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

ABSTRACT

Independent human double screening of titles and abstracts is considered a critical step to ensure the quality of systematic reviews and meta-analyses herein. However, double screening is a costly as well as a time- and resource-intensive procedure that slows the review process, ultimately excluding many researchers from using it. To alleviate this issue and potentially increase the reliability of systematic reviews and meta-analyses, we evaluated the use of OpenAI's GPT (generative pre-trained transformer) API (application programming interface) models as an alternative second screener of titles and abstracts in systematic reviews. Overall, we found that the GPT API models perform on par or even better than common human screening performance in terms of detecting relevant studies. To support future reviews, we develop a reproducible workflow and tentative guidelines for when (and not) reviewers can use GPT API models for title and abstract screening. Our aim is ultimately to make a framework in which the uptake of using GPT API models can be accepted as independent second screeners within state-of-the-art reviews facilitated by evidence institutions such as Cochrane and Campbell Collaboration. To standardize the application of using GPT API models for title and abstract screening tasks, we present the R package AIscreenR.

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

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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.
- Automating screening tools can ease the burden of title and abstract screening
- Automating 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 a highly reliable second screener in social science reviews with better recalls than presented in previous evaluations and on par with human performance.
- We develop empirical benchmarks to make reliable comparisons between AI and human screening performances.
- We provide general guidelines for how and when GPT models safely can used
- We present and validate the R package AIscreenR to ensure standardized conduct of title and abstract screening with OpenAI's GPT API models (and in theory with other models such as Claude 2).

Potential impact for Research Synthesis Methods readers

- Changing the double screening workflow of title and abstract screening in systematic reviews
- Increasing the reliability of large-scale systematic reviews
- Substantial and reliable reduction of human labor in systematic reviews
- Provides a new guideline for reviewers on when and when not to use AI screening tools
- Standardizing screening with prompt-based 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. Manual screening hereof can be a time-consuming and tedious task. However, overlooking relevant studies in this phase can be consequential, potentially 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 bias (Hedges, 1992; Rothstein et al., 2005), which threatens the internal validity of systematic reviews (Shadish et al., 2002). Therefore, independent human double-screening is considered to be the 'golden standard' to hinder a biased selection of relevant studies (Guo et al., 2024; Higgins et al., 2019; Stoll et al., 2019; Wang et al., 2020). This is further supported by the fact that previous research suggests that screeners on average tend to miss between 3% to 24% of all eligible studies depending on the level of content knowledge, which most often has a substantial impact on the final quantitative results (Buscemi et al., 2006; Waffenschmidt et al., 2019). In medicine, this number is in some cases even higher when using student screeners (Ng et al., 2014). Nonetheless, duplicate screening of all identified titles and abstracts is a costly and resource-intensive procedure, potentially 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. Alternatively, reviewers make too narrow searches to keep the number of records down to a manageable size which again heavily increases the risk of overlooking relevant studies (Van De Schoot et al., 2021). Over time all these issues will only grow in size since the complexity of identifying all relevant studies increases with the rapid growth in the number of scientific publications (Bornmann et al., 2021; O'Mara-Eves et al., 2015). Thus, it can be considered an economically inefficient and

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

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

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

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

of omitting a substantial number of eligible 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., Level 3 and Level 4) where they make credible independent deterministic screening decisions. Instead, the vast majority of tools are predominately used to conduct Level 2 tasks such as sorting citation records in prioritized order from highest to lowest probability of being relevant to the review (Kugley et al., 2016; O'Connor et al., 2019; Olofsson et al., 2017). If considerable time savings should be realized in future reviews, it is regarded as all-important that automated tools rise to at least Level 3 of automation (Jonnalagadda et al., 2015; Tsafnat et al., 2014).

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

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

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

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

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

[3] We do not consider the level 4 of automation to be the ideal case since we consider human-in-the-loop operation to be state-of-the-art at the time of writing.

par with human performance but always on par 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 the use of GPT models as independent second screeners in systematic reviews, many questions are left unanswered. Most pressing, it is still unclear how the GPT models can be implemented in systematic reviews in a standardized and reliable manner. In contrast to many well-established automated screening algorithms, there exists no recommended workflow for how to conduct such screenings, including how to make reliable prompts. Even more critically, no software[4] has yet been developed to support and standardize the setup of this screening approach. Therefore, a major aim of this paper is partly to develop a heuristical workflow for how to conduct TAB screening with GPT API models and partly to present the R package `AIscreenR` (version 0.0.1). Our target 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, etc. This might increase the chances of ensuring user deployment and acceptance since complex implementation is often considered to be a major impediment to the wider application of automated screening tools (O'Connor et al., 2019).

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

Concerning requirement *(a)*, we cannot as such fulfill it since the GPT API models we draw upon in this paper are closed-source applications with black-box algorithms. That is our suggested framework is only viable as long as given firms provide access to their LLMs. However, our suggested framework and codes can readily be remodeled to work with other API models, such as models from Claude 2 or Mistral AI where the request body takes the same arguments as OpenAI's

[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.

GPT models. Therefore, our setup aims to be agnostic to the given provider of the given LLM. In theory, our approach can be implemented together with LLMs such for instance Mistral open-source LLMs that can be downloaded locally by the users. We, therefore, understand a "functioning tech" to point, in our case, to the broader family of LLM models, which we believe will be around in some or another form for many years.

A key part of fulfilling Campbell's requirement *(b)*, and not compromising the quality of future systematic reviews, is to show that the GPT API models are not significantly inferior to human screening performance (O'Connor et al., 2019). Thus to make a reliable assessment of this, we developed empirical screening benchmarks to which the GPT API screening performance can be compared. We consider this as the only reliable way to assess whether a given recall is good or bad. Say, for example, that if humans on average tend to miss 20%-25% of all relevant studies during the title and abstract screening phase, then it might be misleading to infer that GPT models with a recall of 0.75% imply that GPT cannot be used as an individual second screener. To construct such a benchmark scheme we mapped the human screening performance of 21 large-scale systematic reviews; 16 Campbell Systematic Reviews, and five systematic reviews conducted by the Norwegian Institute of Public Health (NIPH). Thereafter, we conducted two large-scale classification experiments, where we showed that OpenAI's GPT API models can conduct TAB screening with a performance *at least* on par with human performance relative to our developed benchmarks.

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

Then, to fulfill requirements *(d)* and *(e),* we develop a heuristic for how to test the performance of one's developed prompt(s) and screening as well as assess under what conditions TAB screening with the GPT API models can be accepted to be used as an independent second screener in systematic reviews. We inform these guidelines by the empirical human screening benchmarks developed under requirement *(b)* as well. Since we are working with pre-*trained* models, requirement *(e)* is not as such necessary in our case. Instead, the performance of the prompt(s) used for screening needs to be *tested* and compared against human performance measures before credible TAB screening can be initiated. We return to this point when we show how to develop reliable prompts for TAB screening in later sections. Finally, to accommodate requirement *(f)* we have developed the `AIscreenR` package as an open-source software so that others in the review community can readily contribute to the development and ongoing support of the software. With the exposition sketched

above, we hope to make the uptake of such tools more acceptable and clearer in future reviews. This goes without saying that our approach represents the final solution. Our aim is just to show one way in which GPT API models can be used for TAB screening in large-scale systematic reviews that can inspire future applications of TAB screening with LLMs.

The remainder of the paper proceeds as follows: In Section 2 we review previous evaluations of using OpenAI's GPT models for TAB screening tasks in systematic reviews and reflect on our contributions. In Section 3 we describe the metrics we applied to evaluate the screening performance of the GPT API models and human screeners, respectively. In Section 3, we further develop screening performance benchmarks to assess the performance of the GPT API models. In Section 4, we present our classifier experiment, including our prompt engineering and data used to conduct these experiments. The results of these experiments are also presented in this section. In section 5, we deduce tentative guidelines for when we think reviewers are 'good to go' in terms of using OpenAI's GPT API models as an independent second screener. Moreover, we flesh out 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 and the use of OpenAI's LLMs and what should concern future research as well as the implications of our results and recommendations.

2 RELATED WORK

To our knowledge, the first evaluation of the TAB screening performance of OpenAI's GPT API models was performed by Syriani et al. (2023). Based on five ongoing systematic reviews within the field of software engineering, they compared the TAB screening performance of the GPT API model gpt-3.5-turbo-0301[5] relative to five state-of-the-art machine learning algorithms. Hereto they found that OpenAI's GPT API models perform on par with traditional classifier models, and in some instances even better—without any need for (pre-)training. They only found the models to perform badly when applied on datasets where humans had shown a "high conflict ratio". This might simply indicate that the models perform badly when given unclear inclusion criteria—as humans would do too. Syriani et al. (2023) used Python to reach the GPT API models, but they did not build any publicly available software for others to replicate their workflow.

Guo et al. (2024) tested the leverage of OpenAI's GPT-4 API model[6] for TAB screening of medical research literature. They found that the average recall (referred to as the sensitivity of

[5] This model has been deprecated
[6] It is uncertain what exact model they authors used. We expect it to be the gpt-4-0613 API model.

included paper) and specificity when compared to the final decision of two independent human screeners across six clinical reviews was 0.76 and 0.91, respectively. Based on these results, Guo et al. (2024) infered that the GPT-4 model is proficient in terms of excluding the right studies whereas it is insufficient in finding relevant studies compared to human screening. Consequently, Guo et al. (2024) conclude that GPT API models should not replace human screening but instead be seen as a support tool guarding against human errors. Guo et al. (2024) used Python to call the API models without providing any general user software.

Gargari et al. (2024) applied the gpt-3.5-turbo-0613 API model to conduct TAB screening in one clinical systematic review. In line with Guo et al. (2024), they found GPT to be better at making correct exclusion decisions relative to detecting relevant studies. Therefore, they also recommend 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] so that others can replicate their workflow. Yet this requires reviewers to be rather skilled in Python coding.

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

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

2.1 What we do differently

In this paper, we go beyond previous evaluations in multiple ways and show some key advances in using LLMs for TAB screening relative to (but possibly combined with) traditional machine learning tools. Starting with the latter, one advance of using LLMs is that these models do not need to be pre-trained which, in turn, means that these models are not as (if at all) sensitive to imbalance between relevant and irrelevant records or the number of relevant records in the data as classical machine-learning tools (Campos et al., 2023; König et al., 2023). This is so because the GPT models we applied treat each title and abstract individually without any knowledge of previous decisions. Compared with traditional machine learning algorithms, we will also show that the GPT-4 has the ability to find almost all relevant studies when well prompted.

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

The main difference between this paper and the previous evaluation is further that we aim to make a standardized and user-friendly workflow for how to use GPT API models for TAB screening that are easy to implement in state-of-the-art systematic reviews. We do so by developing the AIscreenR R package and technically quality-assuring it via the conduct of to large-scale classifier

experiment. The AIscreenR is built as a flexible software that allows users to conduct multiple screenings simultaneously based on multiple prompts, API models, iterations of the same request, and nucleus samples (i.e., different top_p values). We allow the user to conduct the same request (i.e., asking the exact same question) multiple times to avoid random noise in the model response (especially when using gpt-3.5 models). When this feature is used the final GPT decision is based on the probability of inclusion across the iterated requests. The inclusion threshold can be determined by the user. This also allows the users to test model response consistency. Moreover, the software is built so that it draws on multi-core processing, which allows the users to speed up the timing of the screening significantly.

To conduct a fair assessment of GPT's ability to conduct TAB screening relative to humans but also to outline reliable guidelines on when to use LLMs for TAB screening (which has not previously been done), requires a clear understanding of common human screening performance in systematic reviews. Therefore, to make a better understanding of common human performance and to develop benchmarks that could be held against the screening performance of GPT, we mapped the human screening performance across 16 Campbell systematic reviews and 5 systematic reviews conducted by the Norwegian Institute of Public Health (NIPH). Relative to the previous evaluations, the contribution of this paper is therefore to put forward a tentative benchmark scheme to which all types of AI screening tools can be compared.

In all the previous evaluations, multiple inclusion/exclusion criteria were added to a single prompt. Yet Gargari et al. (2024) suggested that broader and less specific prompts do not perform well in terms of finding relevant studies. Instead, concisely framed prompts with clear information seem to have a better performance. This could indicate that single-prompt TAB screening is rather restricted to only work within simple and clearly defined reviews where the inclusion of abstracts can be determined by a few inclusion criteria/questions. However, to overcome this issue, we introduce what we have coined *hierarchical screening*. With this approach, we suggest making one concise prompt per inclusion (and/or exclusion) criterion. In the type of screening, studies are only considered relevant if included on all or close to all (say 5 out of 6) of the used prompts. We consider this to represent a reliable and viable way in which TAB screening with GPT API models is also within broad and complex reviews, as we often find within the social sciences.

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

3 METHODS

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

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

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

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

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

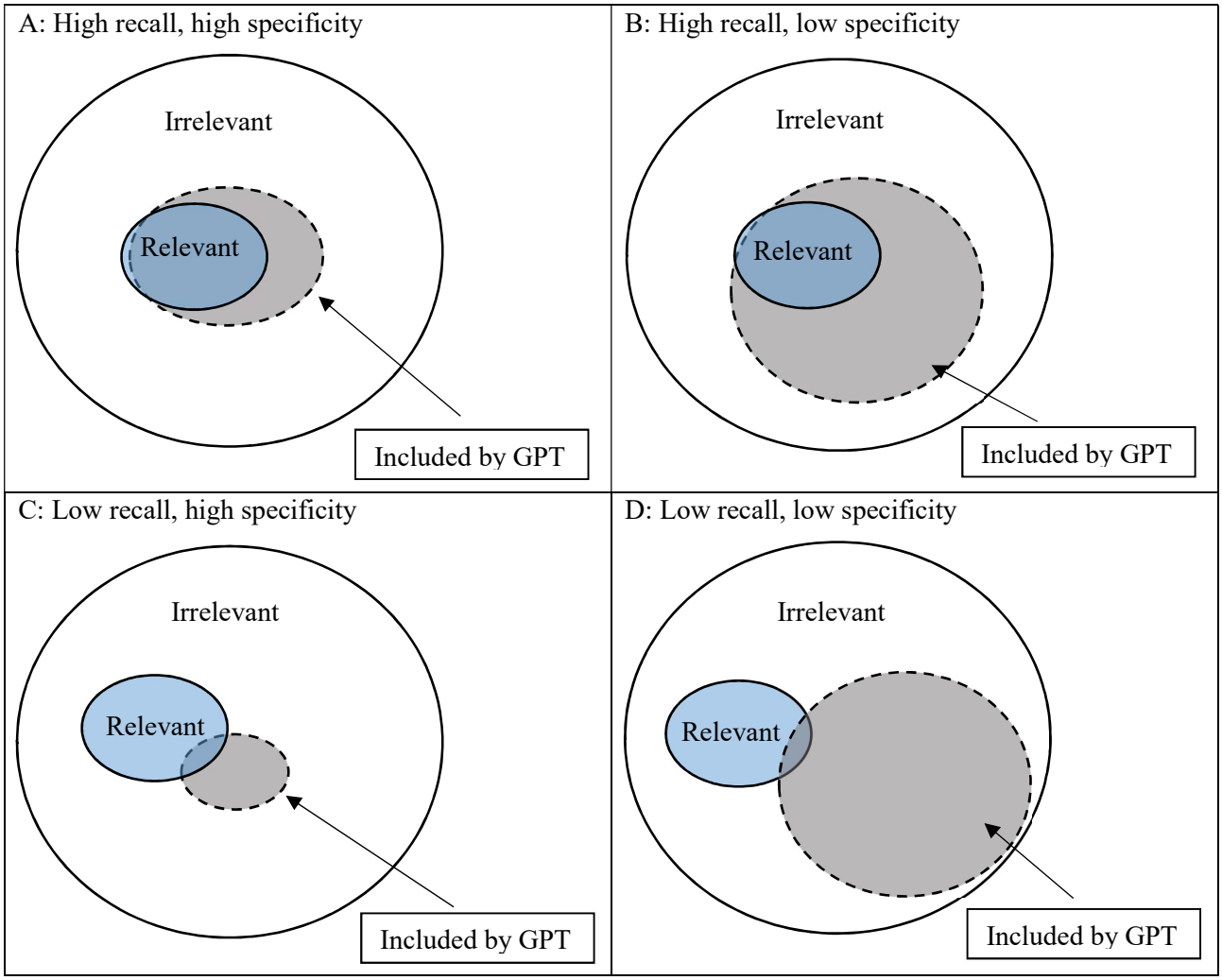
$$Specificity = \frac{TN}{TN + FP} \tag{2}$$

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 our 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 "just" means that reviewers must re-examine the relevancy of a larger share of the pool of references. If reviewers can be sure that they find all relevant studies but have a specificity of say 50%, this still implies that the reviewer can safely exclude 50% of the irrelevant records, which in most instances can be considered to be a significant reduction in the screening workload. Therefore, we think that automated tools should be accepted as long as they come close to scenarios A and B pictured in Figure 1. That is, they are accepted when high recalls can be made to a large extent independently of the accordingly specificity

measure. Yet, this goes without saying that reviewers should accept low specificity rates. We will come back to that in the following sections.

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

FIGURE 1: Recall and specificity performances



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

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

In order to make fair comparisons between human and automated screening performances, we consider it pivotal to have a deeper understanding of acceptable human screening performance in high-standard systematic reviews. This was highlighted by O'Connor et al. (2019) as well. We believe that many previous evaluations of the performance of automated screening tools overlook the fact that individual human screening is not without significant errors either, and automated screening tools must be evaluated in light of this. If we as a community primarily assess the performance of automated tools and accept the tools with the requirement that they can detect (close to) all relevant studies in all instances or on par with very high human performances, then the tools are by design doomed

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 this section, we develop a tentative benchmark scheme on which recommendations for how big error rates should be accepted in high-standard systematic reviews when using automated screening tools TAB screening

3.2.1 The data underpinning the benchmark scheme

The data we used to construct this benchmark scheme was based on the human screening performances in 21 high-standard systematic reviews that used independent duplicate human screening. This includes 16 Campbell Systematic Reviews and 5 reviews conducted by the Norwegian Institute of Public Health (NIPH). A descriptive overview of all the included reviews can be found in Table 2, including the imbalances 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 137,764 title and abstract records, all of which have been double-screened by 44 individual screeners of which 33 were student assistants and/or non-content experts, and 11 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 student assistant screeners, this could potentially downward bias the evaluation metrics for various reasons. For example, student 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. Therefore, we analyzed the Campbell review data separately for student/non-expert and researcher/expert screeners. However, relative recall and specificity rate differences between the two types of screeners could also be driven by authority imbalances between the often more senior content expert and the student screener, making the performances of the expert screeners look better than they actually were. Consequently, we added the screening performance data from five systematic reviews conducted by NIPH in which all TAB screenings and disagreements were conducted and solved by researchers with specific content knowledge related to the given review. This should, thereby, give a clearer picture of common researcher performances in systematic reviews. This data added a further 13,825 title and abstract records that had been independently double-screened by 13 individual researchers. The five NIPH re-

views were conducted from 2021 to 2024. When analyzing all of the above-presented data, we removed all training data to avoid inflating human disagreements unreliably. 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
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 package metafor (Viechtbauer, 2010), including the sandwich estimators herein (Pustejovsky, 2020). To work with ris-file data, we used the revtools package (Westgate, 2019). All materials behind this article can be accessed at bit.ly/3spivoG.

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 agreed upon between a minimum of two human screeners. When working with proportion metrics such as the ones presented in Equations (1) to (3), it is usually advantageous to transform these metrics into measures that have more appropriate statistical properties. This includes having a sampling distribution that more closely mirrors a normal distribution and a variance component that can more reliably be approximated (Viechtbauer, 2022). Therefore, we used the arcsine transformation (Röver & Friede, 2022; Schwarzer et al., 2019) to calculate sampling variance and confidence intervals for the recall, specificity, and balanced accuracy metrics. For the balanced accuracy metric, we calculated the sampling variance of the transformed measure by using the total number of records as the sample size. We did

not use double arcsine transformation (Doi & Xu, 2021) due to the inadequate properties of the back transformation of this measure (Röver & Friede, 2022; Schwarzer et al., 2019). For the *nMCC* metric, we calculated the sampling variance and confidence interval by transforming the correlations to Fisher's z-scores, as typically done in meta-analysis (Borenstein et al., 2009).

To derive the overall average performances across the $recall$, $specificity$, $bAcc$, and the $nMCC$ metrics, we fitted two versions of the so-called *correlated-hierarchical effects* (CHE) working models (Pustejovsky & Tipton, 2021). For investigation related to the 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 performances. Both types of models account for the multi-level structure of the data with 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 the SCE+ model concerns the use of different weighting schemes but the SCE model is generally recognized as the main working horse for deriving subgroup effects and conducting reliable contrast tests (Pustejovsky & Tipton, 2021). For differential effects comparisons, we used the HTZ Wald test suggested by Tipon and Pustejovsky (2015). Across both models, we estimated two sources of heterogeneity. That is the variability of the true performance differences within (ω) and between studies (τ). This allowed us to investigate at what level the largest true difference between the human screener performances existed.

3.2.3 Results

All the 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 0.786, 95% *CI*[0.747, 0.823] and 0.892, 95% *CI*[0.841, 0.934], respectively. Hereto, we found the two average group differences to be statistically distinct from each other with $F(1, 9.79) = 21.9$, $p < 0.001$. We detected minor substantial variations between the performance measures within studies with $\omega = 0.024$ and $\omega = 0.021$ 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 of the assistant screener seems to be consistent across the Campbell reviews. The overall average specificity for assistant screeners was 0.980, 95% *CI*[0.966, 0.990], and for review authors

0.989, 95% *CI*[0.979, 0.995]. We found no statistically significant difference between the two average estimates with $F(1, 12.31) = 1.64$, $p = 0.22$. We did only find very minor non-substantial variation within and between studies with the within-study variability $\omega = 0.004$ as the maximum for author screeners.

For assistant screeners, the average balanced accuracy was 0.877, 95% *CI*[0.860, 0.893], and for authors screeners it was 0.941, 95% *CI*[0.905, 0.968]. We found the difference between the group means to be statistically significant with $F(1, 10.1) = 18.52$, $p = .0015$. Finally, the overall $nMCC$ was 0.859, 95% *CI*[0.831, 0.882] and 0.931, 95% *CI*[0.892, 0.956] for the assistant and author screeners, respectively. These averages were found to be statistically different with $F(1, 9.87) = 14.93$, $p = .003$.

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

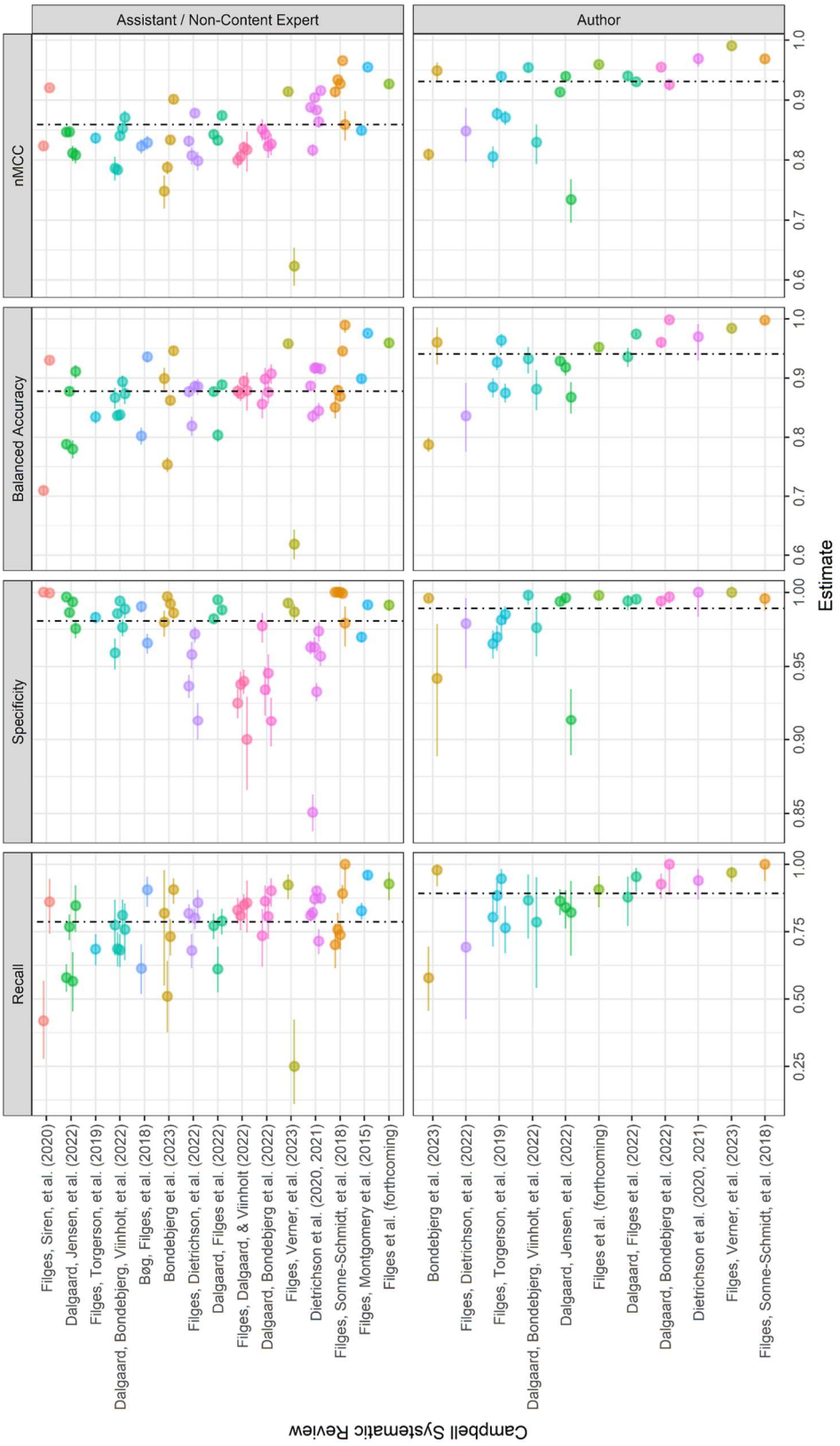
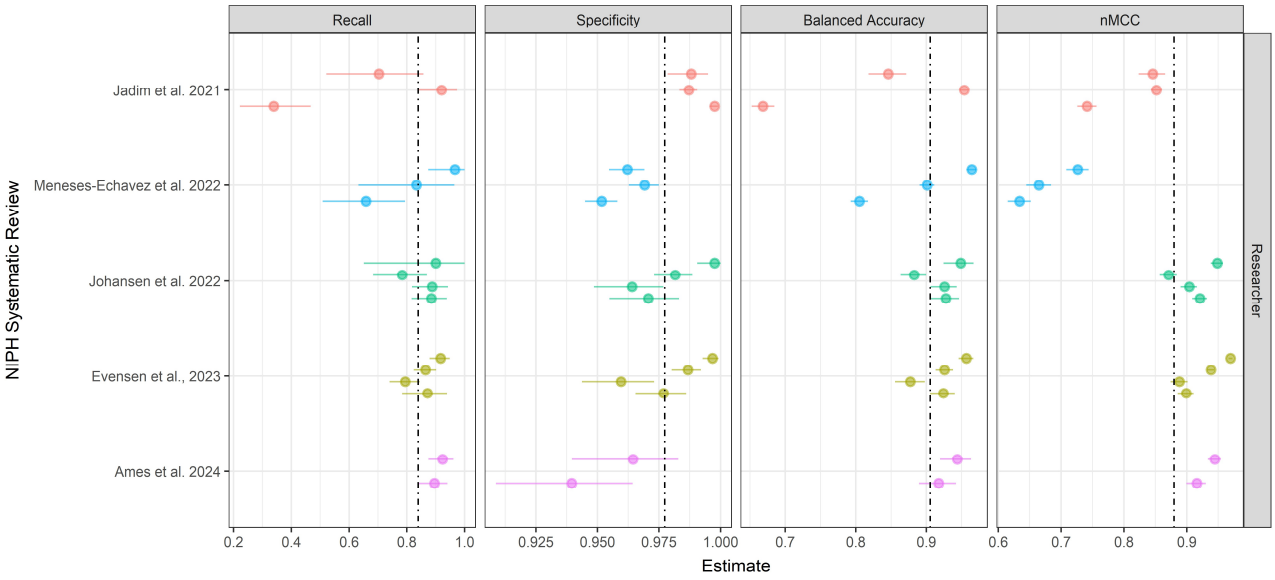


FIGURE 2. Performance measures within Campbell Systematic Reviews across assistant vs. author screeners. Dashed lines indicate the average estimated via the SCE+ model.

FIGURE 3. Researcher-researcher screening performance measures within NIPH Systematic Reviews. Dashed lines indicate the average estimated via the CHE-RVE model



3.2.4 Benchmark scheme

Bearing on the empirical results presented in the previous section, we developed the benchmark scheme presented in Table 3. On this basis, we suggest a course-grained rule of thumb saying that automated tools that can be shown to have recall rates equal to or above 80% and a specificity rate equal to or above 95% should be recognized as performing on par with common human screeners. Consequently, automated screening performances accommodating these thresholds should be accepted as independent screeners in high-standard 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 so low that it excludes the GPT models to function as an independent second screener. Also, we believe that automated tools can still be viable even when they yield a recall rate below 75%. Under such conditions, the automated tool could function as an extra assurance, working as a third screener that forces the duplicate human screeners to double-check close-to-relevant study records. This would enhance the screening, ensuring that the human screeners have not overlooked any relevant records. Finally, we think that automated tools that yield human-like recalls can be used to reduce the total amount of title and abstract records needed to be screened even

if the specificity rate is rather low. 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 can be seen from the benchmark scheme, we do not necessarily conceive a specificity of 100% to be ideal, since we think it is more appropriate that the GPT model forces the reviewers to double-check close-to-relevant references so that we can be assured that we have not missed any relevant studies. As a broad guideline, we do not consider it viable to use automated tools when they yield performance measures below 0.5.

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	Bad 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.	On par with common human screening performance

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

With this benchmark scheme, we aim to make a more flexible tool partially for assessing the screening performance of the automated tools and partially for assessing which screening tasks can be made under what performance conditions, avoiding the discussion ending up with a simple binary for and against automation argumentation. In other words, this scheme allows for case-specific use of GPT API models for various TAB screening tasks. We will interpret the finding of the classifier experiment presented in the next section in light of the benchmark scheme, as well.

4 CLASSIFIER EXPERIMENT

In the following section, we present the data and prompts used as well as the results for two large-scale classifier experiments. Differently from previous research, these classifier experiments aimed to test the performance of GPT API models when applied in social science reviews (proof of concept) and when the request body includes the use of function calling, avoiding the need to describe response behavior in the screening prompt(s). A side-effect of conducting these experiments was further to quality assure the AIscreenR package and ensure that the software yields appropriate screening behavior. We considered this test to be all-important if our suggested screening approach shall be scaled up. We narrowed the investigation to only concern two experiments since our purpose of this paper is not primarily to show that GPT API models work in all instances across all types of reviews. Instead, we aim to show that if set up adequately these models can function as *highly* reliably independent second screeners. This also means that using GPT API models as a second screener is not always ideal. We return to this issue in Section 5.2.

4.1 Data

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

A critique against classification Experiment 1 is it draws on a published open-access review, meaning OpenAI's GPT models can potentially have been trained on this review. If this is the case, this possibly excludes the opportunity to generalize the results of this experiment to applications where GPT API models are used to conduct screenings on prospective reviews where no

previous information has been fed to OpenAI's GPT models. To test and potentially overcome this issue, we conducted a second classification experiment drawing on data from an unpublished/ongoing systematic review. In classification experiment 2, we used screening data from a Campbell systematic review regarding the effects of the FRIENDS preventive programme on anxiety symptoms in children and adolescents conducted by Filges Smedslund et al. (2023).[8] This FRIENDS data in many aspects resembles the FFT data. For example, the inclusion criteria were rather simple and the intervention is well-defined. Moreover, the data is highly imbalanced with 64 relevant records in 2572 records, amounting to an approximate inclusion ratio of 25 relevant per 1000 records.

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

4.2 Prompt engineering

We engineered prompts used for the experiment 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

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

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

TEXTBOX 2: End of prompt added by AIscreenR

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

As previously mentioned, we did not add any instruction regarding how the model should respond to our request in the main prompt, as done in previous research evaluations. Instead, we built two JSON functions providing this instruction to the model (OpenAI, 2024). This should theoretically ensure that we get more reliable and standardized responses from the models. The main JSON respond function[9] we built included the instructions presented in Textbox 3:

TEXTBOX 3: Function call text

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

In the initial phase of our prompt engineering, we assumed that the more detailed background information we could add to the prompt the better the GPT API model would perform. This approach was based on the conception that the model needed to be trained with the correct wording. Yet, from our experience, this is a misperception of how this type of model works. As indicated in the GPT acronym, these models are *pre-training*, meaning that they don't need to be further trained in terms of wording. Instead what they need to work properly are concise (into-the-bone) prompts. Said differently, less is more. It was not that the models were entirely off but the test performance of the models dramatically increased when given more precise prompts with fewer inclusion/exclusion

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

criteria. Based on our experience, we now believe that it is inefficient to include multiple inclusion/exclusion criteria in the same prompt. Instead, we think that each inclusion/exclusion criteria should be prompted individually. We elaborate on this issue in Section 5.1. [10]

4.2.1 Performance tests

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

4.3 Analysis strategy

In both classifier experiments, we evaluated the performance of the GPT API model by using Equations (1) to (3). In this regard, the TP, TN, FN, and FP conditions were determined by comparing the GPT decision with the final decision made by a minimum of 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—just that they seemed to be relevant for full-text screening. Furthermore, in both experiments, we used the gpt-3.5-turbo-0613 and gpt-4-0613 reached from the 'v1/chat/completions/' endpoint. Due to the fact the gpt-3.5 models are generally considered to be less accurate in their response, we repeated the same screening 10 times for each title and abstract when using these models, as also done by Syriani (2023). We did so to test its consistency across the screenings and how it impacted its final inclusion decision. Partly because the gpt-4 models are considered to be more accurate (meaning being more consistent in their responses) and partly because of the cost, we only conducted one screening per title and abstract when calling this type of model. We used invariant top_p and temperature values, using the default value of 1 for both hyperparameters. As previously

[10] Moreover, we show did can be done in one of the accompaning vignette to the AIscreenR package.

mentioned, we interpret the results of the experiments using the benchmark scheme developed in Section 3 (cf. Table 3).

4.4 Results

All results for the two 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%, which can be considered to be on par with human screening. The gpt-3.5-turbo model was also able to reach human-like screening performances. Yet these result was 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.

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

TABLE 4: Results of the two classifier experiments

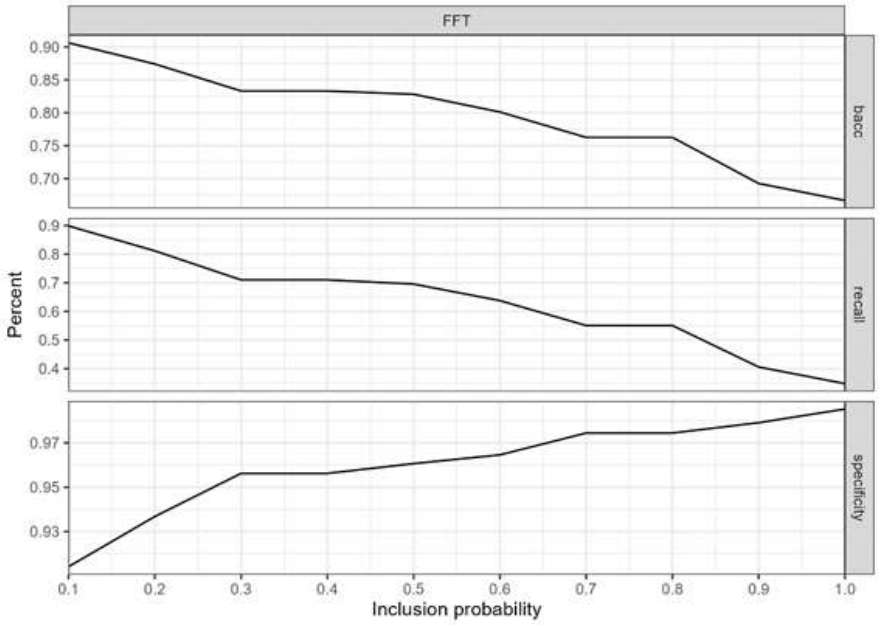
Review **Model**	**Reps**	**Recall (%)** **[TP/(TP + FN)]**	**Specificity (%)** **[TN/(TN + FP)]**	**Raw agreement (%)** **[(TP + TN)/*N*][a]**	**bAcc** **(%)**
FFT					
GPT-3.5-turbo-0613 (incl. prop = .5)	10	69.9 (48/69)	96.1 (3906/4066)	95.6 (3954/4135)	82.8

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

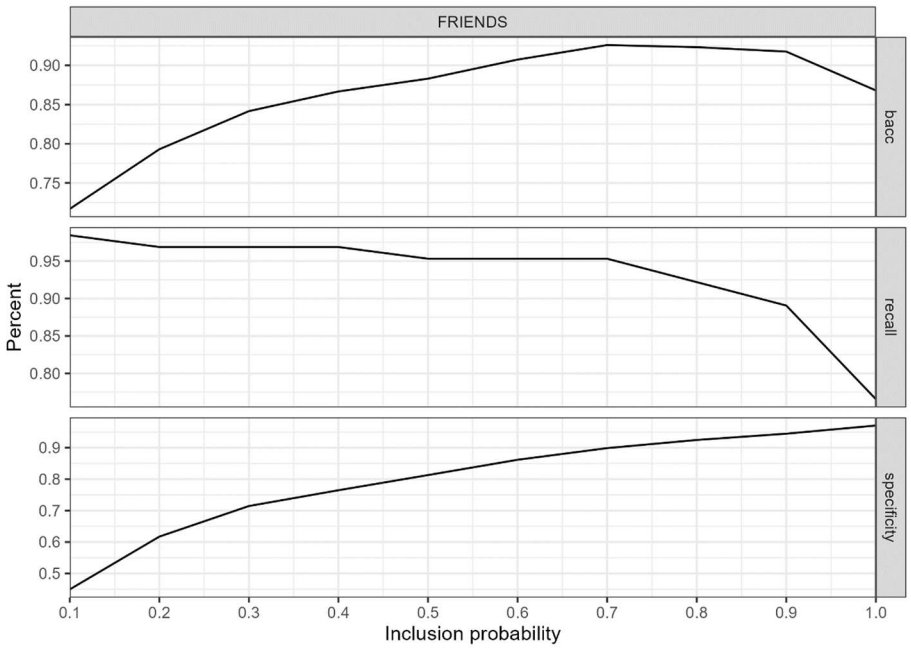
a: *N* is the total number of references

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

A



B



To summarise, we derive the following takeaways from the classifier experiments. First, we found that GPT API models can work as highly reliable and independent second screeners with recall performances on par or even better than common human screeners. 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 our models drew on function calling, aiming to provide more reliable responses from the GPT API models. Second, and in contrast with the performance of classical semi-automated screening tools (König et al., 2023), we partially found that GPT API models are not sensitive to imbalanced data and partially that the GPT-4 API models are capable of reaching recall rates close to 100%. Third, since we used the AIscreenR software to conduct the two classifier experiments, we feel safe to conclude that the software seems to work as expected. Hence, we believe that reviewers can safely use this software in high-standard systematic reviews. Fourth, we found that the GPT API models are not always as prompt-sensitive as suggested in previous evaluations (Gargari et al., 2024). Fifth, we found the GPT-4 API model to be preferable relative to GPT-3.5 since the latter is rather sensitive to the chosen inclusion probability across multiple identical screenings. Based on this finding, we generally recommend not using the GPT-3.5 API models when GPT-4 API models are available. Finally, we found that in some applications, the specificity rate reached by the GPT-4 API model can be seen to be on the lower end compared with human screeners. Yet, we do not find this to be a major issue when having high recall rates (cf. Figure 1) since this can just be seen as an extra

opportunity to double-check close-to-relevant studies. Thus, enhancing the change of not overlooking any relevant study records.

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

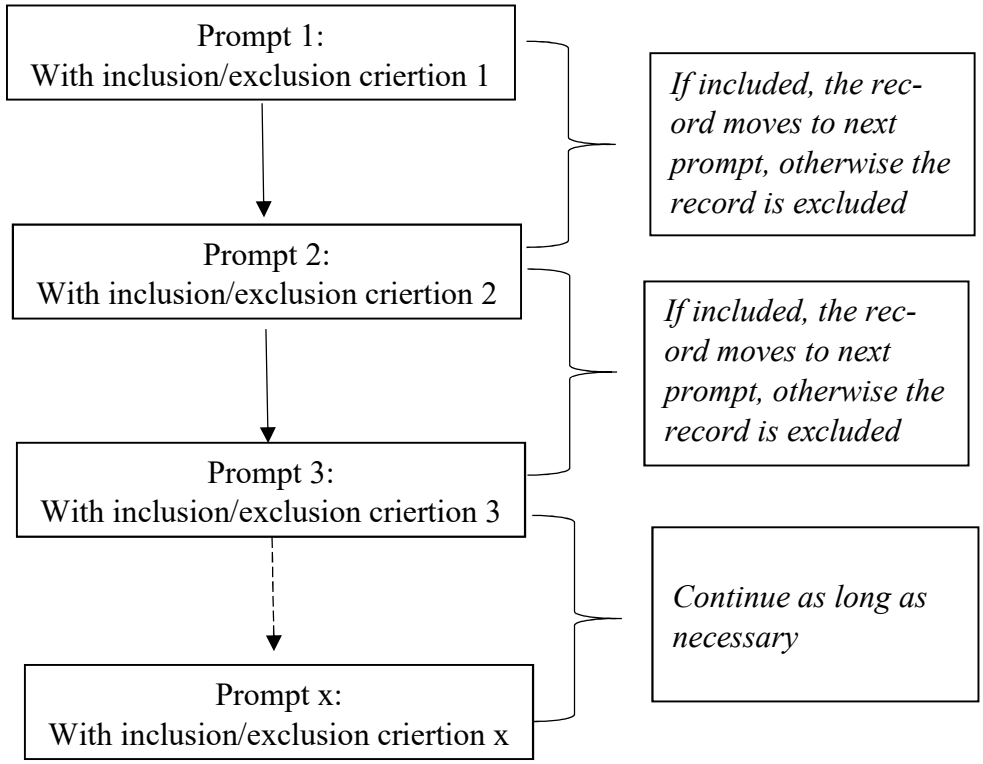
5 TENTATIVE GUIDELINES AND WORKFLOW

Premised on our developed benchmark scheme, our experience, and the results of the two classifier experiments, we have developed the following tentative guideline 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.

Before initiating a full-scale TAB screening with GPT API models, we generally recommend first thoroughly testing the screening performance of the prompt and GPT API model aimed to be used for the screening until it is ensured that the performance passes certain thresholds on the training data. The first step of the testing procedure involves locating approximately 10 relevant and 200 irrelevant study records with titles and abstracts, respectively. Locating more than 10 relevant study records might be ideal to test if the prompt(s) can detect various types of relevant records. That said, we experienced that using fewer than 10 relevant records could also unveil a proper recall performance of the prompt and models. Consequently, we cannot set this step in stone. When locating irrelevant records, we suggest randomly sampling those from the total pool of records. This aims to ensure that the specificity test rate can be generalized to the full sample of study records. After having collected the training dataset composed of the relevant and irrelevant study records, the next step concerns prompt engineering. A key part of developing well-performing prompts entails making them as concisely written as possible, only feeding them with the absolute most necessary information. From our experience, it seems to be the case that the more precisely written, the better the performance of the model. Importantly to remember, these models do not need to be trained, as we initially were caused to believe. Thus, if conducting a complex review including many inclusion/exclusion criteria, we suggest conducting what we have coined hierarchical screening. That is screening with

multiple prompts, where each inclusion/exclusion criteria should be prompted individually. This procedure is depicted in Figure 5. To support this type of screening, we how this can be practically executed in the accompanied vignette to the AIscreenR.

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 (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. If one's financial resources allow it, all title and abstract records could be screened with all prompts. This approach can potentially guard against bad prompting. Assume that one made seven prompts, one for each of the inclusion/exclusion criteria, but one of the prompts wrongly excludes (this should of course be tested beforehand) a large share of relevant records. Then those studies would be lost in the hierarchical screening suggested in Figure 5. If instead all records had been screened with all prompts then one

could avoid the above bias by including all records that were included in 6 out 7 prompts. This approach is less sensitive to the order of the prompts in the hierarchy.

When engineering prompts, we suggest that these should be re-written/refined until they reach the recall rate threshold of 80%, and ideally a specificity of 95%. Recall rates between 75% < 80% can also be accepted, but the reviewers should try to increase this performance as much as possible. Lower specificity rates can also be accepted as long as the recall exceeds 80%. Yet if the specificity rate of 75% 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. Most importantly, we suggest that if a recall rate of 80% cannot be reached, then the given GPT API model should not be used as an independent second screener. This can only be accepted if the given reviewer lacks financial resources since single-screening is less desirable than using a bad-performing GPT API model as an extra assurance of finding all relevant studies. However, the reviewer must be earnest about this shortcoming of the screening. Alternatively, if the thresholds cannot be reached, the GPT API model can still be used as a third screener, again as an extra security for detecting all relevant studies.

When the test has been passed, and the reviewers have decided to leverage the GPT API model as second screener, we suggest that the reviewers screen all study records before initiating the automated screening. Thereby, it is prevented that the human reviewers are impacted by GPT's decision. This also means that we recommend that decisions on whether GPT API model screening is appropriate in a given review should primarily be made before any TAB screening has been initiated. An alternative to manually screening all records at once is that reviewers repeat steps 6 to 9 in Table 5 with batches of 500-1000 study records. This would be an adequate way to steer the screening process and to continuously ensure that the given GPT API model performs as expected. Moreover, this reduces the risk of running large screenings that break for some technical reasons.

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 AI screener disagreed to test the consistency of the automated screening decision but also to get detailed responses for GPT's decisions. If the specificity performance of the GPT screener is high (e.g. 99.5%), 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.

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

Step	Reviewer action
1	Find approximately 10 relevant study records (ideally more).
2	Find a minimum of 200 irrelevant study records (ideally randomly sampled from the pool of records).
3	Construct the test dataset by combining the records from steps 1 and 2.
4	Develop one or multiple prompts and test the(ir) performance.
5	Repeat/refine step 4 until reaching a recall close to 80% or more, and a specificity between 90-100%. If this step cannot be fulfilled, we recommend not to use the GPT API model as a second screener. Thus, human double screening is the ideal solution. Yet, the GPT API model can still be used as a third screener for extra insurance of not missing any relevant studies. In cases where low budgets exclude human duplicate screening, we considered it fair to work with recall performances below 80% since the alternative (i.e., stand-alone single-screening) in these cases is worse.
6	Manually single screen all study records (could be divided into batches of 500-1000 study records). If a GPT API model has shown to be a reliable second screener based on the text data, then this can be done by multiple reviewers/screeeners.
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 detailed presentation of the to conduct TAB screening with GPT API models in practice.

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

- When having access to duplicate screening and the test performance is bad.

- When having few title and abstract records. If having 2000 or less it might be faster to just use human screening. The time needed to be spent on prompt engineering might exceed to time it takes to do manual screening.

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

		Number of studies	
Complexity of the review question(s)		Low	High
	Low	Questionable whether the time if worth investing in prompt development relative to just starting human screening	GPT screening is likely well-suited.
	High	Apply duplicate human screening	GPT screening is potentially well-suited. Consider using hierarchical screening

6 LIMITATIONS

- Black box (but this does not only count for GPT this is often true for human screening as well). Point to alternative models.
- Different performance across model updates
- Hard to keep up with new models and changes to the request body.
- Function tech? We have no control over the existence of OpenAI
- Environmental impact (embrace the critiques from van Lissa). Combine with traditional machine learning and text-mining tools to reduce the number of records needed to be screened by the GPT API model
- Prizing
- Depends on available models
- Generalizabiltiy acroos model are not ensured
- Prompt sensitive
- Convient dataset used to construct the screening performance benchmark scheme.
- Reproducibility

7 FUTURE RESEARCH

- The use of hierarchical prompting in complex reviews. Simple prompts instead of long onces
- Evaluate Mistral which provides the possibility of locally downloading their model. This will overcome issues with deprecations and ensure reproducibility over time.
- Shiny app to ease user set-up challenges (O'Connor et al., 2019) to make the workflow more user-friendly.
- Play with alternative function calls relative to the ones with have developed to test if the can increase the screening performance even futher.

8 DISCUSSION

- Talk about the interface here – cannot replicate the results on the ChatGPT interface
- Reviewers should not consider screening prioritization methods and GPT screening as two incommensurable methods. Instead, the strength from both should ideally be combined.
- Forces review times to make very narrow searches due to lack of ressources to conduct the title and abstract screening rigorously (Guo find in ICloud)
- We believe that the GPT-4 models will perform even better when fed with abstracts following a rigorous structure as in medicine.
- When not to use. If you cannot make the prompt work properly or if you screen very few studies.
- We believe that no automated tool should ever be at level 4 – there shall always be a human-in-the-loop to ensure adequate behavior the the screening tools. Consequently, GPT models used in non-systematic to reduce the number of studies needed to be screened should always include safety checks. For example, reviewers should randomly sample 5-10% of the studies excluded by GPT to test for serious flaws in its decision-making. If serious flaws are detected the reviewers must re-test the used prompt(s) or refrain from using the given GPT model.
- More rapid transfer of knowledge from review to policy, research, and practice
- Makes it possible to help to screen in extreme-sized reviews (Shemilt et al., 2014, 2016)
- Extra security in low-budget and/or time-limited projects where there is only access to a single screener.
- No need for unnecessary restriction on search string.

- To reduce the environmental impact and reduce the number of references needed to be screen. GPT API models could be used on a subset of studies, for example on all references not examined by humans after using priority screening.
- Future models should use seed to ensure reproducible screening. This is currently only available in the beta version but should be implemented in the software over time.
- Draw on 'function call' needs to be updated to work with tools.
- Requires continuous software development.
- When reviewers want to keep duplicate screening, we suggest that the GPT API models can be used as a third screener for extra insurance that all relevant studies are detected.
-

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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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