kenneth Kunen; Mary Ellen Rudin was also there, and a big influence, though not in an official position). I worked on set theory there, but left after one year to take up the position I still hold.

2. And when did you start being interested in philosophy?

JPB: I had had thoughts I recognize in retrospect as philosophical (concerning inverted spectra and solipsism) from a very early age, but never discussed them with anyone for fear of being thought crazy. I first encountered the *word* "philosophy" in *A Child's History of the World*, though the only "philosopher" about whom I remember anything from that work is Solon and what he said to Croesus. I also in high school encountered figures in French literature from Montaigne to Rousseau who are called philosophers in a broad sense, and even wrote the main essay in my “advanced-placement” final exam in French on Pascal. But I did not see any connections that united these diverse materials and the logic I had been studying at Ohio State into a single discipline or enterprise, until the summer between high-school and college when I was working for the post office by day and in the evening working through the one philosophy book in my parent’s house, Russell’s history. (Much later I was for a time to live quite near to and frequently visit the Barnes Museum, where Russell originally delivered the lectures on which the book is based.) On arrival at Princeton I was eager to dig into the subject, and took courses on problems in philosophy (Peter Hempel), theory of knowledge (Gil Harman), philosophy of mathematics (*not* with Paul Benacerraf, who was on leave), and philosophy of “behavioral science”, where I think it was I first encountered the work of Quine. I continued to study Quine on my own independently of courses, as I and some of my friends studied also Marx and Freud. Those two were figures not represented in Princeton’s curriculum at the time, the late sixties, though a course on Marx was taught — by Donald Davidson! — the year after I graduated, at students’ demand. The ability to continue studying philosophy alongside mathematics is what drew me to Berkeley, where I continued in philosophy of mathematics and of language in seminars of Charles Chihara and Dagfinn Føllesdal.

3. More generally, how did you manage to reconcile your interests in mathematics and philosophy?

JPB: Perhaps I never did, but simply flitted back and forth between them for the rest of my career.

4. More biography! You were an undergrad in mathematics at Princeton, and then you returned to Princeton as an Assistant Professor in philosophy. Did you feel completely at home in the philosophy department, or did you miss the mathematics department?

JPB: I am a logician and as such feel completely at home neither in philosophy nor in mathematics. Our subject is, unfortunately, just a bit too small to have departments of its own. I remember being asked in my Princeton job interview “Why do you think a department like ours should be interested in someone who does *Journal of Symbolic Logic* type logic?" and answering "I was wondering that myself". If I got hired despite that reply, I suspect it was mainly due to the influence of the late Dick Jeffrey, who became my most valued colleague. But though I have now been in the philosophy world for well over four decades, and director of undergraduate studies in my department for about three, to this day I feel "in it but not of it". One factor is that among philosophers generally, as fashions have shifted, an interest in and the influence of logic have been in continuous decline throughout the whole course of my career, so that I could hardly in good conscience encourage a graduate student in philosophy who was drawn to the area to do a dissertation in it. (I *have* had a handful of graduate students, generally excellent, over the years, among whom I will mention only the earliest and most celebrated, Penelope Maddy, whom I first met when I was a graduate student and she an undergraduate at Berkeley. Her dissertation led to her book *Realism in Mathematics* (1990).)

For six years, until I got tenure (including the whole period when Maddy was working with me), I tried to keep one foot in each camp, planning to retreat back to mathematics, perhaps at Ohio State, if I didn't make it in philosophy. It was during this period that I did most of my work on measurable selection theorems in descriptive set theory, one happy result of which was that I got to write a joint paper with R. Daniel Mauldin, a fairly frequent co-author of Paul Erdös, and so acquired an "Erdös number" of two. I also at this time did most of my work in tense logic, pursuing Arthur Prior's work on future contingents, to which I had been introduced by Thomas. But these are not the kind of core mathematics done in the Princeton mathematics department (where logic was represented by Kochen, a fraction of Edward Nelson, and a smaller fraction of John Conway, none of whom has been replaced by a logician), and I do not flatter myself that I have ever been a research mathematician on anywhere near the level of those to be met with at the Princeton department's daily tea, or that I could at all have fit in there. So there can be no question of my missing the mathematics department, except insofar as all of us past a certain age miss our youthful student days. But the place where I hung out in my student days, in the collegiate gothic old Fine Hall, with the stained-glass windows of mathematical and physical formulas and the Einstein quotation inscribed over the fireplace, had while I was away at Berkeley been abandoned and

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left to East Asian Studies, who must puzzle over these curious archeological remains, while mathematics and physics moved into a most brutal specimen of brutalist architecture at the bottom of campus, in which symbolically physics inhabits a sprawling, semi-underground space, while mathematics occupies a lofty tower.

5. What would you say is your most important result in logic?

JPB: Though I was only diagnosed in my forties, ever since childhood I have been affected by attention deficit disorder, and this shows in the way I have flitted from topic to topic within logic (and perhaps within this interview). I was not so much unwilling as simply unable to follow Silver's advice to pick a big problem and stick with it. In the anthology of my philosophical papers, *Mathematics, Models, and Modality* (2008), there is an appendix giving capsule summaries of some of my papers in logic that were *not* included in the volume because they were too technical. Looking through them I find it hard to say which is my favorite. My most cited theorem is probably the main result of my dissertation, alluded to above, on the number of pieces there can be in a simply-definable partition of the continuum. I came closest to doing something in applied mathematics in the work on measurable selection theorems and perhaps derived the most satisfaction from the work on indeterminist tense logics, both already alluded to as well, though I also felt a great deal of excitement with a few of the others (on the axiomatization of various tense logics and conditional logics, and on the place of various families of sets defined in indirect ways, especially those connected with Kripke's theory of truth and some its rivals, in established complexity hierarchies).

6. When and how did your interests move from mathematical logic to the philosophy of mathematics?

JPB: I had attended a memorable seminar of Charles Chihara's at Berkeley, on Gödel's theorem and mechanism (the Lucas argument), but I did not know much about his nominalistic views until his book *Ontology and the Vicious Circle Principle* (1973) came out, which first interested me in that topic, the first straightforwardly philosophical topic on which I was to publish. Then came Field's notorious *Science without Numbers* (1980).

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Seeing what these nominalists wished to do in the way of reconstructing mathematics, and the means (different for Chihara and for Field) they were willing to allow themselves, I thought that as a trained logician I could get what they wanted using means they would allow in a more efficient way than they themselves had done. This work became the core of my contributions to my first book, written with Gideon Rosen, *A Subject with No Object* (1997). But Chihara and Field thought that by showing mathematical objects to be in principle dispensable they were showing them to be non-existent, and I was very far from believing that, and I stated why in my paper "Why I Am Not a Nominalist" (1983) where I insisted on the distinction between *hermeneutic* and *revolutionary* nominalism, rejecting both. Then I gradually got drawn into other philosophical disputes.

7. We'll return later to nominalism. Let me first ask you what is, in your opinion, the relationship between logic and mathematics?

JPB: Well, this is a subject the two of us have discussed quite a bit in the past, and doubtless will be discussing more in the future, so you can appreciate that any short answer is going to be seriously incomplete. I see rigorous mathematics as committed to claiming nothing as a theorem that is not logically implied by postulates acknowledged in advance, today usually those of Zermelo-Fraenkel set theory (ZFC), and to supporting claims of theoremhood by presenting proofs establishing that logical implication does hold between postulates and purported theorem. Logic analyzes just what can be meant by "logically implies", and its most basic result, the Gödel completeness theorem, shows that whenever logical implication holds between premises and conclusion a route can in principle be traced from the former to the latter consisting of short, *obvious* logical implications from one intermediate step to another. By means of this analysis a great deal can be established about what can or cannot be established as a theorem assuming various postulates, and the controversies about what is the right way to do mathematics that involved mathematicians in the twenties and thirties, the so-called *Grundlagenstreit*, are replaced by the delineation of a series of stronger and stronger axiom systems starting with very weak fragments of arithmetic (to the exploration of which I contributed a bit in my book on Frege) and ending with ZFC plus powerful large cardinal

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axioms (where I got involved just a bit through measurable selection theorems). The risk of inconsistency increases as one goes up the scale, but also the power to prove theorems, even of elementary number theory. The “reverse mathematics” of Stephen Simpson and Harvey Friedman has established in many cases *exactly* how far up the scale one has to go to get this or that famous theorem of core mathematics, and for me that is the core of mathematical logic.

8. You have been a militant defender of classical logic. Can you briefly explain your motivation?

JPB: In my more careful statements, I have been most concerned to defend classical logic *descriptively*, as a delineation of the norms of classical mathematics. Of course, since I also accept classical mathematics, I am then committed to accepting classical logic *normatively* as well, so far as mathematical theorem-proving is concerned. Outside mathematics there is room for extra-classical logics dealing with tense and mood and the like that play no role in mathematics, and *perhaps* for anti-classical logics that drop some classical laws when one extends the range of premises and conclusions whose relations one is evaluating to take in extra-mathematical subject matter where, say, vagueness is involved. (Though in fact most reasoning with vague concepts can safely ignore their vagueness. For instance, color concepts have vague boundaries, but if one is sorting items of some kind by color, say bricks or tiles, very often none of the actual cases before one are borderline instances.) I have also done some work on intuitionistic mathematics, where a different logic is involved, that one can learn to think in terms of in limited contexts, though I have never had the slightest sympathy for the intuitionist critique of classical mathematics. I recall that in my graduate school qualifying exams I was asked among other questions to write on the clash between David Hilbert, who wrote that depriving the mathematician of the law of excluded middle, as the intuitionists proposed to do, is like depriving a boxer of the use of his fists, and Georg Kreisel, who wrote that, in view of the intended meaning of the intuitionistic logical operators, it was more like depriving non-commutative algebra of the law AB = BA. I answered that what Brouwer, the founder of intuitionism, proposed to do was more like banning the study of

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commutative algebra. This may be right or wrong, but shows where my sympathies lie.

9. You have looked with suspicion at certain heresies that developed in Australia; can you say something about it?

JPB: Actually, the heresy in question, relevance or relevant logic — everything about it is controversial, including what it should be named, and perhaps we should say, following a recent fashion, "relevanx logic", with the "x" representing the alternative for "ce" or "t" — originated at Yale (Alan Ross Anderson and Nuel Belnap), was transmitted to Pittsburgh, then spread in an especially virulent variant to Australia, brought in by immigrants from the U.S. (Robert Meyer) and New Zealand (Richard Sylvan née Routley). Relevanx logicians pretend to reject the inference from A-or-B and not-A to B as "a simple inferential mistake such as only a dog would make" (an allusion by Anderson and Belnap to something Sextus says about the Stoics claiming even dogs reason by this form of argument). And yet, when they prove metatheorems about their own system, they make use of this very form of argument. And when Saul Kripke first pointed this out, no two relevanx logicians gave the same account of or excuse for the practice. The lapse was inevitable, since reasoning according to the forbidden "Disjunctive Syllogism" is ubiquitous and indispensable in mathematics. One can say of relevanx logics what Solomon Feferman said of truth-value-gap logics, that "nothing like sustained ordinary reasoning can be carried on" in these logics. This is in sharp contrast with intuitionistic logic, where the same logic is adhered to in object language and metalanguage alike. But this is not to say that it is impossible to come up with some kind of unintended interpretation in which relevanx logic makes some kind of sense, though so far as I know this has only ever been done for certain fragments, never the whole system.

10. You worked extensively on issues related to the ontology of mathematics. It is customary to divide philosophers of mathematics between Platonists and nominalists. But your position seems to defy this partition. Could you *briefly explain why you think that it is possible to be at the same time anti-Platonists and anti-nominalists?*

JPB: I see Platonism, in any historically serious sense of the label, as maintaining that when one gets behind all merely human representations to ultimate reality, one will find abstract objects there; by contrast, nominalism says that, when one gets behind all merely human representations to ultimate reality, one will find none. My position is that getting behind all merely human representations is not a feasible ambition, and probably not even an intelligible one. You yourself, surely, as a student of Kant, not to mention Nietzsche, will recognize that renouncing this sort of ambition to transcend the human condition is hardly a novelty; I picked it up from my reading of Quine.

11. Is your position in the philosophy of mathematics a metaphysical position that can be generalized to other domains?

JPB: That's two questions, calling for two answers. On the one hand, no, is not a metaphysical position, but an *anti*-metaphysical position. But on the other hand, yes, I do hold to it across the board. I view the whole notion of an ultimate reality composed of objects (abstract or concrete) with properties — the presupposition of ontological debates about which kinds of objects with which kinds of properties are ultimately real — as merely a projection onto the universe of the argument-predicate grammar of human languages, a structure that might not be shared by intelligent extraterrestrials, who would be no worse off for not sharing it. You have probably heard me quote Lichtenberg's dictum that skepticism about one thing is usually the result of blind faith in something else, and I diagnose the fashion for nominalism as a typical case, where a far too naive conception of what is going on in the case of our knowledge of concrete objects — relying on what Russell once aptly called the "stone-age metaphysics" of cause and effect — results in doubts about the possibility of knowledge of abstract objects.

12. In the last two decades, the philosophy of mathematical practice has been rapidly growing. This is a heterogenous trend in the philosophy of

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mathematics that strives to consider the human aspect of mathematics. In this trend, interdisciplinary works are common: philosophy and mainstream mathematics are brought into dialogue, and other disciplines are involved as well. Among these are cognitive science, mathematical education, sociology, history of mathematics, and even ethnomathematics. What is your take on it?

JPB: I see philosophy of mathematics as having gone through alternating periods of looking outward to the place of mathematics among other intellectual enterprises, and inward, to the conduct of mathematical inquiry itself. Needless to say, all such schematic pictures oversimplify, but I will go ahead and expound mine nonetheless. First, from Descartes to Kant the question is how mathematics can achieve what it seems to do, arrive at substantive conclusions about the world by pure thought, and whether physics or metaphysics could achieve reliable results proceeding in the same way. Second, while philosophical discussions during this early period more or less assumed mathematics was being conducted as a rigorous deductive science, as a matter of historical fact it was not, and when in the nineteenth century the systematic attempt at rigorizing mathematics was undertaken in a serious way, with the monumental achievements of Weierstrass and Dedekind, among others, it emerged that there were important issues about how to proceed over which eminent mathematicians, Cantor and Kronecker, Poincaré and Hilbert, radically disagreed. Such disagreement among the specialists is a standing invitation to external philosophical observers to get involved, and they did, to the point that with a figure like Frege one can hardly ask, any more than with Leibniz, whether he was more mathematician than philosopher or more philosopher than mathematician. With the end of the *Grundlagenstreit*, philosophers turned back to looking at the similarities and differences between mathematics and other sciences, this time around focusing not on the distinction between *a priori* and *a posteriori* methods, but between abstract and concrete objects; and it was into this phase of debate that I got drawn in myself. I see "philosophy of mathematical practice" as turning back inward, but this time around for the most part without the kind of invitation presented by the occurrence of serious disagreement among mathematicians. I have been skeptical of some of this work, especially attempts to distinguish "explanatory" from "non-explanatory" mathematical theorems and proofs; philosophy of physics

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expended a good deal of effort without much result in decades-long debates over the nature of "scientific explanation", and I have sometimes feared philosophy of mathematics was tending in the same unfortunate direction.

13. You have been critical of some of the precursors of this trend, instantiated by the works of Hersh and Tymoczko, for example. Moreover, in your 2015 book, Rigor and Structure*, you seemed to be a bit dismissive of these new approaches. Did your views change?*

JPB: Well, Reuben Hersh — with whom I once held a public debate — is very nice in person, and a professional mathematician with a sincere amateur interest in philosophy, but he has unfortunately been drawn, as many amateur philosophers have been in the past, to the notion that mathematical objects are some kind of mental entities, "shared ideas and concepts". We ought to know from Frege such a view is wrong-headed and going nowhere. And speaking of being "dismissive", Hersh and Tom Tymoczko — who by the way was, like Shapiro and myself, a product of the Ross program, a counselor there when I was a student — and a number of others also have seemed to me to fail to see the value of mathematical logic, rather like the old Oxford ordinary language philosophers of the fifties. My strongest negative reaction, however, though I have not published on the topic, has been to the work of Ken Manders (2008), which I think just gets the history of Greek geometry completely wrong, and the fact that so many in the "philosophy of mathematical practice" movement celebrated this work made me question their philosophical judgment. A lot of this, though, is perhaps ultimately more a difference of temperament expressing itself in differences of doctrine.

As for my book (Burgess 2015), I involved myself with the issue of rigor only as a preliminary to the issue of "structuralism" in mathematical ontology, which in my mind had always been simmering "on the back burner" while the debate over nominalism was boiling over on the front burner. I first encountered it in my undergraduate study of philosophy of mathematics through Benacerraf's classic "What Numbers Could Not Be" (1965). Shapiro, too, along with Charles Parsons, and others whom I hold in highest esteem, has written extensively on the issue. I rightly or wrongly thought I had a new take on it, but I won't enlarge on that here. What happened when I set out or sat down to produce an exposition of my view was that the preliminary discussion of rigor grew and grew until it came to occupy at least half the book, as I was forced to confront the following kind of question: Mathematics of the last hundred years or so generally successfully attempts to produce proofs that guarantee that results put forward as theorems rather than conjectures logically follow from accepted postulates. If the theorems do thus follow from the postulates, a fully formalized derivation in a symbolic calculus can in principle be produced. But in the era before automated proof-assistants none ever were produced for any non-trivial result. So what does "rigor", the feature of proofs that provides the guarantee of logical consequence, really consist in? A simple answer — found in various forms in various writers, the *locus classicus* being perhaps the discussion of the late Mark Steiner (1975) — suggests that it consists in producing (the ordinary language counterparts of) enough steps of a formal derivation that filling in the rest would be, for a trained expert, a very tedious but merely routine task. I saw, however, that the appearance of diagrams in some proofs, which cannot in any direct way be thought of as counterparts of linear symbolic formulas, raises a difficulty for this view; and when I came to this issue in my book, I simply called a halt to my discussion of rigor, rightly or wrongly believing I had enough down to serve as background for my treatment of structuralism. As for changes in my view, I admit that, because I had had such a negative reaction to Manders' discussion of diagrams in proofs, I was slow to see that others, including especially my present interviewer, had much more convincing examples. This question of the role of diagrams I now find one area of "philosophy of mathematical practice" in which I can take a real sympathetic interest. Perhaps others will follow.

14. How do you think the philosophy of mathematics will develop in the following years?

JPB: I have no idea. I quote Hermann Weyl in my book as saying that the historical decisions made by mathematicians as to the direction in which to pursue their subject "defy complete objective rationalization", and this is even more true of philosophy, I would say, including philosophy of mathematics.

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15. Let us turn to other philosophical topics. In your early days, you wrote a paper with the provocative title: "Against Ethics." *What was that about?*

JPB: The short answer is: Re-inventing the wheel. The paper argues for the kind of "error theory" in metaethics that (unknown to me at the time) was enunciated way back in the year of my birth by J. L. Mackie, and elaborated by him in a book-length work published about the same time I finished my paper (though my paper was not published until much, much later, by which time I had changed my view, and even lectured on the new version, though I have never written it up and probably now never will). I was most concerned to oppose theories such as emotivism and prescriptivism according to which moral judgments are *not even intended* as descriptions of objective facts. The one methodological principle I advocated that still seems to me correct and underexploited is that when a definition is widely recognized as being a joke, it is probably not a correct analysis of the notion it purports to define. Notably, the *Devil's Dictionary* of Ambrose Bierce, a turn-of-the-century American writer of Civil War tales and horror stories, offers the following definition: "MORAL, adjective. Conforming to the local and mutable standard of right." I argued against Harman's version of moral relativism that if it were true, this definition wouldn't be found in a book of humor.

16. You are currently working on a new project in philosophy of mind. Can you tell us something about it?

JPB: Not much at this stage. The year I was in Wisconsin I read a good deal of philosophy, especially after I learned I would be heading for Princeton, but the one work I spent the most time over was Kripke's (1980) *Naming and Necessity*. It took me years of subsequent reflection to get straight the material on naming, and on necessity, before I could think seriously about the defense of dualism in which the book culminates. The thesis of my own work in progress is that the principle of the supervenience of the mental on the physical, which amounts to the direct contradiction of Kripke's view, is wrong, but more importantly is unimportant, or more precisely, dependent for its appearance of importance on the thought that it is possible to get behind all merely human representations to an ultimate

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reality. That's cryptic, but it will have to do until I get free of other obligations and have time to get back to work on the book.

17. What are your interests in other areas of philosophy?

JPB: They are quite a few, but not ones I have had much time to pursue in a serious way. One sample: I have been much taken with some issues in philosophy of physics, especially concerning the direction of time, and the status of thermodynamics, which is in one sense a fundamental theory, but in another sense not at all like general relativity or quantum field theory. Also, I just once, when Bas van Fraassen was still at Princeton but on leave for a year, taught a graduate seminar on what every philosopher should know about physics, in which, having Bas's skepticism about atomism in mind, I began with Einstein's 1905 paper deriving an expression for Avogadro's number in terms of quantities in principle macroscopically measurable. (It was Jean Perrin who made the measurements.) The philosophy of *applied* mathematics is so woefully underdeveloped (despite Steiner's eloquent call for more work in that area and the contributions of Mark Wilson) that I don't think it can even begin to deal with such a derivation.

Another sample: I have also been concerned with semantics, or rather, with "semantics", and the ambiguity in that term, which began with work of Michel Bréal (inventor of the Olympic marathon race) as a theory of meaning and was somehow turned under the influence of Tarski into a theory of models, leading in my opinion to disastrous confusion or conflation of two important subjects. But in my attempts to trace the history of the migration of the sense of the term "semantics," I soon found that to pursue the question I would have to learn Polish, and then gave it up. I have also had a life-long interest in "pataphysics," but I have said enough.

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