Shut up and let me think!
Or why you should work on the foundations of quantum mechanics as much as you please

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May 11, 2014

Abstract

If you have a restless intellect, it is very likely that you have played at some point with the idea of investigating the meaning and conceptual foundations of quantum mechanics. It is also probable (albeit not certain) that your intentions have been stopped on their tracks by an encounter with some version of the “Shut up and calculate!” command. You may have heard that everything is already understood. That understanding is not your job. Or, if it is, it is either impossible or very difficult. Maybe somebody explained to you that physics is concerned with “hows” and not with “whys”; that whys are the business of “philosophy” —you know, that dirty word. That what you call “understanding” is just being Newtonian; which of course you cannot ask quantum mechanics to be. Perhaps they also complemented these useful advices with some norms: The important thing a theory must do is predict; a theory must only talk about measurable quantities. It may also be the case that you almost asked “OK, and why is that?”, but you finally bit your tongue. If you persisted in your intentions and the debate got a little heated up, it is even possible that it was suggested that you suffered of some type of moral or epistemic weakness that tend to disappear as you grow up. Maybe you received some job advice such as “Don’t work in that if you ever want to own a house”.

I have certainly met all these objections in my short career, and I think that they are all just wrong. In this somewhat personal document, I try to defend that making sense of quantum mechanics is an exciting, challenging, important and open scientific endeavor. I do this by compulsively quoting Feynman (and others), and I provide some arguments that you might want to use the next time you confront the mentioned “opinions”. By analogy with the anti-rationalistic Copenhagen command, all the arguments are subsumed in a standard answer to it: “Shut up and let me think!”

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1 On a personal note

Science alone of all the subjects contains within itself the lesson of the danger of belief in the infallibility of the greatest teachers in the preceding generation... Learn from science that you must doubt the experts. As a matter of fact, I can also define science another way: Science is the belief in the ignorance of experts. — Feynman (1968)

I have recently decided that I will spend some time and effort to try to come up with a consistent and satisfying account of how QM (quantum mechanics) works and what QM means. That is, consistent and satisfying to me.

My decision is based on a fact and a conviction: The first is that I enjoy the topic immensely, the second is that I believe that a conceptual tidying up is an urgent necessity in the field. Both circumstances combined make my decision an easy bargain.

Although many scientists and philosophers certainly agree with me on this, there is a strong and steady current of thought, and statements, that sees such a tidying-up enterprise as futile and entirely irrelevant. Everything is perfectly tidy for them. Their viewpoint (slightly caricaturized or not so much, depending on who we are talking about) is that non-relativistic QM has been completely understood an unspecified number of decades ago, that the so-called “measurement problem” is no problem at all, and that all the discussions about the different “interpretations” of QM are a distraction at best, a waste of the time and effort of otherwise reasonable individuals at worst. What you need to do about QM, they say, is just learn it from some undergraduate-level textbook — which one, it doesn’t matter, since the topic has been carved in stone long ago and now it is just an exceedingly straightforward application of linear algebra. Then you apply it to whatever practical application gets you the most funding, money, h-index or just the fastest path to tenure if you haven’t got it yet, and you are done. If you do not care about money, stability or prestige, you can choose the topic to which you want to apply the finished theory known as QM on the basis of the common interest of your department, of the scientific community, of humankind, or of the whole biosphere, whichever group you consider to be the worthy beneficiary of your efforts.

What is also remarkable is that the advocates of this position not only decline to work in such an irrelevant enterprise as the foundations of QM themselves (something that falls entirely within the bounds of their scientific and personal freedom), but they very frequently add (and they do that with vigor) that you shouldn’t waste your time in that either... or something to that effect. Maybe this intensity of purpose is what has caused the whole viewpoint to be sometimes dubbed “Shut up and calculate!”; a motto which has been attributed to Feynman but that seems to have been actually coined by Mermin (2004) (even if he is not sure about it).\footnote{In fact, although Feynman maybe didn’t say it, he was indeed close. When he is discussing the measurement rules for the double-slit experiment in his famous Lectures, he writes (Feynman, 1963a, p. 10-1): “So at the present time we must limit ourselves to computing probabilities.”}

The proponents of the shut-up-and-calculate approach to the understanding of QM often perform this pastoral work (of trying to save as many lost souls as possible from the bleak fate of irrelevance) wearing the dignified robes of pragmatism, commitment to progress, clarity, and orthodoxy. Also, among their ranks, we can find celebrated scientists and deep thinkers that have achieved significant advances in so many fields, which makes their claims even more appealing.

I don’t know what the experience of the reader has been, but I have met this friction, this headwind, this resistance, frequently in my (short) career. When you start studying,
you are young (typically), everything is new (by definition), and every great scientist you read and meet is more a semi-god than a human to you. Hence, if you happen to encounter the shut-up-and-calculate command more often than the opposite in your early days as a proto-scientist, you might buy it and stop asking questions about the foundations of QM... to yourself and to others as well. I know because I bought it myself.

Fortunately, I have also been gifted (or cursed!) with an innate and relentless curiosity and a lack of respect for authority figures. Hence, along the years, I have read more, I have grown a healthy distrust, and I have come to the conclusion that the shut-up-and-calculate advice is a very bad one. I think that it is just obvious that QM is a conceptual mess at the moment, with many respectable and clever scientists talking past each other, and I believe that acting as if the problem doesn’t exist can only lead to stagnation and to a delay in scientific progress. It is OK to close a topic if it has been completely (or mostly) understood, but it is dangerous and potentially tragic to close it too soon. I think that recommending young scientists (or not-so-young ones) to shut up and calculate is not pragmatic, does not help progress, does not foster clarity (it rather fosters quite the opposite), and it is indeed not the orthodoxy.

As I said, many voices agree with me on this, so I will not tell anything new here. My objective is not novelty but to join my voice to the choir that advocates a deep investigation into the foundations and conceptual structure of QM. In this way, I harbor the hope that the chances of you meeting a voice of this second group (specially at the beginnings of your career) are slightly increased by the existence of this document. Additionally, I will use the opportunity to collect some interesting views on the controversy, and to put my own thoughts in order.

Since I need a great deal of silence to concentrate (specially when I am dealing with complicated topics) I have chosen to entitle these notes “Shut up and let me think!” I think that this exclamation captures the situation as well as my feelings about it quite accurately.\textsuperscript{2}

2 Shut up and let me think!

The position that I introduced in the previous section that defends the lack of a necessity to investigate on the foundations of QM takes (of course) different forms, it comes from different angles and it is stated with different degrees of explicitness and intensity, depending on the personality and beliefs of the person that expresses it. In this section, I will collect some quotations\textsuperscript{3} and I will try to group them according to a number of basic statements that are recurrent in the literature, in conference talks, in corridor conversations and in the minds of our fellow scientists.

Since I think that all these arguments are (at least partially) wrong, I will also provide my personal answers to each of them.

\textsuperscript{2} I was delighted to find that very recently a similarly positive motto was coined by Hardy and Spekkens (2010) to counter the negative anti-rationalistic “Shut up and calculate!” They are more polite than me, and they chose “Shut up and contemplate!”.

\textsuperscript{3} When you quote somebody, it is easy to read something which is not actually said there, to misrepresent, to forget the context, etc. I have tried not to make these mistakes, but given the large number of quotations that I will mention, the odds are that I will be putting the wrong words in somebody’s mouth. For those researchers that are still around, I would love to know if I have misquoted you, and I will promptly include the appropriate addenda to this document if you show me my error. For those that are no longer among us, such as Prof. Feynman, I apologize in advance and I will also accept suggestions from anyone that knows their actual thoughts better than I do.
I will begin some of the sections with a quote not because I want you to see that a very important guy (or gal) agrees with me—remember my lack of respect in authority—but because I think that they have (approximately) said what I think (or the opposite) with a style that is typically much better than mine.

2.1 QM is completely understood

What is the first business of one who practices philosophy? To get rid of self-conceit. For it is impossible for anyone to begin to learn that which he thinks he already knows. — Epictetus, Discourses, 101 AD

In this workshop we are venturing into a smoky area of science where nobody knows what the real truth is. Such fields are always dominated by the compensation phenomenon: supreme self-confidence takes the place of rational arguments. — Jaynes (1990)

This argument denies the premise by basically stating that QM is now completely understood, and it has probably been so since a long time ago. Nothing therefore needs to be done, and all the talk about the “interpretations”, the “measurement problem”, etc. is not physics but philosophy. The proponents of this view enrich their argument with reasons and sub-arguments supporting it, but I will deal with them later. In this section I will mainly concentrate on the core assertion that everything about the meaning and interpretation of QM has been solved already, and I will respond to that.

The first great scientist that I want to mention that suggests everything is understood in QM is Richard P. Feynman. In the very first page of the 3rd volume of his awesome Lectures (Feynman, 1963a), which is dedicated to —what else— QM, he writes:

The gradual accumulation of information about atomic and small-scale behavior during the first quarter of this century, which gave some indications about how small things do behave, produced an increasing confusion which was finally resolved in 1926 and 1927 by Schrödinger, Heisenberg, and Born. They finally obtained a consistent description of the behavior of matter on a small scale.

The same paragraph appears in p. 116 of his “Six easy pieces”, and similarly in (Feynman, 1965, p. 128) he writes:

Electrons, when they were first discovered, behaved exactly like particles or bullets, very simply. Further research showed, from electron diffraction experiments for example, that they behaved like waves. As time went on there was a growing confusion about how these things really behaved —waves or particles, particles or waves? Everything looked like both. This growing confusion was resolved in 1925 or 1926 with the advent of the correct equations for quantum mechanics.

The funny thing is that, although Feynman seemed to think that the confusion was “resolved” with a “consistent description” and the “correct equations” for QM such a long time ago as in 1926-1927, he is the same person that famously wrote (in the next page to the second quote above!):

I think I can safely say that nobody understands quantum mechanics.
Also, in (Feynman, 1982), we can read:

We always have had a great deal of difficulty in understanding the world view that quantum mechanics represents. At least I do, because I’m an old enough man that I haven’t got to the point that this stuff is obvious to me. Okay, I still get nervous with it. And therefore, some of the younger students...you know how it always is, every new idea, it takes a generation or two until it becomes obvious that there’s no real problem. It has not yet become obvious to me that there’s no real problem. I cannot define the real problem, therefore I suspect there’s no real problem, but I’m not sure there’s no real problem.

So it seems that in Feynman’s mind the fact that all controversies have been solved coexisted with a certain, undefined feeling that something was maybe amiss. In my opinion, as every other human being, Feynman was perfectly capable to hold contradictory thoughts in his mind at the same time, and nevertheless display a functional and fruitful behavior. So probably at some level he thought that QM presented no problems (hence the tone of the first two quotes), but in other level he knew that something was not quite right about it. Since he was mostly a pragmatist, he listened more to the first voice than to the second and just went on to produce some of the grandest achievements in modern physics, leaving that unresolved uneasiness in the back of his mind to be dealt with later—or never. Being an extraordinarily honest thinker as he was, he did not pretend that the uneasiness did not exist. He just preferred to advance without looking too much at it, or maybe he found more interesting problems to look at than this one. However, it seems clear that the possibility that everything relevant about the foundations of QM was not in fact understood yet did have a place in his thoughts, as it also floated in many conversations I have had with theoretical physicists, and as the reader has also probably found in her travels.

A much stronger advocate of the no-problem-whatsoever view is another great and respected scientist who is responsible of many important advances in statistical mechanics: Nico van Kampen. In his letter entitled “The scandal of quantum mechanics” (van Kampen, 2008), he attacks a paper by Nikolić (2008) about Bohmian mechanics—one of the strongest proposals in my opinion to solve the conceptual problems of “orthodox” shut-up-and-calculate QM—, and he writes paragraphs such as:

The article by Nikolić with its catchy title is a reminder of the scandalous fact that eighty years after the development of quantum mechanics the literature is still swamped by voluminous discussions about what is called its “interpretation”. Actually quantum mechanics provides a complete and adequate description of the observed physical phenomena on the atomic scale. What else can one wish?

Slightly after that, he proceeds to provide a two-paragraphs summary of what he sees as the complete solution to the measurement problem:

The solution of the measurement problem is twofold. First, any observation or measurement requires a macroscopic measuring apparatus. A macroscopic object is also governed by quantum mechanics, but has a large number of constituents, so that each macroscopic state is a combination of an enormous number of quantum mechanical eigenstates. As a consequence the quantum mechanical interference terms between two macroscopic states virtually cancel and only probabilities survive. That is the explanation why our familiar macroscopic physics, concerned with billiard balls, deals with probabilities rather than probability amplitudes.
Second, in order that a macroscopic apparatus can be influenced by the presence of a microscopic event it has to be prepared in a metastable initial state—think of the Wilson camera and the Geiger counter. The microscopic event triggers a macroscopically visible transition into the stable state. Of course this is irreversible and is accompanied by a thermodynamic increase of entropy.

And he concludes:

This is the physics as determined by quantum mechanics. The scandal is that there are still many articles, discussions, and textbooks, which advertise various interpretations and philosophical profundities.

I tend to agree with van Kampen that the measurement problem is the cornerstone of the conceptual issues that plague the “orthodox” accounts of QM, and I also like his proposal very much. After giving a lot of thought to it, and after comparing it to the most promising alternatives, I might even decide that it is the “right answer” as far as I am concerned. However I fail to see how the existence of his proposal is enough to dismiss the vast literature that explores these topics—and that works on the basis that there is in fact a problem—as “philosophical profundities”.

A specially poignant example of the literature I refer to is the excellent work by Allahverdyan et al. (2013), which, to my understanding, puts van Kampen’s two-paragraph summary of the solution to the measurement problem on solid and concrete mathematical grounds (and which I also found very interesting). There is a funny combination of facts about this very thorough and insightful report:

- It was published online on November 14, 2012—that is, some years after the publication of the mentioned critique by van Kampen (2008).
- van Kampen (2008) cites no similar in-depth study to support his schematic account. He just treats it as an evident truth.
- The article by Allahverdyan et al. (2013) is 166-pages long—somewhat long for an evident truth.
- The authors dedicate their work to “our teachers and inspirers Nico G. van Kampen and Albert Messiah”.
- Far from dismissing all the literature concerning so-called “interpretations” of QM as “advertisements of philosophical profundities”, they write sentences such as the following:

  In spite of a century of progress and success, quantum mechanics still gives rise to passionate discussions about its interpretation. Understanding quantum measurements is an important issue in this respect, since measurements are a privileged means to grasp the microscopic physical quantities.

The problem was thus formulated as a mathematical contradiction: the Schrödinger equation and the projection postulate of von Neumann are incompatible. Since then, many theorists have worked out models of quantum measurements,
with the aim of understanding not merely the dynamics of such processes, but in particular solving the so-called measurement problem. This problem is raised by a conceptual contrast between quantum theory, which is irreducibly probabilistic, and our macroscopic experience, in which an individual process results in a well defined outcome.

[...] The challenge has remained to fully explain how this property emerges, ideally without introducing new ingredients, that is, from the mere laws of quantum mechanics alone. Many authors have tackled this deep problem of measurements with the help of models so as to get insight on the interpretation of quantum mechanics.

[...] Few textbooks of quantum mechanics dwell upon questions of interpretation or upon quantum measurements, in spite of their importance in the comprehension of the theory. Generations of students have therefore stumbled over the problem of measurement, before leaving it aside when they pursued research work. Most physicists have never returned to it, considering that it is not worth spending time on a problem which “probably cannot be solved” and which has in practice little implication on physical predictions. Such a fatalistic attitude has emerged after the efforts of the brightest physicists, including Einstein, Bohr, de Broglie, von Neumann and Wigner, failed to lead to a universally accepted solution or even viewpoint. [...] However, the measurement problem has never been forgotten, owing to its intimate connection with the foundations of quantum mechanics, which it may help to formulate more sharply, and owing to its philosophical implications.

Also, you can find a great paragraph full of references in their page 6 where almost all “philosophical profundities” in the literature are considered to be worth citing... along with the names of those who dared to work in this apparently non-existent problem.

One wonders how the words by van Kampen (2008) can be compatible with the enormous effort done by his disciples to clarify the whole issue. If everything was already solved by 2008, then what is the point of writing a very detailed, 166-page solution in 2012?

The question is of course rhetorical. The answer is that nothing was solved by 2008, and it is probably not solved by now either. The many interesting works in the literature about “interpretations” of QM and the measurement problem are not “advertisements of philosophical profundities” but serious attempts to make sense of a very deep conceptual problem, and the many great scientists that have devoted time and effort to think about this are not delusional and they have not forgotten their pragmatism at home. One might wish that there is no problem with QM, but anyone that has her eyes open sees that in fact there is — the belief that everything is OK is just wishful thinking.

Of course, maybe Bohmian mechanics is not the solution and maybe van Kampen is right about that. But even if that were the case it doesn’t mean that there is nothing to be solved to begin with. Maybe van Kampen’s proposal is not the solution for that matter either.

The excellent work by Allahverdyan et al. (2013) is not the only one we can mention that explores this very interesting issue. In fact, tens of them are published in the Quantum Physics section of the arXiv every month, according to my experience [see, for very recent examples, the pre-print (Hobson, 2013), and an answer to it (Kastner, 2013) in the same week]. It would be preposterous to make an exhaustive list here, so let me quasi-randomly cite some very recent works by well-known scientists, most of them in very renowned journals:
(Bassi et al., 2013, Colbeck and Renner, 2012a,b, Fröhlich and Schubnel, 2012, Kochen, 2013, Lewis et al., 2012, Pfister and Wehner, 2013, Price and Wharton, 2013, Pusey et al., 2012, Saunders et al., 2010, Streltsov and Zurek, 2013). These articles contain many questions that are still partially unanswered as well as the heroic attempts of their authors to solve them. I will not discuss the open issues here (I merely want to make clear that there are in fact many open issues), but I recommend any reader interested in—and puzzled by!—the foundations of QM to enjoy a deep dive in any of these works to begin mapping the landscape.

Another interesting reading that could help gain some perspective about the lack of consensus in the field is the poll conducted by Schlosshauer et al. (2013) to 33 participants (27 of which stated their main academic affiliation as physics, 5 as philosophy, and 3 as mathematics) in the conference “Quantum physics and the nature of reality”, held in July 2011 at the International Academy Traunkirchen, Austria. The authors asked the conference participants 16 multiple-choice questions covering the main issues and open problems in the foundations of QM. Multiple answers per question were allowed to be checked because they were often non-exclusive. The results of the poll are illuminating, and the authors conclude that:

Quantum theory is based on a clear mathematical apparatus, has enormous significance for the natural sciences, enjoys phenomenal predictive success, and plays a critical role in modern technological developments. Yet, nearly 90 years after the theory’s development, there is still no consensus in the scientific community regarding the interpretation of the theory’s foundational building blocks. Our poll is an urgent reminder of this peculiar situation.

All the asked questions are very interesting, but I have taken the liberty to select some of my favorite ones in fig. 2.1. (I especially love the one related to changing your mind about which is the correct interpretation!)

Of course, scientific issues are not decided democratically, and moreover I have already expressed my lack of respect for authority—which I won’t withdraw. Nevertheless, it seems to me that an Ockham’s razor type argument leads to conclude that it is more likely that unsolved issues remain and those few who deny it are victims of the “compensation phenomenon” mentioned in Jaynes’ quote at the beginning of this section, than it is that tens—hundreds? thousands?—of apparently clever and rational scientists of different countries, of different ages, some of which don’t even know each other, are all investigating a no-problem.

Since this kind of plausibility arguments are never definitive, I must admit that it is indeed possible that van Kampen’s view is correct and all the researchers that are pursuing alternative lines just don’t know it yet. Still, it seems that this view has been carefully formalized only in 2012 (Allahverdyan et al., 2013) —so it would appear that we need to grant a little more time to the unconverted if we want to be fair. Even more! The same argument can be used to admit the possibility that the many solid scientists that claim that, say, Bohmian mechanics (Dürr and Teufel, 2009) is the ultimate solution to all of QM’s conceptual problems be right and all those that don’t see it this way are just temporarily wrong. Or the many worlds interpretation (Saunders et al., 2010, Wallace, 2012), or the consistent histories approach (Griffiths, 2003), or quantum Bayesianism (Fuchs and Peres, 2000), or collapse (also “dynamical reduction”) models (Bassi and Ghirardi, 2003), or . . .

I think that the situation can be appropriately grasped if we admit to define the sentences “there is a problem in a theory” or “theory X is understood” as referring not so much to
Figure 2.1: Some results of the poll about foundational attitudes toward QM conducted by Schlosshauer et al. (2013) as described in the text. Used with kind permission by the authors.

Thus, a theory will have a problem when a significant mass of intelligent scientists feels it has one, and a theory will be completely understood when the vast majority of the scientists feels it is so. I think I have proved that QM does have problems and it is not completely understood according to this point of view. In sec. 2.2.5, I will show that there exist theories, such as special relativity, which have no problems and are completely understood in this sense — just QM is not one of them.

Before closing this section, let me quote the words in (Streater, 2007, p. 87), who traces all the alleged wasted effort to a failure in understanding that the “EPR paradox” is no “paradox” at all, and mostly agrees with van Kampen on the no-problem whatsoever diagnostic (even if I am not completely sure that both propose the same solution):

The quantum paradox community is, even after all these years and all the rebuttals, still on the increase. In has spawned larger enterprises, such as Bohmian mechanics, Nelson dynamics, geometro-stochastic dynamics, quantum-state reduction, and the quantum
theories of the brain of Penrose and of Stapp. These are lost causes too, and fall down with the rebuttal of the spooky action at a distance claimed to inspire them.

Again, we have to choose between believing that Streater knows the solution but so many researchers don’t, or believing that no complete solution has been provided yet and it is Streater who doesn’t realize that this is so.

To me, the clear lack of consensus expressed in the following final quotes and epitomized in the poll I mentioned before is a clear sign that the second option is much more likely than the first:

We do not claim to offer any genuinely new or original thoughts on quantum mechanics. However, we have made the experience that there is still a fair amount of confusion about the deeper meaning of this theory –even among professional physicists. (Fröhlich and Schubnel, 2012)

While its applications have made quantum theory arguably the most successful theory in physics, its interpretation continues to be the subject of lively debate within the community of physicists and philosophers concerned with conceptual foundations. (Healey, 2012)

Almost a century after the mathematical formulation of quantum mechanics, there is still no consensus on the interpretation of the theory. (Kochen, 2013)

Even some crazy scientists, such as (Carroll, 2010, chap. 11), are brave enough to call the whole business of interpreting QM “respectable”! Do not take my word for it. Read, read:

There is a respectable field of intellectual endeavor, occupying the time of a substantial number of talented scientists and philosophers, that goes under the name of “interpretations of quantum mechanics”. A century ago, there was no such field as “interpretations of classical mechanics” —classical mechanics is perfectly straightforward to interpret. We’re still not sure what is the best way to think and talk about quantum mechanics. [my emphasis]

Despite these encouraging words, the paper by Englert (2013), which was published in the arXiv curiously the same day as the first version of this manuscript, is the living proof that the “Shut up and calculate!” school is strong and thriving. We could delight in many of its paragraphs, but it actually suffices to read the unusually short abstract:

Quantum theory is a well-defined local theory with a clear interpretation. No “measurement problem” or any other foundational matters are waiting to be settled.

2.1.1 QM is completely understood by omission

Since Bohmian mechanics is so simple and straightforward, only one criticism remains: there must be something wrong with Bohmian mechanics, otherwise it would be taught. And as a consequence, Bohmian mechanics is not taught because there must be something wrong with it, otherwise it would be taught. — Dürr and Teufel (2009)
Despite the strong opinions of van Kampen and Streater discussed in the previous section, and despite many similar public views of other scientists on the topic, it is my impression that the sheer force of the facts behind the lack of consensus that I think have I proved prevents most people from thinking that QM is a finished theory—or if they think it, they don’t say it. There is, however, a subtler way of suggesting the same thing: by omission.

If you write a text and you never say that QM is completely understood, but you don’t say the contrary either (or maybe you mention something about it briefly and en passant), the same viewpoint can be conveyed.

Of course, it would be inefficient to make a detailed comment about the conceptual mess that pervades the foundations of QM in every paper that uses the theory or talks about it. What one should expect, nevertheless, is that such a discussion would be provided in textbooks, lectures and other occasions intended to introduce the topic to newcomers. Failing to mention something in an account of such aim amounts to tacitly stating that the omitted point is not important to get a solid knowledge of the subject.

Since no interpretation is settled as “the right one”, the author of the introductory material has to pick one and talk about it as if it were the only game in town. The Copenhagen view, which is typically seen as the one that most closely corresponds to the “Shut up and calculate!” motto, is chosen to play this role in almost all cases. Therefore, generation after generation of students begin their careers with the implicit impression that this is how things are—as in the most conservative families, the kids are taught to shut up when they are still young and obedient. Only this time they are not told so explicitly (at least not always). Even the proponents of the shut-up-and-calculate approach realize from time to time that telling people not to think may sound a little unscientific.

I have found this omission not only in my own formation as a physicist but also in online university courses (Clark and Galitski, 2013), as well as in most of the otherwise fantastic textbooks about QM that are typically used in such courses. Let me mention only a few examples: (Cohen-Tannoudji et al., 1977a,b, Landau and Lifshitz, 1991, Messiah, 1961a,b, Newton, 2002, Sakurai, 1994, Shankar, 1994). Some other textbooks do assign importance to interpretative matters, but they present them as more or less solved according to one or another proposal of the authors’ liking (Ballentine, 1998). In yet some other textbooks, foundational issues are discussed and the different alternatives presented, but they are often included into the more “advanced topics” and not as concerns affecting the conceptual stability of the whole edifice (Auletta et al., 2009).

Let me quote (Carroll, 2010, chap. 11) again so you can see that I am not the only crazy fellow saying this:

But the history is less crucial to our present purposes than the status of the Copenhagen view as what is enshrined in textbooks as the standard picture. Every physicist learns this first, and then gets to contemplate other alternatives (or choose not to, as the case may be).

It is my content here that what is needed is to begin any introductory course (or textbook) in QM with a warning: “Be careful! The theory that I am going to describe to you next is extremely predictive when one applies its pseudo-rules to concrete experiments creatively. However, our conceptual understanding of it is still fuzzy and preliminary. How to make it solid I do not know, and it is by the way one of the most fascinating open scientific questions that you could decide to tackle once you graduate.” This is not usually done in my experience, and boy would it have been useful to me some years ago!
2.2 OK, maybe we don’t understand it, but that is not our job

What worries me about religion is that it teaches people to be satisfied with not understanding.

— Richard Dawkins, God Under The Microscope, BBC, 1996

I think I can safely say that nobody understands quantum mechanics.

— Feynman (1965)

It is not yet clear to me what “understanding” a theory really is, but it seems a sure bet that it should require that one knows how the different technical terms that appear within it are related to concrete experiments; that one knows how to apply the theory in every practical case without hesitation; that one knows how to relate the theory to other theories properly (think about statistical mechanics, classical mechanics, or relativity in the case of QM); that one feels at ease and satisfied when using the theory and not perpetually asking oneself whether or not the calculations make sense, whether or not the next analytical step that one is planning to take is valid; etc. For me, “understanding” is something that can be predicated about the network of concepts that make up the theory, as well as its embedding in the larger network that comprises the whole worldview of the individual that is trying to “understand”. If something is amiss, if the network seems unstable, this translates into a lack of security when reasoning about the theory and its consequences.

But it is not my purpose to perfectly define what “understanding” means. It suffices to me to appeal to the popular use of the word in everyday life to make my point. In this, I agree with (Feynman, 1963b, p. 8-2):

We cannot define anything precisely. If we attempt to, we get into that paralysis of thought that comes to philosophers, who sit opposite to each other, one saying to the other: “You don’t know what you are talking about!” . The second one says: “What do you mean by know? What do you mean by talking? What do you mean by you?”, and so on.

In fact, Feynman didn’t define the word and yet he used it in the quote that opens this section. The important thing is that, whatever Feynman felt that “understanding” is, he claimed that “nobody understands QM” —not even himself! On the other hand, although this seems to have produced certain restlessness in his mind according to the quotes in sec. 2.1, he didn’t think that this lack of understanding was in any way incompatible with the fact (for him it was a fact) that all the confusion regarding the behavior of matter at the microscopic level was “resolved” with a “consistent description” and the “correct equations” for QM such a long time ago as in 1926-1927 (as we have already quoted him saying). It is the second claim that I challenge. I don’t think that you can say that anything has been “resolved” or that your description is “consistent” unless you understand your theory. Some voices that agree with Feynman that QM is not yet understood circumvent the problem by saying in one way or another that understanding is unnecessary, that physics is not concerned with it, that it is not our job. It is clear from my position that I just don’t think this is true. In my opinion, every scientist needs to understand everything that is related to her field of expertise —and physicists are not the exception. I also think that the assertion that “understanding is not the job of physics” is an example of the normative mode in which people tend to enter when they feel that something is deeply wrong but they don’t want to face the problem head on. In what follows, we will meet some additional unjustified
rules that seem invented ad hoc to protect QM from inquiry, from analysis, from repair. But let me quote here what Svozil (2012) has to say about a quote by Feynman that we will meet again later:

Feynman’s advise to (young, as he seemed to have assumed that the older ones are sufficiently brainwashed anyway) physicists that, while “. . . nobody understands quantum mechanics”, to stop thinking about these issues “. . . if you can possibly avoid it. Because you will get ‘down the drain’, into a blind alley from which nobody has yet escaped. Nobody knows how it can be like that.” —Even if one grants Feynman some rhetoric benefits, the appeal to “stop thinking” is a truly remarkable advice from one of the most popular scientists of his time!

Indeed! It reminds me so much of the layers of protection again rational inquiry that religion has developed —faith, irreducible mystery, being “out-of-bounds” of science, etc. [see (Dennett, 2006) for an illuminating account of these mechanisms]— and yet it is supposed to be physics! It seems to me that, once you admit that QM is not understood —as Feynman did—, you cannot elude your responsibility to understand it by producing ad hoc rules about what can be investigated, what is out of bounds, what you can think about and what not. In words of (Carroll, 2010, chap. 11):

Most modern physicists deal with the problems of interpreting quantum mechanics through the age-old strategy of “denial”. They know how the rules operate in cases of interest, they can put quantum mechanics to work in specific circumstances and achieve amazing agreement with experiment, and they don’t want to be bothered with pesky questions about what it all means or whether the theory is perfectly well-defined.

But before I convince you —yes, I am an optimist— let us analyze some arguments related to this overarching theme that I have dubbed “understanding is not our job”. First of all, two weaker versions of the idea.

2.2.1 OK, maybe understanding is our job, but it is impossible

When a distinguished but elderly scientist states that something is possible, he is almost certainly right. When he states that something is impossible, he is very probably wrong. — The first of Arthur C. Clarke’s three laws

Philosophers’ Syndrome: mistaking a failure of the imagination for an insight into necessity. — Dennett (1991)

Feynman —yes, he again— wrote:

So at the present time we must limit ourselves to computing probabilities. We say “at the present time”, but we suspect very strongly that it is something that will be with us forever —that it is impossible to beat that puzzle— that this is the way nature really is. (Feynman, 1963a, p. 10-1)

And you probably met this objection or similar ones before: The mystery is irreducible, the puzzle is eternal, it is impossible to crack the problem, and so on and so forth. This is the central tenet of the Copenhagen interpretation, as we will see later.
I will not take too much time to discuss this anti-rationalistic argument. It is too weak an adversary. Let me just mention that nobody has ever proved such a thing about anything. So far, we humans have been able to slowly but steadily understand more and more of nature’s secrets, and it doesn’t look to me that QM will be special. In any case, if it were, the burden of the proof is on those that say that understanding is impossible, not on us who have plenty of examples of the contrary.

To be fair, I have to say that I don’t believe that Feynman was anti-rationalistic — far from that! What I think is that, in his classic energetic style, and somewhat uncomfortable because he himself knew that something was wrong with the current understanding of QM, he mixed things a little bit again in the above quote. I will discuss something more about this soon in sec. 2.2.3, but let me here briefly say that believing that QM is “the way nature really is” is perfectly compatible with the position that it is poorly understood and that something should be done about that. Understanding QM is not finding a more fundamental way that “nature really is” and obtaining QM as a special case. (This might be possible or not, and I am OK with both situations.) What is understanding to me is (more or less) described at the beginning of sec. 2.2, and it is independent from that. Whether or not there is an underlying theory below QM is not a puzzle, it is just a question — and one well within the boundaries of physics by the way!

2.2.2 OK, maybe it is possible, but many geniuses have tried with no luck

Quit now, you’ll never make it. If you disregard this advice, you’ll be halfway there. — David Zucker, (North) American film director

If at first you don’t succeed, try, try, again. Then quit. There’s no use being a damn fool about it. — W. C. Fields, (North) American comedian

Taking again the risk of misreading Feynman’s words, let us look at two more quotes by him (one of which we already met):

One might still like to ask: “How does it work? What is the machinery behind the law?” No one has found the machinery behind the law. No one can “explain” any more than we have just “explained”. No one will give you any deeper representation of the situation. We have no ideas about a more basic mechanism from which these results can be deduced. (Feynman, 1963a, p. 1-10)

Do not keep saying to yourself, if you can possible avoid it, “But how can it be like that?” because you will get ‘down the drain’, into a blind alley from which nobody has escaped. Nobody knows how it can be like that. (Feynman, 1965, p. 129)

Probably mixing again the “whys” with the “hows” (see sec. 2.2.3), and also mixing the lack of a proper understanding of QM with the lack of an underlying theory from which QM can be derived, these two quotes nevertheless remind us of a plausibility argument that you have probably heard many times: “A lot of clever guys have spent decades trying to solve this problem. Therefore, it is either impossible or terribly difficult. If you decide to go into it you will most probably lose your mind and spend your last days as a hermit in a dirty hut.”

Well, I don’t deny some degree of truth to this line of reasoning. The problem of understanding QM seems indeed difficult — but it is also very important. What remains is a personal (not a scientific) decision: Do you choose to take these facts as a warning, a “Beware of the dog!” sign, or you choose to take them as a challenge, as an opportunity?
2.2.3 We need to find out the how, not the why

Teology is like a mistress to a biologist: he cannot live without her but he’s unwilling to be seen with her in public. — J. B. S. Haldane

The first more complex idea about understanding QM that floats around (and which is entangled with Feynman’s statements in the two previous sections) will not take me too much to discuss because I basically agree with it. It says that physics does not need to worry about “whys”, but only about “hows”. This is clearly captured in the following quotes by, who else, Feynman:

Newton was originally asked about his theory —‘But it doesn’t mean anything —it doesn’t tell us anything.’ He said, ‘It tells you how it moves. That should be enough. I have told you how it moves, not why.’ But people often are unsatisfied without a mechanism... (Feynman, 1965, p. 37)

So there is no model of the theory of gravitation today, other than the mathematical from. [...] Every one of our laws is a purely mathematical statement in rather complex and abstruse mathematics. [...] Why? I have not the slightest idea. (Feynman, 1965, p. 39)

Although it might seem at first sight that this is another example of a norm invented for the sake of eluding our job of understanding QM, this is not so for an important reason: Basically, the systems which are undisputedly the traditional domains of physics and whose characteristics are embodied in the mathematical objects and equations that are physics’ language do not have whys. Atoms do not have whys, nor do photons, or polymers, or solids, or stars —that is, unless you believe in a God that has embedded his own purpose into every system in the Universe; if you do, then the why of every physical system is God’s why, and it is outside physics too.

It is not easy to define what exactly a “why” is [you can read the great book by Deacon (2012) if you feel like knowing more about it], but I can briefly mention that it requires an end-directness, a final cause. This final cause can be represented in the minds of agents as it is often the case with humans, or it can be free-floating [in the words by Dennett (1995)], like the whys that help us make sense of the evolution of biological organisms. As jokingly expressed by Haldane’s quote at the beginning of this section, it exists a heated debate in biology about whether or not the use of whys is scientifically justified. It is disputed whether biological organisms actually have whys, but it is unanimously accepted that they indeed appear to have them. This begs the question: “Are whys within the domain of biology?” In physics, however, the behavior of the studied systems is typically captured by differential equations which are local in time and do not appear to have any “final cause” built into them. Physical systems do not even appear to have whys. Hence the answer to the question “Are whys within the domain of physics?” is clearly and simply no.

The first quote by Feynman above suggests a different conception of what a “why” is. Feynman seems to think that a why is just a mechanism behind a law. That is, another more fundamental law. In this sense, equilibrium statistical mechanics would be thermodynamics’ why. But also in this sense the answer to the question “Are whys within the domain of physics?” is again trivial: It is yes. In fact, I claim that this kind of whys is such that it would be more precise if we called them “hows”. A mechanism, a law behind the law, seems more a how to me than a why —and that is why it falls within the competences of physics, by the way.
In any case, neither one type of why nor the other are part of what I have called “understanding” (see the beginning of sec. 2.2 for a sketchy definition of the concept). Understanding is not providing a final cause or a mechanism —although both could be part of understanding in particular cases that do present final causes or underlying mechanisms as valid ingredients. Hence, it is true that whys (in the first sense) are outside the scope of physics, but that doesn’t mean that understanding is. As I said, I believe that understanding is a task that no discipline can avoid. If we use the second sense (a more fundamental description), then whys are trivially part of a physicist’s job. In such a case, our obligation is to understand both QM and its hypothetical underlying mechanism.

2.2.4 Philosophy and interpretation as dirty words

There is no such thing as philosophy-free science; there is only science whose philosophical baggage is taken on board without examination.

— Dennett (1995)

All science is either physics or stamp collecting. — Ernest Rutherford

One literary ornament that people sometimes use when trying to defend that understanding QM (or any other theory for that matter) is not the job of the physicists is to assign negative value to neutral terms such as “philosophy” or “interpretation” (“semantics” is also used sometimes) and apply them to any endeavor that they consider futile. We already met an example of this nice practice in the quote by van Kampen in sec. 2.1 where he derogatorily calls most of the literature on the measurement problem and Bohmian mechanics “philosophical profundities”.

As we can read in the blog “Backreaction” (Hossenfelder, 2013), this is not uncommon among physicists:

I know a lot of physicists who use the word philosophy as an insult . . .

Physicists are typically clever guys —I know because I am one!— and, as clever guys often do, they sometimes think that they are more clever than they actually are. They do not say it directly, but they typically assign this excess of intelligence to the discipline itself. “The thing is not that I am extraordinarily clever and handsome”, they say, “but that physics is a superior intellectual scheme to all the rest of ways of studying the world. Since I happen to be a physicist, well, I guess that I am superior too, but I don’t like to say it aloud not to sound immodest.” The quote by Rutherford at the beginning of this section is an example of this humble attitude, and our old friend Richard P. Feynman is also known for having expressed derogatory opinions about philosophy and philosophers now and then. Here you have some quotes from his very enjoyable biography “Surely you’re joking, Mr. Feynman” (Feynman and Leighton, 1985), but I am sure that you can find many more if you spend 10 minutes in Google:

Another guy got up, and another, and I tell you I have never heard such ingenious different ways of looking at a brick before. And, just like it should in all stories about philosophers, it ended up in complete chaos. In all their previous discussions they hadn’t even asked themselves whether such a simple object as a brick, much less an electron, is an “essential object”.

16
In the early fifties I suffered temporarily from a disease of middle age: I used to give philosophical talks about science...

Cornell had all kinds of departments that I didn’t have much interest in. (That doesn’t mean there was anything wrong with them; it’s just that I didn’t happen to have much interest in them.) There was domestic science, philosophy (the guys from this department were particularly inane), and there were the cultural things — music and so on. There were quite a few people I did enjoy talking to, of course. In the math department there was Professor Kac and Professor Feller; in chemistry, Professor Calvin; and a great guy in the zoology department, Dr. Griffin, who found out that bats navigate by making echoes. But it was hard to find enough of these guys to talk to, and there was all this other stuff which I thought was low-level baloney. And Ithaca was a small town.

Notice in this last quote how he gracefully moves from “that doesn’t mean there was anything wrong with them” to “low-level baloney” in just one paragraph.

We can also read the following in (Feynman, 1963b, chap. 2):

Philosophers, incidentally, say a great deal about what is absolutely necessary for science, and it is always, so far as one can see, rather naive, and probably wrong.

We can read him equating “philosophical principles” to feelings and personal likings in (Feynman, 1965, p. 57):

The next question is whether, when trying to guess a new law, we should use the seat-of-the-pants feeling and philosophical principles —‘I don’t like the minimum principle’, or ‘I do like the minimum principle’, ‘I don’t like action at a distance’, or ‘I do like action at a distance’.

Or we can check what he thought about philosophers’ opinions regarding science in (Feynman, 1965, p. 147):

A philosopher once said ‘It is necessary for the very existence of science that the same conditions always produce the same results.’ Well, they do not [...] What is necessary ‘for the very existence of science’, and what the characteristics of nature are, are not to be determined by pompous preconditions, they are determined always by the material with which we work, by nature herself. We look, and we see what we find, and we cannot say ahead of time successfully what is going to look like. [...] In fact it is necessary for the very existence of science that minds exist which do not allow that nature must satisfy some preconceived conditions, like those of our philosopher.

Incidentally, I agree with Feynman that we should avoid inventing norms about “what science is”, “what a theory is”, “what we can talk about”, etc. —I just don’t think that philosophers are the only ones who concoct such ad hoc rules. As I mentioned in sec. 2.2.3 and will expand in sec. 2.3, physicists are also very fond of repeating statements in the spirit of “It is necessary for the very existence of science that the same conditions always produce the same results”, and which seem to come from nowhere.

To be fair, I have to say that it is not only physicists who sometimes neglect the usefulness of other disciplines. This is actually an epidemic. Mathematicians tend to see physicists as careless and without rigor, while physicists sometimes declare that mathematicians worry
about unimportant matters. Chemists usually look at physics’ topics with skepticism (“OK, and how many decades until we know if there is even a small probability that this finds an application?”), while physicists are often heard saying that chemists use the tools of physics without properly understanding them—especially QM. A similar relationship is in place between chemists and biochemists, between biochemists and biologists, between biologists and psychologists, between psychologists and sociologists, and so on and so forth.

Coming back to the relationship between physics and philosophy, you probably experienced the following situation which is also mentioned in the “Backreaction” blog (Hossenfelder, 2013):

I’ve heard talks by philosophers about the “issue” of infinities in quantum field theory who had never heard of effective field theory. I’ve heard philosophers speaking about Einstein’s “hole argument” who didn’t know what a manifold is, and I’ve heard philosophers talking about laws of nature who didn’t know what a Hamiltonian evolution is.

Well, yes... I have been to very bad talks indeed—and not all of them were by philosophers. But on the other hand...

But on the other hand, I’ve met remarkably sharp philosophers with the ability to strip away excess baggage that physicists like to decorate their theories with, and go straight to the heart of the problem. (Hossenfelder, 2013)

Exactly! So it seems that the solution is very simple: stick to good philosophy (physics, chemistry, sociology, ...), and forget the part of the discipline that is low quality. Or, as Dennett more eloquently puts it, accept Sturgeon’s Law and act consequently (Dennett, 2013, sec. II.4):

Ninety percent of everything is crap. Ninety percent of experiments in molecular biology, 90 percent of poetry, 90 percent of philosophy books, 90 percent of peer-reviewed articles in mathematics—and so forth—is crap. Is that true? Well, maybe it’s an exaggeration, but let’s agree that there is a lot of mediocre work done in every field. (Some curmudgeons say it’s more like 99 percent, but let’s no get into that game.) A good moral to draw from this observation is that when you want to criticize a field, a genre, a discipline, an art form, ... don’t waste your time and ours hooting at the crap! Go after the good stuff, or leave it alone. This advice is often ignored by ideologues intent on destroying the reputation of analytic philosophy, evolutionary psychology, sociology, cultural anthropology, macroeconomics, plastic surgery, improvisational theater, television sitcoms, philosophical theology, massage therapy, you name it.

For example, let us read how Hilary Putnam (a philosopher who many would argue belongs to the 10% of non-crap) helped a renowned physicist change his mind about the need of a conceptual tidying-up in QM (Putnam, 2005):

For myself, and for any other ‘scientific realist’, the whole so-called interpretation problem in connection with quantum mechanics is just this: whether we can understand quantum mechanics—no, let me be optimistic—how to understand quantum mechanics in a way that is compatible with the anti-operationalist philosophy that I subscribed to in the pages I just quoted, and that I have always subscribed to. But it took a long time for physicists to admit that there is such a problem. I can tell you a story about that. In 1962 I had a series of conversations with a world-famous physicist (whom I
will not identify by name). At the beginning, he insisted, ‘You philosophers just think there is a problem with understanding quantum mechanics. We physicists have known better from Bohr on.’ After I forget how many discussions, we were sitting in a bar in Cambridge, and he said to me, ‘You’re right. You’ve convinced me there is a problem here; it’s a shame I can’t take three months off and solve it.’

Fourteen years later, the same physicist and I were together at a conference for a few days, and he opened his lecture at that conference (a lecture which explained to a general audience the exciting new theories of quarks) by saying, ‘There is no Copenhagen interpretation of quantum mechanics. Bohr brainwashed a generation of physicists.’ Evidently, he had undergone a considerable change of outlook.

In the following quote, also by Putnam (2010), we can read some more thoughts about the relationship between philosophy and science, we find a clear exposition about the real nature of the so-called “Copenhagen interpretation”, and we learn that the physicist in the above story was the Nobel Prize winner Murray Gell-Mann:

That philosophy is not to be identified with science is not to deny the intimate relation between science and philosophy. The positivist idea that all science does is predict the observable results of experiments is still popular with some scientists, but it always leads to the evasion of important foundational questions. For example, the recognition that there is a problem of understanding quantum mechanics, that is, a problem of figuring out just how physical reality must be in order for our most fundamental physical theory to work as successfully as it does, is becoming more widespread, but that recognition was delayed for decades by the claim that something called the “Copenhagen interpretation” of Niels Bohr had solved all the problems. Yet the “Copenhagen interpretation”, in Bohr’s version, amounted only to the vague philosophical thesis that the human mind couldn’t possibly understand how the quantum universe was in itself and should just confine itself to telling us how to use quantum mechanics to make predictions stateable in the language of classical, that is to say, non-quantum-mechanical, physics! (in my lifetime, I first realized that the “mood” had changed when I heard Murray Gell-Mann say in a public lecture sometime around 1975 “There is no Copenhagen interpretation of quantum mechanics. Bohr brainwashed a generation of physicists!”)

Only after physicists stopped being content to regard quantum mechanics as a mere machine for making predictions and started taking seriously what this theory actually means could real progress be made. Today many new paths for research have opened as a result: string theory, various theories of quantum gravity, and “spontaneous collapse” theory are only the beginning of quite a long list. And Bell’s famous theorem, which has transformed our understanding of the ‘measurement problem’. would never have been proved if Bell had not had a deep but at the time highly unpopular interest in the meaning of quantum mechanics.

To me the issue is clear. The position that physics can be done without appeal to meaning, to interpretation, to “philosophy”, is just untenable. The idea that physics is only a little more than mathematics, that applying physical theories to actual experiments is straightforward, that “Shut up and calculate!” is a good advice, are nothing but expressions of wishful thinking. You can close your eyes in the easiest cases and everything might work well for a while; however, when the range of problems to which you want to apply the theory is enlarged, when you want to relate the theory to other neighboring theoretical schemes,
when you embark in the construction of new models of nature, or when you want to grasp the workings of the theory beyond the blind mechanical implementation of its equations as if they were cooking recipes, ... in all these situations, you might find that you need to take a walk in the dreaded fields of “philosophy”. At that moment, you may find the insights by some philosophers helpful (some philosophers belonging to the non-crumpy 10%, that goes without saying), and you will probably stop using “philosophical” as a dirty adjective.

Let me close this section with some quotes by scientists who have also realized that this is the case, so you can see that Murray Gell-Mann and I are not the only ones to hold this opinion:

> Which view one adopts affects how one thinks about the theory at a fundamental level. (Colbeck and Renner, 2012a)

> How we look at a theory affects our judgment as to whether it is mysterious or irrational on the one hand; or whether it is satisfactory and reasonable on the other. Thus it affects the direction of our research efforts; and a fortiori their results. Indeed, whether we theorists can ever again manage to get ahead of experiment will depend on how we choose to look at things, because that determines the possible forms of the future theories that will grow out of our present ones. One viewpoint may suggest natural extensions of a theory, which cannot even be stated in terms of another. What seems a paradox on one viewpoint may become a platitude on another. (Jaynes, 1990)

> Because so many of the results do not seem to depend in a critical way on the choice of interpretation, some “practical-minded” physicists would like to dismiss the whole subject of interpretation as irrelevant. That attitude, however, is not justified, and a number of practical physicists have been led into unnecessary confusion and dispute because of inattention to the matters of interpretation that we have been discussing. (Ballentine, 1998, p. 239)

### 2.2.5 Understanding equals being intuitive, or being classical

Brad DeLong, in the course of something completely different, suggests that the theory of relativity really isn’t all that hard. At least, if your standard of comparison is quantum mechanics. — Carroll (2011)

Another way in which you can reject the need of understanding QM is by claiming that, actually, what “understanding” is is something unnecessary and henceforth of very little value.

In this spirit, I have met a very curious idea and you have probably met it too: That “being understood” is just being intuitive, and that being intuitive is a feature so much related to everyday experience that by definition we can only “understand” classical mechanics. Thus, when I say that “I don’t understand QM” I would only be rephrasing the vacuous complaint that “QM is not classical mechanics, and that bothers me”. If we take a look at the “cube of theories” (in fig. 2.2) introduced by Gamow et al. (2002) as a humorous present to a female student that the three young friends courted [it seems: (Okun, 2002)], the argument would state that understanding is only possible in the small orange region that comprises classical mechanics with and without gravity. QM is just outside this region and nothing can be done about it but grow a healthy resignation.

This point of view is very clearly found in some quotes by Feynman:
Figure 2.2: Cube of theories (Okun, 2002). The origin corresponds to all physical constants being zero, i.e., $G = h = 1/c = 0$, while the rest of the vertices represent the different theories in which gravitational, quantum, relativistic effects, or a combination of them, are felt and must be accounted for.

Because atomic behavior is so unlike ordinary experience, it is very difficult to get used to, and it appears peculiar and mysterious to everyone — both to the novice and to the experienced physicist. Even the experts do not understand it the way that they would like to, and it is perfectly reasonable that they should not, because all of direct human experience and of human intuition applies to large objects. We know how large objects will act, but things on small scale just do not act that way. So we have to learn about them in a sort of abstract or imaginative fashion and not by connection with our direct experience. (Feynman, 1963a, p. 1-1)

In the beginning of the history of experimental observation on scientific things, it is intuition, which is really based on simple experience with everyday objects, that suggests reasonable explanations for things. But as we try to widen and make more consistent our description of what we see, as it gets wider and wider and we see a greater range of phenomena, the explanations become what we call laws instead of simple explanations. One odd characteristic is that they often seem to become more and more unreasonable and more and more intuitively far from obvious. [...] There is no reason why we should expect things to be otherwise, because the things of everyday experience involve large numbers of particles, or involve things moving very slowly, or involve other conditions that are special and represent in fact a limited experience with nature. (Feynman, 1965, p. 127)

Electrons, when they were first discovered, behaved exactly like particles or bullets, very simply. Further research showed, from electron diffraction experiments for example, that they behaved like waves. As time went on there was a growing confusion about how these things really behaved — waves or particles, particles or waves? Everything looked like both.
This growing confusion was resolved in 1925 or 1926 with the advent of the correct equations for quantum mechanics. Now we know how the electrons and light behave. But what can I call it? If I say they behave like particles I give the wrong impression; also if I say they behave like waves. They behave in their own inimitable way, which technically could be called a quantum mechanical way. They behave in a way that is like nothing that you have seen before. Your experience with things that you have seen before is incomplete. The behavior of things on a very tiny scale is simply different. An atom does not behave like a weight hanging on a spring and oscillating. Nor does it behave like a miniature representation of the solar system with little planets going around in orbits. Nor does it appear to be somewhat like a cloud or fog of some sort surrounding the nucleus. It behaves like nothing you have seen before. (Feynman, 1965, p. 128)

But not only him. Van Kampen also seems to think that this is the case:

The difficulty is that the authors are unable to adjust their way of thinking —and speaking— to the fact that phenomena on the microscopic scale look different from what we are accustomed to in ordinary life. (van Kampen, 2008)

In my opinion, the problem with QM is not that it is counterintuitive, it is not that it is different from classical mechanics. As mentioned in the quotes above, being counterintuitive is completely expected when the theory tackles phenomena that are outside everyday experience. QM talks about the very small, and we never see or experience the very small directly, so it is normal that we find QM counterintuitive. So far so good.

The problem is that many other theories are counterintuitive, but they haven’t produced tens of interpretations. Special relativity is also counterintuitive, because it deals with bodies that move close to the speed of light (which is much faster than the fastest of our spacecrafts). There are some nice counterintuitive effects predicted by special relativity, but there are hardly more interpretations than one, and the prolific discussions in the literature with tens of papers being produced each month about how to understand the quantum mechanical worldview are just missing in the case of special relativity.

I agree with Feynman that there is a psychological resistance to counterintuitive theories:

The difficulty really is psychological and exists in the perpetual torment that results from your saying to yourself, “But how can it be like that?” which is a reflection of uncontrolled but utterly vain desire to see it in terms of something familiar. I will not describe it in terms of an analogy with something familiar; I will simply describe it. (Feynman, 1965, p. 129)

But I think that it is a resistance which can be overcome with time and practice. Special relativity is an example of this.

The very different situation that affects the interpretation of the two, similarly old theories despite the fact that both depart significantly from classical experience is clearly put by Kochen (2013):

Almost a century after the mathematical formulation of quantum mechanics, there is still no consensus on the interpretation of the theory. This may be because quantum mechanics is full of predictions which contradict our everyday experiences, but then so is another, older theory, namely special relativity.
In fact, Feynman also mentions relativity just after the above paragraph:

There was a time when the newspapers said that only twelve men understood the theory of relativity. I do not believe there ever was such a time. There might have been a time when only one man did, because he was the only guy who caught on, before he wrote his paper. But after people read the paper a lot of people understood the theory of relativity in some way or other, certainly more than twelve. On the other hand, I think I can safely say that nobody understands quantum mechanics.

OK, Prof. Feynman, then what is the difference between the two? To me, the answer is that the conceptual foundations of QM are simply a mess, while those of special relativity are not. QM is not yet understood (in the sense of the word defined in sec. 2.2), but special relativity is. This is only possible because understanding has nothing to do with being intuitive or with being classical.

The quote by Streater (2007) (of which we already met a part in sec. 2.1) is a clamorous failure to notice that the different level of consensus between the two theories might be indicating something relevant.

The continuing work on quantum paradoxes parallels a similar output after relativity was first formulated: when simple paradoxes concerning time-dilation or Lorentz contraction were explained, more and more elaborate versions, (twin paradox, thermodynamic paradoxes involving absorption and emission in a gravitational field...) were claimed to show some mathematical or physical inconsistency in the special or general theory. Rebutting these took up the time of the advocates of the theory, and led to clarification. But the activity died out. The quantum paradox community is, even after all these years and all the rebuttals, still on the increase. It has spawned larger enterprises, such as Bohmian mechanics, Nelson dynamics, geometro-stochastic dynamics, quantum-state reduction, and the quantum theories of the brain of Penrose and of Stapp. These are lost causes too, and fall down with the rebuttal of the spooky action at a distance claimed to inspire them.

As we discussed in sec. 2.1, Streater prefers to believe that the reason behind the difference is that the scientists that investigate QM are specially myopic. "Time will come when they will all realize that they are wasting their lives in a lost cause —but boy is it taking long!"

2.3 These are the rules of the game

There are three rules for writing a novel. Unfortunately, no one knows what they are. — William Somerset Maugham

We have already met some examples of ad hoc invented rules and prohibitions that seem to come from nowhere and whose only aim appears to be protecting certain ways of thinking, certain statements: “Understanding is not the job of physics”, “Thou shall not philosophize”, “The puzzle is irreducible”, etc. In this section, I introduce two more such rules which tend to co-occur with the “Shut up and calculate!” command, and which do not stand for half a round once you critically scrutinize them.

When I hear an example of such a rule, I often find that a powerful argument against it is just to answer: “OK, and why is that?” But we can also turn the words by Feynman about philosophers against those physicists that become part-time lawmakers:
...what the characteristics of nature are, are not to be determined by pompous pre-
conditions, they are determined always by the material with which we work, by nature
herself. We look, and we see what we find, and we cannot say ahead of time success-
fully what it is going to look like. [...] In fact it is necessary for the very existence of
science that minds exist which do not allow that nature must satisfy some preconceived
conditions, like those of our philosopher [physicist?]. (Feynman, 1965, p. 147)

2.3.1 The most important thing a theory must do is predict

Prediction is very difficult, especially of the future.

— Niels Bohr (it seems)

You have probably heard something to the effect of: “The great power of physics and
the difference between physics and the rest of sciences is its predictive power”, “The most
important thing a theory must do is predict”, “Economics is the science that explains the
past” (derogatively). Let us read some of our friends about the topic:

So we must talk about what we can predict. (Feynman, 1963a, p. 2-3)

Scientific theories predict the probabilities of outcomes of experiments. (Kochen, 2013)

The basis of a science is its ability to predict. To predict means to tell what will happen
in an experiment that has never been done. (Feynman, 1963a, p. 2-8)

It amazes me how such incredibly concrete rules can be concocted about a set of “things”
so amazingly complex and heterogeneous as scientific theories, but hey, that doesn’t mean
that our friends are wrong. Maybe it is true that prediction is the most precious and
absolutely essential property that a theory must have... or maybe it isn’t. We could ask our
friends why prediction is the gold standard by which scientific theories must be judged, but
it would be a rhetorical question, because the statement is just false.

Prediction is of course great. It shows that your theory is so powerful, that the knowledge
of the world it provides to you is so accurate that you can perform the almost magical trick
of knowing what will happen in advance. It is said that the ability to predict solar eclipses
made native cultures look at invaders as semi-gods a long time ago. I will not deny that
prediction is a good thing, but “the basis of a science”? I think that it is too bold a claim.

For that matter, the conditions that are required for the very act of prediction are not
always present —and yet, it would seem that in many cases we are still “doing science”. Let
us read the following paragraph by Feynman:

The problem has been raised: if a tree falls in a forest and there is nobody there to
hear it, does it make a noise? A real tree falling in a real forest makes a sound, of
course, even if nobody is there. Even if no one is present to hear it, there are other
traces left. The sound will shake some leaves, and if we were careful enough we might
find somewhere that some thorn had rubbed against a leaf and made a tiny scratch
that could not be explained unless we assumed the leaf were vibrating. So in a certain
sense we would have to admit that there is a sound made. (Feynman, 1963a, p. 2-8)

Even if he is not using equations, he is using his knowledge about what sound is (air
pressure waves), how it can move physical objects such as leaves, and how harder objects
than leaves can leave marks on them. By looking at the marks, he sees fit to conclude that “we would have to admit that” a sound “happened” in the forest. As the (probably apocryphal) quote by Bohr at the beginning of this section jokingly puts it, “prediction” is typically understood “of the future” —and yet it seems that Feynman has just used some informal theory to produce reliable claims about the past. The word he uses is a good name for it: “explaining”. This is another possible use that we can give to scientific theories and I don’t know why we should say that this is less important than predicting the future. For example, in the falling tree case above, not only reproducing the whole “experiment” (same forest, same tree, same thorns, same leaves) would have proved costly, but it seems that Feynman though it was also unnecessary.

If you think that what Feynman did in the quoted paragraph is not important or if you think it is not science, you only have to learn about his participation in the investigation about the disaster of the space shuttle “Challenger” in 1986 [see for example (Feynman and Leighton, 1988)]. In that occasion, Feynman contributed to explaining the reasons of the accident using more sophisticated science than just a qualitative understanding of sound —of course, repeating the experiment was also out of the question in that occasion. Similarly, you can think about Darwin’s evolution; a theory which is much more valued for its explanatory powers than for its predictions. We could call all these applications of rigorous rational thinking, all these uses to which we can put our dearest disciplines such as physics and biology, “non-scientific” if we want. In that way, the claim that prediction is the most important part of science will stand (by definition), but what would be the point of it?

The truth is that scientific theories have many properties at which we have to look if we want to decide if we are satisfied with them: predictive power of course, but also explanatory power, internal consistency, compatibility with other theories, ability to suggest new experiments, simplicity, conceptual clarity, etc. We could start an eternal debate about which properties are necessary, which are the most important ones, and which grant the use of the distinguished “scientific” label, but it would probably be a waste of time. If you allow a recommendation, whenever you listen that “QM is predictive and that’s all a proper scientific theory needs to do”, you can ask “OK, and why is that?”, and patiently wait for an answer.

### 2.3.2 You can only talk about what can be measured

We find no sense in talking about something unless we specify how we measure it; a definition by the method of measuring a quantity is the one sure way of avoiding talking nonsense. — (Sir) Hermann Bondi

That only measurable quantities are “properly defined”, “make sense” or that they are the only things about which it is allowed to talk if we want to “avoid talking nonsense” is another of the “pompous preconditions” (using the term by Feynman) that often appear when somebody dares to criticize the present conceptual state of the theory known as QM.

Let us read Feynman on this:

Another thing that people have emphasized since quantum mechanics was developed is the idea that we should not speak about those things which we cannot measure. […] Unless a thing can be defined by measurement, it has no place in a theory. (Feynman, 1963a, p. 2-8)
So it seems that the proponents of the Copenhagen interpretation are not content with introducing the concept of a “measure” into the rules of QM in a completely undefined and mystical way—they also want it to be the only thing we can talk about!

Successful theories are full of examples of ideal concepts which are not directly measurable: centers of mass, actions, probability density functions, Lagrangians, etc. Limiting the concepts about which we can “talk about” thus seems a very bad strategy if we want to make sense of the physical world in which we live in. Moreover (and again), what is the reason behind the “pompous precondition”? Is there any reason at all?

I am tired of defeating weak adversaries, so let us read the answer by Feynman, in the paragraph just after the previous one:

It is a careless analysis of the situation. Just because we cannot measure position and momentum precisely does not a priori mean that we cannot talk about them. It only means that we need not talk about them. [...] It is not true that we can pursue science completely by using only those concepts which are directly subject to experiment. (Feynman, 1963a, p. 2-8)

2.4 Of moral and epistemic weakness

An overarching theme which is much related to everything that we have been discussing is the implication that whoever wants to “understand” QM suffers from a moral or epistemic weakness. As we have seen, it can take many forms:

You can be too demanding, asking for unreasonable things:

Actually quantum mechanics provides a complete and adequate description of the observed physical phenomena on the atomic scale. What else can one wish? (van Kampen, 2008)

You may abandon the righteous path of sound, austere physics lured by the poetic but utterly useless “profundities” and “low-level baloney” that are common in that dignified hobby known as “philosophy”:

The scandal is that there are still many articles, discussions, and textbooks, which advertise various interpretations and philosophical profundities. (van Kampen, 2008)

In the early fifties I suffered temporarily from a disease of middle age: I used to give philosophical talks about science ...(Feynman and Leighton, 1985)

...and there was all this other stuff which I thought was low-level baloney. (Feynman and Leighton, 1985)

You can mistake a theory having conceptual problems with your own psychological inability to accept that a theory is counterintuitive, simply because you are unable to adjust your thinking and talking:

The difficulty is that the authors are unable to adjust their way of thinking—and speaking—to the fact that phenomena on the microscopic scale look different from what we are accustomed to in ordinary life. (van Kampen, 2008)
The difficulty really is psychological and exists in the perpetual torment that results from your saying to yourself, “But how can it be like that?” which is a reflection of uncontrolled but utterly vain desire to see it in terms of something familiar. (Feynman, 1965)

You may let your feelings get in the way of your rational thinking:

The next question is whether, when trying to guess a new law, we should use the seat-of-the-pants feeling and philosophical principles... (Feynman, 1965)

The “paradox” is only a conflict between reality and your feeling of what reality “ought to be”. (Feynman, 1963a, 18-9)

Or you can be childish and refuse to play by the rules that your wise scientific parents kindly transmitted to you for the sake of your own safety and mental well being (see sec. 2.3).

If this is so, if these weaknesses plague you, you can repent and begin the long path to responsibility, self-control and adulthood by reading any solid textbook which talks about “measuring” in the proper way, i.e., treating it as a completely unproblematic term. Or you can remember that *ad hoc* attacks and cheap psychological analyses are the signs that someone has run out of arguments, and just pretend you didn’t listen.

### 2.5 Do not work in QM’s foundations if you want to succeed

The juvenile sea squirt wanders through the sea searching for a suitable rock or hunk of coral to cling to and make its home for life. For this task, it has a rudimentary nervous system. When it finds its spot and takes root, it doesn’t need its brain anymore so it eats it! (It’s rather like getting tenure.)

— Dennett (1991)

When all persuasion strategies have failed, the only thing that remains to be tried is to suggest that your career, your assets, your mental well-being, your “success” in general will be jeopardized if you are fool enough to break the rules and do the forbidden thing. This last-resort (and somewhat desperate) move is in the air all the time, but it is also explicitly used in the otherwise great book by Streater (2007).

The book offers very articulate insights about many topics that have been mentioned here and it is in my opinion a must to anybody who wants to begin to make sense of the field, to know what the questions are, and to meet strong and consistent proposals to answer them. The problem to me is the same as with the statements by van Kampen discussed in sec. 2.1. Calling “lost causes” to open lines of research followed by so many scientists at the moment seems an instance of what the author himself criticizes about “Bohmians” in p. 107:

Bohmians are very wary of jumping to any conclusions; even in easy cases.

The recommendation about your job future as a scientist —which is completely consistent with this attitude— comes in p. 95:

This subject [hidden variable theories of QM] has been thoroughly worked out and is now understood. A thesis on this topic, even a correct one, will not get you a job.
On the one hand, fear of starvation or being an outcast is no scientific argument. On the other hand, this is just false. As you can check by scanning the literature yourself, it is entirely possible to write a thesis in hidden variables, even an incorrect one, and still get a job—although certainly not in Streater’s group, it seems. Maybe he is right and hidden variable theories turn out to be of no value in the end; however, and given that nobody knows at the moment how to clean up the conceptual QM mess, this path could in principle be as good as any other. Check for example (Mermin, 1993, Schlosshauer, 2011) for cogent analyses on the present situation of this line of work, which do not assume from the start that:

Bell’s theorem, together with the experiments of Aspect et al., shows that the theoretical idea to use hidden classical variables to replace quantum theory is certainly a lost cause, and has been for forty years. (Streater, 2007, p. 99)

About Bohmian mechanics, Streater (2007) has a different message for you:

This subject was assessed by the NSF of the USA as follows “…The causal interpretation [of Bohm] is inconsistent with experiments which test Bell’s inequalities. Consequently …funding …a research programme in this area would be unwise”. I agree with this recommendation.

So it seems that the situation is slightly better with Bohmian mechanics than with hidden variables (even if some researchers say that the former is an example of the latter). Although you will get no funding from the NSF if you go all Bohmian, at least the possibility that you might get a job is open. On the other hand, since “funding agencies are wise” is a proposition of uncertain truth value, the above quote is again no scientific argument at all. As the many interesting works that are published in the field every month show, doing research in Bohmian mechanics is entirely justified—that is, if you can live without NSF funding. If you are interested to begin to explore this line (even if that means paying for your own conference fees) you can check the following references: (Dürr and Teufel, 2009, Nikolić, 2008, Oriols and Mompart, 2012). However, be careful, since this would go against the advice in (Streater, 2007, p. 112):

Better steer clear of Bohmians.

My advice to you is slightly different: Better steer clear of people that tell you to steer clear of other people. Although it is a logical contradiction as stated, I am sure that you can minimally modify it so that it both makes sense and becomes a hopefully useful advice.

3 Conclusions

A conclusion is the place where you got tired of thinking.
—Martin H. Fischer

An adventure is only an inconvenience rightly considered. An inconvenience is only an adventure wrongly considered.
—G. K. Chesterton

Since I have completely abused his words, let me quote Feynman again to begin to wrap up:
...mathematicians prepare abstract reasoning ready to be used if you have a set of axioms about the real world. But the physicist has meaning to all his phrases. [...] in physics you have to have an understanding of the connection of words with the real world. It is necessary at the end to translate what you have figured out into English, into the world, into the blocks of copper and glass that you are going to do the experiments with. (Feynman, 1965, p. 55)

This is completely so in my opinion, and I think that I have presented strong plausibility arguments supporting that the task of “understanding the connection of the words with the real world” is something which still has to be achieved in the case of one of humankind’s most successful theories: quantum mechanics.

It is not only very important that we do so, but it is also slightly embarrassing that we haven’t done it yet. In this, I agree with Carroll (2013):

Not that we should be spending as much money trying to pinpoint a correct understanding of quantum mechanics as we do looking for supersymmetry, of course. The appropriate tools are very different. We won’t know whether supersymmetry is real without performing very costly experiments. For quantum mechanics, by contrast, all we really have to do (most people believe) is think about it in the right way. No elaborate experiments necessarily required (although they could help nudge us in the right direction, no doubt about that). But if anything, that makes the embarrassment more acute. All we have to do is wrap our brains around the issue, and yet we’ve failed to do so.

But as him...

I’m optimistic that we will, however. And I suspect it will take a lot fewer than another eighty years. The advance of experimental techniques that push the quantum/classical boundary is forcing people to take these issues more seriously. I’d like to believe that in the 21st century we’ll finally develop a convincing and believable understanding of the greatest triumph of 20th-century physics. (Carroll, 2013)

Now, what we need is clever people that are not afraid of thinking deep, of getting their hands dirty, and of tackling great problems. If you feel like giving it a try and you meet some headwind howling that “everything is understood”, that “understanding is not our job”, or that it is “impossible” or “too difficult”, if some colleagues tell you that “physics does not deal with whys”, that “understanding is just being Newtonian” or that “Thou shall not philosophize”, if they throw invented norms to you, or if weaknesses and career prospects enter the discussion when the debate gets all heated up...you can always answer “Shut up and let me think!” —and get back to work.

Acknowledgements

I would like to thank J. L. Alonso for illuminating discussions. Our conversations about quantum mechanics have been long and many, and I have enjoyed them all —which of course doesn’t mean that our opinions coincide. I also thank Lucien Hardy for pointing out the very similar motto introduced by Rob Spekkens and him, Michel Dyakonov and Chris Fields for noticing that my paper was published the same day as the one by Berthold-Georg Englert
defending just the opposite position, and Giancarlo Ghirardi for nicely pointing out that I had forgotten to mention collapse models. Finally, I thank Iván Calvo, Antonio García Cordero, David Zueco and some anonymous referees at the American Journal of Physics (the first place where I tried to get this published) for correcting some typos in previous versions of the manuscript.

This work has been supported by the grants FIS2009-13364-C02-01 (MICINN, Spain), UZ2012-CIE-06 (Universidad de Zaragoza, Spain), Grupo Consolidado “Biocomputación y Física de Sistemas Complejos” (DGA, Spain).

References

A. E. Allahverdyan, R. Balian, and T. M. Nieuwenhuizen. Understanding quantum measurement from the solution of dynamical models. *Phys. Rep.*, 525:1–166, 2013.

G. Auletta, M. Fortunato, and G. Parisi. *Quantum mechanics*. Cambridge University Press, 2009.

L. E. Ballentine. *Quantum mechanics. A modern development*. World Scientific, 1998.

A. Bassi and G. Ghirardi. Dynamical reduction models. *Phys. Rep.*, 379:257–426, 2003.

A. Bassi, K. Lochan, S. Satin, T. P. Singh, and H. Ulbricht. Models of wave-function collapse, underlying theories, and experimental tests. *Rev. Mod. Phys.*, 85(2):471–527, 2013.

S. Carroll. *From eternity to here: The quest for the ultimate theory of time*. Plume, 2010. [http://bit.ly/1aqMJYt](http://bit.ly/1aqMJYt).

S. Carroll. Is relativity hard?, 2011. [http://bit.ly/19OgvqD](http://bit.ly/19OgvqD).

S. Carroll. The most embarrassing graph in modern physics, 2013. [http://bit.ly/1aqK68V](http://bit.ly/1aqK68V).

C. W. Clark and V. Galitski. Exploring quantum physics, 2013. [https://www.coursera.org/course/eqp](https://www.coursera.org/course/eqp).

C. Cohen-Tannoudji, B. Diu, and F. Laloë. *Quantum mechanics. Volume 1*. Wiley, 1977a.

C. Cohen-Tannoudji, B. Diu, and F. Laloë. *Quantum mechanics. Volume 2*. Wiley, 1977b.

R. Colbeck and R. Renner. Is a systems wave function in one-to-one correspondence with its elements of reality? *Phys. Rev. Lett.*, 108:1–4, 2012a.

R. Colbeck and R. Renner. The completeness of quantum theory for predicting measurement outcomes, 2012b. [http://arxiv.org/abs/1208.4123v2](http://arxiv.org/abs/1208.4123v2).

T. Deacon. *Incomplete nature: How mind emerged from matter*. Norton & Company, 2012.

D. C. Dennett. *Consciousness explained*. Black Bay Books, 1991.

D. C. Dennett. *Darwin’s dangerous idea*. Penguin Books, 1995.

D. C. Dennett. *Breaking the spell. Religion as a natural phenomenon*. Penguin Books, 2006.
D. C. Dennett. *Intuition pumps and other tools for thinking*. W. W. Norton & Company, 2013.

D. Dürr and S. Teufel. *Bohmian mechanics*. Springer-Verlag, 2009.

B.-G. Englert. On quantum theory, 2013. http://arxiv.org/abs/1308.5290.

R. P. Feynman. *The Feynman lectures on Physics. Volume 3. Quantum mechanics*. Addison-Wesley, 1963a.

R. P. Feynman. *The Feynman lectures on Physics. Volume 1. Mainly mechanics, radiation, and heat*. Addison-Wesley, 1963b.

R. P. Feynman. *The character of physical law*. The MIT Press, 12th edition, 1965.

R. P. Feynman. What is Science? *Phys. Teach.*, 7:313–320, 1968.

R. P. Feynman. Simulating physics with computers. *Int. J. Theor. Phys.*, 21:467–488, 1982. http://bit.ly/1cXtgzv.

R. P. Feynman and R. Leighton. *Surely you’re joking, Mr. Feynman*. W. W. Norton & Company, 1985.

R. P. Feynman and R. Leighton. *What do you care what other people think?* W. W. Norton & Company, 1988.

J. Fröhlich and B. Schubnel. Do we understand quantum mechanics - finally?, 2012. http://arxiv.org/abs/1203.3678.

C. A. Fuchs and A. Peres. Quantum theory needs no ‘interpretation’. *Physics Today*, 53:70, 2000.

G. Gamow, D. Ivanenko, and L. Landau. World constants and limiting transition. *Phys. Atom. Nucl.*, 65:1373–1375, 2002.

R. B. Griffiths. *Consistent quantum theory*. Cambridge University Press, 2003.

L. Hardy and R. Spekkens. Why physics needs quantum foundations. *Physics in Canada*, 66:73–76, 2010. http://arxiv.org/abs/1003.5008.

R. Healey. Quantum theory: A pragmatist approach. *Br. J. Philos. Sci.*, 63:729–771, 2012. http://bit.ly/1bM59A0.

A. Hobson. Why decoherence solves the measurement problem, 2013. http://arxiv.org/abs/1308.4055.

S. Hossenfelder. The philosophie of gaps, 2013. http://bit.ly/16RjtZt.

E. T. Jaynes. Probability in Quantum Theory. In W. H. Zurek, editor, *Complexity, entropy, and the physics of information*, page 381. Addison-Wesley, 1990.

R. E. Kastner. Measurement: Still a problem in standard quantum theory, 2013.

S. Kochen. A reconstruction of quantum mechanics, 2013. http://arxiv.org/abs/1306.3951.
L. D. Landau and E. M. Lifshitz. *Quantum mechanics. Non-relativistic theory.* Pergamon, 3rd edition, 1991.

P. G. Lewis, D. Jennings, J. Barrett, and T. Rudolph. The quantum state can be interpreted statistically. *Phys. Rev. Lett.*, 109:150404, 2012. [http://arxiv.org/abs/1201.6554](http://arxiv.org/abs/1201.6554).

N. D. Mermin. Hidden variables and the two theorems of John Bell. *Rev. Mod. Phys.*, 65:803–815, 1993.

N. D. Mermin. Could Feynman have said this? *Physics Today*, May 2004:10, 2004. [http://bit.ly/13tbGA5](http://bit.ly/13tbGA5).

A. Messiah. *Quantum mechanics. Volume 1*. North-Holland, 1961a.

A. Messiah. *Quantum mechanics. Volume 2*. North-Holland, 1961b.

R. G. Newton. *Quantum physics. A text for graduate students*. Springer, 2002.

H. Nikolić. Would Bohr be born if Bohm were born before Born? *Am. J. Phys.*, 76:143, 2008.

L. B. Okun. On the article of G. Gamow, D. Ivanenko, and L. Landau “World constants and limiting transition”. *Phys. Atom. Nucl.*, 65:1370–1372, 2002.

X. Oriols and J. Mompart. Overview of Bohmian mechanics. In *Applied Bohmian mechanics: From nanoscale systems to cosmology*. Pan Stanford Publishing, 2012. [http://arxiv.org/abs/1206.1084v2](http://arxiv.org/abs/1206.1084v2).

C. Pfister and S. Wehner. If no information gain implies no disturbance, then any discrete physical theory is classical. *Nat. Comm.*, 4:1851, 2013. [http://arxiv.org/abs/1210.0194v4](http://arxiv.org/abs/1210.0194v4).

H. Price and K. Wharton. Dispelling the quantum spooks – a clue that Einstein missed?, 2013. [http://arxiv.org/abs/1307.7744v1](http://arxiv.org/abs/1307.7744v1).

M. F. Pusey, J. Barrett, and T. Rudolph. On the reality of the quantum state. *Nat. Phys.*, 8:476–479, 2012.

H. Putnam. A philosopher looks at quantum mechanics (again). *Br. J. Philos. Sci.*, 56:615–634, 2005.

H. Putnam. Science and philosophy. In M. de Caro and D. MacArthur, editors, *Naturalism and normativity*. Columbia University Press, 2010.

J. J. Sakurai. *Modern quantum mechanics*. Addison-Wesley, 1994.

S. Saunders, J. Barrett, A. Kent, and D. Wallace. *Many worlds? Everett, quantum theory, & reality*. Oxford University Press, 2010.

M. Schlosshauer. *Elegance and enigma. The quantum interviews*. Springer-Verlag, 2011.

M. Schlosshauer, J. Kofer, and A. Zeilinger. A snapshot of foundational attitudes toward quantum mechanics. *Stud. Hist. Phil. Mod. Phys.*, 44:222–230, 2013. [http://arxiv.org/abs/1301.1069](http://arxiv.org/abs/1301.1069).
R. Shankar. *Principles of quantum mechanics*. Plenum Press, 1994.

R. F. Streater. *Lost causes in and beyond physics*. Springer-Verlag, Heidelberg, 2007.

A. Streltsov and W. H. Zurek. Quantum discord cannot be shared. *Phys. Rev. Lett.*, 111: 040401, 2013.

Karl Svozil. The present situation in quantum mechanics and the ontological single pure state conjecture, 2012. [http://arxiv.org/abs/1206.6024v1](http://arxiv.org/abs/1206.6024v1).

N. G. van Kampen. The scandal of quantum mechanics. *Am. J. Phys.*, 76:989, 2008.

D. Wallace. *The emergent multiverse. Quantum theory according to the Everett interpretation*. Oxford University Press, 2012.