And now for something completely different: the Elementary Process Theory

Revised, updated and extended 2nd edition of the dissertation with almost the same title

Marcoen J.T.F. Cabbolet, PhD

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Eburon Academic Publishers
Vredenburg 40
3511 BD Utrecht
The Netherlands
tel.: +31-(0)30-2273350
info@eburon.nl / www.eburon.nl

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Permission in writing has been given by Monty Python for the use of their catchphrase 'and now for something completely different' in the title of this 2nd edition.

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Or is it?

Contents

Preface to the 2nd edition

> "*It will not surprise you that after reading this pathetic stuff I come to this conclusion: this is all nonsense, this dissertation contains nothing worth publishing.*"—Andries Brouwer (2008b), eminent mathematician, on my 2007 concept-dissertation

A world with repulsive gravity: what would it be made up of, and how would it function? For me, that's the most fascinating topic there is. In 1996—when I was still in my 20s—I was struck out of the blue by what René Descartes called a 'clear and distinct idea' about the outside world while pondering about repulsive gravity, and ever since I have considered it my calling to develop that idea into a consistent theory of the fundamental workings of a world with repulsive gravity. And for over 25 years I've followed that calling, like a servant of the Lord follows his calling—that is, without striving for worldly pleasures like recognition, fame or fortune. I did not, however, follow any traditional career path. Nowadays one is expected to start out an academic career in physics as a PhD student who develops a preexisting idea of a professor, and to wait with the development of one's own ideas until a certain level of seniority has been reached. But I am not here to live up to the expectations of my contemporaries. Not that I consider myself to be above everybody else, but right from the start I wanted to develop that clear and distinct idea into a theory. And that's what I did, and my research work did bear fruits: not material fruits (money), but immaterial fruits that come in the form of theories and models (and together these form a *research program* as meant by Imre Lakatos). This research monograph integrates all my work into a coherent whole; it can be viewed as a revised, updated, and extended 2nd edition of my dissertation, which I defended in 2011 at the Vrije Universiteit Brussel.

I honestly believe that my work has yielded some good results or, with another word, some good news: within this research program,

(i) we have a candidate for a unifying scheme—the Elementary Process Theory (EPT)—that applies to all four fundamental interactions (gravitational, electromagnetic, weak, strong);

(ii) man has a free will—I consider that God's greatest gift to mankind, although we certainly still have to learn how to use it;

(iii) there is a cyclic process by which energy contained in the vacuum can be harvested, something that one day may be used in technology.

Nevertheless, I have experienced a resistance from the academic community that few before me have experienced. My main opponent Gerard 't Hooft foresaid that "in biblical terms", a "road of suffering" would await me if I would proceed with my research (2009). Now he has lashed out at me many times, but that was the only time he was right: I have experienced resistance to such a degree, that at some point I had the idea that God Himself was pleased with my misery. In the Netherlands alone a one-page article on my work in a university weekly led to a controversy that came to exceed the now historical controversies following the publications of the works of Descartes and Spinoza in the 17^{th} century: it is only a slight exaggeration to say that in 2008 it became a new literary genre in the Netherlands to write an attack against me and my work—note that my work remained unpublished until 2010. But the resistance I've experienced was not limited to the Netherlands: I have also encountered it in the peer review of papers submitted for publication and of proposals submitted for funding. And apart from a handful of individual exceptions, even those working on the topic of repulsive gravity didn't want to have anything to do with me. They all held me in low esteem: they put me on a par with those crackpots who, without relevant education, claim to have proven that modern physics is false and should be replaced by their pseudoscientific theories. Eventually this resistance made me give up all hope for a paid position at a university to do my research work—a hope I had cherished from the moment I started my research endeavor. And I would like to emphasize, as others have done (e.g. Charles Bukowski), that one doesn't give up hope overnight: it's a slow process, like a tree slowly losing his leaves until the very last one, until there isn't even anything left for death to take.

But of course, no one else but me provided the *casus belli* for this collision with the establishment. First of all, the starting point of my research work, the hypothesis that there exists a matter-antimatter repulsive gravity, is impossible from the perspective of widely accepted theories of modern physics. So, upon learning that I worked on a theory that could explain repulsive gravity—note that one can learn that from a superficial reading of my work or even from hearsay—many of my opponents immediately jumped to the conclusion that I didn't know the first thing about physics. Now if I would have worked on any of the open problems of a mainstream research program in physics, those very same people would have embraced me as a new member of the academic community; but as it is, I preferred to work on the problem that arises from considering that, against all odds, repulsive gravity is a fact of nature and asking by which fundamental principles this would be possible, and that fact alone led to much resistance. But I have applied existing methods for theory development, and in addition the development of the EPT has been supervised by a physicist (Sergey Sannikov): let no one say that he didn't know the first thing about physics.

Secondly, en route to identifying the principles by which repulsive gravity would be possible, the first question to be answered was: *how* does repulsive gravity take place? The answer came in the form of the EPT: it consists of seven process-physical principles that abstractly describe the elementary processes by which interactions have to take place for repulsive gravity to exist. However, the abstract-mathematical formulation of these principles—in other words: the generic description of an interaction process—required the development of a new formal language for physics. For that matter, I had to apply mathematical logic to physics: the EPT is formalized in a mathematical-logical framework and is therefore mathematically more abstract than theories of modern physics. This abstractness and the fact that most physicists are not familiar with mathematical logic did not contribute to the accessibility of my work: several opponents have therefore *from a superficial glance at the formalism* jumped to the conclusion that the mathematics in my work are not mathematics at all. But the formalisation has been done by the book under the supervision of an expert (Harrie de Swart), and once one has a grasp of the formalism one sees that at this degree of abstractness the process-physical principles of the EPT are mathematically of great simplicity.

Thirdly, for the EPT to be a theory of physics the formalism had to have a physical interpretation. For that matter, new physical concepts had to be developed and it turned out to be very hard to get it all conceptually coherent. I had to make amendments to my 2010 paper in *Annalen der Physik* in 2011 and 2016 (both were published in the same journal) but that's not all: in this monograph I present a final version of the EPT that differs from the twice revised version in *Annalen der Physik*—the differences concern only details, but still. What turned out to be the main issue, however, was not the newness of the terminology but the fact that the objects postulated to exist in the physical universe are *four-dimensionalistic*, which is to say that these are objects that are extended in time—or, in other words: these are objects that have a time span (like the life of a free neutron). Such four-dimensionalistic objects are called *occurrents*. As it turned out, I myself view the world intuitively in terms of occurrents, and it never occurred to me that others didn't. Of course I knew that the overwhelming majority doesn't look at the world through *four-dimensionalistic glasses* but through *three-dimensionalistic glasses*, which is to say that the overwhelming majority views the world in terms of *continuants*, i.e. three-dimensional objects that continuously move through time (like chairs, tables, molecules, atoms, particles, quarks, bosons, leptons, etc.). But it is only recently that I've discovered the niche in the philosophical literature with publications about four-dimensionalism—I now use the term but I know it only since recently—and so I only thenceforth understood that a four-dimensionalistic world view is completely counterintuitive for the overwhelming majority. And so I now know why several opponents of mine have espoused the view (both in public and behind closed doors) that my physical interpretation of the formalism is bizarre, gibberish, absurd, etc. So in hindsight I consider it a shortcoming of my papers in *Annalen der Physik*, of my 2007 concept-dissertation, and of my 2011 dissertation that four-dimensionalism has not been discussed, and that I have only mentioned in passing that the objects postulated to exist in the universe are extended in time: for that I apologize, and I hope to make up for it with this monograph which uses ample space to introduce the four-dimensionalistic terminology. So, now I can say that the EPT consistently describes the world not in terms of *elementary particles and interactions*, but in terms of *atomic occurrents and transitions*—the atomic occurrents are called 'phase quanta'.

Fourthly, *even if* one was willing to consider repulsive gravity, *and* had mastered the formalism, *and* had understood its physical interpretation, it was still not obvious that the EPT would withstand the test if we would check it against existing knowledge about the universe. In my earlier works (my 2007 concept-dissertation, my Annalen papers, and my 2011 dissertation) I indicated only summarily *how* the EPT could be checked against existing knowledge, but I didn't do any actual checking: that, namely, is not entirely simple. The problem is that the EPT cannot possibly satisfy the *correspondence principle* as commonly understood: due to the abstractness of its mathematical formulation it cannot possibly be proven that the EPT reduces to an existing theory (e.g. Newtonian mechanics) by applying some limit procedure—there is, thus, no easy way to show that the EPT agrees with the knowledge of the physical world that derives from the experimentally successful predictions of existing theories of physics. Therefore, I considered the development of the EPT and the development of a proof that it agrees with existing knowledge to be two distinct research projects. So, on the one hand I did briefly but exactly describe how the EPT could in principle be tested by a scientific method (refined falsificationism), but on the other hand I did not present a proof that the EPT agrees with anything that we already know. That latter fact now led several of my opponents to conclude that the EPT could be dismissed right away as not worthy of further consideration—and they were quite vocal about it, depicting my work as despicable. I consider their conclusion premature, but I understand where it comes from; so, let me apologize for having been summarily about this topic in my earlier works, and let me express my hope that this monograph makes up for it. In my postdoctoral research I have developed a new principle of correspondence, called the 'weak correspondence principle', and I have developed a rigorous proof method for it: to prove that the EPT agrees with the existing knowledge that derives from the successful predictions of an accepted theory of physics, we have to apply that proof method to show that the EPT corresponds weakly to that theory. Part IV of this monograph focusses on this theme, and it contains a completely worked-out proof that the EPT corresponds weakly to Einstein's special relativity. The finale is a model of the EPT that quantitatively models a process of gravitational interaction: this is a concrete step towards a proof that the EPT corresponds weakly to Einstein's general relativity.

That being said, while I take full responsibility for having provided the stimulus that elicited the response from the academic community, I do not agree with the things that have been said about me and my work. It is my sincere opinion, namely, that the right to freedom of opinion and expression does not imply a right to make false statements of fact. My opponents may be men of science, and it may be that none of them would have crossed a line if I would not have come forward with my EPT; but as it is, my opponents have made numerous false statements of fact about me and my work in the mass media, in pamphlets that circulated behind closed doors, and in confidential peer-review reports that served as a proof of the scientific quality of my work. And these false statements cannot be downplayed as 'honest mistakes', since my opponents have not even made an attempt to apply basic principles of good scientific practice: in their responses to my work, my opponents have merely blurted out whatever came first to mind in an emotional outburst without checking the truthfulness of their statements and without carefully reading my work—in some cases even without reading my work at all. To their defense, upon a superficial reading or learning about my work from hearsay, my opponents despised both me and my work. But for a professional scientist—who, in distinction to a layman, should be able to look beyond a first emotional reaction—that is no excuse for violating the commandment "*thou shalt not bear false witness against thy neighbor*" (Exod. 20:16). Yet that is what they did. I have let it known publicly that I do not condone such behavior, e.g. in (Cabbolet, 2008a, 2011c; Cabbolet and De Swart, 2013; Cabbolet, 2014d), but to no avail: in reply I have only been accused of accusing my opponents of 'unethical practices' because I cannot stand criticism. But false statements of fact have nothing to do with criticism. Moreover, I know how I got the idea for my theory, I know the methods of theory development I've used, I know my theory, and I know how to solve its open issues: that means that I *know* when someone has fabricated "facts" about my work. My opponents, on the other hand, do not know how I got the idea for my theory, they never looked at how I developed it, they don't know what it is, and they don't know what the open issues are or how to solve them. Nevertheless, in the grand scheme of things their wrongdoings may still serve a benign purpose: namely, as paradigmatic examples of dystopian behavior in academia they can teach future generations what to avoid.

Now according to the Scripture, Jesus forgave his opponents when he spoke the words "*Father, forgive them, for they do not know what they are doing*" (Luke 23:24). But given the widespread acceptance of principles of good scientific practice in recent decades, professional scientists can be expected to be familiar with the principle of carefulness: with regards to commenting on someone else's work, professional scientists *know* that they should first carefully study the work and that there is no 'anything goes' when commenting on it. Therefore I did—and I still do—expect my opponents to remain within the borders of a scientific discussion. And although I do not consider that an expectation that no one *can* fulfill, it turned out to be an expectation that virtually no one *did* fulfill. I have therefore filed four complaints about the behavior of my opponents at a scientific integrity committee (SIC)—nearly every university in the West has such a SIC to deal with complaints about violations of scientific integrity. But my experiences are incredibly negative: none of them—with the notable exception of the SIC of Eindhoven University of Technology—had any intention to actually investigate the violations of scientific integrity that I reported. In particular the LOWI headed by Kees Schuyt (the LOWI is the national SIC of the Netherlands co-founded by the Royal Dutch Academy of Sciences) didn't do what it should have done, to wit: investigating my complaint about scientific misconduct in the cancelation of my PhD defense in 2008 from the perspective of the principles of good scientific practice, and will deny *ad infinitum* what it actually has done, to wit: dismissing my complaint to protect the reputation of elite scientists. And this is not about subtle nuances as the following typical example demonstrates:

> "*I immediately had the impression that this is the work of a charlatan*"—Andries Brouwer, eminent mathematician, on the 2007 concept-dissertation (2008a).

> "*The judgement emailed by Brouwer ... and the choice of words therein do not provide any ground for the conclusion that this constitutes a violation of scientific integrity.*"—the LOWI dismissing my complaint about Brouwer (Schuyt et al., 2009).

Of course, an explanation of this decision by the LOWI might be that its members are of the opinion that the use of the word 'charlatan' is completely acceptable in a scientific discussion. To test that, I wrote everyone

involved a letter in which I called *him* a "charlatan": it will probably come as no surprise to you that their reactions made clear that this is *not at all* their opinion. But to the defense of Schuyt c.s., I can tell you how they have come to their decision: instead of judging *my complaint*, they asked external referees to judge *my concept-dissertation*; and having learned that they also had objections, Schuyt c.s. held me in low esteem too and they simply dismissed my complaint to prevent that the reputation of Brouwer got tainted by a complaint about a reevaluation procedure by a crackpot whose PhD graduation was rightfully canceled *even though* the procedure by which the cancelation took place might not have been optimal. This is just one example, but what I have against all these SICs who dismissed my complaints is that their decisions create a *Sadean libertinism* in academia, in which principles of good scientific practice are to be obeyed in original research that is to be made public, but can be disregarded with impunity in the back rooms of academic institutions where research on research—such as peer review—takes place that is to remain confidential. (In his novels, the philosopher and writer Marquis de Sade described a libertinism: laws are to be obeyed in the public domain, but what is allowed on private property is *entirely* to the discretion of the libertines, i.e. the property owners. I assume the metaphor is clear.)

All that being said, one might be complacent with the situation that academia finds itself in—worldwide, more PhDs are produced than ever before (Cyranoski et al., 2011), more postdocs are employed at universities and research institutes than ever before (Powell, 2015), and the annual output of research, as measured by the number of publications, is higher than ever before and continues to grow (Sarewitz, 2016)—but I am not. It is my sincere opinion that the enormous ease with which my opponents time and time again flouted the basic principles of good scientific practice in their reactions to my work is symptomatic of an academic community that has gone astray, with virtually everyone on his or her own path in a completely compartmentalized landscape of research programs guided by the desire to pursue a career. With that in mind I've added an appendix on the controversy about my work in the Netherlands to the front matter of this monograph, and I've added a section 'Objections and Replies' to every chapter of its main part: therein the wrongdoings I've encountered

are made explicit, in the hope that it leads to changes for the better—these sections contain referenced quotes from published sources and from unpublished pamphlets for which I never signed a confidentiality agreement, and anonymized quotes from confidential files such as peer-review reports. Now my opponents may cry out that these parts of my work are invalid, because I bear witness of myself. But these parts are about *my work*, not about *myself*. And I can *truthfully* discuss my work because I know my work: without passing judgment on my opponents I can demonstrate *in objective language* that their statements about my work are false, just like any professional physicist who knows Einstein's work can demonstrate in objective language that the claims by laymen about inconsistencies in relativity are false—even if you don't believe in my theory, you can still see that it is true that my opponents made false statements of fact about my work.

Of course, if one of the experimental projects at CERN would establish that gravity is attraction only, none of my work is of any value for physics. There is not a shadow of doubt in my mind that if CERN would put out a press release to that extent, then *even before the ink has dried up* my opponents will shout in national newspapers that my work has been rightfully opposed, rejected and ignored by the community. And indeed, it should then be rejected as a purely hypothetical construction with no relation to reality whatsoever—the question whether or not my opponents have fabricated "facts" when they rejected my work is then entirely moot.

However, should a repulsive gravitational force between matter and antimatter be detected, a completely different scenario unfolds. Just think about these cases that you read about in the news every once in a while, when a tribe, which for centuries has lived completely isolated from the rest of the world, comes in contact with what we call 'western civilization': then they find out that eagles are not gods, that rabbits are not creatures that can turn into stones, etc. And when they realize that, their world view collapses: everything they ever believed in turns out to be not true. That is one of the most serious things, if not *the* most serious, that can happen to a group of people. Interestingly, if repulsive gravity were to be detected experimentally at CERN, then this would happen again: not in some remote area of the Third World, but right here in the modern West. Then the world view of modern physics collapses: then all of a sudden general relativity and modern quantum physics are not true, and then there are no

Higgs bosons, no virtual particles, etc. Then it will be clear for all to see that modern society, not unlike the prediction in 2 Tim 4:3–4, has on the whole gathered itself around teachers, who are completely alienated from truth finding, and who out of self-interest have indoctrinated their students with the fables of a false religion—I'm referring here to these constantly reiterated overstatements in physics, the epitome of which is the downright hubristic claim that the 'God particle' has been observed, which I consider to be a category mistake (Cabbolet, 2018a). And make no mistake, the shock will then be just as big for modern society as it has been for those isolated tribes. In that case, and *only* in that case, my work *might* yield an advancement in our knowledge of the fundamental workings of the universe: then apart from some individual exceptions, the academic community has been *collectively mistaken* in its reaction to my work—even stronger, then the academic community has fulfilled the prediction in Matt 24:24 that false prophets will arise who will mislead the people on earth by showing great signs (such as the "image of a black hole" in 2019, which I consider to be the same category mistake as the Higgs claim). If I live to see it, then I would be happy to discuss which reforms have to be carried out in academia to restore truth finding as the highest value with those who agree with me that one chooses academia to dedicate one's life to truth finding—not to make a career that yields a high social status, not to be able to go to luxury conference resorts on tax-payers' money twice per year, and not to merely earn a living. These reforms will then have to be pushed through by external 'power elites'—be it kings, presidents, or ministers, but somewhere at that level—but that's another story.

As a final word, it is written that no one lights a lamp and puts it under a basket, but rather on a lampstand (Matt. 5:15). By publishing this non-peer-reviewed treatise as an open access monograph, I am putting my lamp on a lampstand: in God I trust that others will see my work.

Marcoen J.T.F. Cabbolet, PhD

Outline

> "*[A]nother person who went off the rails whilst pursuing a PhD … was Marcoen Cabbolet and his 'Elementary Process Theory' claiming that linguistic axiomatic logic is a gateway to new insights in physics. … The quality of contemporary academic analytical philosophy is apparent from the fact that this rubbish got published.*"—Harry Hab on crackpotwatch.wordpress.com (2018)

Although there is currently no indication that this monograph will be widely read—if it will be read at all—I have made an effort to make it the most accessible exposition of my work so far for those interested in the fundamental workings of a universe with repulsive gravity. However, a certain background in physics, philosophy, and mathematics is prerequisite:

(i) in philosophy: historical mainstream ideas, in particular Kantian philosophy; formal logic and axiomatic set theory; general philosophy of science (Popper, Kuhn, Lakatos);

(ii) in physics: thermodynamics; classical mechanics; axioms of special and general relativity; postulates of quantum mechanics;

(iii) in mathematics, besides the prerequisites for (ii): category theory; abstract algebra (groups, rings, fields); differential geometry.

As to the level, although I have, unfortunately, seen several undergraduate and graduate students commenting negatively on my work, I have never seen any (under)graduate who actually understood it—so, this work is best placed at postgraduate level. That being said, the main body of this monograph is divided into four parts. The remainder of this outline is to briefly discuss the contents of these parts.

Part I covers the introduction and the method by which the Elementary Process Theory (EPT) has been developed from the perspective of physics. The titles of the three chapters in this part refer to the phases in which René Descartes developed new knowledge in his *Meditations*.

Chapter 1 is the foundational phase: while Descartes developed an undoubtable truth—his *cogito ergo sum*—in this phase, here it will be shown that (massive) antiparticles must have positive inertial mass and negative gravitational mass under the condition that a matter-antimatter repulsive gravity is a fact of nature. This proof has already been given in the 1950s, but the controversy surrounding my work has made abundantly clear that physicists, as a rule, are not familiar with the material. In addition, the currently ongoing experimental projects, aimed at verifying whether or not massive antiparticles have the said properties, are briefly discussed.

Chapter 2 is the destructive phase: while Descartes rejected then existing knowledge in this phase—which in his work *preceded* the foundational phase—by applying radical doubt, here it is shown that modern interaction theories have to be rejected if repulsive gravity exist, because antiparticles cannot have the aforementioned combination of properties in the frameworks of these theories. In addition, this chapter engages in the destruction of pseudoknowledge by proving that it is simply not true that ultrashort-lived unstable particles postulated by the Standard Model (such as a Higgs boson) have been "observed". A short version of the argument has eventually been published in *Mod. Phys. Lett. A*, see (Cabbolet, 2018a), but the section Objection and Replies extensively treats the toe-curling arguments with which the publication has been held up for years.

Chapter 3 is the constructive phase: while Descartes extended his system in this phase, here the crucial steps are highlighted that were taken on the path from a clear and distinct idea about the fundamental workings of the universe towards an axiomatic theory about elementary processes. New physical concepts are introduced with the help of the more common terminology of *four-dimensionalism*—to keep the text self-contained, its basic notions like continuants, occurrents, and temporal parts are defined in Sect. 3.2—and the terms and predicates of the new formal language for physics, in which the EPT is expressed mathematically, are extensively discussed. This material is largely absent in the first edition and in the papers in *Ann. Phys.*

Part II axiomatically introduces the EPT as a collection of generalized process-physical principles that give an abstract yet exact description of what has to happen at supersmall scale in the individual processes by which interactions take place for matter-antimatter repulsive gravity to exist.

Chapter 4 introduces a finitely axiomatized nonstandard theory $\mathfrak{T}$—a postdoctoral research result published in (Cabbolet, 2021a)—that provides the mathematical foundation for the use of abstract constants as terms of the formal language in which the EPT is expressed. Therewith this 2nd edition differs from the 1st edition, in which this role was fulfilled by a generalization of ZF called 'set matrix theory'. The thing is that we have to learn from the controversy on my work: we will have to accept that set matrix theory, as published in (Cabbolet and De Swart, 2014), is *correct* but mathematically *not interesting*—after all, it is not stronger than ZF. The nonstandard theory $\mathfrak{T}$, on the other hand, is *stronger than* ZF—it will be precisely defined what that means. Readers who are mainly interested in the EPT and its applications can, in principle, skip this chapter largely: the EPT should be understandable after reading the informal introduction to $\mathfrak{T}$ in Sect. 4.1—the takeaway point is that in the framework of $\mathfrak{T}$, *sets* and *functions* are ultimately different things.

Chapter 5 introduces the EPT. The exposition integrates my three Annalen papers, to wit: *Ann. Phys.* **522**, 699-738 (2010); **523**, 990-994 (2011); **528**, 626-627 (2016), into a coherent whole, but with some minor revisions. First of all, to shorten the mathematical formulation of the EPT, its axioms are now expressed as *open formulas* instead of *closed formulas*. Secondly, minor changes have been made to the formalism: constants have been added that refer to monads, a unary existence predicate $\mathbb{E}$ has been added so that formulas of the type $\mathbb{E}\left[\begin{array}{c} t \\ \bar{t} \end{array}\right]$ replace formulas of the type $\left[\begin{array}{c} t \\ \bar{t} \end{array}\right] \in M_E$, and formulas of the type $\left[\begin{array}{c} t_1 \\ \bar{t}_1 \end{array}\right] \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \left[\begin{array}{c} t_2 \\ \bar{t}_2 \end{array}\right]$ replace formulas of the type $\left[\begin{array}{c} 0 \\ 0 \end{array}\right] : \left[\begin{array}{c} t_1 \\ \bar{t}_1 \end{array}\right] \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \left[\begin{array}{c} t_2 \\ \bar{t}_2 \end{array}\right]$. Thirdly, the axiomatization of the EPT has been modified: an existential axiom has been added, and two former axioms have been merged into one new axiom. And last but not least, the term 'monad' has finally been given a satisfactory definition. This finalizes the EPT: nothing should be added to it or taken away from it.

Part III shows how the generalized process-physical principles of the EPT apply to non-relativistic physics and to the mind-body problem in philosophy. Language from systems theory is used to make the connection between the language of the EPT and the languages of existing paradigms: on the one hand, every physicist knows what is meant by a 'system made up of one electron'; on the other hand, we can describe a one-component system with the ontology and principles of the EPT.

Chapter 6 "does" non-relativistic physics in the framework of the EPT. To start with, Sect. 6.1 introduces the notion of a 'monadic system' in the formal language of the EPT, and interprets it in the language of systems theory as a one-component system. Sect. 6.2 models non-relativistic monadic systems: this gives a concrete view on what building blocks of the outside world generally referred to as "elementary particles" (e.g. electrons) are in the framework of the EPT. Next, Sect. 6.3 develops a semi-classical model of interactions by letting a monadic system evolve in an environment that can be described by classical mechanics: by quantitatively modeling the interaction between the system and the surrounding fields, this provides a concrete and quantitative answer to the question how the processes described by the EPT can be viewed as processes of interaction. Sect. 6.4 shows that the EPT, which in Ch. 5 has been proven to be *inconsistent* with orthodox quantum mechanics, is *consistent* with ψ-epistemic quantum mechanics: in the framework of the EPT, a 'quantum system' is deterministic under the surface. The final part develops principles of a quantum field theory for a free particle in the framework of the EPT. This chapter presents results of post-doctoral research that are absent in the first edition: less abstract than Ch. 5, this makes it easier to understand the EPT.

Chapter 7, on the other hand, applies the ontology and principles of the EPT to a physicalist approach to the mind-body problem. In the present-day compartmentalized scientific landscape this topic is seemingly disconnected from physics, but it is not: if an intentional thought can cause a bodily action, which is a physical action, then of course this must happen according to some physical mechanism. The present exposition differs mainly from the first edition by the inclusion of a preliminary discussion of the intelligent neutron: this oversimplified example of a system with free will is to quantitatively illustrate the rather abstract main idea. A second difference is that the work of Benjamin Libet is (briefly) discussed.

Part IV addresses the correspondence relation between the EPT and the body of existing knowledge of the physical world that derives from the successful predictions of theories of modern physics. All material in this part results from post-doctoral research, and is therefore absent in the first edition and in the papers in *Ann. Phys.*

Chapter 8 outlines the general research program aimed at demonstrating correspondence between the EPT and existing knowledge. Sect. 8.1 first precisely defines the nature of the correspondence relation that is to be proven: here the *weak correspondence principle* is introduced, which determines a new intertheory relation in physics. Next, the method is described by which the new correspondence relation is to be proved: in a nutshell, one proves that the EPT corresponds weakly to an existing physical theory T by specifying a categorical model $\mathscr{C}$ of the EPT—this notion will be precisely defined—and proving that $\mathscr{C}$ reduces empirically to T, that is, that $\mathscr{C}$ reproduces the empirically successful predictions of T. From there it is explained what the proposition 'the EPT is a Grand Unifying Scheme' means: to (dis)prove that proposition is the ultimate aim of the research program. Finally, Sect. 8.2 is devoted to the philosophy of the noumenal and the phenomenal in the framework of the EPT, which is related to the theory/model distinction: these are notions that also have a meaning in Kantian philosophy, and the difference in meaning calls for a discussion.

Chapter 9 applies the method set forth in Ch. 8 by completely specifying a categorical model of the EPT incorporating Special Relativity (SR), which rigorously proves that the EPT corresponds weakly to SR, that is, that the EPT agrees with the knowledge of the physical world obtained from the successful predictions of SR. Sect. 9.1 introduces new nonstandard mathematics: Dirac delta functions are defined as ordinary hyperreal functions of real variables. This is a result of postdoctoral research, which has been published in (Cabbolet, 2021b), and which may have applications beyond the research program on the EPT. The remainder of Ch. 9 is admittedly a tedious exercise, but it nevertheless provides a worked-out example of how to apply the proof method developed in Ch. 8.

Chapter 10 introduces a fundamentally new relativistic model of a process by which a gravitational interaction takes place between a massive system and its environment: this model predicts a matter-antimatter repulsive gravity. The exposition is self-contained, and offers an introduction

to the EPT (Sect. 10.2) that is accessible without having read the preceding chapters. This version of the EPT is admittedly weaker than the full version of EPT introduced in Ch. 5 since it doesn't cover processes in which nuclear reactions (fusion, fission, decay, etc.) take place, but it has the distinct advantage that the physical interpretation of the formalism is expressed in the more accessible language of systems theory. The model of a gravitational interaction process presented in this chapter (Sect. 10.3) is a straightforward model of the EPT: as such it's devoid of mathematical elegance, but it does the trick in that it expresses principles of Planck-scale gravitational physics in a generally covariant fashion. The discussion (Sect. 10.4) has been kept to a bare minimum: let's talk further if and when repulsive gravity has been detected.

Physicists primarily interested in a quantitative model of repulsive gravity may read Ch. 10 as a stand-alone essay. Those who want to get acquainted with the research program on the EPT are recommended to read at least Ch. 1-3, Sect. 4.1, Ch. 5-6, Sect. 8.1, and Ch. 10.

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Appendix: on the controversy about my work

> "*Marcoen Cabbolet mentions Brian Martin's work on dissenting views. Me: One person's dissident is another person's crank*"—
> Matt Hodgkinson, head of research integrity at Hindawi (2017)

In 2011 I defended my dissertation *Elementary Process Theory: axiomatic introduction and applications* at the Free University of Brussels (VUB). The theory to which the title refers finds itself opposed by the prevailing ideas about the outside world that are usually referred to by the term 'modern physics': consequently, this theory should not be seen as a contribution to modern physics, but rather as the hard core of a fundamentally new, potentially progressive research program in physics—here the terms 'hard core', 'progressive', and 'research program' are used in the sense meant by Lakatos (1970). And because traditional criteria of quality (mathematical rigor, logical consistency, conceptual coherency, experimental testability) were satisfied, all conditions for a PhD graduation were satisfied in the eyes of the PhD committee.

Viewed in itself this is just another PhD graduation, but it happened to be the case that in January 2008 already a PhD graduation had been planned at Eindhoven University of Technology (TU/e) on the basis of virtually the same dissertation: since then the presentation has been changed a little, but not so the research result.[1] This earlier PhD graduation, however, got canceled one week before the planned date after an unusual *reevaluation* of the already approved concept-dissertation. The outcome of this reevaluation was that "the scientific quality of the research results did not justify a PhD graduation at the TU/e": those who were involved in that

reevaluation concluded that pejoratives like "all nonsense", "devoid of content and devoid of meaning", "utterly unacceptable" and "a disgrace" were applicable to my work. The reevaluation of my 2007 concept-dissertation played a pivotal role in the controversy on my work, which erupted in 2008 and quickly turned into a public display of the worst impudences in science.

That said, of course there are differences between universities and faculties concerning the conditions that must have been met in order to obtain a PhD degree, but these differences are minor: there is no university where one can obtain a PhD degree on the basis of research results of bad scientific quality. This is also true for the VUB: it is not the case that I obtained a PhD degree there *despite of the fact that* my research results were of bad scientific quality. What *is* the case, however, is that the conclusions of the reevaluation of my concept-dissertation at the TU/e are completely false, yet in the decision-making process leading to the cancelation of my PhD graduation these have been uncritically treated as "*statements of fact*" that compromised the quality of my work.

The *why* of this affair is easy to see. To put it mildly, those who were involved in the reevaluation of my 2007 concept-dissertation saw nothing that they valued at first glance: I hadn't followed a PhD program in physics at any university they thought highly of, and my work hadn't yielded an advancement in pure mathematics, nor in modern physics—the EPT even *contradicted* general relativity and the standard model. So to their defense, they have concluded *from there* that my work did not even remotely qualify as a scholarly work, and they have acted accordingly to prevent that it would be officially published as a dissertation.

I have nothing against safeguarding the quality of publications—I even find that a laudable aim—but what I hold against my opponents is that they have *jumped to conclusions*: this is a *malpractice* that goes squarely against all principles of good scientific practice. So it is true that the controversy on my work fits in the tradition in the historical development of physics that new ideas first encounter fierce resistance, but neither the reevaluation of my 2007 concept-dissertation nor the public outcry that followed had anything to do with a scientific discourse—and in this case the truth does not lie in the middle. So, the purpose of this appendix is to expose the wrongdoings in the back rooms of academic institutions by giving an account of the events that led to the controversy about my work.

Prehistory

Originally educated as a physical chemist, at the end of 1996 I was drawn to physics, more specifically: to the foundations of physics, by a clear and distinct idea about the fundamental workings of the physical universe. Through a badly written letter and with some fortune I came in contact with Sergey Sannikov, a Ukrainian physicist who worked at the Institute of Theoretical Physics (ITP) of the Kharkov Institute for Physics and Technology (KIPT). He, Sannikov, was willing to supervise the further development of that idea and he set forth a tailor made study program in physics, mathematics, and philosophy to prepare me, a physical chemist, for research in the foundations of physics. I became a PhD student at the KIPT, and I did succeed in *exactly* identifying a physically complete set of generalized principles that would be universally valid if the aforementioned clear and distinct idea were to be correct. I also succeeded in formalizing these principles in the form of a first-order theory, which I called 'Elementary Process Theory' (EPT) since the generalized principles were process-physical. The plan was to publish this all as a PhD thesis at the ITP KIPT, but Sannikov got severely ill. He eventually died on March 25, 2007—see the obituary in the first edition of this book.

Since I already had the results, I wrote to some universities in the Netherlands (where I lived) asking if there were any perspectives. That way I came in 2005 in contact with the mathematical logician Harrie de Swart of the Center of Logic, Ethics, and Philosophy of Science of Tilburg University, who advised me to reformulate the EPT—which up till then was formulated as 'just' a first-order theory—in mathematical language. De Swart supervised this reformalization of the generalized process-physical principles in a very, very meticulous way: in its final version, the newly developed formalism was free of errors. I was then offered the possibility to defend the end result as a PhD thesis at Tilburg University, and so I became an *external PhD candidate* at the Philosophy Department of Tilburg University in 2006.[2] A PhD committee was established, and all but one of its members approved the dissertation: a Hungarian member abstained from voting because he thought a dissertation should be based on three published papers—which is required in Hungary but not in the Netherlands. However, one of the committee members who approved the dissertation, the physicist-turned-philosopher Stephan Hartmann, demanded some changes

to the text. Truth be said: most of his comments were very good, and have been incorporated in the dissertation that was finally to be defended in 2011. But he also demanded that I dealt with his following comment:

> *Why this assumption of repulsive gravity? Why not developing a theory from the assumption that a distant planet is made of green cheese?—Cabbolet (2008b)*

Rightly or wrongly, that came across to me as *academic bullying*. I let him know that there was no way that I was going to answer that question in my dissertation, but he insisted. I then thought to myself: "I rather have no PhD degree than having to crawl on my knees for that bully." And so, I withdrew the dissertation at Tilburg University with the clearly stated reason that I felt that the behavior of Hartmann was inappropriate (although it had nothing to do with 'scientific misconduct').

Events leading to the cancelation of my PhD graduation

Without further delay I then turned to the TU/e, where I became an external PhD candidate at the faculty of Mathematics and Computer Science in 2007. At the end of that year the submitted thesis was approved unanimously by the PhD committee and a date (January 16, 2008) was set for its public defense as a dissertation in applied mathematics. Upon approval of the dissertation I received, still in 2007, the standard request by the university weekly *Cursor* to do an interview. I agreed, and a one-page article about my work appeared in the last issue of 2007 of Cursor. I was asked by the journalist, Enith Vlooswijk, if I thought my theory was going to be immediately accepted, and so in the article I was quoted to have said

> "*It is absolutely excluded that my theory will be accepted immediately. It necessitates, namely, the rejection of quantum mechanics. But the research programs based on quantum mechanics will not be terminated just like that. That requires convincing experimental evidence supporting my theory.*"—(Vlooswijk, 2007)

With the sentence "It necessitates, namely, the rejection of quantum mechanics" I meant to say that *if you would accept my theory, you'll have to reject quantum mechanics*—these two theories are, namely, incompatible. I believe that this is clear from the context. But get this: there were those

at the TU/e and beyond who took this one sentence out of its context and interpreted it as if I was claiming that my theory renders quantum mechanics obsolete so that it has to be rejected. To their defense I admit that the sentence, when viewed apart from its context, can be read that way. But the point is that not a single one of them has ever asked me if he interpreted things correctly: it was not at all what I meant, but they had already judged me—for them I was a crackpot who lacked all sense of proportion and sought publicity to claim that modern physics had to be replaced by his own theory. Even eight years later, in 2016, I was still confronted with this delusion: at a conference in Varna, Dennis Dieks (Utrecht University) started to accuse me that I "had given an insane interview"—he too had jumped to conclusions on the basis of the above misinterpretation of that one sentence. Anyway, in December 2007 one of those who misinterpreted this one sentence that way was Kees van Hee, the then Dean of the faculty of Mathematics and Computer Science: he summoned me for a private meeting on January 4, 2008. During our conversation it became clear to me that Van Hee would do everything in his power to have the PhD graduation canceled: a religious person would say that Van Hee behaved as if the devil had entered into him. And indeed, later that day Van Hee distributed a circular at the faculty in which he explicitly indicated that he didn't want this PhD graduation to take place:

> "*In my opinion, this dissertation does not belong at our faculty ... [Marcoen] **has never had a PhD position ... and will use this dissertation as evidence of his skills. He already did that in his interview with Cursor. But it is highly likely that he will seek more publicity with his PhD graduation**. And because the physical consequences of his theory are far-reaching, there will probably be attention by the media. By granting Marcoen a PhD degree for this work, we as university in fact indicate that we believe in his theory, and that may lead to considerable damage for our reputation ...*" (emphasis added)[3]—(Van Hee, 2008b)

Within days thereafter, Van Hee had organized—entirely outside the university's PhD regulations—a *reevaluation* of the concept-dissertation as a dissertation in pure mathematics by his close colleagues Jos Baeten and

Andries Brouwer.[4] The comments by Brouwer amounted to nothing but a psychotic rant: it cannot be excluded that Van Hee *arranged* with Brouwer that he would write a scathing review. The following comment, as well as the opening quote of the Preface, stands model both for the language used by Brouwer and for the depth of his comments:

> "*He doesn't know mathematics and doesn't understand it. His style is not the mathematical style. That means that the book improves substantially if [the mathematical chapter] is thrown out. (I think that further improvement can be accomplished by deleting the rest too.)*"—(Brouwer, 2008b)

Remark I It would be a mistake to treat someone who uses this kind of language with respect. Also, the first two sentences are false statements of fact as *objectively proven* by the later publication of the subject matter in a recognized peer-reviewed journal, cf. (Cabbolet and De Swart, 2014). ■

Baeten, on the other hand, made the following comments on the mathematics in the concept-dissertation:

> "*He presents an alternative to ZF based on matrices. It seems to me that one can get this consistent, so it is probable that this can be done. But I see no reason why this is necessary. Matrices can be simply defined in ZF, and I see no reason why this wouldn't be sufficient for the author. Viewed in itself this is a mathematical exercise that has no scientific value.*"—(Baeten, 2008)

Remark II Reading "has no scientific value" as "does not constitute an advancement in pure mathematics", the first, second and last sentence of the above excerpt of Baeten's comments are more or less correct: the alternative to Zermelo-Fraenkel set theory (ZF) was correctly introduced but it does not advance *pure mathematics*. But I cannot agree with the third and fourth sentence: there was, nevertheless, a clearly stated motivation for the introduction of these mathematics—namely, to solve a *philosophical* problem with the formalization of the process-physical principles. If that wasn't clear from the text it would have been easy to explain that, but I have never been asked to: Baeten too had already judged me.[5] ■

Van Hee, having obtained the two pamphlets—I refuse to call these "peer-review reports"—written by Brouwer en Baeten, immediately used his administrative power as a Dean to officially declare the PhD committee that had approved the concept-dissertation "not authorized", and then lobbied the university administration with these two pamphlets claiming that they proved that the concept-dissertation was of insufficient mathematical quality.[6] The administration went along with Van Hee, and took the ad hoc decision to postpone the PhD graduation indefinitely.

Remark III Note that Van Hee was lying through his teeth when he lobbied the university administration: the psychotic rant by Brouwer proved nothing, and Baeten's comments at best proved that the fact that the concept-dissertation did not constitute enough of an advancement in pure mathematics to grant a PhD degree on that basis. But that does not imply that the concept-dissertation was of insufficient mathematical quality: that's a false statement of fact by Van Hee. For comparison, Einstein has applied differential geometry in his theory of general relativity, without advancing the field of differential geometry in pure mathematics. But that does not imply that Einstein's GR is of insufficient mathematical quality.[7]■

Van Hee then summoned me for a second private meeting on January 8, 2008—one week before the planned PhD defense. I was informed that the PhD graduation was postponed indefinitely because my work was of insufficient mathematical quality. The two pamphlets were kept secret—I didn't know about their existence until later—and I was also informed that there was no formal possibility to object to the decision. Furthermore, Van Hee told me that his next step was to have the concept-dissertation reevaluated as a dissertation in modern physics by the Nobel laureate Gerard 't Hooft (a former classroom mate of Van Hee) and by his acquaintance Boudewijn Verhaar. He first contacted 't Hooft, who would later declare the following:

> "*When I heard the description [of the dissertation] by phone I already got a very bad feeling. I have been asked specifically by the Dean [i.e. Van Hee]: how are the physics? It had already been looked at by mathematicians and their finding was that the mathematics in the dissertation were below par.*"—('t Hooft, 2008b)

This clearly indicates manipulation by Van Hee: when contacting 't Hooft,

Van Hee didn't ask him for an impartial review, but rather instigated him to give a negative review.[8] The thus unsurprising comments by 't Hooft on the physics in the concept-dissertation came in two days later:[9]

> "*As expected, the 'theoretical physics' of this work are of the same standard as the mathematical part in the beginning, if not worse. ... A lot of 'formulas' are put forward, but I fail to see what the physical relevance thereof is. Here and there the author tries to say something about quantum mechanics and general relativity, but the few concrete statements are blatantly false. He is, for example, of the opinion that antimatter will be repulsed by the earth's gravitational field. That would completely contradict everything General Relativity stands for, and for me that can only indicate that the author has not the slightest clue of what antimatter is. Also, he talks about a 'phase quantum' for quantum mechanics, but it remains unclear how that notion has been derived and what it means physically. Summarizing, the actually existing problems concerning the unification of quantum mechanics and general relativity remain untouched, and the few concrete statements about physical phenomena do not demonstrate much understanding of these topics. Of course you can throw both QM and GR overboard, as the author seems to be doing further on, but then you are left with nothing and the author does absolutely not indicate how concrete questions about nature should then be answered by 'EPT'.*"—'t Hooft (2008c)

Remark IV 't Hooft passed off three false statements of fact about the physics part as genuine findings of an evaluation of my work.

Falsehood #1 is the claim by 't Hooft in lines 10-11 that I do not have the slightest clue about what antimatter is: this is passed off as a "fact", but it is false. The two preceding premises are correct: it is more or less correct that I, as stated in lines 7-8, am of the opinion that antimatter will be repulsed by the earth's gravitational field—it would have been better to state that I *hypothesized* it, but alas—and it is correct that this goes against General Relativity as stated in lines 8-9. But that doesn't make it a fact that I do not have the slightest clue of what antimatter is: right there, that's where 't Hooft is making stuff up. The truth is that I'm well

aware of the arguments against repulsive gravity, but it just happened to be the case that I found it more interesting to consider the case that repulsive gravity *nevertheless* exists and to think through the consequences thereof.

Falsehood #2 is the claim by 't Hooft in lines 11-12 that I'm talking about a 'phase quantum' *for quantum mechanics*: this too is passed off as a "fact", but it is indicative of a gross misinterpretation. The truth is that the EPT is **not** a quantum theory, and that the notion of a 'phase quantum' is a primitive notion in terms of which the physical interpretation of the formalism of the EPT is expressed. And that means that the next comment in lines 12-13, that it is unclear how it has been derived, misses the mark completely: being a *primitive notion*, the notion of a 'phase quantum' is not a *derived notion* that stems from something else.

Falsehood #3 is the claim by 't Hooft in lines 19-21 that I didn't indicate how the EPT applies to real world problems: again this is passed off as a "fact", but it is false. The truth is that it has clearly been indicated how the EPT can be tested by the scientific method (refined falsificationism): this involves standard techniques such as developing a model of a first-order theory and then deriving testable predictions from the model.

Last but not least, 't Hooft's comments "Summarizing ... these topics" in lines 13-17 clearly indicate that he misinterpreted this work as an attempt to unify QM and GR—a topic where he himself is working on. Summarizing, the actual research question for this work has remained untouched by 't Hooft's comments, and the few concrete statements about my work do not demonstrate much understanding of it. ■

Remark V Fact of the matter is also that 't Hooft emailed his comments at 12:15 pm to Van Hee. His opening sentence was: "The booklet came by mail today. I've had a look at it". Assuming that he arrived at his office at around 09:00 am (which would be normal), the conclusions are thus (i) that he came with his judgment within three hours after having begun to read the work, and (ii) that he ignored Thomas More's advice from his 1516 book *Utopia* that it should be forbidden that a man reacts to a piece on the same day that it is submitted to him, to prevent that he blurts out what first comes to mind and then spends all his energy in defending his own initial reaction under disregard of the common interest. ■

Remark VI On November 5, 2008, 't Hooft gave a lecture at the TU/e, after which he was asked about his role in the cancelation of my PhD graduation; his answer was that this cancellation was "justice" for someone who had "desecrated" the existing laws of physics (Konings, 2008b). This clearly indicates that he has reacted in an emotional outburst. When I started to investigate the assumption of repulsive gravity, I have never intended to desecrate anyone's belief system, nor have I ever had or shown any disrespect for any of those physicists who believe that gravity is attraction only. I did, however, develop a disrespect for the physics community, but only after years and years and years of being confronted with abusive peer-review reports, in which false statements of fact were passed off as genuine conclusions of a serious assessment of the quality of my work. ■

Van Hee wanted to keep the pamphlets written by Baeten, Brouwer, and 't Hooft secret, but he failed: all of a sudden I had all three pamphlets in my possession—I appeal to the *quantum tunneling effect*. Having read them, I felt that my PhD graduation—and therewith my career perspectives in science—had been canceled by an unfair judgment. And so I published an open letter to the Rector Magnificus of the TU/e, Hans van Duijn, in the university weekly Cursor. I clearly stated that my opponents were offering great cry but little wool, and I urged Van Duijn to invoke *audi alteram partem* (2008a)—I deliberately chose the format of an open letter to make sure it didn't "disappear" in a drawer in some back room of the TU/e.

In the meantime also the comments by Verhaar had come in.[10] He had the following to say about the physics part of the concept-dissertation:[11]

> "*After reading the physics part of the dissertation of Cabbolet I have arrived at the conclusion that I cannot make much sense of it. What he calls the Elementary Process Theory is extremely vague. There are no clearly defined theoretical axioms and the relation with experimental data is as good as absent. The reasoning doesn't lead to any new qualitative insight or to quantitative, experimentally verifiable predictions.*"

Remark VII From the opening sentence by Verhaar, as well as from the comments by 't Hooft, it is clear that a superficial c.q. one-time reading is not enough to get a grasp of the EPT—but of course I already knew

that from the members of the PhD committee. This is also true for other physics theories: try to find someone who immediately understood quantum mechanics the first time he read it. That said, the last two sentences are false statements of fact that Verhaar has passed off as genuine findings.

First of all, the axioms of the EPT are precisely formulated in mathematical language: that was also the case in the concept-dissertation and even a monkey could have seen that. Second, the relation with experimental data is there: some observed particles and processes have been formalized in the framework of the EPT. The aim of the PhD project, however, was to identify principles that underlie repulsive gravity: its aim was thus not to prove that these principles agree with experimental data in every aspect. So, similar to what is the case with 't Hooft's comments, the actual research question for this work has remained untouched by Verhaar's comments. As to Verhaar's last sentence, the material *does* lead to a new qualitative insight: namely, insight in the individual processes by which interactions have to take place for repulsive gravity to exist—note that this insight remains true even if repulsive gravity turns out not to exist. And for the falsehood of Verhaar's claim of non-verifiability, see Rem. IV.■

Having obtained the pamphlets written by 't Hooft and Verhaar, Van Hee immediately lobbied the university administration to get them to cancel my PhD graduation, claiming that these latter two pamphlets proved that the physics in the concept-dissertation were of insufficient quality too. Van Duijn, head of the university administration, ignored my open letter in Cursor in which I urged him to apply *audi alteram partem*: the administration went along with Van Hee, and canceled the PhD graduation without giving me the possibility to react to the objections against my work and without giving me the possibility to appeal to the decision. Thereupon the TU/e published a press release stating that my PhD graduation had been canceled because eminent scientists had concluded unanimously that my work was of insufficient quality.

To the defense of the university administration, it is altogether of course the case that they saw themselves two times in a row confronted with the uncomfortable situation that the Dean Van Hee waved pamphlets written by eminent scientists as evidence that the already approved concept-dissertation contained grave errors and that the PhD graduate was grossly

incompetent. So I do understand the panic. But what I hold against them is that they have *uncritically presumed* that the four pamphlets were scientifically accurate and that they have *equally uncritically presumed* that Van Hee was accurate with his explanation on how these comments have to be interpreted: these presumptions rest on an argument from authority, and thus embody a well-known fallacy. They should have applied *audi alteram partem*, a well-established principle in discourse ethics—as Habermas put it: every speaker knows intuitively, that an alleged argumentation is not a serious argumentation, when for example certain participants are not admitted (1991). And make no mistake: the university administration did see my open letter in which I had urged them to apply this principle.

The public controversy: the pack effect

After it became public news that my PhD graduation had been postponed and later canceled because eminent scientists, among whom the Nobel laureate 't Hooft, unanimously concluded that the already approved concept-dissertation was of insufficient scientific quality, the biggest public controversy about a PhD in Dutch history erupted, accumulating some 150-200 publications over the years 2008-2020—thereby even surpassing the now historical controversies following the publications of the works of Descartes and Spinoza in the 17$^{\text{th}}$ century.

The Nobel laureate 't Hooft declared my work *anathema* in a number of public attacks on my work ('t Hooft, 2008b; Konings, 2008a,b; Scholtens, 2008a; Hover, 2008b; Hardeman, 2008)—note that my work had not even been published at that time. To the defense of 't Hooft, I can imagine that the thoughts have come up in his mind that I don't know the first thing about physics and the scientific method and that my work is *illucid*, that is, so far beyond unintelligible that it defies classification: he has, namely, seen that I consider repulsive gravity in my work—which is strictly forbidden by theories of modern physics—and in addition he has failed to get a grasp of the EPT and the method of testing from a first read.[12] But what I hold against 't Hooft is that he has passed off these figments of the mind in the mass media and in the pamphlet that he wrote for Van Hee *as if* these are facts about me or my work: although it isn't *intensional deceit*, fact of the matter is that his comments are outside the framework of a scientific

discourse. For comparison, suppose that archeologist Jones decides to do an excavation on location X, and that archeologist Williams doesn't believe that anything is to be found there. Of course, Williams can then write an opinion piece about *why* he thinks nothing can be found there. But he does not have the right to declare in national newspapers that Jones is a crackpot who knows nothing about archeology and its methods.

Proceeding, others followed the example set by 't Hooft, thereby proving the existence of what Feynman called the 'pack effect': there was no upper limit to the creativity with which "facts" were made up to mock my work, nor to the disrespectfulness that was implied by the language with which this mockery was expressed.[13] For example, within hours after things became publicly known, a Wikipedia page had been created by Fred Lambert, lecturer at the TU/e and self-proclaimed crusader against pseudoscience, where the following was passed off as "encyclopedic knowledge":[14]

> "*Even before his PhD graduation Marcoen sought publicity in the university weekly Cursor of the TU/e. In an interview ... he compared himself with Isaac Newton ... [Gerard 't Hooft] pointed out the vagueness of most parts of the manuscript, and the inconsistency of the verifiable proofs.*"—Lambert (2008)

Remark VIII Note that it is not true that I sought publicity, nor that I compared myself to Newton, nor that 't Hooft has pointed out any inconsistency: Lambert was one of those at the TU/e who misinterpreted my quoted statement in Cursor as discussed on page xxxii and he uncritically accepted the public comments by 't Hooft as "facts". ■

Another example is the following, which was stated by Frank Witte, then lecturer at Utrecht University, under the pseudonym 'Darth Tutor' on the forum of the Dutch national newspaper *De Volkskrant*:

> "*Cabbolet's PhD graduation? The TU/e and Tilburg University should be ashamed of themselves. ... Unbelievable! What a blooper.*"—Witte, 2008

Remark IX Witte never read or even saw the concept-dissertation: like Lambert, he uncritically accepted the public comments by 't Hooft as "facts" about me and my work. ■

These are just two examples, but altogether my opponents know for sure that I am a crank, a crackpot, a charlatan, a pseudoscientist, and a fuck-up, that I know nothing of discrete transitions nor of quantum mechanics, that I had likely bribed the PhD committee, and that I'm out to intentional deceit, that my work is nonsense, based on nonsense, full of mistakes, of sophomore level, below masters level, and so on and so on—all of this was claimed by authors with a university degree (MSc or PhD) in public media or in pamphlets that they had circulated behind closed doors. And those of my opponents who did not publish an attack themselves praised others who did, or praised the administration of the TU/e for their courage to cancel the PhD graduation in such a late stage. Some examples:

> "*Calm but clear, devastating criticism of the fake PhD graduation of Cabbolet*"—MSc physics and science journalist Bruno van Wayenburg (2011), twittering his praise for the hostile opinion piece by Van Joolingen (2011c)

> "*It is evidence of a strong resolve that the TU/e has canceled this PhD graduation. I haven't read the dissertation, but I'm convinced that they have thought this through very well. I do think that people have been sleeping.*"—Herman Beijerinck, physics professor at the TU/e (2008)[15]

To the defense of my opponents it has to be understood that they had all *uncritically presumed* that those four pamphlets—in particular the one by 't Hooft—indeed had revealed "facts" that "proved" that my work was of insufficient scientific quality: **from that perspective** they published what they suspected to be the case as if it was a fact—mostly without having seen my work. So although "facts" about me and my work have been alleged that were fabricated out of thin air, there was no intention to fraud: these fabrications were *unintentional*. What I hold against them is the same as I hold against the authors of the four pamphlets: they have carelessly passed off *figments of the mind* as "facts" about me or my work.

While most of my opponents merely reacted emotionally to the news and at least believed that they were stating the truth at the moment they were making their comments, this does not hold for all of my opponents. Kees van Hee for one has been lying through his teeth, but another one is Frank

Van der Duyn Schouten, then Rector of Tilburg University. In the first week of the controversy, he had an open letter published *simultaneously* both in Cursor, the university weekly of the TU/e, and in Univers, the university weekly of Tilburg University (2008a; 2008b). In that letter he made it publicly known that prior to the affair at the TU/e, the concept-dissertation had already been withdrawn at Tilburg University: he stated that it was withdrawn after physicists—note the plural—in the PhD committee had uttered "severe criticism" to the work, and he claimed that the TU/e had been "misled" by not mentioning the prior affaire at Tilburg University. For the reader this open letter not only reinforced the impression that had emerged from the first week of the controversy—i.e. the impression that my work was of insufficient quality—but it also added a new dimension to the controversy, namely that of intentional deceit. Given his position as the highest official of Tilburg University, Van der Duyn Schouten was taken at his word: this letter therefore led to several articles along the same lines in regional and national newspapers, e.g. (Hover, 2008a; Scholtens, 2008b). But Van der Duyn Schouten was lying through his teeth in his letter. He knew damn well that I had withdrawn the thesis at Tilburg University because of a personal collision with Hartmann and not because of any criticism, and he also knew damn well that the TU/e was informed about the prior affaire at Tilburg University. In fact—I again appeal to the *quantum tunneling effect*—at some point in 2008 I all of a sudden got hold of a letter that Van der Duyn Schouten had written on December the 27$^{\text{th}}$ of 2007 (so *after* the one-page article by Vlooswijk had appeared in Cursor but *before* the controversy broke loose) to Hans van Duijn, the Rector of the TU/e: in it, he stated that Hartmann may have indeed reacted inappropriately (in Dutch: *te kort door de bocht*) and he urged the TU/e to proceed with my PhD graduation (Cabbolet, 2008b). So, make no mistake: this open letter was a *calculated, strategic action* by Van der Duyn Schouten.

Furthermore, at some point the Dutch organized skeptical movement *Stichting Skepsis* became involved, mainly (but not only) in the person of Jan Willem Nienhuys: he was hell-bent on exposing me as a pseudoscientist. It is true that Nienhuys has a PhD in pure mathematics and for some time has been a lecturer in mathematics at the TU/e, and it is true that that background may be enough for exposing quackery in medicine—e.g. when claims about the effectiveness of an alleged cure for a disease are

not backed up by double blind testing on a sufficiently large group—but still that background is insufficient for evaluating new developments in the foundations of physics. But by writing about my work Nienhuys nevertheless pretended to be competent in that area, and therefore by doing so he actually engaged in the quackery he wanted to accuse me of. He produced two papers in journals of organized skeptical movements (Nienhuys, 2014, 2015): for laymen—the readership of the journals—Nienhuys' papers may come across as profound analyses of my work, but fact of the matter is that his papers lack substance to such a degree that the term 'pseudoskepticism' applies. That is, his two papers are purely aimed at winning the readership over for his preconceived conclusion that I'm a crackpot and my work is illucid. The following sarcastic statement by Nienhuys betrays this preconceived conclusion:

> "*in an interview, the candidate expressed himself very optimistically about his manuscript*"—Nienhuys (2014)

So make no mistake: Nienhuys is one of those at the TU/e who misinterpreted my statement in the one-page article in Cursor, (Vlooswijk, 2007), as discussed on page xxxii. That Nienhuys' papers are merely aimed at getting the readership to agree with that preconceived conclusion is then evident from the fact that he uses several well-known dishonest tricks, described by the philosopher Arthur Schopenhauer in his 1831 book *The Art of Being Right*, to win an audience over for one's own idea. Some examples:

> "*He could also have proposed [instead of repulsive gravity] that gravity may be caused by dancing devils (fallen angels!). Or he could have formulated his theory from the hypothesis that you can let the wand of your opponent fly upwards with the spell 'Expelliarmus'.*"—Nienhuys (2014)

> "*The great physical theories of the* 20^{th} *century are built on established experimental facts that in many cases have been found by purposely searching for the boundaries of the possible. You can forget about creating new physics by merely talking in a different and extremely cumbersome way about infinite sets.*"—Nienhuys (2014)

> "*… In 2009 Cabbolet learned from Gerard 't Hooft that photons are their own antiparticles.*"—Nienhuys (2015)

The first of these quotes concerns *false metaphors*: the use of metaphors that favor one's own view is, in fact, dishonest trick #12 mentioned by Schopenhauer. In analysis, the use of metaphors is *not done*. And not just that: Nienhuys' use of false metaphors to ridicule my work is a tell-tale sign that his papers have nothing to do with an objective evaluation. The second quote concerns dishonest trick #28 mentioned by Schopenhauer: *persuade the audience, not the opponent*. Here Nienhuys wants to contrast the way I have developed my theory with the way accepted theories have been developed to expose me as someone who knows nothing about theory development. Now the readers of the journals in which he has published are not only laymen, they are also quite gullible: they take Nienhuys at his word, so in their eyes Nienhuys has ruthlessly established here that my theory is so bad that it already can be rejected by looking at the method of development—it is not even necessary to look at the theory itself. However, the quoted statement contains two false statements of fact by Nienhuys: it is **neither** the case that accepted theories of physics have been developed from experimental data, **nor** that I have developed my theory by "talking about infinite sets". To elaborate, fact of the matter is that the epistemic sources for accepted theories of physics are observations *and reasoning*—not observations alone. As a result, these theories were speculative at the moment of publication: they yielded predictions that were *absolutely not* supported by observations. It is, for example, absurd to claim that Einstein's general relativity has been developed from observations of gravitational time delay and deflections of photons by the gravitational field of the sun, or that Dirac's theory of antimatter has been developed from the discovery of the positron—it is the other way around: these observations have been done because the community wanted to test these predictions. So these accepted theories have not been built on experimental facts: what is the case is that these accepted theories are theories that initially were speculative yet rigorous and that have become accepted because crucial predictions have been confirmed by experiments. Therefore, the first claim by Nienhuys betrays incompetence in the history of physics and general philosophy of science—to his defense: he's a mathematician. As to Nienhuys' second claim, fact of the matter is that the process-physical principles of my theory, the EPT, are mathematically expressed in a framework for mathematical logic, but it is flat-out wrong to state that these process-physical principles have been

developed by thinking about sets or matrices of sets. That is, I have *first* developed the process-physical principles and *thereafter* I have developed the mathematical-logical framework in which these can be expressed: not the other way around. So, it is not the case that I have *first* developed a mathematical-logical framework and *thereafter* have created new physics by giving it a physical interpretation. For comparison: Einstein's physical ideas of general relativity are mathematically expressed in the framework of tensor algebra, but it is flat-out wrong to state that Einstein has *developed* his general relativity by merely thinking about tensors. This second claim by Nienhuys betrays ignorance of general methods of theory development in physics—it is even questionable whether he understands the difference between *hypothesizing* that matter and antimatter repulse each other gravitationally (which is what I did) and *claiming* that repulsive gravity exists (which I didn't). The last quote is an *ad hominem attack*: this is dishonest trick #16 mentioned by Schopenhauer. And not only that: it's also a false statement of fact—back in 1997, literally the first thing that was brought up in my investigation into repulsive gravity was that photons are their own antiparticle according to the Standard Model: 't Hooft hasn't taught me that, nor has he taught me anything else. So, there you have it: the prominent member of the skeptical movement Nienhuys has himself resorted to passing off outright fabrications as "facts"—make no mistake: with the quoted statement Nienhuys wants to win his readership over for his preconceived conclusion that I'm a crackpot who is ignorant of the relevant literature. I believe my point is herewith sufficiently proven. With regards to the skeptical movement, it is one thing to expose quackery in medicine—which, I believe, is a good thing—but it is another thing to publish papers in which the author resorts to *dishonest tricks* to sway public opinion against the latest development in avant-garde science. Now that the skeptical movements are doing precisely that, the question that we can ask ourselves is this: do public libraries and university libraries really need to have a subscription to their journals paid by tax-payers' money?

A third group that has engaged in calculated, strategic action is formed by several prominent members of the Royal Dutch Academy of Sciences: their contributions to the controversy about my work have remained behind closed doors, but nevertheless these contributions were crucial in maintaining the decision to cancel my PhD graduation at the TU/e. Altogether,

about a dozen members of the Royal Dutch Academy of Sciences are up to their neck in the cancelation of my PhD graduation and the decision to maintain that cancelation. As mentioned in the section 'Events leading to the cancelation of my PhD graduation' of this chapter, my PhD graduation at the TU/e was canceled without the possibility to appeal to the decision. So the only possibility that I had was to show that widely agreed upon principles of good scientific practice were violated in the process of reevaluation of the concept-dissertation: that would establish that the reevaluation was bogus, which in turn would establish that the reason for the cancelation of the PhD graduation was bogus. So I filed a complaint about violations of principles of good scientific practice at the Scientific Integrity Committee (SIC) at the TU/e. They ruled that 11 (!) persons, including Van Hee and Van Duijn, had violated scientific integrity in the process. Such a ruling by a SIC, however, has formally the status of an advice to the university administration: in this case, the administration of the TU/e simply ignored the advice of the SIC of the TU/e and officially decided to maintain the cancelation of the PhD graduation—note that by doing so, the university administration maintained the conclusions of a reevaluation process that by then had been established to be bogus. Then there was only one option left: namely, to take the case to the Royal Dutch Academy of Sciences. For such cases—complaints about scientific misconduct, that is—the Royal Dutch Academy of Sciences had, together with the union of Dutch universities, founded a special institute: the national organ for scientific integrity (acronym: LOWI). So, I submitted my complaint to the LOWI headed by Kees Schuyt, but what happened there really defies the idealistic presupposition that 'truth finding' is the highest value in academia. I was summoned for a private hearing by the LOWI, during which I was asked to first summarize my complaint: at the moment I started talking, members of the LOWI—most notably Hans Vliegenthart—already started to shake their heads in disbelief. I should have walked away right there and then: the rest was simply a waste of time and effort. Instead of evaluating the course of affairs at the TU/e from the perspective of the principles of good scientific practice, the LOWI then set up their own reevaluation of the concept-dissertation, for which they approached the physicist Carlo Beenakker—as reported in (Cabbolet, 2014d)—and a philosopher: both were (very) negative. Beenakker is undoubtedly an excellent expert in his

own field, condensed matter physics, but he falsely yet firmly believes that this implies expertise in the foundations of physics—that he is no expert in that area may be evident from the fact that he gave a lecture in 2016 (which I attended) about Schroedinger's cat to introduce the quantum-mechanical notion of entanglement to the general public, thus demonstrating a lack of understanding of the difference between the notions 'superposition' and 'entanglement'. His comments on my concept-dissertation lacked substance to such a degree that the term pseudoskepticism applies. The following quote from his "review report" stands model for its scientific standard:

> "*My third objection to a PhD graduation on the basis of this manuscript is that it has not even partially been published in the scientific literature.*"—Beenakker, cf. (Cabbolet, 2014d)

The point here is that prior publication of the material in the dissertation is not a requirement for a PhD graduation in the Netherlands: Beenakker's argument is not a valid objection. The comments by the philosopher, the only non-member of Royal Dutch Academy of Sciences involved in the course of affairs at the LOWI, were also of the level of pseudoskepticism, as evidenced by the following quote from his "review report":

> "*all these definitions etc. may yield the impression that we are dealing with a carefully developed physical theory. But that is absolutely not the case. On p. xlv we read … that the EPT cannot make quantitative predictions. … Cabbolet admits this, but 'one should no longer think in terms of falsifiable or verifiable theories, one has to think in terms of degenerating or progressive research programs.' That … is substandard. Typical for research programs, progressive or degenerating, is that we can speak of falsifiability and confirmability with the help of auxiliary hypotheses. … Any indication thereof is missing*"

So, he judges my entire work as 'substandard' *because* it lacks an indication of how the EPT could be tested in a research program by means of auxiliary hypotheses. That's a false statement of fact: this was explained on p. 66 ff. of that booklet.[16] But for Schuyt c.s., the members of the LOWI, the arguments by Beenakker and the philosopher were valid enough. They *formally* gave me the possibility to reply to the objections but they *actually*

ignored my reply in its entirety: the members of the LOWI simply closed ranks. That is, the members of the LOWI simply sided with the other prominent academics involved in the cancelation of my PhD graduation c.q. the controversy about my work, and the LOWI officially ruled that my complaint was baseless on all counts. To the defense of the members of the LOWI, evidently they reacted initially with disbelief to the complaint that I submitted, and of course that disbelief was strengthened by the "review reports" of Beenakker and the philosopher: they've acted correspondingly. But what I hold against the members of the LOWI is that they were never really interested in objectively evaluating the course of affairs at the TU/e from the perspective of the principles of good scientific practice: they held me in low esteem, and they *therefore* felt justified to dismiss my complaint in its entirety—that is, it was their view that my complaint should be dismissed in its entirety to avoid that the reputations of prominent academics got damaged c.q. tainted by a complaint about a reevaluation procedure by a crackpot whose PhD graduation was rightfully canceled *even though* the procedure by which the cancelation took place might not have been optimal. Schuyt c.s. will deny this *ad infinitum*, but make no mistake: the members of the LOWI have abused their discretion to rule on my complaint to protect the reputation of prominent academics and that's it. After that, I informed the then president of the Royal Dutch Academy of Sciences, Robbert Dijkgraaf, about the wrongdoings at the LOWI. However, Dijkgraaf—a former PhD student of 't Hooft—stonewalled me: he never gave any reply to me directly, but he did comment negatively on my work much later in a Belgian newspaper (Dzikanowice, 2012). Finally, in 2016 I noticed that the scope for complaints that could be submitted to the LOWI had been widened: it was henceforth also possible to submit a complaint about the LOWI itself. And so I did submit a complaint about the LOWI led by Schuyt. However, the new members of the LOWI ruled in 2016 that they could not take my complaint in consideration because too much time had elapsed since the decision by Schuyt c.s. in 2009. I, on the other hand, am of the opinion that misconduct cases cannot have an expiration date, in particular when it concerns fabrications of "facts": genuine facts, namely, contribute to the growth of knowledge, but when it concerns fabrications then these "facts" amount to a body of pseudoknowledge, that is, a body of statements falsely believed to be knowledge. I'll leave it at that.

Epilogue: speculation about causes

There are critics of the orthodoxy who, having experienced resistance from the academic establishment, accuse that academic establishment of a "conspiracy" or of forming a "modern inquisition". But even though real persons have held real meetings to thwart my research, I believe that this is a mistake: one should not attribute to conspiracy that which can be adequately explained by stupidity (Hanlon's razor). That is, the resistance that I've experienced from the academic establishment is nothing but the sum of isolated, individual responses to my work, and those isolated, individual responses to my work share the common denominator that they are (virtually) all instances of committing the same stupidity, namely passing off false statements of fact—which are nothing but figments of the imagination that popped up in an emotional outburst—as genuine "facts" about me or my work *without even having attempted to check the truthfulness of these "facts"*. And committing this stupidity is *unethical behavior* in science, since it violates the widely accepted principle of carefulness, one of the basic principles of good scientific practice.

However, I don't want to call my opponents "stupid", certainly not my main opponent Gerard 't Hooft: they are very intelligent individuals, capable of successfully working on the most advanced research programs in physics or mathematics.[17] Furthermore, the fact that they have made their comments about me and my work publicly available indicates that they are not aware that they have committed a stupidity c.q. that they are behaving unethically—as Brian Martin put it:

> "*there is [no] conscious conspiracy of evil schemers who set out to destroy dissidents. Just the opposite. Those who attack dissent sincerely believe that* ***they*** *are doing the right thing.*" (emphasis original)—Martin (1998)

That, however, raises two questions:

(1) How can any such intelligent individual commit such a stupidity *without* realizing that it is unethical?

(2) How can this unethical behavior in response to my work be *the rule* rather than *the exception*?

Below I answer these two questions without claiming an absolute truth.

Ad (1). Of course we can only speculate about what goes on in someone's mind, but fact of the matter is that the way senior physicists have responded to my work becomes a prediction if we model the workings of the mind using Spinoza's *Ethics*. This yields the following response-in-an-outburst model:

(i) as a rule, a senior physicist strives to persevere in his career, which is intimately interwoven with the assumptions that gravity is attraction only and that any new theory of physics must yield the contemporary theories of modern physics by applying some limit procedure: he believes in these assumptions himself, he teaches these to his students, and all of his research has been based on these assumptions;

(ii) given (i), a senior physicist thus *automatically* experiences sadness as soon as he finds out that my work implies that these assumptions must be wrong—and one can find that out from a superficial reading, or even from hearsay—*because* it opposes his conatus, that is, *because* if these would be wrong then his entire career would be based on falsehoods, and it would become more difficult to persevere in it;

(iii) the senior physicist then *automatically* experiences hatred towards me and my work as the cause of the sadness;

(iv) affected by the hatred, the senior physicist then *automatically* experiences the desire to mock me and/or my work;

(v) affected by that desire, *automatically* derisive thoughts come up that would joy the senior physicist if these could truly be said of my work or of me;

(vi) this desire lasts only for a short time, but the senior physicist commits the stupidity to give in to it by passing off these fabrications of his mind as if they were "facts" without even attempting to check their truthfulness and without self-reflection—this yields pseudoskepticism;

(vii) the senior physicist then feels good about it afterwards *because* he acted this way—not the other way around: it is **absolutely not** the case that he acted this way *because* he felt good about it!

This model "works" in the vast majority of cases (my estimate: 99%), so from James' pragmatic perspective it's true *instrumentally*. Physicists like

to see themselves as modern scientists, yet those who have responded to my work with pseudoskeptical attacks have behaved as automata as predicted by a theory from the 17$^{\text{th}}$ century!

EXAMPLE X To see how the response-in-an-outburst model works, we can use it to explain how 't Hooft, in his pamphlet used for getting my PhD graduation canceled, could pass off the following fallacious one-liner as a "fact" about the mathematical part of my 2007 concept-dissertation:[18]

> "*Although I do not completely speak the language that mathematicians use in set theory, I know enough about it to see that the mathematical part of this work does not satisfy the quality standard demanded by mathematicians.*"—'t Hooft (2008c)

The crux is that 't Hooft (later admitted that he) has had no education in mathematical logic, the branch of mathematics applied in the concept-dissertation. So, listing through the concept-dissertation he has not recognized the mathematics *typographically* as mathematics that he knows. And upon that finding, affected by the desire to mock my work, the thought has popped up that it is *not mathematics at all* as in step (v) of the model. This is nothing but a fabrication of the mind, but he has passed it off *without checking its truthfulness* as a "fact" about my work, as in step (vi) of the model. And make no mistake: 't Hooft feels good about it as in step (vii) of the model—he will refuse to retract the fallacious claim quoted above even after being confronted with conclusive evidence to the contrary. ■

Question (1) on page l is herewith answered. By changing only names, a general response-in-an-outburst model is obtained for the response of an overly specialized expert to a work that opposes his connatus, that is, a work that is not in line with the basic assumptions of "his" research field.[19]

Ad (2). Individuals who have responded to my work as predicted by the response-in-an-outburst model have what I call a 'cult-of-narrowness' mindset, which *differs wildly* from the mindset of an authentic scientist.[20] The latter, namely, is committed to truth finding and is therefore aware (a) that truth finding is a joint effort to find out the truth about something and (b) that the truth that will eventually be uncovered might deviate from

his own belief(s): he welcomes well-founded criticism to learn from it, and *naturally* suspends his disbelief when evaluating someone else's ideas. So, an authentic scientist simply isn't affected by a piece that dissents from his own core belief(s) as predicted by the response-in-an-outburst model.

That means that the *observable* fact that the response-in-an-outburst model "works" in the vast majority of cases indicates that a senior physicist, as a rule, is out of touch with his authentic self and alienated from truth finding. That, however, should come as no surprise in this era of overspecialization in research and education. In his book *The Path*, co-authored with Christine Gross-Loh, Harvard scholar Michael Puett warns for the danger that the ruts and patters in our interactions that we as human beings during our lives fall into, often from a young age, can define us to the extend that we falsely associate them with ourselves and start thinking of them as our personality (2016): I believe that this danger *as a rule* materializes during one's career as a researcher in modern physics.

First of all, the university educations have become too narrow. It is not necessarily a bad thing that the student is being told what to think about the physical universe, but on the other hand he never gets exposed to the landslides in human thinking caused by the greatest ideas in history, nor is he taught how to analyze a theory, how to self-reflect or how to wilfully suspend his disbelief: the analytical skills remain undeveloped. But it is not just that. From the PhD position on, over the years a physicist usually only gets to work on a series of excessively narrow research topics that are always situated within a larger research program, in which a hard core of theories immune to revision is uncritically accepted and in which the negative heuristics dictate that criticism of that hard core is *not interesting*. And so the physicist accustoms to and eventually accepts the compartmentalization of physics and the roles physicists play in the mainstream research programs. That not only limits his ability to think out of the box: the modern physicist eventually gets out of touch with his authentic self. That is, as a senior physicist he may *think* that he has aligned his life with his authentic self, yet he is *really* only being true to the ruts and patterns of the compartmentalized landscape of modern physics that he has fallen into from day one as a young student. He is not really, honestly allowing ideas other than his own to challenge his core beliefs. What he actually does is the opposite. He only considers those results from other research

programs acceptable that allow him to stay true to these same ruts and patterns that have defined him—lacking the ability to wilfully suspend his disbelief, he is only committed to truth finding insofar as that truth lies within the mainstream research programs he is working on. He has locked himself into a very limited version of what he could be: he is out of touch with his authentic self and is completely alienated from truth finding. That answers question (2) on page l.

Summarizing, I believe that the overspecialization in research and education causes a 'cult-of-narrowness' mindset to prevail among physicists, and that this 'cult-of-narrowness' mindset causes individuals to respond to my work in a way as predicted by the response-in-an-outburst model on page li.[21] **But, truth be said, there still are authentic individuals in physics, also among those who do not support my work!**