Acquiesced and unrefuted

The growth of scientific myths

Kåre Letrud

Thesis for the degree of Doctor Philosophiae (dr. philos.)
University of Bergen, Norway
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[O]nce an error gets into print it "will live on and on in libraries carefully catalogued, scrupulously indexed . . . silicon-chipped, deceiving researcher after researcher down through the ages, all of whom will make new errors on the strength of the original errors, and so on and on into an exponential explosion of errata." (Former fact-checker and editor of The New Yorker, Sara Lippincott, as quoted in McPhee, 2009)

Table of Contents

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List of Papers

Letrud, K., & Hernes, S. (2016). The diffusion of the learning pyramid myths in academia: an exploratory study. *Journal of Curriculum Studies*, 48(3), 291-302. doi:10.1080/00220272.2015.1088063

Letrud, K., & Hernes, S. (2018). Excavating the origin of the learning pyramid myth. *Cogent Education*, 5(1), 1-17. doi:10.1080/2331186X.2018.1518638

Letrud. K. (2019). The Gordian Knot of Demarcation: Tying Up Some Loose Ends. *International Studies in the Philosophy of Science*. doi:10.1080/02698595.2019.1618031. Forthcoming.

Letrud, K. (2019) *The propagation of myths in academic publications: A case study*. In preparation for review.

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Abstract

This thesis explores the phenomenon of scientific myths distributed in academic discourses. Drawing on a set of myth-examples, I explicate a definition of the term 'scientific myth', arguing that it ought primarily to be characterised by the tension between a lack of epistemic warrant on the one hand, and an extensive proliferation in formal academic channels of publications on the other. I then delineate scientific myths from the closely associated pseudosciences: The sciences, although distributing some unreliable statements, do not bestow upon such statements the same authority and importance as the pseudosciences do.

From these demarcative deliberations, I proceed to address the question of myth-diffusion, of how misconceptions grow to scientific myths. Discussing myth-propagation, I argue that a fundamental explanation for myth-spreading is deficient epistemic practices. Omissions of citations, although a both common and pertinent explanation, only account for some aspects of myth spreading.

In the context of research debates, the issue of myth diffusion also includes efforts at debunking scientific myths in academic publications. Our findings indicate that there is an 'Affirmative Citation Bias', that counteracts debunking attempts by effectively disarming the critique: Instead of being distributed, the critique is buried in an avalanche of myth-affirming publications.

Finally, I discuss whether scientific myths ought to be somewhat vindicated. They could conceivably have an anti-dogmatic function in academic debates, by representing alternatives to the established consensus, in accordance with Millsian and Feyerabendian ideals for truth seeking discourse. I conclude in the negative: Scientific myths have dogmatic aspects themselves.

Acquiesced and Unrefuted: The Growth of Scientific Myths

Academic debates, lectures, articles, monographies, anthologies, text-books, conference talks and -proceedings, often distribute unsubstantiated, or even falsified, statements. This is unsurprising, as errors are bound to happen. However, when the distribution of particular statements becomes extensive and persistent it seems fitting to refer to these statements as 'myths', or more specifically, 'scientific myths'. This phenomenon has gained little attention in the philosophy of science, and there is currently no uniform use of the term in the field (for a rough and general disambiguation of other meanings of the term, see appendix A):

Paul Feyerabend describes the authority and success of science itself as a myth, a fairy-tale of 'a subtle but carefully balanced combination of inventiveness and control.' (1975, p. 167). Whereas Karl Popper uses the issue of myths in science primarily to illustrate the uncertainty of theories in general: Science originates from myths, he claims (1969, p. 38), and what distinguishes the scientific myths from their ancestors, is neither their form nor their subject matter, it is their boldness (1974, pp. 980-981) and the attitude of critique with which they are distributed (1969, p. 127). Thus, all efforts, scientific ones included, at explaining the world and ourselves are myths, according to Popper. He does sporadically also use the term 'myth' simply as 'false idea' (e.g. 1969, pp. 53, 347), as 'a false story that is widely accepted' (1994, p. 34).

In a wider academic context, 'myth' appears to be predominantly used in this latter meaning, as 'false idea', and often rather derogatorily: In titles of research articles myths are not merely falsified, rejected, or corrected, they are quite violently 'broken' (Rakover, 1986); 'busted' (Malhotra, Noakes, & Phinney, 2015); 'shattered' (Baird & Gangl, 2006); 'exploded' (Wiley & Harnischfeger, 1974); and 'punctured' (DeBoer, Seaver, Angel, & Armstrong, 2008). Alternatively, they are 'exposed' (Cheadle & Sampson, 2003); 'dispelled' (Billings & Raven, 2013); and 'debunked' (Chavkin, 1989). When these authors categorise a scientific idea as a myth, they do not merely label it as false, they arguably cast aspersions on the idea (and indirectly so on the individuals who subscribe to it), adding insult to rejection. Thus, the term seems to serve a polemical function, perhaps more so than an analytical one.

These initial, and rudimentary, accounts of academic 'myth' usage will likely prompt the question: Does the philosophy of science need a scientific myth concept? Popper's concept merely serves an illustrational function, and the academic use of this term seems to amount to little more than name-calling. Is there a need for another superfluous, vague, value-laden concept to talk about flawed claims? Larry Laudan, when rejecting the use of similar terms, writes:

> If we would stand up and be counted on the side of reason, we ought to drop terms like 'pseudo-science' and 'unscientific' from our vocabulary; they are just hollow phrases which do only emotive work for us. As such, they are more suited to the rhetoric of politicians and Scottish sociologists of knowledge than to that of empirical researchers. (1983, p. 125)

I believe, however, that philosophers of science, as well as researchers in general, can put a 'scientific myth' concept to good use. For theoretical and philosophical discussions, it will be a supplement to the conceptual toolbox, allowing differentiation between forms of fringe science. For empirical use, explicating a definition of 'scientific myth' will enable studies of scientific myths as a phenomenon in its own right, thereby opening up several routes of inquiry. Demarcating scientific myths will facilitate studies of the mechanics of scientific myth-propagation, and the myths' effects on research and knowledge, on political deliberations, and professional practices.

In a practical perspective, developing a definition for this phenomenon may ease and increase the effort of identifying and debunking myths proliferating in academia, and hopefully counteract their impact. These myths substitute research-based beliefs. They effectively obscure epistemic blind spots, concealing the fact that these issues have been inadequately examined.

Appropriate questions remain unasked, for authoritative answers to them are already circulating in the research literature, in journals, textbooks and encyclopaedias. Thus, addressing scientific myths can potentially reduce work and money wasted, and conceivably improve the quality of, say, healthcare, and teaching.

Furthermore, the term is useful, not despite of its value-ladenness, but because of it. It implicitly accentuates the flawed research behind the flawed claims – the practices, structures, and incentives that allow and incite myths to proliferate within the academic debates. For example, addressing scientific myths introduces a collectivistic perspective on questionable research practices that otherwise are regarded merely as trivial and individual mishaps. Slight breaches of accepted research practices are minor issues when considered separately, but they accumulate into a substantial problem. In this thesis, I try to accentuate some of these minor forms of bad research, and recast their role and importance.

I shall pursue a definition of 'scientific myth' based on a set of myth-examples, and argue that 'scientific myths' is best addressed as statements that are extensively propagated by academic publications, despite their inadequate warrant. As such, these myths do not differ significantly from pseudoscience: They are statements that are unsupported by, or even incompatible with, the relevant research on the issue. They do, however, play different roles in their respective disciplines. While scientific myths are extensively distributed in academic publications, the pseudosciences can be defined as being based on statements such as these.

I shall seek to answer two key questions about the diffusion of scientific myths: what causes the propagation of scientific myths? And, how is the decade-long distribution of these myths possible in the critical, even sceptical, environment of research? The first question, I argue, is inherently normative due to the mores regulating scientific discourse. I seek to answer it by exploring an episode of myth dissemination, and theorise some potential breaches of responsible research practices that can explain the propagation. In order to answer the second question, Chief Librarian Sigbjørn Hernes and I examine the academic reception of myth-debunking publications, and we[1] argue that these efforts are not only inefficient, but may also be counterproductive.

As suggested by this introduction, the term 'scientific myth' is value-laden, and negatively so. I have argued that the use of the term is primarily derogatory, and that the use of scientific myths in epistemological and practical deliberations have negative consequences. But, perhaps is this a one-sided view. They could conceivably have a positive function in academic debates. Using Feyerabend's principle of theoretical pluralism I discuss whether scientific myths supplies the debate with alternative perspectives that help to correct or bring the dominant theories forwards.

What Are These Things Called 'Scientific Myths'?

Before proposing a definition of 'scientific myth', I shall introduce four cases that I consider examples of scientific myths: The Learning Pyramids; The Hawthorne Effect; The Yerkes-Dodson Law; The Inflexible Sphex. These shall exemplify and thematise the issue for the reader, and serve as paradigms and borderline cases in the ensuing discussions.

The Learning Pyramids

The Learning Pyramids have inspired and informed this thesis in several ways, and constitute a cogent example of what I consider to be a scientific myth. The singular term 'Learning Pyramid' is somewhat misleading, for 'The Learning Pyramid' subsumes a plethora of incompatible models (some with other names, or no name), and there is no consensus regarding which is the right one (there is none). The diversity of these conceptions is so comprehensive that unless one is familiar with their common history, their kinship can easily go unrecognised.

These models all make comparative, and usually quantitative, claims about the effect of different learning and perception modalities on retention, such as listening to a lecture, reading, participating in discussions, teaching others, and so forth. The rates of retention usually follow

[1] In this cover essay 'we' refers to the collaboration between me and Hernes.

increments of 10: e.g. 10 %, 20 %, 30 %, 50 %, 70 %, and 90 %. Some versions contrast the effectiveness of lectures and reading, with practicing and teaching others. Others stress the value of direct experience as the best way to increase retention. In general, the Learning Pyramids hold almost every form of learning superior to reading and attending lectures. I shall in this cover essay use the singular form 'Learning Pyramid' when referring to specific varieties of this conception, while the plural 'Learning Pyramids' extends to the whole family of these retention models.

Hernes and I (2018) demonstrated that proto-versions of these models were being published at least as early as 1852, predating by decades the field of empirical retention studies. We also showed that they started out as unquantified folk psychological sayings about memory persistence, that gradually evolved into quantified models of amount of information retained, while increasingly becoming associated with research. In publications from 1906 and onwards, we found several varieties of these categories and retention effects quantified by fractions (usually fractions of 10). Gradually these fractions were converted into percentages (Letrud & Hernes, 2018). In 1967 training manager and advisor at Mobil Oil, D.G, Treichler, published a version that later, and quite incidentally, became widely cited. Under the headline 'people generally remember', Treichler relates this version of the retention rates:

> 10 % of what they read
>
> 20 % of what they hear
>
> 30 % of what they see
>
> 50 % of what they hear & see
>
> 70 % of what they say
>
> 90 % of what they say as they do a thing (1967, p. 15. Emphasis in the original.)

Sometime during the sixties or seventies, these retention charts were synthesised with a completely unrelated model: Edgar Dale's 'Cone of Experience'. This amalgamation introduced the common triangular design as well as learning modalities pertaining to educational media.

U.S. educationist Edgar Dale plays an essential but irreproachable part of the history of the learning pyramid. In 1946, he published the book *Audio-Visual methods in teaching*, where he introduced a triangular model ranking different learning aids according to their level of abstraction.[2] In the next edition (1954) the model has a conical shape, and accommodates for educational television. In the third version (1969) Dale superscripted U.S. psychologist Jerome Bruner's categories 'enactive', 'iconic', and 'symbolic' on the cone.

Dale sought to rank the different forms of audio-visual aids according to their level of abstractness and concreteness. The model was not intended for making general claims about the efficiency of some methods of learning over others, or that the learning process necessarily would have to move from concrete experience to abstraction, or the other way around. In the first edition Dale makes it a point to specify this under the heading 'What the cone is and is not':

> If we realize now that the bands on the cone frequently interlap and blend into one another, and that a child who can read and write use verbal symbols, there will be no mistaking our cone-device for a hierarchy or rank order of learning processes. It is understood for its intention – to show how sensory aids are classified in terms of more or less concreteness and abstractness. (1946, p. 47)

Despite these clarifications, his 1946 model became so frequently misinterpreted as a hierarchical model of learning effectivity, that he devoted six pages in the second (1954) and third (1969) edition to address these misconceptions.

[2] A facsimile of Dales' 1946 cone can be found in the attached article 'The diffusion of the learning pyramid myths in academia: an exploratory study' (Letrud & Hernes, 2016).

At some point the misrepresentations of the cone as models of learning efficiency were synthesised with the aforementioned retention chart. Despite blanks in this part of the pyramid's history we can claim that it did happen: Several versions show the chart superimposed on Dale's triangular model. The earliest known version of the amalgamated Learning Pyramids is attributed to Bruce Nyland, under the name 'Cone of Learning' (cf. appendix B). The model features a pyramid very similar to Dale's Cone of Experience, with categories and percentages near identical to those corroborated by Treichler. At what time this synthesis happened is not known, nor who initiated it. U.S. engineering educator James E. Stice gathers that he first encountered Nyland's amalgamated cone as a hand-out at a trainer-seminar at the University of Wisconsin sometime between 1970 and 1973 (Stice, personal communication). The oldest published synthesised Learning Pyramid we uncovered is featured in R. Ted Nugent's (1977) *Family Cluster Programs. Resources for intergenerational bible study*. The following year Ann R. Bauman (1978, p. I 15) published a similar model in a trainer manual for the National Institute on Drug Abuse.

Despite their idiosyncrasies, plasticity, and lack of evidence, the Learning Pyramids have proliferated within educational debates both inside and outside of academia for more than one and a half centuries, and they are currently widely publicised in peer-reviewed journal articles and encyclopaedia articles. The extent of the academic distribution, however have been efficiently obscured by the diversity of the models and their names. We identified versions of the Learning Pyramids featured in 418 peer-reviewed journals, and in 11 field-specific encyclopaedia articles, all published between 1990 and 2013 (Letrud & Hernes, 2016). Currently, our list contains 524 peer reviewed articles and 14 encyclopaedia articles published between 1990 and 2019.

The Hawthorne Effect

Much like the Learning Pyramids, the Hawthorne Effect comes in a wide range of incompatible varieties (Adair, 1982). Unlike the Learning Pyramids, these theories did originate from an actual study: an investigation of worker productivity at the Hawthorne Works factory in Hawthorne, Illinois, conducted between 1924 and 1933. These studies resulted in, among other things, the surprising discovery that the workers increased their productivity, simply by being the centre of attention, or less colloquially: 'Variously defined, the central idea is that behaviour during the course of an experiment can be altered by a subject's awareness of participating in the experiment.' (Jones, 1992). Different interpretations and expositions of the Hawthorne Effect have been widely propagated for decades, despite that the original data show no evidence of such effects (Jones, 1992), and that the studies themselves have several and grave methodological flaws (Kompier, 2006).

Searching Scopus for articles and reviews mentioning the 'Hawthorne Effect', produced 4070 titles (search date 14 May 2019). A non-representative sample (the 20 latest publications on the list) produced 15 articles affirming the effect (two were unavailable to me, and three did not address the effect). Furthermore, the effect is widely reiterated by methodology text-books (books.google.com, search string *method AND "hawthorne effect",* produced nearly 500 titles).

The Yerkes-Dodson Law

My third myth-example is an oft-cited study by Yerkes and Dodson exploring 'the relation of strength of stimulus to rate of learning' (1908, p. 459). They presented dancing mice with one white and one black passage-way, and habituated them to select the white passage-way, by administering an electric shock whenever they chose the black passage-way. Reportedly, the electric chock increased their black and white discrimination, unless it was too strong, or too weak.

The original study was tentative and preliminary, with no statistical tests or analyses being performed, and the design had several flaws. There were for instance no consistent levels of shock between the sets, and the study was severely underpowered, with sample-sizes never exceeding four mice (Hancock & Ganey, 2003; Teigen, 1994). Furthermore, one of the three sets produced a linear curve, rather than an inverted 'U' (Teigen, 1994).

The golden mean of the Yerkes-Dodson law (also referred to as 'the Inverted U theory'), has become a well-established law of psychology, being applied to nearly every aspect of life.

There is a variety of models describing the effects of punishment, reward, motivation, drive, arousal, anxiety, tension or stress upon learning, performance, problem-solving, coping or memory (Corbett, 2015; Hancock & Ganey, 2003; Teigen, 1994). Yerkes and Dodson's 1908 article has 2,861 citations in Scopus, and 2,534 in Web of Science (search date 4 June 2019).

The Inflexible Sphex

The story of the repetitive to the point of rigid behaviour of digger wasps is less widespread than the above myth examples, but is nonetheless informative. Based solely on a tendentious reading of single cherry-picked inconclusive century-old study, cognitive scientists, led by Daniel Dennett and Douglas Hofstadter, have repeatedly claimed that sphex digger wasps mechanically repeat the same behaviour endlessly, however inefficient, and hence irrational. When returning to the burrow with a paralysed cricket, she (i) drags the game to the threshold before she (ii) enters and inspects the burrow. She then (iii) returns to drag the cricket inside. If the researcher moves the cricket a few inches away while the wasp inspects the burrow, the wasp routinely and invariably repeats steps (i) and (ii) before proceeding with step (iii). Moving the cricket each time she is inside inspecting the burrow will catch the wasp in a behavioural loop. Despite an extensive number ethological studies reporting flexible behaviour, cognitive scientists have since the 1960s reiterated the claim that the sphex's rigidity hinders adaption of its behaviour to the situation, presenting it as an established contrastive illustration to human consciousness and cognition (Keijzer, 2013).

The distribution of the Inflexible Sphex is less extensive than the three-aforementioned myth-examples. Keijzer lists 11 publications on cognitive science or philosophy of cognition, which is not a substantial number (and is unlikely intended to be exhaustive). Google Scholar estimates in total 404 citations of Dean Wooldridge (1963, 1968) on the mechanical behaviour of the Sphex, but only 66 of these mentions 'wasp', and 59 'Sphex' (search date 16 May 2019). Additional searches in Google Scholar with the truncated term 'Sphexish*', Hofstadter's popular term for the behaviour, produced 72 results, of which approximately 20 were peer reviewed articles propagating the conception. Presumably, these findings include duplicates, and I suspect that the total number of peer reviewed publications propagating this model is well below 100.

Defining 'Scientific Myth'

In this section I explicate a definition of the term 'scientific myth'. Explicative definitions[3] are suggested rules for language use (Carnap, 1962; Næss, 1975). '[T]he purpose is … to improve upon the definiendum by refining or supplementing its meaning.' (Quine, 1951, p. 25). Explications are tailored for the needs of a specified context, and they are assessed by their aptness, not their veracity.

Explicative definitions, unlike real definitions, are not descriptions of objective essences (e.g. Water is H_2O) (Greimann, 2012). Instead, these definitions are put forward as attempts at clarifying the meaning of vague or ambiguous terms, in order to facilitate communication and practical application in a specific context. As such, they are used more frequently in the social sciences than in the natural sciences (the definition of 'planet' being a famous case of explication in the natural sciences (Murzi, 2007)). Scientific myth has more in common with the concepts of sociology (like power, and poverty) than with the atomic properties of chemistry.

The normative approach of explicative definitions distinguishes them from descriptive, lexical definitions of existing usage. And unlike the neologisms of stipulative definitions, they propose a meaning for a term already in use (Belnap, 1993; Gupta, 2015).

The definition of 'scientific myth' proposed by this thesis is intended to describe and subsume a wide range of flawed statements proliferating in academic discourses, including, but not limited to, the above examples. Whether the reader accepts the rationale behind this definition or not, will largely depend on the perceived theoretical and practical usefulness of such

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[3] In Norwegian-speaking contexts, particularly in Examen philosophicum (a 10 ECTS compulsory introduction to the history of philosophy and critical thinking), these are referred to as *regelgivende/stipulative/normative (ord-/nominal-) definisjoner*. Alistair Hannay translates Næss' 'regelgivende definisjon' with 'prescriptive definition' (Næss, 1966): 'a definition based on a decision to the effect that an expression T in a certain specified context, or in all contexts, is to be interpreted in the same way as another expression U.' (1966, p. 45). Næss' prescriptive definitions include both explicative definitions, as well as stipulative definitions (Næss, 1975, p. 66).

a concept. I have argued for the usefulness of a developing a scientific myth concept in the introduction, and hopefully the reader has found the four myth-examples representative, and perhaps even motivating.

The criteria for explicative definitions are context sensitive. What counts as a good explicative definition largely depends on its ability to serve a specific need. E.g., political debates will often require definitions that are different from judicial, and scientific debates. However, Carnap (1962) has seminally suggested four general criteria for assessing explications[4] that can also be applied in this context: Similarity, Exactness, Fruitfulness, and Simplicity:

The criterion of Similarity seeks to balance the conflicting needs of making a better concept, while keeping sufficient correspondence to the old to make it an improvement, not a replacement (thereby changing the subject). This means subsuming all instances of the category, and only them (Brun, 2017).[5] How closely the definiens (explicatum) ought to correspond to the definiendum (explicandum) is debated (Hanna, 1968), but I suspect keeping some degree of familiarity in this respect is at least pragmatically wise, to prevent the proposed explication from being received less favourably.

Carnap's criterion of Exactness is unclear, but I gather that the definiens needs to reduce vagueness, as well as be adequately distinct from other, potentially overlapping, concepts, used in the relevant context: '[T]he rules of its use (for instance, in the form of a definition), is to be given in an exact form, so as to introduce the explicatum into a well-connected system of scientific concepts.' (1962, p. 7). For the definition of scientific myths, this requires that it reduces vagueness, but also that it can be adequately delimited from related concepts, and from the closely associated 'pseudoscience' in particular.

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[4] Brun (2016) notes that Carnap's explications are not necessarily definitions, but I shall, admittedly for the mere sake of convenience, make no attempt at keeping them separate

[5] Coincidentally, the general criteria for definitions offered by the standard theory for definitions, eliminability and conservativeness ('(1) a definition of a word should explain all the meaning that a word has, and (2) it should do only this and nothing more.' (Belnap, 2008, p. 276)) has wrongly been ascribed to Stanisław Leśniewski for years, constituting, in the words of Urbaniak and Hämäri (2012), a myth.

Fruitfulness requires that the definiens supports the formulation of universal statements, and laws in particular. The fruitfulness of the 'scientific myth' definition would presumably be judged on its usability for general descriptions and explanations of scientific myth proliferation. And finally, the definiens ought to be as Simple as allowed by the three foregoing criteria.

I propose the following definition of scientific myth: *Let 'scientific myth' signify the same as 'factual statement propagated by a large number of reviewed academic publications to an extent and duration that is inordinate to the overall weight of the evidence and counterevidence published'.*

As suggested by the above myth examples, this thesis uses the term 'science' in a wide sense, including the social sciences (as well as the humanities). I suspect speaking of science in a narrow sense would exclude many, perhaps the majority, of scientific myths: I have so far not come upon scientific myths, in the sense of the present thesis, about issues belonging exclusively to the natural sciences (the applied natural sciences not necessarily included), and I suspect there may be a general, Kuhnian (1970), point to be made about the normal sciences being less susceptible to myth-distribution than pre-paradigmatic disciplines. Thus, the myth examples that I have let inform these deliberations are predominantly on issues belonging to, and distributed by, the social sciences.[6]

By 'factual statement', I mean that it is stated in the affirmative, and that it can be assessed as being true or false. The distribution of scientific myths is affirmative, i.e. as asserting the myth. In the context of publishing, this consists of stating the myth without contradicting it, or qualifying its veracity. Thus, if communicated as a speculation, or as some sort of heuristic, it will not be an instance of myth-distribution. Furthermore, there are scientifically accepted ways of assessing its truth value. The methods for assessing such statements varies between the fields, and comprise both theoretical and empirical arguments, and quantitative and qualitative approaches. I tend towards not limiting myths to empirical claims, where the disciplines have

[6] There is, admittedly, a theoretical risk that my choice of paradigms will be inadequate to address scientific myths of natural sciences, and make the discussion lopsided in favour of the social- and health sciences.

accepted methods for assessment of non-empirical claims (although, the existence of mathematical or metaphysical myths is presumably a hypothetical issue). The above myth-cases include theories about relationships between independent variables (stimulus, learning activities, being observed) and dependent variables (learning, retention, and behaviour), and general descriptions (the digger wasp's rigid behaviour). It can perceivably also comprise claims concerning particular facts, events, or persons. For instance, several apocryphal Freudian quotes (Elms, 2001), and the unsupported attribution of 'Let them eat cake.' to Marie Antoinette (Barker, 1993), may qualify as scientific myths.

The expression 'overall weight of the evidence' might suggest that there must exist at least some evidence. This is not the intention. I seek to include not only instances where the evidence is weak, or flawed, but also negative (even falsifying), or simply missing or non-existent.

As for the review status of academic publications, I have opted for including several forms of review: unblinded, single- and double-blinded, both peer-review and editorial review. The latter admittedly have a lesser status, because the editors rarely have specialised knowledge about every subject treated by the papers submitted to them. It is a form of academic gatekeeping, however. At this point, the reader might correctly interject that preprint repositories like arXiv, PsyArXiv, and bioRxiv (and presumably also the newly launched medRxiv)[7] are major debate arenas and publication channels for several of the STEM fields, the life sciences, as well as economics and psychology, and that the above proposed definition ought to include publication channels such as these. However, these repositories do not contain the final version of record for

[7] The moderators of arXiv arguably serve as a similar function as reviewers: 'Our expert moderators verify that submissions are topical and refereeable scientific contributions that follow accepted standards of scholarly communication (as exemplified by conventional journal articles).' ("arXiv Primer," 2019). While biorRxiv follows an even more moderate line of moderation: 'Articles are not peer-reviewed, edited, or typeset before being posted online. However, all articles undergo a basic screening process for offensive and/or non-scientific content and for material that might pose a health or biosecurity risk and are checked for plagiarism.' ("About bioRxiv,"), as does medRxiv: 'Articles on medRxiv are not peer-reviewed, edited, or typeset before being posted online. All manuscripts undergo a basic screening process for offensive and/or non-scientific content and for material that might pose a health risk and are checked for plagiarism.' ("About medRxiv," 2019). I could not find specific information about the moderation for PsyArXiv, only: 'PsyArXiv uses post-moderation. When you submit this preprint, it will be assigned a DOI and become publicly accessible via PsyArXiv. Your preprint will only become private if rejected by a moderator.' ("Create Preprint," 2019)

these texts, and by the time they have undergone review and revisions, and been published by a publisher, any references they contained to a scientific myth could have been removed. Nonetheless, the distribution of research articles appears to be changing, as does peer review (e.g. PubPeer), and this part of my definition may become defunct in the future.

When addressing the distributive aspect of scientific myths, the definition is admittedly vague (in re the criterion of Exactness). I cannot specify the number or type of publications required, nor what constitutes an inordinate distribution level, or the weight of evidence (negative evidence included). Quantifying the level of distribution, and specifying the required evidence and type of academic publication would be arbitrary. There are presumably different publication practices in different fields, some prefer journal articles to monographies or anthologies, for instance. And what makes a level of distribution inordinate to the weighted evidence would depend on the evidence-forms and -standards of the field: Where the researchers share a common paradigm, and agree on the criteria for evaluation, a smaller distribution could conceivably be considered a myth, compared to pre-paradigmatic fields, where there is less agreement concerning the criteria for puzzle-solving.

Whether the suggested definition of 'scientific myth' is apt for the above specified needs depends primarily on its ability to clarify the term in an exact, simple, generalisable, and adequately conservative way. In the following I shall make my arguments for addressing scientific myths in the form of reviewed academic publications rather than beliefs, and why I believe 'scientific myths' ought to include inadequately substantiated statements, and not necessarily those that are false. I shall address the criterion of Simplicity primarily when arguing for approaching scientific knowledge as publications rather than beliefs, and Similarity when rejecting falsehood as a necessary criterion for scientific myths. Delimiting scientific myths from pseudoscience, I seek to demonstrate that although somewhat vague, the definition is Exact at least in this respect, and I also show how it can be Fruitful, by allowing explanations of scientific myth proliferation (I shall, for the reader's convenience, capitalise the initials of the criteria 'Similarity', 'Exactness, 'Fruitfulness' and 'Simplicity' when applied).

Scientific Myths as Distributed by Academic Publications

I hold that one ought to speak about scientific myths as distributed by academic publications, and not as beliefs, i.e. as propositional attitudes of individual researchers or groups of researchers. My line of argument is as follows: I briefly argue the case that scientific myths are in essence collective phenomena, which would place them within a framework of intentional collectivism, if referred to as beliefs. However, discussing three central models of collective epistemology: Commitment, Distributed, and Summative (following Bird, 2014, 2019), I argue that the first two fail to subsume scientific myths, and while the third is more successful in this respect, I still consider it to be an unsatisfactory solution. Instead I make a case for addressing myths in the form of academic publications: It is in better agreement with the criterion of Simplicity, but there is also a formal aspect to scientific knowledge that is closely related to the status of scientific publications, and the study of scientific myths is itself closely integrated with the main medium of scientific knowledge.

Regarding the collective character of scientific myths, it seems evident that myths are not private misconceptions. A myth as myth is discernible and describable at a supra-individual level, and not as the discrete propositional attitudes of individuals. Consider the oft-used analogy of rumours and myths with an epidemic, or a pandemic: In order to study an epidemic, one must consider the speed of spreading, the geographical diffusion, the demographics of the carriers, the replication cycle of the infectious agent, and so on. Likewise, only when subsuming these separate beliefs as a 'scientific myth', is it possible to accentuate the spreading, and effects of these misconceptions.[8]

[8] A similar parallel between cultural units of information or imitation ('memes'), and viruses, is famously drawn by Richard Dawkins in 'The Selfish Gene' (1976). The evolution, diversification and adaption of the Learning Pyramids can presumably be framed in somewhat Darwinian terms. However, I have chosen not to write a section 'Of myths and memes', because the meme theory has only minor (if any) explanatory power on scientific myth distribution and

Myths, and scientific myths, are sometimes defined as beliefs, or convictions (e.g. McGee, 1985; Segal, 2015), thus requiring a group of researchers to hold this belief. To the best of my knowledge, there are three models for group beliefs currently being debated: the commitment model, explaining collective belief as an agreement between members of a body reached through an internal process; the distributed model, where the collective belief of a community is the product of the distribution of cognitive labour; the summative model, seeing collective belief as reducible to individual beliefs. I shall in the following discuss all three models, and argue that they all face the same problem: While these models are differently equipped to describe scientific myths as collective beliefs, they all require that these myths are internally consistent. As exemplified by the Learning Pyramids and the Yerkes-Dodson Law, these models must allow for considerable discrepancies between the different versions they subsume, and a group collectively believing in one of these myths, will necessarily hold several incompatible beliefs.

The Commitment Model. Margaret Gilbert (e.g. 1987, 2004) has offered an account of collective belief that disconnects collective belief from the individual intentional states of the group members, addressing these beliefs much like one would the public positions and policies of formal groups and organisations. As a member of a social group one sometimes finds oneself committed to a joint belief that is not necessarily one's own. Gilbert paints a picture of a poetry discussion group that collectively arrives at the conclusion that the last line of Philip Larkin's 'Church Going' is quite moving. The group ascribes to this belief as a whole: 'We are agreed that the last line is moving'; 'We think that…'; 'We decided that…' (1987, p. 190). Although some, or even several, members may find the ending bathic, or may have yet to reach a conclusion, they nonetheless, and for a variety of reasons, can decide to 'let a certain interpretation "stand"' (p. 191) as the position of the group. In principle, this belief may stand even if none of the members of the group holds this belief themselves.

development. Its original intent was to illustrate the importance of the replicator in natural selection, not propose a substantial theory of human culture (Dawkins, in Blackmore, 1999).

> A joint commitment is the creation of all the parties to it, rescindable only with the concurrence of all. Insofar as it involves a type of self-directed order, it involves an order issued jointly by all the parties to all the parties. (2004, p. 100)

Venting of beliefs incompatible with the belief of the social group are usually qualified by expressions like 'it is my personal belief', or 'personally, I think that', thus making a clear distinction between private beliefs and those of the group. Failing to qualify the private nature of this belief will predictably provoke a rebuke, a disapproval of the breach of the mores comprised in the joint commitment.

The internal mores and dynamics of small-scale group-discussions, as well as the policies of formal and hierarchical entities like governments and organisations correspond with local research groups, where the members are committed to a result, an interpretation of data, or the viability of a hypothesis, by adding their name to the list of authors when submitting a manuscript, or to an application for a research grant. However, the commitment model is not easily transferrable to the wider, less integrated community of scientists. Bird, when discussing the commitment model and scientific knowledge, follows Karin Knorr Cetina in that large research teams, like those at CERN, does not, and cannot, come together to agree on a common understanding and commitment, due to the highly varied and specialised forms of expertise and tasks involved (Bird, 2014, p. 47). Also, wider and looser knitted groups of researchers can share knowledge without committing to it in a group context, e.g. 'anthropologists know *Homo Sapiens* originated in Africa' (2014, p. 48. Italics in the original).[9]

[9] Regarding the norms regulating expressions of disagreement, voicing (moderately) dissenting views in this wider, non-formal research context will presumably be somewhat easier (unless concerning paradigmatic issues, Kuhn (1970)). It may also be an efficient strategy for pursuing a scientific career and making a name for oneself. Furthermore, when contended, such views are rarely qualified as personal, subjective utterances. Instead, supported with arguments and evidence, they make a claim for general acceptance.

Most of the scientific myths used as examples in this cover essay proliferate within this wider, looser, community of scientists. Scientific myths, such as the Learning Pyramids, are not local beliefs shared by close-knit groups, but widespread, both disciplinary[10], geographically[11], and temporally: The Learning Pyramids are more than one and a half century old, continually evolving and diversifying, and have been propagated by authors separated by lifetimes, disciplines and national borders. This is not a joint commitment by the members of a constituted body.

Furthermore, few of the scientific myths described in this cover essay will qualify as group beliefs as commitments, because the myths often consist of a set of incompatible beliefs. As exemplified by the Learning Pyramids, and the Yerkes-Dodson Law, these myths display considerable discrepancies between the different versions they subsume. If the distributors of the Learning Pyramids committed to a collective statement saying that 'We believe that The Learning Pyramid is correct', e.g. as a collegial community of educational and didactic researchers, this would be an agreement in name only. They would in reality commit to quite different models. And if, by an internal process, the group came to agree on the Learning Pyramid as the belief of the group, the process would presumably remove the ambiguity, and make them commit to one particular version of the myth.

The Distributed Model. The Distributed Model describes systems where complex epistemic tasks are broken down to specific functions executed by members of the group. The division of cognitive labour allows specialists to produce information, which in turn is coordinated by other members, like a crew navigating an escort carrier or landing a commercial

[10] We found the Learning Pyramids in journals and encyclopaedias from several fields. Within the natural sciences there are journals on Agriculture; Biology; Chemistry; Engineering; Material Research; Mathematics; Medicine and Health studies (Burn Care; Cancer; Geriatrics; Gynaecology; Nursing; Nutrition; Odontology; Ophthalmology; Paediatrics; Pharmacy; Physiology; Plastic Surgery; Psychiatry; Radiology; Resuscitation; Surgery; Transplantation); and Veterinary Studies. While the Social Sciences includes: Accounting; Architecture; Computing; Criminal Studies; Design; Economics; Education; Geography; International Studies; Language; Law; Library studies; Management; Music; Political Studies; Psychology; Social Studies; Sociology; Theology; Tourism.

[11] The countries of the authors' institutions suggested that the Learning Pyramid have spread to Australia; Austria; Bahrain; Bangladesh; Belgium; Botswana; Brazil; Canada; China (including Macau and Hong Kong); Denmark; Egypt; Finland; France; Germany; Great Britain; Greece; India; Iran; Italy; Jamaica; Japan; Korea; Lithuania; Malaysia; Mexico; the Netherlands; New Zealand; Nigeria; Norway; Oman; Pakistan; Romania; Serbia; Saudi Arabia; South Africa; Spain; Sri Lanka; Sweden; Switzerland; Taiwan; Turkey; United Arab Emirates; USA; Venezuela.

aeroplane (Hutchins, 1995a, 1995b). The USS Palau crew members have no joint commitment to, or full knowledge of, the navigational information and deliberations, but still, this ship can be said to be an epistemic subject knowing both its speed and course, in virtue of constituting an extended cognitive system.

In 'How a cockpit remembers its speed' (1995b), Edwin Hutchins describes the cockpit of an airplane as a cognitive system. This system includes, in addition to the pilots, external representations, like booklets, tables, notes, labels and instruments, that work as a long term- and working memory, allowing the pilot flying and the pilot not flying to cooperate and coordinate the operations required for landing. Within this system information is retained and kept for later flights, and, it allows representational states to be produced and retained for ongoing procedures. This way, the cockpit can be said, according to Hutchins, to be a functional cognitive system, producing, processing, and distributing knowledge as a whole. A similar use of external representations can also be found in research, Bird notes:

> … printed and now online journal articles, reference resources, and datasets are the obvious example for wider science, for these are the principal means by which scientific information is communicated between scientists; we may include informal means of communication, letters, emails, and blogs, as well as educational resources, television broadcasts, podcasts, and the like. We ought also to include the non-human means of generating and representing information, which may include experimental equipment, satellites, computers running data analysis software, and even robot scientists. (2014, p. 49)

Bird argues that the Distributed Model can present the wider scientific community as an epistemic subject, integrated by a Durkheimian organic solidarity: Experts depend and build on knowledge and know-how developed by scientists from other fields, e.g. palaeontologists date fossils using radiometric methods measuring the radioactive decay (Bird, 2014). Evolutionary biologists presumably turn to historians for knowledge about a particular period, say, the industrialisation of Great Britain.

So far, the Distributed Model appears to be the best bet for a collective scientific myth belief model. The Learning Pyramids spread across disciplinary boundaries, and are widely accepted as knowledge produced by experts on educational studies, or subject didactics. However, if Bird, through the functional perspective of the integrative forces of division of epistemic labour, has succeeded in rendering the scientific community as a unified subject capable of having an epistemic state, I cannot see how a belief in a scientific myth like the Learning Pyramids can be ascribed to this subject:

First, while these myths are widespread and popular, they are nowhere near enjoying the level of authority and acceptance like Out of Africa, or the Big Bang. Ascribing a belief in the Hawthorne Effect, or the Learning Pyramids, would necessarily require that the subject consisted of a subsection of the collective subject: 'some researchers believe', or 'there are researchers who believe'. Second, if there is an opinion that can be ascribed to the wider scientific community on the matter of a scientific myth, it would presumably be that the myth is de facto false. Based on the published research on memory and learning, and expert opinions on learning psychology, the Learning Pyramids are untenable. And third, if belief in one of these myths were ascribed to the scientific community as a whole, this epistemic subject would necessarily hold internally inconsistent beliefs, due to the variations of these myths, not *a* belief.

The epistemic systems described by the distributed model, as well as the social groups of the commitment model, are systems for production of rational, well founded opinions and decisions. Depending on the available evidence and deliberations, the researchers as a collective can 'suspect', 'hope', or 'become increasingly convinced' that *p* is the case, either by joint commitment or by accepting the expert opinion on the matter. But on contentious matters, the unity of the collective subject seems to break down: While an individual subject may be in a conflicting epistemic state, in a state of cognitive dissonance, confused by opposing evidence, the collective as a collective cannot hold a similar state. Both the poetry discussion group and the crew will have to produce a unified belief, by joint commitment or by distributed epistemic labour, before they as a group can be said to have a propositional attitude. Speaking about scientific myths as group-beliefs must be done in a way that allows a wide range of different and conflicting beliefs to be held by its members. I shall argue that the summative model may be in a better position to account for conflicting beliefs, but it is still less apt than approaching myths as publicised statements.

The Summative Model. The summative view of group belief was seminally articulated by Anthony Quinton:

> To ascribe mental predicates to a group is always an indirect way of ascribing such predicates to its members. With such mental states as beliefs and attitudes the ascriptions are of what I have called a summative kind. To say that the industrial working class is determined to resist anti-trade-union laws is to say that all or most industrial workers are so minded. (1976, p. 17)

The summative model is reductionistic. Saying that the industrial workers believe *p* as a group, is merely a metaphorical way of stating that most or all members of the group believes that *p*. It is, however, unlikely that the myths I have used as examples are so widely held that they qualify as group beliefs in the above summative sense. I shall therefore have to extend Quinton's model to also include fewer members. A belief in *p* in these cases can be ascribed to '(some) members of Group G': '(Some) didactic and educational researchers believe that *p*', or to some quantification of the number of these researchers (e.g. 'three out of 10).

Although not strictly consistent with Quinton's approach, it does correspond to how one often speaks about group beliefs when not specifically addressing a consensus, or a common belief or opinion. Also, the analogy of disease still holds: not all or most of a population need to be infected by a contagious disease to call it an epidemic or a pandemic.

As for *p*, these group members hold beliefs about retention and learning that are similar to the beliefs of only some members, and different from most others: e.g. Tom, Dick and Harry believes that the best way of learning is practice, Hans and Franz believes that direct experiences are more efficient, and Pierre and Paul believes that teaching others are the best way of learning. When these beliefs, primarily to an external observer, are recognised as variations of the Learning Pyramids, they are grouped together under this umbrella term. However, none of the individual members' beliefs correspond to this general and external description.[12]

Thus, it is possible to speak about a scientific myth as the belief of a group of researchers, if the above Procrustean concessions are made. The results are metaphors for both subject and belief, and it is not likely an informative way of addressing these myths conceptually. That said, empirical studies of scientific myths will presumably need to address scientific myths as beliefs at some point, and the summative model will likely be the best approach for operationalising the concept for this use.[13]

The publication approach. So far, my negative arguments. I cannot see how an epistemically distributed, or an internally committed, collective subject can hold a scientific myth like a Learning Pyramid, or the Hawthorne Effect, to be true, and I therefore reject these approaches. Addressing myths as aggregated individual beliefs is on the other hand possible, but comes with a cost: It makes a somewhat strained concept of group belief. Instead, I shall choose a Simpler approach, and address myths as distributed by academic publications. This avoids the above issues of group belief, and furthermore, and perhaps more importantly, it accentuates a

[12] Although several researchers presupposes that the(ir) Learning Pyramid is more or less common knowledge (being shared by other researchers, and recognised by these researchers as shared (Vanderschraaf & Sillari, 2014)), they can at best be said to have mutual belief.

[13] Thanks to Hedda Hassel Mørch for convincing me that there was more to the summative model than I originally recognised.

formal aspect of scientific knowledge that is downplayed when discussing the propositional attitudes of collective or individual subjects.

Among the non-human vehicles of scientific cognition identified in the above quote by Bird, I believe that the academic publications are of particular significance for the scientific collaboration. Academic publications are on the one hand external representations, working as a system for long-term information retention[14]. There is, however, also a markedly official aspect to the academic publication of research: Research published in academic channels implicitly receives a seal of approval that makes it part of the scientific corpus, formally recognised as a legitimate contribution to the debate by the gatekeepers: the editors, the reviewers and the publishers. When published in formats like academic books, reviewed journals, and proceedings, these works are formally accepted as research and they become available for an audience of experts.[15] It also becomes legitimate to cite them in other academic writings, and applications, more so than before being published, or if published in channels like magazines, blogs, or predatory journals.

Furthermore, what the research says on a particular matter is predominantly established through literature reviews and systematic reviews. Such reviews of scientific knowledge could conceivably describe a state of knowledge that few individual researchers have actually held until then, but still can be said to have been the consensus of the field for years. The scenario is not far-fetched: Parents were for a long time advised to let their infants sleep on their front, despite the documented increase in sudden infant death syndrome (R. Gilbert, Salanti, Harden, & See, 2005).

Referring to myths as claims formally recognised by academic publication-channels allows me to set aside the contentious ontological issue of a collective epistemic agent, and the nature of this collective belief. It is also consistent with the cacophony of scientific discourse: The scientific

[14] Speaking about works in the scientific corpus exclusively as an information repository, would, of course, be hermeneutically misleading

[15] I suspect text-books play a crucial role in the distribution of academic myths. Their academic status, however, is not clear. One the one hand, they are supposed to describe the general consensus in the field, on the other hand, they do not seem to enjoy the same authority as published research.

corpus contains statements and reasoning from all areas of knowledge, different theoretical levels, and types of data, that complement, corroborate, or contradict each other. This corpus extends beyond the jointly committed, coordinated, or aggregated beliefs of researchers. Once a claim is published, it is detached from whatever personal commitment the author had or has to it, being instead submitted to the discretion of the readership. The scientific corpus is the result of a collaborative effort that spans decades, and the literature of a specific field is presumably more extensive than any one researcher may be familiar with during her professional life, much less command.

Furthermore, addressing scientific myths in the form of published research is heuristically appropriate, simply because scientific myths are ex hypothesi detected and studied through their proliferation in academic publications, in accordance with their above noted formal standing. A focus on these publications will keep the definition close to the phenomenon I seek to investigate.

Scientific Myths as Insufficiently Warranted Statements

Myths are seemingly closely associated with falsehood (e.g. McGee, 1985). 'In today's parlance, myth is false. Myth is "mere" myth.' (Segal, 2015, p. 5). Likewise, the introductory remarks about Popper, and the use of myths in academic publications point in the same direction. Thus, disassociating 'scientific myth' from falsehood will presumably deviate from a conservative understanding of the term, and fail to meet the criterion of Similarity. Even though this understanding is by no means ubiquitous[16], it is the position I shall oppose, because a perceived Humpty-Dumpty approach could possibly stunt the general acceptance of the definition.

Making falsehood a sufficient criterion for scientific myths will extend the scope of the definition to the point where it subsumes every scientific idea ever falsified, including statements

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[16] In the popular TV-show 'MythBusters' presenters Hyneman and Savage used a scientific-like approach to assess myths, rumours and adages, and their conclusions included not only 'Busted', but also 'Plausible' and 'Confirmed', thus treating falsehood as an accidental feature of myths.

that were once both justifiable and reasonable at their time, e.g. Aristotelian physics (Kuhn, 2000), phlogiston (Allchin, 1992), and aether theory (Laudan, 1981). I shall therefore leave sufficiency, and instead focus the discussion on falsity as a necessary criterion: I argue that falsehood ought not to be held as a necessary criterion, if the definition is to be apt. I also try to demonstrate that disconnecting scientific myths from falsehood does not run counter to any unwavering semantic intuitions, i.e. the criterion of Similarity.

Necessary falsehood. There is a slight possibility that one or more of the four cases that I used to introduce scientific myths are true. I would not bet on the Sphex, or the Learning Pyramids, but there is a kernel of truth in the Hawthorne Effect, and possibly also in the Yerkes-Dodson Law.

While Wickström and Bendix (2000) argue that the original Hawthorne Studies did not show adequate evidence of the effect, they reckon it is more or less a truism: 'Today, it is generally accepted that all people reflect upon their situation and react to it when they consider this appropriate. There is no need to call this a special "effect".' (p. 366). Instead, 'researchers should introduce specific psychological and social variables that may have affected the outcome under study but were not monitored during the project, along with the possible effect on the observed results' (p. 363). There probably is such a thing as a Hawthorne Effect, but it is a general and trivial participant response, without consideration for context nor the particular mechanism addressed. To say that the effect is false, is therefore not strictly correct, and the Hawthorne Effect would have to be excluded from the list of scientific myths, as would several other unwarranted, vague half-truths in the same situation. This would make the concept very narrow, and thus less useful, presumably being applicable to only a few statements proliferating in academia. And, in the context of research, the indiscriminate distribution of falsified statements and insufficiently corroborated statements represents a problem for research in very similar ways, particularly so normatively.

As for any strong semantical associations between myths and falsity, I shall try to demonstrate that 'scientific myths' are not exclusively associated with conceptions contradicted by evidence, but may also be research-based, although inadequately. I shall make my argument with the help of a thought experiment, thus avoiding the messiness of ambiguous and debatable real-world cases:

Imagine some medical didactic researchers informally observing a phenomenon among their students when engaged in group work: The participants can all be classified as being a procrastinator, a proactive member, an obfuscator, a pedant, a freeloader or a front-runner. These roles come in pairs, and they are seemingly interdependent, in that a group without procrastinator has no proactive member, and vice versa, and the same goes for obfuscators and pedants, freeloaders and front-runners. The researchers tentatively propose 'the Socially Posited Antagonism Theory' (SPAT) based on these observations. Their published paper offers merely anecdotal evidence for its qualified conclusion. Nevertheless, the theory becomes widely cited in academic books, encyclopaedias and peer-reviewed journals, and SPAT becomes an established fact in the field of medical didactics. The effect is used to describe, explain, and facilitate group dynamics among students and professionals in medical schools and hospitals, and even doctor-patient relations.

The outcome of later inquiries into SPAT may either corroborate or contradict the theory, or turn out inconclusive. However, the results of these investigations are irrelevant when it comes to classifying SPAT as a myth. The extensive and uncritical academic reception and distribution are not. These models' claim to myth-infamy is that they have been propagated persistently by academic publications, all the while being inadequately corroborated. What makes SPAT a myth is its uncritical and overplayed distribution, when compared to its limited warrant.

It is conceivable that future ethological research will agree that the behaviour of the Sphex in fact is rigid to the point of irrational, or, that educational researchers will produce adequate empirical evidence that supports a hierarchy of retention and learning modalities similar to some version of the Learning Pyramids. The corroborators of these claims may triumph when supporting data are produced post publication. However, if the extent of the diffusion until then has exceeded the overall weight of the available evidence, they will have to concede that what they indeed have been spreading until that point has merely been a myth.

Diffusion. Both extensive and sustained diffusion are necessary conditions for a scientific myth. Unless widely distributed, and for some time, a conception does not qualify as a myth. A few research articles reiterate the popular myth that the Great Wall of China is visible from the moon (e.g. Fogg, 1993; Pheng, 2007). However, the extent of the academic diffusion of this claim appears to be limited, and it hardly qualifies as scientific myth in the above suggested meaning. Meanwhile, if several authors contribute to the academic distribution of an unsubstantiated statement, but only for a few years, this should presumably be referred to as an academic hype, or perhaps a fad, rather than a myth.

The Inflexible Sphex (Keijzer, 2013) has a more extensive academic distribution than the myth about the Great Wall of China, but considerably less than the other three introductory cases. As myth considered, the Sphex may amount to a borderline case. In accordance with the above discussion of the collective nature of scientific myths, extension and duration does not chiefly apply to the zealous propagation by a few researchers, even if authoritative, or otherwise high-profile (Keijzer names Daniel Dennett and Douglas Hoftstadter as significant propagators), but to the extent of the collective distribution. The same goes for extensive myth-distribution by particular journals.

The normativity of the scientific myth concept is based on the extent of this distribution: 'Scientific myth' reproaches not primarily the authors, the reviewers and the editors, for their role in introducing the myth to the academic community, but rather the scientific community for their responsibility for distributing unsupported statements, instead of directing appropriate critique against them. This way, they allow myths to become part of the scientific corpus, which they as professionals are entrusted to maintain. Daniel Jung, in a scathing review of a particularly sloppy, yet widely cited, IT management article, proficiently conveys the normative issue at hand:

> [T]his field of research has obviously a much bigger and more urgent problem than what could be addressed by the retraction of a single article, which is ultimately just a sad case of very poorly executed research. To the community: How could this go undetected for five years? Quite the contrary: how could it become a key reference in the field? How could even renowned professors and prominent scholars in the field cite claims from this article, re-cite both themselves and others, and relay references to an article that they cannot have read? Or even worse: not just read, but accepted as solid research? I have read them all, and there is not even the faintest expression of doubt as to the validity, reliability and integrity of this study in any of them, even in the "critical literature review" meta-articles. How could the erroneous author attributions (including misspellings) be taken at face value and be relayed again and again? (2019, p. 7)

Jung pin-points what I believe is the root of the value-ladenness and pejorative character of the term 'scientific myth' noted in the introduction. Scientific myths, it appears, are acquiesced and indiscriminately distributed in academic channels when they instead ought to have been scrutinised and criticised. Exclusively blaming the originator of a myth would be making him a Prügelknabe for the offences of the scientific community. It is the community that turns what is originally a singular misconception or a sloppy piece of research into an authoritative idea, by citing it, by distributing it, by getting behind it, by making it a part of their argument, and bestowing their professional authority on it, and by neglecting or underplaying its weaknesses.

The decade- (e.g. the Hawthorne Effect and the Inflexible Sphex), even century-long (e.g. the Learning Pyramids and the Yerkes-Dodson Law), propagation of scientific myths is incompatible with the idea of science as self-correcting. When research fails to meet this standard, it deviates from expectations of care and attentiveness, the postponement of judgment, the systematic and critical evaluation of sources, arguments, and evidence. This includes cases where these notions have gained a considerably higher level of formal acceptance than the available evidence would suggest, as well as in cases where conflicting evidence is ignored.

Despite my effort at stressing the critique of the collective that I believe is the central, as well as the most relevant and interesting aspect of the concept, it still leaves ample room for denigration of particular notions and publications, as well as their associated persons, for those who feel they need to do so. However, the burden of evidence required by using this concept is now heavier than by merely expressing strong disagreement in a heated debate.

Scientific Myths and Pseudoscience

There is a certain kinship between the concepts scientific myth and pseudoscience. Both are used derogatorily about statements that are presented as – or at least equivalent to – scientific, yet fail when measured against science. These, and adjacent terms are also sometimes conflated, or seen as aspects of the same phenomenon, e.g. pseudohistory as consisting of scientific myths (Allchin, 2004). To fulfil the criterion of Exactness, it seems prerogative to demonstrate that the suggested definition can be adequately delimited from pseudoscience.

In order to demarcate these two conceptions, I have opted for Sven Ove Hansson's definition of pseudoscience: His focus on statements and warrant corresponds to my above approach to scientific myths, and this should presumably facilitate a comparison of the two. Hansson postulates science (in a wide sense) as authoritative, allowing him to formulate a definition of pseudoscientific statements as unreliable doctrinal statements about issues belonging to the domain of scientific disciplines (Hansson, 2009, 2013). This departs from the more common approach of starting out by describing that which constitutes the scientific process and knowledge, e.g. falsifiability, puzzle solving ability or progress, and afterwards demarcating pseudoscience by showing how it fails to uphold these criteria (2009, 2013, 2017).

However, before I could demarcate pseudoscience and scientific myths, I needed to modify Hansson's definition slightly, because in its original form it did not actually define the term 'pseudoscience', only 'pseudoscientific statement', leaving the nature of these disciplines opaque (for both theoretical and practical purposes):

> A statement is pseudoscientific if and only if it satisfies the following three criteria:
> 1. It pertains to an issue within the domains of science in the broad sense (the criterion of scientific domain).
> 2. It suffers from such a severe lack of reliability that it cannot at all be trusted (the criterion of unreliability).
> 3. It is part of a doctrine whose major proponents try to create the impression that it represents the most reliable knowledge on its subject matter (the criterion of deviant doctrine). (Hansson, 2013, pp. 70–71)

Furthermore, and rather unfortunately, his definition subsumes bad science in the form of scientific myths, effectively making my demarcation between the concepts a non-starter: Myths like the Learning Pyramids and the Inflexible Sphex pertain to issues within educational psychology and ethology. They are both unreliable, being at odds with the current knowledge status, and they are doctrinal by their persistent proliferation. Therefore, I have suggested the following alterations to the definiendum and the third criterion of Hansson's definition, that define the definiendum 'pseudoscience', and allow me to propose a demarcation between pseudoscience and scientific myths:

> A pseudoscience is an epistemic discipline that contains a sufficient number of statements that satisfy all of the following three criteria:

1. It pertains to an issue within the domains of science in the broad sense (the criterion of scientific domain).

2. It suffers from such a severe lack of reliability that it cannot at all be trusted (the criterion of unreliability).

3. It is a core statement of its discipline (the criterion of significance). (Letrud, 2019a)

By replacing Hansson's third criterion, of deviant doctrine, with a criterion of disciplinary significance, I have tried to make these statements indicative of the pseudoscientific status of their respective discipline. I propose that the pseudosciences can be recognised by their unreliable core statements: statements that are fundamental to the discipline, and major part of its identity (e.g. water memory in homeopathy): Pseudosciences are epistemic disciplines based on premises that are in conflict with or unsubstantiated by those disciplines that are considered authoritative on the matter.

To exemplify the definition for the reader, I shall have a crack at the case of chiropractic. Chiropractic is an alternative form of medicine where a chiropractor manipulates joints in order to relieve pains in the muscles and the joints. The British National Health Service[17] is currently offering these treatments in some areas (NHS, 2017), although not without contention: The British Chiropractic Association somewhat infamously (Coghlan, 2010) stated that manipulation of the spine helps children with, among other things, asthma, colic, and frequent ear-infections (British Chiropractic Association, 2003). While each of these statements about the curative effects from various spine-manipulation techniques can, and ought to, be criticised, doubts concerning the reliability of chiropractic as a discipline would presumably be raised as an accumulated afterthought to these critiques, not by the particular critiques of the individual

[17] The British NHS is not unique in this respect. In Norway, The Norwegian Directorate of Health authorises chiropractors, and the health care system reimburses the patients for parts of the consultation and therapy fees. Norwegian chiropractors supposedly treat several ailments including asthma, bedwetting and baby colic. ("Kiropraktor – behandling og betaling [Chiropractor - treatment and payment]," 2018)

statements. In comparison, consider the following statements made by the International Chiropractors Association:

(1) A vertebral subluxation is the result of spinal bones with improper motion or position affecting nerve communications between your brain and your body.
(2) Distorted nerve communications can be an underlying cause of many health problems beyond just headaches and back pain. (Reggars)

The warrant of these core statements makes the backbone for chiropractic as a discipline. The existence of vertebral subluxation, however, appears to be entirely unsubstantiated (Homola, 2016). Still, vertebral subluxation and its effects are widely accepted in the field of chiropractic: 'Subluxation/joint dysfunction … is deeply ingrained in the psyche, education, and clinical practices of many within the profession, and its continued relevance is undeniable.' (Good, 2016, p. 31) (see also Gliedt et al., 2015).

Chiropractors have in the later years debated the future standing of vertebral subluxation in the profession (e.g. Reggars, 2011; The European-South African Education Collaboration). This debate also addresses the identity and direction of the field, as well as its legitimacy and scientific status (e.g. Keating Jr et al., 2005; Reggars, 2011). The following excerpt from this debate illustrates and parallels to some extent the above deliberations concerning core statements:

> The dogma of subluxation is perhaps the greatest single barrier to professional development for chiropractors. It skews the practice of the art in directions that bring ridicule from the scientific community and uncertainty among the public. Failure to challenge subluxation dogma perpetuates a marketing tradition that inevitably prompts charges of quackery. Subluxation dogma leads to legal and political strategies that may

amount to a house of cards and warp the profession's sense of self and of mission. (Keating Jr et al., 2005, p. 8)

If the internal debate leads the chiropractor community to reject subluxation, they will also reject chiropractic in its present form, thereby establishing a new, less speculative, chiropractic discipline, that avoids being labelled as a pseudoscience (presumably remaining criticisable in several other respects).

The distinguishing feature that allows a demarcation of scientific myths from pseudoscience, lies not so much in the statement itself, but rather in the role these statements play in their respective disciplines. When unwarranted statements are widely distributed within a discipline (or disciplines), they are myths. However, when they are made the base for an entire discipline, the result is a pseudoscience.

The growth of Scientific Myths

The introductory scientific myths cases, and the suggested definition, inevitably raise the question of how such misconceptions persevere and proliferate in academic publications, thus growing to scientific myths. Although the question is largely practically motivated, this thesis does not aspire to prescribe solutions to the practical problem of deterring scientific myth dissemination, but rather to help prepare such efforts by outlining an aetiology for scientific myth distribution. Also, using the definition successfully as basis for explanations, will also demonstrate that it is Fruitful, at least in this respect.

I approach this problem in two ways. I perform a case study of the academic practices that propagate the myths, and (together with Hernes) a case study of the academic reception of myth-debunking efforts. Both studies focus on reviewed academic publications, in accordance with the definition.

The first case study sheds light on the research practises that lead to the propagation of myths, within a normative framework. The paper argues that poor citation practices alone is inadequate to explain the spreading of scientific myths. Instead, I seek to map out a more extensive set of plausible explanations of this phenomenon, and argue that the proliferation of scientific myths is overall the result of epistemic practices that fall under inadequate Organised Scepticism.

The second case study focus on the academic reception of debunking publications. Studying the citations of three articles arguing against the flawed but oft-published Hawthorne Effect, we find that the majority of publications that cite these critical works are affirming the theory.

The Propagation of Scientific Myths

In 'The propagation of myths: A case study' (2019b) I argue that the issue of myth propagation in academic publications is inherently normative: In the context of research, one needs to ask 'how did these researchers fail?'. This is motivated by the higher standards for research than for other epistemological areas, and that researchers are expected to implement criticism, attention, care, and precision into their works.

Examining a particular episode of Learning Pyramid distribution, I find evidence of secondary (and tertiary) citations, and citation plagiarism. However, these explanations only partly explain the propagation. Based on the character of the various instances of myth distribution, I argue that there are more fundamental and epistemological issues at hand: I theorise that the distributors of the Learning Pyramid have either not read the paper they cite, merely skimmed it, or they have read it but failed to understand it. Alternatively, they have refrained from assessing the Learning Pyramid, simply because it is so widely distributed, and uncritically accepted it as the reigning consensus. I also suggest that the propagation can be explained by the authors disconnecting their belief in the correctness of the model from the available evidence[18], and that their citation does not necessarily reflect any confidence in the cited work as support for their belief. Citing a source in support of the validity of the Learning Pyramid thus becomes hand-waving, a matter of formality rather than of justification.

In the paper, I hesitated to claim that myth propagation episodes may have been deliberate, although I suspect some may have indeed taken a short-cut, knowingly misrepresenting the argumentative weight of the citation for their Learning Pyramid. However, it is generally speaking a plausible explanation for some instances of scientific myth propagations, particularly when considering myth-distribution where incentives for propagation are strong: The infamous five sentence-letter in the New England Journal of Medicine (Leung, Macdonald, Stanbrook, Dhalla, & Juurlink, 2017; Porter & Hershel, 1980), where addiction from a limited and monitored distribution of opioids to in-patients was deemed low, was widely cited for opioid-based chronic-pain relief outside hospitals (Zhang, 2017). It does not seem far-fetched to assume that economical motives contributed to the distribution of this flawed information in political and medical contexts.[19]

The Subduction of Scientific Myth Debunking

The propagation of scientific myths only partly explains how they proliferate for decades. Considering the censorious character of academic debates, one would expect that these myths were actively and successfully denounced. However, observations Hernes and I did during our studies of the Learning Pyramids, suggested that debunking efforts were not as efficient as one would hope: Several authors would cite a myth-critique, but summarily reject it, or even cite the debunking article as affirming the myth.

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[18] Which goes to show that beliefs do have a role to play in studies of scientific myth.

[19] Motives for propagating this myth in research publications could possibly also include a desire to raise the standing and influence of pain relief research and practice: 'The APS [American Pain Society] became an advocacy group for opioid treatment of pain and eventually received most of its support from Purdue Pharma (Stamford, Conn), maker of the opioids MS Contin® and OxyContin®. In 1995, the APS proposed that pain be measured as the "5th Vital Sign" and trademarked the term.' (deShazo, Johnson, Eriator, & Rodenmeyer, 2018, p. 596)

We theorised that there were three main ways of citing debunking efforts (apart from remaining neutral): (i) agree with the criticism, and refrain from using the theory in one's own works, (ii) disagree with the debunking arguments, and justify the continued use of the theory, and (iii) cite it as an affirming source for the theory (Letrud & Hernes, 2019). The theorising about myth-propagation in the previous section (Letrud, 2019b), probably applies to (iii) as well.

A conservative, slight pessimistic, estimate could be that these three groups were roughly the same size, meaning that the sum of the myth-affirmative citations (ii+iii) would outnumber the negative citations (i) by 2 to 1. Such a ratio would of course be inconvenient for scientific myth debunkers, but hardly dramatic. However, from our observations we had reason to suspect that (iii) the group that cited erroneously, was disproportionately large. And, there was also a clear incentive that would reduce the size of (i) the group reiterating the debunking: If they accepted the arguments, there was no reason for them to incorporate the theory and the critique in their own works, spending time and pages on something that in the context of their paper was irrelevant.

Exploring citations of three articles critical of the Hawthorne Effect[20], we did indeed find that the reception was skewed: of 613 articles 468 affirmed the Hawthorne Effect, while only 40 rejected it (the remaining 105 were neutral); a ratio of near 12 to one. We termed this citing mechanism 'Affirmative Citation Bias' (ACB).[21]

Although not voicing our suspicion in the paper, we have a hunch that ACB increases over the years due to the accumulating effects of practices described in the above section, meaning the citations over time will become even more skewed. There are arguably indications of

[20] If the reader remains unconvinced about the categorisation of the Hawthorne Effect as a scientific myth, it ought to be noted that the general findings of the ACB study does not depend on the mythical status of the Hawthorne Effect as a premise. If anything, this would suggest that the mechanism also works on critique of controversial ideas.
[21] 209 of the 613 articles did *not* cite the critical articles as being affirmative. This would suggest that as many as 34% may have consulted the articles they cited, which is higher than Simkin and Roychowdhury's more pessimistic estimate of 20% (2006).

such pattern in the reception of Franke and Kaul (1978), Jones (1992), and perhaps Wickström and Bendix (2000).

Teigen, noting a similar tendency of authors citing his (1994) critique of the Yerkes-Dodson Law as corroborating the Yerkes-Dodson law, writes:

> I have high hopes that the article will have reached half a thousand citations in ten years' time, slipstreaming Yerkes and Dodson's posthumous triumph. At the same time, the downward line [representing the number of correct citations in the figure] suggests that it by then will be misquoted, consistently, by everyone. (Teigen, 2017. My translation.)

We believe ACB can contribute to the current debate on science's ability to self-correct (Ioannidis, 2005, 2012). There are, however, several questions that will need to be answered:

How common is ACB? The Hawthorne Effect is not only a theory of research-subject behaviour, but is also part of research methodology, being frequently cited in method sections and method text-books. Theoretically, this dual character gives the Hawthorne Effect a particular resistance against debunking (we suspect that the use of the model is often motivated by methodological prudence). However, the reception of Teigen's critique of the Yerkes-Dodson Law (1994) indicate that a similar distribution of citations can be found for other myth-debunking efforts.

How closely connected are ACB and scientific myths? We suspect that theories, once they reach the status of scientific myths, becomes harder to eradicate. If the flawed statement is well known and widespread, there are presumably many authors believing it to represent the consensus, and therefore cut corners by not reading the critique, and instead cite myth-debunking papers as being myth-affirming. It may turn out, however, that this bias also influences discourses on contentious issues other than scientific myths.

Does ACB primarily affect interdisciplinary cited theories, like the Hawthorne Effect and the Yerkes-Dodson law? While they originated within sociology and psychology respectively, they are frequently cited in several other scientific disciplines. The Inflexible Sphex is an ethological theory, but is propagated by cognitivist scientists, while the Hawthorne Effect is cited in nearly every conceivable area of health science, despite originating in Administrative Studies. Researchers are presumably more competent and confident when evaluating research from their own field. Trusting the authority, competence and consensus of other fields of research could make them impervious to solitary critiques of the legitimacy of widespread ideas.

A Pluralistic Argument for Scientific Myths?

Are scientific myths solely detrimental to the academic debate? In this section, I discuss whether Paul Feyerabend's principle of theoretical pluralism can justify the propagation of scientific myths as a way of counteracting dogmatism. I conclude that, although offering alternatives to the reigning consensus, scientific myths inescapably become dogmas themselves due to their widespread and indiscriminate distribution.

In several of his works, Feyerabend advocates scientific proliferation: 'Invent, and elaborate theories which are inconsistent with the accepted point of view, even if the latter should happen to be highly confirmed and generally accepted' (1981c, p. 105). Following closely John Stuart Mill's defence of free speech in 'On Liberty', Feyerabend (1981a) argue that (i) contested theories still may turn out to be true, and rejecting them would be to assume one's own infallibility. The resurrection of atomism and the moving earth theory of antiquity serves to exemplify this. Furthermore, (ii) theories contain parts of the truth, to different degrees. It is possible to discover further truths by colliding these theories.[22] Also, (iii) adhering to the uncontested consensus will inevitably become a matter of dogmatic response, with no contrasting

[22] What this amounts to, is not clear, but it is possibly, and at least partially, a matter of letting theories compete, in order to force them into greater articulation (Feyerabend, 1993, p. 21).

perspectives to highlight its rationale or (iv) to grow a personal understanding or conviction about its significance (Feyerabend, 1981a, p. 139; Mill, 1947 [1859], pp. 52-53).

More specifically, alternative theories can make sense of facts that are otherwise rejected as deviational noise. In some cases, a discrepancy between fact and theory cannot be demonstrated unless a competing theory can produce meaningful evidence from this noise (Feyerabend holds that there are no theory-independent descriptions of observations (e.g. 1993, p. 27)). According to Feyerabend, it was not until the Kinetic Theory was confirmed that the well-known, but not well understood, Brownian motions could be used to corroborate Statistical Thermodynamics at the cost of the Second Law of Phenomenological Thermodynamics.

To give these alternative theories a chance to prove their worth, they need to be protected from severe testing. To do this, researchers ought to embrace the principle of tenacity[23]: 'stick to this one theory even if the actual difficulties it encounters are considerable.' (1970, p. 203). And, even more pointed: 'Having adopted tenacity we can no longer use recalcitrant facts for removing a theory, *T,* even if the facts should happen to be as plain and straight-forward as daylight itself.' (1970, p. 205).

Regarding scientific myths, consider once more the Learning Pyramids: These are not only claims about the comparative efficiency of perception and learning modalities. They compare categories in a way that neither educational nor psychological research would consider obvious (e.g. perception modalities vs learning strategies). They also represent alternatives to the standard psychological theories about memory, e.g. making perception modality a major variable for depositing information in the long term memory (Letrud & Hernes, 2018). Because they were never thoroughly expelled from educational research, the original list of perception modalities from the 1800s had time to evolve into a family of more complex theories, and adapt to the emerging context of educational media by adopting relevant categories. Thus, the modern

[23] 'Principle', Feyerabend stresses, is intended as an mnemonic (1970, p. 205), rather than a strict rule, I surmise.

Learning Pyramids are not only far more multifarious than they originally were, they also make a wide range of empirical claims about learning that (although not easily) can be tested. If a non-trivial part of a learning pyramid version turns out to be true, it will definitely represent a major contribution to education studies and learning psychology.

There is a general case to be made about pluralism and the distribution of controversial yet inadequately substantiated theories. I cannot see, however, that scientific myths ought to be counted among these. A smaller distribution of an unwarranted alternative theory could be considered a sign of a healthy and living debate. But, when extensively distributed, like the Learning Pyramids, the Hawthorne Effect and the Yerkes-Dodson Law, it approaches a status of dogma that conflicts with the explicit intention of theoretical pluralism.

Indeed, there is an air of dogma with which myths like the Learning Pyramids are distributed: of the hundreds of peer reviewed articles we found propagating versions of the Learning Pyramids, the majority is stating it as a fact without discussion or even a citation (Letrud & Hernes, 2016). Any expressed concerns are summarily dismissed. The same goes for reiterations of the Hawthorne effect. Feyerabend notes that indiscriminatingly applying the principle of theoretical pluralism 'open[s] the door to all sorts of wild speculations' and 'crazy ideas' (1981b, p. 196).[24] The difference between the 'crank' and the 'respectable thinker'[25], does not lie in the available evidence for the theory, its perceived plausibility, or expectations of its future success: 'It is this further investigation, the details of it, the knowledge of the difficulties, of the general state of knowledge, the recognition of objections…' (1981b, p. 199). The crank is content in his point of view, and does not elaborate, develop, or test his theory, or even recognise difficulties inherent in his position.

[24] For real-life examples of unmoderated theoretical pluralism, see viXra.org, an electronic e-print repository for papers that are unsubmittable to arXiv.org.

[25] I have not found these perspectives in his later writings, and I suspect he found them resembling too much the scientific arrogance he criticised.

Conclusion

In this thesis, I have sought to introduce the issue of scientific myths, arguing that a scientific myth concept potentially can contribute to both theoretical and practical deliberations. By exploring the Learning Pyramids, Hernes and I have developed a particularly informative case of scientific myths to serve as a paradigm for the discussions. Based on this case, and three others, I have suggested a definition of 'scientific myth', arguing that 'scientific myth' in the context of research ought to be approached in the form of academic publications rather than beliefs, and to denote a lack of warrant rather than a lack of veracity. 'Scientific myths', I argue, ought to mean statements that are insufficiently warranted when compared to their level of academic distribution. Due to their relatively limited diffusion and the oft inconsistent statements subsumed by these scientific myths, they are not easily described as group beliefs. Approaching these myths in the form of statements published in formal academic publications makes the definition simpler, and accentuates an important formal aspect of scientific knowledge.

In order to further elucidate the concept, I have contrasted scientific myths with pseudoscience (thereby also demonstrating how a scientific myth concept can contribute in a philosophical debate on pseudoscience). I have argued that the pseudosciences, e.g. homeopathy and chiropractic, are recognisable by the unsubstantiated or falsified statements forming the core of these disciplines. A scientific myth, in comparison, does not play such a significant disciplinary role.

And, I have presented two plausible explanations of scientific myth distribution, inadequate Organised Scepticism, and Affirmative Citation Bias: Due to the critical nature of scientific discourse, the 'how' question of scientific myth spreading is not adequately addressed by an exclusively descriptive explanans. A normative approach is needed. Exploring a case of Learning Pyramid propagation, I try to show that an exclusive focus on citation errors are inadequate, and that an explanation requires that also the more fundamental epistemic practices are addressed. And, we believe we have identified a citation mechanism that subdues myth-criticism, not only making them inefficient, but possibly also counterproductive, by contributing to the continued distribution of scientific myths in academic publications.

This thesis has hopefully succeeded in establishing scientific myths as a topic of research, and provided some worthwhile contributions, conceptually as well as explanatory. However, there are several issues in relation to scientific myths that I have not addressed:

I have suggested that the scientific myth susceptibility is higher for the social sciences for distribution than for the natural sciences. I offer only a few, and rather unspecific, guidelines for assessing the scientific myth status of specific statements. I have asked some questions about ACB that will need to be answered to develop a more substantial understanding of this phenomenon. And, while the motivation for addressing the propagation and unsuccessful debunking of scientific myths is to reduce or hinder myth distribution, I have not made suggestion as to how to do this. All these issues will have to be the subject of later research.

Appendix A

There are at least two conceptions of 'scientific myth' that, although irrelevant to this specific inquiry, are so common that they ought to be described and differentiated from the scientific myth concept discussed. The first is the plethora of myths or legends about historical scientific figures, discoveries and experiments that represent somewhat idealised conceptions about the scientific method, the institutions of science, or major scientists. The second are the popular yet false titbits distributed by non-academic channels.

Archimedes experienced a stroke of genius while having a bath, and ran naked through the streets of Syracuse shouting 'Heureka! Heureka!'. He was later killed by an impatient roman soldier; Archimedes was so occupied with geometry that he failed to recognise the danger of the situation. These stories of the immersed genius unaware of his social and practical surroundings were reported years after they supposedly happened, and they are most likely apocryphal (Berrey, 2010).[26]

Myths about science and scientists are not merely tall tales. They can also stem from historical events, but become magnified and dramatised into idealised narratives. Douglas Allchin, exploring myths about science and the rhetoric about its history, institutions, and major profiles, describes them as 'popular histories of science that romanticise scientists, inflate the drama of their discoveries, and cast scientists and the process of science in monumental proportion.' (2003, p. 329). These stories of discovery and justification are larger-than-life narratives. They are legendary.

Within this normative category, I also include myths about science and the scientific process that not necessarily are narratives, but still idealisations, e.g. scientific evidence is scientific proof, scientists are particularly objective (McComas, 1998).

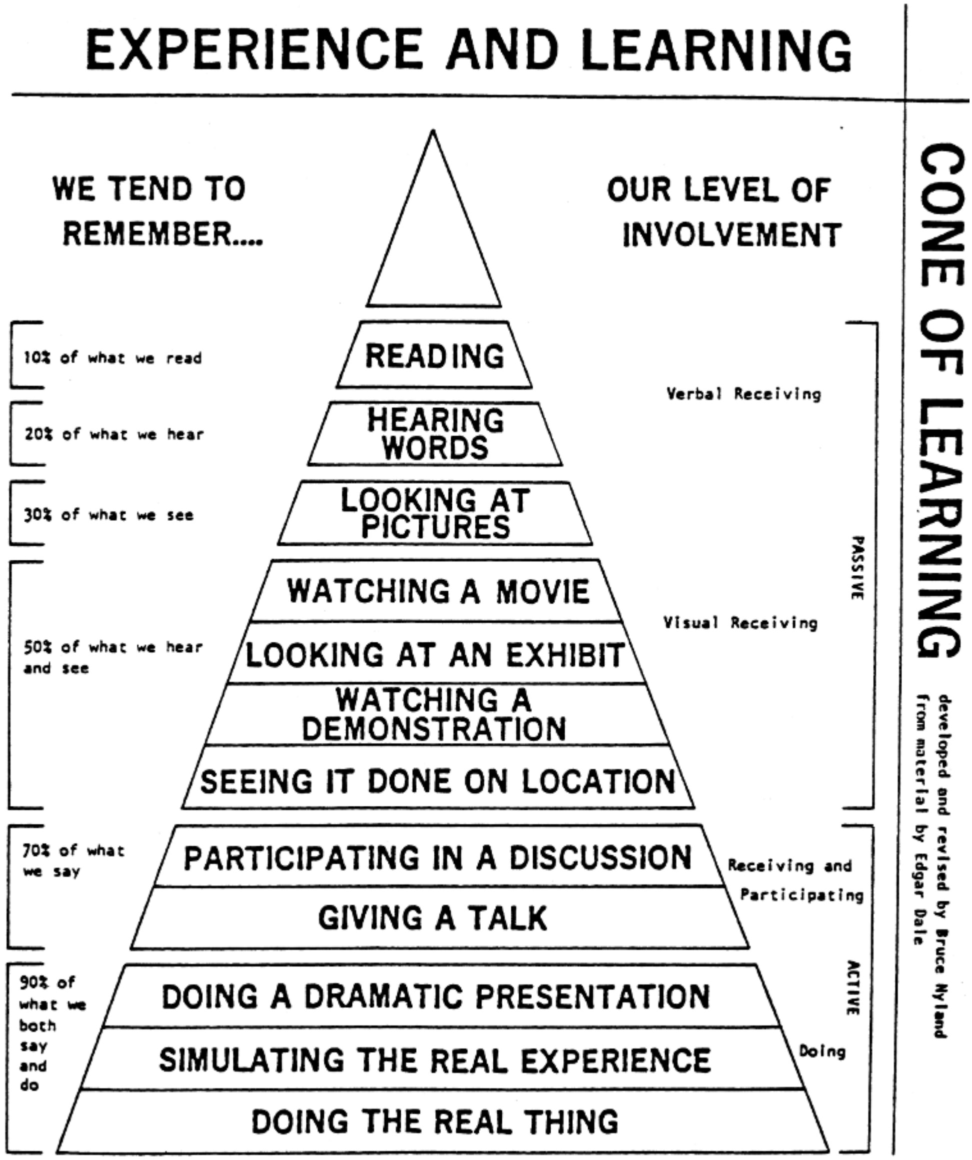
[26] The trope seems to predate Archimedes. For instance, in Theatetus (174A) Plato retells the story of how Thales fell into a well because he was too occupied observing the heavens to see where he was going.

Another common scientific myth-conception is popular but demonstrably false folklore: According to popular beliefs, Christopher Columbus demonstrated that the earth was spherical by sailing west to the Americas; If food falls on the floor, it will take at least five seconds before it becomes contaminated, and is safe to eat if picked up within that period; Goldfish have only three seconds memory; Earthworms, when severed, become two individual earthworms. Pigeons explode if they eat uncooked rice.

One could argue that the designation 'scientific' is superfluous in the above examples – that these are myths, plain and simple, primarily spread outside of academia. However, they all have an air of science about them: The 'five-second rule' is by its subject matter a microbiological idea. Christopher Columbus' circumnavigation is one of history. That said, nearly every field of descriptive knowledge has a corresponding area of research. Regardless of what misconception we might hold, it necessarily has to be about some historical, biological, geological, physical, chemical, social, cultural, cosmological or psychological fact. Furthermore, these myths all have a hint of systematic observations, and experiments, even if they mention no research. And, they all make quite interesting claims about the world that extend beyond our field of sense perception and everyday experience, much like your garden-variety popular-science report.

Among these myths, we can count Bangerter and Heath's 'scientific legend': 'a widespread belief … that propagates in society, originally arising from scientific study, but has transformed to deviate in essential ways from the understanding of scientists' (2004, p. 608). The concept arose from a case study of the so-called 'Mozart Effect', which originally was a study of the effect from listening to music composed by Mozart on college students' spatial intelligence. This study quickly became a widely cited claim about infants and general development of intelligence through its reception in the media and popular culture.

Appendix B


EXPERIENCE AND LEARNING
WE TEND TO REMEMBER....
OUR LEVEL OF INVOLVEMENT
10% of what we read
READING
20% of what we hear
HEARING WORDS
30% of what we see
LOOKING AT PICTURES
Verbal Receiving
50% of what we hear and see
WATCHING A MOVIE
LOOKING AT AN EXHIBIT
WATCHING A DEMONSTRATION
SEEING IT DONE ON LOCATION
Visual Receiving
PASSIVE
70% of what we say
PARTICIPATING IN A DISCUSSION
GIVING A TALK
Receiving and Participating
90% of what we both say and do
DOING A DRAMATIC PRESENTATION
SIMULATING THE REAL EXPERIENCE
DOING THE REAL THING
Doing
ACTIVE
CONE OF LEARNING
developed and revised by Bruce Nyland from material by Edgar Dale

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Articles I-V

II

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TEACHER EDUCATION & DEVELOPMENT | RESEARCH ARTICLE

Excavating the origins of the learning pyramid myths

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Abstract: The family of cognitive models sometimes referred to as the "Learning Pyramid" enjoys a considerable level of authority within several areas of educational studies, despite that nobody knows how they originated or whether they were supported by any empirical evidence. This article investigates the early history of these models. Through comprehensive searches in digital libraries, we have found that versions of the Learning Pyramids have been part of educational debates and practices for more than 160 years. These findings demonstrate that the models did not originate from empirical research. We also argue that the contemporary Learning Pyramids, despite their continued modifications and modernizations, have failed to keep up with the developments of cognitive psychology. The conception of memory implied by the Learning Pyramids deviates significantly from the standard picture of human memory.

Subjects: Learning; Educational Research; Education Studies; History of Education; Philosophy of Education; Theories ofLearning; Teachers & Teacher Education; Theory of Education; Teaching & Learning; Educational Psychology

Keywords: learning pyramid; cone of experience; cone of learning; scientific myth; neuro-myth; neuroscience; learning modalities

1. Introduction

Uncorroborated and even refuted claims about educational psychology and educational neuroscience appear repeatedly in educational studies, practices and debates. It is not uncommon among educators to believe that we use only 10 per cent of the brain, and have different learning styles (for these and other learning myths see Geake, 2008; Goswami, 2006; Howard-Jones, 2014; Kirschner & Merriënboer, 2013; Rato, Abreu, & Castro-Caldas, 2013). Some of these myths even reach academic status (Kirschner, 2017). This article addresses a similar myth of learning

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PUBLIC INTEREST STATEMENT

Some ideas about learning and teaching widely cited by educators and educational researchers are either unsubstantiated, or even proven to be plain wrong. We search for the origin of one of these conceptions, the Learning Pyramid, and demonstrate that this is one particularly tenacious myth: It is more than 160 years old. We can definitively conclude that the Learning Pyramid did not originate from research, because the field of learning psychology is at least 20 years younger than the model. By contributing to the debunking of the Learning Pyramid, the paper seeks to limit the academic diffusion of this myth.

psychology that has circulated widely among educators as well as educational researchers: a family of models that ranks the retention effects from various presentation and perception modalities. There are several versions of this notion, and those that go by the name of "Learning Pyramid" are probably among the best known. We shall use this as a blanket term for all these different models.

We shall present the findings from a search for the original source of these models, and demonstrate that primitive versions were published in the early 1850s. We shall also argue that it is unlikely that the Learning Pyramids originated from empirical studies, because they predate by decades the entire field of experimental retention studies. Furthermore, we shall argue that the current Learning Pyramids are ill suited for research as well as for educational practices. The present-day versions of the Learning Pyramids feature more or less the same folk-psychological conceptions of learning and memory as they did in the 1800s. Consequently, they contradict current consensus in psychology concerning the structure and function of memory, and they fail to adopt essential insights into the principles of learning developed by memory studies.

According to the Learning Pyramid models, one supposedly remembers very little from hearing or attending lectures. Reading is near equally inefficient, whereas seeing something, for instance, a film or a demonstration, results in a higher degree of retention. Furthermore, talking and participating in discussions, having direct experiences, practising, and teaching others are extremely efficient. The models often neatly quantify the effects on retention of these ways of learning in increments of five or 10 per cent, for instance 10%, 20%, 30%, 50%, 75%, or 90%. Apart from such general similarities, there is neither a consensus about the number and nature of the modalities nor their levels of efficiency, as exemplified by the following three recent quotations:

> ... research suggests that that [*sic*] learning does not occur in isolation but by teams working together to solve problems ... and that on average, students retain 10% of what they read and 30% of what they see; whereas students retain 50% of group interaction and 90% of what they act on ... (Rogers, 2011, p. 609)

> According to Hansen ... students retain 25% of what they listen to, 45% of what they listen to and see, and 70% when they manipulate, control and modify experiments, putting into practice what they are learning. (Bravo, van Joolingen, & de Jong, 2006, p. 769)

> The benefits of more holistic pedagogical approaches are demonstrated in social science teaching in the finding that "students retain 10% of what they read, 20% of what they hear, 30% of what they see, 50% of what they see and hear, 70% of what they say, and 90% of what they do and say together"... (Johnson, 2016, p. 319)

The models' lack of uniformity impedes efforts at criticizing them as a unitary phenomenon. Indeed, owing to the extensive variations between these models, their affinities and common ancestry were exposed only some years ago (Januszewski & Betrus, 2002; Molenda, 2004; Subramony, 2003). Searches for the alleged studies behind the Learning Pyramids have turned out empty-handed (e.g. Holbert, 2009; Lalley & Miller, 2007; Molenda, 2004; Subramony, 2003; Thalheimer, 2006). Lalley and Miller's (2007) search revealed no studies supporting some of the more common varieties of the Learning Pyramids' ranking of learning efficiency:

> The research reviewed here demonstrates that use of each of the methods identified by the pyramid resulted in retention, with none being consistently superior to the others and all being effective in certain contexts. (Lalley & Miller, 2007, p. 76)

Nevertheless, the models have accrued a veneer of authority within medical and engineering educational research, however unwarranted (Holbert, 2009; Masters, 2013). Furthermore, we recently demonstrated that versions of these retention models have been propagated within several other areas of subject-didactic research and educational technology, in at least 418 peer-reviewed articles, as well as in 11 encyclopaedia articles, all published between 1990 and 2013. In comparison, merely 15 articles (including one encyclopaedic article) questioned or criticized the validity of some version of the learning pyramid (Letrud & Hernes, 2016). The distribution of these conceptions by academic journals is problematic in an epistemological perspective. However, the repeated academic publication of the Learning Pyramids might have wider consequences. When peer-reviewed journals and encyclopaedias uphold these models, they imbue them with a scientific legitimacy and authority that transcends academic educational debates. This might augment the current spreading and recognition of these models in professional, public, and political deliberations within such diverse areas of education as curriculum, didactics, and even school architecture, for example (Letrud & Hernes, 2016).

More rigorous knowledge about the early history of the Learning Pyramids shall hopefully contribute to the effort of busting and vitiating these myths, and impede their widespread dispersion and acceptance. There are several gaps in the early history of these models. We know neither how and when they originated nor who produced them. All the same, the lack of information about any original research has not deterred the spreading of these ideas. We are aware that shouldering the burden of proof for the nonexistence of research corroborating the Learning Pyramids is a precarious project. One might argue that the inability to secure proof for its nonexistence may be read as evidence for the existence of this research, despite the evident fallaciousness of the inference. We must stress, then, that those who distribute these models have the responsibility of supplying adequate evidence.

2. Method

In 1967, D.G. Treichler published a version of the Learning Pyramid models in the magazine *Film and audio-visual communication* that went on to become an oft-cited source. Treichler, an affiliate with Mobil Oil Company (later ExxonMobil), claimed that “people generally remember”:

> 10% of what they read
> 20% of what they hear
> 30% of what they see
> 50% of what they hear & see
> 70% of what they say
> 90% of what they say as they do a thing (Treichler, 1967, p. 15)

Treichler asserted that these numbers came from studies, but he did not say where they could be found. There have been some attempts at unveiling the source of the model and its original form. Dr Michael Molenda traced the origin of the model to a Paul John Phillips at the University of Texas, who asserted that these numbers were supported by research performed at the US Army’s Ordnance School in Aberdeen, Maryland, in the early 1940s. However, searches for documentation supporting Phillips’ claim came up empty handed (Molenda, 2004). Dr James E. Stice (2009) questioned this report, and argued that the model originated sometime between 1934 and 1955. Stice also noted that it seemed more closely associated with ExxonMobil. The basis for this claim is a handout he received at a workshop in the early 1970s, featuring a near identical model marked “Socony-Vacuum Oil Company”, which was in fact the name used by ExxonMobil in the period 1934–1955. It was renamed “Socony Mobil Oil” in 1955 and “Mobil Oil Company” in 1966, which is the company Treichler was affiliated with in 1967 (Stice, 2009). Quite recently, Subramony, Molenda, Betrus, and Thalheimer (2014) reported findings of claims resembling those made by Treichler, published in 1913, 1914, 1920, and 1922. This documented that the models did exist in various forms in the early 1900s. However, the information shed no light on the models’ origin, nor did it point in the direction of any corroborating research. The findings revealed no association with the US Army or the oil industry, and they offered no clues regarding the age or origins of the data.

Ideally, the tracking down of the origins of a scientific idea involves following references that directly leads to the original publication, or indirectly, where each reference constitutes a stepping-stone. Unfortunately, the lack of known references to earlier sources hinders any further attempts at tracking the origins of the Learning Pyramids. However, the last decade or so has seen the digitalization of thousands of books that are normally only accessible at a few libraries or archives. This has made it possible to throw a wider net in the search for the origins of the retention chart, and it has proved to be more efficient than a laborious and time-consuming search for references.

We searched primarily Google Books (books.google.com) and HathiTrust (hathitrust.org). We limited the search period up to 31 December 1940, and produced a large number of search-strings from Treichler's chart by inflecting the personal pronouns' number and gender, conjugating verbs, and by adding different types of scales, including different standards of spelling, like "per cent" and "percent" (Table 1). The most efficient search-strings turned out to be variations of the phrase *"of what we hear" AND "of what we see"*.

Because there is a plethora of general statements concerning retention, identifying findings as being early versions of the Learning Pyramids presented some problems. It was important to differentiate between occurrences of the model, on the one hand, and similar but unrelated claims, on the other. At the same time, it was also vital to avoid rigidity in order to allow the recognition of new variants of the model. The Learning Pyramids are constantly changing, and we could only assume that this would also be the case with the early instances of the models. We could not presume specific enumerations of these effects, nor even that they were quantified. The findings needed to be models ranking learning and perception modalities and their corresponding effect on retention, and the categories of these models had to be adequately similar to the familiar and common versions. We established the following criteria for this study: The finding would have to be a claim about at least three modalities and their comparative effect on retention, or two if these effects were also quantified in ways similar to current versions. In every case, at least two of these modalities had to be contained in Treichler's list. This would ensure that there were adequate similarities to establish a kinship and sufficient leeway for unforeseen varieties. We obtained facsimiles of all the publications in order to verify the findings.

3. Results

The oldest confirmed findings of the model were published in 1852, 1859, 1862, 1871, 1898, and 1901. Albeit not quantified, all of these authors ranked the retention from different learning modalities in ways consistent with Treichler's version of the chart. The oldest finding that quantified these effects was published in 1906 (Roads). We found 63 models published between 1906 and 1940, in conference proceedings, bulletins, books, magazines, newspapers, and academic journals. The earliest finding was published in "The British controversialist and impartial inquirer" in 1852 by an author writing under the signature "C.W., Jun":

> It has been truly and eloquently remarked that what we read, often fails to produce a lasting impression upon the mind; what we hear of, finds no permanent abiding place in the memory; but that which we see become engraven upon the recollection, it survives all the vicissitudes and changes we may encounter, its image its ever at our call, and not unfrequently accompanies its possessor down to the last hours of his earthly sojourn. (C.W., Jun, 1852, p. 130)

Reading, hearing, and seeing are all categories from Treichler's retention chart and their relative effect on retention seems to be consistent with his version. Seeing is more effective than reading or hearing. Although there admittedly is no explicit ranking of these last two modalities, the author listed them in the same sequence as Treichler did, possibly suggesting a similar hierarchy of efficiency.

Table 1. Construction of search-strings

Scale	("of what")	Subject	Modalities		
Percent		I	See/-s	Saw	Have seen
Per cent		You	Hear/-s	Heard	Have heard
Tenth/-s		He	Say/-s	Said	Have said
Ninth/-s		She	Write/-s	Wrote	Have written
Eighth/-s		We	Read/-s	Read	Have read
Seventh/-s		They	Do/-es	Did	Have done
Sixth/-s			See/-s and hear/-s	Saw and heard	Have seen and heard
Fifth/-s			Hear/-s and see/-s	Heard and saw	Have heard and seen
Fourth/-s					
Quart/-s					
Third/-s					
Half					
Tithe					

We find a hierarchy of modalities quantified for the first time in 1906 by Reverend Charles Roads. However, whereas the earlier models all address the persistence of memories, Roads' versions focus on the amount of information retained. Here Roads discusses the pedagogical usefulness of having large illustrations from the Bible on the wall of the Sunday school's main room:

> This would afford that most valuable expression by the student by both word and act. "We remember one tenth of what we hear, five tenths of what we see, seven tenths of what we say, nine tenths of what we do". (Roads, 1906, p. 583)

Interestingly, Roads repeats the retention model at the end of his article. This time the model appears to be modified according to his personal experiences:

> Whether it is true of all adults may be doubted, but in children, probably the impression made through the eyes is ten times greater than that which is made through the ears. The child remembers one tenth of what he hears, but he retains five tenths if he also sees it and seven tenths if he then expresses it, nine tenths if he can get it by his own action. (Roads, 1906, p. 584)

Roads here applies the numbers to children only. Furthermore, the categories in the first version appear to be discrete, rendering the effect of each learning mode individually, whereas several of the categories in the second version are additive, suggesting that multimodality increases the effect on retention.

Between 1906 and 1940, we find that the list of learning modalities and retention was an oft-published and widespread conception, as seen in Table 2. The terms "least", "less", "more", and "most" are not consistently used by these authors, but indicate the relative strength of the impression made on the mind, and the durability associated with the different modalities. As Table 2 shows, the authors generally agreed on the ranking of these modalities. On the other hand, the findings demonstrate comprehensive variations. There was no consensus on what constituted a complete list of modalities and levels of retention effect, subjects, or whether the effects were discrete or additive. We found no consistent use of percentages, nor of numerators and denominators in the fractions. Interestingly, the peculiarly neat numbers with increments of 10% propagated by Treichler's list in 1967, as well as by the modern Learning Pyramids, seem to originate from conversions of tenth fractions that are frequent among the findings: 1/10 = 10%, 3/10 = 30%, and so on.

4. Discussion

The task of unearthing the origins of the Learning Pyramids is not completed. Even if the findings conclusively date the model to the 1850s, the challenge of tracing the model back to its beginning still stands. The model is probably considerably older, given that is referred to as a true and eloquent remark in 1852 (C.W., Jun., p. 130), and as "a well known fact" in 1859 ("On the art of spelling", p. 245). However, predating the model further is now of less importance, because the earliest findings amply refute the existence of original empirical studies. We know that the empirical study of retention is a relatively recent area of research. It initially emerged in the 1870s and 1880s, pioneered by, among others, Wilhelm Wundt, and the first publication of experimental work on retention is generally considered to be Ebbinghaus' *Über das Gedächtnis: Untersuchungen zur experimentellen Psychologie* (*On memory: investigations in experimental psychology*) in 1885 (Mandler, 2007). In his study, Ebbinghaus primarily focused on the effect of repetitions on retention, and not on the effects of learning or perception modalities (Ebbinghaus, 1964 [1885]). As far as we know, Münsterberg and Bigham published the earliest systematic study of the effect of stimulus attributes on retention (1894) more than forty years after the earliest known publication of this conception.

The likelihood that the field of experimental psychology empirically verified the hierarchy and numbers of the retention models decades after being published in the mid-1800s and early 1900s

Table 2. Publications featuring the learning pyramid models pre-1940

Publication	Subject	Hear	Read	Write	See	See, hear	Say/ express	Say, hear	See, hear, express	Touch	Do	Do, say	See, do	See, hear, do
C.W., Jun. (1852, p. 130)	"We"	Less	Less		More									
On the art of spelling (1859, p. 245)	"We"	Least			Less	More								Most?
Forgetfulness and how to cure it (1862, p. 725)	"We"	Less	Less		More									
Cheever (1871, p. 210)	"We"	Least			Less		More?				Most			
Stubblefield (1898, p. 335)	"We"	Least			More	Most								
Teaching hints for intermediate classes (1901, p. 730)	"We"	Least			Least	More								
Roads (1906, p. 583)	"We"	1/10			5/10		7/10				9/10			
Roads (1906, p. 584)	"The child"	1/10				5/10			7/10		9/10?			
Haskell (1913, p. 638)	"We"	2/10			5/10					7/10	9/10			
Wayland (1914, p. 162)	"We"	1/10			5/10						9/10			
The annual meeting of Western Forestry and Conservation Association (1915, p. 29)	"People"	2/10			1/10		5/10	3/10			7/10	9/10		
Allen (1915, p. 255)	"We"	2/10			1/10	3/10	5/10				7/10	9/10		
Fowler (1915, p. 345)	"We"	Least	More			Most								
The graded lessons (1915, p. 597)	"We"	1/10			3/10	5/10								
Hincks (1915, p. 25)	"We"	2/10			5/10									
Robertson (1915, p. 4)	"We"	1/10		5/10	7/10						9/10			
Thomas (1915, pp. 72–73)	"We"	1/10			3/10	5/10	7/10				9/10			
Western Forestry and Conservation Association—Second session (1915, p. 61)	"We"	2/10			1/10	3/10	5/10				7/10	9/10		
Bring the children to the fair (1916, p. 1)	"We"	1/10			7/10		3/10				5/10			

(*Continued*)

Table 2. (*Continued*)

Publication	Subject	Hear	Read	Write	See	See, hear	Say/ express	Say, hear	See, hear, express	Touch	Do	Do, say	See, do	See, hear, do
Illustrations pay (1917, p. 5)	"The average man"	1/10			3/10	5/10								
Johnson (1917, p. 94)	"We"	1/10			5/10						9/10			
Calkins (1918a, pp. 22–23)	"We"	10%	15%		20%									
Calkins (1918b, p. 117)	"We"	10%	15%		20%									
Cronk (1918, p. 368)	"We"	3/10									9/10			
Day's issues treated by agency officers (1918, p. 9)	"We"	1/10			3/10	5/10								
General discussion on advertising (1918, p. 4)	"We"	3/10			1/10	5/10								
Iden (1918, p. 268)	"We"	1/10			3/10		5/10				7/10			
Kendall and Stryker (1918, p. 78)	"We"	1/10			5/10						9/10			
Packard (1918, p. 118)	"A child"	1/10			3/10						7/10			
Quelling the Restless Club (1918, p. 496)	"We"	1/10			3/10	5/10								
Richards (1918, p. 2)	"We"		1/10		3/10						9/10			
Robertson (1918, pp. 45–46)	"We"	1/10		5/10	7/10						9/10			
Baldwin (1919, p. 56)	"Children"	1/10			3/10	5/10								
Cooper (1919, p. 134)	"We"	1/10			5/10						9/10			
Every Child Loves to Dress Up (1919, p. 247)	"The spectator"	3/10			8/10						9/10			
Illustrations pay (1919, p. 18)	"The average man"	1/10			3/10	5/10								
Orr (1919, p. 275)	"We"	1/10									9/10			

(*Continued*)

Table 2. (*Continued*)

Publication	Subject	Hear	Read	Write	See	See, hear	Say/ express	Say, hear	See, hear, express	Touch	Do	Do, say	See, do	See, hear, do
Saunders (1919, p. 1)	"We"	1/10			3/10									
Beard (1920, p. 33)	"We"	2/10			5/10									
Conat (1920, p. 14)	"We"	1/10			5/10									
Iden (1920, p. 382)	"We"	1/10			3/10		5/10				7/10			
Smith (1920, p. 8)	"Students"	1/10	1/10								9/10			
Willard and Case (1920, p. 82)	"A general farm audience"	1/10			3/10	5/10	7/10				9/10			
Iden (1921, p. 266)	"We"	1/10			3/10		5/10				9/10			
Podhaski (1921, p. 214)	"An Individual"	1/5			3/5									
Smith (1921, p. 73)	"People"	1/10	1/10		3/10	5/10	7/10				9/10			
Stevens (1921, p. 754)	"We"	5%			50%									
Tunmore (1921, p. 31)	"A man"	1/10			3/10	5/10	7/10				9/10			
Burritt (1922, p. 55)	"We"	1/8	1/8		3/8						7/8			
Cassell (1922, pp. 24, 14)	"We"	1/10			3/10						9/10			
Scherr (1922, p. 6)	"We"	1/10			3/10						9/10			
Stevenson (1922, p. 147)	"The average man"	1/10			3/10	5/10								
Ticilos (1922, p. 18)	"The average individual"	1/5			3/5	4/5								
Bricker and Rochester (1923a p. 242)	"Students"	3/10 1/10			5/10									
Bricker and Rochester (1923b, pp. 221–222)	"Students"	1/10 3/10			5/10									

(*Continued*)

Table 2. (*Continued*)

Publication	Subject	Hear	Read	Write	See	See, hear	Say/ express	Say, hear	See, hear, express	Touch	Do	Do, say	See, do	See, hear, do
Moody (1923, p. 11)	"Children"	1/10			3/10						6/10			
Parke (1923, p. 387)	"We"	3/8			5/8						7/8			
Stevenson (1923, p. 177)	"The average man"	1/10			3/10	5/10								
Art in our college (1924, p. 1)	"We"	1/10			5/10						9/10			
Cronk (1925, pp. 281–283)	"We"	less			more						9/10			
What should be done? (1926, p. 116)	"A boy"	1/10					5/10				7/10			
Doing and remembering (1928, p. 16)	"A general audience"	1/10			3/10	5/10	7/10				9/10			
Doing means remembering (1928, p. 133)	"A general audience"	1/10			3/10		5/10				9/10			
Dixon (1928, p. 5)	"A general farm Audience"	1/10			3/10	5/10	7/10				9/10			
Students learn most by doing (1928, p. 1)	"A general audience"	1/10			3/10	5/10	7/10				9/10			
Graeber (1930, p. 7)	"We"	1/10				2/5					9/10			
Goode (1932, p. 429)	"The ordinary citizen"	1/10			3/10	5/10								
Mead and Orth (1934, p. 8)	"We"	1/10			One half						9/10			
Wood (1936, p. 258)	"We", "children"	Less			More						Most			
Canfield (1940, p. 353)	"The average man"	1/10			3/10	5/10								

is minuscule. In 1912, V.A.C. Henmon published a review of retention studies focussing on the effects of presentation modalities of auditory, visual, and motoric presentations (e.g. articulating and writing). These categories are to some extent comparable to the modalities seeing, hearing, saying, writing, and (perhaps) doing. Henmon reviewed 29 studies on retention published between 1894 and 1910, all focusing on the relation between presentation modality and retention. He found that the findings were inconclusive as to whether visual presentations gave higher retention than auditory. Rather:

> … Meumann … states that a single method of presentation for all cases can not be declared to be most advantageous. The value of a method of presentation varies with the nature of the material, the type of imagery of the learner and the procedure in presentation. (Henmon, 1912, p. 80)

Addressing the effect of multimodality, Henmon determined that the reported findings were indecisive. Münsterberg and Bigham reported "significant superiority in the combined method" (Henmon, 1912, p. 81). Quantz on the other hand found no advantage in the combined method, considering it a possible hindrance of retention (Henmon, 1912, p. 81). Instead, the studies repeatedly found that the types of material (images, nonsense syllables, numbers) influenced retention, in accordance with Meumann's above remark. Consulting the publications in Henmon's review leaves the impression of early retention studies as a precocious field, despite the radical new effort of measuring and quantifying higher mental processes. The results reported by these often meticulous and laborious studies are much more refined, qualified and detailed than those reported in the Learning Pyramids, contemporary versions included. In fact, it seems that the association of the Learning Pyramids with research emerged several decades after the publication of the earliest models. A column in the insurance-agent magazines *Field Notes* and *The Insurance Field* points in the direction of "the Carnegie Bureau of Scientific Research" ("Illustrations pay", 1917; p. 5; "Illustrations pay", p. 28, 1919; respectively). We have not been able to verify the existence of this institution. Some years later, the model was attributed to "The Carnegie Institute of Technology" (Stevenson, 1922; p.147; 1923; p. 177). This institute, now part of Carnegie Mellon University, was not established until 1912 ("CIT: More than 100 years", n.d.).

Another insurance-sales magazine related the numbers to a "Munsterberg", who "after tests, said that we remember one-tenth of what we hear, three tenths of what we see and five tenths of what we both see and hear" ("Day's issues", 1918, p. 9). This probably refers to the German psychologist Hugo Münsterberg (1863–1916), and he is possibly the same person who is referred to in a dental journal a few years later. The rates of retention reported, however, are not the same:

> Years ago when Professor Munsterberger [*sic*] of Harvard was teaching psychology he gave 298 students a memory test. He found that by reading to them they retained one-tenth of what they heard; by lecturing they retained three-tenths of what they heard; by picturing it to them they retained five-tenths of what they saw. (Bricker & Rochester, 1923b, pp. 221–222)

Other authors (Iden, 1918, 1920, 1921; Packard, 1918) attributed similar numbers to US psychologist G. Stanley Hall, for example:

> G. Stanley Hall, says that one-tenth of what a child hears becomes a permanent part of that child, as well as does three-tenths of what he says, and seven-tenths of what he does. (Packard, 1918, p.118)

We suspect the authors attached the names Münsterberg and Hall in order to add authority the retention claims, as both were major figures in the new field of experimental psychology. This presumably corresponds to the more recent practice of ascribing this kind of claims to US psychologist William Glasser (e.g. Ascough, 2002; Diachun, Dumbrell, Byrne, & Esbaugh, 2006).

The successes of early experimental psychology may have inspired the various attempts at quantifying the models, and we also suspect that they have motivated the shift of focus from amount of time to the amount of information retained. But these early studies do not corroborate them in any way.

A set of learning modalities similar to those distributed by Treichler (1967) were at some point fused with a misreading of Edgar Dale's Cone of experience as a hierarchy of learning modalities (Molenda, 2004; Subramony, 2003), and these early categories were supplemented and partly replaced with categories of presentation modalities like "audiovisual", "demonstrations", and "discussion groups". The resulting hybrid models mix separate and composite perception modalities, and complex presentation modalities (e.g. "audiovisual"; "exhibit"; "demonstrations"). Later versions also include learning strategies (e.g. "practice"; "immediate use"; "teach others").

5. Contemporary retention research and the learning pyramids

Today, more than 160 years after the first known publication of a proto-Learning Pyramid, books, proceedings, peer-reviewed articles, and encyclopaedias continue to distribute the progeny of these early models (Holbert, 2009; Letrud & Hernes, 2016; Masters, 2013). We must admit that theoretically, there may be studies published between 1912 and today that to a degree offer support to some version of the Learning Pyramids. However, the extensive and fruitless searches performed for this research leaves us confident that these studies are non-existent.

But, some might argue, even if the Learning Pyramids are unsubstantiated as theoretical models, surely they may serve a practical function? We believe that they would be a poor choice for heuristic models. We shall argue that the conception of memory assumed by the Learning Pyramids fails to accommodate basic insights into the structure and function of memory advanced in the last decades by cognitive psychology. Instead, the Learning Pyramids' accounts of human memory are simplistic and inadequate. Despite the numerous transformations and diversifications of the Learning Pyramids from the 1800s up until today, these models have retained more or less the same folk-psychological conception of learning and memory as they featured in their early days: According to the Learning Pyramids, memory is a passive depository directly accessible for storage via a set of perception modalities. The amount of material retained, and duration, is primarily, if not exclusively, an outcome of modes of perception and modes of presentation. We shall make our argument after a short rendition of some key features of memory.

The standard picture of human memory is in large part based on Atkinson and Shiffrin's Modal Model of Memory. This model has been "a prominent guiding framework for research" since the seventies (Healy & McNamara, 1996, p. 143). The standard picture distinguishes three types of memory stores: sensory-, working- (sometimes referred to as "short-term"), and long-term memory. We base the following presentation of the standard picture on three common psychology textbooks: Baron (1992); Holt et al. (2015); Smith et al. (2003). Together these publications illustrate the level and longevity of the consensus on memory:

The sensory memory stores the totality of the immediate information captured by the sensory apparatus. It handles all perceptual modalities, but most prominently we process visual and auditory information through iconic and echoic memory. This information is transient; however, we are able to transfer these memories to our working memory for operation by directing attention to some of the information in the sensory memory. We can code this information into the working memory with a visual code, a phonological code, alternatively with a semantic code: as mental pictures, sounds, or by some meaningful associations, respectively. "Mental pictures" comprises both text as well as images (Baron, 1992; Holt et al., 2015; Smith et al., 2003). UK psychologist Alan Baddeley has suggested the addition of an episodic buffer that allows integration and manipulation of memories from both working and long-term memory (Holt et al., 2015)

The capacity of the working memory is limited. We are generally able to hold seven units of information (e.g. names, phone numbers, items on a shopping list), give or take two, and only for a few seconds (Baron, 1992; Smith et al., 2003). Later research suggests that the number of units is even lower (Holt et al., 2015). In order to preserve these fleeing moments of life and learning, we must transfer them into our declarative or nondeclarative long-term memories (Baron, 1992; Holt et al., 2015; Smith et al., 2003).

When we are primed, conditioned, or learn skills, whether they are motoric, cognitive or perceptual, we store this knowledge in the nondeclarative (or "procedural") memory. The performance, like riding a bike or reciting the alphabet, does not require conscious retrieval. Whereas personal experiences, facts and concepts are primarily stored in the declarative memory, and retrieving this information does require a conscious effort (Baron, 1992; Holt et al., 2015; Smith et al., 2003).

Unlike sensory memory and working memory, information retained by long-term declarative memory is not structured according to modality. Whether we receive information verbally, by images, or in writing, this is rarely the format in which these memories are stored. Rather, we preserve the general gist of meaning of what we read, hear and see, and rarely verbatim or eidetic. Declarative memory comprises semantic memory and episodic memory. Semantic memory concerns facts and concepts, like the year of the French Revolution. Episodic memory is autobiographical, and we code the information in relation to ourselves (Baron, 1992; Holt et al., 2015; Smith et al., 2003), for example a memory of a slightly disappointing visit to the remains of the Bastille.

Preserving semantic knowledge is primarily a question of making meaningful connections through elaborative rehearsal. Encoding several connections in the new information makes it part of our web of knowledge and thereby better integrated and more accessible (Baron, 1992; Holt et al., 2015; Smith et al., 2003). An elaborated and increased understanding of the Storming of the Bastille during the French Revolution creates several associations and connections in the material, and thereby multiple retrieval routes and increased accessibility of the particularities, causes, and significance, of this event.

Admittedly, the above rendition of the standard picture of human memory is merely a compendiary presentation of the structure and workings of our memory. However, the standard picture arguably represents the consensus of cognitive psychology, and has so for decades even though some issues are under debate. The Learning Pyramids' conception of memory deviates from this standard picture in significant ways. First, according to the Learning Pyramids the transferral of verbal information to declarative long-term memory is a straightforward mechanic allocation of information independent of a process of encoding, equivalent to a tape recorder that more or less accurately stores the information transferred to it. Whereas, in the standard picture, encoding semantic memories into the declarative memory involves construction of information networks.

Second, the stress placed on perception modalities and presentation modalities by several categories in the Learning Pyramids contradicts the way the semantic declarative memory is structured. Although modalities like seeing and hearing are significant to some degree in relation to working memory, they play a minor role in the encoding, long-term storage and retrieval of the information.

Third, although the most effective learning strategies in several Learning Pyramids, like "practice" and "immediate use", are indeed recognizable as forms of encoding, they render primarily the effect of repetition. Admittedly, repetition is a well-tried method of transferring information to long-term memory, like drilling the alphabet, or the ten-time table. However, these are primarily strategies for encoding information into the non-declarative memory. Without addressing the essential educational issue of deep processing, i.e. the elaboration and incorporation of new information into a meaningful whole, we question whether the Learning Pyramids will be able to meet the needs of educators and educational researchers for a substantial catalogue of didactic strategies, even if they were adequately corroborated.

It seems paradoxical that the Learning Pyramids are often cited in seemingly constructivist approaches to learning, as arguments for student activation. The model itself describes a one-way street of communication, of information passing from teacher to students, rather than the students' internal construction of knowledge. It stresses repetition as a means of encoding, rather than contextualization of information into a meaningful network, and knowledge as a fixed entity. In short, the Learning Pyramids appear to corroborate a realist position in educational philosophy.

6. Conclusion

The Learning Pyramid models were published at least as early as the 1850s, and the hierarchy of learning modalities originated as no more than a conjecture. Despite their lack of evidence, the Learning Pyramids have been part of educational debates and practices since the 1850s, and have since grown from a mere saying, or a commonsensical idea, into a family of quasi-scientific models. Today, several areas of educational studies consider these models authoritative. However, despite the plasticity and many varieties of the Learning Pyramids, they have failed to implement advances made by cognitive psychology, and ignored contradicting evidence. Consequently, they ought to be rejected both as theoretical and heuristic models.

The Learning Pyramids' message of activity-based learning probably seems just as fresh and modern to educationists today as they must have done in the mid-1800s, and we speculate that they have served as quick and easy ways of contending one's preference for varieties of activity and experience-based learning all these years. However, despite their progressive and constructivist appearance, the Learning Pyramids are first and foremost traditional realist learning models, ranking the efficiency of modes of information transferral.

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