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

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DATA AVAILABILITY STATEMENT
R codes for replication of all examples provided in this paper are available on the Open Science Framework at https://osf.io/apdfw/. The AIscreenR R package can be assessed at https://mikkelvembye.github.io/AIscreenR/

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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 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, we evaluated the use of OpenAI's GPT (generative pre-trained transformer) API (application programming interface) models as an alternative to human 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 against common human screening performances in high-quality systematic reviews, and we conducted three large-scale classifier experiments on three systematic reviews with 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 inclusion/exclusion criteria in a review, and show that this can make 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 be used as independent second screeners of titles and abstracts.
- We present and validate the AIscreenR package to allow for 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 reductions of human labor in systematic reviews without compromising the reliability of 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). In effect, independent human double-screening is considered the 'golden standard' to hinder a biased selection of 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 resource-intensive procedure, often 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 narrow their 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 these issues will only grow in magnitude as 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). As such, it may be considered an economi-

cally inefficient and unsustainable use of human resources to rely solely on (duplicate) human screening of titles and abstracts in future systematic reviews[1] (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 a prioritized order, thereby allowing for a more efficient screening (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 these tools/procedures may always be expected to 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 for reviewers 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, there is no clear rule for determining when it is safe to stop screening with regard to finding all or close to all eligible references. Although various stopping rules have been proposed, the adequacy of these rules 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, it seems like these tools are generally not capable of replacing an independent human second screener without a significant risk of omitting

[1] We already see instances of systematic reviews where the number of records needed to be screened 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).

a substantial number of eligible studies[2] (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., Levels 3 and 4) where they make credible independent (i.e., none human-assisted) 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 a review (Kugley et al., 2016; O'Connor et al., 2019; Olofsson et al., 2017). To achieve considerable time savings in future reviews, it is regarded as all-important that automated tools at least elevate 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 potential solution to bridge the gap between Levels 2 and 3/4 of automation[3] is to use large language models (LLM), such as the generative pre-trained transformer (GPT) models that have recently been introduced by OpenAI. Initial 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 being

[2] To alleviate this issue, a new tentative guideline termed SAFE has been developed in which it is suggested to use multiple machine learning algoritmes in order to detect all relevant references in the bulk of records (Boetje & van de Schoot, 2024). However, this framework has not yet been thouroughly enough tested to know if the SAFE procedure allows reviewers to detect all relevant studies using machine learning algoritms included in screening softwares, such as ASReview.

[3] Although the automaed tool can make none human-assisted decissions, we still consider it all-important that all screening task are made with human-in-the-loops. This also means that even though automated tools can make realiable decisions, we still believe that these should never be used as stond-alone single/first screeners in high-quality reviews.

on par with human performance, and consistently being 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 OpenAIs GPT models for title and abstract screening (henceforth TAB screening) represent a vital first step for validating these models as independent second screeners in systematic reviews, many questions remain unanswered. 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, no common workflow and guidelines exist for how to conduct GPT-based TAB screenings, including how to make reliable prompts. Even more critically, no software[4] has yet been developed to support and standardize the setup of GPT-based TAB screenings. Therefore, the aim of this paper is threefold: 1) to test and validate the TAB screening performance of GPT API models, 2) to develop a heuristical workflow for how to conduct TAB screening with GPT API models, and 3) to present the R package AIscreenR (Vembye, 2024). 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).

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 contribution. In Section 3 we describe the metrics we applied to evaluate the screening performance of the GPT API models and human screeners, respectively. Furthermore, we develop screening performance benchmarks to assess the performance of the GPT API models. In Section 4, we present three classifier experiments. This includes presentations of the prompt engineering and data underlying these experiments as well as the results of the experiments. In Section 5, we deduce tentative guidelines for when it is acceptable (and unacceptable) to use GPT API models as independent second screeners. 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 for TAB screening in high-quality reviews, and what should concern future research as well as the implications of our results and recommendations.

[4] To our knowledge, GPT models has so far only be 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.

2 RELATED WORK

To our knowledge, the first evaluation of the 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-0301[5] relative to five state-of-the-art machine learning algorithms. The authors found the performance of the GPT API models to be on par with traditional classifier models, and in some instances even better—without any need for (pre-)training. Moreover, the models only performed poorly when applied on datasets/reviews in which humans had shown a "high conflict ratio", thereby indicating that the poor performance was due to unclear inclusion/exclusion criteria (leading to poor human performance as well). Syriani et al. (2023) used Python to reach the GPT API models but did not build any publicly available software for others to replicate their workflow.

Guo et al. (2024) tested the use of OpenAI's GPT-4 API model[6] 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—were 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 correctly excluding irrelevant 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 reach 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 codes,[7] thereby allowing others to 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

[5] This model has been deprecated.
[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

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.

2.1 What we do differently

In this paper, we go beyond previous evaluations in multiple ways. First of all, we develop a new benchmark scheme for interpreting TAB screening performances in high-quality systematic reviews. Relative to previous research, we considered it all-important to develop this scheme for two reasons, 1) to make a fair assessment of GPT API models' TAB screening performances relative to humans, and 2) to make guidelines for when (and when not) to use GPT API models for TAB screening. To construct a benchmark scheme based on empirical human screening performances from high-quality systematic reviews, and thereby gain a deeper understanding of typical screener performances in high-quality systematic reviews, we mapped human screening TAB screening performances across 17 Campbell Systematic Reviews and 5 systematic reviews conducted by the Norwegian Institute of Public Health (NIPH).

A key part of validating the use of GPT API models for TAB screening in high-quality systematic reviews, and not compromising the quality of future systematic reviews, is to show that these models are not significantly inferior to human screener performances (O'Connor et al., 2019). As with the previous evaluation, we, therefore, conducted multiple classifier experiments. However, we tweaked these experiments differently than previous research. Firstly, we set up three experiments with three levels of complexity, and so that each experiment overcame shortcomings not accounted for by the others. Since all previous evaluations were based on either medical or natural science reviews, a side-effect of the experiments was to add to the generalizability of previous evaluations by showing that GPT API models can exhibit promising screening performance in social science reviews as well. Secondly, we drew on function calling in the request body (OpenAI, 2024). This allowed us to make prompts without the need to explicitly specify how the model should respond to the request, as otherwise done in previous evaluations. 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). Also, we built our function calls so that they also allowed the model to express its uncertainty beyond making binary decisions (i.e., include or exclude). For example, if the models did not have enough information to make a reliable decision, then the given study record was added to the pool of included studies. This aimed to reduce the models' ability to overlook potentially relevant studies. Thirdly, we invented *multi-prompt screening* where we made one concise prompt per inclusion (and/or exclusion) criterion), instead of adding

all inclusion/exclusion criteria to a single prompt, as done in all previous evaluations. When using multiple screening prompts, study eligibility was then based on whether a study record was included in all or close to all (say 5 out of 6) of the used prompts. We show that this screening approach could make GPT API models perform well even within highly complex review settings.

Finally, a major difference between this paper and previous evaluations is that we aim to provide a standardized and user-friendly workflow for how or when to use GPT API models for TAB screening; a workflow that is easy to implement in state-of-the-art systematic reviews. We do so by developing the AIscreenR R package (Vembye, 2024), which was, furthermore, quality-assured via the conduct of the three above-mentioned classifier experiments.

3 METHODS

This section describes the metrics that we used to develop our empirically informed screening benchmarks against which the screening performance of the GPT API models can be held when evaluating their screening performance. This section also describes the data and results consolidating the screener performance benchmark scheme.

3.1 Performance metrics

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* (sometimes referred to as the sensitivity) and *specificity* metrics since these are intuitive to understand and interpret and are not sensitive to imbalanced data. That is when data contains a large difference in the proportion between inclusion and exclusion references, which is commonly found in systematic reviews (Brunton et al., 2017). The recall metric "represents the proportion of relevant records being correctly classified" (Hou & Tipton, 2024), and can be written as

$$Recall = \frac{TP}{TP + FN} \tag{1}$$

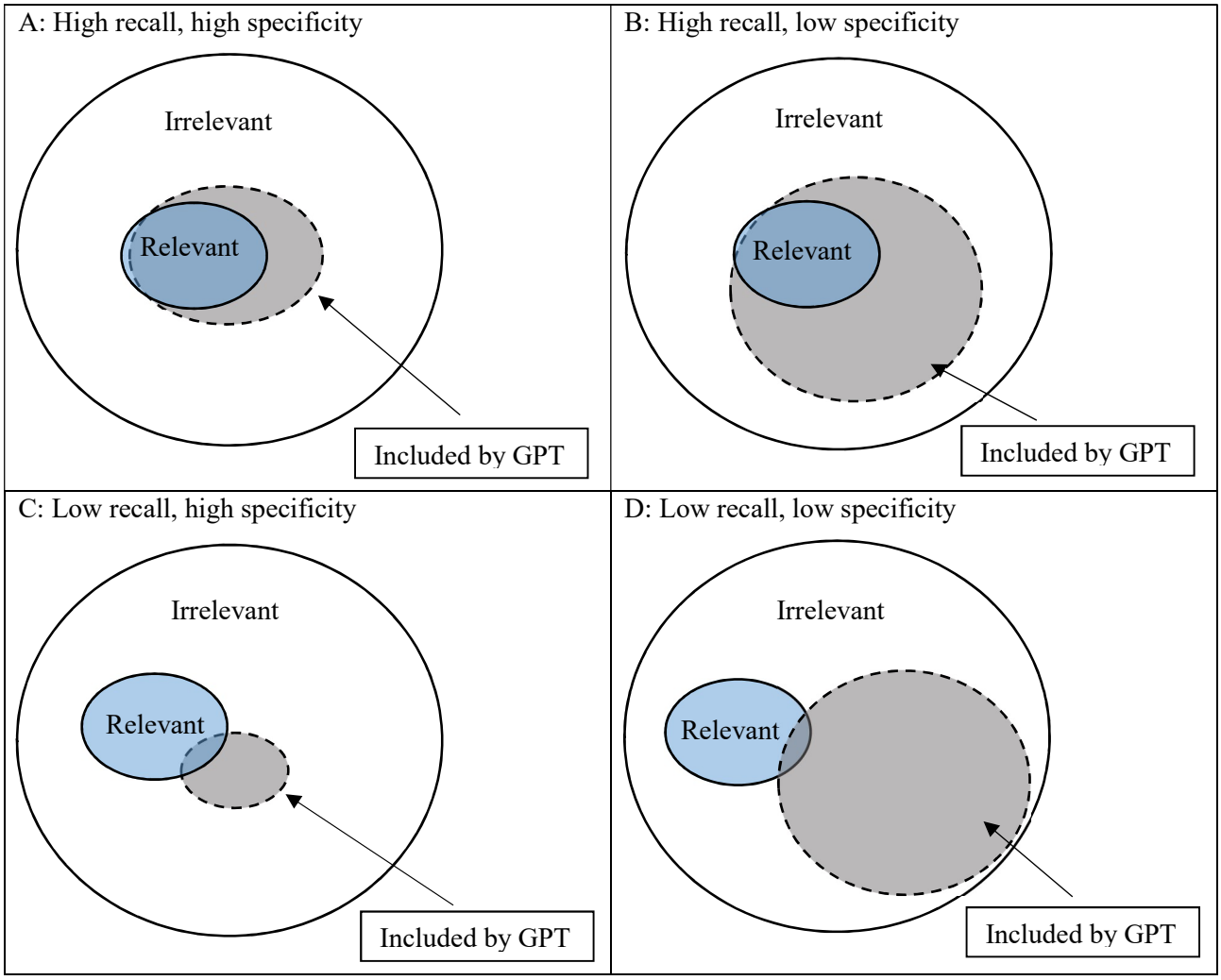
where TP (true positive) represents all the studies that are correctly included, and FN (false negative) is the number of studies falsely excluded. Notably, FNs are the most consequential decisions that can be made in terms of inducing biases in systematic reviews. By contrast, the specificity metric "measures the ability to exclude all references that should be excluded" (Syriani et al., 2023), and is given by

$$Specificity = \frac{TN}{TN + FP} \tag{2}$$

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 type of 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 confidently exclude 50% of the irrelevant records, which in most instances can be considered 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 accompanied specificity rate. Yet, this goes without saying low a specificity rate 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 single human screener decisions to final decisions 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 metrics above 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. To exemplify, if one simply uses the raw agreement metric with imbalanced data then the screening performance will most often be overestimated. 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, which makes the core of a proficient TAB screener. To overcome this issue, we used two overall metrics that ac-

count for imbalances*; 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 given by

$$bAcc = \frac{Rec + Spec}{2} \tag{3}$$

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.

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

3.2 Developing a benchmark scheme for evaluating TAB screening performances in high-quality systematic reviews

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 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. Hereto, we think it is important to acknowledge that individual human screening is not without significant errors 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 independently double-screened by at least two individuals. A total of 46 individual screeners participated in this process, 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. This, as well, is not without problems. Differences in performance between the two types of screeners may not only reflect different levels of content expertise but 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 had been 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 a total of 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

Source Authors	Short title	$n_{included}/N$	Ass.[a]	Aut.[b]
Campbell review				
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, Klokker 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, Sonne-Schmidt 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
NIPH review				
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 førstegangspsykose [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 metafor package (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 of the *recall*, *specificity balanced accuracy*, and the *nMCC* metrics across the included studies, 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; the variability of the true screener performances 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

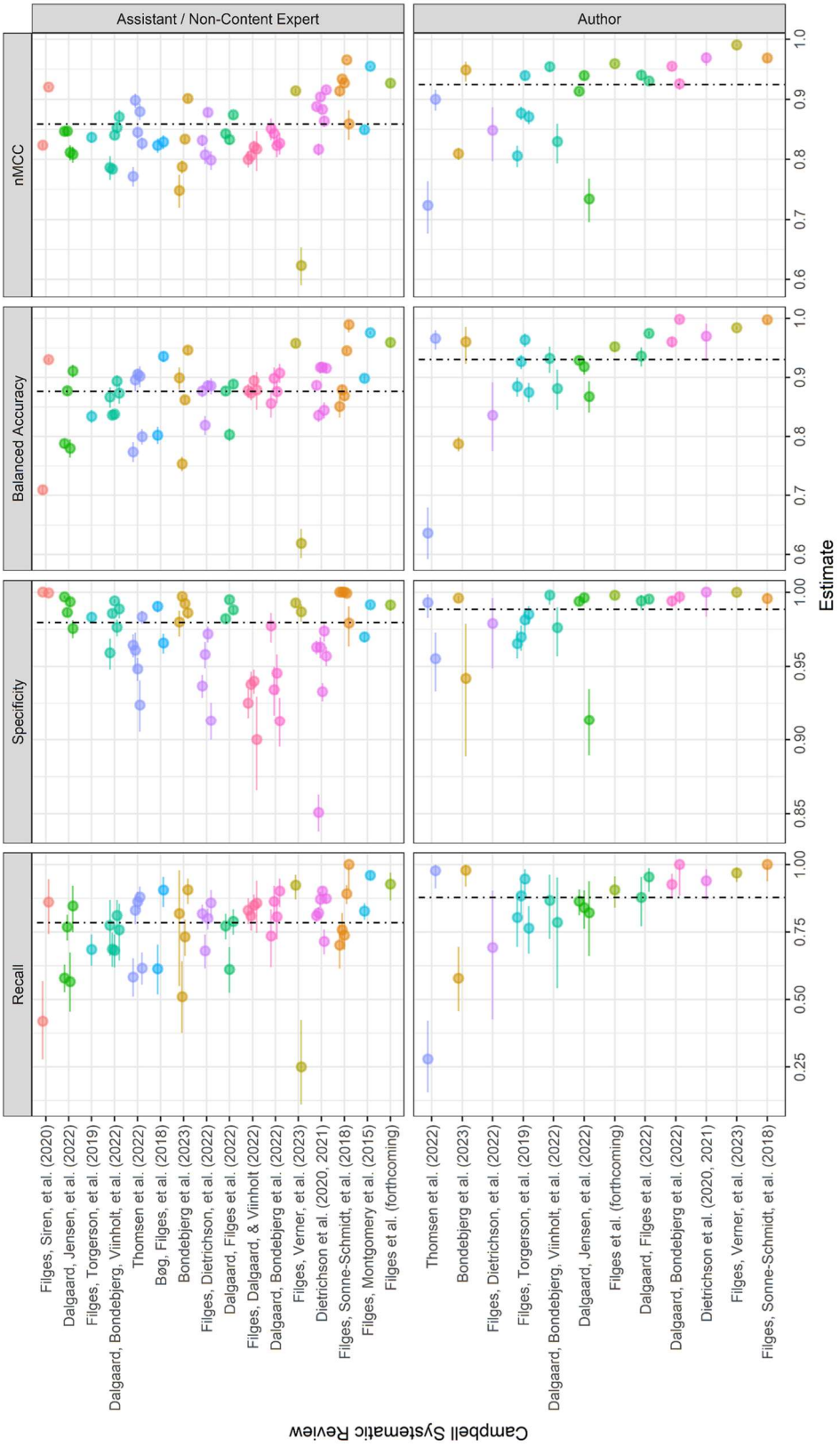
All individual screening performances across the included reviews and how these are distributed around the overall performance means are exhibited in Figures 2 and 3. We found the overall average recall rate for the assistant and author screeners in the included Campbell reviews to be 0.782, 95% *CI*[0.747, 0.817] and 0.881, 95% *CI*[0.823, 0.931], respectively. Hereto, we found the two groups' average 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. The group averages were found to be statistically different from one another with $F(1, 11) = 9.65$, $p = .01$.

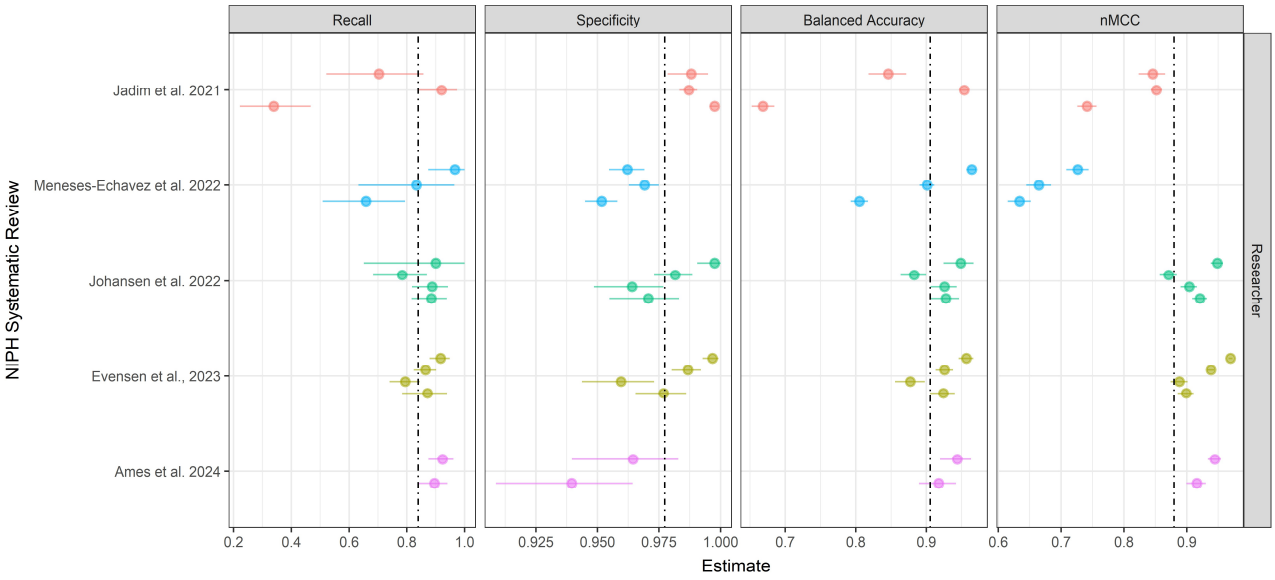
Based on these results it seems like researcher screeners are substantially better at detecting relevant studies than assistant screeners. Yet, this difference may be driven by factors other than mere screening quality (as noted, the researchers do not only tend to have higher levels of content expertise – they are also in an authority relation to the assistants, which may inflate the researchers' performance relative to that of the assistants). Interestingly, when investigating the NIPH data, which was in all cases based on independent researcher-researcher screening comparisons, we found performance patterns closer to the performance of the assistant screeners in the included Campbell reviews. 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 screeners 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 screening benchmark scheme presented in Table 3.

TABLE 3: Screening performance benchmarks

	Values				
Metric	**.0 < 0.5**	**0.5 < 0.75**	**0.75 < 0.8**	**0.8 < 0.95**	**0.95 <**
Recall	Ineligible performance	Low 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
Specificity	Ineligible performance	Low 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 the TAB screening performance is unacceptability low. Gray areas represent insufficient performance conditions but some applications with these performance measures might still be viable. Green areas represent acceptable screening performances on par with or better than human screening.

On this basis, we suggest—as a course-grained rule of thumb—that if an automated tool can reach a recall rate equal to or above 80% and specificity rates above 95%, they can be said to resemble common human screener performance in the context of high-quality systematic reviews. 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 too low for a GPT API model to function as an independent second screener. Furthermore, we believe that automated tools can still be useful, even if they yield a recall rate between 50-75%. Under such conditions, the automated tools 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 merely 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 may be used to reduce the total number 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. However, we have added graduated shades of red, where the light red color for a specificity rate below 0.5 indicates that we cannot reject that specificity rates below 0.5 (as long as the recall is high) can be fruitful in some extreme cases. For example, in extreme-sized reviews, 30% screening reductions might represent substantial workload savings, amounting to multiple days of work saved. Nonetheless, our general recommendation—based on what is typically encountered in systematic reviews—is not to accept any measures below 0.5.

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 trivial 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 EXPERIMENT[8]

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, and 2) when using multi-prompt screening in complex review settings. A side-effect of conducting these experiments was further to quality assure the AIscreenR package (Vembye, 2024) and ensure that the software yields appropriate screening behavior among other things when drawing on function calling. 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. The main purpose of this paper is not 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 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 classifier Experiment 1, we tested the performance of the GPT API models 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 the GPT API models' performances 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 proof of concept purposes. Thus, if the GPT API models could not achieve satisfactory performance in this

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

context, they 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 studies per 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).

A critique against classifier 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 classifier experiment drawing on data from an unpublished/ongoing systematic review. In classifier 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] The 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 studies per 1000 records.

A fair critique of experiments 1 and 2 is that they both represent very simple TAB screening cases that rarely resemble systematic review structures commonly encountered by reviewers. To address this issue, we, therefore, conducted a third classifier 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 classifier experiment 3, we used screening data from an ongoing Campbell Systematic Review of the effects of different testing frequencies on students' academic achievement (Thomsen et al., 2022). Compared to the screenings in experiments 1 and 2, we considered this 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

[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.

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-facetted phenomenon encompassing a variety of approaches and a heterogeneous terminology (tests are not just called tests, but may also be referred to 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, respectively. 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

"We are screening studies for a systematic literature review. The topic of the systematic review is the effect of the FRIENDS preventive programme on reducing anxiety symptoms in children and adolescents. The FRIENDS programme is a 10-session manualised cognitive behavioural therapy (CBT) programme which can be used as both prevention and treatment of child and youth anxiety. The study should focus exclusively on this topic and we are exclusively searching for studies with a treatment and a comparison group. For each study, I would like you to assess: 1) Is the study about the FRIENDS preventive programme? 2) Is the study estimating an effect between a treatment and control/comparison group?

Short/concise background information

Inclusion/exclusion criteria

Then when given study IDs (if not provided by the user, these are automatically generated), titles, and abstracts, the AIscreenR automatically pastes this together with 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 aimed 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 (i.e., one function calling yielding simple trinary results and another yielding descriptive screening responses) 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 function[10] we built included the instructions presented in Textbox 3:

[10] Find the exact functions here: bit.ly/3Vl0SRp

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 was 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. The test performance of the models did thus dramatically increase 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). All engineered prompts used for Experiment 3 are presented in Appendix A. We elaborate further on multi-prompt screening in Section 5. [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, and 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,

[11] Moreover, we show how this can be done in one of the accompanying vignettes to the AIscreenR package (Vembye, 2024).

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 models 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-turbo models are generally considered to be less accurate in their responses, 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-3.5 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 higher costs of using these models, 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. That is the default value of 1 for both hyperparameters.

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, using the benchmark scheme from Section 3, 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 of only 69%.

When used on the FRIENDS data, the gpt-4 model performed extremely well with performance measures that can considered to exceed common human screening performances. Specifically, 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 spent 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 would be theoretically 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 reduces the risk of overlooking relevant studies, which might be even more important in complex review settings where exclusion decisions may more often be 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 the 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.

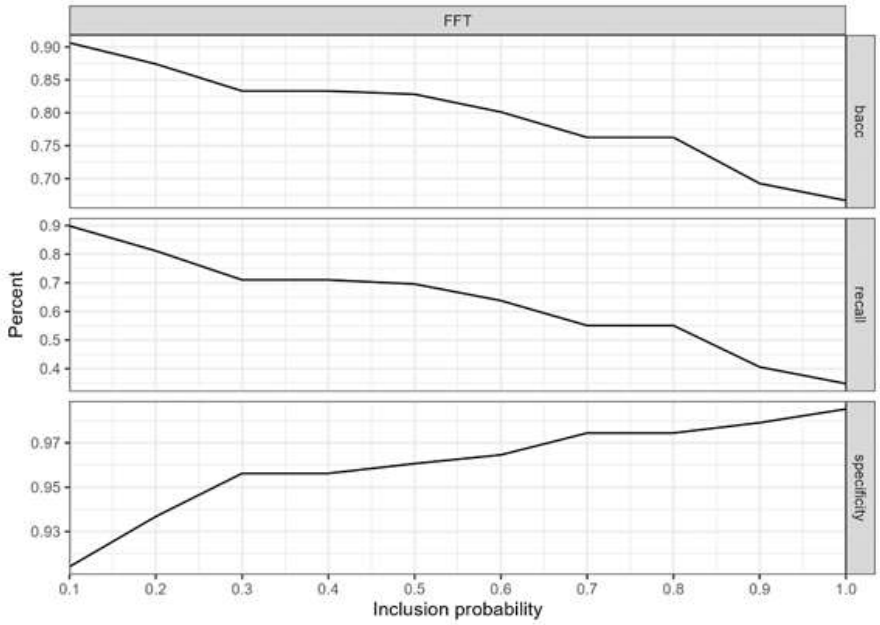
TABLE 4: Results of the two classifier experiments

Review Model	**Reps Per Prompt**	**Recall (%) [TP/(TP + FN)]**	**Specificity (%) [TN/(TN + FP)]**	**Raw agreement (%) [(TP + TN)/*N*][a]**	**bAcc (%)**
FFT					
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
FRIENDS					
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
TF					
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

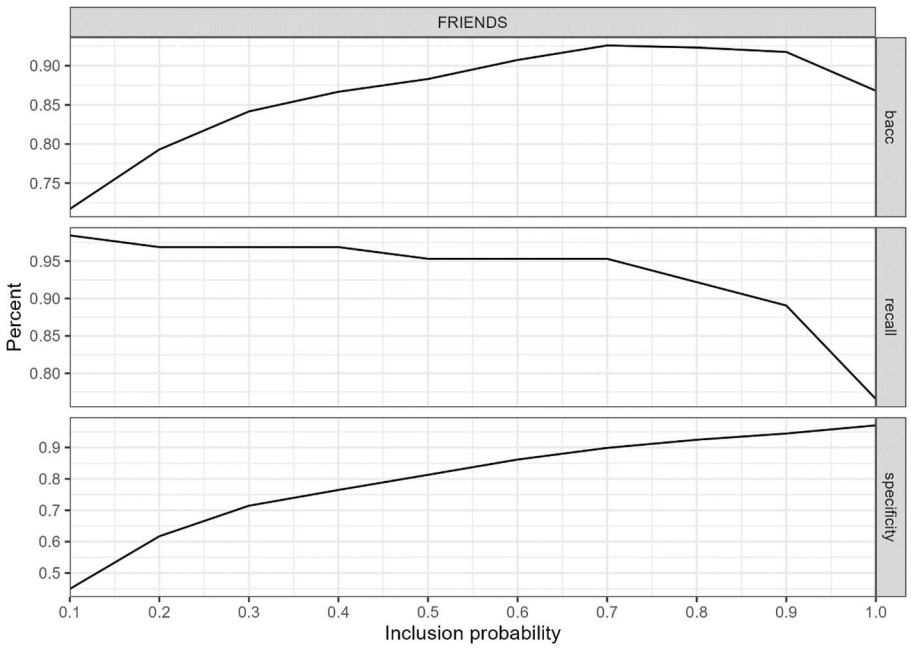
a: N is the total number of references

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 better than common human screeners, even in highly 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, in our third classifier experiment, we used multi-prompt screening instead of adding all inclusion/exclusion criteria to a single prompt. Moreover, it can be caused by the fact that we instructed the models to include abstract with little information. 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 (Vembye, 2024) to conduct all classifier experiments, we feel confident to conclude that the software works as expected. Hence, we believe that reviewers can confidently use this software in high-quality systematic reviews as well. Fourth, our results suggest 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 to use the GPT-3.5 API models when GPT-4 API models are available. Moreover, in cases where researchers have to rely on GPT-3.5, different inclusion probability-based inclusion thresholds should be considered (cf. Figure 4). 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 high-quality 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. This 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

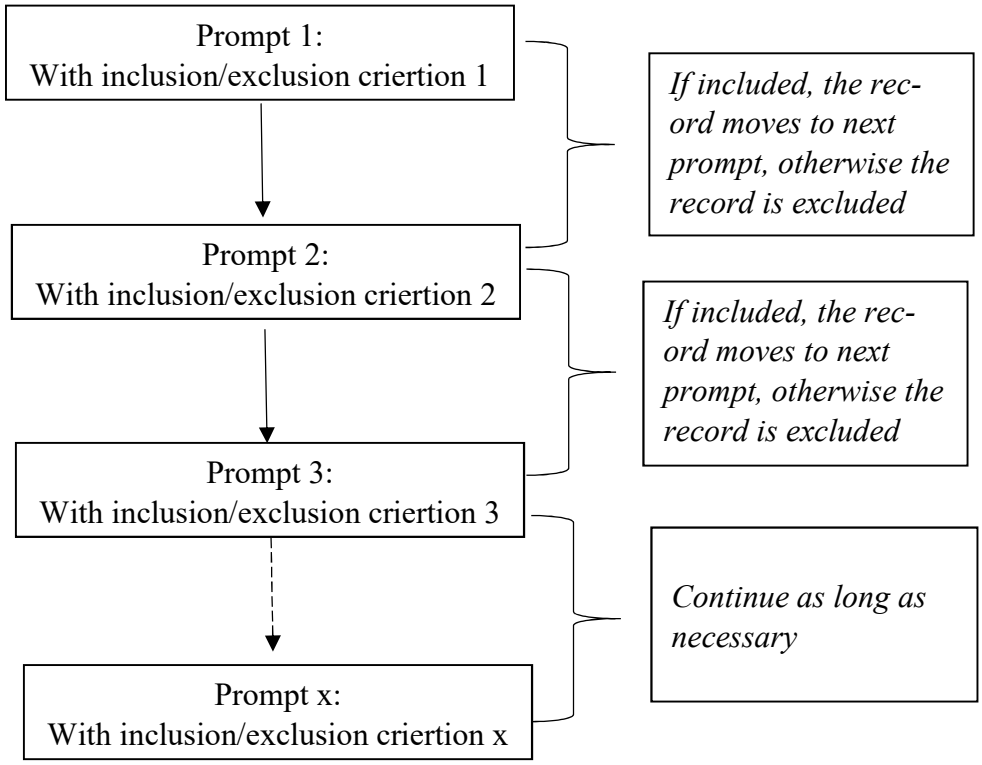
Step	**Reviewer action**
1	Find approximately 10 relevant study records (ideally more).
2	Find a minimum of 200 irrelevant study records (ideally randomly sampled from the entire pool of records).
3	Construct the test dataset by combining the records from steps 1 and 2.
4	Develop one or multiple prompts 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 *not* to use the GPT API model as an independent 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 and validating the screening performance of the prompt(s) and GPT API models 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 prompts 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. If any relevant studies are detected among the randomly sampled study records, these can just be added to the pool of relevant 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. The models do not need to be trained and should therefore in general only be fed with a minimum of information. 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 considered irrelevant if it is excluded at any step in the multi-prompt screening. This procedure is depicted in Figure 5[12].

[12] To support this type of screening, we show how this can be practically executed in one of the accompanied vignettes to the AIscreenR (Vembye, 2024).

FIGURE 5: *Hierarchical screening*



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 (as suggested by 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 strongly 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 six 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 overcome the above bias by including all records that were included in 5 out of 6 prompts, as we did in classifier Experiment 3.

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% and 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. In this case, single-screening is still less desirable than using a bad-performing GPT API model as an extra 'pair of eyes' to increase one's chances 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 high-quality reviews. Alternatively, if the thresholds cannot be reached, the GPT API model can still be used as a third screener, again providing 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 decisions. In general, we recommend that decisions on whether GPT API model screening is appropriate in a given review should be made before the main TAB screening has been initiated or after the human screening has been conducted. An alternative to manually screening all records at once is to 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 revolutionary 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., less than 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 quickly set up a reliable screening with the FRIENDS data. Therefore, if the complexity of a review's inclusion/exclusion criteria is low, it may be advantageous to conduct a rapid investigation of whether GPT API model screening is appropriate in the specific case, even if the number of records is not high. However, we do not think reviewers should spend/waste too much time on this task in such cases. Table 6 visualizes the conditions under which 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 minor discrepancies to be acceptable and human-like. 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 consider 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 across 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. That said, we do think it is important to note that most human duplicate screenings also represent black box operations that are hardly replicable, and we believe the GPT API models should be judged in light of this. This goes without saying that we should not strive to make screening replicable and reproducible since this would clearly increase the transparency of high-quality reviews. Yet, 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 in high-quality reviews.

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 in the request body. 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 again think it is pivotal that future research investigates if downloadable GPT models can perform on par with OpenAI's GPT API models. This would secure a more stable applicability of using GPT models for TAB screening, supporting the functionality of this technology.

Even though, the use of GPT API models as second screeners can be considered to be more efficient than using a human second screener, reviewers should be aware that it still can induce a significant cost to one's project, 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 classifier 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 GPT API models to reduce the total cost. In extreme-size reviews (i.e., 100,000< references), reviewers could consider combining priority screening/classifier modeling with the GPT API screening. For example, the GPT API screener could then 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, say, 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 together. This is just one idea of how a more cost-efficient screening could be set up in 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, incommensurable 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 as it may be 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. A key purpose of our benchmark scheme is, thus, also to guide reviewers on when GPT API screening might *not* be appropriate, which would be the case if prompt performances are not on par with human screening (or the time needed to reach satisfactory performance exceeds the time required for a human second screener to independently screen the titles and abstracts). 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 have or at least require some minor R coding skills. Thus, more generic solutions might be favorable. For example, by embedding this screening approach in a shiny app or by incorporating it directly in existing 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 when using the ChatGPT internet interface. Specifically, the GPT API models reached from the 'v1/chat/completions' endpoint worked significantly better relative to the GPT models embedded in the ChatGPT interface. Consequently, we consider it pivotal that future research clearly distinguishes between OpenAI's GPT models when doing research with them, so that different GPT model performances are not unnecessarily mixed up. In this paper, we have narrowly focused on the use of OpenAIs 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. It is, therefore, also unknown how our results generalize to the performance of using the 'v1/completions' endpoint as a second screener. Further, it should also be noted that comparisons between our results and the results of prior evaluations are restricted by the fact that we do not know what exact model endpoints were used by Syriani et al. (2023) and Guo et al. (2024).

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 typical screener performances more systematically and across various research fields such as medicine and the social sciences to make even more refined screening guidelines.

8 CONCLUSION AND DISCUSION

Human duplicate title and abstract 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. Moreover, we found that the GPT-4 model outperforms the GPT-3.5-turbo model, and we therefore recommend primarily using the GPT-4 model for TAB screening. Moreover, we found that GPT API models *can* yield specificity rates that are on par with humans, but in some applications appear to be slightly over-inclusive (i.e., they yield lower specificity rates than typical human screeners). We do, however, not necessarily consider this a deficit as long as the models obtain high recall rates since low specificity rates do not induce a bias—they just force human reviewers to double-check a higher number of records.

Our results contrast previous research in which GPT API models were found to perform well in terms of specificity but less well in terms of recall (Gargari et al., 2024; Guo et al., 2024). While it would be premature—based on our data—to make hard conclusions about the reasons for the higher performance (in terms of recall) in our classifier experiments, some differences are worth noting in terms of the workflow used by us compared to prior evaluations. First, as noted earlier, contrary to prior evaluations of GPT API models for TAB screening, we relied on function calling (OpenAI, 2024), thereby improving the models' response consistency. Second, we instructed the models to account for uncertainties when TAB screenings were based on limited information. Third, in Experiment 3 (our most complex case), instead of adding all inclusion/exclusion criteria to the

same prompt, we introduced and used multi-prompt screening, using one concise prompt per inclusion/exclusion criteria in the review.

Based on our findings, we believe TAB screening with GPT API models can revolutionize the way duplicate title and abstract screening is conducted in high-quality systematic reviews since these show the ability to work at 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. To further support automated GPT API model-based screenings, we developed the AIscreenR R package. Among other things, this allows 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, something that speeds up the screening significantly.

A key part of setting up a reliable GPT API screening is to thoroughly validate the performances of one's screening prompt(s) before making any full-scale screening. For such assessments, we developed a new, empirically informed benchmark scheme for interpreting acceptable and unacceptable screening performance in high-quality reviews. This was based on the typical screening performance found in 22 high-standard systematic reviews across 157,828 independent duplicate human screening decisions. Deduced from this investigation, we suggest that if an automated screening yields a recall rate (i.e., the ability to correctly include relevant studies) above 80%, it should be acknowledged as being on par with typical human performance and can be confidently used as an independent second screener. 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 serious biases.

It is important to note that no matter how much effort is invested in developing good prompts, GPT API models—like humans—can err and, therefore, it is of vital importance that GPT API screening is combined with other traditional screening techniques such as forward and backward citation tracking to ensure that potentially missed studies re-enter the review. In that regard, GPT-based screenings are not different from screenings conducted by humans. Although our recommendations allow for minor errors, we generally recommend not to use GPT API screening, if reviewers cannot reach satisfying recall and specificity rates. In a similar vein, 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.

With this paper, we have strived to make the foundation on which evidence organizations (such as Cochrane and Campbell Collaboration) and review journals can accept and assess the use of TAB screening with GPT API models. According to the Campbell Collaboration, the acceptance of using 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). These requirements have played a key part in this paper, and we have used them as the main pillars to build the suggested screening framework. Concretely, we have aimed to accommodate requirement *(a)* by building our framework and codes so that they can readily be remodeled to work with other API models than OpenAI's. This means that our setup aims to be agnostic to the given provider of the given LLM and will be viable as long as reviewers have public access to LLM models. We aimed to support Campbell's requirement *(b)* by developing the new benchmark scheme and by showing that GPT API screening is perfectly appropriate in high-quality reviews, whereas the development of the AIscreenR package and the quality tests hereof were meant to accommodate Campbell's requirement *(c)*. Moreover, to fulfill requirement *(f)*, we built the AIscreenR package as open-source software so that others in the review community (e.g., the Evidence Synthesis Hackathon, Campbell Collaboration, or the EPPI-Reviewer team) can readily contribute to the development and ongoing support of the software. Finally, we also developed our suggested workflow and guidelines to underpin requirements *(d)* and *(e)*. Requirement *(e)* is as such not necessary in our case since we are working we *pre*-trained models. 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.

However, 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 models for TAB screening since human screening decisions most often represent black-box operations as well. Nonetheless, 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 costs of using GPT models may 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). In addition, a more 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 and possibly can imagine. 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 in the given databases. Moreover, it makes it possible to screen literature for extreme-sized reviews (Shemilt et al., 2014, 2016) that would otherwise have been considered unmanageable and/or unremunerative for 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 a huge potential exists 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, thereby reducing the risk of relevant studies being overlooked. Combining traditional tools and GPT screening could furthermore reduce the cost of using GPT API models since it reduces the number of titles and abstracts needed to be screened by the 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 rigid human labor, and where no legal issues arise. Even more edifying,

GPT API model screening can ensure a more rapid transfer of usable knowledge to research, practice, and policy, which ultimately underpins the core rationale for doing systematic reviews.

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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.

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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.