**“So you confirmed, replicated and emptied your file-drawer... now what?”
A Structural Realist’s Guide to Theory Evaluation in Psychological Science**

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Wordcount: 14900

“Questions of principle are sometimes thought to be too unpractical to be important, but I suggest that that is certainly not the case in *our* subject.”

- Ashby (1962), a practitioner of psychological science

The editors of the special section on replicability in psychological science in the November 2012 issue of *Perspectives* suggested introduction of parallel, cross-discipline reforms on all aspects of the practice of psychological science. The reforms that proved to be effective should be adopted (Pashler & Wagenmakers, 2012). This was a necessary and urgent plea to change an entire scientific discipline. The issue hosted more than 20 papers, containing excellent suggestions for the resolution of various aspects of the current crisis of confidence. We must ask however, by what measure will the effectiveness of reforms be evaluated? Does the replication rate have to go up to 10% from 1% (Makel, Plucker, & Hegarty, 2012)? Do we wait until only confirmatory research is taken seriously (Wagenmakers, Wetzels, Borsboom, van der Maas, & Kievit, 2012), or until our blindness to alternative hypotheses has been resolved (Fiedler, Kutzner, & Krueger, 2012)? In this paper we express the sincere concern that even if all three reforms just mentioned were achieved, this would still not guarantee the survival of psychology as the prime discipline for the scientific study of human behavior after the imminent replicability apocalypse.

What is missing from the debate? The word we are looking for is *theory* and we were astonished to find that it was used in only 8 out of the 23 papers published in the PPS issue. Out of those 8 papers 4 actually discussed causes and solutions of the crisis in the context of *theory evaluation*. Though we are excited about the current momentum for change and actively support several of the proposed initiatives, we also believe it to be impossible to successfully resolve issues like replicability without examining how the discipline evaluates and revises the theories it generates about reality, human behavior to be precise. Our concerns are based on more than just a word count; in this paper we will diagnose theory evaluation in psychological science afflicted with three life-threatening symptoms that need to be cured:

- 1) *The interpretation fallacy*. Psychologists have the tendency to value experimental findings according to the gain in explanatory power they provide for the theory that predicted the measurement context. The causes for this fallacy may be specifically related to the object of study in psychological science and therefore its persistence may be understandable, but we will argue it is a serious obstacle for the advancement of the discipline.
- 2) *'Empiarrhea'*, which refers to the huge structureless empirical record that is continuously being filled with phenomena that are mainly descriptive in nature. Moreover, empirical findings carry no authority at the level of competing theories laying explanatory claim to the same parts of reality. As a consequence, in all sub-disciplines of psychology there are many different theories that are "true" at the same point in time¹. That is, theories are either, knee-deep in empiarrhea and stuck in an empirical gridlock, or they display an ontological indifference with respect to predictions made by theories that compete to explain the same phenomena.
- 3) *The Columbus delusion* refers the belief expressed in many empirical studies that phenomena are "out there" in reality, waiting to be discovered by a clever experimental manipulation, like a fire rainbow, a giant squid, or a large land mass across the ocean where the best available maps said "here be dragons". A laboratory experiment is not a field study and even then we would argue that "*It is the theory which decides what can be observed*" (Einstein as quoted by Heisenberg, 1971, pp. 62–63).

More generally speaking, these symptoms combined express our impression that a majority of practitioners of psychological science seem to be unaware of the important advances made by the rest of science (sometimes even centuries ago), about the workings of the universe: Psychological science regularly gives a solo performance. As we will show in our discussion of an example of theory evaluation (the strong criticism received by the complex systems approach to the study of psychological phenomena), there is also a disconnection with the historic scientific record (theoretical and empirical) of the discipline. What may be gained by opening up to this history and to other scientific disciplines include general lessons on how to theorize and gather information about the universe, but also specific knowledge about the structure and behavior of living systems. This is not at all an easy task and in some cases it would require a sabbatical of several years to catch up on things. The most awkward example of the three symptoms interacting to reveal the ignorance of psychologists to the state of the rest of science, is the much discussed psi paper by Bem (2011) in which it was claimed theories about the laws of physics needed some serious revisions in order to account for his empirical findings. Thankfully, none of the funding agencies and scientists working to construct the Large Hadron Collider took this work seriously. Sadly though, they probably stopped taking seriously the entire discipline of psychological science and we can't blame them for doing so considering the revealing information about scientific practices that followed (e.g. as related by Stroebe, Postmes, & Spears, 2012).

We will elaborate on the causes and effects of these afflictions, but this

¹ We thank Rick Dale for pointing out the fact that this symptom could also be described as 'theorrhea' caused by the theorist's craving for monism (cf. Hasselman et al., 2011).

should not distract the readers' attention from our main message that we intend to be positive. This paper should be seen as a practical field guide to theory evaluation in a discipline that lost confidence in the value of its empirical record. In fact we respond to the call by Fiedler et al. (2012) to start "[...] *an open conversation about creative methodologies that can tackle fundamental issues of hypothesis generation and selection.*" We have no intention to provide a complete philosophy of science for the study of human behavior; we present a heuristic, or practical stance with regard to the advancement of scientific knowledge known in philosophy of science as *structural realism* (SR). SR promises the best of both worlds (cf. Worrall, 1989) as it demands a commitment to a specific ontology without falling prey to reductionist fundamentalism. In the first part of the paper we explain what it is we mean by a theory and its scientific evaluation, and why the three symptoms pose a threat to this endeavor. In the second part we present our structural realist's guide to theorizing about the universe.

1.1 FORMALISM, THEORY, ONTOLOGY AND MEASUREMENT: LEARNING FROM THE GIANTS

Every scientific theory is based on ontology; it refers to the entities, or building blocks that are posited to exist as constituents of reality, the metaphysics behind the theory. For example, the alchemical ontology of the classical elements as the building blocks of the universe (earth, water, wind, fire and an elusive fifth element, the *Æther*) has proven to be a very powerful idea throughout history. Modeled after the elemental ontology was the humour ontology that was possibly even more successful judging by its longevity, roughly from 400 BCE (Hippocrates, 400 BCE/1923, p. 61) to 1855 (Remak, 1855; Virchow, 1855). Theories about the functions of the body were based on the balance of four essential fluids: Blood (*sanguis*), phlegm (*phlegma*), yellow bile (*chole*) and black bile (*melanos chole*). The four humours (sometimes a the fifth, Spirit or Soul), were linked to a taxonomy of human personality or temperaments which could be sanguine, phlegmatic, choleric, or melancholic and thus constituted an early biological theory of personality and psychopathology (Freeman, 1997; Núñez & Freeman, 2000). In fact, the Eysenck model gives a classification of personality as a mixture of four basic elements related to the Hippocratic temperaments (Eysenck, 1985). Can this really be described as the success of a scientific idea about reality? One could also argue it was too general to be of any use for an actual scientific theory, or be confirmed or falsified by the scientific method. The idea of essential substances that need to be brought back in balance to cure disease is still very much alive, sometimes even surviving falsification by science (e.g. resolving serotonin depletion in clinical depression by administering SSRIs, Lacasse & Leo, 2005). In what follows we will discuss the role of ontology in theory evaluation.

An important thing to note is that ontology is not the same as a theory, it is an assumption about the constituents of reality whose description can be anything from metaphorical prose to mathematically formalized. A single ontology may spawn scientific theories about many different aspects of reality, but different theories about the same aspect of reality can also be based on one and the same ontology. Obviously, the latter theories will compete for the accuracy of their predictions about those observable phenomena. In the popular history of science it is common

to present clashes between theories about the same aspect of reality, as clashes of different ontology. A well-known example is the wave or particle ontology physical theories about light and matter were based on. Comparing ontologies is however not the purpose of theory evaluation. Theory evaluation refers exclusively to an assesment of the abillity of a theory to produce testable predictions (*predictive power*) and accurate predictions (*empirical accuracy*), it does not directly concern the justification or falsification of ontology (although such things may occur during the evaluation process).

The main message of this paper is that ontology is what scientific theories should be realist about, not the scientist who uses the ontology to construct the theory. The interpretation fallacy occurs when a scientist rejects a theory or hypothesis based on the fact that measurement outcomes predicted by this theory cannot be interpreted or explained in terms of the ontology the scientist is familiar with, or realist about. We will provide several clear examples of this fallacy from psychological science below. However, in order to fully appreciate our concerns about the advancement of psychology as a discipline of science, we discuss theory evaluation and ontology justification in the context of the most successful scientific theories about reality to date: The physical theories about the quantum world.

The birth of quantum theory at the beginning of the 20th century has its origins in the lack of a unified understanding of a set of fundamental empirical findings. In retrospect, the simultaneous wave and particle like behaviour of matter and light is often considered the driving force behind the development towards a concise theoretical framework. A major breakthrough in the foundations of quantum mechanics was its axiomatic formulation, which is largely attributed to a combination of John von Neumann's mathematical progress (1932) and Paul Dirac's theoretical advancement (1930). The formalism was completed with the addition of axioms on the statistical interpretation of quantum phenomena (by Max Born) and a dynamical law for the time-evolution of quantum systems (by Erwin Schrödinger).

Box 1. Defining theory, measurement and interpretation within the domain of a formalism reflecting a structure in reality.

What was achieved by these giants of science was the integration of several simultaneously existing theories about different appearances of reality, based on conflicting ontology, into a single underlying formalism. Perhaps the most prominent result was the profound insight that the ‘Schrödinger picture’ and ‘Heisenberg picture’, which represented two previously opposing views on reality, are actually isomorphic, that is, equivalent for all intents and purposes. In addition, the formalism allowed for many other views on reality such as the interaction or ‘Dirac picture’, which are all equivalent as long as they obey the basic quantum formalism. The basic formalism doesn’t make any initial ontological claims, but merely lays down the general foundation from which scientific endeavours should depart. Also note that the physics community evaluated what was necessary to formally and consistently describe *all the phenomena* in the empirical record. They took seriously the precision and accuracy of their theories, even though they were based on different ontology. They subsequently created or recruited the mathematical tools they needed to provide the unified description. A lesson to be learned here is that in the end, both ontologies (i.e. waves and particles) were discarded as primary descriptions of nature’s smallest constituents. Rather, the focus shifted towards further unraveling of the structure beneath opposing and equivalent descriptions. In the process, it required the development of new ontological vehicles (and related theories) such as quantum fields, the exotic quarks, and the even more esoteric strings and branes.

To recapitulate, a formalism is a set of postulates and rules of deduction that define a domain in reality scientific endeavors can theorize about (see Box 1).

Theoretical predictions based on a formalism concern observables that give measurement outcomes given a measurement context. This context is part of the prediction by a theory. The theory is evaluated based on its predictive power and empirical accuracy. Theories about strings and branes mentioned earlier are based on beautiful mathematical formal systems, but they have very low predictive power because they do not produce testable predictions (yet). Ontology plays a descriptive role; it is something like a prototype vehicle for understanding reality scientists can use to build theories. Theories may gain explanatory power through ontology, which is the ability to reassure both the scientist and layman that they can understand the phenomena the theory describes. Theories departing from different ontologies, within the same formalism, can eventually lead to equally accurate descriptions of reality. In other words, ontology will become irrelevant provided the theory it is based on continues to be positively evaluated.

The success of this scientific method is exemplified by the fact that theories based on the quantum formalism describe the quantum world with unparalleled accuracy. The most precise prediction of a measurement outcome made by a scientific theory to date is the prediction by Quantum Electrodynamics (QED) of the existence of an anomalous electron magnetic dipole moment (which is anomalous to the Dirac equation). According to the most accurate calculations the anomaly, when measured in a metal trap with a cylindrical cavity whose resonance structure is known, should have a value of: $a_{\mu}^{\text{QED}} = 11659180.4(5.1) \times 10^{-10}$ (Aoyama, Hayakawa, Kinoshita, & Nio, 2008; Hagiwara, Martin, Nomura, & Teubner, 2007). The most accurate empirical measurement of the predicted value of the anomaly in the described measurement context is: $a_{\mu}^{\text{exp}} = 11659208.0(6.3) \times 10^{-10}$ (Bennett et al., 2006).

1.2 THE REALIST QUESTION

What about the interpretation of these measurement outcomes? Does this accuracy imply its objects of measurement, like this anomalous magnetic dipole moment, actually exist as objects in reality? Can we attribute properties to these objects based on the measurement outcomes? In other words, what should we be realist about in science?

The French philosopher, mathematician, engineer and theoretical physicist Henri Poincaré, gave an early answer to the realist question in 1889. In those days the nature and properties of the luminescent *Æther*, that was assumed to fill the entire universe, were a hot topic in cosmology. About an existence-proof for the *Æther*, that was still lacking in the empirical record, Poincaré wrote:

"Whether the ether exists or not matters little - let us leave that to the metaphysicians; what is essential for us is, that everything happens as if it existed, and that this hypothesis is found to be suitable for the explanation of phenomena. After all, have we any other reason for believing in the existence of material objects? That, too, is only a convenient hypothesis; only, it will never cease to be so, while some day, no doubt, the ether will be thrown aside as useless." (Poincaré, 1889/1905, p. 211).

Poincaré could not have expected the very thing he described would actually happen in his lifetime. The year the translation of his 1889 book was published in English, Albert Einstein published his theory of special relativity that eventually led to the *Æther* being “*thrown aside as useless*” (however, see this curious paper by Dirac, 1952). The scenario Poincaré sketches for the fate of the *Æther* reflects the realization that a physical science does not concern itself with the way things are *actually*, but with the way things are *apparently*, according to the theory.

This is the beauty of the scientific study of reality. The answers to those philosophical questions, interesting as they may be, are irrelevant for theory evaluation and thus irrelevant for the success a theory may have in predicting phenomena in reality. There are dozens of interpretations of quantum physics² many heralded by respected Nobel laureates, but none can be shown to be primary over others. This plenitude of interpretations of quantum physics does however not harm the predictive power and empirical accuracy of its theories in any way: Highest ever! It is true that quantum theories have a very low descriptive or explanatory power, due to all those possible interpretations. It may be amazing enough that the universe obeys the laws of quantum physics, but how it can be like that is not something this theory can answer. We can now formulate an existential question for psychological science itself: Does psychological science want to produce theories that are precise and accurate in the empirical sense, or does it want theories with high explanatory power, whose ontology its practitioners can be realist about? (Indeed, ‘both’ is not an option, as we will explain later).

This is a difficult question for psychological science due to its object of study. In fact, the quote by Poincaré may contain a clue that should raise some sympathy for the dire position the discipline finds itself in today. About the existence of material objects Poincaré noted that this, like the *Æther*, is just a convenient hypothesis about the constituents of reality. However, it is one that “*will never cease to be so*”. Why should material objects receive this special status? Material objects are sensory phenomena, manifolds of immediate sensory experiences, or direct experience. The luminesfurous *Æther* on the other hand was one of the invisible constituents of reality physicists needed to imagine into existence, because everything appeared to happen as if it really were there. It was like the fifth element, a quintessence to complete our understanding of the world around us. In a way, it is “easy” for physicists to identify something like the luminesfurous *Æther* as a theoretical concept that may be discarded. In fact, perhaps the greatest advances in physics have been made after the scales of measurement depleted or inflated beyond the capacity of registration by the senses as direct experience. Returning for a moment to the classical elements and the humour ontology, their existence-proof to our senses prompted ancient scholars to theorize about their role in the workings of the physical universe and the human body. This is a very sensible and scientific thing to do. Theories were *constructed* based upon their empirical evidence as “measured” by our senses. For psychological science, the classical analogue to the *Æther* as the quintessence would be Spirit or Soul.

Here we have identified an ancient problem of philosophy: Is our internal

² See e.g. http://en.wikipedia.org/wiki/Interpretations_of_quantum_mechanics

world, the mind, or our consciousness, part of reality? Many scholars have argued it should be thrown aside as useless in scientific theories, just like the *Æther*. This means we can identify it as a construct, theorize about it or choose to ignore it in our theories. We must acknowledge though that in psychological science there is a thin line between the theoretical concept of a mind and the actual sensory phenomenon of mind. Is the line too thin? For example, to the mind's eye the external continuous universe appears filled with discrete objects, our internal thoughts also appear discrete, so do our memories of events and the words we use to communicate. Therefore, we may be naturally inclined to construct theories based on discrete components, without even considering continuous architectures of mind as an alternative (Spivey & Anderson, 2008; Spivey & Dale, 2006). We conjecture the origin of theoretical diversity and issues with reproducibility in psychological science are often due to fundamental category mistakes in which theoretical constructs that emerged from human theorizing about reality are mistaken for direct sensory experiences, quite literally observations, due to the thin line that separates them. Such theoretical constructs, about which only a scientific theory should be a realist, are mistaken for an actual, or at least a perceptual constituent of reality. As a result the implicit theoretical object or ontology is essentially excluded from scientific inquiry.

An example of such an object that escapes genuine theoretical and empirical inquiry in psychological science is the concept of the mental representation (cf. Haselager, de Groot, & van Rappard, 2003; Spencer & Schöner, 2003). It is associated to discrete cognitive architectures and is frequently used to formulate hypotheses, as an object of measurement or as a vehicle to interpret experimental results. Representations are also attributed specific properties (carriers and/or encoders of information) and are even given powers of causation (Cox & Hasselman, 2013; Hommel, Müsseler, Aschersleben, & Prinz, 2001). Few authors who use the concept however, refer to the theoretical or ontological status of the representation, for example by empirically questioning its existence. After careful analysis Haselager et al. (2003) concluded that cognitive science lacks a proper operationalization of the concept of representation and is therefore unable to discuss whether a system has representations or not. Still, its use continues, far beyond the domain of cognitive science and theories are constructed that have mental representation at their theoretical core, while viable alternative theoretical frameworks are ignored (cf. Dreyfus, 2002).

1.3 THE INTERPRETATION FALLACY IN PSYCHOLOGICAL SCIENCE: COMPLEX SYSTEMS ROACH?

The interpretation fallacy in psychological science arises when a researcher is confronted with a theory that is based on an ontology that is very different from the one he or she prefers. The problem is this preference is more often implicit than not, possibly for reasons explained in the previous paragraph. We will present striking examples of the expression of this implicit preference for ontology that leads to immediate and strong rejection of the alternative ontology. We cannot be certain however that the scholars expressing the preference also perceive it as such. An additional complication (for which we have less sympathy) is that it is generally not known what the formalism is within which a researcher in psychological science is

constructing his or her theories. Both unknowns can be the source of some misunderstanding at best, but quite often the result is not very pretty.

Here is a quote from a reviewer commentary of a manuscript co-authored by two of the current authors. It was rejected for publication because: *“As a general comment, I'm afraid I find this work conceptually misconstrued, methodologically flawed, and, ultimately, theoretically uninformative. [...] Many terms used in the background and discussion are vague, if not void of specific content, such as flexibility and stability, coordinative patterns, self-organization, and so on.”* Another commentary, a different rejected manuscript: *“If this means anything, then I was unable to determine what. I was not convinced by the claim that self-organization is here anything other than a “fancy metaphor”, certainly nothing that the authors present establishes this.”* Don't worry, both manuscripts were published in other journals and are cited by scientist who find the results useful in their work, but the point here is of course that the papers were rejected because the reviewer could not determine what the theoretical predictions, the results and their interpretation had to do with his or her own familiar perspective on reality.

There are also several articles in which authors explicitly complain about the fact that they cannot interpret predictions and measurement outcomes of theories in terms of their own preferred constituents of reality. A clear example is a recent commentary by Wagenmakers, van der Maas, and Farrell (2012) entitled: *“Abstract Concepts Require Concrete Models: Why Cognitive Scientists Have Not Yet Embraced Nonlinearly Coupled, Dynamical, Self-Organized Critical, Synergistic, Scale-Free, Exquisitely Context-Sensitive, Interaction-Dominant, Multifractal, Interdependent Brain-Body-Niche Systems.”* The authors evaluate the promise of 15 years of the complex systems approach to cognitive science and claim it has failed to live up to its promise because: 1) phenomena associated with complex systems (such as fractal scaling and self-organized criticality) are *“mysterious phenomena”*; 2) Cognitive scientists are not interested in the methods of complexity science, because they are too *“vague”, “too general”* and *“mostly about 1/f noise”* and 3) Cognitive scientists are not interested in complexity science because they want to *“infer latent cognitive processes”* (text in parentheses are literal quotes from the paper). The authors conclude the claims of the approach will appear to the cognitive scientist as: *“mostly speculation, wrapped in jargon, inside wishful thinking”*. On the bright side, at least multiple scales will be involved in their evaluation. This tendency to explicitly prefer results that can be interpreted as a mechanism of component processes over these *“mysterious phenomena”* is encountered in several papers by this group of authors (Torre & Wagenmakers, 2009; Wagenmakers, Farrell, & Ratcliff, 2004, 2005).

We see this as a classical case of the interpretation fallacy at work. In other words, because these scholars discard the theory and its predictions as vague, uninteresting and void of meaning, because they evaluate it for its explanatory power, which has no place in theory evaluation. To convince the reader who has never heard the terminology before that we are really dealing with a clash of ontology, the mysterious phenomena listed above such as (multi-) fractal scaling and self-organization are exactly described by mathematical formalisms (Arneodo, Bacry, Jaffard, & Muzy, 1997; Aschwanden, 2011; Ben Mabrouk, 2008; Mandelbrot & van Ness, 1968; Riedi & Mandelbrot, 1995; Zentrum, 1995) and observed in concrete

models like recurrent neural networks, potential functions, self-organizing maps and dynamic field models (Haken, Kelso, & Bunz, 1985; Hopfield, 1982; Suyari, Kamitani, & Matsuba, 2000; Thelen, Schöner, & Scheier, 2001). Processes like self-organized criticality and emergence are considered lawful universal phenomena based on first principles by physicists, that's why they are general (cf. Aschwanden, 2011; Verlinde, 2011). Modern roboticists report to have drawn inspiration from complex system approaches in ecological psychology, embodied, embedded cognition and situated cognition, disciplines they consider to be cognitive science (Rome, 2008, p. 5). The cognitive psychologist apparently does not consider the work of these scientist actual playing pieces on their chessboard. This is odd, considering that traditional cognitive architectures have not produced usable systems for the intelligent, adaptive robotic control in dynamic environments, not even for an artificial roach (cf. Clark, 1999a; Lenat, 1998; Schöner & Santos, 2001). The chess AI was excellent though, but it wouldn't scale down, not without getting rid of the representations (cf. Chemero, 2009; Clark, 1999b; Edelman, 2003). This shows the possible danger of allowing ontology to play a role in the evaluation of a theory. The roboticists will leave us staring at an empty chessboard, as they will soon have learned more about complex adaptive human behavior than psychological science ever did (e.g. see the program of cognitive developmental robotics Asada et al., 2009).

One of the great physicists of the 20th century, Richard Feynman, candidly explained how large the gap between interpretation and theory evaluation in the context of the extremely low explanatory power of quantum theories can be:

“The difficulty really is psychological and exists in the perpetual torment that results from your saying to yourself, ‘But how can it be like that?’ which is a reflection of uncontrolled but utterly vain desire to see it in terms of something familiar.... Do not keep saying to yourself...‘But how can it be like that?’ because you will get...into a blind alley from which nobody has yet escaped. Nobody knows how it can be like that.” (Feynman, 1994, p. 123)

This quote was used by Bem (2011) to justify why he didn't provide a satisfactory theory or mechanism to explain precognition and retroactive influence. The argument seems to be that it should just be accepted these phenomena exist based on the empirical evidence he presented, enter the Columbus delusion. Even if quantum physics could come up with a theoretical model: *“we have no familiar everyday phenomena in which information travels backwards in time.”* (Bem, 2011). Feynman's quote is indeed about the inability to interpret quantum physics in terms of everyday experiences, but the torment is due to the fact that the physicist also has the most accurate formal description of those phenomena, a scientific theory. In fact, although other authors have focused on statistical inference and replication, we argue that a mature science would have rejected Bem's paper on theoretical grounds alone. No need to discuss estimation of Bayesian priors for the psi phenomenon (Bem & Utts, 2011; Wagenmakers, Wetzels, Borsboom, Kievit, & van der Maas, 2011; Wagenmakers & Wetzels, 2011), the empirical success of the entire discipline of quantum physics is the prior that cannot just be ignored by a genuine scientific endeavor.

Feynman states the main cause for the physicist's torment is psychological.

We have already argued this might be the case perceptually, but what is expressed here is the sheer frustration felt by very intelligent men and women when they cannot understand something. Bem's statement as we paraphrased it here, provides an accurate description of the cause of the general resistance felt by certain authors to consider predictions by theories based on different, or more abstract ontology. We must conclude though, that by asking for explanations in terms of familiar phenomena, they evaluate the explanatory power of the theory and not its ability to produce precise and accurate predictions. Concerning the complex systems approach, the number of studies that bring observables like fractal scaling under experimental control and can thus subsequently be evaluated for precision of predictions and empirical accuracy is steadily increasing (Correll, 2008, 2011; Kello, Beltz, Holden, & Van Orden, 2007; Kuznetsov & Wallot, 2011; Orden, Holden, & Turvey, 2005; Wijnants, Bosman, Hasselman, Cox, & Van Orden, 2009; Wijnants, Cox, Hasselman, Bosman, & Van Orden, 2012). Do not expect however that these authors will attempt to explain their results in terms of mechanisms or components (Diniz et al., 2011 compares predictions by opposing views).

Every concrete model is born out of abstract thoughts. The real sin is in forgetting about those thoughts and mistaking them and the concrete models to represent objects in reality. A scientific discipline employing a method of theory evaluation based on the ability of its scientists to understand why nature behaves as it does, is at best an immature physical science, probably just stamp collecting (cf. Ernest Rutherford), at worst however, it is not a scientific endeavor at all (cf. psychological science described as a Cargo Cult Science, Feynman, 1974).

1.4 LOST IN EMPIARRHEA: THE ELUSIVE ÆTIOLOGY OF DEVELOPMENTAL DYSLEXIA

All the phenomena in the empirical record of a discipline of science were observed in the context of some philosophical perspective in reality. A scientist not being aware of this fact is not a problem exclusive to psychological science. Its probability of occurrence is likely higher, due to the reasons that cause the interpretation fallacy. The tendency to neglect the philosophical foundations of empirical records is accurately summarized by Dennett (1995) who writes: "*There is no such thing as philosophy-free science; there is only science whose philosophical baggage is taken on board without examination.*" When this philosophical baggage is not examined, contamination by all three symptoms is very likely. The interpretation fallacy must be at work because the scientist does not examine the ontology that produced the results. It leads to the Columbus delusion, as the scientist doesn't notice a philosophical perspective guides his empirical inquiries. Finally, it can cause the empiricist's blind-spot, because anomalies to the implicit ontology that generated the data are not recognized as such.

This turns the scientific cycle into a vicious cycle because the implicit ontology will not be subject to falsification. Anomalies, or unexpected results are interpreted as a failure of the measurement process to uncover (parts of) the familiar implicit ontology (i.e., we need a bigger and better measurement apparatus / a different test to measure the elusive predictor / a larger sample of subjects). Another possibility is they are seen as new phenomena that need to be accounted

for (or ‘saved’) by constructing auxiliary hypotheses. Such a boundary of instant hypotheses serves to protect the current stable theoretical core from falsification (cf. Lakatos, 1976). The empiricist’s blind-spot causes scientific endeavors to generate a huge unstructured record of empirical facts that have no say whatsoever on the level of competing theories: ‘Empiarrhea’. These theories are *theories of construction* (Box 2, left pane), which account for empirical findings as if they were phenomena predicted by a theory, whereas in fact they are just existence proofs to the implicit ontology (possibly no more than descriptions of actual sensory phenomena). This is a scenario not unlike the historical one sketched in the context of the alchemical and humour ontology. Does the picture we sketch actually apply to psychological science?

An example of what happens when all three symptoms interact to disturb theory evaluation in a domain of scientific inquiry; we turn to the theories about the aetiology of developmental dyslexia. Developmental dyslexia refers to the inability to acquire a proficient level of reading and spelling performance after about two years of regular literacy education in the native language of a child. Though many dyslexic readers will benefit from a timely intervention, the impairment will persist into adulthood with varying severity. Estimates of people afflicted with this specific learning disorder vary from country to country and ranges from 5-20% of the population (Blomert, 2005). What exactly causes developmental dyslexia, its aetiology, is still the topic of heated debates. This has been the case for quite some time, at least since competing theories appeared in the literature in the 1970s. These theories disputed whether the reading deficit was caused by impaired auditory processing or impaired phonological processing (Bradley & Bryant, 1978; Tallal & Piercy, 1973). What may be reasonably expected of a discipline of science is that it works towards a more unified description of reality instead of a more diverse one: What started with two theoretical accounts is today a plethora of different theories, deficits and hypotheses, all published in peer reviewed journals, backed by a considerable empirical record, containing everything from behavioral data, neurophysiological data, to genetic data. Data were observed experimentally, by comparing subpopulations (age, language, reading experience, comorbidity, etc.) or by decade long prospective studies.

A quick review of the recent literature reveals the deficits reported span almost *all modalities of perception* (Beaton, Edwards, & Peggie, 2006; Breznitz, 2003; Goswami, Fosker, Huss, Mead, & Szűcs, 2010; Huss, Verney, Fosker, Mead, & Goswami, 2010; Skoyles, 2004; Talcott et al., 2003; Tallal, 2004), include deficits in *motor control* (McPhillips & Jordan-Black, 2007; Nicolson & Fawcett, 2006; Ramus, Pidgeon, & Frith, 2003; Savage, 2004), *balance* (Rochelle & Talcott, 2006; Stoodley, Fawcett, Nicolson, & Stein, 2005), *attention* deficits (M. Reynolds & Besner, 2006; Shaywitz & Shaywitz, 2008; Valdois, Bosse, & Tainturier, 2004), *impaired cognitive abilities* (Aleci, Piana, Piccoli, & Bertolini, 2010; Heim et al., 2008; Helland, 2007), *fluency of naming* (Araujo, Pacheco, Faisca, Petersson, & Reis, 2010; Vaessen, Gerretsen, & Blomert, 2009) *learning* (Menghini, Vicari, Mandolesi, & Petrosini, 2011; Nicolson, Fawcett, Brookes, & Needle, 2010; Vicari et al., 2005) and *language* (Berninger, 2000; Joanisse, Manis, Keating, & Seidenberg, 2000; Koster et al., 2005). The studies reported here represent just a small anthology of the literature and the

actual number of deficits proposed by scientists is much larger.

There were such high expectations when in vivo brain imaging and gene sequencing became available as a tool for psychological science. However, neuroanatomical facts, neural and genetic correlates of behavior of individuals and populations related to developmental dyslexia have not been decisive in the resolution of any theoretical dispute. It has not provided a consensus on the aetiology of the reading disorder (Dowker, 2006; Marinelli, Angelelli, Di Filippo, & Zoccolotti, 2011; Pugh et al., 2001; Ramus, White, & Frith, 2006; Ramus, 2003, 2004; Stanovich, 1988), neurobiological evidence has not given more scientific credibility to one theory of (impaired) reading over others (Heath, Bishop, Hogben, & Roach, 2006; Howes, Bigler, Burlingame, & Lawson, 2003; Mechelli, Gorno-Tempini, & Price, 2003; Ramus et al., 2003; Ramus & Szenkovits, 2008) and it has certainly not helped with achieving a clearer definition of developmental dyslexia as a specific learning disability or developmental psychopathology (Bishop & Snowling, 2004; Fletcher & Lyon, 2008; Frith, 1999; Landerl & Wimmer, 2000; Lyon, Shaywitz, & Shaywitz, 2003; Lyon, 1995; Wolf, 1999). To date, there is no specific genetic profile (cf. Grigorenko, 2001), no specific brain structure or function (cf. Caylak, 2009; Eckert, 2004) and no specific psychometric test profile (Blomert & Vaessen, 2009; Ramus & Szenkovits, 2008; Wimmer & Schurz, 2010) that can serve as the ultimate diagnostic tool for developmental dyslexia. The main difference with the times of Galaburda and Kemper who in 1979 presented evidence for the anomalous cytoarchitecture in a post-mortem study of the brain of an adult developmental dyslexic reader, appears to be that now every proposed deficit comes with its own neural correlates, treatment program and often their own associated genetic anomalies as well (Grigorenko, 2001; Ramus, 2004).

What appears to have occurred here is that *any* significant pattern in the data was interpreted as a phenomenon of significance for the ætiology (i.e. the Columbus delusion). Although direct tests of predictions by different theories exist, the empirical results have no authority on the level of the competing theories, even if those results pertain to empirical studies of the brain or genes (possibly due to the interpretation fallacy). These theories are in empirical gridlock, or perhaps due to the sheer volume of relevant phenomena, there is ontological indifference. In any case, empiarrhhea is the result and each phenomenon is saved to support a specific theoretical deficit and each deficit ends up with its own anatomical and functional brain anomaly and genetic anomaly. If there is anything neuroscience and genetics have done, it is that they have provided the theoretical core of the deficits an ontological truth status: They must exist as actual constituents of reality. The result of the three symptoms is multiple disciplines of science contributing to produce theories of construction.

Frankly, we find this very disturbing. We could have chosen other subjects to arrive at the same conclusion, but in the case of developmental dyslexia it is a real possibility that lives of children were adversely affected because scientist are unable to properly evaluate the theories they invent. This possibility becomes more likely when we consider the 'technology' that the scientific disciplines involved produce: Interventions that should ameliorate reading and spelling performance of dyslexic readers. A whole range of at times quite esoteric interventions, some without any

apparent relation to reading and spelling, continue to be proposed by serious scientists in serious scientific journals. The interventions reveal we are dealing with theories of construction and scientists who are a realist about the phenomena those theories predict. The proposed treatments are clearly inspired by the ontology associated with the deficit and the rationale behind most of these interventions is not much different from releasing or increasing quantities of a humour to resolve an imbalance. Recent examples of studies reporting a positive effect are: Adding Fish Oil (Omega-3 fatty acids) to the diet to improve the diminished myelination of the magnocellular part of the central nervous system (e.g. Cyhlarova et al., 2007 → *magnocellular deficit hypothesis*); Using coloured lenses and coloured overlays to improve reading fluency (e.g. Lightstone, Lightstone, & Wilkins, 1999; Whiteley & Smith, 2001) → *visual disturbance / visual stress hypothesis*); Music therapy (e.g. Overy, 2003 → *rhythm / coordination imbalance hypothesis*); Intensive training with tonal sweeps and acoustically modified speech (e.g. Tallal, 2004 → *auditory temporal processing hypothesis*); Neuro-Feedback training (e.g. Breteler, Arns, Peters, Giepman, & Verhoeven, 2010 → *brain dynamics deficit hypothesis*); Training motor skills (e.g. D. Reynolds, Nicolson, & Hambly, 2003; D. Reynolds & Nicolson, 2007 – *cerebellar deficit hypothesis*); Presenting stimuli to visual, auditory and touch modalities opposite a dysfunctional hemisphere (e.g. Smit-Glaudé, van Strien, Licht, & Bakker, 2005) → *hemispheric balance model of reading and dyslexia*); Training Rapid Serial Naming of pictures, colours, numbers and letters (e.g. Eleveld, 2005 – *rapid naming / fluency deficit hypothesis*). These interventions were chosen because they appear rather unorthodox and they all but one report positive effects. There are of course many more published effective interventions based on more familiar repetition learning of reading and spelling performance.

We find it hard to imagine that anyone would conclude that these theories, the associated empirical record and technology, are real in the scientific sense: They all precisely and accurately predict phenomena in reality. We also do not believe that this plethora of theories is entirely due to QRPs, post-hoc hypothesizing, faulty review procedures or worse. What we *can* imagine, is that each and every theory has a high explanatory power. Of course, if dyslexic readers as a population have impaired motor skills and their cerebellum has anomalous anatomy and function, one should attempt to train their motor skills and see if reading and spelling performance improves. It does, measurable as a significant effect compared to control groups 18 months after the training (D. Reynolds & Nicolson, 2007). We believe, like the physicists working towards the quantum formalism that all the phenomena in the empirical record should be accounted for. They cannot all be anomalies to one another. Let's assume for a moment that what we are really dealing with here is an implicit ontology that escapes from being falsified due to the interpretation fallacy, the empiricist's blind spot, and the Columbus delusion. What could it be?

We will return to suggest an answer to this question that may be more general than just the case of developmental dyslexia, after we introduce our proposal for dealing with theoretical diversity. We hope it will eventually turn every psychological scientist into a theory evaluator. All that is needed is a sensible ontological commitment, theory evaluation as the giants intended it, and the structural realists's

Box 1. An adaptation of Einstein’s original schema for theories of principles (see footnote 3 for acknowledgements). **E** is the manifold of sensory experiences, reality as we experience it. **S** refers to “laws” of nature, which together (or in isolation) constitute a theory about **E**. **A** is a system of axioms, propositions and postulates, the formalism of Box1.

guide to theorizing about the universe.

2. STRUCTURAL REALISM: THE BEST OF BOTH WORLDS?

To summarize the story so far, there may be three related causes that help explain the current state of theory revision in psychological science:

- 1) It is the result of a science that often uses interpretation, or the evaluation of explanatory power to decide on the plausability of theoretical predictions and measurment outcomes. This makes scientific theory evaluation almost impossible.
- 2) It is the result of a science that is mostly unaware of the formalism and ontology that guides the attention of empirical inquiry, generating trivial measurement contexts and phenomena (with respect to the authority of theories), that flood the empirical record. If there is no consensus formalism like in physics, theory evaluation is almost impossible because anomalies and alternative hypotheses can easily be considered to not pertain to the domain of the theory under attack. If the ontology of a theoretical prediction is not recognized for what it is, the prediction will likely be negatively evaluated as meaningless, uninteresting

or incomprehensible.

- 3) It is the result of a science that constructs its theories in order to try and save as many phenomena as possible that support the existence of a specific (implicit) ontology. Anomalous phenomena should be considered failed confirmations of predictions of measurement outcomes generated by a theory (Wagenmakers, Wetzels, et al., 2012). However, it appears almost all the empirical phenomena can be saved (cf. Ferguson & Heene, 2012 and the Dyslexia example), which makes it difficult to uphold the idea that the discipline is evaluating and revising its theories at all.

A crisis of confidence often harbors a crisis of identity and in paragraph 1.2 we asked an existential question, explanatory power or precision and accuracy? Psychological science considers itself an empirical science, but there two possible definitions of this term: 1) A science of empiricists. 2) A science that tests the theories it produces using empirical evidence (i.e. not a purely axiomatic deductive science). We believe number 2 should be the correct answer and if it were, we would have advised a switch from theories of construction to theories of principles and this would have been sufficient to recover from the crisis. However, the discipline may have evolved (or degenerated) into the first meaning: A science whose practitioners uphold the view that experience, especially of the senses, is the only source of knowledge. As is shown in Box 2, this is exactly what theories of construction are about, their “laws”, S are directly derived from E , the manifold of immediate sensory experiences. Box 2 depicts Einstein’s view on scientific theorizing about reality³ as related in a letter to a friend in 1954. He rejected theories of construction and called them products of an immature science. A theory of principles should be preferred, but creating one is no easy challenge. There is no logical route to arrive at a formal system A directly from E . The formalism is created due to an intuitive leap, a creative act in which a conjecture is raised to a postulate (sometimes indeed a matter of aesthetics, cf. Giner-Sorolla, 2012). The only route that is deducible by logic and reason flows from the formalism in A to the derived laws S and from those laws to E . The theory, and only the theory decides what we observe. To create a theory of principles implies sacrificing explanatory power, because the theoretical objects that more precisely and accurately describe reality, are no longer connected to familiar sensory phenomena. This was not just a coincidental oddity for Einstein, he firmly believed that the fundamental notions of physics cannot be induced from experience and they cannot be justified a priori on the basis of our faculty of knowledge. The latter strongly opposes Kant’s notion of incomensurability between Biology and Physics (e.g. Hasselman, Turvey, Seevinck, & Cox, 2011); Seevinck, 2011).

What complicates the shift from constructed to principled theories in the case of psychological science is the implicit, hidden role of ontology. We want to stress that a confirmatory research agenda will not solve these problems (although we wholeheartedly agree it should be implemented), nor can reciprocal informing of empirical inquiry and formal modelling (Forstmann, Wagenmakers, Eichele, Brown,

³ The interpretation of Einstein’s philosophy of science is based on the work of Jeroen van Dongen (e.g. 2010), Michael Seevinck (e.g. 2011) and of course Einstein himself.

& Serences, 2011) or the use Bayesian inference. This is also the main issue raised by Fiedler et al. (2012) and we can only join them in their plea for rewarding strong inference in psychological science. We propose this may be achieved by the way structural realism answers the realist question.

What *should* we be realist about in science, if anything at all? A scientific realist believes that our best scientific theories do not merely save the empirical phenomena, but rather that it is reasonable to believe that they capture, in an approximate way, the inner structure of the universe. From the previous paragraphs it might have become clear that it should not be measurement outcomes or ontology we should be realist about in science. The history of science teaches us one certainty: Ontology will be proven wrong, awkward, silly, or downright preposterous eventually (Humours; the *Æther*; Phlogiston; Gemmules; the *Id*, *Ego* and *SuperEgo*; *Vis Nervosa*; Heat and Cold Particles, etc.). There is a way to reconcile the lessons from the history of science and the modesty about what scientific theories tell us about reality. Worrall (1989) presented structural realism (SR) as: “*the best of both worlds*”, as capturing the main pro-realist argument (the no miracles argument) while countering adequately the main anti-realist argument (the pessimistic meta-induction, i.e., the argument from the history of scientific revolutions). The no miracles argument runs as follows: “[...] *some scientific theories enjoy enormous empirical success; if these theories are not even approximately true, their success is miraculous; on the other hand, if these theories are approximately true, their success is not miraculous; we should thus infer that such theories are approximately true.*” (Ainsworth, 2010).

At the basis of the pessimistic meta-induction argument is the fact that there has been ontological discontinuity across theory-change (French & Ladyman, 2003). The argument lists occurrences of such changes in the history of science and concludes by induction that it is most likely that the ontology of our currently accepted theories will also be radically revised sooner or later. This is taken to undermine scientific realism. For example, Fresnel’s wave theory of light was, by any standards, empirically successful: it correctly predicted various surprising results, for example, the existence of a white spot at the center of the shadow that is cast by an opaque disc that is held in light coming from a point source. This theory presupposed that light is a wave transmitted by an all-pervading mechanical medium we have encountered before, the luminiferous *Æther*. Since physicists no longer believe that such a medium exists, the theory appears to be not even approximately true (Ainsworth, 2010).

Worrall (1989) proposed SR (in its epistemic form) as a means of reconciling these two arguments (early versions of structural realism can be found in the works of Russell, Poincaré, Carnap). The idea is that 1) we should believe what scientific theories tell us about the structure of the unobservable world, but 2) we should be skeptical about what they tell us about the posited ontology of the unobservable world. Thus, for example, “[...] *it seems right to say that Fresnel completely misidentified the nature of light, but nonetheless it is no miracle that his theory enjoyed the empirical predictive success that it did; it is no miracle because Fresnel’s theory, as science later saw it, attributed to light the right structure.*” (Worrall, 1989,

p. 117). Worrall claims that Fresnel's theory attributes to light the right structure because Maxwell's theory of electromagnetism (which succeeded Fresnel's) retained Fresnel's equations. Fresnel's theory attributed to light the wrong nature because the *interpretation* of these equations is altered. The alleged mechanical nature of light was indeed replaced by the field-theoretical one (i.e., electromagnetic). In general, Worrall claims that with respect to such revisions in ontology, "[t]here was continuity or accumulation in the shift, but the continuity is one of form or structure, not of content" (Worrall, 1989, p. 117).

So again the conclusion is that interpretation (i.e. to provide content) is irrelevant for theory evaluation. The goal of theory evaluation is to uncover structure. Fresnel's equations were retained, because it is important that everything happened according to those equations and not because of what scientists imagined those physical quantities to represent in reality. However, interpretation did play a kind of perturbing role here: The empirical accuracy of the equations was high, its ontology untenable as a constituent of reality and this prompted the search for something new. At these special stages of theory revision, interpretation can instigate a scientist to be bold and raise a conjecture to a postulate. Defining a system of axioms or formalism is like taking an inspired intuitive leap: It occurs when scientists start to notice that a particular interpretation of a theory seems to generate new predictions or allows explanation of known anomalies to current theory. When something like that happens, a change of formalism and ontology and perhaps unification may be imminent. Getting hung up on one version of reality certainly does not increase the chances of such a leap ever occurring. Einstein prescribed the following and it applies to the practitioner of psychological science now more than ever. A scientist:

"[...] must appear to the systematic epistemologist as a type of *unscrupulous opportunist*: he appears as *a realist* insofar as he seeks to describe a world independent of the acts of perception; as *idealist* insofar as he looks upon the concepts and theories as free inventions of the human spirit (not derivable from what is empirically given); as *positivist* insofar as he considers his concepts and theories justified only to the extent to which they furnish a logical representation of relations among sensory experiences. He may even appear as a *Platonist* or *Pythagorean* insofar as he considers the viewpoint of logical simplicity as an indispensable tool of research." (Einstein, 1949. Reply to Criticisms in Schilpp, 1949. Emphasis added).

To build a theory of principles and raise a conjecture to a postulate, a scientist must be an unscrupulous philosophical opportunist and stretch his mind and creativity to explore and be inspired by the perspectives of many different versions of reality philosophers have to offers. That is why we believe science is one of the arts studying the structure of reality and not an industry that produces facts about reality.

This realism about structure allows for a modest scientific realism. What makes structural realism stand apart from other varieties of realism is that it is more cautious in its realist claims, so as not to fall prey to the pessimistic meta-induction

over the history of science, yet at the same time, to remain sufficiently substantive to provide a basis for the no-miracle argument. The only explanation for the fact that in sequences of successively accepted theories each theory is empirically and technologically at least as, and generically more, successful than its predecessor is that they latch onto the structure of the physical world better and better. Modern day versions of structural realism assert that all we can know about the world is its structure (epistemic variety), or that structure is all there is (ontic variety). For the purposes of this paper it is not important to adopt one of these two variants of structural realism (there are more); the important thing is that we should focus on structure and not get hung up on ontology. The first step to take is to change perceived purpose of the scientific cycle.

2.1 THE STRUCTURAL REALIST'S GUIDE - STEP 1:

TURN THE SCIENTIFIC CYCLE INTO A TEST BED FOR ONTOLOGY

The examples discussed to reflect the three symptoms show that most of psychological science is probably hung up on ontology, but not aware of it. The example about the theories of developmental dyslexia illustrates it is likely the scientific cycle has turned into a vicious cycle. Given this predicament, what work can SR do for the advancement of psychological science? Some authors would argue that the reductionist approaches of the physical sciences can not be applied to psychological science and that the current theoretical diversity is fundamentally linked to the field, something we should accept as inevitable (McCauley & Bechtel, 2001), just like representationalism and mechanistic theories (Bechtel, 2009). This position is known as explanatory pluralism (e.g. Dale, Dietrich, & Chemero, 2009; Dale, 2008) and we interpret it to promote a utilitarian approach to theory evaluation where there can be a peaceful coexistence of theories (ontological indifference) until one theory has proven to be more useful than others. We will not discuss this position here (see Hasselman, Dale, et al., 2011), but kindly disagree with this program and promote the SR approach: Ontological commitment should be the driving force behind theory evaluation. This can only invoke a fear of reductionist fundamentalism and blinding monism, if it were done without the realization that ontology is only a temporary vehicle for understanding. The explicit goal of ontological commitment is to crash-test theory and ontology against reality. We can only learn about the quality of our vehicles, the structure they latched onto, when they break down. Then we should proceed to build new ontological vehicles, maybe using the parts that survived the crash-test, otherwise design something completely new. The scientific cycle should be like a test bed for ontology, not a trench war or a neutral zone.

The SR approach actually allows for a neutral monism (Stubenberg, 2010; Tully, 1999), which is something very different from ontological indifference. Our theories of psychological phenomena need to be about compositional relations (Healey, 2010), not about a compositional hierarchy implied by a strict monism: *"The aim of science is not things themselves, as the dogmatists in their simplicity imagine, but the relation between things"* (Poincaré, 1905, p. xxiv). A number of scientists studying psychological phenomena have argued to adopt this aim of science in the

past, perhaps now is the time review their pleas (e.g. Chemero, 2003; Gibson, 1979; Kugler, Shaw, Vincente, & Kinsella-Shaw, 1990; Michaels & Carello, 1981; Shaw & Turvey, 1999; Thelen, Kelso, & Fogel, 1987; Turvey & Shaw, 1999). The number of revisions and unifications of theories of principles, not the number of individual confirmations and replications of theories of construction, is what will eventually define the success of the field of psychological science.

It is useful to discuss whether methods of the physical sciences apply to psychological science. The problem with some frequently used arguments is that they suffer from the Columbus delusion: Physical systems are not as complex as human beings, who can be studied on so many different levels of analysis (e.g. Dale, 2008) and are extremely noisy (e.g. West, 2010). That's true, but scientific reason dictates this implies we should start by educating ourselves about the successes of physical theories and not settle for less, like early 20th century inferential statistics and models based on Newtonian dynamics and a universe of efficient causes. In fact, the latter comment is somewhat of an insult to Newton's achievements; most of psychological science believes their theories and statistical models describe change processes (pre-post, within subject designs, age as a predictor, longitudinal studies using three measurement occasions), without ever considering a model based on his differential calculus actually describing physical change processes (e.g. Smith & Thelen, 2003; Vallacher & Nowak, 2009; van Geert, 1991).

The levels studied in physics span the infinitesimal quantum scale to the size of the universe and this has not been a problem. In fact there are theories describing the connections between relevant levels such as statistical mechanics that have been applied to provide a formal description of group dynamics in living systems (Schweitzer, Ebeling, & Tilch, 2001). Turbulence is about as noisy as it gets in the real world and very successful analysis techniques and models have been developed to make sense of detecting structure in extremely noisy environments (Takens, 1981; Zbilut, Giuliani, & Webber Jr., 1998). These techniques have already found their way to the study of human physiology and psychology (e.g. de Graag, Cox, Hasselman, Jansen, & De Weerth, 2012; Goldberger et al., 2002; Richardson, Dale, & Kirkham, 2007; Shockley, Baker, Richardson, & Fowler, 2007). Here we must point to the fact that experimental psychology has been using the classical reductionist approach of perturbation analysis and factorial experiments since it's emergence as a field of science (Baayen, 2010; Bechtel, Abrahamsen, & Graham, 1998; Donders, 1868; Townsend, 1984). A physical system is brought into a static controlled environment, is perturbed by an impulse or stimulus in that environment and is subsequently studied for its impulse response. These responses are then analyzed using statistical models that search for latent sources of unique variance and random noise in order to recover the unique effect of the perturbation on the system. It works quite well for classical physical systems, but should we really expect the same for living systems? They are indeed more complex as was argued for, so why study humans as classical physical systems? The search for unique effects of causally efficient factors (components) in a perturbable system *is* fundamentalist reductionism (Chemero & Turvey, 2007; Ihlen & Vereijken, 2010; Van Orden, Pennington, & Stone, 2001).

The structural realist will remember Einstein's dictum and realize that the fact that we observe independent components in a factorial experiment is decided

by a theory about mechanistic component architectures of cognition and behavior (e.g. Stepp, Chemero, & Turvey, 2011). Experiments based on different architectures have no reason to try and look for components and they will predict measurement contexts, observables and outcomes that bear no resemblance to factors or efficient causes (e.g. Lichtwarck-aschoff, 2012; Stephen, Dixon, & Isenhowe, 2009; Van Orden, Holden, & Turvey, 2003; Wijnants et al., 2009). As long as the theories are evaluated based on their predictive power and empirical accuracy, the scientific cycle can do its work. It can be expected that theories based on two different views on reality cannot both be understood within one view. We have argued that using this fact to discard a theory as useless, means it is evaluated for its lack of explanatory power to a particular scientist's preferred metaphysics. Like with religious beliefs, the factors that decide this preference are often trivial and have to do with the geographical location of a scientist's academic upbringing, advisors, colleagues, that one paper that changed everything, etc. Therefore, this preference should never play a role in theory evaluation. If both theories prove to be precise and accurate, finding a common framework should be the goal of scientific endeavors. This was achieved in the quantum formalism that revealed the wave and particle ontologies of matter and light were two sides of the same structural coin.

How do we change our theories into crash-test dummies and set the scientific cycle to uncover structure? We are realists, so we are well aware this cannot happen overnight. Therefore we present a priority list with some guidelines for practitioners and students of psychological science to initiate change:

- *We're in this together.* Recognize ontological commitment and the clashes that will result from such commitments for what they are: A means to build better vehicles for scientific discovery that will benefit the entire discipline (i.e. not a way to build better scientific careers, or a fan base, or a commercial enterprise). Open review, open access to papers, data and materials, open science is what we need (e.g. Nosek, Spies, & Motyl, 2012).
- *Be an unscrupulous philosophical opportunist.* Be promiscuous! Realize divorcing your favorite ontology for a younger, smarter, better version, is a scientific virtue and not a scientific sin. Do not stay together for the offspring you created, history of science will reveal they were hideous little monsters anyway.
- *Be a militant unification theorist!* Unification does not equal reductionism, when theories are unified more structure of reality is uncovered, not less. The fear of reductionist fundamentalism arises only when the ontology the unified theory is based upon is given reality- and truth- status by its users. Instead, try to realize the truth: *"There is no spoon!"* (Wachowski & Wachowski, 1999) and there certainly is no compositional hierarchy of nature to be uncovered (Healey, 2010).
- *What you cannot create, you do not understand.*⁴ When you start to experience delusions about having found the one-true ontology for psychological phenomena and in your dreams a unified architecture of consciousness and human behavior is emerging at the horizon, ask yourself the following question: *"Where are the robots?"* The walking, talking, thinking, dreaming robots you

⁴ On the day of his death, the blackboard in Richard Feynman's room apparently read: *"That which I cannot create, I do not understand"*.

were promised in the sixties and seventies to take care of your daily chores in the year 2000... so you could focus on the important stuff. The intelligent machines that accurately diagnose disease and operate on you while you have your breakfast... where are they? One even starts to wish for the evil unstoppable ones who set out to destroy their creators. Yes, we still have long way to go before we can rightfully claim to have acquired scientific knowledge about human behavior. Looking at what has been accomplished already can never be an argument to stop looking for better accomplishment in science.

2.2 THE STRUCTURAL REALIST'S GUIDE - STEP 2:

FEELING THE PAST – EVIDENCE FOR ANOMALOUS RETROACTIVE INFLUENCES ON THE FUTURE

The SR stance dictates that the theoretical diversity of any science should somehow be linked, that is, there always is an underlying structure of which all theories are mere concretizations or instantiations in a particular domain. This provides us with a measure by which any science may be said to advance, if they address this diversity. Future versions of formalism and theory will have a deeper insight into the underlying structure of reality. Whether ontology will be able to provide deeper and better explanations of reality is doubtful. The 'discovery' of a structural connection between a set of phenomena or between existing theoretical constructs, driven either by converging empirical evidence or by conjectured theoretical relationships, is what scientific progress is all about. As an important corollary of this process, it helps us to realize when ontologies meet their makers and are ready to be recycled for spare parts, or should be discarded completely. The only way such progress can be achieved is to make the search for these structural connections an explicit goal of the scientific endeavor. We identified this goal as a missing chapter in the discussion so far; hopefully we have at least convinced the reader that an ontological trench war will get us nowhere.

We need to set the example and take a step towards exploring a connection to the work that was used to illustrate our critiques on contemporary theory evaluation (paragraph 1.3). As may be clear by now, our perspective on reality is SR, which means we commit to an ontology to guide our scientific inquiries. We are currently of the opinion it is sensible to assume that human beings should be studied as physical, living systems whose behavior is based on first principles and according to natural law. This means theories about phenomena of human psychology should be based on the same structure as theories about other life forms. Claiming exclusive, or radically different structure for human life than in biology or other life sciences, in our opinion, excludes it from being a scientific claim (e.g. because of the Bem-prior).

The complex systems approach does not deny there are components to be found in human behavior; the definition of a system is a set of components that are structured by an organizing principle. The point is that the dynamics of a system differ when it is studied as a whole (interactions dominate behavior) or when parts of the system are studied in isolation, for instance in a classical perturbation experiment (components dominate behavior). Contrary to most classical physical

systems, living systems are open, dissipative and in a state far-from-thermodynamic equilibrium (i.e. they can maintain and develop the complexity of their own structure by extracting energy from the environment that is dissipated back as heat). Further distinctions can be made based on the adaptability (e.g. Freeman, Kozma, & Werbos, 2001) and the intentionality (e.g. Shaw, Kadar, & Kinsella-Shaw, 1994) of the behavior of living systems. We therefore prefer the term agent-environment system, also because we do not want to exclude artificial life. Complexity Science provides tools (metaphorical, experimental and formal) to study such systems, but we will be the first to acknowledge that there is no consensus theory, physical or otherwise, of living complex systems. Some authors feel that meaning and information are missing from current descriptions (Barbieri, 2006), others add closure to efficient cause (impredicativity, Chemero & Turvey, 2010) or self-organized criticality (Van Orden, Kloos, & Wallot, 2009). In fact, we have good reasons to believe (elaborated below) that the current tools of complexity science do not suffice for the study of agent-environment systems (see e.g. Cox, Hasselman, & Seevinck, 2011).

Such is our current temporary perspective on this particular domain of reality, to which a possible connection was reported in the critical paper discussed earlier, by Wagenmakers et al. (2012). The authors noted that studies had shown standard models of cognition could sometimes be seen as special cases of nonlinear, models with many interacting components (complex systems). They conjectured that the tension between old science and the complex systems perspective might only be apparent, as the latter justifies the former. We gladly unwrap our alleged wishful thinking from our jargon and speculations and raise this conjecture to a postulate!

Postulate 1: In psychological science, a model of component-dominant behavioral phenomena associated with a classical mechanical system, is justifiable only when it can be shown to be a special case of a model of the interaction-dominant behavioral phenomena associated with a complex living system.

This should not be considered a paradigm-shaking postulate, unless one does not agree with the idea that human beings are complex living systems. The conjecture by Wagenmakers et al. (2012) is not new however and has been known to psychological science since the 1940s through the work of W. Ross Ashby published in the *Journal of General Psychology* (Ashby, 1945, 1947). Much of his scientific work dealt with explaining how complex living systems like human beings may be studied by psychology as self-organizing dynamical systems and that there is no fundamental objection to consider a complex system a machine, or a mechanism (Ashby, 1945, 1947, 1962). This covers exactly the problems with the complex systems approach reported by Wagenmakers et al. (2012). Ashby preferred clear definitions and formal descriptions, striking in the current context is the following quote:

“Here I have completed this bird's-eye survey of the principles that govern the self-organizing system. I hope I have given justification for my belief that these principles, based on the logic of mechanism and on information theory, are now essentially complete, in the sense that there is now no area that is grossly *mysterious*.” (Ashby, 1962, p. 274)

There need be no mystery in psychological science about self-organization and complexity to those who want to model mechanisms and information. We recommend anyone who doubts this, to study the three papers by Ashby cited here.

We elaborate on postulate 1. Ashby provides two arguments for his proposition. The first is what we have been arguing for; organization is in the eye of the beholder. Two researchers observing the same beehive may conclude differently about its organization. One sees the hive as the result of the interactions of 50.000 bee-parts and concludes a high level of organization, while another who observes whole states, such as activity and dormancy, may observe no organization at all (Ashby, 1962, p. 259). The second argument is about the independence of dynamics and organization. A system, that appears an irreducible whole of interacting parts, can always be shown to consist of separate parts by suggesting what a part might look like and assuming a similar (isomorphic) part exists. If both parts are isomorphic to the whole, the parts will indeed be observed. Ashby provides a formal example of this phenomenon and concludes:

“Thus, subject only to certain requirements (e.g. that equilibria map into equilibria), *any dynamic system can be made to show a variety of arbitrarily assigned “parts”,* simply by a change in *the observer’s viewpoint.*” (Ashby, 1962, p. 260).

That about covers what we were after, a structural connection expressed in formal language and it has been known to psychological science for some time. We conjecture that components and component dynamics (mechanisms) are the default ontologies for many theories in psychological science today, due to the adoption of the experimental methods of classical physics intended to study classical systems. Ashby explicitly mentions the systems he has in mind are not restricted to classical mechanical systems with Newtonian dynamics (Ashby, 1945, p. 14). Apparently this suggestion made by the ecological psychologists in the 1970s, the dynamical systems theorists in the 1980s and 90s and currently by proponents of the complex system approach, 73 years after Ashby, is extremely controversial in psychological science. Perhaps those proponents, should they have suggested their stance as a temporary ontological vehicle with strong roots in the history of the discipline instead of the new true perspective on reality (i.e. the SR way), there might have been less resistance. We are prepared to raise our conjecture (which is definitely not new) to a postulate as well:

Postulate 2: In psychological science, the observer’s perspective (a theory) decides what may be observed (as observables). It predicts measurement contexts, which consist of a preparation procedure, a measurement procedure and a theoretical object of measurement.

We feel “The Future” (Ashby, 1962, p.274) a paragraph on exciting new developments is somewhat depressing to read:

“As a particular case, a type of system that deserves much more thorough investigation is the large system that is built of parts that have many states of equilibrium. Such systems are extremely common

in the terrestrial world; they exist all around us, and in fact, intelligence as we know it would be almost impossible otherwise [...]”(Ashby, 1962, p. 755).

This cannot be a good sign for a discipline of science that wants to advance: A majority of the commentaries by Wagenmakers et al. (2012) as we related them here can be parried by formal arguments written down 52+ years ago by a scientist operating in the same discipline. Moreover, as noticed earlier, the opinion on what may be interesting ontological vehicles to explore in future studies, was less controversial then today. Meanwhile mathematics, physics, biology, physiology and computer science have expanded their object of study to include complex dynamical systems and networks and there now exists a science called Complexity Science in which psychological science is grossly underrepresented.

There are more messages from history that appear to have had little effect on the future state of affairs. Feynman in his 1974 Caltech address in which he basically referred to educational science and psychological science as poor science gave the advice to a student of psychology to first replicate a finding before trying another manipulation:

“She was very delighted with this new idea, and went to her professor. And his reply was, no, you cannot do that, because the experiment has already been done and you would be wasting time. This was in about 1947 or so, and it seems to have been the general policy then to not try to repeat psychological experiments, but only to change the conditions and see what happened.” (Feynman, 1974)

In hindsight and with absolute bias, these examples show that psychological science could have known in 1947 that its future could be bright if it had revised its theoretical outlook on living systems according to interdisciplinary consensus and its research practices that produce only theories of construction according to the giants of science.

How do we change our theories and practices to be better informed by the history and consensus positions of all of science? Again a priority list with some guidelines to initiate change:

- *Retroactive literary search, feel the past!* When considering ontology revision, either because the ontology appears mysterious (it isn't), or because it is thought to be something new (no, it isn't), look in the scientific record for the earliest references instead of the most recent ones and actually study those sources. Those shoulders are giant-sized for a purpose you know.
- *Check with the neighbors.* Make a friendly social call and examine consensus positions in other disciplines of science. Especially the ones that pertain to the study of living systems, information networks, adaptive behavior and other things that change over time.
- *Educate yourself, your colleagues and your students.* The era in which there was one scientific discipline of natural philosophy has past. There aren't *some* things you don't know about, there's a whole universe of science you don't know

about. No shame in that, it is the same every scientist. What would be of great use for psychological science though, is to intensify the study and usage of the language that allows a formal description of reality, mathematics. Mathematics is a tool you can recruit to do theoretical work for you. If there is no mathematics around to describe the phenomena you are interested in, ask a mathematician to create it for you, it is what they do for a living.

2.3 THE STRUCTURAL REALIST'S GUIDE - STEP 3:

AGREEING TO DISAGREE: CONSENSUS, DAMNED CONSENSUS AND STATISTICS

The second postulate expresses the core of our message: Something might be gained if a commonly used observer's perspective were identified and explicated and evaluated for its role in incapacitating theory evaluation in psychological science. One way to find out about this is to ask what formalism is most commonly used in psychological science. Perhaps the least controversial answer is that it is a set of theorems and axioms that describe ergodic systems (Molenaar & Campbell, 2009; Molenaar, 2008) and probabilities of random events. Assuming the object of study in psychological science is an ergodic system is very convenient as it opens up the possibility to use the laws of probability to the predict properties of the system: Ensemble statistics (central tendency measures) represented as the expected values of random variables. This is consistent with our story so far; the dynamic system theorists and proponents of the complex systems approach argue that the system studied is non-ergodic. We argued the source of the opposition against these approaches is a fundamentally different ontology, but it may also concern a completely different formalism describing a different domain in reality from which theories depart. Essential is the word *dynamic*; random processes describe change in ergodic systems where future states are not, or only slightly influenced by the history of system states. Most models of change for complex living systems concern processes that are essentially deterministic where future states are completely defined by the history of the system. This means studying the change of ergodic systems over time, is essentially not very different from observing many systems of different 'ages' at one point in time. The same logic holds for the repeated measurements of one observable of an ergodic system: Throwing one die 100 subsequent occasions gives the same expected measurement outcomes as throwing 100 dice on one occasion. That is why psychological science, most of the time, collects data in samples of human beings and calculates properties of the sample that are generalized to reflect properties of the population. The problem is, this is not what the 'technology' of psychological science is about. Psychological science applied to reality is about individuals, a patient, a consumer, a student, an employee, a developing child, an aging grandmother. In order to be able to attribute properties of the ensemble, the population to properties of the individual (statistical syllogism), the ergodic assumption is essential (but by no means assures it will be accurate or even possible in principle, see Ellis & Van den Wollenberg, 1993).

Here we return to our discussion of the theories of developmental dyslexia. It is likely that the component-dominant ontology of mechanical systems with Newtonian dynamics, together with erroneous use of statistical syllogism based on

the assumption of ergodicity caused differences between ensemble properties of populations to be wrongly attributed as actual deficits in individual dyslexic readers. This has been suggested in the literature, in the form of dyslexic readers being singled out as a separate and 'pathological' group whereas they may just represent the lower tail of a normal distribution of reading ability (Lyon & Moats, 1997; Shaywitz et al., 1999; Shaywitz, Escobar, Shaywitz, Fletcher, & Makuch, 1992). The component ontology subsequently provides a mechanism solution to restore performance by specific intervention, targeting the assumed deficient component, while the deficiency might be just apparent (e.g. Hasselman, 2011; Hendriks & Kolk, 1997; Wijnants, Hasselman, Cox, Bosman, & Van Orden, 2012). This view on pathology and impaired performance has also been criticized for neuropsychological disorders (Van Orden & Kloos, 2003; Van Orden & Paap, 1997; Van Orden et al., 2001). Recently the validity of the classification system of mental disorders (DSM IV) was questioned (Borsboom, Cramer, Schmittmann, Epskamp, & Waldorp, 2011). The authors conclude the inherent complexity of presumed mental disorders as evidenced by a network analysis of their symptoms, explains why ætiological, neuroscientific and genetic approaches fail to find the causes of mental disorders. We have great confidence that a similar analysis for the symptoms associated with developmental dyslexia will also reveal such complexity.

There is no doubt that the component ontology is successful. Even though we can explain and question the nature of its scientific popularity and intuitive success, to both scientist and layman, it still spells success. Apparently opponents of the paradigm should come up with more convincing evidence and arguments that prove something is fundamentally wrong about it. What we believe cannot be ignored here is that it is just too soon to say complexity science is a viable alternative for studying psychological phenomena, mainly because complexity science lacks a proper consensus theory about such phenomena backed by scientists studying living systems. This is thus not exclusively a problem of psychological science. More than a decade ago Strohman (1997) recognized it was too early for epigenesis (the interaction dominant theory of gene expression) and complexity science to provide a viable replacement for genetic determinism in biology, although both looked viable and with potential. It may now be the case that contemporary Biology and Genetics have accepted Epigenetics as a full-grown paradigm. It was achieved without the necessity to invoke the full range of complexity terminology that was once deemed necessary (Gottlieb, 1991; Strohman, 1997; Valsiner, 2007). Biological determinism moved to a new neighborhood and is firmly grounded in psychological science, often disguised as brain determinism or cognitive internalism (e.g. Clark, 1999a; Freeman, 1997; Smith & Thelen, 2003; Smith, 2005).

The final step to take for the discipline according to the SR approach, in addition to relentless ontological commitments by structural realists who know the purpose of their temporary tunnel vision, besides moving towards maturity by building on the structures already uncovered in the history of science (including its own history), is to achieve consensus about what it is that psychological science studies. What is needed is something akin to the early Solvay conferences in Brussels (i.e., the first five (1911-1927) chaired by H.A. Lorentz, where the leading theoretical physicists of the time met to discuss the future of physics). History relates many

entertaining stories about the clashing and smashing and displays of ontological commitment that took place at those conferences. Arguments were sometimes even based on irrational beliefs, aesthetics and thus interpretation of measurement outcomes, like Einstein's famous quote about a deity, some dice and the universe. The scientists participating were always aware of the fact that progress would have been made once the debris had settled. Even though individual scientist continued to disagree about its interpretation, the quantum formalism that emerged was a consensus formalism, supported by the majority of the field. Theories that departed from the formalism competed for precision and accuracy of their predictions, not being bothered by the confusion about their explanatory power.

We propose a list of the issues that in our opinion desperately need consensus resolutions in psychological science. The examples we provide are based on the assumption, we hope is not controversial, in which it is stated that theories of psychological science operate within the larger domain of theories that describe living physical systems as agent-environment systems (as explained in paragraph 2.2). If no consensus can be achieved, we believe a rapture is imminent which will see many scientists trained as psychologists turn to the interdisciplinary field of complexity science, joining physics, mathematics, biology, cybernetics, physiology, AI and robotics. A separate philosophy of science has already been defined (Stepp et al., 2011). Either way, how psychological science as a field, will resolve these issues, will define the future of the discipline:

1. *Consensus about relevant psychological phenomena.* When is a phenomenon a psychological phenomenon and when must theories about psychological phenomena be informed by theories about similar phenomena in other living systems? Is the distinction automatic or unreflective versus conscious or reflective behavior (e.g. Rietveld, 2010)? Is the sheer complexity of a behavior or performance by a living system sufficient? Then, should theories about complex behavior be informed by the architectural feats of social insects (Kugler et al., 1990)? Can psychological architectures explaining human memory performance be falsified by empirical data on chimps showing far superior working memory over humans (Inoue & Matsuzawa, 2007)?
2. *Consensus about psychological systems.* What is the defining nature, in terms of the necessary and sufficient (minimal) structural requirements, in which we may expect to observe *psychological phenomena*? Is this an ergodic system? If not, what kind of a system is it? Does it need to be a living biological system, in isolation or in a social environment? Does it need a central processing unit (e.g. a brain) or can its intelligence be distributed? Although capable of displaying mechanistic component dynamics (postulate 1 & 2), is it fundamentally a dynamic, interaction-dominant, self-organizing system, or can we only claim it must be a non-ergodic system?
3. *Consensus about relevant levels of description.* Where in psychological systems should we look for these psychological phenomena and where for the physical 'causes' of those phenomena? Can they be expressed as principles and laws? The genetic level, the neuronal level, the neurotransmitter level, the hormonal level, the body, the environment? Which levels can and should we connect by theory?

As a first suggestion we proposed to generalize the concept of a complex brain-body-environment system or animal-environment system in which we may observe psychological phenomena to: *agent-environment system*. A necessary condition for a psychological system then would be that it is identifiable as an agent in an environment, which can be a social environment. This makes having a brain, a body, or being alive in the biological sense, sufficient conditions for systems in which we may expect to observe psychological phenomena, but not necessary conditions. Another example: Being able to understand a phenomenon as resultant of information processing and/or computation is likely to be a sufficient condition for classifying it as a psychological phenomenon, but not a necessary condition as studies in direct perception and (radical) embodied cognition have consistently shown (e.g. Dotov, Nie, & Chemero, 2010; Michaels & Carello, 1981; Stephen et al., 2009; Warren, 1984).

3. BEYOND ANALOGY: TOWARDS A FORMALISM FOR AGENT-ENVIRONMENT SYSTEMS

“A difficulty of much psychological theorizing is vagueness in the terms employed. In this work, the above ideas have been studied in mathematical form throughout, the definitions and proofs being given corresponding precision.” (Ashby, 1945, p. 13)

Throughout this paper we have discussed vagueness of scientific discourse, either perceived vagueness due to conflicting ontology, due to lack of operationalization of a concept, or as evidenced by an empirical record that does not appear to contain anomalies that pose a threat to any proposed theory. As we alluded to on several occasions, it would be very beneficial to the scientific endeavor of psychology to move beyond analogy and towards formalizing the descriptions of the system and phenomena it studies. If the problem were framed like this by Wagenmakers et al. (2012) including the necessary self-reflection, we would have agreed and joined the scolding parade, laughing at our shared misfortunes. Take the often cited drift diffusion model as introduced by Ratcliff (e.g. Ratcliff & Rouder, 1998) or more recent versions (e.g. van Ravenzwaaij, van der Maas, & Wagenmakers, 2012): It models the accumulation of information by assuming an accumulation process that is a model of a very specific physical change process known as Fickian diffusion (e.g. Mandelbrot, 2001). Why choose this type of diffusion and not other kinds more commonly associated with living systems? What is meant by information? This is a mathematically well defined concept in physics, information science and even biology (e.g. Barbieri, 2008; Freeman, 2000), but here we don't really know. This is not less vague than using self-organized criticality to predict outcomes of an experiment, both are comparisons by analogy (e.g. van der Maas, 1994). The argument of not being specific enough applies to many models. If the Cusp catastrophe can be applied to detect transitions in attitudes (van der Maas, Kolstein, & Pligt, 2003) as well as transitions in X-ray emission from the black-hole binary Cygnus X (Wagenmakers, Molenaar, Grasman, Hartelman, & van der Maas, 2005), we might as well write a paper entitled: “Human cognition and a galactic

black hole: a discussion on nonlinear dynamics and phase transitions” (cf. Wagenmakers, Farrell, et al., 2005). Note however that the physics community prefers to discuss stochastic variability of X-ray emissions of black hole binaries in terms of $1/f$ scaling and self-organized critical processes (e.g. Aschwanden, 2011; Polyakov, Neilsen, & Timashev, 2012).

A possible outcome would be a consensus position that included the rejection of both the ergodic system and the tools that complexity science provides. All we can really say is that the system under study must be non-ergodic. That would be an exciting result, as psychological science would then need to define the formal tools it deemed necessary to describe psychological phenomena. What the composition principle of a psychological system should be, the principles guiding its time evolution, the emergence of novelty, how wholes relate to parts and how to attribute properties to wholes and parts by measurement. We conclude with a quote that reflects the most important message we want to convey, be a realist about what scientific theories tell us about the unobservable structure of the world, but reject what scientific theories tell us about the constituents of the world:

“It follows that a substantial part of the theory of organization will be concerned with *properties that are not intrinsic to the thing but are relational between observer and thing.*” (Ashby, 1962 emphasis in original)

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