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The persistence of matching teaching and learning styles: A review of the ubiquity of this neuromyth, predictors of its endorsement, and recommendations to end it

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Educational neuroscience tries to bridge neuroscience and education. It tries to combat *neuromyths*: beliefs that appear grounded in neuroscientific research but that are not supported by empirical evidence. One such neuromyth claims that matching teaching style to students' preferred learning styles (e.g., visual teaching to visual learning) will lead to improved academic outcomes. The only formal way to test this *meshing hypothesis* is by finding a statistical crossover interaction effect which shows that matching teaching and learning styles improves academic outcomes, while non-matching teaching and learning styles negatively affects academic outcomes. Several studies are reviewed and none of these yielded empirical support for the meshing hypothesis. Reviewed studies suggest that educators widely believe the veracity of the meshing hypothesis. Predictive factors are discussed: even having some formal knowledge of neuroscience does not protect educators from endorsing neuromyths like the meshing hypothesis. An elaboration on teaching *focused* neuroscience to future educators is provided as a potential solution.

KEYWORDS

neuromyth, learning styles, meshing hypothesis, matching teaching and learning styles, educational neuroscience

1. Introduction: Neuromyths and learning styles

Have you ever heard the statement that we only use 10% of our brains? If so, you have been exposed to an example of a *neuromyth*: a common belief that appears to be founded in neuroscience, but for which there is no empirical basis. Howard-Jones (2014) traced the term back to Alan Crockard who used it to describe scientifically unsupported ideas about the brain in medicine. However, medicine is not the only discipline associated with neuromyths: for example, the field of educational neuroscience specifically attempts to apply empirical findings from neuroscience to educational practice (Thomas et al., 2019) and it should seek to eliminate neuromyths (Goswami, 2006). Since Goswami's opinion paper, an influential report on the brain and its role in learning has been published (Organization for Economic Cooperation and Development, 2007), which includes a chapter on dispelling neuromyths in education.

Such publications have caused the scientific interest in neuromyths in education to increase substantially.

The neuromyth that we only use 10% of our brains may be relatively harmless, although it does provide a feeding ground for all sorts of transcendental interventions and trainings online, but there are more impactful neuromyths. For example, many educators appear to hold the belief that students have predominant learning styles that optimize their learning outcomes (e.g., Torrijos-Muelas et al., 2021) and these educators may devote considerable resources to classifying their students' learning styles to optimize their learning (Papadatou-Pastou et al., 2021; also see Pashler et al., 2008, for some examples of the financial ramifications of this practice).

Learning styles can be defined in different manners: Coffield et al. (2004) reviewed no fewer than 71 different models of learning styles. Here, we will provide an overview of three common learning styles theories, which are all relevant to the current paper. Kolb (2015) theorized that optimal learning is experiential and that students move through a learning cycle that runs from experience and observation (together called experience grasping) through conceptualization to experimentation (together called experience transforming) and then back to experience. Over time, learners strengthen one pole of experience grasping and one pole of experience transforming. The combinations of different poles yields four types of learners: for example, someone who prefers to learn through concrete experience and active experimentation would be called an accommodator, while someone who learns optimally through abstract conceptualization and active experimentation would be referred to as a converger. Other combinations of experiencing, conceptualizing, and observation, yield so-called divergers and assimilators. People can be assigned to these learning styles through Kolb's (1985) Learning Style Inventory. Although some authors have found support for Kolb's model (e.g., JilardiDamavandi et al., 2011; Abdulwahed and Nagy, 2013), it has also been criticized strongly; critics have pointed out that his Learning Style Inventory falsely dichotomized continuous variables like abstract and concrete experiential learning (Manolis et al., 2013) and that, among other shortcomings, there is little empirical evidence for Kolb's claims (Smith, 2010).

Mumford (1997) modified Kolb's model by aligning the steps in Kolb's learning cycle with managerial theory, making Kolb's work more applicable to business settings. This led to semantic changes like renaming Kolb's experimentation phase "concluding from the experience." Honey and Mumford's adaptation of Kolb's theory yielded four learning styles: the activist, the reflector, the theorist, and the pragmatist, which can be used by managers to gain insight in their work behaviors and to learn more from their everyday experiences. Managers can be classified into one of these four learning styles through use of the Learning Styles Questionnaire (Honey and Mumford, 1992). The utility of the use of this model has been called into question by Coffield et al. (2004), who criticized the lack of empirical support for and independent research into this model.

In a self-published book, Fleming (2001) expanded a learning styles model proposed by Barbe et al. (1979), and claims that specific sensory modalities dominate learning in different individuals. This model suggests the existence of visual and aural learners, persons who prefer to learn through reading and writing, and kinesthetic learners, which has yielded the common VARK acronym. Fleming has acknowledged that some persons may prefer two or more sensory modalities to learn optimally, and has referred to such persons as multimodal learners. Assessment of the VARK Learning Styles Inventory has yielded some support for the validity of this instrument, but have also cautioned its use (Leite et al., 2010). The concept of sensory learning modalities has been used by Dunn et al. (1989) to develop their Learning Styles Inventory, which can be used by educators to assess students' learning styles and to design classroom activities that meet all learners' (multi)sensory needs. The idea that specific sensory modalities dominate learning in different individuals has been heavily criticized and this approach has been labeled pseudoscientific (Lilienfeld et al., 2010).

In the pragmatic realm of the classroom, learning style theories are often reduced to the idea that students prefer to receive instruction that is delivered through a specific sensory modality, typically either visually, auditorily, or tactilely/kinesthetically (Dunn et al., 1989) *and* that providing students with their preferred method of instruction will optimize their learning. It is crucial to emphasize that it is scientifically widely accepted that students may prefer learning through a certain modality, for example, visually rather than auditorily (Pashler et al., 2008; Rogowsky et al., 2020); some educators have been shown to confuse such ideas with the concept that matching teaching and learning styles improves educational outcomes (Deligiannidi and Howard-Jones, 2015); such confusion emphasizes the need to investigate why educators believe these kinds of neuromyths and what can be done about that. The idea that matching a student's preferred learning style (e.g., visual) to a corresponding teaching style (e.g., visual to visual; auditory to auditory) would lead to improved educational outcomes *and* that matching a student's preferred learning style (e.g., visual) to a non-corresponding teaching style (e.g., visual to auditory) would negatively affect outcomes, is not widely supported empirically (Pashler et al., 2008; Howard-Jones, 2014; Newton et al., 2021). Pashler et al. (2008) have referred to this idea, that matching teaching and learning styles improves learning outcomes while non-matching teaching and learning styles negatively affects learning outcomes, as the *meshing hypothesis.* Although the belief in the veracity of this meshing hypothesis is ubiquitous among educators (see section "2. Empirical support for the meshing hypothesis" below), there is a substantial paucity of empirical evidence to support it (e.g., Willingham et al., 2015). This has even led textbook publisher Pearson (2016) to issue a white paper that states that "there is a striking lack of evidence to support the core learning styles claim that customizing instruction based on students' preferred learning styles produces better learning than effective universal instruction" (p. 3).

If there is such a scarcity of empirical support for the meshing hypothesis, why do learning style assessments remain so popular in the classroom? As with most myths, neuromyths often contain a kernel of truth. For example, consider the 10% brain use neuromyth. It is true that we do not use every structure in our brain for every single cognitive task, but "we use most of our brains most of the time" (Geake, 2008).

In the past years, a number of excellent reviews and empirical studies have been published in this field. The current paper will provide an overview of this work, but its main goal is to move beyond describing the situation of educators' belief in neuromyths like the meshing hypothesis and to try and understand *why* educators endorse that hypothesis and what can be done about this.

The current paper does not purport to be a systematic review; it is a narrative review and the author made diligent effort to uncover as many recent and relevant publications as possible, with the goal of providing the reader as unbiased and structured an overview of this research field as a narrative review allows. Therefore, first, empirical evidence for the meshing hypothesis will be discussed. This evidence was acquired through an exhaustive search in the PubMed and PsycInfo databases, using "learning style*" and "meshing hypothesis" as search terms. The uncovered literature was scanned and judged for relevance. Next, the paper will provide an overview of the ubiquity of the belief in learning styles among educators and the pragmatics of believing in the meshing hypothesis will be discussed, followed by a review of factors that predict an endorsement of neuromyths. Finally, recommendations to eliminate neuromyths from instruction in education will be provided. In doing so, this paper hopes to contribute to a reduction of the oversimplification of neuroscience in educational settings and to try and establish a two-way dialog between neuroscience and education, as advocated by Goswami (2006), which does not just affect individual institutions, but even curricular development on a national scale (Purdy and Morrison, 2009).

2. Empirical support for the meshing hypothesis

This section of the paper is intended to provide an overview of the lack of support for the so-called meshing hypothesis. A number of excellent reviews have been published in this field and these reviews will be cited here. Most of these reviews and much of the empirical work published in this field is very descriptive, and these works emphasize *that* there is a problem, but not *why* that problem exists. Once reader is (re)familiarized with the meshing hypothesis and the lack of empirical support for that hypothesis, I will review the ubiquity of educators' endorsement of this hypothesis. Pashler et al. (2008) coined the term *meshing hypothesis* to describe the claim that instruction style should be matched to students' preferred learning style to optimize their educational outcomes. For example, according to this hypothesis, "visual learners" should be presented with visual information to optimally learn material, whereas "auditory learners" should be provided with auditory content to maximize those learners' educational performance. If academic performance is indeed enhanced by meshing teaching and learning styles, then such meshing should be widely implemented in education. The utility of the adoption of such an approach crucially depends on empirical support for the meshing hypothesis. In this section, such empirical evidence is reviewed and evaluated.

Pashler et al. (2008) describe a specific statistical approach to test the meshing hypothesis, which involves several criteria: participants have to be classified as having a specific learning style (e.g., visual vs. auditory), they must be randomly assigned to a specific instruction style condition (e.g., visual or auditory), they must all be tested in the same sensory modality (e.g., be subjected to a written test, irrespective of instruction style) to make performance comparable, and, crucially, the results of the experiment have to demonstrate a crossover interaction. This crossover interaction criterion implies that students who prefer a certain learning style (e.g., visual learning) will perform better following a compatible instruction style (visual) and worse following an incompatible instruction style (e.g., auditory; see **Figure 1**). This crossover interaction is crucial, because it is the only way to unequivocally demonstrate that instructing students in a style that matches their learning style *improves* their learning performance, while instructing them in a style that does not match their learning style *decreases* their learning performance. Now consider an empirical outcome that is described as unacceptable evidence for the meshing hypothesis by Pashler et al. (2008) Visual learners and auditory learners can receive either visual or auditory instruction. If the experimental results suggest that *all* students who were instructed in a visual style outperform *all* students who were instructed in an auditory style, no crossover interaction occurred, and the conclusion of the study should be that visual instruction improves learning outcomes irrespective of learning style (**Figure 1**).

Pashler et al. (2008) reviewed four published studies that investigated the efficacy of meshing learning styles and instruction styles, and found that only one of those studies reported a crossover interaction, but that study actually suffered from methodological issues such as using derivative results: the participants' raw final scores did not reveal the imperative crossover interaction. Concretely, 75% of the reviewed studies did not yield a crossover interaction, and thus failed to provide support for the meshing hypothesis.

The crossover interaction methodology described by Pashler et al. (2008) was used by Rogowsky et al. (2015), who classified adult participants as visual or auditory learners according to the Learning Styles Inventory by Dunn et al. (1989). These participants then either read a passage from a book (visual learning), or listened to an audio recording of that same passage (auditory learning), and finally answered questions about that passage in a written format that was identical for both learning conditions. The results of this study did not yield a crossover interaction: visual learners outperformed auditory learners, irrespective of the modality of instruction (visual or auditory) that was used, a situation analogous to the hypothetical data pattern illustrated in panel C of **Figure 1**, except in the figure, auditory learners outperform visual learners. Therefore, this experiment did not provide evidence for the meshing hypothesis. Rogowsky et al. (2020) performed a similar study, focusing on fifth graders (10–11 year-olds). The authors' experimental design was comparable to their 2015 study: children were classified as visual or auditory learners according to Dunn et al. (1989) Learning Styles Inventory, and then text comprehension was tested following visual (reading) or auditory presentation of a text. Again, visual learners consistently outperformed auditory learners, and once again, the imperative crossover interaction was absent: in this study, "auditory learners" did not perform better than "visual learners" following instruction in the auditory modality. Therefore, this experiment, too, did not provide evidence for the meshing hypothesis.

Massa and Mayer (2006) did not use the crossover interaction terminology, but they contrasted the performance of visual and verbal learners by assessing whether the former group learned more from picture-based help screens while the verbal learners benefited more from text-based help screens. The authors found that *all* participants (both college students and non-college educated adults) learned more from picture-based help screens, irrespective of being a visual or a verbal learner. Aslaksen and Lorås (2018)

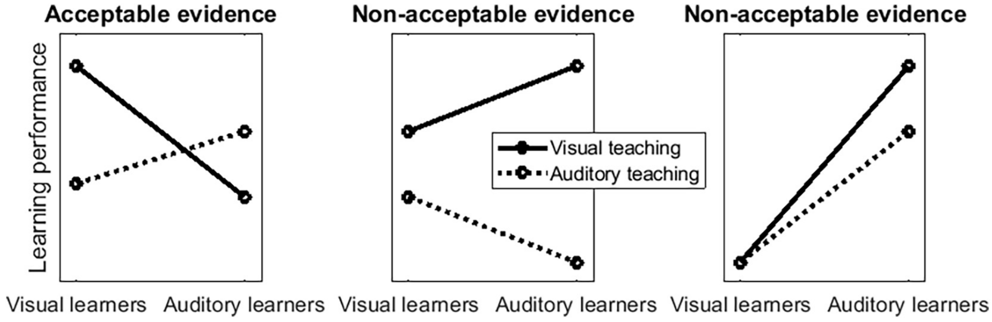

FIGURE 1
Acceptable and non-acceptable evidence for the meshing hypothesis. **(A)** Acceptable evidence for the meshing hypothesis requires a full crossover interaction. In this case, matching teaching and learning styles improves learning performance and not matching teaching and learning styles decreases learning performance. **(B)** An example of unacceptable evidence for the meshing hypothesis, where one teaching method (visual teaching) consistently outperforms another teaching method (auditory teaching), irrespective of students' preferred learning styles. **(C)** Another example of unacceptable evidence for the meshing hypothesis, where one teaching method (visual teaching) consistently outperforms the other teaching method and visual learners consistently perform worse on their assessment than auditory learners, irrespective of learning style. Only studies that report a crossover interaction provide acceptable evidence for the meshing hypothesis. Figures based on Pashler et al. (2008), copyright © 2008 by SAGE Publications; reprinted by Permission of SAGE Publications.

reviewed ten studies that used the Pashler et al. (2008) crossover interaction method (including the aforementioned work by Massa and Mayer, 2006), and concluded that the rigorous application of this method did not yield a reliable relationship between learning style and positive learning outcomes in the reviewed studies. This included a study on older adults (Constantinidou and Baker, 2002), as well as a study that measured catheter placement attempts—a kinesthetic outcome variable (Papanagnou et al., 2016). Although they provided almost no detail about the studies investigated, a similar lack of reliable findings was reported by Rohrer and Pashler (2012) in their very short review of twenty studies in this field.

Although the crossover interaction analysis described by Pashler et al. (2008) is methodologically very rigorous, there are other methods to test whether learning styles positively influence learning outcomes. For example, Krätzig and Arbuthnott (2006) tested university students who were classified by themselves and by an objective questionnaire as visual, verbal, or kinesthetic learners. Each participant performed a memory test in three modalities: they had to remember and recreate abstract line drawings (visual memory), remember and reproduce a short story presented auditorily (auditory memory), and manipulate shapes while blindfolded and then draw remembered shapes (kinesthetic memory). Firstly, there was a low correspondence between the students' self-reported learning styles and the objectively determined learning styles: a correspondence was successfully established in only 44.6% of all participants. This calls into question how well people are able to classify *their own* learning style. Secondly, there was no reliable relationship between learning style and memory test: Pashler et al. (2008) critical crossover interaction was not established; in fact, kinesthetic learners performed well on the *visual memory test.* Similarly, Sandmire et al. (2000) also reported no effect of learning styles, classified based on Kolb's (1985) Learning Style Inventory, on the performance of a collaborative exercise in a course on neuroscience taken by occupational and physical therapy students.

Finally, in more descriptive studies, Liew et al. (2015) failed to find a reliable relationship between learning styles and academic performance in preclinical undergraduate medical students, and Shreffler et al. (2019) reported no connection between self-identified learning styles and academic success in sport management students. These findings, again, call the validity and utility of learning styles, and therefore, by extension, the meshing hypothesis, into question.

The studies reviewed here all yield similar results: using different instruments to classify learning styles, and sampling different populations of participants of different ages, no evidence has been reported that suggests a reliable relationship between learning styles and academic outcomes. Interestingly, one study described a relationship between learning styles and psychosocial factors like experiencing coherence or burnout in students at a German university (Burger and Scholz, 2014); such psychosocial factors are interesting and if they do not represent a spurious correlation, they may be worthy of further exploration, but they are of little relevance to the meshing hypothesis. The next section will discuss how common the belief in learning styles is.

3. The ubiquity of the neuromyth of the veracity of the meshing hypothesis

The previous section has reviewed the paucity of empirical support for the meshing hypothesis. Concerningly, belief in neuromyths like the utility of matching teaching styles to learning styles, are quite prevalent among educators, as a number of empirical studies and reviews have shown (Howard-Jones, 2014; Willingham et al., 2015; Düvel et al., 2017; Aslaksen and Lorås, 2018; Torrijos-Muelas et al., 2021). The same caveat that was offered in the previous section applies here too: most of the work reviewed here shows *that* there is a problem but not *why* there is a problem. Once the reader is (re) familiarized with how widespread the endorsement of the meshing hypothesis is in educators, this paper will analyze *why* this may be the case and *how* the problem can be addressed.

Dekker et al. (2012) assessed 212 primary and secondary school teachers with an interest in educational neuroscience from The Netherlands and the United Kingdom. These teachers were asked to indicate whether they believed 32 statements about educational neuroscience to be true. About half of these statements were neuromyths; of particular interest here is, "Individuals learn better when they receive information in their preferred learning style (e.g., auditory, visual, kinesthetic)," which clearly represents the meshing hypothesis. Remarkably, 93% of the teachers from the UK and 96% of the Dutch teachers endorsed this false statement as true. Ferrero et al. (2016) found similar results when they presented this same statement to Spanish teachers (ranging from kindergarten to secondary school teachers and vocational education teachers): 91.1% of these teachers believed this neuromyth to be true. These authors also conducted a meta-analysis to establish cross-cultural endorsement of neuromyths across countries and reported that 85.8–98.5% of educators in several European and South American countries and in China believed the aforementioned statement about learning styles to be true. Hughes et al. (2020) reported that over 79% of investigated Australian teachers endorsed the veracity of the meshing hypothesis.

Grospietsch and Mayer (2019) investigated the beliefs in neuromyths, including the veracity of the meshing hypothesis, in 550 preservice science teachers who specialized in biology. These participants were at different levels of their training, ranging from first-year education students to postgraduate students. All participants were asked to indicate the truthfulness of a set of statements, including, "Individuals learn better when they receive information in their preferred learning style (e.g., auditory, visual, kinesthetic)"; 93% of participants endorsed this statement. Interestingly, advanced students, who had already completed a course on human biology which included a neuroscience component, less frequently (90%) considered this statement to be true than first-year students (97%). Although it is encouraging that neuroscientific education reduced the belief in this neuromyth somewhat, it should be noted that nine out of ten advanced education students who were studying to be science teachers still believed this neuromyth to be true.

The findings reported by Grospietsch and Mayer raise an interesting question: does having knowledge of neuroscience reduce the belief in neuromyths? This question will return in section "5. Predictors of the endorsement of neuromyths and the meshing hypothesis." Macdonald et al. (2017) compared the belief in neuromyths among 598 educators, 3,045 members of the general public, and 234 persons with a high exposure to neuroscience (i.e., persons who indicated to have taken many college-level neuroscience courses but who were not educators). Several neuromyths were presented, including, "Individuals learn better when they receive information in their preferred learning style (e.g., auditory, visual, kinesthetic)." Belief in several neuromyths was investigated, but of specific relevance here, 93% of the members of the general public believed in the learning styles neuromyth, 76% of educators did, and 78% of persons with high neuroscience exposure endorsed this neuromyth. Also relevant in the current context were responses to the statement, "Children have learning styles that are dominated by particular senses": 88% of the general public endorsed this statement, 71% of educators did, and 68% of persons with high neuroscience exposure did. So, having more neuroscience knowledge reduces the belief in the learning styles neuromyth—relative to the general public's view—but it does not eliminate it. In fact, educators without specific neuroscience knowledge endorsed this particular myth only marginally less often than neuroscience experts did. Overall, having taken multiple neuroscience courses reduced the belief in neuromyths.

The results reviewed above are concerning, because they suggest an overwhelming belief in the veracity of a neuroscientific fable. This conclusion is compounded by the observation that educators' belief in the neuromyth of the meshing hypothesis has not decreased over recent years, either (Newton and Salvi, 2020). However, endorsement of a neuromyth by educators does not necessarily imply that they also implement that neuromyth in their teaching practice. The next section will explore this relationship.

4. The pragmatic effects of endorsing the meshing hypothesis and limitations in reviewed studies

It is important to distinguish between believing in the veracity of the meshing hypothesis and incorporating it into educational practice. This section will review evidence that investigates that correlation. Newton and Salvi (2020) reported a strong relationship between the percentage of educators who endorsed a belief in the utility of matching instruction style to learning styles (89.1%) and the percentage of educators who indicated that they also (planned to) match instruction style to learning style (79.7%). However, when Newton and Miah (2017) surveyed 114 higher education instructors about their views on learning styles, including their endorsement of the statement, "Individuals learn better when they receive information in their preferred learning style (e.g., auditory, visual, kinesthetic)." These authors found that only 58% of educators believed this statement to be true: the lowest endorsement percentage reported in all studies reviewed here. Crucially, only 33% of the investigated educators indicated they had actually used learning styles in the classroom in the past 12 months, and 33.1% of participants indicated having administered a learning styles inventory during that time span. Interestingly, 64% of these educators agreed with the statement, "I try to organize my teaching to accommodate different student Learning Styles (e.g., visual, kinesthetic, assimilator/converger)." As stated before, students do tend to prefer to learn through certain modalities: that is not a neuromyth, and presenting material in different sensory modalities (e.g., verbally and visually) is a useful teaching practice. Newton and Miah (2017) results reveal an interesting discrepancy between *believing* in learning styles (58%) and *using* learning styles in the classroom, likely due to an endorsement of the meshing hypothesis (around 33%). This is somewhat promising, because it suggests that even if educators believe in the neuromyth of the utility of matching teaching and learning styles, that belief is not necessarily associated with trying to match teaching and learning styles in the classroom.

Methodologically, we must be mindful of an important caveat in the interpretation of the literature on neuromyths like the meshing hypothesis that Newton and Salvi (2020) mention: almost all studies reviewed here have subjected participants to (minor variations on) the statement by Dekker et al. (2012): "Individuals learn better when they receive information in their preferred learning style (e.g., auditory, visual, kinesthetic)." If the meaning of such a statement is unclear, such unclarity might be compounded over multiple studies, biasing the literature in this field. For example, it may have been unclear to participants what it truly means for students to "receive information in their preferred learning style" (Newton and Salvi, 2020). As was already mentioned in the context of Newton and Miah's (2017) study, if educators believe that matching teaching styles to learning styles is useful, that does not mean they also actually implement such strategies in the classroom: evidence for this association seems inconclusive at this time but even in the most optimistic study, where "only" 58% of educators endorsed a belief in learning styles, no less than 33% of educators actually implemented the meshing hypothesis in their teaching (Newton and Miah, 2017).

Crucially and pragmatically, is providing instruction that accommodates different learning styles truly associated with improved educational outcomes? Based on the literature which used the rigorous crossover interaction method proposed by Pashler et al. (2008) reviewed above, the answer seems to be that there are no benefits to meshing instructional style and learning style. Interestingly, Horvath et al. (2018) evaluated whether award-winning educators were more likely to believe in neuromyths, like the veracity of the meshing hypothesis, by comparing their endorsement of neuromyths to those of non-award-winning educators studied by other authors. Horvath et al. (2018) reported that 84% of award-winning educators endorsed the statement, "Individuals learn better when they receive information in their preferred learning style (e.g., auditory, visual, kinesthetic)," while 93% of non-award-winners endorsed this statement. This difference was statistically significant, but in thirteen out of the total fifteen statements about educational neuromyths, there was no such difference between award-winning and non-award-winning educators. Horvath et al. (2018) interpret this as an absence of evidence for the idea that endorsing neuromyths impacts teaching quality, either in a positive or a negative manner. However, as mentioned before, endorsing a *belief* in a neuromyth does not imply *implementing* that neuromyth in one's teaching—and Horvath et al. (2018) study did not investigate whether these award-winning educators did so.

This section has provided some pragmatic implications and limitations of the studies reviewed in this paper and has attempted to provide clarity on whether believing in the neuromyth of the utility of matching teaching and learning styles matters in a pragmatic sense, because the literature in this field does not always distinguish between believing in learning styles and applying that belief in the classroom. Having said that, several studies have shown that tailoring instruction to learning styles does not lead to improved educational outcomes (see section "2. Empirical support for the meshing hypothesis"). This does raise an interesting question: given that matching teaching styles to learning styles has continuously been proven to be a fruitless endeavor (cf. see section "2. Empirical support for the meshing hypothesis"), why do so many individuals believe in this neuromyth? This question will be the focus of the next section.

5. Predictors of the endorsement of neuromyths and the meshing hypothesis

The great psychologist William James (1896) gave a lecture entitled *The will to believe* in 1896, in which he discussed the willingness to believe in the absence of evidence. The studies that have been reviewed here, have provided no evidence for the idea that meshing learning styles and instructional styles leads to improved educational outcomes for students, and yet most educators investigated appear to endorse this meshing hypothesis (see section "3. The ubiquity of the neuromyth of the veracity of the meshing hypothesis"). As stated multiple times, many reviews have been published that all suggest that this is the case. Following a review of all this evidence in the current paper, in this section, we will move beyond mere description and discuss *why* so many educators endorse the neuromyth of the meshing hypotheses.

Modern-day educators are strongly encouraged to learn about the brain and to incorporate neuroscientific knowledge into their teaching: for example, Goswami (2006) mentions that teachers indicated receiving over 70 emails a year about attending courses on educational neuroscience. Unfortunately, neuroscience has been shown to have a "seductive allure": Weisberg et al. (2008) demonstrated that explanations of psychological phenomena that were accompanied by *irrelevant* neuroscientific information were judged as more satisfying than explanations that did not include *irrelevant* neuroscientific information. Interestingly, this effect occurred both in participants without any neuroscience knowledge and in students who were taking a neuroscience course. Only neuroscience experts who were about to pursue, were currently pursuing, or had completed advanced degrees in cognitive neuroscience (or cognate fields) did not rate explanations of psychological phenomena that included irrelevant neuroscientific information as more satisfying. These observations, both the seductive allure of neuroscience and the encouragement of educators to incorporate educational neuroscience into their teaching, may contribute strongly to the common endorsement of the neuromyth of the veracity of the meshing hypothesis of learning styles. The problem may be compounded by the large amount of information that appears to support the meshing hypothesis that is available to educators: we will turn next to this issue.

Newton (2015) showed that a large body of literature that is indexed by the Education Resources Information Center (ERIC) either explicitly or implicitly endorses the utility of matching teaching styles to learning styles. It should be noted that the ERIC database also includes non-peer reviewed articles and that Newton intended to index what information was available to the "casual inquirer," who is simply looking to incorporate learning styles in their teaching and who may only consider the abstracts of the published articles, without taking their methodological rigor into account. He found that a query of the ERIC database yielded many articles that directly or indirectly endorse the meshing hypothesis: in fact, Newton reports that this database yielded about five times more articles of that nature than the PubMed database, which indexes mostly medical papers. Although this might suggest that users utilizing the ERIC database are exposed to more articles that endorse the meshing hypothesis, in a follow-up study, Newton et al. (2021) found that many articles indexed by medical databases also endorsed this neuromyth. It should be noted that although these papers may *appear* to endorse the utility of learning styles, only one of the 112 papers reviewed used the rigorous crossover interaction analysis described by Pashler et al. (2008) to evaluate the benefits of using learning styles to improve educational outcomes.

This work by Newton (2015) and Newton et al. (2021) reveals a serious problem: the casual reader can easily find a lot of literature that *appears* to endorse the veracity of the meshing hypothesis—if nothing else, then by sheer volume—but that does not actually use the imperative statistical test to ascertain whether those learning styles are beneficial for educational outcomes. Therefore, a casual reader might easily be swayed to believe that there is evidence for the meshing hypothesis, *while that is not actually the case* (cf. see section "2. Empirical support for the meshing hypothesis"). As an example, consider the work by Stander et al. (2019). Based on a review of the literature, these authors conclude that physiotherapists are characterized by certain learning styles, and that this observation can be used to construct "potentially effective training programs" for physiotherapy students (p. 8), which appears to endorse the use of learning styles in educational practice. However, in their Introduction, these authors state that, "It has been postulated that educators who recognize, understand, and respond to the learning styles of their students, assist optimal learning and retention of important concepts and information. *However, there is no clear correlation between learning styles and subsequent knowledge acquisition. Also, a preferred learning style does not imply the only way in which that individual learns*" (p. 2, emphasis added). This work by Stander et al. (2019) illustrates Newton (2015) and Newton et al. (2021) point: these authors *appear* to support the meshing hypothesis, but actually indicate that there is no empirical evidence for this hypothesis, which might confuse a casual reader. Given the confirmation bias that is present in many levels of human information processing (Nickerson, 1998), it is not unthinkable that an overwhelming amount of literature that *appears* to support the meshing hypothesis facilitates endorsement of the meshing hypothesis by educators worldwide.

It would be interesting to investigate whether there are any factors that can predict whether casual readers of the learning styles literature are more likely to fall prey to endorsing this neuromyth. We turn to this question next.

Dekker et al. (2012) did not just evaluate educators' beliefs in neuromyths, but they also tested their participants' knowledge of neuroscience. Interestingly, educators with *more* knowledge of the brain were also *more* likely to endorse neuromyths. Ferrero et al. (2016) reported a similar result. Other factors like age, gender, and reading popular science magazines or peer-reviewed science journals did not reliably predict a belief in neuromyths. Interestingly, Ferrero et al. (2016) also found that educators who identified as female were *more* likely to endorse neuromyths and that reading scientific journals *decreased* the belief in neuromyths, while the reading of educational magazines *increased* the belief in neuromyths. These two studies seem to suggest that having some knowledge of the brain may actual be detrimental, as it is associated with an increased belief in neuromyths. It should be emphasized that Dekker et al. (2012) and Ferrero et al. (2016) studied predictors of neuromyths in general, not specifically predictors for belief in the meshing hypothesis. However, belief in the veracity of the meshing hypothesis was among the neuromyths investigated by these authors.

What qualifies as "some" knowledge, though? In the study by Dekker et al. (2012) participants indicated whether they had an interest in "scientific knowledge about the brain and its influence on learning" and they demonstrated that interest by indicating the correctness of several statements about the brain. Ferrero et al. (2016) participants self-reported their interest in neuroscience, indicated whether they had received any in-service training in this field, read educational magazines or peer-reviewed journals and books, blogs, or websites on neuroscience. Although interesting, these variables may reflect relatively little and potentially unsystematic knowledge of neuroscience. Of the studies reviewed here, Macdonald et al. (2017) tested the group of participants with the highest level of neuroscientific knowledge: their participants with high neuroscience exposure had taken "many" courses on the brain and neuroscience at the college level. Even these "neuroscience experts" endorsed 46% of the neuromyths they were presented with (including a True/False statement on the veracity of the meshing hypothesis): however, bear in mind that these participants were *not* educators, but persons with degrees in fields like (social) science, medicine, and nursing.

6. Recommendations to end the widespread endorsement of the meshing hypothesis

Based on work reviewed in the previous section, it appears that having some knowledge of neuroscience may make persons more likely to endorse neuromyths in general. This phenomenon is reminiscent of the "recurring risk" mentioned by DeGrasse Tyson (2019): "knowing enough about a subject to think you are right, but not enough about the subject to know you are wrong." In this final section, I would like to present recommendations—based on the work reviewed above—to minimize the endorsement of neuromyths like the meshing hypothesis by educators.

Macdonald et al. (2017) reported that systematic neuroscientific education appears to provide some protection against the believe in neuromyths, but even armed with that knowledge, participants endorsed almost half of the neuromyths they were presented with—including the neuromyth of the veracity of the meshing hypothesis. This provides more evidence for the seductive allure of neuroscience that was described before: making something sound neuroscientific, for example by casting the classification of learning styles in terms of sensory processing in different brain areas, may make such a definition sound more appealing and convincing. This is likely to affect novice students of cognitive neuroscience. For example, a typical introductory course of cognitive neuroscience might cover vision, which is initially processed in the occipital lobes at the back of the brain, and hearing, which is processed by the temporal lobes at the sides of the brain. To someone inexperienced in neuroscience, such observations might be taken as support of distinct visual and auditory learning styles: after all, input from these sensory modalities is processed in different brain areas that are not even close together. More advanced students of neuroscience will become aware of multisensory neurons that respond to input from different sensory modalities and sensory integration areas (see, e.g., Wallace et al., 2004). In my—anecdotal—experience, students require a couple of undergraduate neuroscience courses before they fully grasp concepts like these, which, frankly, are very complicated.

Therefore, my first recommendation would be to offer more than one course in neuroscience to education students. As this may be difficult to implement in what are often clearly prescribed programs,[1] I would recommend including one *relevant* and *systematic* neuroscience course in the degree programs of education students. Relevant should be taken to mean: focused on what education students actually need. It would be hard to argue that knowledge of executive functions like working memory and planning ability and their underlying neuroscience is useless to prospective educators. However, many introductory neuroscience courses include a class on taste and smell, which might be less relevant to future educators. By eliminating topics that are less relevant, more time can be spent on information students can use in their careers. It might be better to focus on neuroscientific issues that are directly relevant to (aspiring) educators, for example by focusing on the so-called three *R*s: reading, writing, and arithmetic, and the neuroscience behind these cognitive processes (cf. Goswami, 2006). As a result, education students will build up more relevant neuroscience knowledge, which, hopefully, might facilitate distinguishing false information that seems plausible (or "alluring," to use the Weisberg et al., 2008, terminology) because it is accompanied by irrelevant neuroscientific information, or information that appears to be founded in neuroscientific principles. Such an approach might also mitigate the finding from Dekker et al. (2012) which suggests that having *more* knowledge of the brain predicted an endorsement of more neuromyths. One would hope that having more *relevant* knowledge of the brain would reduce educators' endorsement of neuromyths.

The reader will note the use of the words "hopefully" and "might" in the above paragraph. Whether such a proposed approach would be effective in reducing the endorsement of neuromyths like the meshing hypothesis is an empirical question that I intend to focus on in a future study. To make a less speculative suggestion: Dekker et al. (2012) have published a simple, 32-item true/false questionnaire about neuromyths which could easily be covered in a single class of any course on cognitive neuroscience. Such an approach would be expected to yield *systematic* neuroscientific knowledge in future educators. My third recommendation would be to retain instructors in cognitive neuroscience who are informed on neuromyths and who can explicitly dispel such myths with and for their students.

Finally, educational training might wish to encourage students to read peer-reviewed publications, which Ferrero et al. (2016) showed to be a protective factor in the endorsement of neuromyths (although note that Dekker et al., 2012 found that reading peer-reviewed literature was not a reliable predictor of the endorsement of neuromyths). Although it could be argued that this is an example of suggesting that correlation implies causation, it seems difficult to defend the position that reading peer-reviewed journals would be detrimental to the knowledge of the reader. Interestingly, Ferrero et al. (2016) did show that reading (presumably non-peer-reviewed) educational magazines *increased* the endorsement of neuromyths. Indeed, Dekker et al. (2012) found that knowledge of the brain was predicted by reading popular science magazines and, as stated above, that having more knowledge of the brain, in turn, was associated with the endorsement of more neuromyths. This makes sense in light of the abundance of (non-peer reviewed) literature that *appears* to support the meshing hypothesis as described in the work by Newton (2015) and Newton et al. (2021) and the confirmation bias discussed in section "5. Predictors of the endorsement of neuromyths and the meshing hypothesis." Training for educators might therefore seek to emphasize reading peer-reviewed journals over educational and popular science magazines, as the latter may appear to support the meshing hypothesis and other neuromyths.

7. Discussion, limitations, and implications

The studies reviewed here have not established an empirical basis for the meshing hypothesis, which proposes that matching instruction style to learning styles improves educational outcomes. However, among educators, the belief in several neuromyths, including the utility of matching teaching to learning styles, is ubiquitous. Unfortunately, interest in and knowledge of neuroscience were not necessarily protective factors against believing in neuromyths.

Some shortcomings in the studies reviewed should be mentioned. A belief in neuromyths like the meshing hypothesis does not necessarily pose an issue for educators, because believing in such myths does not imply that the teaching strategies like matching teaching styles to learning styles are implemented in the classroom. Having said that, some of the studies reviewed do suggest that at least a third of the educators who believe in the veracity of the meshing hypothesis *do* adjust their teaching style according to that belief (Newton and Miah, 2017). It would be worthwhile to follow up on this and include more questions about teaching practice in future studies. Clearly, the descriptive work has been done, but it would be interesting to establish a clearer, causal relationship between the belief in neuromyths, teaching practice, and educational outcomes. It would also be interesting to consider the dependent variables in such research: although drawing shapes after touching them while blindfolded (Krätzig and Arbuthnott, 2006) is indeed a valid way to test kinesthetic memory, this measurement does appear to be lacking ecological validity. After all, what aspect of any school curriculum would be represented by such a test?

It was interesting that Dekker et al. (2012) found no effect of gender on the belief in neuromyths, while Ferrero et al. (2016) reported that educators who identified as women were more likely to endorse neuromyths. Both studies included large samples ($N > 200$) and the percentage of female educators was at least roughly comparable in the two studies: 77% (UK) and 64% (Netherlands) in Dekker et al. (2012) and 72% (Spain) in Ferrero et al. (2016) Clearly the gender effect—or lack thereof—could be

1 It would obviously be unrealistic to expect prospective educators to take so many neuroscience courses that they become neuroscientists. This is compounded by the observation from Macdonald et al. (2017) that even neuroscience experts endorsed 46% of the neuromyths they were presented with. Those neuroscience experts were not educators who received the relevant and systematic course in neuroscience that is proposed here, though.

an artifact in one of the two datasets—but which one? Given the unequal gender distribution in the educational profession, as was reflected in both studies mentioned above, gender effects on the belief in neuromyths like the veracity of the meshing hypothesis might be worth exploring further.

A final question that remains, is where to go from here? Belief in the meshing hypothesis may or may not impact teaching practices (see section "4. The pragmatic effects of endorsing the meshing hypothesis and limitations in reviewed studies"), but there seems to be little ambiguity about the futility of adjusting teaching style to match students' learning styles. From that perspective, it would be beneficial to reduce or eliminate beliefs in neuromyths like veracity of the meshing hypothesis among educators. One simple reason to do so is to reduce the resources (time, energy, effort) that are wasted on adjusting teaching styles to meet the learning styles of various students: again, this practice has been repeatedly demonstrated to *not* lead to improved educational outcomes. Furthermore, there is a financial implication to trying to match teaching and learning styles. Not only may educators and institutions be wasting time and effort on assessing individual students' learning styles, but Pashler et al. (2008) quote, for example, a price of $1,225 for a summer training program in learning styles, and a $2,225 fee for a program to be certified to conduct research on learning styles by the International Learning Styles Network, whose website was not accessible at the time of writing this paper. It need not be emphasized that paying to be certified to do assessment to implement an idea that is not supported by empirical evidence is not a sensible use of funds. To add insult to injury, assessing students' learning styles has even been described as "time-consuming and boring to the students" (Al-Kadri, 2008).

As mentioned by Pashler et al. (2008), eliminating neuromyths like the veracity of the meshing hypothesis might also affect the locus of control in students, who now sometimes blame educators for not performing well. Instructors at the post-secondary level may be familiar with statements from students like, "I failed this test because you presented the material verbally and I am a visual learner." How could one blame students for falling prey to believing widely spread neuromyths like the veracity of the meshing hypothesis? The unfortunate consequence of such false beliefs is the use of neuromyths like the meshing hypothesis as terminal arguments for failing courses. I recall one student who dropped an obligatory course on statistics because he was "right-brained" and, insidiously, he was therefore convinced that this material would forever be beyond him, no matter how much effort he made (left-brained vs. right-brained dominance dictating academic strengths and weaknesses is another educational neuromyth; many of the studies reviewed here also assessed educators' endorsement of this neuromyth: see, e.g., Dekker et al., 2012; Macdonald et al., 2017; Grospietsch and Mayer, 2019).

Having some knowledge of neuroscience and the role of the brain in education does not appear to be a protective factor against the endorsement of neuromyths by educators. As indicated in section "5. Predictors of the endorsement of neuromyths and the meshing hypothesis" above, questions can be raised about what constitutes "neuroscientific knowledge" in educators. For example, reading popular scientific magazines—which likely cover seductively alluring neuroscientific topics—was not demonstrated to protect against a belief in neuromyths, while reading peer-reviewed journals might do so (at least in one study: Ferrero et al., 2016; it was not a protective factor in Dekker et al., 2012). As Goswami (2006) notes, educators are bombarded with trainings that claim to be grounded in neuroscience, but that may spread misinformation that people without a systematic or advanced background in neuroscience might find difficult to detect. To bridge this "gulf between neuroscience and the classroom" (Goswami, 2006), I would strongly recommend the incorporation of a *focused* course, which would yield more relevant and systematic knowledge of cognitive neuroscience, on educational neuroscience in educational curriculums. As has been discussed above, having general knowledge about neuroscience does not prevent educators from endorsing neuromyths. Fortunately, there seems to be a willingness in educators to integrate neuroscience and education (Pickering and Howard-Jones, 2007).

In conclusion, it should be emphasized again that although this paper has shown that there seems to be no reliable empirical evidence to suggest that matching teaching styles to learning styles has any effect on educational outcomes, it does *not* claim that presenting educational material in different sensory modalities is useless. As Macdonald et al. (2017) and other authors, including Pashler et al. (2008) point out, there is nothing wrong with presenting the same educational material to different sensory modalities, for example verbally *and* visually, or visually *and* auditorily; anecdotally, many educators appear to naturally do that. Doing so is likely to be beneficial to different students who *prefer* to receive information through different sensory modalities. Again, it is well established in neuroscience and in education that different students prefer to receive information in different sensory modalities, but that does not imply that those students have different "learning styles." If nothing else, the mere repetition of information that is presented to different sensory modalities may be helpful, because repetition has been shown to positively aid memorizing material (Macdonald et al., 2017; see also Wickelgren, 1981)—just do not confuse that concept with the existence of learning styles.

Author contributions

The author confirms being the sole contributor of this work and has approved it for publication.

Conflict of interest

The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

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