

GPT API Models Can Function as Highly Reliable Second Screeners of Titles and Abstracts in Systematic Reviews: A Proof of Concept and Common Guidelines

ABSTRACT

Independent human double screening of titles and abstracts is considered a critical step to ensure the quality of systematic reviews and meta-analyses herein. However, double screening is a costly as well as resource-intensive procedure that decelerates the review process, ultimately excluding many researchers from using it. To alleviate this issue and potentially increase the reliability of systematic reviews and meta-analyses herein, we evaluated the use of OpenAI's GPT (generative pre-trained transformer) API (application programming interface) models as alternative second screeners of titles and abstracts. To make a comprehensive evaluation of this screening approach, we developed a new benchmark scheme for interpreting the performances of automated screening tools based on typically human screening performances in high-quality systematic reviews and conducted three large-scale classification experiments on three systematic reviews containing different levels of complexity typically encountered in high-quality reviews. Across all experiments, we show that the GPT API models can perform on par or even better than common human screening performance in terms of detecting relevant studies while showing high exclusion performance as well. Hereto, we introduce the use of multi-prompt screening, that is making one concise prompt per review's inclusion/exclusion criteria, and show that this is a pivotal technique to make the use of GPT screening work in highly complex review settings. To support future reviews, we develop a reproducible workflow and tentative guidelines for when reviewers can or cannot use GPT API models as independent second screeners of titles and abstracts. Our aim is ultimately to make a framework for using GPT API models acceptable as independent second screeners within high-quality systematic reviews. Finally, to standardize and scale up this application, we present the R package AIscreenR.

KEYWORDS: *title and abstract screening, OpenAI's GPT API models, systematic review, screening benchmarks, AIscreenR*

HIGHLIGHTS

What is already known

- OpenAI's GPT API models have shown promising performance in terms of working as a second screener of titles and abstracts within various scientific fields.
- Automated screening tools can ease the burden of title and abstract screening
- Automated screening tools most often cannot detect/classify all relevant studies, which in turn, can induce the so-called 'artificial screening biases'

What is new

- We show that OpenAI's GPT API models can function as highly reliable second screeners in social science reviews with recall performances at least on par with human screeners.
- We introduce the concept of multi-prompt screening and show that this approach can make GPT API models work as reliable second screeners even in highly complex review settings.
- We develop a new benchmark scheme based on typical human screening performances from high-standard systematic reviews partially to make judgments about acceptable (and unacceptable) screening performances in high-quality reviews and partially to make reliable comparisons between the screening performance of humans and automated screening tools in general.
- We provide general guidelines for how and when GPT API models can (and cannot) safely be used as independent second screeners of titles and abstracts.
- We present and validate the AIScreenR package to ensure standardized conduct of title and abstract screening with (OpenAI's) GPT API models (and in theory with other models such as Claude 2).

Potential impact for Research Synthesis Methods readers

- Changing duplicate screening of title and abstract screening in systematic reviews.
- Increasing the reliability of large-scale systematic reviews.
- Making substantial and reliable reductions of human labor in systematic reviews.
- Providing a new guideline for reviewers on when and when not to use automated screening tools in high-quality systematic reviews.
- Standardizing screening with prompt-based Large Language Models (LLMs).

1 INTRODUCTION

Systematic reviews are essential tools for informing policy, research, and practice. Hence, it is all-important that systematic reviews adhere to the highest scientific standards. Yet systematic reviews are time-consuming, potentially hindering a timely transfer of usable knowledge. Distinct from other types of reviews, systematic reviews are defined as the process of collecting, assessing, and synthesizing findings from (ideally all) relevant scientific studies using explicit and replicable research methods (Gough et al., 2017; Hou & Tipton, 2024). A critical first step to ensure the quality of systematic reviews and meta-analyses herein involves detecting all eligible references related to the literature under review (Polanin et al., 2019). This entails searching all pertinent literature databases relevant to the given review, most often resulting in thousands of title and abstract records that need to be screened for relevance. Manual screening hereof can indeed be a time-consuming and tedious task. However, overlooking relevant studies at this stage can be consequential, leading to substantially biased results if the missed studies are systematically different from the detected ones. In fact, this can be seen as a special case of publication/selection/searching bias (Brunton et al., 2017; Hedges, 1992; Rothstein et al., 2005), which threatens the internal validity of systematic reviews (Shadish et al., 2002). Therefore, independent human double-screening is considered to be the 'golden standard' to hinder a biased selection of relevant studies (Guo et al., 2024; Higgins et al., 2019; Stoll et al., 2019; Wang et al., 2020). This is further supported by previous research suggesting that screeners on average tend to miss between 3% to 24% of all eligible studies depending on the level of content knowledge, which most often has a substantial impact on the final quantitative results (Buscemi et al., 2006; Waffenschmidt et al., 2019). In medicine, this number is in some cases even higher when using student screeners (Ng et al., 2014). Nonetheless, duplicate screening of all identified titles and abstracts is a costly and resource-intensive procedure, possibly requiring several months of skilled, full-time human labor (Campos et al., 2023; Hou & Tipton, 2024; Shemilt et al., 2016). Consequently, many reviewers refrain from using duplicate screening methods due to low budgets or narrow time limits, for instance (Pacheco et al., 2023). Alternatively, reviewers make too narrow searches to keep the number of records down to a manageable size which again seriously increases the risk of overlooking relevant studies (Van De Schoot et al., 2021). Over time all these issues will only grow in size since the complexity of identifying all relevant studies increases with the rapid growth in the number of scientific publications (Bornmann et al., 2021; O'Mara-Eves et al., 2015). Thus, it can be considered an economically inefficient and unsustainable use of human resources only to rely on

(duplicate) human screening of titles and abstracts in future systematic reviews¹ (Shemilt et al., 2016), and changes are needed to maintain a high quality of large-scale systematic reviews.

A possible solution, and an alternative to human double-screening, is to use (semi-)automated screening tools based on text-mining and/or machine-learning algorithms to act either as a second screener, a course-grained classifier, or to sort citation records in prioritized order (Cohen et al., 2006; Gartlehner et al., 2019; O'Mara-Eves et al., 2015; Van De Schoot et al., 2021). The use of automated screening tools is considered invaluable in supporting living reviews and has shown a promising ability to reduce the screening workload by 30% to 70% (O'Mara-Eves et al., 2015; Perlman-Arrow et al., 2023). However, a clear disadvantage of these substantial workload savings is that it is expected that they will always result in missing at least 5%-10% of all eligible references since "a 100% recall rate with a stochastic algorithm is generally considered unattainable" (Hou & Tipton, 2024, p. 3). This seems to create a screening paradox which might be one of the main reasons why many reviewers tend to mistrust the application of machine-learning tools (O'Connor et al., 2019). While trying to reduce selection biases caused by single screening, automated screening potentially introduces a novel type of publication/selection bias defined by König et al., (2023) as the 'artificial screening bias' (ASB).

An additional challenge is that most automated screenings are based on supervised and active learning methods. This means that they need to be trained on a large enough set of in- and excluded references to perform adequately which in turn can be a time-consuming task, as well. Moreover, when automation tools are used for prioritized screening, it is most often unknown when it is safe to stop screening with regard to finding all or close to all eligible references. Albeit, various stopping rules have been proposed, the adequacy of these is sensitive to a range of factors such as the length of the database, the prevalence of relevant studies, and the balance between relevant and irrelevant records (Campos et al., 2023; König et al., 2023; Van De Schoot et al., 2021).

To date, many automated screening tools have been thoroughly evaluated (Burgard & Bittermann, 2023; Kugley et al., 2016). From these evaluations, the overall picture is that they are generally not capable of replacing an independent human second screener without a significant risk

¹ But already now, we see that in some applications of systematic reviews, the number of records needed to be screened way exceeds what can be considered an economically efficient and sustainable use of human resources, either due to very broad terms needed to be added to search string to cover all relevant studies (see e.g., Thomsen et al., 2022) or due to a broad aim of the review as is often the case with scoping review and evidence and gap maps (see e.g., Bondebjerg, Filges, et al., 2023).

of omitting a substantial number of eligible studies² (Gartlehner et al., 2019; O'Mara-Eves et al., 2015; Olorisade et al., 2016; Rathbone et al., 2015). By using the level of automation heuristic (c.f. Table 1) developed by O'Connor et al. (2019), it can be said that current automated tools generally fail to function at the highest levels of automation (i.e., Level 3 and Level 4) where they make credible independent deterministic screening decisions. Instead, the vast majority of tools are predominately used to conduct Level 2 tasks such as sorting citation records in prioritized order from highest to lowest probability of being relevant to the review (Kugley et al., 2016; O'Connor et al., 2019; Olofsson et al., 2017). If considerable time savings should be realized in future reviews, it is regarded as all-important that automated tools at least rise to Level 3 of automation (Jonnalagadda et al., 2015; Tsafnat et al., 2014).

TABLE 1. Levels of automation for human-computer interactions*

Level	Task
Level 4	Tools perform tasks to eliminate the need for human participation in the task altogether, e.g., fully automated article screening decision about relevance made by the automated system.
Level 3	Tools perform a task automatically but unreliably and require human supervision or else provide the option to manually override the tools' decisions, e.g., duplicate detection algorithms and software, linked publication detection with plagiarism algorithms and software.
Level 2	Tools enable workflow prioritization, e.g., prioritization of relevant abstracts; however, this does not reduce the work time for reviewers on the task but does allow for compression of the calendar time of the entire process.
Level 1	Tools improve the file management process, e.g., citation databases, reference management software, and systematic review management software.

*Adopted from O'Connor et al. (2019)

A possible solution to bridge the gap between Levels 2 and 3/4 of automation³ is to use the newly developed large language models (LLM), such as the generative pre-trained transformer (GPT) models introduced by OpenAI. The first evaluations of using OpenAI's GPT API (application programming interface) models for screening medical and software engineering titles and abstracts have generally yielded promising results with recall and specificity measures in most instances on

² To overcome/reduce this issue, a new tentative guideline termed SAFE has been developed in which it is suggested to use multiple machine learning algorithms in order to detect all relevant references in the bulk of records (Boetje & van de Schoot, 2024). However, we do not consider this framework to have been thoroughly enough testing yet to know if the SAFE procedure allows reviewers to detect all relevant studies with the machine learning algorithms including in screening softwares such as ASReview.

³ Although the automated tool can make none human-assisted decisions, we still consider it all-important that all screening task are made with human-in-the-loops. This also means that even though GPT API model potential can make reliable claims, we believe that these should never be used as stand-alone single/first screeners in high-quality reviews.

par with human performance but always on par with or superior to classical machine-learning tools (Guo et al., 2024; Syriani et al., 2023).

Although previous applications and evaluations of using OpenAI's GPT models for title and abstract screening (henceforth TAB screening) represent a vital first step for validating the use of GPT models as independent second screeners in systematic reviews, many questions are left unanswered. Most pressing is that it is still unclear if and how the GPT models can be implemented in systematic reviews in a standardized and reliable manner. In contrast to many well-established automated screening algorithms, there exists no recommended workflow and guideline for how to conduct such screenings, including how to make reliable prompts. Even more critically, no software⁴ has yet been developed to support and standardize the setup of this screening approach. Therefore, a major aim of this paper is partly to develop a heuristical workflow for how to conduct TAB screening with GPT API models and partly to present the R package AIscreenR (version 0.0.1). Our goal is to develop an easy-to-implement framework that draws on commonly accessible RIS-file data typically used with standard review software such as Covidence and EPPI-Reviewer, among others. This might increase the chances of ensuring user deployment and acceptance since complex implementation is often considered to be a major impediment to the wider application of automated screening tools (O'Connor et al., 2019).

Furthermore, there has not yet been laid any solid foundation on which evidence organizations (such as Cochrane and the Campbell Collaboration) can accept and recommend the use of such tools per se. According to the Campbell Collaboration, for them to accept the incorporation of automation tools in their reviews “*requires (a) functioning tech (b) proof that it is functioning appropriately (c) the tech embodied in usable products (d) agreed guidelines for appropriate use (e) training (f) ongoing support.*” (Campbell Collaboration, 2023). Therefore, the overarching goal of this paper is to construct a framework in which TAB screening with GPT API models can be said to meet requirements set forth by evidence organizations. In the following part, we briefly explicate how we aim to build this framework.

Concerning requirement (a), we cannot, as such, fulfill this demand since the GPT API models we draw upon in this paper are closed-source applications with black-box algorithms. Consequently, our suggested framework is only viable as long as given firms provide access to their LLMs.

⁴ To our knowledge, GPT models have so far only been implemented in the EPPI Reviewer software with the aim to support automated data extraction from full texts (see EPPI-Centre, 2024) and not for TAB screening purposes.

However, our suggested framework and codes can readily be remodeled to work with other API models, such as models from Claude 2 or Mistral AI where the request body takes the same arguments as OpenAI’s GPT API models. Therefore, our setup aims to be agnostic to the given provider of the given LLM. In theory, our approach can be implemented together with other LLMs such as Mistral’s open-source LLMs that can be downloaded locally by the users. We, therefore, understand a “functioning tech” to point, in our case, to the broader family of LLM models, which we believe will be around for many years.

A key part of fulfilling Campbell’s requirement (*b*), and not compromising the quality of future systematic reviews, is to show that the GPT API models are not significantly inferior to human screening performance (O’Connor et al., 2019). Thus to make a reliable assessment of this, we developed empirical screening benchmarks to which the GPT API screening performance can be compared. We consider this as the only reliable way to assess whether a given recall is good or bad. For example, if humans on average tend to miss 20%-25% of all relevant studies during the title and abstract screening phase, then it might be misleading to infer that GPT models with a recall of 0.75% imply that GPT cannot be used as an individual second screener. To construct such a benchmark scheme we mapped the human screening performance of 22 large-scale systematic reviews; 17 Campbell Systematic Reviews, and five systematic reviews conducted by the Norwegian Institute of Public Health (NIPH). Thereafter, we conducted three large-scale classifier experiments, where we showed that OpenAI’s GPT API models *can* conduct TAB screening with a performance at least on par with human performance relative to our developed benchmarks, even when applied in a highly complex review setting. In this framework, we introduce the concept of multi-prompt screening that resembles the common way humans usually screen titles and abstracts (Valentine, 2009). These experiments further aim to show that GPT API models are perfectly viable as independent second screeners in social science reviews.

We aim to fulfill requirement (*c*) by developing the AIScreenR software. A side-effect of conducting the above-mentioned classifier experiments, mentioned under requirement (*b*), was further to ensure that the AIScreenR package works reliably.

Then, to fulfill requirements (*d*) and (*e*), we develop a heuristic for how to test the performance of one’s developed prompt(s) and screening as well as assess under what conditions TAB screening with the GPT API models can (and cannot) be accepted to be used as an independent second screener in systematic reviews. We inform these guidelines by the empirical human screening bench-

marks developed under requirement (b) as well. Since we are working with *pre-trained* models, requirement (e) is not as such necessary in our case. Instead, the performance of the prompt(s) used for screening needs to be *tested* and compared against human performance measures before credible TAB screening can be initiated. We return to this point in later sections. Finally, to accommodate requirement (f) we have developed the AIScreenR package as an open-source software so that others in the review community (e.g., the Evidence Synthesis Hackathon) can readily contribute to the development and ongoing support of the software.

With the exposition sketched above, we hope to make the uptake of such tools more acceptable in future state-of-the-art systematic reviews. This goes without saying that our approach represents a final solution. Our aim is merely to show one way in which GPT API models can be used for TAB screening in large-scale systematic reviews that can inspire and be transferred to future applications of TAB screening with LLMs. The remainder of the paper proceeds as follows: In Section 2 we review previous evaluations of using OpenAI’s GPT models for TAB screening tasks in systematic reviews and reflect on our contributions. In Section 3 we describe the metrics we applied to evaluate the screening performance of the GPT API models and human screeners, respectively. In Section 3, we further develop screening performance benchmarks to assess the performance of the GPT API models. In Section 4, we present our classifier experiments, including our prompt engineering and data underlying these experiments. The results of these experiments are also presented in this section. In Section 5, we deduce tentative guidelines for when it is acceptable to use GPT API models as an independent second screener. Moreover, we elaborate on how we think reliable prompts can be developed in future reviews. Finally, in sections 6 to 8, we recapitulate by reflecting on the limitations of our work, the prospect of using OpenAI’s LLMs, and what should concern future research as well as the implications of our results and recommendations.

2 RELATED WORK

To our knowledge, the first evaluation of the TAB screening performance of OpenAI’s GPT API models was performed by Syriani et al. (2023). Based on five ongoing systematic reviews within the field of software engineering, they compared the TAB screening performance of the GPT API model gpt-3.5-turbo-0301⁵ relative to five state-of-the-art machine learning algorithms. Hereto they found that OpenAI’s GPT API models perform on par with traditional classifier models, and in some instances even better—without any need for (pre-)training. They only found the models to perform badly when applied on datasets where humans had shown a “high conflict ratio”. This might simply

⁵ This model has been deprecated

indicate that the models perform badly when given unclear inclusion/exclusion criteria—as the humans did too. Syriani et al. (2023) used Python to reach the GPT API models, but they did not build any publicly available software for others to replicate their workflow.

Guo et al. (2024) tested the leverage of OpenAI’s GPT-4 API model⁶ for TAB screening of medical research literature. They found that the average recall (referred to as the sensitivity of included papers) and specificity when compared to the final decision of two independent human screeners across six clinical reviews was 0.76 and 0.91, respectively. Based on these results, Guo et al. (2024) inferred that the GPT-4 model is proficient in terms of excluding the right studies whereas it is insufficient in finding relevant studies compared to human screeners. Consequently, Guo et al. (2024) concluded that GPT API models should not replace human screening but instead be seen as a support tool guarding against human errors. Guo et al. (2024) used Python to call the API models without providing any general user software.

Gargari et al. (2024) applied the gpt-3.5-turbo-0613 API model to conduct TAB screening in one clinical systematic review. In line with Guo et al. (2024), they found GPT to be better at making correct exclusion decisions relative to detecting relevant studies. Therefore, they also recommended not replacing any human raters with the gpt-3.5 API model. Gargari et al. (2024) reached the API model via Python, and they shared their codes⁷ so that others can replicate their workflow. Yet this requires reviewers to be rather skilled in Python coding.

On a related line of research, Alshami et al. (2023), Khraisha et al. (2024), and Issaiy et al. (2024) all investigated the TAB screening performance of using ChatGPT from the internet interface. Alshami (2023) found that using the ChatGPT interface exhibits performance measures similar to the API models. By contrast, Khraisha et al. (2024) and Issaiy et al. (2024) found that using GPT-3.5 and GPT-4 via the ChatGPT interface worked insufficiently compared to human performance. As we will later discuss further, we found a similar pattern when we compared the performance of OpenAI’s GPT API models with that of the ChatGPT interface. To be precise, the GPT API models reached from the *v1/chat/completions* endpoint worked significantly better relative to the GPT models embedded in the ChatGPT interface. In fact, we were not able, by any means, to replicate the results obtained from the API models on the ChatGPT interface. We, therefore, consider it pivotal that future research clearly distinguishes between OpenAI’s GPT models when doing research with them so that the performance of different GPT models is not unnecessarily mixed up. In the paper, we narrowly

⁶ It is uncertain what exact model the authors used. We expect it was the gpt-4-0613 API model.

⁷ Can be found at <https://github.com/mamishere/Article-Relevancy-Extraction-GPT3.5-Turbo>

focus on the use of OpenAI's GPT API models reached from the 'v1/chat/completions' endpoint, not to be confused with the GPT models behind the ChatGPT interface or the 'v1/completions' endpoint. On this note, it was unclear what exact model Syriani et al. (2023) and Guo et al. (2024) used during their investigations, whereas Gargari et al. (2024) used the same endpoint as we drew upon in this paper.

2.1 What we do differently

In this paper, we go beyond previous evaluations in multiple ways and show some key advances in using LLMs for TAB screening relative to (but possibly combined with) traditional machine learning tools. In contrast to all the previous evaluations of using GPT API models for TAB screening, we are the first to draw on function calling in the request body (OpenAI, 2024). This allows users to make prompts without the need to explicitly specify how the model shall respond to the request. The specific advance of function calling is that this permits users to make more refined and concise prompts, which, in turn, ensures that users are getting "more reliably (...) structured data back from the model" (OpenAI, 2024). We believe that the use of function calling potentially explains why we in later sections find significantly better recall performances (i.e., the ability to detect relevant studies) of using the GPT API models than previous evaluations. Differently from the previous evaluation, we have built our function calls so that they also allow the model to express its uncertainty relative to merely making binary decisions (i.e., include or exclude) as all previous evaluations have done. That is if the GPT API model, for example, does not have enough information to make a reliable decision, then the given title and abstract is added to the pool of included studies. This significantly reduces the models' ability to overlook potentially relevant studies. Moreover, we built two different types of function calls thus that users can both get simple/trinary (i.e., $1 = \{\text{include}\}$, $1.1 = \{\text{uncertain}\}$, and $0 = \{\text{exclude}\}$) and/or descriptive responses back from their screening requests. We consider the simple function call to be the main work engine. However, getting detailed descriptive responses can be pivotal especially when examining discrepancies between GPT and human screener decisions.

The main difference between this paper and previous evaluations is further that we aim to make a standardized and user-friendly workflow for how to use GPT API models for TAB screening that are easy to implement in state-of-the-art systematic reviews. We do so by developing the AIScreenR R package and technically quality-assuring it via the conduct of three large-scale classifier experiments. The AIScreenR is built as a flexible software that allows users to conduct multiple screenings simultaneously based on multiple prompts, API models, iterations of the same request, and nucleus samples (i.e., different top_p or temperature values). The software further allows the user

to send the same request (i.e., repeatedly asking the same question) multiple times to avoid random noise in the model response (especially relevant when using GPT-3.5 models). When this feature is used, the final GPT decision is based on the probability of inclusion across the iterated requests, and the specific inclusion threshold can then be determined by the user. This also allows the users to test model response consistency. Moreover, the software is built so that it draws on multi-core processing, which allows the users to speed up the timing of the screening significantly.

A fair assessment of GPT's ability to conduct TAB screening relative to humans but also to outline reliable guidelines on when (and when not) to use LLMs for TAB screening (which has not previously been done), requires a clear understanding of typical human screening performance in high-quality systematic reviews. Therefore, to make a better understanding of common human performance and to develop benchmarks that could be held against the screening performance of GPT, we mapped the human screening performance across 17 Campbell systematic reviews and 5 systematic reviews conducted by the NIPH. Relative to the previous evaluations, the contribution of this paper is, therefore, also to put forward a tentative benchmark scheme on which screening performances in high-quality reviews can be assessed.

In all the previous evaluations, multiple inclusion/exclusion criteria were added to a single prompt. Yet Gargari et al. (2024) suggested that broader and less specific prompts do not perform well in terms of finding relevant studies. Instead, concisely framed prompts with clear information seem to have a better performance. This could indicate that single-prompt TAB screening is rather restricted to only work within simple and clearly defined reviews where the inclusion of abstracts can be determined by a few inclusion criteria/questions. However, to overcome this issue, we introduce *multiple-prompt screening* and show that it can make GPT API models work within highly complex review settings. With this screening approach, we suggest making one concise prompt per inclusion (and/or exclusion) criterion. Study eligibility can then be decided based on whether a study record was included in all or close to all (say 5 out of 6) of the used prompts.

Finally, all previous evaluations were based on medical or natural science reviews, and we add to the generalizability of these results by showing that GPT API models all exhibit promising screening performance in the more wildly social science reviews as well.

3 METHODS

This section describes the metrics that we used partially to evaluate the screening performance of the GPT API models and partially to develop empirical screening benchmarks to hold against the screening performance of the used GPT API models. Moreover, the section describes the data and results we used to develop our suggested screening performance benchmarks.

3.1 Metrics we use to evaluate the performance of the GPT models

To evaluate the screening performance of the GPT API models, we used a range of different metrics. The choice of metrics was primarily informed by the recommendations made by O'Connor et al. (2019) and Syriani et al. (2023). The two main metrics we used to evaluate the performance of the GPT API models were the *recall* (by some defined as the sensitivity) and *specificity* metrics since these are intuitive to understand and interpret and are not sensitive to imbalanced data (i.e., data with a large difference in the proportion between inclusion and exclusion references, as is commonly the case in systematic reviews). The recall metric “represents the proportion of relevant records being correctly classified” (Hou & Tipton, 2024), and can be written as

$$Recall = \frac{TP}{TP + FN} \quad (1)$$

where *TP* (true positive) represents all the studies that are correctly included, and *FN* (false negative) is the number of studies falsely excluded. By contrast, the specificity metric “measures the ability to exclude all references that should be excluded” (Syriani et al., 2023), and is given by

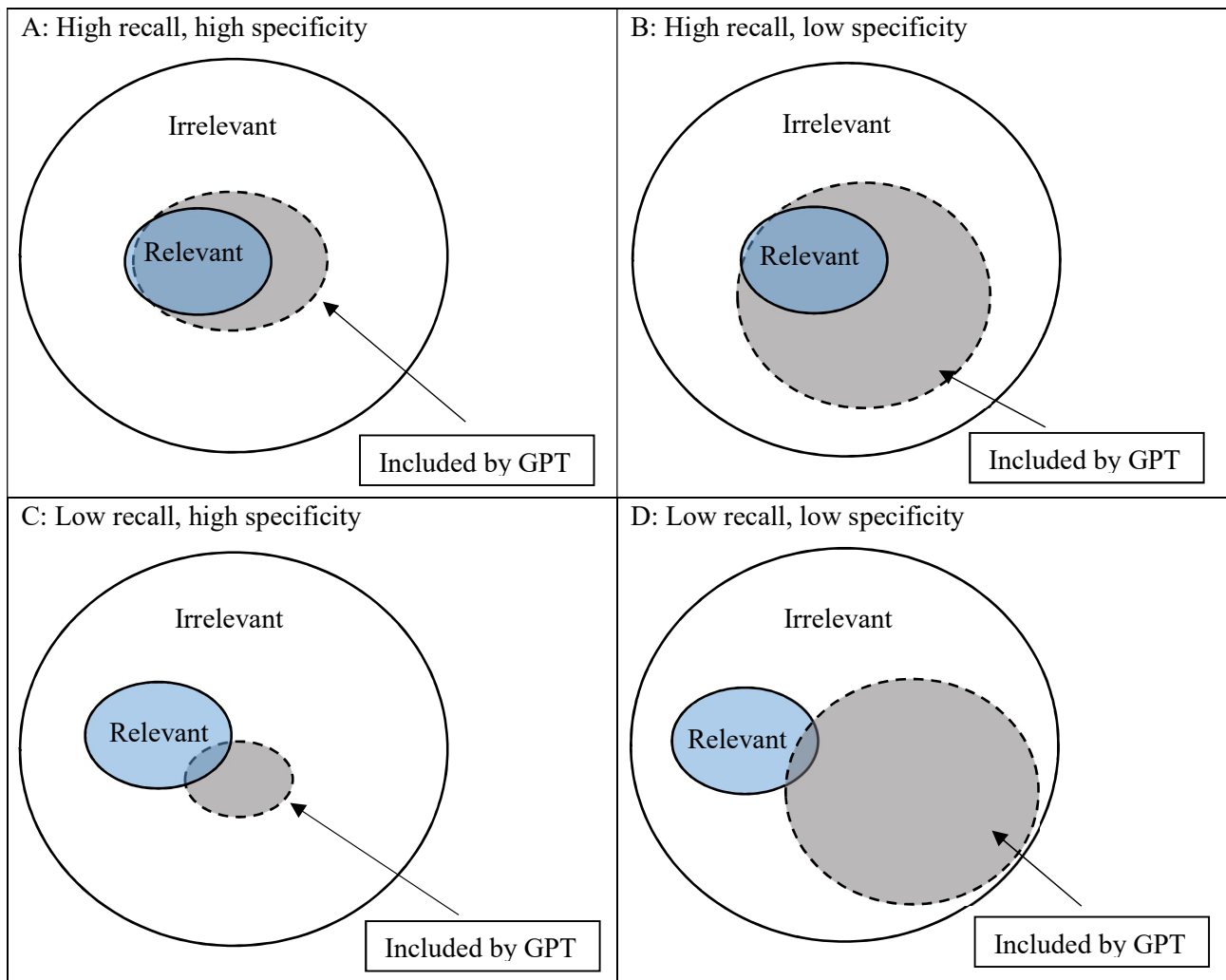
$$Specificity = \frac{TN}{TN + FP} \quad (2)$$

where *TN* (true negative) represents all the studies that are correctly excluded, and *FP* (false positive) is the number of studies falsely included. In this regard, we consider the recall measure to be the absolute most important performance measure in this use case since missing relevant studies, that is having a low recall, is the main reason for automated tools to potentially introduce a serious bias in systematic reviews (Hou & Tipton, 2024). Whereas, a low specificity is not consequential when it comes to inducing biases. It just means that reviewers must re-examine the relevancy of a larger share of the total pool of references. If reviewers can be sure that they find all relevant studies but have a specificity of say 50%, this still implies that the reviewer can safely exclude 50% of the irrelevant records, which in most instances can be considered to be a significant reduction in the screening workload. Therefore, we think that automated tools should be accepted as long as they come close to scenarios A and B pictured in Figure 1. That is, they are accepted when high recalls can be made to a large extent independently of the accordingly specificity measure. Yet, this goes without saying low

specificity rates should be accepted per se. We will come back to this aspect in the following sections.

For our benchmark development, the TP , TN , FN , and FP conditions were determined by comparing the single human screener decision with the final decision agreed upon between a minimum of two human screeners. In our classifier experiment, the conditions were determined by comparing the GPT decision with the final decision made by a minimum of two independent human screeners.

FIGURE 1: Recall and specificity performances



Note: The blue-colored circles indicate the proportion of relevant title and abstract records; the gray-colored circles represent the proportion of records included by the screener; the white circles represent the proportion of irrelevant records that are correctly excluded by the screener.

The two above metrics concern the inclusion or exclusion performances individually but it might also be desirable to include metrics that incorporate the overall performance across the

inclusion and exclusion metrics. A typical issue with such metrics is that they are very sensitive to imbalances in the data. That is for example when the proportion of irrelevant records is much larger relative to the proportion of relevant records, which is most often the case in systematic reviews. To exemplify, if one simply uses the raw agreement metric with imbalanced data then the screening performance will most often be overestimated. For example, assume that you have 10 relevant records per 1000 records, then you could end up reaching a raw agreement of 99% if the given screener just excluded all records. Although the screening performance seems to be high it obviously hides the fact that the given screener was unable to detect any relevant studies. To overcome this issue, we used two overall metrics that account for imbalances. That is *the balanced accuracy* ($bAcc$) and *the normalized Matthew correlation coefficient* ($nMCC$). The former balances the accuracy of the performance across the recall and specificity metrics and is simply an average of those metrics, and is given by

$$bAcc = \frac{Rec + Spec}{2} \quad (3)$$

The $nMCC$ metric, on the other hand, is considered to be the metric that mostly maximizes the use of the four quantities, TP , TN , FP , and FN and it has been shown to have better statistical properties than other popular metrics such as the Receiver Operating Characteristic Curve (ROC AUC) (Chicco & Jurman, 2023). It can be calculated as follows.

$$nMCC = \frac{(TP \times TN - FP \times FN)}{2\sqrt{(TP + FP)(TP + FN)(TN + FP)(TN + FN)}} + 0.5 \quad (4)$$

3.2 Human screening performance for benchmark development

In order to make fair comparisons between human and automated screening performances, we consider it pivotal to have a deep understanding of acceptable human screening performance in high-standard systematic reviews (O'Connor et al., 2019). We believe that many previous evaluations of the performance of automated screening tools to a large extent ignore the fact that individual human screening is not without significant errors either, and automated screening tools must be evaluated in light of this. If we as a community primarily assess the performance of automated tools and accept the tools with the requirement that they can detect (close to) all relevant studies in all instances or on par with very high human performances, then the tools seem by design to fail. Automated screening tools will always err to some degree, as will humans (Waffenschmidt et al., 2019), and the important factor here is to ensure that the difference between the error rates is acceptable. What is acceptable is of course up to discussion but in the next sections, we develop a tentative benchmark scheme for

interpreting (acceptable and unacceptable) error rates of screening performances in high-standard systematic reviews. The overall purpose of this scheme is to aid and establish a common guideline for evaluating automated screening results for systematic reviews across different fields of research.

3.2.1 The data underpinning the benchmark scheme

The data we used to construct this benchmark scheme was based on human screening performances in 22 high-standard systematic reviews that used independent duplicate human screening. This included 17 Campbell Systematic Reviews and 5 reviews conducted by the NIPH. A descriptive overview of all the included reviews can be found in Table 2, including the imbalance in the given dataset. The included Campbell systematic reviews, represent all Campbell reviews that have been conducted by the Danish Center for Social Science Research in which independent duplicate human screening has been used and tracked. Concretely, this data includes 144,003 title and abstract records, all of which have been double-screened by 46 individual screeners of which 36 were (student) assistants and/or non-content experts, and 10 were researchers/authors of the given review, respectively. The Campbell reviews were conducted from 2015 to 2024. Since all of the included Campbell reviews drew on assistant (i.e., non-content-expert) screeners, this could potentially downward bias the evaluation metrics for various reasons. For example, assistants might lack sufficient profound content knowledge regarding the topic under review, potentially hindering them from reaching high recall rates. Thus, their performances might not necessarily be comparable with the common screening performance of content expert screeners. Hence, we analyzed the Campbell review data separately for assistant/non-expert and researcher/expert screeners. However, relative recall and specificity rate differences between the two types of screeners could also be driven by authority imbalances between the often more senior content expert and the assistant screener, making the performances of the expert screeners look better than they actually were. Therefore, we added the screening performance data from five systematic reviews conducted by NIPH in which all TAB screenings were conducted by researchers with specific content knowledge related to the given review. This should, thereby, give a clearer picture of common expert/researcher performances in systematic reviews. This data added 13,825 title and abstract records that had been independently double-screened by 13 individual researchers. The five NIPH reviews were conducted from 2021 to 2024. When analyzing all of the above-presented data, we removed all training data to avoid inflating human disagreements. In other words, all presented screening performances represent after-training screening performances.

TABLE 2: Description of studies used to develop benchmark scheme

GPT AS SECOND SCREENER OF TITLES AND ABSTRACTS

Source Authors	Short title	<i>n</i> _{included} / <i>N</i>	Ass. ^a	Aut. ^b
<i>Campbell review</i>				
Bøg et al. (2018)	Deployment of personnel to military operations	106/2899	2	-
Bondebjerg et al. (2023)	The effects of small class sizes on students' academic achievement, socioemotional development and well-being in special education	244/11860	4	2
Dalgaard, Bondebjerg, Klokke et al. (2022)	Adult/child ratio and group size in early childhood education or care to promote the development of children aged 0–5 years	258/3667	4	2
Dalgaard, Bondebjerg, Viinholt et al. (2022)	The effects of inclusion on academic achievement, socioemotional development and wellbeing of children with special educational needs	373/14491	5	2
Dalgaard, Filges et al. (2022)	Parenting interventions to support parent/child attachment and psychosocial adjustment in foster and adoptive parents and children	424/13106	3	2
Dalgaard, Jensen et al. (2022)	PROTOCOL: Group-based community interventions to support the social reintegration of marginalised adults with mental illness	557/17614	4	3
Dietrichson et al. (2020, 2021)	Targeted school-based interventions for improving reading and mathematics for students with or at risk of academic difficulties in Grades K-6 [plus 7-12]	2952/15273	6	1
Filges, Dalgaard et al. (2022)	Outreach programs to improve life circumstances and prevent further adverse developmental trajectories of at-risk youth in OECD countries	387/4890	4	-
Filges, Dietrichson et al. (2022)	Service learning for improving academic success in students in grade K to 12	619/6269	4	1
Filges, Montgomery, et al. (2015)	The Impact of Detention on the Health of Asylum Seekers	573/10061	2	-
Filges, Siren et al. (2020)	Voluntary work for the physical and mental health of older volunteers	43/14919	2	0
Filges, Smedslund et al. (2023)	PROTOCOL: The FRIENDS preventive programme for reducing anxiety symptoms in children and adolescents	96/2745	1	1
Filges, Sonneschmidt et al. (2018)	Small class sizes for improving student achievement in primary and secondary schools	303/7802	5	1
Filges, Torgerson, et al. (2019)	Effectiveness of continuing professional development training of welfare professionals on outcomes for children and young people	298/5147	1	4
Filges, Verner et al. (2023)	PROTOCOL: Participation in organised sport to improve and prevent adverse developmental trajectories of at-risk youth	158/7021	2	1
Thomsen et al. (2022)	PROTOCOL: Testing frequency and student achievement: A systematic review	627/6239	5	2
<i>NIPH review</i>				
Ames et al. (2024)	Acceptability, values, and preferences of older people for chronic low back pain management	144/425	-	2
Evensen et al. (2023)	Sutur av degenerative rotatorcuff-rupturer [Rotator cuff repair for degenerative rotator cuff tears]	418/2499	-	4

Jardim et al. (2021)	Effekten av antipsykotika ved førstegangpsykose [The effect of antipsychotics on first episode psychosis]	73/3924	-	3
Johansen et al. (2022)	Samværs-og bostedsordninger etter samlivsbrudd [Custody and living arrangements after parents separate]	143/1525	-	4
Meneses Echavez et al. (2022)	Psykologisk debriefing for helsepersonell involvert i uønskede pasienthendelser [Psychological debriefing for healthcare professionals involved in adverse events]	45/5452	-	3

Note: *a.* Ass. denotes student/non-content expert screener; *b* Aut. denote authors of the review

3.2.2 Statistical analysis used to derive benchmarks

All statistical data analyses were conducted using R 4.4.0 (R Core Team, 2022) in RStudio (RStudio Team, 2015). For the main analyses, we used the package *metafor* (Viechtbauer, 2010), including the sandwich estimators herein (Pustejovsky, 2020). To work with *ris*-file data, we used the *revtools* package (Westgate, 2019). All materials behind this article can be accessed at <https://osf.io/apdfw/>.

From the data presented in the previous section, we estimated all the performance metrics via Equations (1) to (4). The *TP*, *TN*, *FP*, and *FN* conditions used in these equations were determined by comparing the single human screener decision with the final decision reached by a minimum of two human screeners. When working with proportion metrics such as the ones presented in Equations (1) to (3), it is usually advantageous to transform these metrics into measures that have more appropriate statistical properties. This includes having a sampling distribution that more closely mirrors a normal distribution and a variance component that can more reliably be approximated (Viechtbauer, 2022). Therefore, we used the arcsine transformation (Röver & Friede, 2022; Schwarzer et al., 2019) to calculate sampling variance and confidence intervals for the *recall*, *specificity*, and *balanced accuracy metrics*. For the balanced accuracy metric, we calculated the sampling variance of the transformed measure by using the total number of records as the sample size. We did not use double arcsine transformation (Doi & Xu, 2021) due to the inadequate properties of the back transformation of this measure (Röver & Friede, 2022; Schwarzer et al., 2019). For the *nMCC* metric, we calculated the sampling variance and confidence interval by transforming the correlations to Fisher's z-scores, as typically done in meta-analysis (Borenstein et al., 2009).

To derive the overall average performances across the *recall*, *specificity* *balanced accuracy metrics*, and the *nMCC* metrics, we fitted two versions of the so-called *correlated-hierarchical effects* (CHE) working models (Pustejovsky & Tipton, 2021). For the investigation regarding differential performances between assistant and author screeners, we applied the *subgroup correlated effects* (SCE+) model, whereas we used the CHE-RVE model when analyzing the NIPH performance

data. Both types of models account for the multi-level structure of the data with the screener performance measures nested within studies. At the same time, the models account for the correlation between the within-study performance estimates. The sample correlation, ρ , is often entirely or partially unknown and must be imputed. In all the used working models, we assumed $\rho = .7$. To guard against model misspecification both models have incorporated robust variance estimators. The main difference between the two models is that they draw on slightly different weighting schemes but the SCE model is generally recognized as the main working engine for deriving subgroup effects and conducting reliable contrast tests (Pustejovsky & Tipton, 2021). For differential effects comparisons, we used the HTZ Wald test suggested by Tipton and Pustejovsky (2015). Across both models, we estimated two sources of heterogeneity. That is the variability of the true performance differences within (ω) and between studies (τ). This allowed us to investigate at what level the largest true difference between the human screener performances existed.

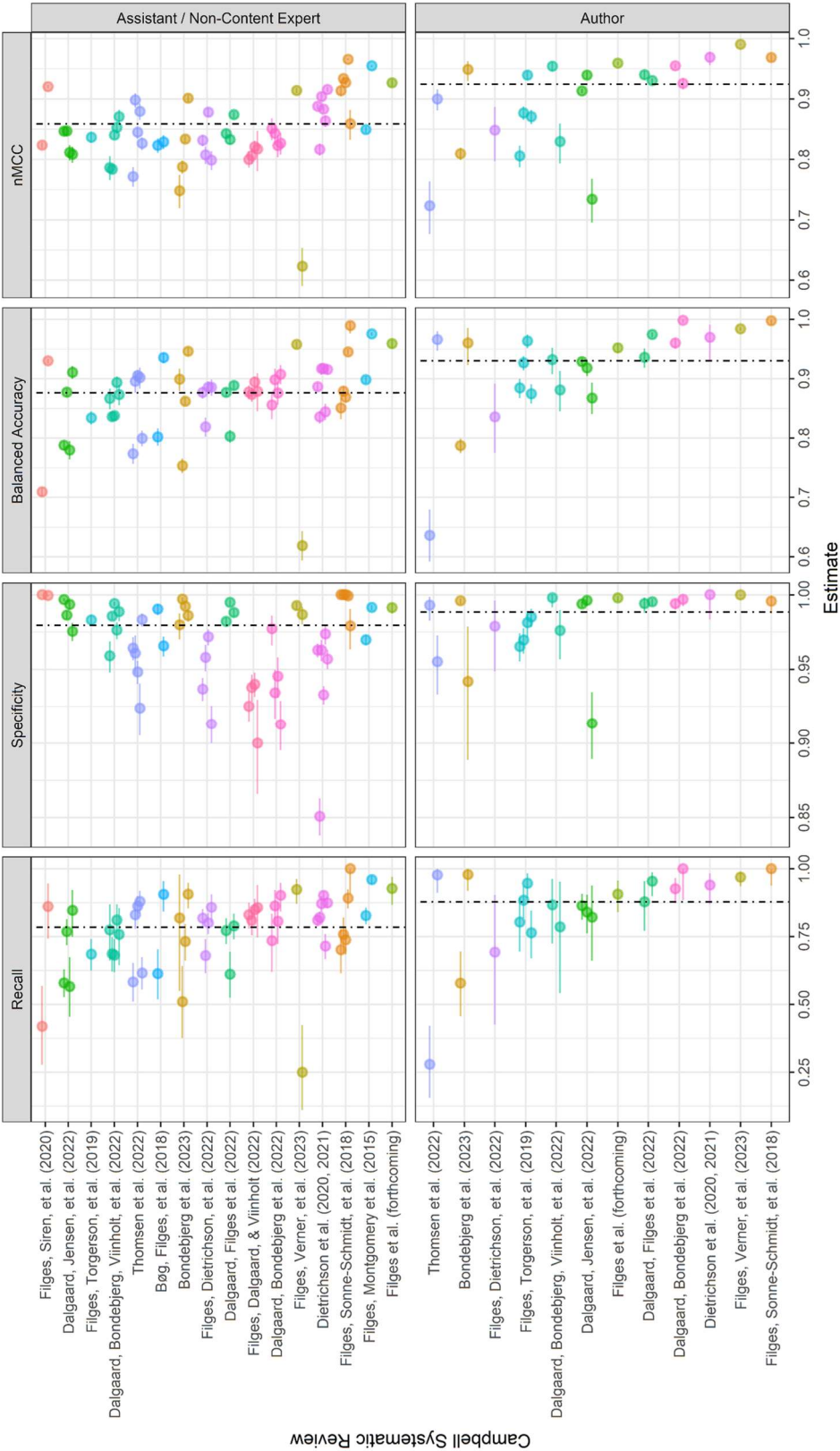
3.2.3 Results

All individual screening performances across the included reviews and how these are distributed around the overall performance means are exhibited in Figures 2 and 3. We found the overall average recall rate for the assistant and author screeners in the included Campbell reviews to be 0.782, 95% $CI[0.747, 0.817]$ and 0.881, 95% $CI[0.823, 0.931]$, respectively. Hereto, we found the two groups averaged recalls to be statistically distinct from each other with $F(1, 10.3) = 14.58, p = .003$. We detected minor substantial variations between the performance measures within studies with $\omega = 0.026$ and $\omega = 0.035$ for the assistant and author screeners, respectively. We were not able to detect any true differences in performances between studies, indicating that the average screening performance seems to be consistent across the Campbell reviews both for assistants and expert screeners. The overall average specificity for assistant screeners was 0.980, 95% $CI[0.966, 0.990]$, and for review authors 0.988, 95% $CI[0.980, 0.995]$. We found no statistically significant difference between the two average estimates with $F(1, 13.6) = 2.08, p = 0.172$. We did only find very minor non-substantial variation within and between studies with $\omega = 0.004$ as the maximum for author screeners.

For assistant screeners, the average balanced accuracy was 0.874, 95% $CI[0.857, 0.890]$, and for authors screeners it was 0.933, 95% $CI[0.899, 0.961]$. We found the difference between the group means to be statistically significant with $F(1, 10.1) = 18.22, p = .002$. Finally, the overall $nMCC$ was 0.860, 95% $CI[0.835, 0.882]$ and 0.925, 95% $CI[0.880, 0.953]$ for the assistant and author screeners, respectively. These averages were found to be statistically different from one another with $F(1, 11) = 9.65, p = .01$.

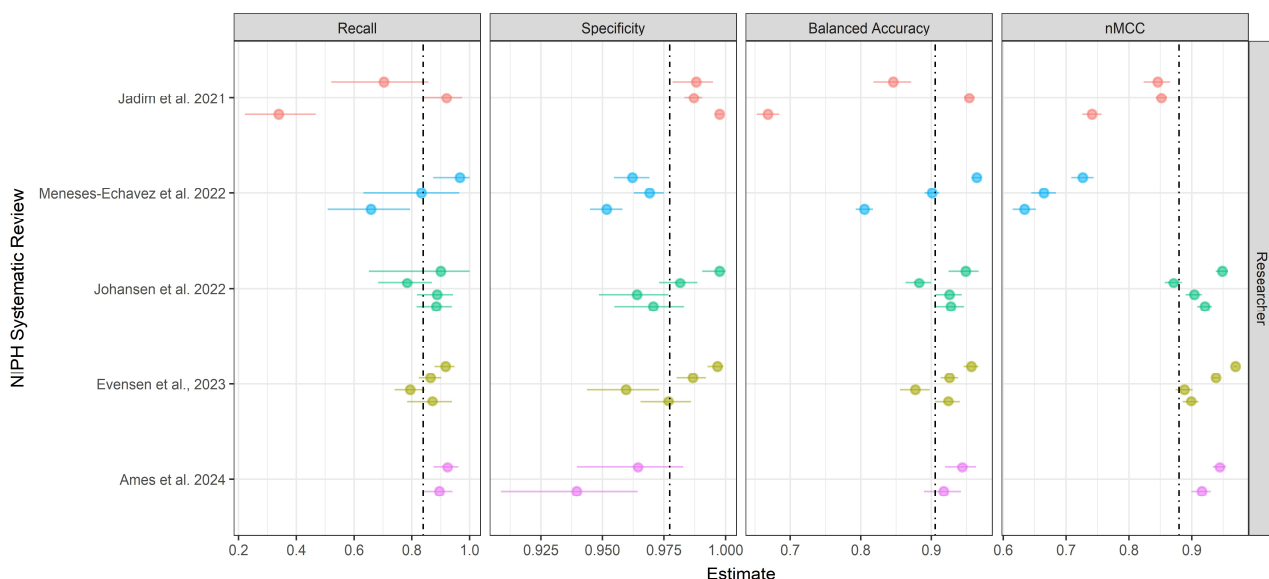
Based on these results it seems like that research screeners are substantially better at detecting relevant studies relative to assistant screeners. Yet this difference can be driven by other factors than the actual screening performance of the assistants. Interestingly, when investigating the NIPH data, which was only based on independent researcher-researcher screening comparisons, we found performance patterns closer to the performance of the assistant screeners in the included Campbell review. The overall recall rate in the NIPH data was 0.839, 95% $CI[0.737, 0.920]$. Again, we primarily found minor true variation between the screener recall performances within studies with $\omega = 0.029$ and $\tau = .0$. The overall average specificity rate was 0.977, 95% $CI[0.955, 0.992]$, with almost no true variability either at the levels of the performance measure or the study. The overall average balanced accuracy and $nMCC$ were 0.905, 95% $CI[0.859, 0.943]$ and 0.879, 95% $CI[0.720, 0.951]$, respectively.

FIGURE 2. Performance measures within Campbell Systematic Reviews across assistant vs. author screeners. Dashed lines indicate the average estimated via the SCE+ model.



Note: Dashed lines indicate the average estimated via the CHE-RVE model

FIGURE 3. Researcher-researcher screening performance measures within NIPH Systematic Reviews.



Note: Dashed lines indicate the average estimated via the CHE-RVE model

3.2.4 Benchmark scheme

Bearing on the empirical results presented in the previous section, we developed the benchmark scheme presented in Table 3. On this basis, we suggest a course-grained rule of thumb saying that automated tools that can be shown to have recall equal to or above 80% and specificity rates above 95% resemble common human screener performance. There are of course nuances to this broad guideline since we believe that automated tools can also be useful under less restrictive conditions as well. Hold against common human error rates, we consider a recall rate between 0.75 to 0.80 to be acceptable because it closely mirrors the common recall rate of assistant screeners. As a consequence, and in contrast with previous evaluations (Guo et al., 2024), we would not necessarily interpret a recall rate of 0.76 to be so low that it excludes the GPT models to function as an independent second screener. Furthermore, we believe that automated tools can still be viable even when they yield a recall rate between 50-75%. Under such conditions, the automated tool could function as an extra assurance, working as a third screener that forces the duplicate human screeners to double-check close-to-relevant study records. This would enhance the screening, ensuring that the human screeners have not overlooked any relevant records.

As can be seen from the benchmark scheme, we do not necessarily conceive a specificity of 100% to be ideal, since we think it is alright that the GPT API models are over-inclusive to some extent. This forces the reviewers to double-check close-to-relevant references which in turn assures that fewer or no relevant studies are missed. Thus, we think that a specificity rate equal to or

above 80% is acceptable as long as the recall rate is equal to or above 80% (c.f. Figure 1) as well. We, therefore, suggest that automated screening performances reaching recall and specificity rates above 80% should be accepted as independent screeners in high quality systematic reviews.

Finally, we think that automated tools that yield high recalls can be used to reduce the total amount of title and abstract records needed to be screened even if the specificity rate is below 80%. This would especially be relevant when working with very large amounts of title and abstract records (see an example of this in Shemilt et al., 2014). As a course-grained guideline, we do not consider it viable to use automated tools when they yield performance measures below 0.5.

TABLE 3: Screening performance benchmarks

Metric	Values				
	$.0 < 0.5$	$0.5 < 0.75$	$0.75 < 0.8$	$0.8 < 0.95$	$0.95 < 1.0$
<i>Recall</i>	Ineligible performance	Not ideal performance. Only use for extra security as a third screener (Can be used as second screener if resources are scarce since the alternative is worse)	On par with non-content expert screeners. Can be accepted.	On par with common researcher screening performance	Better than common human performance and traditional machine learning tools
<i>Specificity</i>	Ineligible performance	Not ideal performance. Only use to reduce the total number of records if having a high recall.	Low performance. Only use to reduce the total number of records if having a high recall.	Can be accepted if having a high recall rate above 80%	On par with common human screening performance

Note: Red areas indicate conditions under which a TAB screening performance is unacceptability low. Gray areas represent insufficient performance conditions but some applications with these performance measures are still viable. Green shaded areas represent acceptable, on par with common screening performance, or better than human screening, respectively.

With this benchmark scheme, we aim to make a more flexible tool partially for assessing the screening performance of automated tools in general and partially for assessing which screening tasks can be made under what performance conditions. This allows for more case-specific discussions regarding the adequacy of using GPT API models for TAB screening tasks in systematic reviews, avoiding rigid for and against discussions. Furthermore, we will use this benchmark scheme for interpreting our conducted classifier experiment that we present in the next section.

4 CLASSIFIER EXPERIMENT⁸

In this section, we present the data and prompts used as well as the results for three large-scale classifier experiments. Differently, from previous research, these classifier experiments aimed to test the performance of GPT API models 1) when applied in social science reviews, 2) when the request body includes the use of function calling, avoiding the need to describe response behavior in the screening prompt(s), and 3) when using multi-prompt screening in complex review settings. A side-effect of conducting these experiments was further to quality assure the AIscreenR package and ensure that the software yields appropriate screening behavior. We considered this test to be all-important if our suggested screening approach shall be scaled up. We narrowed the investigation to only include three experiments each representing different levels of complexity in terms of how well the included interventions are defined. In other words, the purpose of this paper is not primarily to show that GPT API models work in all instances. Instead, we aim to show that if configured adequately these models can function as *highly* reliably independent second screeners across various types of common systematic review questions. This also means that using GPT API models as a second screener is not always ideal for various reasons. We return to this issue in Section 5.1.

4.1 Data

In classification experiment 1, we tested the performance of AIscreenR in the context of a Campbell systematic review concerning the effects of functional family therapy (FFT) on drug abuse reduction for young people in treatment for nonopioid drugs conducted by Filges et al. (2015). By leveraging a previously published review, we were able to immediately evaluate AIscreenR's performance against the inclusion and exclusion decisions made by two human screeners during the original review. Moreover, the inclusion criteria of the review were rather simple and the intervention represented a well-defined intervention. This made it an ideal initial test case for our proof of concept purposes. Thus, if AIscreenR could not achieve satisfactory performance in this context, it would unlikely be able to do so in the context of more complex reviews. Another interesting feature of this experiment is that it was based on a highly imbalanced dataset with only 69 relevant records out of 4135 records. That amounts to an approximate inclusion ratio of 17 relevant in 1000 records. This made it an ideal case to test if and to what extent screening with GPT API models was sensitive to data imbalances as is the case with all traditional semi-automated tools (König et al., 2023).

⁸ All replication materials behind this experiments can be accessed at <https://osf.io/apdfw/>.

A critique against classification Experiment 1 is that it draws on a published open-access review, meaning that OpenAI’s GPT models can potentially have been trained on this review. If this is the case, this possibly excludes the opportunity to generalize the results of this experiment to applications where GPT API models are used to conduct screenings on prospective reviews where no previous information has been fed to OpenAI’s GPT models. To test and potentially overcome this issue, we conducted a second classification experiment drawing on data from an unpublished/ongoing systematic review. In classification experiment 2, we used screening data from a Campbell systematic review regarding the effects of the FRIENDS preventive programme on anxiety symptoms in children and adolescents conducted by Filges, Smedslund et al. (2023).⁹ This FRIENDS data in many aspects resembles the FFT data. For example, the inclusion criteria were rather simple and the intervention is well-defined. Moreover, the data is highly imbalanced with 64 relevant records in 2572 records, amounting to an approximate inclusion ratio of 25 relevant per 1000 records.

A fair critique of experiments 1 and 2 is that they both represent very simple TAB screening cases, not necessarily resembling common systematic review structures. To accommodate this issue, we, therefore, conducted a third experiment that aimed to investigate if GPT API models can be used for TAB screening in what we consider to be a highly complex review setting. For classification experiment 3, we used screening data from an ongoing Campbell Systematic Review of the effects of different testing frequencies on student achievement (Thomsen et al., 2022). We considered this to be a difficult screening case because the review draws on rather subtle inclusion/exclusion criteria that include notions that are not well-defined. One of the reasons why this particular case is difficult is that the intervention (i.e., student testing) is a type of learning strategy that is ubiquitous in education and used in a variety of ways and for very different purposes. Testing can be used as a formative tool, e.g. to promote retention of academic content, adjust instructional strategies, and uncover student needs for remediation or more intensive support. In most school systems, testing is also used summatively for assigning grades, determining graduation or certification, and for school accountability assessment. Important to note here is that the distinction between formative and summative testing is not clear-cut as tests can serve both purposes simultaneously and can have more or less stakes attached to them, both from a student and from a school perspective. It follows that testing is not a uniform type of intervention, but a multi-faceted phenomenon encompassing a variety of approaches and a heterogeneous terminology (tests are not just called tests, but may also be referred to

⁹ We conducted this experiment on the 4th of November 2023. This was before the corresponding protocol where published on the 15th of December 2023.

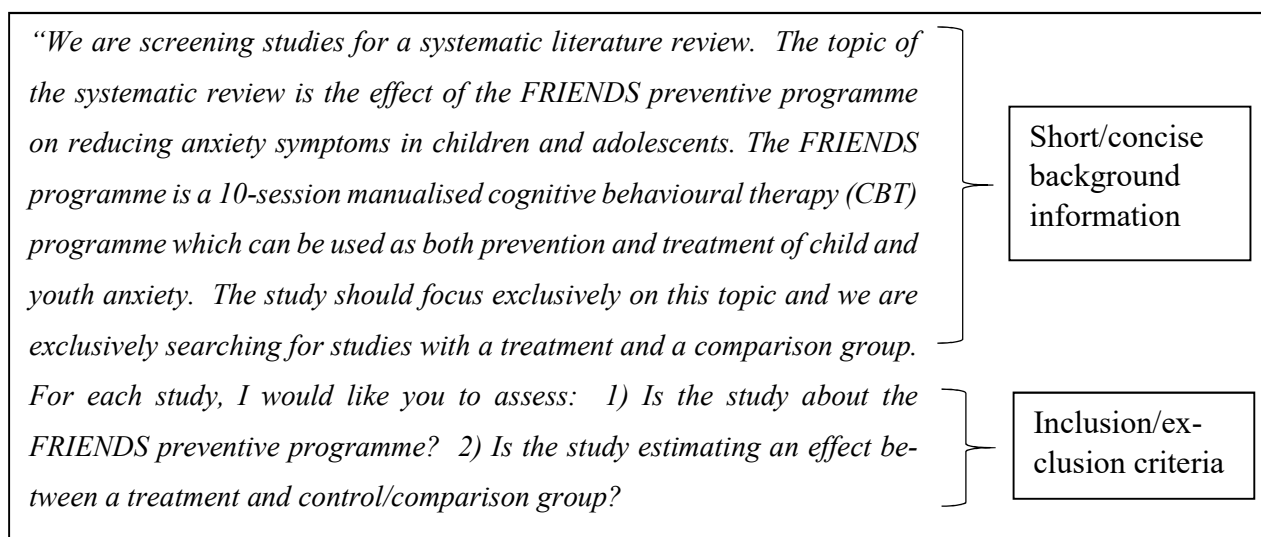
as e.g. quizzes, progress-monitoring, curriculum-based measures, and retrieval practice). Judging the eligibility of particular interventions therefore requires subject matter familiarity. Therefore, and contrary to Experiment 1, we think that if the GPT API models can achieve satisfactory performances in this context, they would likely be able to do so in most review contexts. The data we used for the experiment consisted of 2000 irrelevant and 100 relevant records randomly sampled from the total pool of 5612 irrelevant and 627 relevant records. We did so to ease the screening since this screening was based on multi-prompt screening, meaning that all title and abstract records were screened with six individual prompts each including one inclusion criterion.

For all datasets, we excluded all study records without an abstract. This excluded 208, 150, and 41 study records for the FFT, FRIENDS, and testing frequency (henceforth TF) data, respectively. For the FRIENDS data, we further deleted 20 titles and abstracts containing a myriad of special symbols, causing the GPT response to return insufficient JSON data from the server.

4.2 Prompt engineering

For Experiments 1 and 2, we engineered prompts so that they included an introduction section describing the general aim of the review followed by the inclusion/exclusion criteria of the review. To exemplify, Textbox 1 exhibits the prompt used for classifier experiment 2.

TEXTBOX 1: Prompt example



Then when given study IDs (if not provided by the user, these are automatically generated), titles, and abstracts, the AIScreenR automatically pastes together the text presented in Textbox 2:

TEXTBOX 2: End of prompt added by AIScreenR

“Now, evaluate the following title and abstract for Study [the study id is inserted here]: -Title: [the study title is inserted here] -Abstract: [the study abstract is inserted here]”

Pasting the prompt together with each title and abstract aims to guard against model drifting/hallucinations. As previously mentioned, we did not add any instruction regarding how the model should respond to our request in the main prompt, as done in previous research evaluations. Instead, we built two JSON functions providing this instruction to the model (OpenAI, 2024). This should theoretically ensure that we get more reliable and standardized responses from the models. The main JSON respond function¹⁰ we built included the instructions presented in Textbox 3:

TEXTBOX 3: Function call text

“If the study should be included for further review, write '1'. If the study should be excluded, write '0'. If there is not enough information to make a clear decision, write '1.1'. If there is no or only a little information in the title and abstract also write '1.1'. When providing the response only provide the numerical decision.”

In the initial phase of our prompt engineering, we assumed that the more detailed background information we could add to a single prompt the better the GPT API model would perform. This approach was based on the conception that the model needed to be “trained” with the correct wording. Yet, from our experience, this is a misperception of how this type of model works. As indicated in the GPT acronym, these models are *pre-trained*, meaning that they do not need to be further trained in terms of wording. Instead what they need to work properly are concise (into-the-bone) prompts. Said differently, less is more. It was not that the models were entirely off but the test performance of the models dramatically increased when given more precise prompts with fewer inclusion/exclusion criteria. Therefore, for Experiment 3, we developed and evaluated the concept of multi-prompt screening where each inclusion/exclusion criteria were prompted individually. This approach to a large extent resembles the common way screening guidelines are constructed and used by humans when conducting first-level screening of titles and abstracts (see Valentine, 2009, p. 141).

¹⁰ Find the exact functions here: bit.ly/3VI0SRp

All engineered prompts used for Experiment 3 are presented in Appendix A. We elaborate further on multi-prompt screening in Section 5.1.¹¹

4.2.1 Performance tests

Before initiating the three classifier experiments, we tested the performance of our developed prompts. For the FFT review, we started by testing the prompt on one relevant reference only. Hereto, we refined the prompt until the models consistently included this particular study record. Then, we scaled up the test to include 200 references, including 150 irrelevant and 50 relevant records. This test yielded results very similar to the ones presented in Table 4. Thereafter, we screened all records with the GPT API models to investigate whether the test performances persisted when used in the full sample of records. For both the FRIENDS and TF reviews, we tested the prompts on 150 irrelevant and 50 relevant study records randomly sampled from the total pool of irrelevant and relevant records, respectively. After detecting results very similar to the ones presented later in Table 4, we initiated the full screening.

4.3 Evaluation design

In all three classifier experiments, we evaluated the performance of the GPT API model by using Equations (1) to (3). In this regard, the TP , TN , FN , and FP conditions were determined by comparing the GPT decision with the final decision made by agreement between at least two independent human screeners. Human inclusion at this first level of screening did not necessarily imply that study records were relevant for the final review—merely that they were considered to be relevant for full-text screening. In Experiments 1 and 2, we used the gpt-3.5-turbo-0613 and gpt-4-0613 reached from the ‘v1/chat/completions/’ endpoint. Since the gpt-3.5 models are generally considered to be less accurate in their response, we repeated the same screening 10 times for each title and abstract when using this model, as also done by Syriani (2023). We did so to test its consistency across the screenings and how it impacted its final inclusion decision. The final inclusion decision of GPT was then based on the probability of inclusion across the repeated requests. In part because the GPT-4 models are considered to be more accurate (meaning that they are more consistent in their responses) and in part because of the cost, we only conducted one screening per title and abstract when calling gpt-4-0613. For Experiment 3 involving multi-prompt screening, we only drew on GPT-4 and the final inclusion decision of GPT was then based on the probability of inclusion across all used prompts. In our main analysis of Experiment 3, we included study records if they were included by GPT in at least 5 out of the 6 used prompts. For all experiments, we used invariant top_p and temperature values.

¹¹ Moreover, we show how this can be done in one of the accompanying vignette to the AIScreenR package.

That is the default value of 1 for both hyperparameters. As previously mentioned, we interpreted the results of the experiments using the benchmark scheme developed in Section 3 (cf. Table 3).

4.4 Results

All results for the three classifier experiments are presented in Table 4. As can be seen from Table 4, the gpt-4 model yielded recall and specificity rates equal to 89.9% and 93.3% in Experiment 1, which can be considered to be on par with human screening. The gpt-3.5-turbo model was also able to reach human-like screening performances. Yet these results were substantially impacted by the chosen inclusion probability threshold, indicating that these model generally yields rather inconsistent decisions, especially when it comes to detecting relevant studies. Figure 4A shows the decision sensibility of the gpt-3.5 model across inclusion probabilities for the FFT data. When setting the inclusion probability equal to 0.2 (meaning that gpt-3.5 included the study in at least 2 out of 10 screenings), the gpt-3.5 model yielded a recall of 81% and a specificity of 93.7%. However, when setting the inclusion probability equal to 0.5 yielded a performance unacceptably low compared to human screening with a recall below 80%.

When used on the FRIENDS data, the gpt-4 model performed extremely well with performance measures that can be considered to exceed common human screening performances. Concretely, it yielded a recall of 98.4% (only missing one relevant study) and a specificity rate of 97.4%. In this regard, the gpt-3.5 model performed closely on par with humans with a recall of 95.3% and specificity of 89.9% when the inclusion probability was set to be 0.7. Yet again, the performance of gpt-3.5 model was highly influenced by the chosen inclusion probability. Figure 4B shows the decision sensibility of the gpt-3.5 model across inclusion probabilities for the FRIENDS data. An impressive fact regarding these results is further that we approximately spend 5-10 minutes engineering the used prompt presented in Textbox 1. In fact, the prompt represents a first trial prompt. Unless we were extremely lucky to hit the right prompt in the first trial, this might indicate that in some applications the GPT API models are not as prompt-sensitive as often expected. Yet, we only presume this to be the case in very specific circumstances as is the case here where the screening involves a standardized intervention with a specific name.

Finally, when used on the TF data, the gpt-4 model yielded a recall performance on par with humans with a recall of 80% when studies were included in at least 5 out of 6 prompts. This, furthermore, exceeded three out of six human recalls within this review (see Thomsen et al. (2022) under column 1 in Figure 2). The model also yielded an acceptable specificity, that is a specificity of ~84%. Relative to human performance, the model in this case was rather over-inclusive. Yet again,

we do not necessarily consider this to be disadvantageous, since it provides extra insurance of not overlooking relevant studies, which might be even more important in complex review settings where hard decisions are difficult to make at the first level of screening due to insufficient information in the abstracts. Since these results are underpinned by data from an unpublished review, a side-effect of this experiment is further that it shows that GPT API models can work in complex settings where we can be quite sure that the GPT models as not been trained on review data. Moreover, when studies were included in at least 3 out of 6 prompts, the gpt-4 model was able to reach a recall of 95%, but the specificity was quite low, that is 67%. However, if this approach was used it could potentially, and most importantly safely, reduce the total screening workload by approximately 64%.

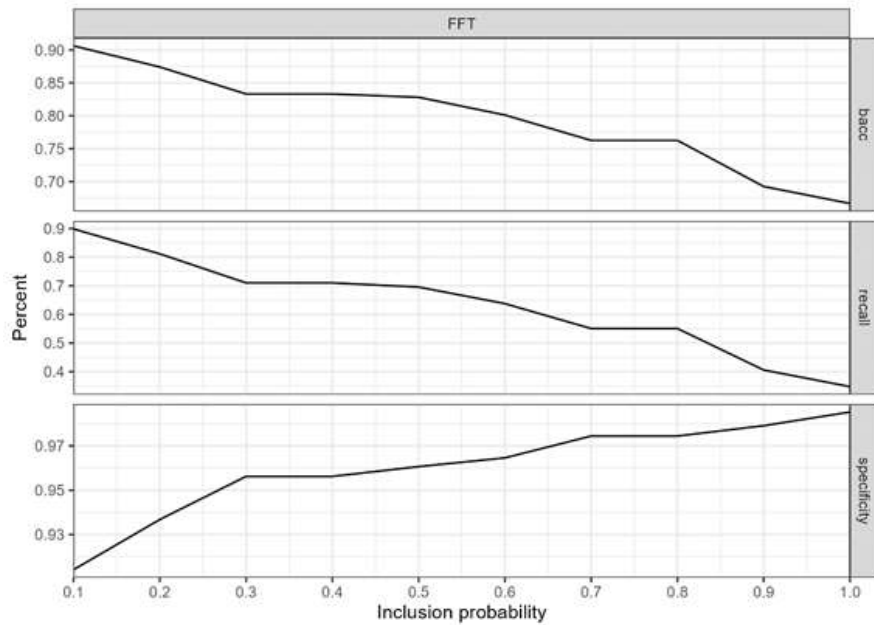
TABLE 4: Results of the two classifier experiments

Review Model	Reps Per Prompt	Recall (%) [TP/(TP + FN)]	Specificity (%) [TN/(TN + FP)]	Raw agreement (%) [(TP + TN)/N]^a	bAcc (%)
<i>FFT</i>					
gpt-3.5-turbo-0613 (incl. prop = .5)	10	69.9 (48/69)	96.1 (3906/4066)	95.6 (3954/4135)	82.8
gpt-3.5-turbo-0613 (incl. prop = .2)	10	81.2 (56/69)	93.7 (3809/4066)	93.5 (3865/4135)	87.4
gpt-4-0613	1	89.9 (62/69)	93.7 (3810/4066)	93.6 (3872/4135)	91.8
<i>FRIENDS</i>					
GPT-3.5-turbo-0613 (incl. prop = .5)	10	95.3 (61/64)	81.3 (1918/2508)	81.6 (2100/2572)	88.3
gpt-3.5-turbo-0613 (incl. prop = .7)	10	95.3 (61/64)	89.9 (2254/2508)	90.0 (2315/2572)	92.6
gpt-4-0613	1	98.4 (63/64)	97.4 (2442/2508)	97.9 (2518/2572)	97.9
<i>TF</i>					
gpt-4-0613 (incl. ≤ 5 out of 6 prompts)	1	80 (80/100)	83.8 (1676/2000)	83.6 (1756/2100)	81.9
gpt-4-0613 (incl. ≤ 4 out of 6 prompts)	1	89 (89/100)	74.3 (1486/2000)	75 (1575/2100)	81.6
gpt-4-0613 (incl. ≤ 3 out of 6 prompts)	1	95 (95/100)	67 (1340/2000)	68.3 (1435/2100)	81

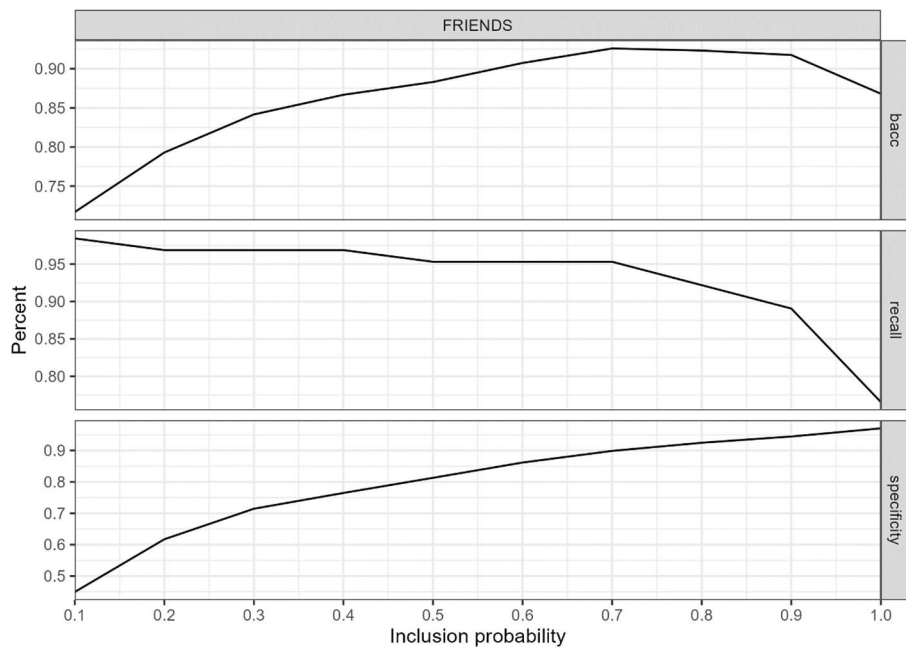
a: N is the total number of references

FIGURE 4: Decision sensibility of the gpt-3.5-0613 model across inclusion probabilities

A



B



Note: The inclusion probability on the x-axis is calculated from the number of times the given title and abstract record was included over the 10 repeated requests. For example an inclusion probability of 0.1 means that the record was included in 1 out of 10 requests.

To summarise, we derive the following conclusions from the classifier experiments. First, we found that GPT API models can work as highly reliable and independent second screeners

with recall performances on par or even better than common human screeners, even in very complex screening settings. This finding contrasts previous evaluations (Gargari et al., 2024; Guo et al., 2024) finding that the GPT API models mainly have high performances in terms of correctly excluding irrelevant records. This discrepancy might be explained by the fact that we drew on function calling aiming to provide more reliable responses from the GPT API models and that we used multi-prompt screening, circumventing that all inclusion/exclusion criteria were added to a single prompt. Second, and in contrast with the performance of classical semi-automated screening tools (König et al., 2023), we partially found that GPT API models are not sensitive to imbalanced data and partially that the GPT-4 API models are capable of reaching recall rates close to 100%. Third, since we used the AIscreenR software to conduct all classifier experiments, we feel safe to conclude that the software seems to work as expected. Hence, we believe that reviewers can safely use this software in high-standard systematic reviews. Fourth, we found that the GPT API models are not always as prompt-sensitive as suggested in previous evaluations (Gargari et al., 2024). Fifth, we found the GPT-4 API model to be preferable relative to GPT-3.5 since the latter is rather sensitive to the chosen inclusion probability across multiple identical screenings. Based on this finding, we generally recommend not using the GPT-3.5 API models when GPT-4 API models are available. Finally, we found that in some applications, the specificity rate reached by the GPT-4 API model can be seen to be on the lower end compared with human screeners. Yet, we do not find this to be a major issue when having high recall rates (cf. Figure 1) since this can just be seen as an extra opportunity to double-check close-to-relevant studies. Thus, enhancing the change of not overlooking any relevant study records.

Overall, we think that using GPT API models for TAB screening tasks in state-of-the-art systematic reviews has huge potential—also as independent second screeners in complex reviews. Furthermore, we believe that the relevancy of using LLMs will only increase over time as the models improve, which demands a standardized setup to ensure reliable use of these in systematic reviews. In the next section, we, therefore, develop a tentative guideline and workflow for how such screening can be set up in practice.

5 TENTATIVE GUIDELINES AND WORKFLOW

Premised on our developed benchmark scheme, our experience, and the results of the three classifier experiments, we have developed the following tentative guidelines and workflow for when and how GPT API models can be used as independent second screeners of titles and abstracts. All steps in this process are fleshed out in Table 5.

TABLE 5: Workflow for how to conduct TAB screening with GPT API models

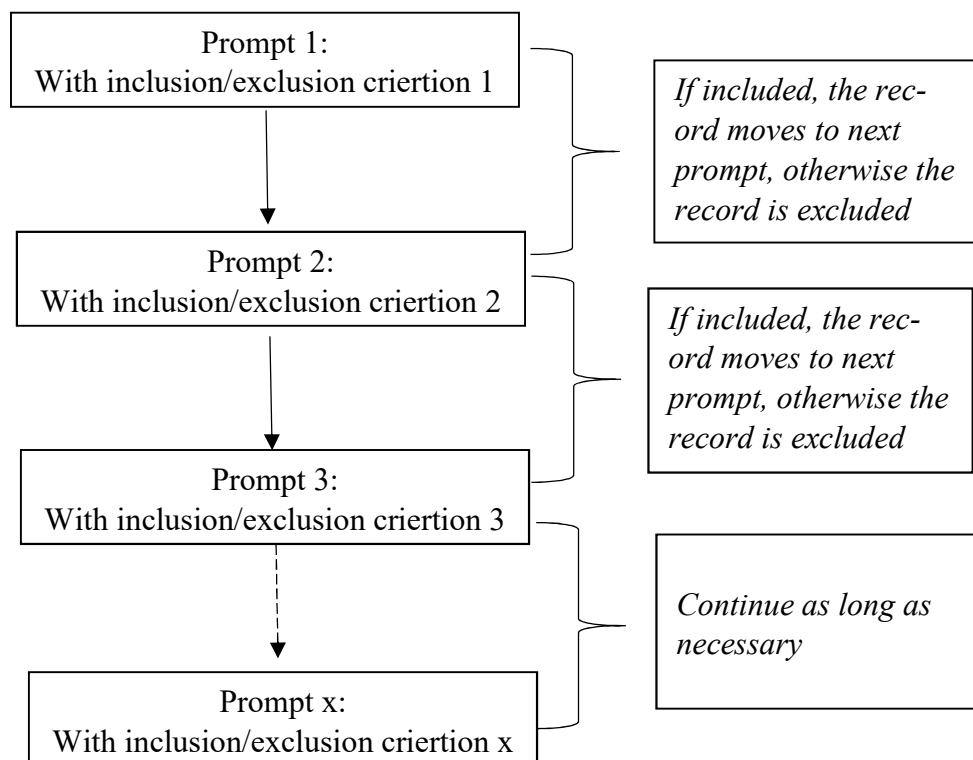
Step	Reviewer action
1	Find approximately 10 relevant study records (ideally more).
2	Find a minimum of 200 irrelevant study records (ideally randomly sampled from the pool of records).
3	Construct the test dataset by combining the records from steps 1 and 2.
4	Develop one or multiple prompts and test the(ir) performance.
5	Repeat/refine step 4 until reaching a recall close to 80% or more, and a specificity equal to or above 80% (ideally between 90-100%). If this step cannot be fulfilled, we recommend <i>not</i> to use the GPT API model as a second screener. Thus, human double screening is the ideal solution. Yet, the GPT API model can still be used as a third screener for extra insurance of not missing any relevant studies. In cases where low budgets exclude human duplicate screening, we considered it fair to work with recall performances below 80% since the alternative (i.e., stand-alone single-screening) in these cases is worse.
6	Manually single screen all study records (could be divided into batches of 500-1000 study records). If a GPT API model has shown to be a reliable second screener based on the text data, then this can be done by multiple reviewers/screeners.
7	Download ris-files individually for included and excluded references. Load this data into R and track the human decision.
8	Run the full TAB screening with the GPT API model. Consider removing all study records without an abstract and human screen those references.
9	Investigate and solve disagreements between the human and automated screening decisions.

Note: See the vignette accompanying the AIScreenR package for a detailed presentation of the to conduct TAB screening with GPT API models in practice.

Before initiating a full-scale TAB screening with GPT API models, we generally recommend thoroughly testing the screening performance of the prompt(s) and GPT API model aimed to be used for the screening until it is ensured that the screening performances pass certain thresholds within the training setting. The first step of the testing procedure involves locating approximately 10 relevant and 200 irrelevant study records including titles and abstracts, respectively. Locating more than 10 relevant study records might be ideal to test if the prompt(s) can detect various types of

relevant records. That said, we experienced that using fewer than 10 relevant records could also unveil a proper recall performance of the prompt and models in more simple screening cases. Consequently, we cannot set this step in stone. When locating irrelevant records, we suggest randomly sampling those from the total pool of records. This aims to ensure that the specificity test rate can be generalized to the full sample of study records. After having collected the training dataset composed of the relevant and irrelevant study records, the next step concerns prompt engineering. A key part of developing well-performing prompts entails making them as concisely written as possible, only feeding them with the absolute most necessary information. Importantly to remember, these models do not need to be trained, as we initially were caused to believe. Thus, if conducting a complex review including many inclusion/exclusion criteria, we suggest conducting what we have coined multi-prompt screening. That is screening with multiple prompts, where each inclusion/exclusion criteria should be prompted individually. All title and abstract records are then screened with all prompts. Alternatively, one could conduct what we define as hierarchical screening where a study record is excluded if it is excluded at any step in the multi-prompt screening. This procedure is depicted in Figure 5¹².

FIGURE 5: *Hierarchical screening*



¹² To support this type of screening, we show how this can be practically executed in the accompanied vignette to the AIScreenR.

If using hierarchical screening, we suggest ordering the prompts so that the prompts excluding the largest body of references appear first and prompts with more specific inclusion/exclusion criteria following thereafter (Brunton et al., 2017). This approach will be more efficient both in terms of money and time. A further side-effect of this approach is that all title and abstract records will be mapped on what exact inclusion/exclusion criteria they were excluded upon. However, a shortage of this screening approach is that it is heavily dependent on the quality of the used prompts. Although more costly, we, therefore, recommend using multi-prompt screening where all title and abstract records are screened with all prompts, since this approach potentially guards against insufficient prompting. Assume for example that one made seven prompts, one for each of the inclusion/exclusion criteria, but one of the prompts wrongly excludes a large share of relevant records at the early stage of the screening when scaled up from the test setting. Then those studies would be lost in the hierarchical screening suggested in Figure 5. If instead all records had been screened with all prompts then one could avoid the above bias by including all records that were included in 6 out of 7 prompts, as we did in classifier experiment 3, for instance. This approach is less sensitive to the order of the prompts in the hierarchy.

When engineering prompts, we suggest that these should be re-written/refined until they reach recall and specificity rate thresholds of at least 80%. Recall rates between $75\% < 80\%$ can also be accepted, but the reviewers should try to increase this performance as much as possible. Lower specificity rates can also be accepted as long as the recall exceeds 80%. If the specificity rate of 80% cannot be reached, then the GPT API models should mainly be used to reduce the total number of study records needed to be screened by two independent reviewers. More importantly, we suggest that if a recall rate of 80% cannot be reached, then the given GPT API model should not be used as an independent second screener. This can only be accepted if the given reviewer lacks financial resources since single-screening is less desirable than using a bad-performing GPT API model as an extra assurance of finding all relevant studies. However, the reviewer must be earnest about this shortcoming of the screening, and we do not think this should be accepted in state-of-the-art reviews. Alternatively, if the thresholds cannot be reached, the GPT API model can still be used as a third screener, again as an extra security for detecting all relevant studies.

When the test has been passed, and the reviewers have decided to leverage the GPT API model as second screener, we suggest that the reviewers screen all study records before initiating the automated screening. Thereby, it is prevented that the human reviewers are impacted by GPT's deci-

sions. In other words, we recommend that decisions on whether GPT API model screening is appropriate in a given review should primarily be made before the main TAB screening has been initiated. An alternative to manually screening all records at once is that reviewers repeat steps 6 to 9 in Table 5 with batches of 500-1000 study records. This would be an adequate way to steer the screening process and to continuously ensure that the given GPT API model performs as expected. Moreover, this reduces the risk of running large screenings that break for some technical reasons, which in turn hinders unnecessary money waste.

When all study records have both been screened by human and automated screeners, reviewers should investigate and solve disagreements. In this regard, it can be advantageous to re-screen all study records where humans and the automated screener disagreed to test the consistency of the automated screening decision but also to get detailed responses for GPT's decisions. For the latter purpose, we mainly recommend using the GPT-4 model since it provides substantially better descriptions of its screening behavior. If the specificity performance of the GPT screener is high (e.g. $> 99\%$), the reviewers can consider just letting all study records that have been included by either human or GPT enter the full-text screening stage. Whether this is viable of course depends on the number of records needed to be screened.

5.1. When not to use GPT API models for TAB screening?

Although we think that GPT API models can have a huge impact on TAB screening in systematic reviews, we can envision at least two cases, beyond when the test performance thresholds are not met, where we find this screening approach to be inappropriate. That is, for example, when the complexity of the review question(s) or/and inclusion/exclusion criteria is high and the number of reference records needed to be screened is low (e.g. < 2000). In such a situation, it might take longer to construct reliable prompts than instantly initiating the duplicate human screening. In general, we think that when having few records, it is better merely to let humans double-screen all records because it is more time-efficient relative to engineering well-performing prompts. That said, we experienced that we were able to set up a reliable screening with the FRIENDS data within a few hours. Therefore, when having few records and the complexity of the review is low, it can be advantageous to conduct a rapid investigation of whether GPT API model screening is appropriate in the specific case. However, we do not think reviewers should spend/waste too much time on this task in such cases. Table 6 describes under what conditions, we consider it adequate and inadequate to use GPT API models for TAB screening tasks in systematic reviews.

TABLE 6: When to use GPT API models for TAB screening

	Number of studies		
		Low	High
Complexity of the review question(s) and/or inclusion criteria	Low	Questionable whether the time is worth investing in prompt development relative to merely initiating human screening	GPT screening is likely well-suited.
	High	Apply duplicate human screening	GPT screening is potentially well-suited. Consider using hierarchical or multi-prompt screening

6 LIMITATIONS

Although we have strived to make a comprehensive evaluation of the use of GPT API models for TAB screening tasks, our study has some important limitations. First of all, none of our analyses were pre-registered. However, to at least ensure openness and thereby make it possible to replicate our work, we have shared all data, codes, and material behind the analyses conducted in this study. It can be accessed at <https://osf.io/apdfw/>. A clear limitation of the shared material is that it will not necessarily be possible to make exact replications of our results since minor model decision deviations can appear from screening to screening. Yet, we still firmly believe that the overall patterns of our results can be replicated, and we gladly invite readers to test this hypothesis. Moreover, it is important to note that humans would probably also change their screening decisions if they had to reiterate their first TAB screening. Therefore, we consider these minor discrepancies to be very human-like and acceptable. Nonetheless, in future applications, reviewers will be able to set a specific seed (currently a beta argument) to the request body ensuring the reproducibility of the given screening. We did not use this functionality since it was not developed at the time when we ran our experiments but we considered it to be a helpful feature for future applications.

Another clear limitation is that the models we drew on in this paper represent black box and closed-source algorithms. Though, we can show that they can be used for TAB screening tasks at the current state of time, and with the current models, we are not able to say anything about why the models work. Model dependency is a major issue when working with GPT API models since we

do not know how they are trained and/or will develop. This also means that the generalizability across different models and over time is unclear. Consequently, we cannot infer that the results of our experiments are generalizable to other GPT-4 models such as the GPT-4o or the GPT-4-turbo, and, more so, to other models such as the API models from Claude or Mistral AI. From a scientific point of view, and to increase the transparency of the GPT API screening, it is, therefore, important that future research revolve around investigating the performance of local, open-source, and downloadable models such as the ones provided by Mistral AI.

On a similar, but technical line, it is furthermore extremely demanding and time-consuming to keep up with new model developments and updates as well as how they are reached via the API. Model deprecation is a serious threat to the validity of our suggested approach. For now, the GPT-4-0613 model is stable but we expect that this model will eventually deprecate as with previous models. Already, the original function calling arguments have been deprecated and moved to the tools argument. Therefore, future research must evaluate whether our results can be exerted with other GPT API models such as updated models as well as the GPT-4o and GPT-4-turbo, etc. Likewise, the GPT-3.5-turbo-0613 model that we drew upon is expected to deprecate during 2024, so eventually one cannot replicate the screening we have made with this model. On this note, we think it is pivotal that future research investigates if and how downloadable GPT models perform such as the ones provided by Mistral AI. This would secure a more stable applicability of using GPT models for TAB screening, supporting the functionality of this technology. That said, we do think it is also important to note that most human duplicate screenings represent black box operations that are hardly replicable, and we believe the GPT API models should be judged in light of this. To be clear, this goes without saying that we should not strive to make screening replicable and reproducible since this increases the transparency of high-quality reviews. We just do not think that the black box argument should be a major reason for not drawing on GPT API models for TAB screening.

Even though, the use of GPT API models as second screeners can be considered to be more efficient than using a human screener, reviewers should be aware that it still can induce a significant cost to one's project to conduct GPT API model screening, especially when working with GPT-4 models and multi-prompt screening together. To exemplify, we spent approximately \$220 making the 12,600 screening requests (2100 references x 6 prompts) for the third classification experiment. This can potentially limit the use of GPT API model screening. Therefore, we recommend using the models carefully and in some applications, it might be advantageous to combine traditional classifier tools with LLMs such as the GPT API models to reduce the total cost. In extreme-size

reviews (i.e., $100,000 < \text{references}$), reviewers could consider combining priority screening/classifier modeling with the GPT API screening. For example, the GPT API screener could be used as an extra guardian, checking the performance of one's selected stopping rule (Boetje & van de Schoot, 2024; Campos et al., 2023; König et al., 2023) either by screening a subsample of 1000 references on the wrong side of the set threshold or by randomly sample 1000 references from the pool of studies considered to be irrelevant. Then all references on the right side of the threshold of the stopping rule could be screened by at least one human and the GPT screener. This is just one idea of how a more cost-efficient screening could be set up in very large-scale reviews, and reviewers could play around with other solutions as well. Hereto, it is important to stress that we do not think that traditional automated tools and the GPT API screener should be seen as two competing tools. Instead, they should be used together to overcome each other's disadvantages. Furthermore, we believe this deficit of prizing will be negligible over time as the models get cheaper and more local models become available. For now, it might be beneficial for future research to investigate the performance of GPT-4o or GPT-4-turbo since these models are significantly cheaper than the GPT-4 model we used.

The screening approach that we suggest is limited by its prompt dependency, meaning that this screening approach is in theory rather sensitive to the prompt(s) made by the user. This can potentially complicate the leverage of the GPT API screening and make it time-consuming to build well-performing prompts. Reviewers must, therefore, always make thorough consideration of whether the use of GPT API screening is resource-efficient in the given review case. On the same line, a key reason for us to develop the screening benchmark scheme is also that it guides reviewers on when GPT API screening might not be appropriate. Most importantly that is when prompt performances are not on par with human screening. This aims to hinder reviewers from using bad/sensitive prompts and thus avoid inducing a recall bias in systematic reviews.

Although we have strived to make a user-friendly setup for GPT API screening, a prominent limitation is that our screening approach is function-based, meaning that reviewers need to require minor R coding skills. Thus, a more generic solution might be favorable. For example, embedded in a shiny app or by incorporating this approach directly in screening tools similar to what has been done with the data extraction GPT API tool in the EPPI-Reviewer (EPPI-Centre, 2024). A tempting solution to accommodate user-friendliness is to copy our approach to the ChatGPT interface. However, it is pivotal to stress that we have not been able to replicate any of the screening results we obtained from the GPT API models. Consequently, we recommend reviewers not use the ChatGPT

for title and abstract screening before more appropriate performance of this approach has been validated.

Finally, several caveats should be mentioned regarding the data underlying the benchmark scheme that we have developed for interpreting screener performances in high-quality reviews. That is, it is based on screener performances deduced from a convenient sample of systematic reviews, possibly restricting the generalizability of the estimated average screening performance measures. Even so, we believe that the screening performance measures provide key insights regarding what has previously been accepted in high-standard reviews, and they indicate that the screener performances seem to be comparable across distinct disciplines. Yet, future research may usefully investigate common screener performances more systematically and across various research fields such as medicine and social science to make even more refined screening guidelines.

8 CONCLUSION AND DISCUSSION

Human duplicate title and abstract (TAB) screening in systematic reviews is time-consuming, requiring a substantial amount of human labor which decelerates the review process and thereby the dissemination of important knowledge for practice, research, and policy. In this study, we have shown that OpenAI's GPT API models can function as highly reliable second screeners even in complex review settings, making it possible to substitute one human in the duplicate screening process and reallocate human resources. Our findings suggest that when configured correctly GPT API models can perform on par with or even surpass human screeners with regard to finding relevant studies. Hereto, we found that the GPT-4 model outperforms the GPT-3.5-turbo model. Therefore, we recommend primarily using the GPT-4 model for GPT API screening. Moreover, we found that GPT API models yield a specificity rate on par with humans, but in some applications, they appear slightly over-inclusive relative to human screeners (i.e., they yield lower specificity rates than typically human screeners). However, we do not necessarily consider this to be a deficit of the models as long as they also contain high recall rates. This merely forces the human reviewers to double-check close-to-relevant records, which ultimately guards against overlooking relevant studies. In general, our results stand in contrast to most previous research (Gargari et al., 2024; Guo et al., 2024), finding that GPT API models mainly perform adequately in terms of excluding irrelevant studies. On the one hand, this discrepancy might be explained by the fact that the GPT API models we used drew on function calling (OpenAI, 2024), which improves the models' response consistency. On the other hand, all previous research added all inclusion/exclusion criteria to the same prompt, thus the discrepancy might be

explained by the fact that we invented the use of multiple-prompt screening which is based on making one concise prompt per inclusion/exclusion criteria of the review. This approach was especially decisive for making it work in complex review settings.

Based on our findings, we believe TAB screening with GPT API models can revolutionize the way duplicate title and abstract screening can be done in high-quality systematic reviews and make automated tools move up to the highest levels of automation (c.f. O'Connor et al., 2019), where they yield none human-assisted *second* screener decisions. However, this necessitates the need to standardize this screening approach to make it scalable and acceptable in high-quality reviews. Therefore, we also developed a reproducible workflow and tentative guidelines for when such screenings can be accepted in high-quality reviews. Moreover, we have written the AIscreenR R package, allowing reviewers to draw on features such as function calling (i.e., making prompts without the need to explicitly specify how the model shall respond to the screening request) as well as multi-core processing that can speed up screening significantly. A key part of setting up a reliable GPT API screening is to conduct thorough pilot testing of the screening prompt(s) before making any full-scale screening. For such assessments, we developed a new benchmark scheme for interpreting acceptable and unacceptable screening performance in high-quality reviews based on the typical screening performance found in 22 high-standard systematic reviews across 157,828 duplicate human screening decisions. Deduced from this investigation, we suggest that automated screenings yielding a recall rate (i.e., the ability to correctly include relevant studies) above 80% during the pilot testing should be acknowledged as being on par with human performance and can be safely used as independent second screeners. This recommendation approximately resembles the average recall rate we found in the mapped high-quality reviews. In addition, we suggest that a specificity rate (i.e., the ability to correctly exclude irrelevant studies) equal to or above 80% should be accepted in high-standard reviews as long as the recall is equal to above 80% as well since a low specificity rate does not induce any biases. As previously mentioned, it just requires humans to double-check close-to-relevant records, which guards against overlooking relevant studies. In this regard, it is important to note that since our recommendations are based on the acceptance that the GPT API models can err to some degree (as accepted for human screeners), it is of vital importance that GPT API screening is combined with other screening techniques such as forward and backward citation tracking to ensure that potentially missed studies re-enter the review. Although we allow for minor errors, we recommend not to use GPT API screening, if reviewers can reach satisfying recall and specificity rates. On a similar line, we never think a GPT API model should be used as a stand-alone screener. There must

always be a human in the loop, meaning that humans must always take the role of the first screener of titles and abstracts in high-quality systematic reviews.

Some caveats and limitations follow our work. First of all, we agree with Schoot et al. (2021) that transparency and reproducibility represent the highest scientific standards. Yet, OpenAI's GPT API models are based on black box algorithms. Nonetheless, we do not believe that this argument should prevent reviewers from using OpenAI's GPT API model for TAB screening seen in the light of the fact that human screening most often represents black-box operation as well. However, we consider it all-important that future research investigates the performance of alternative open-source GPT models. A side-effect of such research would further be that the cost of using GPT models can be substantially reduced which can be a major barrier to using GPT-4 models for TAB screening at the current point in time. These models are still rather expensive (in absolute terms, not compared to hiring a human screener). Thus, another line for future research could be to investigate the performance of cheaper GPT-4 models such as GPT-4o and GPT-4-turbo.

A general challenge when using GPT API models is that it requires a substantial amount of software maintenance to keep up to date with the newest model developments. Therefore, it requires continuous software development, for this screening approach to be viable which, in turn, will probably require collaborations in the research community to ensure the stability of the software over time.

Although this study has some important limitations, we believe that the implications of this work are rather extensive beyond what we have presented. First, using well-functioning automated tools renders the possibility for reviewers not to make unnecessary restrictions on their search string to steer the number of study records, which, in turn, increases the likelihood of finding all or close to all relevant studies for the review. Moreover, it makes it possible to screen extreme-sized reviews (Shemilt et al., 2014, 2016) that would otherwise have been considered unmanageable by humans. Second, this approach can be all-important in elevating the quality of reviews conducted by single researchers restricted by resources such as low budgets and/or time. Third, we believe that there exists a huge potential in combining traditional automated tools and GPT modeling. For example, GPT API models could play a key part in validating a decided stopping rule (Campos et al., 2023; König et al., 2023) whereto it could partly be used to screen records close to the stopping rule on the wrong side, and partly be used to more precisely detect relevant studies on the right side of a given stopping rule, ensuring that humans did not overlook any relevant studies. Combining traditional tools and GPT screening could furthermore reduce the cost of using GPT API models. Another application

could also be that GPT API models are used together with prioritization resampling algorithms such as the one suggested by Hou and Tipton (2024) to come closer to reaching recall rates closer to 100%, which are generally considered unattainable when using stochastic algorithms. Fourth, even if reviewers prefer to use duplicate human screening, we think that using a GPT API model as a third screener would be valuable since it can guard against missing relevant studies due to human screener drifting.

To recapitulate, we believe that using GPT API models can change duplicate TAB screening in high-quality reviews across all kinds of scientific disciplines. In fact, we envision that the GPT-4 models will perform even more adequately when used on more structured abstracts as typically found in medicine. Moreover, we think this is an ideal use case where artificial intelligence (AI) can meaningfully take on tedious human labor, and where no legal issues arise. Even more edifying, GPT API model screening can ensure a more rapid transfer of key knowledge to research, practice, and policy, which ultimately underpins the core rationale for doing systematic reviews.

ACKNOWLEDGEMENT

Thanks to Jens Dietrichson, Trine Filges, Tiril Borge, Heather Melanie R. Ames, and Christopher James Rose for valuable comments and sharing of screening data. Also thanks to Sofie Elgaard Lisager Jensen and Johan Klejs for testing the AIScreenR software and for valuable inputs to the workflow.

FUNDING STATEMENT

This manuscript was funded by VIVE Campbell, Denmark

DATA AVAILABILITY STATEMENT

To adhere to the reproducibility framework proposed by Olorisade et al. (2017), replicate codes can be found at OSF bit.ly/3spivoG:

CONFLICT OF INTEREST STATEMENT

The authors declare no conflict of interest.

REFERENCES

* marks studies used for the benchmark development

- Alshami, A., Elsayed, M., Ali, E., Eltoukhy, A. E. E., & Zayed, T. (2023). Harnessing the power of ChatGPT for automating systematic review process: Methodology, case study, limitations, and future directions. *Systems*, 11(7), 351.
- Ames, H., Hestevik, C. H., & Briggs, A. M. (2024). Acceptability, values, and preferences of older people for chronic low back pain management; a qualitative evidence synthesis. *BMC Geriatrics*, 24(1), 1–22. <https://doi.org/10.1186/s12877-023-04608-4>
- Boetje, J., & van de Schoot, R. (2024). The SAFE procedure: a practical stopping heuristic for active learning-based screening in systematic reviews and meta-analyses. *Systematic Reviews*, 13(1), 81.
- Bøg, M., Filges, T., & Jørgensen, A. M. K. (2018). Deployment of personnel to military operations: impact on mental health and social functioning. *Campbell Systematic Reviews*, 14(1), 1–127. <https://doi.org/https://doi.org/10.4073/csr.2018.6>
- Bondebjerg, A., Dalgaard, N. T., Filges, T., & Viinholt, B. C. A. (2023). The effects of small class sizes on students' academic achievement, socioemotional development and well-being in special education: A systematic review. *Campbell Systematic Reviews*, 19(3), e1345.
- Bondebjerg, A., Filges, T., Pejtersen, J. H., Kildemoes, M. W., Burr, H., Hasle, P., Tompa, E., & Bengtsen, E. (2023). Occupational health and safety regulatory interventions to improve the work environment: An evidence and gap map of effectiveness studies. *Campbell Systematic Reviews*, 19(4), e1371. <https://doi.org/https://doi.org/10.1002/cl2.1371>
- Borenstein, M., Hedges, L. V., Higgins, J. P. T., & Rothstein, H. R. (2009). *Introduction to meta-analysis* (1st ed.). John Wiley & Sons.
- Bornmann, L., Haunschild, R., & Mutz, R. (2021). Growth rates of modern science: a latent piecewise growth curve approach to model publication numbers from established and new literature databases. *Humanities and Social Sciences Communications*, 8(1), 1–15.
- Brunton, J., Stansfield, C., Caird, J., & Thomas, J. (2017). Finding relevant studies. In D. Gough, S. Oliver, & J. Thomas (Eds.), *An introduction to systematic reviews* (2nd ed., pp. 93–122). Sage.
- Burgard, T., & Bittermann, A. (2023). Reducing Literature Screening Workload With Machine Learning. *Zeitschrift Für Psychologie*.
- Buscemi, N., Hartling, L., Vandermeer, B., Tjosvold, L., & Klassen, T. P. (2006). Single data extraction generated more errors than double data extraction in systematic reviews. *Journal of*

Clinical Epidemiology, 59(7), 697–703.

Campbell Collaboration. (2023). *Stepping up evidence synthesis: faster, cheaper and more useful*.
<https://www.campbellcollaboration.org/news-and-events/news/stepping-up-evidence-synthesis.html>

Campos, D. G., Fütterer, T., Gfrörer, T., Lavelle-Hill, R. E., Murayama, K., König, L., Hecht, M., Zitzmann, S., & Scherer, R. (2023). *Screening Smarter, Not Harder: A Comparative Analysis of Machine Learning Screening Algorithms and Heuristic Stopping Criteria for Systematic Reviews in Educational Research*.

Chicco, D., & Jurman, G. (2023). The Matthews correlation coefficient (MCC) should replace the ROC AUC as the standard metric for assessing binary classification. *BioData Mining*, 16(1), 1–23.

Cohen, A. M., Hersh, W. R., Peterson, K., & Yen, P.-Y. (2006). Reducing workload in systematic review preparation using automated citation classification. *Journal of the American Medical Informatics Association*, 13(2), 206–219.

Dalgaard, N. T., Bondebjerg, A., Klokke, R., Viinholt, B. C. A., & Dietrichson, J. (2022). Adult/child ratio and group size in early childhood education or care to promote the development of children aged 0–5 years: A systematic review. *Campbell Systematic Reviews*, 18(2), e1239. <https://doi.org/10.1002/cl2.1239>

Dalgaard, N. T., Bondebjerg, A., Viinholt, B. C. A., & Filges, T. (2022). The effects of inclusion on academic achievement, socioemotional development and wellbeing of children with special educational needs. *Campbell Systematic Reviews*, 18(4), e1291.
<https://doi.org/10.1002/cl2.1291>

Dalgaard, N. T., Filges, T., Viinholt, B. C. A., & Pontoppidan, M. (2022). Parenting interventions to support parent/child attachment and psychosocial adjustment in foster and adoptive parents and children: A systematic review. *Campbell Systematic Reviews*, 18(1), e1209.
<https://doi.org/10.1002/cl2.1209>

Dalgaard, N. T., Flensborg Jensen, M. C., Bengtsen, E., Krassel, K. F., & Vembye, M. H. (2022). PROTOCOL: Group-based community interventions to support the social reintegration of marginalised adults with mental illness. *Campbell Systematic Reviews*, 18(3), e1254.
<https://doi.org/10.1002/cl2.1254>

Dietrichson, J., Filges, T., Klokke, R. H., Viinholt, B. C. A., Bøg, M., & Jensen, U. H. (2020). Targeted school-based interventions for improving reading and mathematics for students with,

- or at risk of, academic difficulties in Grades 7–12: A systematic review. *Campbell Systematic Reviews*, 16(2), e1081. <https://doi.org/10.1002/cl2.1081>
- Dietrichson, J., Filges, T., Seerup, J. K., Klokke, R. H., Viinholt, B. C. A., Bøg, M., & Eiberg, M. (2021). Targeted school-based interventions for improving reading and mathematics for students with or at risk of academic difficulties in Grades K-6: A systematic review. *Campbell Systematic Reviews*, 17(2), e1152. <https://doi.org/10.1002/cl2.1152>
- Doi, S. A., & Xu, C. (2021). The Freeman–Tukey double arcsine transformation for the meta-analysis of proportions: Recent criticisms were seriously misleading. *Journal of Evidence-Based Medicine*, 14(4), 259–261. <https://doi.org/https://doi.org/10.1111/jebm.12445>
- EPPI-Centre. (2024). *Automated data extraction using GPT-4*. <https://epi.ioe.ac.uk/cms/Default.aspx?tabid=3921>
- Evensen, L. H., Kleven, L., Dahm, K. T., Hafstad, E. V., Holte, H. H., Robberstad, B., & Rissstad, H. (2023). *Sutur av degenerative rotatorcuff-rupturer: en fullstendig metodevurdering [Rotator cuff repair for degenerative rotator cuff tears: a health technology assessment]*. <https://www.fhi.no/publ/2023/sutur-av-degenerative-rotatorcuff-rupturer/>
- Filges, T., Andersen, D., & Jørgensen, A.-M. K. (2015). Functional Family Therapy (FFT) for Young People in Treatment for Non-opioid Drug Use: A Systematic Review. *Campbell Systematic Reviews*, 11(1), 1–77. <https://doi.org/https://doi.org/10.4073/csr.2015.14>
- Filges, T., Dalgaard, N. T., & Viinholt, B. C. A. (2022). Outreach programs to improve life circumstances and prevent further adverse developmental trajectories of at-risk youth in OECD countries: A systematic review. *Campbell Systematic Reviews*, 18(4), e1282. <https://doi.org/https://doi.org/10.1002/cl2.1282>
- Filges, T., Dietrichson, J., Viinholt, B. C. A., & Dalgaard, N. T. (2022). Service learning for improving academic success in students in grade K to 12: A systematic review. *Campbell Systematic Reviews*, 18(1), e1210. <https://doi.org/https://doi.org/10.1002/cl2.1210>
- Filges, T., Montgomery, E., Kastrup, M., & Jørgensen, A.-M. K. (2015). The Impact of Detention on the Health of Asylum Seekers: A Systematic Review. *Campbell Systematic Reviews*, 11(1), 1–104. <https://doi.org/https://doi.org/10.4073/csr.2015.13>
- Filges, T., Siren, A., Fridberg, T., & Nielsen, B. C. V. (2020). Voluntary work for the physical and mental health of older volunteers: A systematic review. *Campbell Systematic Reviews*, 16(4), e1124. <https://doi.org/https://doi.org/10.1002/cl2.1124>
- Filges, T., Smedslund, G., Eriksen, T., & Birkefoss, K. (2023). PROTOCOL: The FRIENDS

preventive programme for reducing anxiety symptoms in children and adolescents: A systematic review. *Campbell Systematic Reviews*, 19(4), e1374.

<https://doi.org/https://doi.org/10.1002/cl2.1374>

Filges, T., Sonne-Schmidt, C. S., & Nielsen, B. C. V. (2018). Small class sizes for improving student achievement in primary and secondary schools: A systematic review. *Campbell Systematic Reviews*, 14(1), 1–107. <https://doi.org/10.4073/csr.2018.10>

Filges, T., Torgerson, C., Gascoine, L., Dietrichson, J., Nielsen, C., & Viinholt, B. A. (2019). Effectiveness of continuing professional development training of welfare professionals on outcomes for children and young people: A systematic review. *Campbell Systematic Reviews*, 15(4), e1060. <https://doi.org/https://doi.org/10.1002/cl2.1060>

Filges, T., Verner, M., Ladekjær, E., & Bengtsen, E. (2023). PROTOCOL: Participation in organised sport to improve and prevent adverse developmental trajectories of at-risk youth: A systematic review. *Campbell Systematic Reviews*, 19(2), e1321. <https://doi.org/https://doi.org/10.1002/cl2.1321>

Gargari, O. K., Mahmoudi, M. H., Hajisafarali, M., & Samiee, R. (2024). Enhancing title and abstract screening for systematic reviews with GPT-3.5 turbo. *BMJ Evidence-Based Medicine*, 29(1), 69 LP – 70. <https://doi.org/10.1136/bmjebm-2023-112678>

Gartlehner, G., Wagner, G., Lux, L., Affengruber, L., Dobrescu, A., Kaminski-Hartenthaler, A., & Viswanathan, M. (2019). Assessing the accuracy of machine-assisted abstract screening with DistillerAI: a user study. *Systematic Reviews*, 8(1), 277. <https://doi.org/10.1186/s13643-019-1221-3>

Gough, D., Oliver, S., & Thomas, J. (2017). *An introduction to systematic reviews* (2nd ed.). Sage.

Guo, E., Gupta, M., Deng, J., Park, Y.-J., Paget, M., & Naugler, C. (2024). Automated Paper Screening for Clinical Reviews Using Large Language Models: Data Analysis Study. *J Med Internet Res*, 26, e48996. <https://doi.org/10.2196/48996>

Hedges, L. V. (1992). Modeling Publication Selection Effects in Meta-Analysis. *Statistical Science*, 7(2), 246–255. <http://www.jstor.org/stable/2246311>

Higgins, J. P. T., Thomas, J., Chandler, J., Cumpston, M. S., Li, T., Page, M., & Welch, V. (2019). *Cochrane handbook for systematic reviews of interventions* (2nd ed.). Wiley Online Library. <https://doi.org/10.1002/9781119536604>

Hou, Z., & Tipton, E. (2024). Enhancing recall in automated record screening: A resampling algorithm. *Research Synthesis Methods*, n/a(n/a).

<https://doi.org/https://doi.org/10.1002/jrsm.1690>

Issaiy, M., Ghanaati, H., Kolahi, S., Shakiba, M., Jalali, A. H., Zarei, D., Kazemian, S., Avanaki, M. A., & Firouznia, K. (2024). Methodological insights into ChatGPT's screening performance in systematic reviews. *BMC Medical Research Methodology*, 24(1), 78.

<https://doi.org/10.1186/s12874-024-02203-8>

Jardim, P. S. J., Borge, T. C., & Johansen, T. B. (2021). *Effekten av antipsykotika ved førstegangpsykose: en systematisk oversikt [The effect of antipsychotics on first episode psychosis]*. <https://fhi.no/publ/2021/effekten-av-antipsykotika-ved-forstegangpsykose/>

Johansen, T. B., Nøkleby, H., Langøien, L. J., & Borge, T. C. (2022). *Samværs-og bostedsordninger etter samlivsbrudd: betydninger for barn og unge: en systematisk oversikt [Custody and living arrangements after parents separate: implications for children and adolescents: a systematic review]*. <https://www.fhi.no/publ/2022/samvars--og-bostedsordninger-etter-samlivsbrudd-betydninger-for-barn-og-ung/>

Jonnalagadda, S. R., Goyal, P., & Huffman, M. D. (2015). Automating data extraction in systematic reviews: a systematic review. *Systematic Reviews*, 4(1), 78. <https://doi.org/10.1186/s13643-015-0066-7>

Khraisha, Q., Put, S., Kappenberg, J., Warraitch, A., & Hadfield, K. (2024). Can large language models replace humans in systematic reviews? Evaluating GPT-4's efficacy in screening and extracting data from peer-reviewed and grey literature in multiple languages. *Research Synthesis Methods*, n/a(n/a). <https://doi.org/https://doi.org/10.1002/jrsm.1715>

König, L., Zitzmann, S., Fütterer, T., Campos, D. G., Scherer, R., & Hecht, M. (2023). *When to stop and what to expect—An Evaluation of the performance of stopping rules in AI-assisted reviewing for psychological meta-analytical research*.

Kugley, S., Wade, A., Thomas, J., Mahood, Q., Jørgensen, A.-M. K., Hammerstrøm, K., & Sathe, N. (2016). Searching for studies: A guide to information retrieval for Campbell. *Campbell Systematic Reviews*, 13(1), 1–73. <https://doi.org/10.4073/cmg.2016.1>

Meneses Echavez, J. F., Borge, T. C., Nygård, H. T., Gaustad, J.-V., & Hval, G. (2022). *Psykologisk debriefing for helsepersonell involvert i uønskede pasienthendelser: en systematisk oversikt [Psychological debriefing for healthcare professionals involved in adverse events: a systematic review]*. <https://www.fhi.no/publ/2022/psykologisk-debriefing-for-helsepersonell-involvert-i-uønskede-pasienthende/>

Ng, L., Pitt, V., Huckvale, K., Clavisi, O., Turner, T., Gruen, R., & Elliott, J. H. (2014). Title and

- Abstract Screening and Evaluation in Systematic Reviews (TASER): a pilot randomised controlled trial of title and abstract screening by medical students. *Systematic Reviews*, 3, 1–8.
- O'Connor, A. M., Tsafnat, G., Thomas, J., Glasziou, P., Gilbert, S. B., & Hutton, B. (2019). A question of trust: can we build an evidence base to gain trust in systematic review automation technologies? *Systematic Reviews*, 8(1), 1–8.
- O'Mara-Eves, A., Thomas, J., McNaught, J., Miwa, M., & Ananiadou, S. (2015). Using text mining for study identification in systematic reviews: a systematic review of current approaches. *Systematic Reviews*, 4(1), 1–22.
- Olofsson, H., Brolund, A., Hellberg, C., Silverstein, R., Stenström, K., Österberg, M., & Dagerhamn, J. (2017). Can abstract screening workload be reduced using text mining? User experiences of the tool Rayyan. *Research Synthesis Methods*, 8(3), 275–280.
- Olorisade, B. K., Brereton, P., & Andras, P. (2017). Reproducibility of studies on text mining for citation screening in systematic reviews: Evaluation and checklist. *Journal of Biomedical Informatics*, 73, 1–13. <https://doi.org/https://doi.org/10.1016/j.jbi.2017.07.010>
- Olorisade, B. K., de Quincey, E., Brereton, P., & Andras, P. (2016). A critical analysis of studies that address the use of text mining for citation screening in systematic reviews. *Proceedings of the 20th International Conference on Evaluation and Assessment in Software Engineering*, 1–11.
- OpenAI. (2024). *Function calling*. <https://platform.openai.com/docs/guides/function-calling>
- Pacheco, R. L., Riera, R., Santos, G. M., Sá, K. M. M., Bomfim, L. G. P., da Silva, G. R., de Oliveira, F. R., & Martimbianco, A. L. C. (2023). Many systematic reviews with a single author are indexed in PubMed. *Journal of Clinical Epidemiology*, 156, 124–126.
- Perlman-Arrow, S., Loo, N., Bobrovitz, N., Yan, T., & Arora, R. K. (2023). A real-world evaluation of the implementation of NLP technology in abstract screening of a systematic review. *Research Synthesis Methods*, 14(4), 608–621.
- Polanin, J. R., Pigott, T. D., Espelage, D. L., & Grotpeter, J. K. (2019). Best practice guidelines for abstract screening large-evidence systematic reviews and meta-analyses. *Research Synthesis Methods*, 10(3), 330–342. <https://doi.org/https://doi.org/10.1002/jrsm.1354>
- Pustejovsky, J. E. (2020). *clubSandwich: Cluster-robust (sandwich) variance estimators with small-sample corrections (0.5.5)*. cran.r-project.org. <https://cran.r-project.org/web/packages/clubSandwich/index.html>
- Pustejovsky, J. E., & Tipton, E. (2021). Meta-analysis with robust variance estimation: Expanding

- the range of working models. *Prevention Science*, 23(1), 425–438.
<https://doi.org/10.1007/s11121-021-01246-3>
- R Core Team. (2022). *R: A language and environment for statistical computing*. R Foundation for Statistical Computing, Vienna, Austria. <https://www.r-project.org/>
- Rathbone, J., Hoffmann, T., & Glasziou, P. (2015). Faster title and abstract screening? Evaluating Abstrackr, a semi-automated online screening program for systematic reviewers. *Systematic Reviews*, 4(1), 1–7.
- Rothstein, H. R., Sutton, A. J., & Borenstein, M. (2005). Publication bias in meta-analysis. In H. R. Rothstein, A. J. Sutton, & M. Borenstein (Eds.), *Publication bias in meta-analysis: Prevention, assessment and adjustments*. Wiley Online Library.
- Röver, C., & Friede, T. (2022). Double arcsine transform not appropriate for meta-analysis. *Research Synthesis Methods*, 13(5), 645–648. <https://doi.org/https://doi.org/10.1002/jrsm.1591>
- RStudio Team. (2015). *RStudio: Integrated development for R*. RStudio, Inc., Boston, MA.
<https://www.rstudio.com/>
- Schwarzer, G., Chemaitelly, H., Abu-Raddad, L. J., & Rücker, G. (2019). Seriously misleading results using inverse of Freeman-Tukey double arcsine transformation in meta-analysis of single proportions. *Research Synthesis Methods*, 10(3), 476–483.
<https://doi.org/https://doi.org/10.1002/jrsm.1348>
- Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). *Experimental and Quasi-Experimental Designs for Generalized Causal Inference* (2nd ed.). Cengage Learning, Inc.
- Shemilt, I., Khan, N., Park, S., & Thomas, J. (2016). Use of cost-effectiveness analysis to compare the efficiency of study identification methods in systematic reviews. *Systematic Reviews*, 5, 1–13.
- Shemilt, I., Simon, A., Hollands, G. J., Marteau, T. M., Ogilvie, D., O’Mara-Eves, A., Kelly, M. P., & Thomas, J. (2014). Pinpointing needles in giant haystacks: use of text mining to reduce impractical screening workload in extremely large scoping reviews. *Research Synthesis Methods*, 5(1), 31–49. <https://doi.org/https://doi.org/10.1002/jrsm.1093>
- Stoll, C. R. T., Izadi, S., Fowler, S., Green, P., Suls, J., & Colditz, G. A. (2019). The value of a second reviewer for study selection in systematic reviews. *Research Synthesis Methods*, 10(4), 539–545. <https://doi.org/10.1002/jrsm.1369>
- Syriani, E., David, I., & Kumar, G. (2023). Assessing the Ability of ChatGPT to Screen Articles for Systematic Reviews. *ArXiv Preprint ArXiv:2307.06464*.

- Thomsen, M. K., Seerup, J. K., Dietrichson, J., Bondebjerg, A., & Viinholt, B. C. A. (2022). PROTOCOL: Testing frequency and student achievement: A systematic review. *Campbell Systematic Reviews*, 18(1), e1212. <https://doi.org/https://doi.org/10.1002/cl2.1212>
- Tipton, E., & Pustejovsky, J. E. (2015). Small-sample adjustments for tests of moderators and model fit using robust variance estimation in meta-regression. *Journal of Educational and Behavioral Statistics*, 40(6), 604–634. <https://doi.org/10.3102/1076998615606099>
- Tsafnat, G., Glasziou, P., Choong, M. K., Dunn, A., Galgani, F., & Coiera, E. (2014). Systematic review automation technologies. *Systematic Reviews*, 3(1), 74. <https://doi.org/10.1186/2046-4053-3-74>
- Valentine, J. C. (2009). Judging the quality of primary research. *The Handbook of Research Synthesis and Meta-Analysis*, 2, 129–146.
- Van De Schoot, R., De Bruin, J., Schram, R., Zahedi, P., De Boer, J., Weijdem, F., Kramer, B., Huijts, M., Hoogerwerf, M., & Ferdinands, G. (2021). An open source machine learning framework for efficient and transparent systematic reviews. *Nature Machine Intelligence*, 3(2), 125–133.
- Viechtbauer, W. (2010). Conducting meta-analyses in R with the metafor package. *Journal of Statistical Software*, 36(3), 1–48. <https://doi.org/10.18637/jss.v036.i03>
- Viechtbauer, W. (2022). *Miller (1978)*. [https://www.metafor-project.org/doku.php/analyses:miller1978?s\[\]=proportion](https://www.metafor-project.org/doku.php/analyses:miller1978?s[]=proportion)
- Waffenschmidt, S., Knelangen, M., Sieben, W., Böhn, S., & Pieper, D. (2019). Single screening versus conventional double screening for study selection in systematic reviews: a methodological systematic review. *BMC Medical Research Methodology*, 19(1), 132. <https://doi.org/10.1186/s12874-019-0782-0>
- Wang, Z., Nayfeh, T., Tetzlaff, J., O’Blenis, P., & Murad, M. H. (2020). Error rates of human reviewers during abstract screening in systematic reviews. *PloS One*, 15(1), e0227742.
- Westgate, M. J. (2019). revtools: An R package to support article screening for evidence synthesis. *Research Synthesis Methods*, 10(4), 606–614. <https://doi.org/https://doi.org/10.1002/jrsm.1374>

Appendix A: Multi-prompt screening

TEXTBOX A1

PROMPT 1: “We are conducting a systematic review, which examines the potential effects of different frequencies or intensities in testing on the academic achievement or testing-related anxiety of primary and secondary school students.

We want to include studies with quantitative measures. For each study, we would like you to assess:

1) Does the study report quantitative measures?”

PROMPT 2: “We are conducting a systematic review, which examines the potential effects of different frequencies or intensities in testing on the academic achievement or testing-related anxiety of primary and secondary school students.

Only investigations performed in a school setting on children or students (ages 4-18 years old) are relevant for this review. This means that experiments performed in laboratories must be excluded, because we are only interested in real school settings and educational systems. For each study, we would like you to assess:

1) Does the intervention take place within a school setting?”

PROMPT 3: “We are conducting a systematic review, which examines the potential effects of different frequencies or intensities in testing on the academic achievement or testing-related anxiety of primary and secondary school students.

We only want to include studies that investigate children or students attending either primary or secondary school, this means from kindergarten until grade 12. In other words, we are looking for studies where the participants are students 4-18 years old. For each study, we would like you to assess:

1) Are the participants in the study children or students attending either primary or secondary school, this means from kindergarten until grade 12.”

PROMPT 4: “We are conducting a systematic review, which examines the potential effects of different frequencies or intensities in testing on the academic achievement or testing-related anxiety of primary and secondary school students.

The study must entail testing students or children. The testing can be standardized and non-standardized tests as well as formative assessments and summative tests, and high-stakes and low-stakes exams. This also include repeated testing, interim assessment testing, class quizzes, multiple choice testing, progress monitoring assessments or measures, curriculum-based measurement or assessments, retrieval practice measures or assessments, etc. For each study, we would like you to assess:

1) Does the study report on tests or testing of students or children?”

TEXTBOX A1 (Continued)

PROMPT 5: “We are conducting a systematic review, which examines the potential effects of different frequencies or intensities in testing on the academic achievement or testing-related anxiety of primary and secondary school students.

We like to include randomized controlled trials (RCT), field experiments, quasi-experimental studies (QES), or observational studies, which use a control/comparison research design to examine effects. This means that the study must compare at least two groups of students or children. Such studies can have many labels and the different designs can have different notations. The most common sub-categories of randomised controlled trials and quasi-experimental studies are: individual randomised assignment, cluster randomised assignment, stratified/blocked random assignment, pseudo-randomisation, matching cohort studies, difference-in-differences, regression-discontinuity designs, instrumental variable designs, propensity score matching, case-control studies, etc. Studies employing a within-subject design are also eligible for inclusion. For each study, we would like you to assess:

1) Is the study a randomized controlled trial (RCT), a field experiment, a quasi-experimental study, an observational study, or a study employing a within-subject design?”

PROMPT 6: “We are conducting a systematic review, which examines the potential effects of different frequencies or intensities in testing on the academic achievement or testing-related anxiety of primary and secondary school students.

In the review, we would like to include studies that measure students' academic achievement. In this review, we do not restrict measures of academic achievement to specific subjects. For each study, we would like you to assess:

1) Does the study report on measures of academic achievement or academic skills?”

Textbox A1 presents all the prompts we engineered and used to conduct the third classifier experiment. When added to the AIScreenR, each of the above six prompts was pasted together with the text present in Textbox 2 in the main paper.