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META-ANALYSIS

Increasing Video Lecture Playback Speed Can Impair Test Performance – a Meta-Analysis

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Abstract
Increasing the playback speed of video lectures is popular amongst students as a time saving strategy, but does this negatively impact test performance? Here, we conducted a meta-analysis to examine the effect of increasing video lecture playback speed on content test performance. A meta-regression with robust variance estimation was used to aggregate data from 110 effect sizes, stemming from 24 studies of learning from lecture videos. The results demonstrated that increasing the playback speed of lectures can negatively impact content test performance, but this cost is small (and often non-significant) for speeds 1.5 x and slower. In addition, we found no evidence of moderation of this cost by a number of theoretically important variables (e.g., test type, lecture duration). These results contribute important insights into a popular study strategy and one that is likely to be a mainstay in educational settings for years to come.

Keywords Playback speed · Video lectures · Time-compression · Increased speed · Online Learning · Words per minute · Online lectures · Learning strategy

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Introduction

As technology advances, new forms of instruction emerge. In the Internet Age this has included the introduction of massive open online courses (MOOCs), often provided for free, and many traditional education institutions (e.g., Universities) offering online or partially online (i.e., blended learning) courses (Gorissen et al., 2012; Schaffhauser, 2016). When courses are delivered online, one of the primary means of communicating to-be-learned content is through video or recorded lectures. Even courses offered through in-person modes of delivery may provide video lectures as supplemental learning materials, a resource for students to catch up on missed lectures, or as a part of flipped classrooms where students engage with learning material outside of the classroom (Gorissen et al., 2012; Pastore & Ritzhaupt, 2015). Video lectures provided online have numerous advantages over traditional in-person lectures as they can, for example, be viewed at any time, can be viewed from anywhere with an Internet connection, and can be rewatched. In addition, learners are provided with a level of control over the content not afforded by traditional in-person lectures. In the present investigation we focus on one dimension of that control, namely, the option to alter the playback speed of the video.

Many video players allow learners to select a faster or slower speed to view the lecture depending on their needs, usually by selecting a multiple of the default speed (0.75x, 1.25x, 2x etc.). In a recent survey of Canadian undergraduate students who had taken courses with video lectures, a large majority (89%) reported using these tools to alter the playback speed of lectures, of which 96% choose to increase the playback speed (Tharumalingam & Risko, 2025). The most frequent reason for increasing playback speed is that it enables students to save time, and the most commonly selected speeds were 1.25x and 1.5x (Tharumalingam & Risko, 2025; see also Jacobson et al., 2018 and Zureick et al., 2018). A related potential advantage of increasing the playback speed of a video lecture is that it reduces the duration of the lecture and as a result may support sustained engagement (Lang et al., 2020). Many studies have demonstrated that time viewing the lecture is inversely associated with engagement and directly associated with mind-wandering (Guo et al., 2014; Ozan & Ozarslan, 2016; Risko et al., 2012; Wilson & Korn, 2007). Murphy et al. (2023) found some evidence that increasing playback speed reduces mind wandering in one experiment, while a subsequent experiment did not find this association. Wilson et al. (2018) found that increased speed had no impact on mind wandering.

Provided many learners are choosing to increase the playback speed of video lectures, it would be valuable to know what impact this decision has on learning lecture material. On the one hand, it seems intuitive that increasing the rate of information presentation (i.e., increasing playback speed) will increase cognitive demand as learners need to process information more quickly (Mo et al., 2022), leading to learning costs. For example, in studying lists of words to be recalled later, performance decreases as presentation rate increases (e.g., Bernbach, 1975; Glanzer & Cunitz, 1966). That said, early research demonstrated that human speech can be comprehended at a rate much faster than the normal speaking rate (Barron, 2004). Thus, Cheng et al., (2021; see also Ritzhaupt et al., 2008) argued that in typical lectures there might be unused processing capacity that could allow for at least moderate increases in cognitive demands without negatively impacting learning. Speeded content could also be argued to be more stimulating and hence increase engagement (Ilie & Thompson, 2006). Lastly, video lectures are often multimedia presentations that include both narration and visual information (i.e., text or images on slides). The presence of multiple channels alone may serve to ameliorate negative impacts of increasing playback speed. For example, bullet points on a slide that mirror spoken information might function as a kind of backup if the spoken information is missed due to the increased playback speed.

When researchers have examined the impact of increasing the playback speed of video lectures on test performance, results have been mixed with numerous studies demonstrating costs (e.g., Edmiston, 1986; Pastore, 2012; Song et al., 2018), or lack thereof (e.g., Kiyak et al., 2023; Nagahama & Morita, 2017a; Ritzhaupt et al., 2011), and some even demonstrating benefits (e.g., Mo et al., 2022; Murphy et al., 2023; Nagahama & Morita, 2018), even at the same playback speed. These contradictory findings may be the result of differing methodologies between studies such as differences in lecture topics, video formats, difficulty, or test formats. Against this background, a meta-analytic approach would be valuable to quantify the influence of speeding lectures on learning with a higher degree of certainty, to quantify the degree of heterogeneity in prior work, and to evaluate the influence of potential moderators which could account for the contradictory findings of previous studies. Along these lines, a recent meta-analysis of 7 studies reported by Cheng et al. (2021) found that increasing playback speed by 1.4x-1.5x was associated with a small ($g = -0.21$) cost in learning, and speeding by 1.8x-2x was associated with a slightly larger ($g = -0.36$) reduction in learning. While this meta-analysis provided important insights, it fell short on three critical fronts: (1) it did not include several relevant articles that were published at the time, (2) their statistical power was limited given only 10 effect sizes were included, and (3) they did not investigate potential moderating variables. Furthermore, while not a limitation of Cheng et al. (2021), the pace of research on increasing the playback speed of video lectures has clearly increased, with several articles published since their meta-analysis. With the growing availability of online courses (Ness et al., 2021), and the increasing popularity of flipped classrooms (Song & Kapur, 2017), it seems reasonable to suggest that the use of video lectures will increase in post-secondary education. As such, understanding the impact of playback speed on learning will be critical for the development of effective learning tools that keep students engaged and facilitate understanding of the material. As such, we build on the work by Cheng et al. (2021) here by conducting a more comprehensive meta-analysis that includes consideration of potential factors moderating the effect of playback speed on subsequent test performance.

In the present investigation, we focus on studies which have investigated the effect of increasing playback speed by some multiple of the original speed (the standard way this factor is manipulated) and its subsequent impact on learning the lecture content (i.e., a content test; some post-lecture measure of how well individuals learned the information in the lecture). In a secondary meta-analysis, we consider theoretically relevant moderators that could be extracted from most published studies. The first of these potential moderators is the format of the content test that participants had to complete following the learning phase. There was wide variation in the nature of the tests used across studies. We were able to provide a broad grouping into three levels: multiple choice, recall, and combined (i.e. tests which combined multiple choice and recall). Multiple choice tests require recognizing the correct answer amongst alternatives and as such might be argued to put less demands on memory than recall tests that provide less environmental support for retrieval (Lindenberger & Mayr, 2014; Ritzhaupt et al., 2011). Thus, costs of speeding might be greater in the case of recall tests and intermediate in cases that combine recall and multiple choice (i.e., the 'combined' category) relative to multiple choice tests. The second moderator we examined was the duration of the lecture. If speeding increases cognitive demands, then one might expect that this increased cognitive load would become more difficult to maintain as lectures increase in duration. Thus, the costs of speeding might be greater for longer lectures. Lastly, we considered several relevant study parameters including subject of the lecture, participant age, lecture language, publication year, and words per minute of the base lecture as moderators.

In the current investigation, we focused on factors thought to be most critical to online learning. This focus, of course, leaves open several interesting questions: How might controlling subject difficulty or prior knowledge impact the effect of increasing playback speed? How does increasing playback speed influence other aspects of the learning experience such as metacognitive accuracy, affect, and motivation? We are not neglecting these questions; rather, the literature has either not addressed them systematically yet or, to the extent that it has, it is arguably too early to start applying meta-analytic tools (i.e., there are not enough studies). However, the literature is clearly in a place where we can provide a comprehensive answer to the question of how increasing playback speed of video lectures impacts test performance. As this area of research grows rapidly, the current meta-analysis will provide guidance for future work by illuminating critical issues to consider and by offering an overview of research conducted to-date.

Methods

The data, analysis code, and pre-registration for this study are available on the Open Science Framework (OSF; https://osf.io/zj62u/). Developments during the meta-analysis led to deviations from the pre-registration. We note when these deviations occur.

Study Search, Exclusion, and Inclusion Criteria

A literature search was conducted using the University of Waterloo's Omni library catalogue and Google Scholar. We also mined reference lists of relevant papers to find additional studies. Omni searches across 432 databases including APA

PsychArticles, Biomed Central, JSTOR, PsycINFO, and Pubmed, while Google Scholar also searches across a wide array of databases. The following Boolean search string was used to cover all the potential terms used for playback speed alteration and content test performance: 'playback speed OR video lectures OR altered speed OR speeded lectures OR slowed lectures OR time-compressed OR accelerated playback AND comprehension OR recall OR performance'. The last three search terms were not included in the pre-registration but were found to be useful for finding relevant material. To find unpublished reports, we emailed the first author of each report retrieved through the initial search to inquire into whether they possessed any unpublished research pertaining to the subject of video lecture playback speed. Three researchers replied and one had relevant unpublished material. Therefore, this process yielded one additional report. We also included yet-to-be-published studies from our own lab.

We included studies which attempted to understand the effect of increasing playback speed on content test performance. The basic experimental structure of studies deemed suitable for inclusion was the following: A video lecture shown to a control group at the default speed (i.e., 1.00x speed) and the same lecture shown at an accelerated rate to an experimental group. Both groups were given identical content tests, and the results were compared. The difference between the standard speed and speeded condition on the latter outcome measure is the effect that is the focus of this meta-analysis. While we pre-registered including studies with slowing (e.g., 0.75x), given the small number of studies ($m = 1$) with these conditions we decided to focus solely on speeding.

Our selection criteria excluded within-subject studies because there was only one and the within-group standardized mean is a different effect size metric (Harrer et al., 2021), necessitating conversion to a common effect size metric which is undesirable due to potential biases which may arise (Valentine & Pigott, 2020). In some cases, studies with a within-subject design were included if an analysis was reported that analyzed speed as a between-subjects factor as well. One study was removed because it involved many lectures across an extended period and participants could control the speed after assignment to condition. In addition, we focused on video lectures that included a visual component which was relevant to the content. We did this because video lectures often include slides or graphics which accompany the lecturer's spoken delivery of the material, and this content speeded includes both an increase in the speaking pace and in the presentation rate of the images moving on the screen. This decision therefore led to the exclusion of studies making use of audio-only recordings of lectures (see PRISMA diagram below). There were no restrictions set based on age of participants, year of publication, nor the language of the publication.

Search Results

Figure 1 displays a PRISMA flow chart which details our study search and filtering steps. From this process, 24 studies were found to be suitable for inclusion. Most studies contributed more than one effect size. The total number of effect sizes

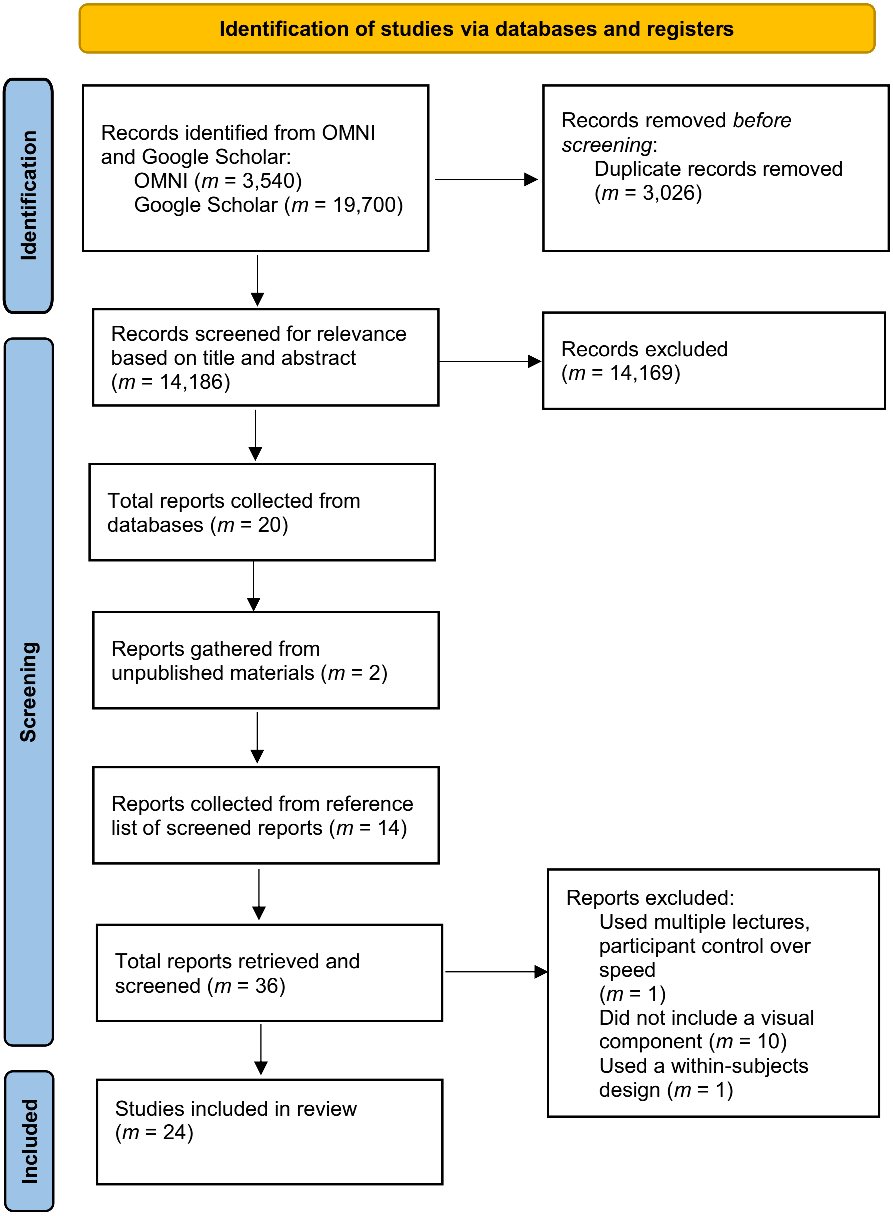

Fig. 1 PRISMA flow diagram of the literature search

included was 110, with an average of 4 effects per study and ranging from 1–12 effect sizes per study. Studies came from the United States, Canada, Turkey, China, and Japan. They were conducted in English, Turkish, Mandarin, and Japanese, and were published between 1971–2023. Most studies (m = 19) were conducted in-person with university undergraduates, but one study was conducted online with university undergraduates and four were conducted online with participants from online participant pools such as Prolific and Mechanical Turk. Participant ages ranged from 18–72 and lecture length ranged from 5 to 30 min.

Because we chose to focus on video lectures that were speeded by a particular factor, this also excluded any studies where content was speeded by having the lecturer speak at an accelerated pace. For example, Simonds et al. (2006) conducted an experiment which varied speaking rate across three conditions by having the same lecturer speak at three different rates.

For each eligible study, we collected information regarding potential moderators including the speed used (in multiples of the original speed), format of the test (divided into multiple choice, recall, and combined), duration of the video lecture (in minutes), age of participants, language of the learned materials, publication year, lecture subject, and the words per minute (WPM) of the base lecture. Lecture subjects were divided into life sciences, social sciences, engineering, math, and language. Table 1 lists the characteristics of each study.

Data from the selected papers was initially extracted by TT and reviewed by EFR. An independent coder then replicated the extraction of all data from the studies included in the review to ensure the accuracy of the data that was extracted. After adjusting for minor differences in precision, there was a high level of agreement between the datasets extracted by both coders. With respect to effect sizes (which required agreement on the means, standard deviations, and sample sizes for control and experimental conditions), the coders initially agreed on 72% of the cases. With respect to moderators, agreement ranged from 74 to 100% with most over 90%. The lowest agreement was with the topic moderator where we attempted to convert subjects into broader topics. In many cases this was ambiguous. After consultation, all disagreements were resolved.

Statistical Approach

We used Hedges' *g* as a metric of the standardized mean difference for each study. According to Harrer et al. (2021), Hedges' *g* is a preferable standardized mean metric over Cohen's *d* when sample sizes are small, as Cohen's *d* tends to have an upward bias with small sample sizes. Group sample sizes in the included studies ranged from $n=6$ to $n=80$. Using the mean, standard deviation, and sample size of each group, we employed the *meta* package (Balduzzi et al., 2019) for *R* (version 2.2.4) to compute Hedges' *g* as well as variances for the calculated effect sizes.

Between-study heterogeneity was tested with Cochran's Q and was quantified by calculating 95% prediction intervals. If Q is equal to or smaller than the degrees of freedom, then it indicates the degree of heterogeneity observed is equal to or less than that expected based on within-study variation alone, and if Q is larger than the degrees of freedom, then this is an indication that there is greater heterogeneity than expected based on within-study variation alone (Cochran, 1954). The p value associated with the test indicates whether the difference is significant. Providing the prediction intervals for the model quantifies the range of "true" effects expected in a new study; at the time of writing, this metric is considered the current "gold-standard" with respect to

Table 1 Summary data from each eligible study included in the meta-analysis

Authors (Year)	Speeds Tested	*k*	Lecture Subject	Lecture Category	Test Format	Lecture Duration (Minutes)	Lecture Words per Minute
Ashburner et al. (submitted)	1.5x, 2x	2	Psychology, English, Economics	Social Sciences	Multiple Choice and Recall	10	136
Benz (1971)	1.5x, 2x	2	Geography	Social Sciences	Multiple Choice	30	162
Chen et al. (2024)	2x	5	History, Real Estate Appraisals	Social Sciences	Multiple Choice	27.38	162, 166
Edmiston (1986)	1.25x	1	Psychology	Social Sciences	Multiple Choice	20	Unknown
Jacobson et al. (2018)	2x, 3x	2	Unknown	Unknown	Multiple Choice	9	179
Kiyak et al. (2023)	2x	1	Neoplasia	Life Sciences	Multiple Choice	10.51	122
Mo et al. (2022)	1.25x, 1.5x, 2x	3	Computer Science	Engineering	Multiple Choice	16	210
Murphy et al. (2022)	1.5x, 2x, 2.5x	3	History, Real Estate Appraisals	Social Sciences	Multiple Choice	12.93, 14.45	157, 166
Murphy et al. (2022)	1.5x, 2x, 2.5x	3	History, Real Estate Appraisals	Social Sciences	Multiple Choice	12.93, 14.45	157, 166
Murphy et al. (2023)	2x	8	Real Estate Appraisals	Social Sciences	Multiple Choice	12.96	157, 166
Nagahama & Morita (2017a)	1.5x, 2x	4	Humanities	Social Sciences	Multiple Choice	9	164, 168
Nagahama & Morita (2017b)	1.5x, 2x	4	Network Infrastructure	Engineering	Multiple Choice	9.2	168
Nagahama & Morita (2018)	1.5x, 2x	8	Network Infrastructure	Engineering	Combined	9.2	168
Nagahama et al. (2019)	2x	4	Network Infrastructure	Engineering	Combined	9.2	168
Pastore (2010)	1.5x, 2x	8	Heart Physiology	Life Sciences	Multiple Choice and Recall	12.16	164
Pastore (2012)	1.5x, 2x	8	Heart Physiology	Life Sciences	Multiple Choice	12.16	164
Perez et al. (2018)	2x	1	Nutrition	Life Sciences	Recall	6.79	Unknown
Risko et al. (2024)	1.5x	4	Statistics	Math	Recall	10	172
Ritzhaupt et al. (2008)	1.4x, 1.8x	2	Education	Social Sciences	Multiple Choice	20.9	150
Ritzhaupt et al. (2008)	1.4x, 1.8x	2	Education	Social Sciences	Multiple Choice	20.9	150
Ritzhaupt et al. (2011)	1.5x, 2x, 2.5x	12	Geography	Social Sciences	Multiple Choice and Recall	10	150
Ritzhaupt et al. (2015)	1.25x, 1.5x	4	Navigating the Financial Aid System	Social Sciences	Multiple Choice	26.53	150
Ritzhaupt et al. (2015)	1.25x, 1.5x	4	Navigating the Financial Aid System	Social Sciences	Multiple Choice	26.53	150
Song et al. (2018)	1.5x	2	Ultrasonography	Engineering	Multiple Choice	12, 15	Unknown

Table 1 (continued)

Authors (Year)	Speeds Tested	*k*	Lecture Subject	Lecture Category	Test Format	Lecture Duration (Minutes)	Lecture Words per Minute
Tran et al. (2024)	1.25x, 1.5x, 1.75x, 2x, 2.5x, 3x	12	Psychology	Social Sciences	Multiple Choice and Recall	10	158
Wilson et al. (2018)	1.6x, 1.7x	4	Psychology	Social Sciences	Recall	11.16, 11.8	170

Note. *k* is the number of effect sizes contributed by the study. Combined test format refers to tests that combined multiple choice and recall the quantification of heterogeneity and is preferred over outdated metrics such as I^2, which are no longer recommended for reporting (Borenstein, 2023).

It is important to note that in the reported meta-analysis, numerous studies contributed more than one effect size. There were several reasons for this, including multiple experiments being reported in the paper, experiments that tested multiple speeds (e.g., 1.5x and 2x), or the use of multiple test formats conducted on the same participants (e.g., separate recall and multiple-choice tests). For example, the study by Ritzhaupt and Barron (2008) tested 1.5x, 2x, and 2.5x playback speeds, and featured both recall and multiple-choice tests, leading to a total of 6 effect sizes of interest. The fact that a single study may contribute several effect sizes has important implications for our meta-analysis. According to Hedges et al. (2010), effect sizes cannot be considered independent if they were collected from the same publication. This is because many effect sizes may be computed from the same control group and multiple effect sizes may be derived from the same participants if multiple tests were used. It is also possible that involvement of the same researchers, the same sample of participants, or use of the same operationalization of the dependent variable will also lead to dependent effect sizes.

Various methods have been developed to deal with such dependencies among effect sizes. One approach is multi-level meta-analysis (Harrer et al., 2021). This technique allows for the ability to cluster dependent effect sizes based on the study from which they came. When ignoring these dependencies and treating effect sizes as independent, heterogeneity may be artificially lowered, which could lead to bias. Therefore, a multi-level analysis with study-clustered effect sizes provides a more accurate sense of heterogeneity. Another approach for dealing with dependency is robust variance estimation (RVE). This method allows one to correct for dependencies among effect sizes without the need for full specification of the nature of these dependencies (Hedges et al., 2010). It does so by providing an estimator of the covariance matrix of meta-regression coefficients which can be used when there are clusters of dependent estimates, as was the case here (Hedges et al., 2010; Pustejovsky & Tipton, 2021). Pustejovsky and Tipton (2021) developed a meta-regression model specification which incorporates both multi-level analysis and RVE. We therefore combined multi-level analysis and RVE to account for dependent effect size estimates.

RVE assumes either a hierarchical or correlated effects model. The hierarchical model is used when dependencies arise solely from the fact that the same researchers are conducting the study and likely using similar techniques for each sample. The correlated model assumes that dependence arises from the fact that several effect sizes are derived from the same sample (Pustejovsky & Tipton, 2021). Since both situations applied to the collected data, a combined hierarchical-correlated model will likely better capture the true nature of the dependencies amongst effect sizes in our analysis. The *clubSandwich* (version 0.5.8; Pustejovsky, 2022) and *metafor* (version 4.0.0; Viechtbauer, 2010) packages for *R* allow us to use a combined correlated and hierarchical effects model, as recommended by Pustejovsky and Tipton (2021).

Here, we used three-level meta-regression with RVE random effects models.[1] First, using the *clubSandwich* package in *R*, we imputed a variance–covariance

[1] The pattern of results were similar when using a multi-level model without RVE.

matrix that models effect size dependencies and therefore provides the RVE component of the analysis. We specified the cluster which associated dependent effect sizes with each other; in our case, effect sizes were clustered at the publication level. We also assumed a constant correlation rho of 0.8 (see Fisher & Tipton, 2015) and conducted a sensitivity analysis to examine whether changing the values of rho significantly altered the results. Our sensitivity analysis showed that using lower values of rho produced models with much lower heterogeneity, however, the qualitative patterns remained the same. As such, we used the more conservative (higher) value. Second, using the *metafor* package in *R* allowed us to employ the variance–covariance matrix when conducting a multi-level analysis, and thus enabled the combination of RVE and multi-level analysis.

While speed is a continuous variable, it is most often treated as categorical in the literature. For example, in the meta-analysis by Cheng et al. (2021), speeds were treated categorically; specifically, speeds were grouped into 1.4x-1.5x and 1.8x-2x. We decided to report two sets of analyses to address this issue while staying consistent with how speed is most commonly treated in the literature. In the first, we treat speed as categorical and focus on the most popular speeds studied in the literature: 1.25x, 1.5x, 2x, 2.5x. Effect sizes with these speeds were pooled together and used to generate a meta-regression while treating each speed categorically. In the second analysis, which was not pre-registered, we treated speed as a continuous predictor. The latter allows us to include all studies in a single meta-regression, enabling us to include all of the data points in a single analysis. In addition, it allows for consideration of a non-linear effect of increasing playback speed on test performance. To examine the possibility of a non-linear relation between playback speed and test performance, we also included a non-linear model. Lastly, we assessed the effects of other potential moderators to provide the most powerful test of their influence since all effect sizes would be included in the model as opposed to assessing moderator effects separately for each speed (e.g., 1.25x, 1.5x, 2x, 2.5x).

Results

Primary Analyses

First, we constructed a multi-level meta-regression model without any moderators which compared non-speeded lectures to speeded lectures without accounting for the exact speed used (i.e., an intercept-only model). This provides an estimate of the pooled effect size across all the studies included in the meta-analysis (ignoring how much the lecture was speeded in the experimental condition, the duration of the lecture, etc.). Using multi-level analysis and RVE, the pooled effect size was $g = -0.28$, *95% CI* [−0.40, −0.16], *95% PI* [−0.97, 0.41], $z = 4.57$, $p < 0.001$, and there was considerable heterogeneity, $Q(109) = 937$, $p < 0.001$. Of note, the fact that the prediction interval crosses 0 suggests that there are some cases where the "true" difference between the control and experimental groups is negligible or even reversed. However, given that this estimate ignores how much the lecture is speeded, it is of limited value as a point estimate. It indicates that across all the studies included (ignoring the amount of speeding in the study) speeding led to a cost and that there is a moderate amount of heterogeneity across these studies. Figure 2 shows a forest plot with playback speeds from 1.25x to 1.8x, and Fig. 3 shows a forest plot with playback speeds from 2x to 3x. Positive values (right of the line) indicate better test performance at that speed and negative values (left of the line) indicate worse test performance at that speed.

To test for publication bias we used the "Egger Sandwich" analysis method (Rodgers & Pustejovsky, 2021), which is a typical Egger's regression test of publication bias that accounts for RVE-compatible data structures. According to Rodgers and Pustejovsky (2021), if the result of the test differs significantly from zero this indicates the presence of publication bias. The result obtained from running Egger's regression test on our data was significant, $B = -0.38$, $p = 0.02$, suggesting evidence consistent with publication bias. To visualize publication bias, we created a cluster-robust significance funnel plot (Fig. 4) which, like classic funnel plots, plot Hedges' g of each effect size on the x-axis and the standard error of each effect size on the y-axis (Mathur & VanderWeele, 2020). These plots also include a line which shows where studies with exactly $p = 0.05$ would lie. Studies to the left of the line represent "affirmative" results supporting the existence of a significant effect (negative in this case) and studies to the right represent "non-affirmative" studies. The extent of asymmetry in this plot suggests

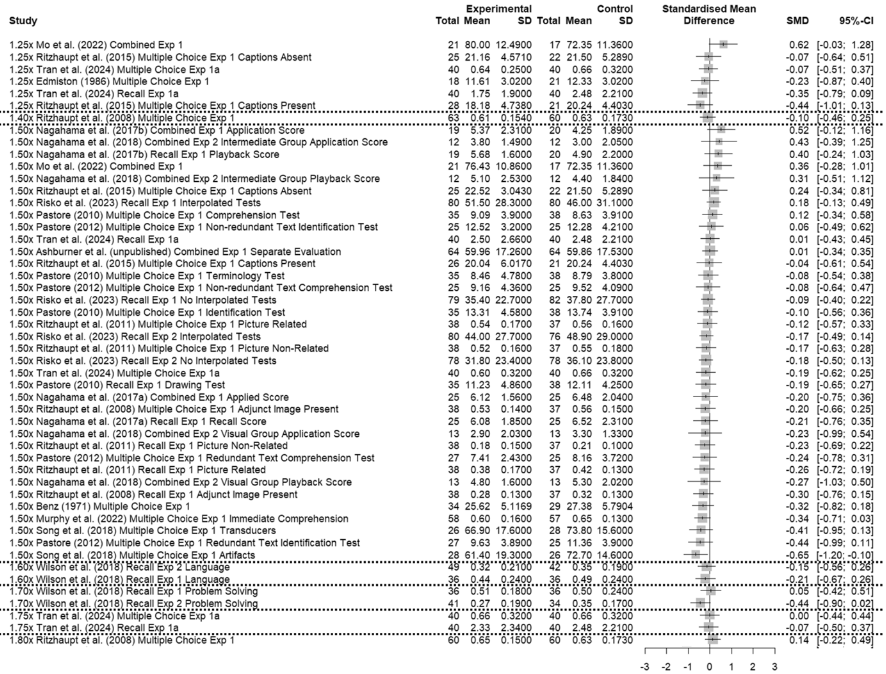

Fig. 2 Forest plot of effect sizes from 1.25x to 1.8. SMD = Standardized mean difference. Gray squares vary in size according to the weight contributed by that effect size. Dotted lines used to separate speeds

Study	Experimental Total	Experimental Mean	Experimental SD	Control Total	Control Mean	Control SD	SMD	95%-CI
2.00x Nagahama et al. (2018) Combined Exp 2 Intermediate Group Application Score	12	3.90	1.9700	12	3.00	2.0500	0.43	[-0.39; 1.25]
2.00x Murphy et al. (2023) Multiple Choice Exp 3 Younger Adults with Mind Wandering Probes	47	0.57	0.1900	41	0.50	0.1800	0.37	[-0.05; 0.80]
2.00x Nagahama et al. (2018) Combined Exp 2 Visual Group Application Score	13	3.80	1.3900	13	3.30	1.3300	0.36	[-0.41; 1.12]
2.00x Nagahama et al. (2017b) Combined Exp 1 Application Score	20	4.85	1.3900	20	4.25	1.8900	0.35	[-0.27; 0.98]
2.00x Murphy et al. (2023) Multiple Choice Exp 1 Younger Adults	49	0.64	0.1600	49	0.61	0.1700	0.18	[-0.22; 0.58]
2.00x Murphy et al. (2023) Multiple Choice Exp 2 Younger Adults with Mind Wandering Probes	50	0.60	0.1700	51	0.57	0.1700	0.18	[-0.22; 0.57]
2.00x Nagahama et al. (2017b) Recall Exp 1 Playback Score	20	5.05	1.9100	20	4.90	2.2000	0.07	[-0.55; 0.69]
2.00x Nagahama et al. (2017a) Combined Exp 1 Applied Score	25	6.60	2.2400	25	6.48	2.0400	0.06	[-0.50; 0.61]
2.00x Ritzhaupt et al. (2011) Recall Exp 1 Picture Non-Related	38	0.21	0.1200	37	0.21	0.1000	0.00	[-0.45; 0.45]
2.00x Ritzhaupt et al. (2011) Multiple Choice Exp 1 Picture Non-Related	38	0.54	0.1600	37	0.55	0.1800	-0.06	[-0.51; 0.39]
2.00x Ritzhaupt et al. (2011) Multiple Choice Exp 1 Picture Related	38	0.54	0.1800	37	0.56	0.1600	-0.12	[-0.57; 0.34]
2.00x Chen et al. (2024) Multiple Choice Exp 4 Distractions Absent	56	0.51	0.1700	56	0.53	0.1600	-0.12	[-0.49; 0.25]
2.00x Ritzhaupt et al. (2008) Multiple Choice Exp 1 Adjunct Image Present	38	0.54	0.1500	37	0.56	0.1500	-0.13	[-0.59; 0.32]
2.00x Nagahama et al. (2019) Recall Exp 1 Normal Speech Recall Score	10	4.70	1.9000	10	5.00	1.7000	-0.16	[-1.04; 0.72]
2.00x Ashburner et al. (unpublished) Combined Exp 1 Separate Evaluation	71	62.06	19.4900	71	65.40	19.7900	-0.17	[-0.50; 0.16]
2.00x Chen et al. (2024) Multiple Choice Exp 3 Textual Presentation	55	0.53	0.1900	55	0.57	0.2100	-0.20	[-0.57; 0.18]
2.00x Nagahama et al. (2018) Combined Exp 2 Intermediate Group Playback Score	12	3.90	2.4900	12	4.40	1.8400	-0.22	[-1.03; 0.59]
2.00x Murphy et al. (2022) Multiple Choice Exp 1 Immediate Comprehension	59	0.62	0.1400	57	0.65	0.1300	-0.22	[-0.59; 0.14]
2.00x Nagahama et al. (2018) Combined Exp 2 Visual Group Playback Score	13	4.80	2.0500	13	5.30	2.0200	-0.24	[-1.00; 0.52]
2.00x Pastore (2010) Multiple Choice Exp 1 Comprehension Test	35	7.69	3.1800	38	8.63	3.9100	-0.26	[-0.72; 0.20]
2.00x Ritzhaupt et al. (2008) Recall Exp 1 Adjunct Image Present	38	0.28	0.1400	37	0.32	0.1300	-0.29	[-0.75; 0.16]
2.00x Kıyak et al. (2023) Multiple Choice Exp 1 Immediate	30	10.16	2.4600	30	11.26	4.0600	-0.32	[-0.83; 0.19]
2.00x Murphy et al. (2023) Multiple Choice Exp 1 Older Adults	53	0.59	0.1500	58	0.64	0.1400	-0.34	[-0.72; 0.03]
2.00x Pastore (2010) Multiple Choice Exp 1 Terminology Test	35	7.54	2.9500	38	8.79	3.8000	-0.36	[-0.82; 0.10]
2.00x Pastore (2012) Multiple Choice Exp 1 Non-redundant Text Identification Test	27	10.63	4.6400	25	12.28	4.2100	-0.37	[-0.91; 0.18]
2.00x Chen et al. (2024) Multiple Choice Exp 4 Distractions Present	57	0.47	0.1400	53	0.53	0.1600	-0.40	[-0.78; -0.02]
2.00x Murphy et al. (2023) Multiple Choice Exp 2 Older Adults with Mind Wandering Probes	51	0.57	0.1300	71	0.63	0.1500	-0.42	[-0.78; -0.06]
2.00x Tran et al. (2024) Multiple Choice Exp 1a	40	0.52	0.3200	40	0.66	0.3200	-0.43	[-0.88; 0.01]
2.00x Chen et al. (2024) Multiple Choice Exp 2	50	0.53	0.1400	50	0.60	0.1700	-0.45	[-0.84; -0.05]
2.00x Murphy et al. (2023) Multiple Choice Exp 3 Younger Adults no Mind Wandering Probes	43	0.47	0.1700	42	0.55	0.1800	-0.45	[-0.88; -0.02]
2.00x Ritzhaupt et al. (2011) Recall Exp 1 Picture Related	38	0.35	0.1700	37	0.42	0.1300	-0.46	[-0.92; 0.00]
2.00x Nagahama et al. (2017a) Recall Exp 1 Recall Score	25	5.32	2.7600	25	6.52	2.3100	-0.46	[-1.03; 0.10]
2.00x Tran et al. (2024) Recall Exp 1a	40	1.55	1.7100	40	2.48	2.2100	-0.47	[-0.91; -0.02]
2.00x Perez et al. (2018) Multiple Choice Exp 1	66	6.77	2.1412	77	7.94	2.7502	-0.47	[-0.80; -0.14]
2.00x Mo et al. (2022) Combined Exp 1	17	66.47	12.3800	17	72.35	11.3600	-0.48	[-1.17; 0.20]
2.00x Chen et al. (2024) Multiple Choice Exp 3 Pictorial Presentation	54	0.49	0.1600	54	0.58	0.1800	-0.52	[-0.91; -0.14]
2.00x Nagahama et al. (2019) Recall Exp 1 Synthetic Speech Recall Score	10	4.90	1.8000	10	6.00	2.1000	-0.54	[-1.43; 0.36]
2.00x Murphy et al. (2023) Multiple Choice Exp 3 Older Adults no Mind Wandering Probes	51	0.51	0.1800	48	0.61	0.1700	-0.57	[-0.97; -0.16]
2.00x Jacobson et al. (2018) Multiple Choice Exp 1 Trial 2	16	35.00	14.1600	16	44.38	17.1200	-0.58	[-1.29; 0.13]
2.00x Pastore (2010) Recall Exp 1 Drawing Test	35	8.51	5.6100	38	12.11	4.2500	-0.72	[-1.19; -0.25]
2.00x Pastore (2012) Multiple Choice Exp 1 Redundant Text Comprehension Test	25	5.52	2.2200	25	8.16	3.7200	-0.85	[-1.43; -0.27]
2.00x Pastore (2012) Multiple Choice Exp 1 Non-redundant Text Comprehension Test	27	6.48	2.8800	25	9.52	4.0900	-0.85	[-1.42; -0.28]
2.00x Pastore (2010) Multiple Choice Exp 1 Identification Test	35	10.09	4.2900	38	13.74	3.9100	-0.88	[-1.36; -0.40]
2.00x Murphy et al. (2023) Multiple Choice Exp 3 Older Adults with Mind Wandering Probes	51	0.49	0.1900	49	0.67	0.1700	-0.99	[-1.41; -0.57]
2.00x Nagahama et al. (2019) Combined Exp 1 Synthetic Speech Applied Score	10	1.80	1.0000	10	3.00	1.3000	-0.99	[-1.93; -0.05]
2.00x Benz (1971) Multiple Choice Exp 1	37	19.86	5.3549	29	27.38	5.7904	-1.34	[-1.88; -0.80]
2.00x Pastore (2012) Multiple Choice Exp 1 Redundant Text Identification Test	25	6.48	2.6100	25	11.36	3.9000	-1.45	[-2.08; -0.82]
2.00x Nagahama et al. (2019) Combined Exp 1 Normal Speech Applied Score	10	2.00	1.2000	10	4.40	0.9000	-2.17	[-3.32; -1.02]
2.50x Tran et al. (2024) Multiple Choice Exp 1b	40	0.54	0.2500	40	0.71	0.2500	-0.67	[-1.12; -0.22]
2.50x Murphy et al. (2022) Multiple Choice Exp 1 Immediate Comprehension	57	0.55	0.1500	57	0.65	0.1300	-0.71	[-1.09; -0.33]
2.50x Ritzhaupt et al. (2011) Multiple Choice Exp 1 Picture Related	40	0.44	0.1700	37	0.56	0.1600	-0.72	[-1.18; -0.26]
2.50x Ritzhaupt et al. (2011) Multiple Choice Exp 1 Picture Non-Related	40	0.42	0.1500	37	0.55	0.1800	-0.78	[-1.24; -0.32]
2.50x Tran et al. (2024) Recall Exp 1b	40	1.18	2.2100	40	3.23	2.7200	-0.82	[-1.28; -0.36]
2.50x Ritzhaupt et al. (2008) Multiple Choice Exp 1 Adjunct Image Present	40	0.43	0.1300	37	0.56	0.1500	-0.92	[-1.39; -0.45]
2.50x Ritzhaupt et al. (2011) Recall Exp 1 Picture Non-Related	40	0.09	0.1400	37	0.21	0.1000	-0.97	[-1.44; -0.50]
2.50x Ritzhaupt et al. (2011) Recall Exp 1 Picture Related	40	0.27	0.1600	37	0.42	0.1300	-1.01	[-1.49; -0.54]
2.50x Ritzhaupt et al. (2008) Recall Exp 1 Adjunct Image Present	40	0.18	0.1000	37	0.32	0.1300	-1.20	[-1.69; -0.71]
3.00x Tran et al. (2024) Multiple Choice Exp 1b	40	0.42	0.3200	40	0.71	0.2500	-1.00	[-1.47; -0.53]
3.00x Jacobson et al. (2018) Multiple Choice Exp 1 Trial 2	15	25.33	17.2700	16	44.38	17.1200	-1.08	[-1.84; -0.32]
3.00x Tran et al. (2024) Recall Exp 1b	40	0.85	1.4500	40	3.23	2.7200	-1.08	[-1.55; -0.61]

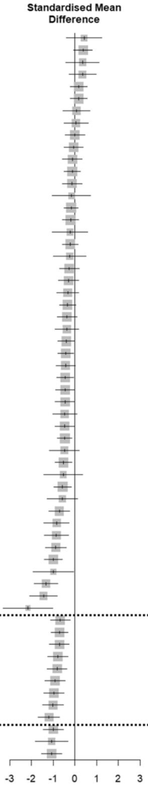

Fig. 3 Forest plot of effect sizes from 2 to 3x. SMD = Standardized mean difference. Gray squares vary in size according to the weight contributed by that effect size. Dotted lines used to separate speeds

the potential presence of publication bias. We calculated an adjusted point-estimate of the effect size for speed assuming an 'average' level of publication bias of 4.7 (in psychology, authors are on average 4.7x more likely to publish a significant effect; Mathur & VanderWeele, 2020). The adjusted point estimate was $g = -0.16$ and was still statistically significant, $p < 0.001$. As noted above, as a point estimate this value indicates only that across all the studies included (ignoring the amount of speeding in the study) speeding led to a cost, in this case after adjusting for an average amount of publication bias. The publication bias analyses were not pre-registered.

Speed as a categorical variable. First, we treated speed as a categorical variable. Although ten distinct speeds were used across studies, here we report the effect sizes for 1.25x, 1.5x, 2x, and 2.5x because these were the most studied speeds in the literature. We created a model using only these speeds as distinct categories rather than treating speed as a continuous variable to generate a distinct effect size for each speed. In Table 2 we have provided estimates for the 1.25x, 1.5x, 2x, and 2.5x speeds based on the model treating speed as categorical. Using multi-level analysis and RVE, overall speed was found to be a significant

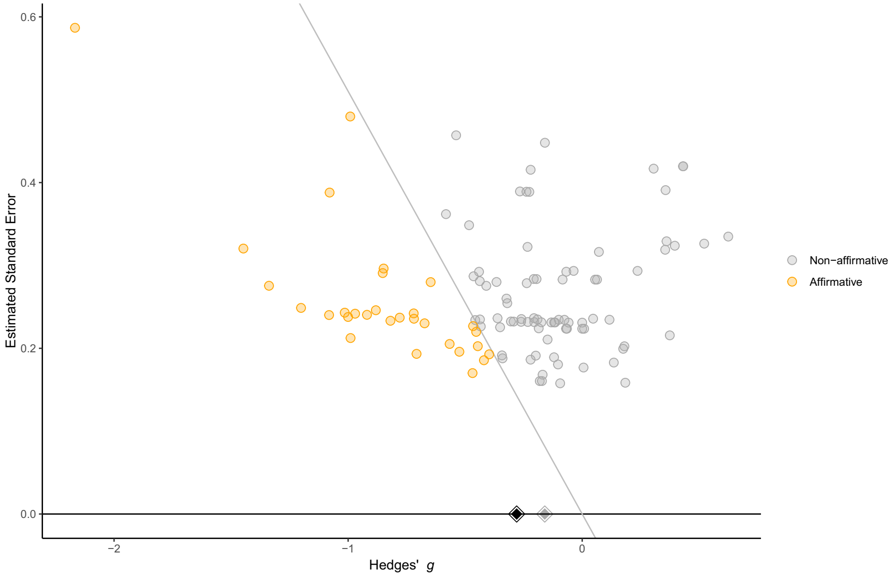

Fig. 4 Significance funnel plot. Points to the left of the line indicate affirmative results, points to the right of the line indicate non-affirmative results. The black diamond represents the robust mean point estimate of the pooled effect size from multi-level analysis with RVE ($g = -0.28$). The gray diamond represents the estimate when adjusting for publication bias ($g = -0.16$)

categorical moderator, $Q_M(3) = 47.34$, $p < 0.001$. The test for residual heterogeneity remained significant, $Q_E(95) = 519.93$, $p < 0.001$. We next created a no intercept model to compare each effect size to 0. To further quantify heterogeneity, we calculated the 95% prediction intervals (PI) for each effect, which give us an indication of how the effect size may vary across populations (Borenstein, 2023). The effect size for 1.25x ($k = 6$), $g = -0.14$, $p = 0.34$, and 1.5x ($k = 36$), $g = -0.09$, $p = 0.19$, were not significantly different from 0, and would be considered small negative effects. The effect size for 2x ($k = 48$) was significant, $g = -0.36$, $p < 0.001$, and would be considered a small to medium negative effect. Finally, the effect size for 2.5x ($k = 9$) was also significant, $g = -0.86$, $p < 0.001$, and would be considered a large negative effect. These results demonstrate that performance can be negatively affected by increased playback speed, but for speeds at 1.5x and below this effect is very small (and statistically non-significant).

In the model above, each of the four speeds are included together. In Cheng et al. (2021) the authors analyzed each speed separately. We conducted a similar analysis for each speed by generating models only using the data from those speeds rather than including all speeds in one model and treating them as

Table 2 Effect size estimates (Hedges' g) and associated 95% confidence intervals and prediction intervals for 1.25x, 1.5x, 2x, and 2.5x speeds for the categorical, continuous linear, and thin plate spline models

Speed	*k*	Measure	Categorical $Q_M(3) = 47.34$ $Q_E(95) = 519.93$	Continuous (Linear) $Q_M(1) = 56.82$ $Q_E(109) = 596.87$	Continuous (Thin Plate Spline) $Q_M(3) = 69.36$ $Q_E(108) = 539.67$
1.25x	6	g	–0.14	0.07	–0.16
		95% CI	(–0.43, 0.15)	(–0.07, 0.21)	(–0.37, 0.05)
		95% PI	(–0.73, 0.45)	(–0.46, 0.61)	(–0.69, 0.37)
1.5x	37	g	–0.09	–0.08	–0.09
		95% CI	(–0.23, 0.05)	(–0.20, 0.03)	(–0.21, 0.03)
		95% PI	(–0.62, 0.44)	(–0.61, 0.45)	(–0.59, 0.42)
2x	49	g	–0.36	–0.39	–0.35
		95% CI	(–0.49, –0.23)	(–0.51, –0.28)	(–0.47, –0.22)
		95% PI	(–0.89, 0.17)	(–0.92, 0.14)	(–0.85, 0.16)
2.5x	9	g	–0.86	–0.7	–0.88
		95% CI	(–1.10, –0.63)	(–0.86, –0.55)	(–1.08, –0.68)
		95% PI	(–1.42, –0.30)	(–1.24, –0.16)	(–1.40, –0.35)

distinct categorical variables. The effect size for 1.25x ($k=6$) was not significant, $g=-0.09$, $p=0.59$, $Q(5)=13.39$. Unlike the analysis above, the effect size for 1.5x ($k=36$) was marginally significant, $g=-0.12$, $p=0.067$, $Q(35)=61.94$. The effect size for 2x ($k=48$), $g=-0.35$, $p<0.01$, $Q(47)=332.34$, and for 2.5x ($k=9$), $g=-0.83$, $p<0.01$, $Q(8)=11.04$, both remained significant. If we combine the 1.25x and 1.5x speeds thus increasing the number of effect sizes, then the effect size estimate is significant, $g=-0.12$, $p=0.041$, $Q(42)=81.29$, indicating a small cost even for these small increases in playback speed.

Speed as a continuous variable. Next, we included speed as a continuous linear moderator in the model, centered at 1x, again using multi-level analysis and RVE. All speeds were included in this model to generate a linear coefficient relating the playback speed to test performance. Speed was found to be a significant continuous moderator, $Q_M(1)=56.82$, $p<0.001$. The coefficient for speed was $b=-0.62$, $p<0.001$. Thus, as speed increased (indexed by the multiple of the original speed), test performance became worse. There was still a significant amount of residual heterogeneity between effect sizes: $Q_E(108)=596.87$, $p<0.001$. We used this model to predict the effect size at 1.25x, 1.5x, 2x, and 2.5x for comparison to the categorical models above (see Table 2). In addition, the top panel of Fig. 5 shows the linear model fit to a scatterplot of the effect sizes.

The latter continuous model treats speed as a linear predictor. Evidence of a non-linear trend in that analysis is arguably present at the two extremes included (i.e., 1.25x, 2.5x) where the model underpredicts the cost relative to the categorical estimates of effect sizes at those speeds (see Table 2). As such, to explore the possibility of a non-linear relation between speed and performance, we used a spline model that fits separate polynomials across different regions of the predictor where we expect the relation between the predictor and outcome variable to differ (James et al., 2013). Specifically, we fit a thin plate spline (Wood, 2017) to the data with degrees of freedom = 4. Figure 5 bottom panel shows the thin plate spline model fit to a scatterplot of the effect sizes. For comparison, we used this model to predict effect sizes, confidence

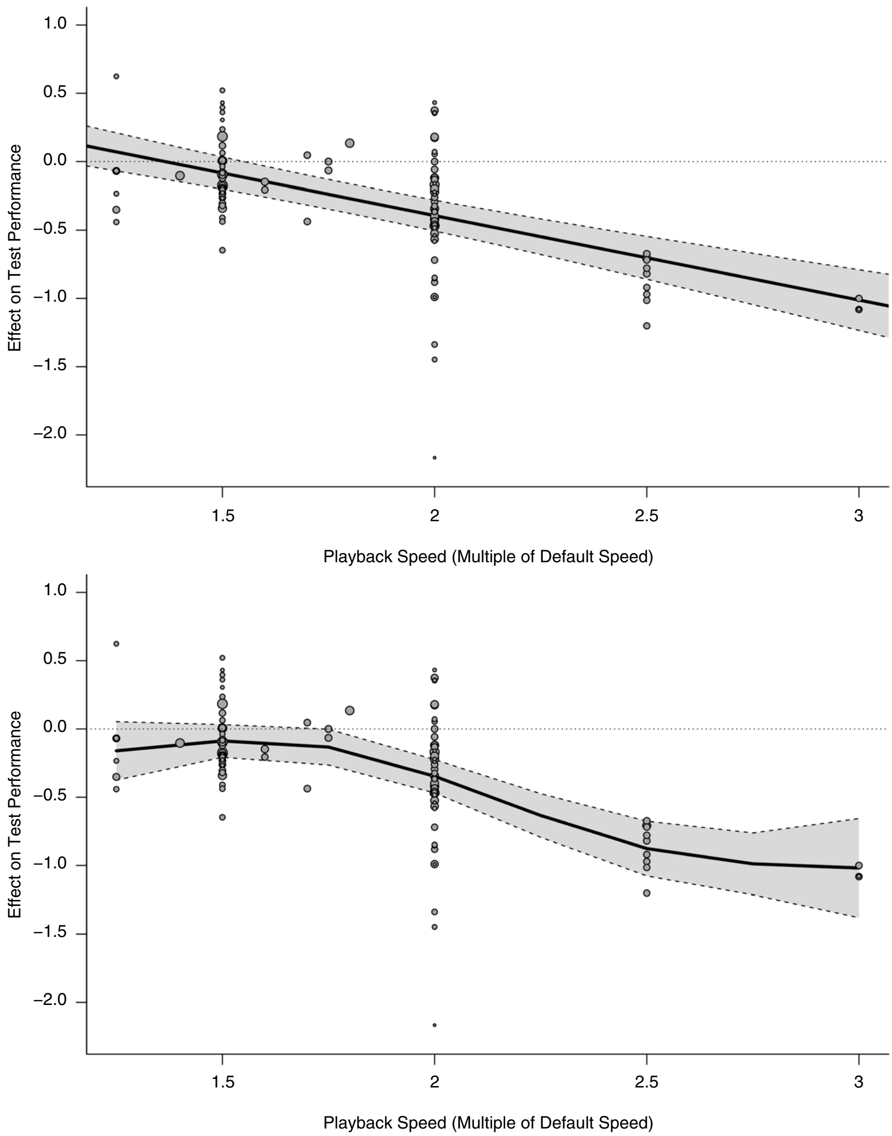

Fig. 5 The linear model (top) and thin plate spline model (bottom) fitted to a scatterplot of effect sizes. Size of dots in the scatterplot indicate RVE adjusted weight of the effect size. Shaded region represents 95% confidence interval

intervals, and prediction intervals for 1.25x, 1.5x, 2x, and 2.5x (see Table 2). This model appears to better capture the extreme values than the linear model.

Secondary Analyses

Next, we focused on moderators other than speed including test format, lecture length, age, year of publication, language the participants were tested in, lecture subject, and WPM of the base lecture. Each moderator was tested separately in a model along with speed (continuous, linear) using the maximum amount of data available (i.e., all complete cases) to look at the unique effect of each moderator on performance, over and above the contribution of playback speed. An additive and interactive model were also created for each moderator. To determine the significance of the moderator, models with the moderator were compared to models without them and, similarly, testing the significance of the interaction involved comparing a model with vs. without the interaction term.[2] The moderator analyses were not pre-registered. In this analysis, the only moderator that was significant was age, $g = -0.01$, $p = 0.003$. As age increased, the effect of an increase in speed became more negative. It should be noted that age was missing from many studies and as such the analysis with age as a moderator included 53 effect sizes. In addition, the effect of age was driven by four effect sizes from the only study that included an older adult sample (mean age ~ 73; Murphy et al., 2023). Removing these effect sizes results in no significant effect of age. Consistent with this idea, Murphy et al. (2023) also found an effect of age such that older adults suffered a larger cost of increasing playback speed. No interaction models were found to provide a significantly better fit than the model with only main effects. Null effects of moderators should be considered cautiously provided many had limited variability. This caution is particularly warranted when assessing interactions given the limited amount of data available in each cell of the design.

Discussion

Increasing the playback speed of a lecture reduces time costs without compromising the amount of content covered. This strategy appears to be employed often by students when viewing video lectures (Tharumalingam & Risko, 2025; Zureick et al., 2018), and as such, understanding how it influences lecture processing is important. In the present meta-analysis, we collected studies which manipulated the playback speed of a video lecture to understand the effect of playback speed on learning. Overall, as playback speed increased the cost to content test performance increased.

[2] We also conducted an additional moderator analysis wherein the moderator was assessed in a model without speed (see Roberts et al., 2022). For categorical variables this involved first forming an interceptless model with only the moderator of interest and using a Wald test to determine whether the moderator was significant. For continuous predictors, we mean-centered the variable and then tested the moderator by itself with an intercept included. The results were largely the same as that reported in the main text, except that Subject was a significant moderator, and this was due to Math being more resistant to playback speed differences than Life Sciences and Social Sciences. There were only four effect sizes with Math as the Subject and all were 1.5x. Thus, this effect is likely a function of the smaller effect of increasing playback speed with 1.5x than a genuinely smaller effect of speeding in Math lectures.

That said, the cost to test performance at moderate and more popular levels of speeding (e.g., 1.25x, 1.5x) were small and non-significant unless combined together. We did not find strong evidence that this effect was moderated by the length of the lecture, or test type, though these factors were only considered in exploratory analyses. We did find that age moderated the effect such that the cost increased in magnitude with increasing age of the sample (though age was not provided by many of the papers), but this was a function of a single study that did find an effect of age.

Contextualizing the Cost

We first address the fact that performance decreases as speed increases. While unsurprising in the sense that, at some playback speed, costs must emerge given our limited cognitive capacity, the present investigation offers at least an interim estimate of that cost. When compared against the meta-analytic estimates provided by Cheng et al. (2021), the overall effect at 1.5x was somewhat smaller here ($g = -0.09$ categorical; $g = -0.08$ linear; $g = -0.09$ thin plate spline) compared to what Cheng et al. found for 1.4x-1.5x ($g = -0.21$) and the overall effect at 2x ($g = -0.36$ categorical; $g = -0.39$ linear; $g = -0.35$ thin plate spline) was comparable here to what Cheng et al. found for 1.8x-2x ($g = -0.36$). Cheng et al. (2021) found that the overall cost at 1.4x-1.5x was significant. In the categorical model including 1.25x, 1.5x, 2.0x, and 2.5x the 1.5x effect was not significant, but when 1.25x and 1.5x effect sizes were combined it was significant. Hence, there is likely a small cost at 1.5x and below, however, the cost in this speed range is small and the prediction interval clearly included many non-negative values. Tharumalingam and Risko (2025) found that 1.25x and 1.5x were the most popular speeds used by students amongst their survey sample, demonstrating that students may be increasing video lecture speeds in a manner that does not have a large negative impact on their test performance (though it seems unlikely that the cost is zero). As lecture speed approaches 2x and beyond, the costs are statistically significant. Our analysis expands upon the work by Cheng et al. by providing models treating speed as a continuous variable which can be used to estimate the effect of speeds which are not commonly examined in the literature. By pooling the results of many more studies, we also increase confidence in the results. Additionally, by characterizing the degree of heterogeneity, we demonstrated that the effect of speed may be moderated by other factors. While our moderator analysis could not identify the source of this variation, characterizing the heterogeneity may allow future researchers to examine and understand the observed variation. However, it is important to acknowledge the limitations of using these models to predict effects beyond the speeds captured by the data, as the real relation between speed and performance at these speeds may differ from the model which best fits the data of speeds that have been examined experimentally.

Whether the observed costs of increasing playback speed are worth the time savings is the decision of the individual learner. The effect size estimates provided here are standardized mean estimates. To understand these costs, we can imagine a hypothetical situation where the average score for a test is 75% and the standard deviation is 20%. Using the categorical model from Table 2, a class who views the video lecture at 1.5x should expect a cost of approximately 0.09 standard deviations, which gives us a cost of about 2%. If instead the class increased the playback speed to 2x, then we would expect a cost of approximately 0.36 standard deviations or about 7% and at 2.5x the cost would be 0.86 standard deviations and about 17%. Note that this example uses point estimates and that the prediction intervals around these values are large.

In the present investigation we examined two theoretically important moderators of the cost of increasing playback speed – test format and lecture duration – along with several other moderators. Test format and lecture duration were not found to be significant, and neither were found to interact with speed. This was also true of the other moderator variables included with the sole exception being age of sample. As age increased the cost associated with increasing speed increased. This might reflect differing levels of familiarity with speeding in different age groups. For example, video lectures and the tools used to play them have only become commonly used recently. It is important to note that this moderator analysis was exploratory. It was made possible by natural variation in experimental methodologies across the studies, rather than these factors being the topic of experimental evaluation in and of themselves. For example, while a variety of test types were used, the majority of studies enlisted multiple choice tests and within our “recall” category there were clearly distinct test types (e.g., short answer, drawing tests etc.). In addition, there was a limited range in the lecture durations tested (i.e., between 9 and 30 min). Thus, before accepting that any of these moderators have no effect (or that age has an effect) on playback speed and test performance more work is needed. Furthermore, the amount of heterogeneity discovered between studies may indicate that other, unaccounted-for moderating variables could be responsible for the observed variation. The results presented here should serve as inspiration for future confirmatory research that specifically focuses on understanding these and other potential moderators of the effect of speeding lectures on test performance.

Underlying Mechanisms

The present meta-analysis was focused on estimating the effect size associated with increasing playback speed at varying levels, not to test a particular mechanism responsible for those effects. That said, it is worth speculating on this mechanism in light of the results reported here. A useful framework in the educational psychology domain for considering the effect of increasing playback speed is cognitive load theory (CLT; Sweller, 1994), according to which there is a limit on the amount of information which can be handled by our information-processing system (Sweller, 1994). Information must be temporarily retained in working memory before becoming consolidated into long-term memory through schema construction (Sweller, 1994); therefore, if the amount of information being presented to the learner exceeds their capacity to temporarily retain the information in working memory, learning will be impaired. Assuming processes like schema construction are time-dependent, as the rate of information presentation increases, the amount of information that would need to be stored in working memory would increase. Once this capacity is exceeded, then performance impairments should be evident. Consistent with this general idea, lecture playback speed has been associated with increased cognitive load. For example, Mo et al. (2022) measured self reported cognitive load and found that 1.25x and 1.5x were associated with higher cognitive load than the default speed. Pastore (2010) and Pastore (2012) found that participants who viewed a lecture at 2x speed experienced more cognitive load than participants who viewed it at 1.5x or 1x. Jacobson et al. (2018) measured cognitive load for each of their speeded conditions using the NASA TLX scale. They found that cognitive workload was significantly higher in the 2x and 3x conditions compared to the 1x condition. In a similar vein, Risko et al. (2024) measured effort as an index of cognitive load and reported that participants who viewed a lecture at 1.5x reported that it required more effort than in the 1x condition.

How can we understand the smaller/absent effects of increasing speed at 1.25x and 1.5x and the arguably rapid decreases afterward? One idea is that the organic speech rate of a typical lecture (i.e., ~ 150 WPM) leaves spare capacity for most learners such that moderate increases do not exceed their capacity limits. As playback speed increases, the likelihood that it would exceed an individual learner's capacity would increase until it exceeded most/all learner's capacities. The latter type of explanation fits well with the nonlinear form of the function relating speed to test performance costs reported here. In addition to spare capacity, another "protective" factor worth considering is that increasing playback speed could increase engagement (e.g., Ilie & Thompson, 2006; Lang et al., 2020). That is, the organic speed of lectures might be too slow, leading to boredom (e.g., Mo et al., 2022) and/or the spare capacity noted above at the organic speed might leave resources available for mind wandering (Jacobson et al., 2018). Consistent with this idea, when Tharumalingam and Risko (2025) surveyed participants about their reasons for speeding, a large number reported that increasing playback speed made it easier to pay attention. Testing this idea in the context of interactions between increasing playback speed and mind wandering have led to mixed results (Murphy et al., 2023; Wilson et al., 2018). Murphy et al. (2023) reported two experiments examining increasing playback speed and mind wandering and in one experiment increasing playback speed was associated with reduced mind wandering and in the other it was not. Interestingly, in the experiment that found an effect of increasing playback speed on mind wandering one of the speeds was 0.75x and it was this condition that was associated with the highest rate of mind wandering. The experiment that did not find an effect of increasing speed compared 1x to 2x. Thus, the potential protective effect of increased engagement requires further experimental examination. Nevertheless, if such an effect exists it might help explain the small/non-existent effects of increasing playback speed with moderate increases (i.e., the increase in load being counteracted by an increase in engagement).

The mechanisms suggested above provide clear directions for future research. For example, the potential for speeding to increase engagement clearly needs further attention. As noted above, the results have been mixed to date. Turning to the interactions between load and increasing playback speed, if the negative effects of increasing playback speed represent the rate of information becoming increasingly likely to exceed an individual's WM capacity, then individual differences in WM capacity should predict effects of increasing playback speed. That is, the slope of the function relating playback speed to test performance should be steeper for those lower in WM capacity. In a similar vein, according to this mechanism, we might expect the difficulty of the material to moderate the influence of playback speed. In cognitive load theory, difficulty has been conceptualized in terms of intrinsic load (Sweller, 1994). Sweller (2010) defines intrinsic load as the degree of element interactivity of the content: if the learner must simultaneously learn about various elements which interact with each other, intrinsic cognitive load is said to be high. Low element interactivity is associated with low intrinsic cognitive load since learning any given element is not dependent upon learning any other element. For example, learning the individual chemicals on the periodic table might be associated with low intrinsic cognitive load, but being required to learn about how different chemicals interact to form bonds might be associated with higher intrinsic cognitive load. Increasing playback speed might be expected to have particularly adverse effects in high element interactivity contexts, given the need to hold multiple pieces of information active to support their integration. For example, if an individual is presented with elements A, B, and then C and has to integrate them, increasing the rate with which new elements are being presented might lead to interference (e.g., elements D and E being presented while an individual is still trying to integrate A, B, and C). Lastly, again related to the interaction between load and increasing playback speed, an additional factor which future studies should consider more is the prior knowledge of participants. This factor is related to difficulty in the sense that prior knowledge will modulate intrinsic load. Again, one might expect that learners with a higher degree of prior knowledge on a subject will be less affected by the increase in load caused by increasing playback speed of a video lecture. Preliminary evidence consistent with this notion was provided by Wilson et al. (2018) who showed that there was no cost of speeding at 1.6x and 1.7x for learners with prior knowledge compared with a small cost for those without prior knowledge.

Future Directions

While we have discussed a number of future directions related to testing the mechanisms underlying the costs (or lack thereof) reported here, a number of other potential future directions emerged in our review of the literature. One factor that will be important to consider in the future is learner control of playback speed. In the studies reviewed here, researchers randomly assigned individuals to conditions. While this is a necessary part of assessing causation, if part of the goal is understanding the impact of learners increasing playback speed, then it is important to acknowledge that learners "in-the-wild" have control over the speed as well as the ability to pause, rewind, and rewatch video lectures. If individuals can successfully monitor their learning and control playback speed accordingly, then this would mitigate costs that might appear in studies where that control is unavailable. Whether learners can monitor and effectively control playback speed is an open question. On the one hand, Risko et al. (2024) found in one experiment that participants' content test performance was impaired at 1.5x speed but their estimates of their predicted performance (after the lecture but before the test) did not change, suggesting an insensitivity to the cost. Murphy et al. (2022), on the other hand, generally found individuals' predictions about their performance to be accurate (i.e., when there was a cost participants predicted there would be). Clearly more research is needed to understand interactions between metacognitive monitoring and control in the context of speeded lectures. While we draw attention above to several weaknesses of the present literature, it should not go unnoticed that the studies on which this meta-analysis are based are impressively diverse in terms of the number of different research groups, locations, subjects, test formats, and lecture durations etc. (see Table 1).

The focus in the present investigation was on the effect of increasing playback speed on content test performance. While this issue is likely to be (and should be) front and center in individuals' minds when considering increasing playback speed, there exist a number of other variables related to learning and the learning experience more broadly that are worth examining. As noted above, how increasing playback speed impacts metacognition will be important to consider further. In addition, how increasing playback speed impacts a learner's affective experience of the lecture will also be important. A few studies have begun to address this issue. For example, Risko et al. (2024) have investigated the relation between speeding and affect and found speeding to be associated with increases in negative affect and decreases in liking. These preliminary findings suggest that speeding may have a negative impact on the learning experience.

Another factor worth considering in future research is the extent to which experience consuming material with increased playback speed modulates one's ability to consume that information. For example, might experience or training at higher speeds reduce the negative impact of increasing playback speed? This idea was examined by Jacobson et al. (2018). They had three groups view multiple video lectures at either 1x, 2x, and 3x then transfer to a 3x condition. The authors found no evidence that practice at the higher speeds led to less of a cost in the final 3x condition. That said, the amount of practice was minimal and it remains an open question what effect (if any) prolonged experience consuming material at increased playback speeds might have.

Limitations

As with all meta-analyses, our study is potentially subject to the "apples and oranges" problem, where differences in the methodologies of studies call into question whether it is meaningful to pool their results together (Harrer et al., 2021). For example, studies differed in lecture subjects, ranging from geography to physiology to statistics, and possibly difficulty. While we could obtain information about the subject of each lecture, determining relative test difficulty would be difficult. While we were able to broadly categorize tests between multiple choice, recall, and combined, this does not provide a concrete measure of difficulty. Furthermore, the lecture subjects covered by the included studies do not exhaust the possible subjects which may be taught through video lectures. Future studies should seek to broaden the content covered by video lectures in order to determine whether some topics are more impacted by increasing playback speed than others.

Our moderator analysis also suffered from a number of issues. Not all studies reported important information such as age of the participants and even when age was reported, it was usually given as the average age for all participants rather than providing a separate age for each treatment group. In addition, relying on natural variation in moderators can fail to capture sufficient variability in that moderator. For example, we speculated that the length of the lecture may affect the impact of speeding, but the durations of lectures ranged from 9 to 30 min with an average of 13 min across effect sizes. This clearly misses out on lectures with longer durations but is representative of video lectures provided by MOOCs which appear to average close to what our sample covered (Geri et al., 2017; Gutiérrez-González et al., 2024; Valor Miro et al., 2018). Lastly, most participants were university students from English speaking countries; therefore, we cannot conclude that our findings are generalizable to the global population.

While our findings do seem to demonstrate a robust pattern, it is important to note that these findings were based on a relatively low number of studies, and many of the studies involved the same researchers, materials, etc. For example, the two studies by Pastore (2010 and 2012) used the same video lecture and performance test in two separate studies. The same could be said for the studies by Nagahama and Morita (2017a, 2017b, 2018, Nagahama et al., 2019). While we attempted to account for dependency amongst effect sizes by combining multi-level meta-analysis with robust variance estimation, there could be dependencies that are not fully captured by the models we utilized.

Lastly, bias could be introduced by the literature search process itself. While we attempted to be as exhaustive as possible, it is possible that some studies were missed because they were not detected by our search process. These limitations must be considered when interpreting the findings of this analysis.

Conclusion

We conducted a meta-analysis to determine the effect of increasing the playback speed of video lectures on subsequent content test performance. Twenty-four studies were gathered resulting in 110 effect sizes that were pooled together into a meta-regression model. The results of this meta-analysis show that as playback speed increases content test performance decreases, but there appears to be little to no effect for speeds up to 1.5x. Given the popularity of increasing playback speed and the important role video lectures will play in higher education in coming years, future research aimed at providing a deeper understanding of its impacts seems prudent.

Author Contribution **Theepan Tharumalingam**: Writing – original draft, Writing – review and editing, Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Software, Visualization.

Dr. Brady R. T. Roberts: Writing – review and editing, Conceptualization, Formal analysis, Investigation, Methodology, Software, Validation, Visualization.

Dr. Jonathan M. Fawcett: Writing – review and editing, Conceptualization, Formal analysis, Investigation, Methodology, Software, Validation.

Dr. Evan F. Risko: Writing – review and editing, Conceptualization, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Supervision, Validation.

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Data Availability Data openly available at https://osf.io/zj62u/

Declarations

Ethical Approval This study did not involve human or animal subjects and therefore did not require ethical review.

Conflict of Interest The authors declare that there are no conflicts of interest related to this study.

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