

Cognitive Effects and Correlates of Reading Fiction: Two Preregistered Multilevel Meta-Analyses

Lena Wimmer¹, Gregory Currie², Stacie Friend³, Jörg Wittwer¹, and Heather J. Ferguson⁴

¹Department of Education, University of Freiburg

²Department of Philosophy, University of York

³Department of Philosophy, Birkbeck, University of London

⁴School of Psychology, University of Kent

Despite significantly increasing research efforts, the psychological effects of reading fiction remain under debate. We present two preregistered meta-analyses synthesizing cognitive effects and correlates of reading fiction. In Meta-Analysis 1 (371 effect sizes/70 experiments), reading fiction led to significant small-sized cognitive benefits, $g = 0.14$, 95% confidence interval (CI) [0.06, 0.21]. This effect of fiction reading was moderated by the comparison group (effects were greater when reading fiction was compared with watching fiction or reading nothing than when reading fiction was compared with reading nonfiction) and the outcome variable (significant effects emerged for empathy and mentalizing only). In Meta-Analysis 2 (559 effect sizes/114 studies reporting correlations), lifetime exposure to print fiction was linked with significant small-sized cognitive benefits, $r = .16$, 95% CI [0.13, 0.19]. This effect was moderated by outcome variable (effects were greatest for verbal abilities, followed by general cognitive abilities and empathy/mentalizing/outgroup judgments), fictionality of the print material (greater effects were found for fiction than nonfiction), publication status (published work exhibited greater effects than unpublished work), type of assessment measure (larger effects emerged when neither the outcome nor print exposure were assessed via self-report, than when either the outcome or print exposure were assessed via self-report), participant group (community samples showed greater effects than student samples), study design (greater effects were found for correlation than for experimental designs), and percentage of female participants (via a negative relationship with cognitive benefits). Together, these meta-analyses provide robust evidence for a small-sized positive relationship between reading fiction and cognitive benefits.

Public Significance Statement

This research project suggests that people who read a lot of fiction have better cognitive skills than people who read little or no fiction. These benefits are small in size across various cognitive skills, but of medium size for verbal and general cognitive abilities, for example, intelligence. Importantly, there is a stronger association between reading fiction and cognitive skills than between reading nonfiction and those skills. However, whether the benefits are caused by reading fiction or by one or more other variables remains to be determined through future research.

Keywords: fiction, cognition, empathy, theory of mind, meta-analysis

We regularly encounter written fiction in our everyday lives. Increasing rates of reading fiction during the Covid-19 pandemic (Anuar et al., 2021; Cho et al., 2021; Davies et al., 2022) illustrate

the important role that fiction plays in humans' lives, even though attitudes toward reading (as indexed by how much students and their parents report liking reading) have become more negative

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Lena Wimmer  <https://orcid.org/0000-0001-8434-1766>

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Lena Wimmer contributed equally to data curation, formal analysis, investigation, project administration, visualization, and writing—original draft. Stacie

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Correspondence concerning this article should be addressed to Lena Wimmer, Department of Education, University of Freiburg, Rempartstraße 11, 79098 Freiburg im Breisgau, Germany. Email: lena.wimmer@ezw.uni-freiburg.de

internationally over the preceding decades (Hooper, 2020). While reading fiction is indisputably a popular activity, its psychological effects are debated (e.g., Best, 2021). The present article reports two meta-analyses synthesizing the cognitive effects of fiction-reading assignments using experiments and the cognitive correlates of lifetime exposure to written fiction.

Psychological Effects of Reading Fiction

Fictions, in the sense relevant here, can be conceived roughly as texts understood not to be intended as factually true, at least in large part; they typically involve reference to nonreal events, objects, and persons (e.g., Gerten & Köppe, 2009). Contemporary pragmatic approaches (e.g., Currie, 1985; Eco, 1994; Lamarque & Olsen, 1994) stress that fictions are based on converging intentions of a work's author and its readers (see also the concept of the writer-reader contract; Tierney & LaZansky, 1980), which in turn evolve from cultural conventions. For example, the content of *The Count of Monte Christo* (Alexandre Dumas) was made up by its author, and this is typically recognized by readers. In sum, nonfictional works are generally expected to be true to reality, whereas this requirement is relaxed for fictional ones (Friend, 2012).

Although entertainment is widely identified as the immediate purpose of most fiction consumption (e.g., Dubourg & Baumard, 2022), written fictions are also widely promoted as educational tools (e.g., Erikson et al., 2020; Schultz, 2020; Vrasidas et al., 2015). So, it is important for us to know what sorts of positive outcomes (if any) they tend to foster. In the next paragraphs, we provide an overview of some of the key issues in this controversy.

Empathy and Theory of Mind (ToM)

Regarding the effects of reading fiction, the field of social cognition (referring to the perception, interpretation, and response to social information; Fiske & Taylor, 2013) has attracted the most scholarly activity in recent years; special attention has been given to empathy and ToM. Generally speaking, empathy is an umbrella term for processes that help people share and comprehend others' emotions (H. J. Ferguson & Wimmer, 2023; Håkansson Eklund & Summer Meranius, 2021; note that we consider this a working definition as there is at present no generally accepted definition of empathy; see also Happé et al., 2017). Empathy is often considered multifaceted and hierarchical in that it involves lower and higher level processes (Schurz et al., 2021; Zurek & Scheithauer, 2017). Emotion recognition—decoding others' emotional states—exemplifies a lower level process (Preston et al., 2020). Affective or emotional empathy, that is, sharing others' emotions while conscious that one's emotional response is triggered by that of the other person (Cuff et al., 2016), and cognitive empathy, that is, understanding others' emotional state (Preston et al., 2020), are higher level processes. ToM, also known as mentalizing, is about comprehending mental states in general, not only emotional ones (Preston et al., 2020).

Some theoretical approaches (e.g., Mar, 2018a; Mumper & Gerrig, 2019) predict that reading stories—including but not exclusively fictional ones—improves empathy and ToM. As opposed to this, Mar and Oatley's (2008) simulation model assumes that reading fictional rather than nonfictional narratives especially has these benefits. More precisely, it maintains that fictional narratives, which are mostly about social relationships and interactions, function as simulations of

the social world, encouraging readers to imagine fictitious situations which abstract from reality. The fictionality of the simulations allows readers to adopt protagonists' perspectives in a distanced way, while the abstraction and simplification enable them to grasp the rules of the social world more clearly. Increases in empathy and ToM are predicted outcomes of these imagined simulations.

Indeed, several studies have suggested that reading short fictional texts enhances empathy and ToM (for a review, see Dodell-Feder & Tamir, 2018) and that lifetime exposure to print fiction is positively correlated with both capacities (for a review, see Mumper & Gerrig, 2017). However, two central issues remain unresolved. The first is the robustness of these findings in view of multiple failed replications and studies with null results (see, e.g., Chlebus et al., 2020; Lenhart & Richter, 2022; Panero et al., 2016; Samur et al., 2018; Wimmer, Currie, et al., 2021, 2022). Second, a capacity for high levels of empathy and ToM is generally considered desirable (e.g., A. M. Ferguson et al., 2021; Goldstein & Winner, 2012) and research confirming that reading fiction has robust positive effects on these capacities is likely to be welcomed. It is worth bearing in mind, however, the possibility that fiction has adverse effects in these areas. By adverse effects, we mean either an increase in a generally undesirable type of cognition, for instance, racist attitudes or stereotypical or inaccurate beliefs, or a decrease in an inherently desirable type of cognition, for instance, prosocial or accurate beliefs. According to Best (2020), engaging the brain network that subserves empathy and ToM during reading fiction may lead to source-monitoring errors; readers may not be able to link their knowledge with its source, so cannot reliably determine whether knowledge has been acquired from the real or from a fictional world. In that case, readers may seek to use schemas in the real world, which are valid only in a fictional world; similarly, inferences drawn during reading fiction could be mistakenly transferred to the real world. It is also possible that empathizing with fictional characters may, in some circumstances, reduce a person's capacity for empathy (Currie, 2020, Section 11.7, drawing on work on "moral self-licensing" by, e.g., Effron & Conway, 2015).

Knowledge, Biases, and Prejudices

Similar disputes about whether the effects of fiction are positive or negative are familiar in two other domains. (It is important to note that there need not be uniform answers in any of these cases: effects may be positive for some fictions in some contexts and negative in others.) First, it is often suggested that written fiction is a source of knowledge in the sense that it enables readers of all ages to acquire correct information and desirable attitudes (Best, 2021). Note, however, that a study by Wimmer, Currie, et al. (2021) found that frequent readers of fiction and frequent readers of nonfiction demonstrate equally high levels of general knowledge. Similarly, Hopkins and Weisberg (2017), in their review, state that "the current body of evidence does not allow us to conclude definitively whether learning from fiction is the same, better, or worse than learning from other sources" (p. 62). What is more, a number of experiments have demonstrated that readers may easily acquire incorrect information from fiction even though they are aware that authors of fiction do not stick reliably to the truth (e.g., Appel & Richter, 2007; Prentice et al., 1997; Rapp et al., 2014; Wheeler et al., 1999). A question unresolved by current research is whether beliefs acquired from fiction are on balance more likely to be correct than incorrect.

Second, there is a dispute regarding fiction-based effects on biases and prejudices. A number of experiments have shown that reading fiction can be used effectively for bias and prejudice reduction, for example, in terms of race bias (D. L. Johnson et al., 2014), prejudice against Arab Muslims (D. L. Johnson et al., 2013), and attitudes toward stigmatized groups more broadly (Vezzali et al., 2015). However, according to Goffin and Friend (2022), although these experiments provide evidence that fictions can reduce harmful biases and prejudices, they fail to demonstrate that fictions do not also increase them. In fact, these authors give reasons to think that adverse effects are more likely than positive ones.

The earlier mentioned simulation model (Mar & Oatley, 2008) does not pertain to fiction-based effects on knowledge, biases, and prejudices, particularly if these cognitions are not social in content. A theoretical approach concerning both social and nonsocial cognitive effects of reading fiction has been put forward by Consoli (2018). Whether reading fiction leads to desirable or undesirable cognitive outcomes is thought to depend on the accessibility of text content—highly accessible content would lead to shallow processing associated with unwanted outcomes, whereas less accessible content would lead to increased epistemic vigilance linked with cognitive benefits. The impact of accessibility on cognitive effects is conditioned on a number of variables, including reader characteristics (e.g., motivation, experience with subject matter, or personality). In sum, there are issues about the types of psychological outcomes that are influenced by reading fiction and about the desirability of these outcomes. These and other continuing debates over the cognitive effects of reading fiction highlight the need for a systematic literature review, as provided in a quantitative way by meta-analyses, to help resolve those debates.

Previous Meta-Analyses

Two existing meta-analyses can be viewed as steps in this direction, with both focusing specifically on social cognitive outcomes. First, Dodell-Feder and Tamir (2018) synthesized 51 effect sizes from 14 experimental studies in which participants were assigned short reading tasks. Studies were included if they followed a true experimental design with random allocation to condition, if reading fiction was compared with either reading nothing or reading nonfiction, and if social cognition was assessed as a dependent measure. When compared with control conditions, reading fiction was found to have significant but small-sized benefits for social cognition, $g = 0.15$. Moderator analyses tested whether this overall effect was modulated by the following variables: sample type (i.e., whether research participants were students or recruited from Amazon's Mechanical Turk—this could be critical due to differing incentives [i.e., participation in exchange for course credits vs. monetary compensation]); type of comparison group (i.e., whether reading fiction was compared with reading nothing or reading nonfiction—this is relevant to the question whether effects are driven by reading fiction in particular or reading in general); type of dependent measure (i.e., whether dependent measures were based on behavioral performance or on self-report—this is important since self-report measures have limited validity, especially for assessing cognition; e.g., Williams et al., 2017); type of outcome (i.e., whether mentalizing or emotion sharing was assessed—given that these are considered related but distinguishable constructs, both outcomes could be differently affected by reading fiction); participant age (this could be of interest

due to cross-generational alterations in reading habits and cognition's sensitivity to developmental changes); and the percentage of female participants in the sample (this seems reasonable since females have a stronger preference for fiction than males; e.g., Hu et al., 2023; Summers, 2013, and also tend to read more than males overall; Bradshaw & Nichols, 2004).

None of these variables was found to moderate the aggregate meta-analytic effect. To examine publication bias, the publication status of each study (i.e., whether it was published or not) was incorporated as a moderator variable, and a funnel plot and Egger's test were implemented. None of these tests suggested the presence of publication bias. Applying the same criteria for study inclusion as Dodell-Feder and Tamir (2018; except that only published work, written in English, was deemed eligible), Quinlan et al. (2023) investigated the evidential value of this body of research through a p -curve analysis. This analysis involved 13 studies, eight of which were included in Dodell-Feder and Tamir's (2018) meta-analysis. In principle, the results confirmed the evidential value. However, as statistical power was low, the evidential value was not robust.

Second, Mumper and Gerrig (2017) synthesized effects from 30 studies in which the frequency of reading fiction was correlated with social cognitive skills. Original empirical research was included if it represented a healthy adult sample (i.e., if participants were not recruited because they had been given a certain clinical diagnosis); if it examined a correlation between an indicator of lifetime print fiction and/or nonfiction exposure and a measure of social cognition (i.e., mentalizing or empathy); and if it was reported in English. The volume of both fiction and nonfiction reading was found to be significantly positively correlated with empathy, aggregate $r_s = .07$ and $.05$, respectively, and also with mentalizing, aggregate $r_s = .21$ and $.09$. Moderator analyses examined whether these synthesized effects were impacted by the following variables: publication status and percentage of participating women as in Dodell-Feder and Tamir (2018), and additionally the type of experimental design—this variable reflected whether dependent measures were assessed only after a reading assignment or if no such assignment had taken place, given that this sort of task can affect dependent measures. None of these variables emerged as a significant moderator. Aside from the moderator analysis for publication status, Mumper and Gerrig (2017) did not examine the risk of publication bias.

Importantly, the synthesis by Dodell-Feder and Tamir (2018) pertains to the effects of a single reading assignment typically comprising one short story or text excerpt. A single reading unit does not represent systematic training rooted in frequent engagement. Hence, the effects observed by Dodell-Feder and Tamir (2018) cannot be due to an acquisition of social-cognitive skills based on regular training (Eekhof et al., 2022; Mar, 2018a, 2018b). Other processes must be at work instead. For example, Mar has proposed that consuming a narrative, including reading a fictional story, could evoke a "social-processing mind-set" (Mar, 2018b, p. 259) or a "mentalizing mental mode" (Mar, 2018a, p. 469). These are states prompting better performance in subsequent tasks assessing social cognition. In the language of cognitive psychology, such facilitation of cognitive processes through prior activation is termed priming (see Lenhart & Richter, 2022). In the studies included in the meta-analysis by Mumper and Gerrig (2017), in contrast, participants were not usually tasked to read

before their social-cognitive abilities were tested, and cases in which such assignments were involved were controlled for statistically (by including the type of experimental design as a moderator variable). Hence, the findings reported by Mumper and Gerrig (2017) do not reflect processes of priming but could, in principle, trace back to the effects of repeated regular practice of social-cognitive skills through reading fiction.

Taken together, these two meta-analyses (Dodell-Feder & Tamir, 2018; Mumper & Gerrig, 2017) make important contributions to the literature as they show that reading fiction is accompanied by small-sized benefits for social cognition. However, although the body of research covered was adequate given the infancy of the field at the time, it is likely to have limited power. This may explain why neither meta-analysis detected any significant moderator variables. Furthermore, both meta-analyses were confined to the area of social cognition, so do not permit conclusions about broader cognitive effects and correlates of reading fiction.

The Present Meta-Analyses

We carried out two meta-analyses to significantly update and extend previous work. The hypotheses, methods, and analyses of both reviews were preregistered at the Open Science Framework, <https://osf.io/mk6tz>. Meta-Analysis 1 paralleled the review by Dodell-Feder and Tamir (2018), and Meta-Analysis 2 paralleled the one by Mumper and Gerrig (2017). We applied the same inclusion criteria as these authors with the following modifications: distinct from Mumper and Gerrig (2017) but in line with Dodell-Feder and Tamir (2018), we did not restrict samples to adults but included the full range of literate ages. Following Mumper and Gerrig (2017) but differing from Dodell-Feder and Tamir (2018), we did not include patient samples (note that our literature search did not detect any study that was excluded for working with a patient sample, so that ultimately this criterion did not have an effect; see Figure 1). Like Mumper and Gerrig (2017), we included studies reported in English only. Like Dodell-Feder and Tamir (2018), in Meta-Analysis 1, we included reading nothing and reading nonfiction as comparison conditions, in addition to any condition that did not involve reading fiction, such as watching fiction. This was deemed important to address the question of whether potential effects are due to fiction consumption, regardless of the activity of reading. Finally, in view of the controversies described (i.e., whether fiction-based effects on empathy and ToM are robust and/or linked with adverse effects, whether knowledge acquisition is a specific benefit of reading fiction or applies to reading nonfiction to a comparable or even higher degree, whether beliefs learned from fiction are on balance more likely to be correct than incorrect, and whether reading fiction is more likely linked with an increase or a reduction in biases and prejudices) we aimed to synthesize effects on all potentially longer lasting cognitive outcomes reflecting either desirable or adverse traits rather than states. Following Cattell and Scheier (1961), we understand traits (e.g., creativity, verbal abilities) as temporally sustained behavioral dispositions that generalize across situations, whereas states (e.g., story-world absorption, identification with story characters) are thought to fluctuate both across time and situational circumstances. This means that we expanded our focus beyond social cognition. State-level indicators of cognition were not considered because they do not inherently mirror a more or less persistent cognitive benefit or disadvantage.¹ Direct measures

of literacy were also excluded since we were interested in outcomes going beyond reading ability.

We examined the same moderator variables as Dodell-Feder and Tamir (2018) and Mumper and Gerrig (2017), that is, sample type, type of comparison group, type of assessment measure (as in Dodell-Feder & Tamir's, 2018 synthesis, this concerned whether measures were based on behavioral performance or on self-report), type of outcome, participant age, percentage of female participants, publication status, and type of experimental design, except for the following amendments: Dodell-Feder and Tamir's (2018) variable sample type (students vs. Amazon's Mechanical Turk workers) may reflect not just differing incentives, but also differences in participant supervision—students often complete study materials face to face with a researcher in a laboratory, whereas Amazon's Mechanical Turk workers by definition participate through an online survey, typically without researchers monitoring them. To allow us to disentangle the effects of incentives and supervision, we implemented two moderator variables: one for the type of participant group (students vs. community) and one for study format (in person vs. online). For Meta-Analysis 1, we added the length of reading assignments as a moderator variable. If reading fiction achieves purported effects on cognition because the cognitive skills relevant to real life are practiced during reading, then the size of effects should grow with increasing length of reading assignments. If other processes drive these effects (for instance, if fiction-reading assignments serve to temporarily prime cognitive skills that are assessed immediately after the assignment), no moderating influence would be expected. For Meta-Analysis 2, we implemented the fictionality of print exposure as an explicit moderator. Hence, we were able to ascertain whether correlations with cognitive outcomes are stronger for fiction than for nonfiction exposure. In contrast, Mumper and Gerrig (2017) ran separate analyses for fiction and nonfiction exposure, which did not allow such direct comparisons.

The following research questions were investigated in Meta-Analysis 1:

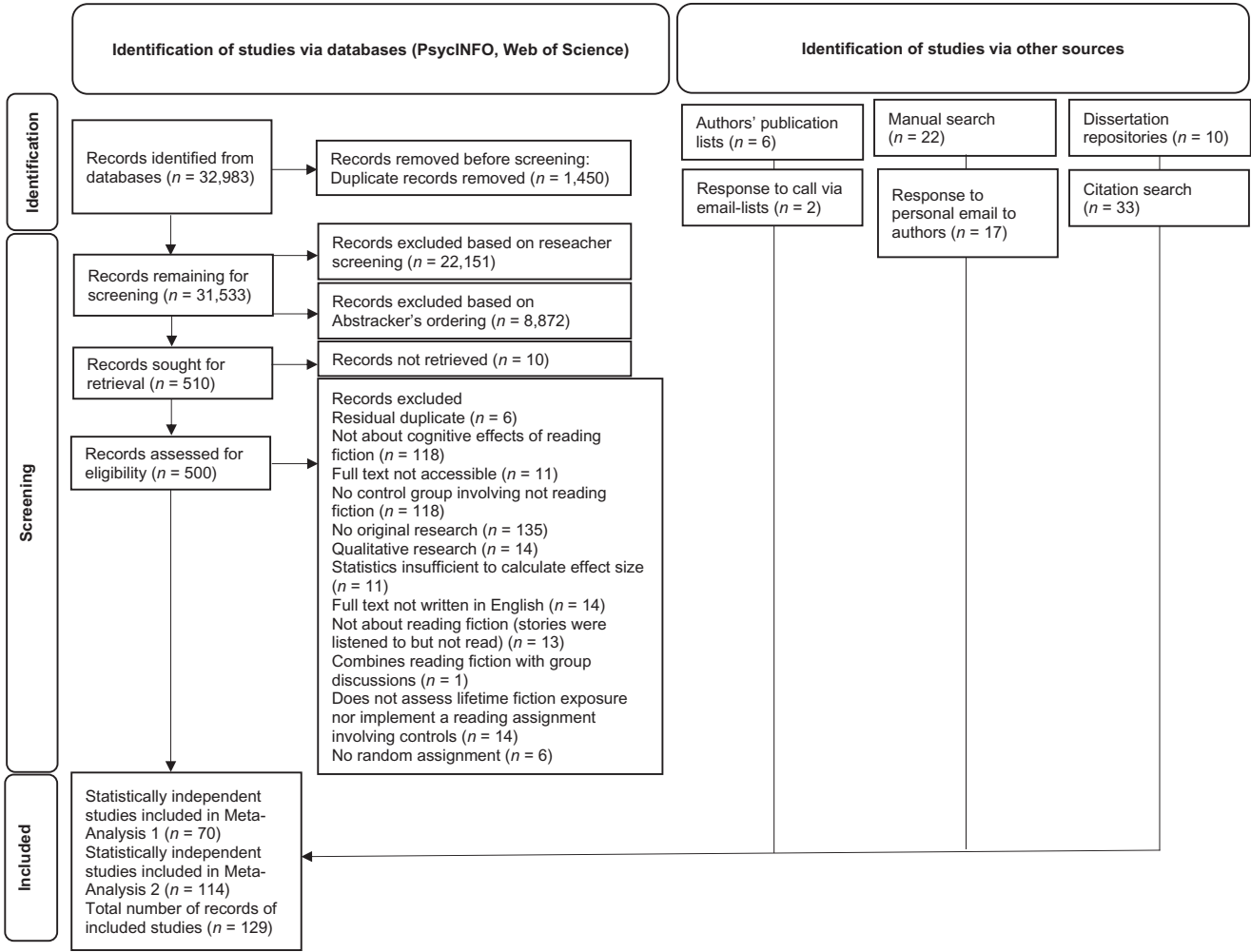
Research Question 1.1: How many experimental studies have investigated the cognitive effects of reading fiction?

Research Question 1.2: What is the synthesized effect size across these experiments?

Research Question 1.3: Does this total effect size depend on the moderator variables outlined above (i.e., type of assessment measure, publication status, study format, type of participant group, outcome variable, type of comparison group, participant age, percentage of female participants, length of reading assignment)?

¹ This is in conflict with our discussion of Dodell-Feder and Tamir's (2018) meta-analysis where we questioned the persistence of their findings. In its core, our argument concerned the brevity of the reading assignments, not the measures used to assess social cognition. Nevertheless, trait-level indicators, such as the ones employed, should not be sensitive to short interventions, which in turn raises concerns as to whether those indicators are in fact set at the level of traits. While we are unable to resolve this issue within the context of this article, we restrict the focus of the current meta-analyses to indicators that are typically believed to reflect longer lasting cognitive benefits or disadvantages, even if these may be generally considered state-level indicators in the future.

Figure 1
PRISMA Flowchart of the Literature Search



Note. PRISMA = Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

Meta-Analysis 2 was conducted to answer the following research questions:

Research Question 2.1: How many correlational studies have investigated the cognitive effects of reading fiction?

Research Question 2.2: What is the synthesized effect size across these studies?

Research Question 2.3: Does this total effect size depend on the moderator variables outlined above (i.e., type of assessment measure, publication status, study format, type of participant group, outcome variable, fictionality of print exposure, study design, participant age, percentage of female participants)?

Materials and Method

Due to a high degree of overlap regarding materials and method, this section is reported jointly for the two meta-analyses.

Selection Criteria

Meta-Analysis 1

We considered research that meets the following inclusion criteria:

1. It follows a true experimental design with random assignment to condition.
2. It includes a control condition that does not involve reading fiction.
3. It includes at least one measure of cognition that goes beyond both literacy and state-level indicators; following the APA dictionary of psychology (VandenBos, 2015), we defined cognition as all forms of knowing and awareness, such as perceiving, conceiving, remembering, reasoning, judging, imagining, and problem-solving.
4. Full text is written in English.

The only exclusion criterion was:

- research that focuses on patient samples (however, we included data from nonclinical or control groups where appropriate)

Meta-Analysis 2

We included research that fulfills the following criteria:

1. It correlates at least one measure of lifetime exposure to print fiction or nonfiction with at least one measure of cognition that goes beyond both literacy and state-level indicators; cognition was defined as in Meta-Analysis 1.
2. Full text is written in English.

We applied the following exclusion criterion:

- research that focuses on patient samples

Reports that did not contain sufficient information to assess eligibility were excluded. If reports did not include sufficient statistical information for analysis or if this information could not be calculated from publicly shared data, authors with findable current contact details whose reports were published within the past 10 years were contacted. In cases where the necessary details still could not be obtained, reports were excluded.

Search Strategy

We performed a joint literature search for both meta-analyses. On April 21, 2022, a database search was carried out using PsycINFO (via EBSCOhost) and Web of Science (via Clarivate), in each case with the search term “*fic* OR non-fic* OR nonfic**” (no specific search fields selected). To retrieve unpublished work, the same search term was used on the PsyArXiv Preprints server (<https://psyarxiv.com>; August 30, 2022) and on the following dissertation repositories: Dissertations Express (<https://dissexpress.proquest.com/search.html>; August 30, 2022), Open Access Theses and Dissertations (<https://oatd.org>; August 31, 2022), and Theses Canada Portal (<https://library-archives.canada.ca/eng>; August 30, 2022). Our preregistration additionally included the repository OpenThesis (<https://www.openthesis.org>); however, this website was not accessible at the time of data collection, so it was not included as an information source. We also searched the publication lists of authors whose studies were included (at least one author of each report was considered). In addition, we contacted these authors for unpublished studies and put out a call for unpublished studies via the mailing lists of the Society for Text and Discourse (September 7, 2022) and the International Society for the Empirical Study of Literature (September 14, 2022). Furthermore, we searched the reference lists of articles meeting inclusion criteria and also the reference lists of the reviews/meta-analyses by Appel et al. (2021), Barnes (2018), Best (2020, 2021), J. M. Black et al. (2021), Burke et al. (2016), Dodell-Feder and Tamir (2018), Eekhof et al. (2022), Goffin and Friend (2022), Hopkins and Weisberg (2017), Jacobs and Willems (2018), Koopman (2018), Mar (2018b), Mumper and Gerrig (2017), Oatley et al. (2018), and Wimmer and Ferguson (2023). We also considered research reports found through manual search. Three authors were contacted since full-text reports did not include all the statistics needed to calculate effect sizes; two replied, with one providing the requested details.

Data Extraction and Coding of Moderator Variables

All steps reported within this section were carried out by Lena Wimmer. Titles and abstracts of the reports retrieved through database search (i.e., involving PsycINFO and Web of Science) were screened using Abstrackr (Wallace et al., 2012). This tool utilizes machine-learning algorithms trained by researchers' screening decisions in order to present citation abstracts in such a sequence that those with the highest probability of meeting inclusion criteria appear first within the screening process. Tsou et al. (2020) demonstrated that Abstrackr is effective for this purpose. They further suggested that when adopting a conservative approach, it is feasible to stop screening after 70%–80% of reports have been examined. In line with this suggestion, screening was terminated after 22,661 out of 31,533 possible inspections, corresponding to a screening rate of 71.86%. Records identified through other sources were screened based on titles and additionally on abstracts if the title was compatible with the topic of the current meta-analyses. To further assess the eligibility of the entries remaining after screening, each full text was examined. The final set of reports used for analyses was approved by all authors.

The following information was extracted from reports for both meta-analyses:

- author names
- year of publication
- title
- publication status (published vs. unpublished)—Research was regarded as published if respective reports were released by a publishing company, for example, as journal articles or chapters in edited books, and otherwise as unpublished, for example, dissertations and manuscripts submitted to but not accepted for publication in a journal.
- study format (online vs. in person)—This variable was primarily meant to reflect the degree of participant supervision, which is typically relatively high in research carried out in person and relatively low in online research. Consequently, study formats within which participants completed paper-pencil-based measures remotely, that is, without researcher supervision, were coded as online.
- types of participant groups (students vs. community)—This variable was geared toward the distinction between convenience samples, which in psychological research routinely consist of students who receive course credits in exchange for participation, and nonconvenience samples, who are recruited from the general public and often receive monetary compensation. In line with this, school pupils were coded as community samples, as were samples that included students as a minority.
- sample size—Split by experimental group for Meta-Analysis 1 and total sample size for Meta-Analysis 2.
- participant age in years (mean and standard deviation)—Split by experimental group for Meta-Analysis 1 and for the total sample for Meta-Analysis 2.
- percentage of female participants—Split by experimental group for Meta-Analysis 1 and for the total sample for Meta-Analysis 2.
- outcome variable—Groupings served three aims: first, that subcategories were in line with contemporary cognitive concepts; second, that the subcategories were distinct; and third,

that the number of outcome variables that could be assigned to one of the subcategories was maximized. This was meant to maximally exhaust the current pool of studies in the moderator analyses to test for the effects of different outcome variables. As a result, the following subcategories were used for Meta-Analysis 1: empathy (a collective term for processes conducive to sharing and comprehending others' affects; Håkansson Eklund & Summer Meranius, 2021); knowledge (e.g., understood as true belief, as in Allen et al., 1992); mentalizing (understanding mental states; Preston et al., 2020); moral cognition (thought processes relevant to morality, i.e., the repertoire of customs and values prevalent in a given society to govern social conduct; Moll et al., 2005); outgroup judgments (judgments about people from a group that participants do not belong to or identify with; VandenBos, 2015); and other thinking processes (cognitive processes not covered by the aforementioned subcategories). Meta-Analysis 2 was based on the following subcategories: creativity (the capacity to generate novel and useful products; Mumford, 2003); general cognitive abilities (processes contributing to reasoning, planning, problem-solving, abstract thinking, understanding complex ideas, and learning fast and from experience; Gottfredson, 1997); verbal abilities (the capacity to apprehend and communicate adequately with words; VandenBos, 2015); and empathy, mentalizing, moral cognition, outgroup judgments, and other thinking processes (as defined in Meta-Analysis 1).

- name of assessment measure (e.g., Interpersonal Reactivity Index; Davis, 1980).
- results—For Meta-Analysis 1, we extracted means and standard deviations of the dependent measure, split by experimental group, or alternative statistics if these were not reported; for Meta-Analysis 2, we used bivariate correlations between lifetime exposure to print fiction or nonfiction with each cognitive outcome.
- type of assessment measure that specifically targeted the use of self-report measurements—These assessments are prone to biases such as socially desirable responding, a limitation meant to be circumvented in other measures, especially those based on behavioral performance (e.g., Moosbrugger & Kelava, 2020). Hence, the risk of bias was deemed to be higher for self-report than for performance-based measures. For Meta-Analysis 1, we evaluated each dependent measure according to whether it relied on self-report or behavioral performance. For Meta-Analysis 2, the assessment of both the independent variable (i.e., lifetime print exposure) and the dependent variable(s) was taken into account. The levels of this moderator variable were, first, both the outcome and print exposure were assessed via self-report; second, either the outcome or print exposure was assessed via self-report; and third, neither the outcome nor print exposure was assessed via self-report.

For Meta-Analysis 1, we additionally extracted the following information:

- type of condition that was compared to reading fiction—The current pool of studies contained reading nonfiction, reading nothing, and watching fiction.

- length of the reading assignment as word count—If the precise word count was not reported, it was estimated based on numbers provided at <https://self-publishingschool.com/how-many-words-in-a-novel/>.

The following information was extracted for Meta-Analysis 2 only²:

- study design (correlational vs. experimental)—Experimental designs referred to studies in which cognition was assessed after a reading assignment, whereas no such assignments were set within correlational designs.

The coding scheme is available at <https://osf.io/dus2w/>.

Statistical Analysis

Effect size calculations were based on the formulas provided in Borenstein (2009) and Lipsey and Wilson (2001) as well as the esc package in R (R Version 4.2.3). All further meta-analytical steps were carried out using the R metafor package (Viechtbauer, 2010). If not otherwise mentioned, we adopted the standard 5% significance level for inferential tests. Meta-analytical code can be accessed at <https://osf.io/dus2w/>. For Meta-Analysis 1, effect sizes were calculated as bias-corrected Hedges' *g*, representing the standardized mean difference between reading fiction and the comparison group, such that positive effect sizes represent better performance in the fiction group. In order to replicate Dodell-Feder and Tamir's (2018) approach, we used raw unadjusted means and standard deviations and compared only postreading between-groups scores. For Meta-Analysis 2, effect sizes were obtained by transforming the correlations from each contributing study into a Fisher's *Z* score, then averaging the *Z* scores together, and transforming the averaged *Z* score back into an aggregate correlation for ease of interpretation. Positive correlations indicate a positive linear relationship between lifetime exposure to written fiction and cognitive benefits. If cognitive skills were assessed more than once within a study, we utilized only the correlation between print exposure and the first cognitive assessment.

Effect sizes were weighted by their inverse variance within both meta-analyses. We observed two types of dependencies in our data: first, more than one dependent measure was assessed in the same sample, and second, the same group of participants was compared more than once with another group of participants on the same dependent measure. Thus, we implemented multilevel random-effects meta-analytic models that account for variance at the different levels, in particular, variance in the observed effect sizes (Level 1), variance between effect sizes within a study (Level 2), and variance between studies (Level 3). Since there were cross-level dependencies, we generated cluster-robust standard errors, statistical tests, and confidence intervals on estimates from the multilevel meta-analytic models. To gauge heterogeneity, we calculated $\chi^2(Q)$ and Higgins I^2 (Harrer et al., 2021) statistics.

²Our preregistration included fiction genre as a moderator variable. However, this could not be put into effect since the vast majority of studies reported general fiction scores only but did not differentiate particular fiction genres.

The robustness of our findings was assessed by the following sensitivity analyses (paralleling the approach taken by Dodell-Feder & Tamir, 2018): first, we examined effect sizes for influential outliers, defined as effect sizes with standardized residual values exceeding 3.0 (Cohen & Cohen, 2003) whose Cook's distance values exceeded $4/(n - k - 1)$ (Fox, 1991). Second, we employed a leave-one-out procedure to gauge the impact of each individual effect size (i.e., rerunning the multilevel model leaving out one effect size at a time) and study (i.e., rerunning the multilevel model leaving one study out at a time) on the overall effect and amount of heterogeneity.

The impact of moderator variables was tested in a series of meta-regression models, with each model including one moderator variable. Finally, the following tests ascertained the risk of publication bias: we included publication status (published vs. unpublished) as a moderator within metaregression models; we also compiled funnel plots and conducted respective Egger's tests. In addition to these preregistered analyses, we carried out *p*-curve analyses using the *p*-curve app (Version 4.6; <https://www.p-curve.com/app4/>).

Transparency and Openness

All meta-analytic data, analysis code, and research materials (including our coding scheme) are available at <https://osf.io/dus2w/>. This project was preregistered at the Open Science Framework: <https://osf.io/mk6tz>.

Results

To follow the conventions of multistudy articles, the statistical results of Meta-Analysis 1 are reported before the results of Meta-Analysis 2.

Meta-Analyses 1 and 2: Number of Studies

Our database search detected 32,983 entries on April 21, 2022. During the initial selection process, duplicates were removed. After titles and abstracts were screened, full texts of the remaining records were sought and checked for eligibility. Figure 1 provides detailed information on the number of entries excluded at each step. Finally, 39 records were found eligible for one or both meta-analyses. A further 90 reports were detected through means other than database search (as outlined above, namely authors' publication lists, manual search, dissertation repositories, responses to calls via email lists, authors' responses to personal emails, and search of reference lists). In sum, 129 reports were included in the present meta-analyses, 12 of which were included in both meta-analyses. Meta-Analysis 1 included 70 and Meta-Analysis 2 included 114 statistically independent studies.

Meta-Analysis 1

Characteristics of Studies and Samples

The current pool of studies involved 5,640 participants assigned to read fiction and 5,532 participants allocated to a control condition. For individual studies, sample sizes of groups assigned to read fiction varied between $n = 12$ (F. Hakemulder, 2000, Study 2) and $n = 342$ (Kidd & Castano, 2017a; Panero et al., 2016), interquartile range (IQR) (30.25, 78.75), while those of control groups varied between $n = 12$ (F. Hakemulder, 2000, Study 2) and $n = 189$ (Kidd & Castano, 2017a; Panero et al., 2016), IQR (30, 75.5).

Mean ages ranged from 5.55 (Justice et al., 2005) to 44.00 years (Maxim, 2022, Sample 2), IQR (19.80, 34.20). Regarding participant gender, the percentage of female respondents varied between 38.20% (Vezzali et al., 2021, Experiment 2) and 87.00% (Lenhart & Richter, 2022, Experiment 2), IQR (52.60%, 76.37%). About 71.43% of studies were published (vs. unpublished), 55.88% were conducted in person (vs. online), and 61.43% worked with a community (vs. student) sample. For 76.20% of effect sizes, reading fiction was compared to reading nonfiction, while 19.83% of effect sizes concerned comparisons with watching fiction. The length of the reading assignment ranged from 273 (Kilaru et al., 2014) to 350,100 words (Vezzali et al., 2021, Experiment 2), IQR (1,500, 6,000). In terms of the outcome variables, empathy was addressed by 28.65% of comparisons, knowledge by 11.89%, mentalizing by 21.62%, moral cognition by 21.35%, outgroup judgments by 14.05%, and other thinking processes by 2.43% (for exemplary measures of each outcome variable, see Table 1). Dependent measures were based on behavioral performance for 56.00% of comparisons and on self-report for the remaining 44.00%.

Meta-Analysis and Robustness Checks

Our primary research question addressed whether and to what extent reading fiction, compared with control activities, enhances cognition. According to our meta-analysis of 371 effect sizes from 70 studies, compared with control activities, reading fiction led to greater cognitive benefits. This effect was small in size but statistically significant, $g = 0.14$, 95% confidence interval (CI) [0.06, 0.21], $p = .0004$. The presence of heterogeneity was indicated by a significant Q statistic, $Q(370) = 1,086.50$, $p < .0001$. Furthermore, total I^2 was 72.11%, indicating a substantial amount of true variance (vs. sampling error) in effect size estimates. The majority of this came from the between-study cluster, $I^2_{\text{Level3}} = 52.38\%$, compared with the within-study cluster, $I^2_{\text{Level2}} = 19.72\%$. In sum, these findings suggest that reading fiction leads to small-sized cognitive benefits and that the effect sizes differ systematically between studies due to factors that vary between studies (e.g., participant age, type of assessment measure). Robustness of the effect was confirmed using sensitivity analyses: standardized residual values ranged from -1.22 to 1.62 ; hence, all figures were below the critical cutoff of 3.0 (as absolute value; Cohen & Cohen, 2003). Using the leave-one-out procedure at the effect size level, aggregate effect size estimates varied between $g = 0.13$ and $g = 0.14$, while total I^2 was between 71.33% and 72.77%. For the leave-one-out procedure at the study level, aggregate effect size estimates ranged from $g = 0.12$ to $g = 0.15$, and total I^2 from 70.22% to 73.79%.

Moderator Analyses

A series of moderator analyses tested whether the significant amount of heterogeneity could be explained by two types of factors: categorical (e.g., type of assessment measure) and continuous variables (e.g., sample age). A summary of the overall meta-analytic effect and the effects of these moderator variables is given in Table 2. For categorical moderators with up to three levels, we ran metaregression models with an intercept. In this case, the p value of the corresponding effect size parameter (see Table 2) indicates whether the effect of the intercept differs significantly from zero.

Table 1

Outcome variable	Measure	Abbreviation	Description	Method of assessment	Meta-Analysis 1: original studies using measure	Meta-Analysis 2: original studies using measure
Creativity	Versions of the alternate uses task (Dippo, 2013; George & Wiley, 2019; Guilford, 1967)	AUT	List creative uses of common objects	Behavioral performance	N/A	Black and Barnes (2021b); Wimmer et al. (2024)
	Behavioral Identification Form (Vallacher & Wegner, 1989)	BIF	Choose one of two alternative descriptions (one lower and one higher in level) of an act	Behavioral performance	N/A	Stanisic (2019)
	Insight problems taken from Förster et al. (2004)	N/A	Solve problems likely to provoke an impasse, the conquest of which is typically described as “aha” experience	Behavioral performance	N/A	Stanisic (2019)
Empathy	Reading the Mind in the Eyes Test—Revised (Baron-Cohen et al., 2001)	RMET	Selecting one of several response options, determine actors’ mental state from photograph presenting only their eye region	Behavioral performance	Black and Barnes (2015a), De Mulder et al. (2017), Dodel-Feder et al. (2022), Kidd and Castano (2013), Lenhart and Richter (2022), and others	Castano et al. (2020), Kidd and Castano (2017b), Samur et al. (2017), Schwerin and Lenhart (2022), Takahashi et al. (2023)
	Interpersonal Reactivity Index (Davis, 1980)	IRI	Using a rating scale, indicate the following tendencies: take others’ perspective, feel empathic concern for others, transpose oneself into fictitious characters, feel distressed in tense interpersonal settings	Self-report	D. L. Johnson et al. (2013), Pigden (2021), Savitri and Nulha (2021), Seddio (2017), Walkington et al. (2020), and others	Eekhof et al. (2021), Liu and Want (2015), Tabullo et al. (2018), Waytz et al. (2015), Whalen (2010)
	Eye-tracking paradigm on sad versus emotionally neutral videos	N/A	Watch sad versus emotionally neutral videos while participants’ pupil size and fixations on the actresses’ eye region are tracked	Implicit measure/hot self-report	Wimmer, Currie, et al. (2022)	Wimmer, Currie, et al. (2022)
General cognitive abilities	Versions of verbal fluency tasks (see, e.g., Sincoff & Sternberg, 1987)	N/A	Produce as many words of a given category as possible within a fixed time frame	Behavioral performance	N/A	A. E. Cunningham and Stanovich (1991), Stanovich and Cunningham (1992), Tabullo et al. (2020)
	Grade point average	GPA	Average of academic grades achieved	Behavioral performance	N/A	Spear-Swerling et al. (2020), Tabullo et al. (2020)
	Versions of matrices tests (e.g., Raven, 1962)	N/A	Indicate the option that completes a pattern arranged in a matrix	Behavioral performance	N/A	Brybaert et al. (2020), Grant (2011), Lenhart et al. (2023), Stanovich and Cunningham (1992)
Knowledge	Science knowledge about dinosaurs	N/A	Select the single correct response regarding dinosaurs	Behavioral performance	Willis (1998)	N/A
	Autism Stigma and Knowledge Questionnaire (Harrison et al., 2017)	ASK-Q	State whether one agrees with, disagrees with, or is uncertain about the accuracy of statements on ASD in terms of diagnosis, etiology, treatment, and stigma	Behavioral performance	Stern (2020)	N/A
	Recall of opioid prescription guidelines	N/A	Write down all information one can recall from a reading without reviewing it	Behavioral performance	Kilaru et al. (2014)	N/A
Mentalizing	Yoni task (Shamay-Tsoory & Aharon-Peretz, 2007)	N/A	Identify the item Yoni (cartoon outline of a face) refers to based on a sentence and specific cues (e.g., Yoni’s eye gaze and/or facial expression)	Behavioral performance	De Mulder et al. (2017), Kidd and Castano (2013), Turner and Vallée-Tourangeau (2020)	De Mulder et al. (2017), Turner and Vallée-Tourangeau (2020), Van Kuijk et al. (2018)
		IPT-15	Having watched a video vignette involving social interactions, select the correct	Behavioral performance	None	Bischoff (2013), Mar et al. (2006)

(table continues)

Table 1 (*continued*)

Outcome variable	Measure	Abbreviation	Description	Method of assessment	Meta-Analysis 1: original studies using measure	Meta-Analysis 2: original studies using measure
Moral cognition	Interpersonal Perception Task-15 (Costanzo & Archer, 1993)	N/A	statement about the interaction based on inferences from characters' dynamic nonverbal cues	Behavioral performance	Wimmer, Currie, et al. (2022)	Wimmer, Friend, et al. (2021)
	Revised version of the Frith-Happé animations task (White et al., 2011)	N/A	Responding to multiple-choice questions, ascribe mental states/interactions to animated triangles	Self-report	None	J. M. Black and Barnes (2021c)
	Moral Identity Questionnaire (J. E. Black & Reynolds, 2016)	IAT	On a Likert scale, rate the importance morality plays in one's identity and the emphasis one puts on acting in accordance with moral principles	Implicit measure/behavioral performance	Pigden (2021), Wimmer, Currie, et al. (2022)	Redman (2017), Wimmer, Currie, et al. (2021), Wimmer, Friend, et al. (2021)
	Versions of the Implicit Association Test (Greenwald et al., 1998)	MJT	Respond to a concept with the same key used to respond to either a moral or immoral concept; the more the key pairings with immoral concepts require longer response latencies than key pairings with moral concepts, the higher one's level of moral cognition	Behavioral performance	Dodell-Feder et al. (2022), Kidd and Castano (2019)	Black and Barnes (2021a), Xu et al. (2022)
Outgroup judgments	Version of the moral judgment task (e.g., Young et al., 2012)	ASI	Using a rating scale, assess the moral permissibility of actions varying in the valence of their actual results (neutral vs. negative) and in the valence of their results as expected by the agent (neutral vs. negative)	Self-report	Redman (2017)	Redman (2017)
	Ambivalent Sexism Inventory (Glick & Fiske, 1997)	N/A	Using a rating scale, indicate the degree to which one agrees with statements reflecting hostile and benevolent sexism	Implicit measure/behavioral performance	D. L. Johnson et al. (2013)	None
	Word fragment completion task	GTS-R	Complete word fragments that have either a positive and a neutral completion or a negative and a neutral completion. Positive outgroup judgments are reflected by a high number of positive versus neutral completions; negative outgroup judgments are reflected by a high number of negative versus neutral completions	Self-report	Orellana et al. (2022)	None
Verbal abilities	Genderism and Transnegativity Scale (Tebbe et al., 2014)	N/A	Using a rating scale, state the extent to which one agrees with statements reflecting negative attitudes and the inclination toward violence toward transgender individuals	Behavioral performance	N/A	Mar and Rain (2015), Martin-Chang et al. (2021)
	Sentence completion task (items taken from the reading section of the SAT)	N/A	Choosing from multiple response options, decide how to best complete a sentence	Behavioral performance	N/A	Brysbaert et al. (2020)
Other thinking processes	Yes/No Vocabulary Test (Lemhöfer & Broersma, 2012)	N/A	Indicate whether or not a string of letters is a word in a given language	Behavioral performance	N/A	Grant (2011), Stanovich and Cunningham (1992), Vermeiren et al. (2023)
	Vocabulary subtest of the Nelson-Denny Reading Test (Brown et al., 1993)	MEQ	State the meanings of words selecting one of multiple response options each	Self-report	Maxim (2022)	None
Other thinking processes	Adapted version of the Memories Experiences		Using a rating scale, rate the vividness of a retrieved memory			

Questionnaire (Bakhtiar et al., 2022; Østby et al., 2012) Adapted version of the Relationships Beliefs Inventory (Eidelson & Epstein, 1982)	RBI	Using a rating scale, rate the degree to which one supports maladaptive beliefs about romantic relationships, namely that disagreement is destructive, that mindreading is expected, that partners cannot change, that the sexes are different, and that sexual perfectionism is expected and that sexual perfectionism is expected	Self-report	None	Stern et al. (2019)
	Essentialism Scale (Bastian & Haslam, 2006)	Using a rating scale, indicate the extent to which one believes that human attributes are immutable in the sense that individuals fall into discrete categories, that differences between individuals permit many inferences regarding further differences, and that human attributes have a biological basis	Self-report	None	Castano et al. (2021)

Note. Three exemplary measures per outcome variable, meant to show the diversity of operationalizations across studies, are provided. The RMET is categorized as a measure of empathy, whereas in most other research, including Dodel-Feder and Tamir's (2018) meta-analysis, it is treated as a measure of mentalizing. The current classification is based on evidence suggesting that the RMET indicates emotion recognition rather than mentalizing (Oakley et al., 2016) and on the common classification of emotion recognition as a component of empathy (see the Introduction section). SAT = Scholastic Aptitude Test; N/A = not applicable; ASD = autism spectrum disorder.

The effect of each of the subsequent moderator levels is compared to the intercept. Hence, the p values of these effects reflect whether the effect of the moderator level differs significantly from that of the intercept and so suggest whether the respective variable exerts a moderating influence. For categorical moderators with more than three levels (e.g., outcome variable), we ran metaregression models without an intercept. Here, the effect of each moderator level is tested against zero, as reflected in the corresponding p values. This was followed up with pairwise contrasts (reported in the text, not in Table 2), for which the significance level was adjusted using the Bonferroni method. For continuous moderators, the effect size parameter is a regression weight b , so that p values indicate whether there is moderation in terms of a significant linear relationship between moderator and outcome.

According to the metaregressions, the overall meta-analytic effect was moderated significantly by, first, the type of comparison group. Specifically, effect sizes were greater when reading fiction was compared with either watching fiction, $g = 0.31$, or reading nothing, $g = 0.23$, than when reading fiction was compared with reading nonfiction, $g = 0.10$ ($ps < .006$). A second moderating effect emerged for the outcome variable: significant effects were observed only when the outcome was either empathy, $g = 0.16$, or mentalizing, $g = 0.15$, whereas effects did not differ significantly from zero for the remaining outcomes (i.e., knowledge, moral cognition, outgroup judgments, and other thinking processes; $ps > .11$). Pairwise comparisons failed to reveal significant differences between effects of individual outcome variables ($ps > .07$, with $p_{\text{critical}} = .003$). There were no further significant moderations ($ps \geq .05$).

Publication Bias

The following analyses of publication bias included published work only. Visual inspection of the funnel plot revealed some degree of asymmetry as there were more data points to the right than to the left of the mean effect (see Figure 2A). Yet, although the slope of Egger's regression test for funnel plot asymmetry was positive ($b = 2.25$, $SE = 1.76$), $t(48) = 1.28$, $p = .21$, it was not significant. That is, the precision of the measured effect was not significantly linked with the magnitude of the effect.

According to p -curve analysis (see Figure 3A), the right skewness of the p curve was significant, $Z = -10.99$, $p < .001$ (full p curve), and $Z = -9.59$, $p < .001$ (half p curve). Therefore, the current pool of studies can be thought to have an evidential value that reading fiction leads to cognitive benefits. Additionally, the p curve did not suggest evidential inadequacy (i.e., flatter than 33% power), $Z = 4.98$, $p > .999$ (full p curve), and $Z = 11.72$, $p > .999$ (half p curve). The estimated power of the tests included in the p curve was 71%.

Meta-Analysis 2

Characteristics of Studies and Samples

The current pool of studies involved 30,503 participants. Sample sizes of individual studies varied between $n = 33$ (Stansfield & Bunce, 2014) and $n = 4,775$ (Lenhart et al., 2023), IQR (104.00, 262.80). Mean ages ranged from 8.67 (Arnold et al., 2019) to 59.29 years (Wimmer & Ferguson, 2023; Wimmer, Currie, et al., 2021), IQR (19.64, 31.98). Regarding participant gender, the percentage of female volunteers varied between 39.68% (Allen et al., 1992) and 100% (Diekman et al., 2000; Study 1), IQR (55.62%,

Table 2
Meta-Analytic Results

Moderator variable	Meta-Analysis 1										Meta-Analysis 2									
	Number of studies	Number of ES	Effect size estimate					<i>p</i> informs whether ...	<i>Q</i>	Number of studies	Number of ES	Effect size estimate					<i>p</i> informs whether ...	<i>Q</i>		
			Parameter	95% CI	<i>SE</i>	<i>t</i>	<i>p</i>					Parameter	95% CI	<i>SE</i>	<i>t</i>	<i>p</i>				
Overall estimate	70	371	<i>g</i> = 0.14	[0.06, 0.21]	0.04	3.75	.0004	... <i>g</i> is different from 0	1,086.50***	114	559	<i>r</i> = .16	[0.13, 0.19]	0.02	10.33	<.0001	... <i>r</i> is different from 0	5,825.12***		
Categorical moderators																				
Type of assessment measure																				
Behavioral performance (Meta-Analysis 1)/neither print exposure nor outcome assessed via self-report (Meta-Analysis 2) (intercept)		206	<i>g</i> = 0.11	[0.03, 0.19]	0.04	2.75	.0076	... <i>g</i> is different from 0	1,041.26***		246	<i>r</i> = .19	[0.15, 0.24]	0.02	−8.33	<.0001	... <i>r</i> is different from 0	5,723.39***		
Either print exposure or outcome assessed via self-report (vs. intercept)		165	<i>g</i> = 0.18	[0.09, 0.28]	0.05	1.55	.1265	... effect of moderator is different from 0			275	<i>r</i> = .13	[0.08, 0.18]	0.03	−2.28	.0245	... effect of moderator is different from 0			
Self-report (Meta-Analysis 1)/both print exposure and outcome assessed via self-report (Meta-Analysis 2) (vs. intercept)											36	<i>r</i> = .17	[0.08, 0.27]	0.05	−0.35	.7289	... effect of moderator is different from 0			
Publication status																				
Published (intercept)	50	267	<i>g</i> = 0.18	[0.09, 0.27]	0.05	3.91	.0002	... <i>g</i> is different from 0	1,036.65***	85	428	<i>r</i> = .18	[0.14, 0.22]	0.02	9.44	<.0001	... <i>r</i> is different from 0	5,679.33***		
Unpublished (vs. published)	20	104	<i>g</i> = 0.04	[−0.11, 0.18]	0.07	−1.98	.0512	... effect of moderator is different from 0		29	131	<i>r</i> = .11	[0.05, 0.17]	0.03	−2.04	.0436	... effect of moderator is different from 0			
Study format																				
Online (intercept)	30	127	<i>g</i> = 0.07	[−0.00, 0.15]	0.04	1.89	.0630	... <i>g</i> is different from 0	1,027.97***	50	278	<i>r</i> = .15	[0.11, 0.18]	0.02	7.70	<.0001	... <i>r</i> is different from 0	5,206.74***		
In person (vs. online)	38	235	<i>g</i> = 0.18	[0.04, 0.32]	0.07	1.51	.1345	... effect of moderator is different from 0		47	223	<i>r</i> = .20	[0.13, 0.27]	0.04	1.47	.1442	... effect of moderator is different from 0			
Type of participant group																				
Students (intercept)	27	173	<i>g</i> = 0.12	[0.01, 0.23]	0.05	2.26	.0273	... <i>g</i> is different from 0	1,084.48***	51	244	<i>r</i> = .13	[0.09, 0.18]	0.02	6.52	<.0001	... <i>r</i> is different from 0	5,607.33***		
Community (vs. students)	43	198	<i>g</i> = 0.15	[0.00, 0.29]	0.07	0.34	.7336	... <i>g</i> is different from 0		54	284	<i>r</i> = .21	[0.15, 0.27]	0.03	2.33	.0215	... effect of moderator is different from 0			

[illegible]

(table continues)

Table 2 (continued)

Moderator variable	Meta-Analysis 1					Meta-Analysis 2				
	Number of studies	Number of ES	Effect size estimate			Number of studies	Number of ES	Effect size estimate		
			Parameter	95% CI	SE	t	p	Parameter	95% CI	SE
Continuous moderators										
Participant age	57	326	$b = -0.01$	$[-0.01, 0.00]$	0	-1.17	.2464	$b = -0.00$	$[-0.01, 0.00]$	0
Percentage of female participants	63	330	$b = -0.00$	$[-0.01, 0.00]$	0	-0.76	.4476	$b = -0.00$	$[-0.01, -0.00]$	0
Length of reading assignment	53	315	$b = -0.00$	$[-0.00, 0.00]$	0	-0.99	.3276			

Note. Number of ES but not of studies is reported for variables varying within and between study. ES = effect sizes; CI = confidence interval. *** $p < .001$.

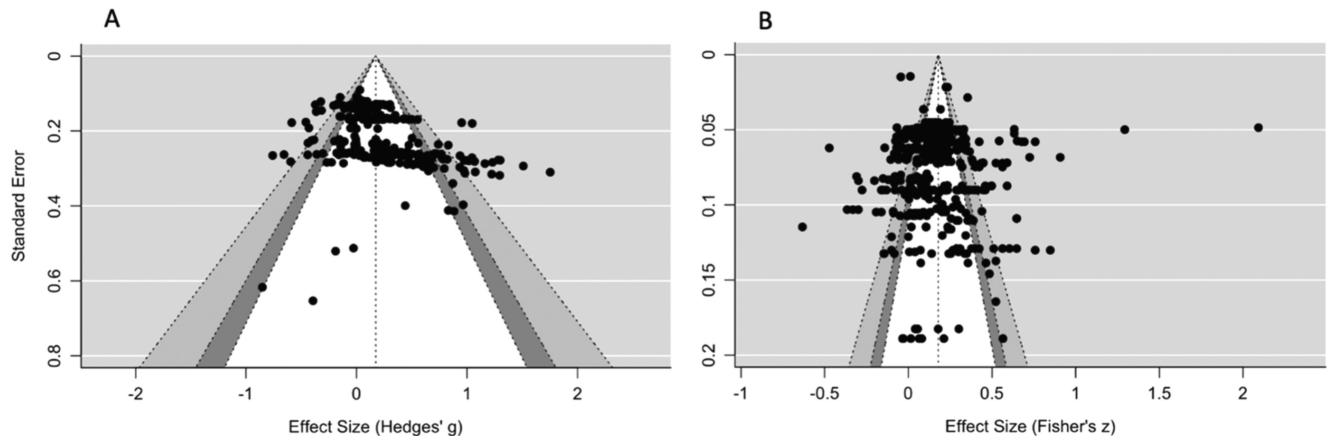
76.85%). About 74.56% of studies were published (vs. unpublished), 45.63% were conducted in person (vs. online), and 50.47% worked with a community (vs. student) sample. The study design of 86.92% of comparisons was correlational (vs. experimental). Turning to outcome variables, creativity was targeted by 2.16% of comparisons, empathy by 43.17%, general cognitive abilities by 12.05%, mentalizing by 6.29%, moral cognition by 13.31%, out-group judgments by 4.50%, verbal abilities by 8.99%, and other thinking by processes by 9.53% (for exemplary measures of each outcome variable see Table 1). Neither the outcome nor print exposure was assessed via self-report for 44.17%, either the outcome or print exposure was assessed via self-report for 49.37%, and both the outcome and print exposure were assessed via self-report for the remaining 6.46% of comparisons.

Meta-Analysis and Robustness Checks

The primary research question concerned whether and to what extent lifetime exposure to written fiction is associated with cognitive benefits. Our meta-analysis of 559 effect sizes from 114 studies revealed that there is a significant small-sized, positive association between lifetime exposure to written fiction and cognitive abilities ($r = .16$, 95% CI $[0.13, 0.19]$, $p < .0001$). The presence of heterogeneity was indicated by a significant Q statistic, $Q(558) = 5,825.12$, $p < .0001$. In addition, total I^2 was 91.33%, indicating a substantial amount of true variance (vs. sampling error) in effect size estimates. The majority of this was rooted in the within-study cluster, $I^2_{\text{Level2}} = 54.56\%$, compared to the between-study cluster, $I^2_{\text{Level3}} = 36.78\%$. In sum, these findings suggest that lifetime exposure to written fiction is associated with small-sized cognitive benefits and that the effect sizes differ systematically between studies due to factors that vary within studies (e.g., outcome variable). Robustness of the effect was confirmed using sensitivity analyses: standardized residual values ranged from -0.80 to 1.93 ; thus, all figures were below the critical cutoff of 3.0 (understood as absolute value; Cohen & Cohen, 2003). Using the leave-one-out procedure at the effect size level, effect size estimates varied between $r = .16$ and $r = .17$, while total I^2 was between 90.10% and 91.53%. For the leave-one-out procedure at the study level, effect size estimates ranged from $r = .16$ to $r = .17$, and total I^2 from 90.08% to 91.68%.

Moderator Analyses

A series of regression analyses, parallel to that of Meta-Analysis 1, tested whether the significant amount of heterogeneity can be accounted for by one or several moderator variables. Indeed, meta-regressions yielded several significant moderations. Specifically, the type of assessment measure significantly moderated the overall meta-analytic effect—effects were smaller when the assessment of either print exposure or the outcome was based on self-report, $r = .13$, than when the assessment of neither print exposure nor the outcome was based on self-report ($r = .19$, $p = .02$), whereas effects did not differ significantly between the two constellations in which either both or none of the variables were assessed based on self-report ($r = .17$, $p = .73$). Publication status emerged as a further moderator as the meta-analytic effect was significantly greater for published, $r = .18$, than for unpublished studies ($r = .11$, $p = .04$). The overall meta-analytic effect was also moderated by type of participant group; effects observed in community samples

Figure 2*Funnel Plots of the Effect Sizes From Published Studies*

Note. (A) Meta-Analysis 1. (B) Meta-Analysis 2. Effect sizes on the *x* axis are plotted against their standard error on the *y* axis. The dotted vertical line represents the mean meta-analytical effect. Within the funnel shape, the white area reflects the 90% CI of the mean effect, the dark gray area reflects the 95% CI of the mean effect, and the light gray area reflects the 99% CI of the mean effect. CI = confidence interval.

($r = .21$) were significantly greater than those observed in student samples ($r = .13$, $p = .02$).

Regarding the outcome variables, all effects except those associated with moral cognition ($r = .08$, $p = .08$) differed significantly from zero ($r_s > .08$). Pairwise contrasts ($p_{\text{critical}} = .00179$) showed that effects linked with verbal abilities were significantly greater than those associated with all other outcome variables ($p_s < .0001$). In addition, general cognitive abilities were related to significantly smaller effects than verbal abilities but to greater effects than empathy, mentalizing, and outgroup judgments ($p_s < .0004$). The remaining contrasts were not significant ($p_s > .013$).

Beyond this, fictionality acted as a significant moderator variable since lifetime exposure to written fiction, $r = .16$, was linked with greater effects than lifetime exposure to written nonfiction³ ($r = .09$, $p < .0001$). Also, the overall meta-analytic effect was found to be moderated by study design in that correlational designs, $r = .18$, were linked with significantly greater effects than experimental designs ($r = .10$, $p = .008$). Finally, the percentage of female participants exerted a significant moderating impact, meaning that the higher the percentage of female participants within a study sample, the smaller the overall meta-analytic effect ($b = -0.00$, $p < .001$). No other moderations reached significance ($p_s > .13$).

Publication Bias

Paralleling Meta-Analysis 1, publication bias analyses included published work only. Visual inspection of the funnel plot suggested some extent of asymmetry since there were more data points to the right than to the left of the mean effect (see Figure 2B). Again, the slope of Egger's regression test for funnel plot asymmetry was positive but not significant ($b = 1.33$, $SE = 2.31$), $t(83) = 0.58$, $p = .57$, reflecting that the precision of the observed effect was not significantly associated with the magnitude of the effect.

Turning to *p*-curve analysis (see Figure 3B), the *p* curve was significantly right-skewed ($Z = -41.16$, $p < .001$, full *p* curve) and ($Z = -39.57$, $p < .001$, half *p* curve). Hence, the included studies can be assumed to have evidential value in support of the proposal

that lifetime exposure to print fiction is correlated with cognitive benefits. Furthermore, the *p* curve did not reveal evidential inadequacy (i.e., flatter than 33% power), $Z = 27.48$, $p > .999$ (full *p* curve), and $Z = 36.53$, $p > .999$ (half *p* curve). The estimated power of the *p* curve analysis was 98%.

Discussion

In view of ongoing debates about fiction-related cognitive benefits and detriments, the present set of meta-analyses quantitatively synthesized research on cognitive outcomes associated with reading fiction.

Meta-Analysis 1

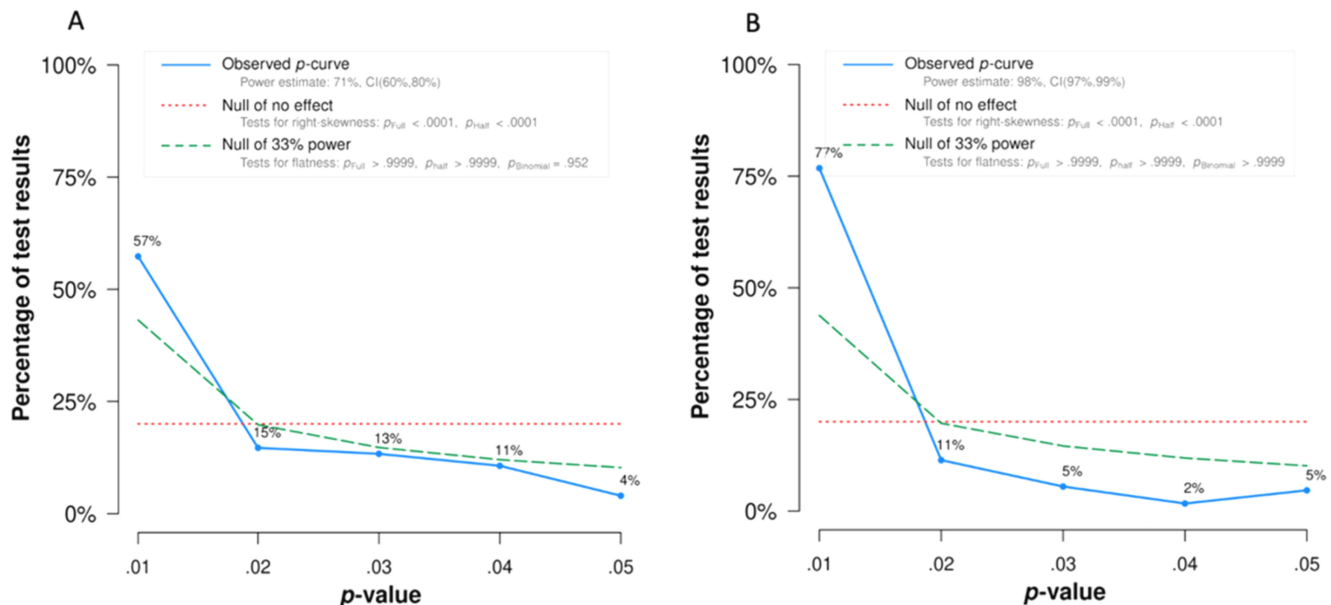
Meta-Analysis 1 looked at the effects of experimental studies involving fiction-reading assignments. First of all, we determined how many experimental studies have investigated the cognitive effects of reading fiction. According to our literature search, the relevant number is 70 experiments, which is considerably higher than the body of research (i.e., 14 experiments) summarized by a previous meta-analysis (Dodell-Feder & Tamir, 2018).

Secondly, we were interested in the overall effect of reading fiction on cognition. A three-level random-effects meta-analytic model involving 371 effect sizes revealed that compared to control activities, reading fiction led to significant small-sized cognitive benefits ($g = 0.14$, 95% CI [0.06, 0.21], $p = .0004$). Sensitivity analyses, including checks of standardized residual values and leave-one-out procedures, endorsed the robustness of this effect.

Thirdly, we attempted to identify variables that modulate the overall meta-analytic effect. Two of the variables under investigation were found to exert a moderating influence, namely the type of comparison group and the outcome variable.

³ For this particular metaregression model, a further 158 effect sizes were included that reflected nonfiction exposure. These effect sizes were not used in any other analyses.

Figure 3
P-Curve Plots Depicting the Distribution of Significant p Values



Note. (A) Meta-Analysis 1. (B) Meta-Analysis 2. CI = confidence interval. See the online article for the color version of this figure.

Greater effects emerged when reading fiction was compared with either watching fiction or reading nothing than when reading fiction was compared with reading nonfiction. This finding touches on the question of whether effects are driven by reading in general, fiction consumption in general, or the combination of reading with consuming fiction (i.e., reading specifically fiction). The current pattern of results suggests that fiction consumption in terms of watching fiction does not have a share in the effect, whereas the activity of reading does account for a portion of the effect. Beyond this, reading particularly fiction was found to make a distinct contribution. This pattern goes against the assumption that consuming narratives in general, independent of medium (e.g., watching vs. reading) and fictionality (i.e., applying to fictional as well as nonfictional narratives), leads to benefits for social cognition. Nevertheless, due to the low number of experiments involving watching fiction, the findings of this meta-regression should be interpreted with caution.

Effects of reading fiction turned out to vary by outcome variable. Significant positive effects emerged for empathy and mentalizing. In contrast, effects on all other outcomes (in particular, knowledge, moral cognition, outgroup judgments, and other thinking processes) did not differ from zero. Additionally, there were no significant differences between any of the outcomes. In sum, only empathy and mentalizing seem to benefit from experimental interventions of reading fiction, and even then, only to a small extent.

Although the overall meta-analytic effect was not significantly greater for published than for unpublished studies ($p = .05$), the latter effect did not differ significantly from zero ($g = 0.04$, 95% CI $[-0.11, 0.18]$, see Table 2). This result indicates that the current pool of studies may be limited by publication bias. However, the remaining publication bias analyses suggested otherwise: according to Egger's test, the funnel plot was not significantly asymmetric and a p -curve analysis supported the evidential value of the present body

of research. Given that p -curve analysis is thought to detect p hacking, it is not a generic tool for assessing publication bias. The file drawer problem (Rosenthal, 1979) exemplifies sources of publication bias not targeted by p -curve analysis. Taken together, our analyses do not suggest a publication bias in terms of p hacking but do not rule out other causes of publication bias, such as the file drawer problem.

The aggregate effect was stable across participant characteristics, including gender, age, and type of participant group (students vs. community). So, despite the fact that these characteristics are associated with different reading habits or incentives for research participation, respectively, they did not have a significant impact on the synthesized effect. Perhaps reading assignments like the ones used within the experiments have the power to overrule their influence. Likewise, study format (online vs. in person), although typically linked with different levels of participant supervision, did not act as a significant moderator variable. It is possible that attention checks, which are commonly included in online studies (to adjust for lack of in-person monitoring), served their purpose.

Interestingly, the length of reading assignments did not emerge as a significant moderator. This result does not support the assumption that reading fiction affects cognition via practicing related skills, since such a relation would have elicited a positive relationship. The lack of an effect of reading length could be explained by mechanisms independent of systematic practicing, for instance, priming (see Lenhart & Richter, 2022); reading fiction may merely "warm up" participants' cognitive skills without a lasting transfer effect for real life. It should, however, be noted that the word count of most reading assignments varied between 1,500 and 6,000 words, which is rather short and within a narrow range that does not provide much room for substantial associations with cognitive outcomes. Moreover, there seems to be a consensus that a single reading task

involving approximately 6,000 words falls short of the length required for longer lasting effects on cognition (Eekhof et al., 2022; Mar, 2018a, 2018b). Hence, the present body of research does not provide a rigorous test of the assumption that reading fiction leads to genuine changes in cognitive outcomes.

Furthermore, the current meta-analysis is limited in that it may underestimate the true sizes of effects. This is because we only considered postassessments even though some of the included experiments reported preassessments. This analytical approach was taken to replicate that of Dodell-Feder and Tamir (2018), who “used raw unadjusted means and standard deviations, and compared only post-reading between-groups scores” (p. 1715). We acknowledge that preassessments could have been used to calculate more accurate effect sizes for pre–post designs.

Meta-Analysis 2

Meta-Analysis 2 pooled effects of studies in which lifetime exposure to written fiction was correlated with cognitive skills. Similar to Meta-Analysis 1, our first research question addressed the number of respective studies. Our literature search yielded 114 studies, which exceeded the volume of research (i.e., 30 studies) synthesized by a previous meta-analysis (Mumper & Gerrig, 2017).

The second research question addressed the overall correlation of lifetime fiction reading with cognition. According to a three-level random-effects meta-analytic model involving 559 effect sizes, there is a significant small-sized positive relationship between lifetime fiction reading volume and cognitive abilities ($r = .16$, 95% CI [.03, 0.19], $p < .0001$). Adopting the same sensitivity analyses as in Meta-Analysis 1, this effect turned out to be robust, so not the result of individual outliers at either the effect size or the study level.

To answer the third research question, we sought to discern moderators of the aggregate meta-analytic effect. Indeed, several of the variables under investigation were found to play a moderating role.

The scenario in which either the outcome or print exposure was assessed via self-report was linked with a lower correlation coefficient than the scenario in which none of these variables was assessed via self-report, but the association did not differ when either both or none of the variables were assessed based on self-report. This pattern goes against the assumption that effects are driven by studies that rely on self-report measures. Among the outcome variables, all correlations but the one for moral cognition were significantly greater than zero. Furthermore, lifetime exposure to written fiction was most strongly related to verbal abilities, next strongly linked with general cognitive abilities, and third strongly linked with empathy, mentalizing, and outgroup judgments. Due to the correlational nature of the studies under investigation, this pattern of results alone does not suggest that improved cognitive outcomes (apart from moral cognition, which was not significantly linked with fiction reading volume) are caused by greater amounts of reading fiction. Instead, individuals with high cognitive skills could be more strongly attracted to written fictions than individuals with comparatively lower skills. Yet another explanation for the positive correlation between lifetime exposure to written fiction and cognitive skills would be the existence of third variables causing increases in both the frequency of reading fiction and cognitive outcomes.

A further metaregression is relevant to the question of whether effects on cognition are specific to reading fiction or apply to reading in general. Lifetime exposure to written fiction was linked with a

stronger correlation than lifetime exposure to written nonfiction, although both correlations were significantly positive. In line with the findings of Meta-Analysis 1, the activity of reading seems to contribute to the overall effect. Additionally, reading fiction in particular appears to be associated with distinct benefits. In terms of participant characteristics, the overall meta-analytic effect was larger in community samples, who often receive monetary compensation in exchange for participation, than in student samples, who are typically reimbursed through course credits. Yet, these sample types differed on other characteristics as well, namely age ($M = 29.13$, $SD = 12.63$ vs. $M = 20.75$, $SD = 2.16$), study format (64.1% online vs. 34.2% online), and percentage of female participants ($M = 58.75\%$, $SD = 10.43$ vs. $M = 73.18\%$, $SD = 11.24$). Of these, only the percentage of females proved to moderate the aggregate meta-analytic effect: the higher the proportion of females, the lower the association between lifetime fiction reading and cognition. Possibly, males benefit more from reading fiction than females. Given that males tend to read less fiction than females, there may be greater room for fiction-based improvements among them. In addition, since the percentage of female participants differed between the two sample types (community vs. students), it is possible that the difference between sample types was confounded by different incentives or gender balance.

Finally, publication status emerged as a significant moderator—the aggregate correlation was significantly greater for published than for unpublished studies, with significant overall effects at both levels of this moderator variable. Even though this finding suggests the presence of a publication bias, the results of the remaining publication bias analyses were contradictory: the funnel plot was not found to be significantly asymmetric; additionally, a p -curve analysis supported the evidential value of the current pool of studies. So, just as in Meta-Analysis 1, the tests do not indicate publication bias in the sense of p hacking, but do not exclude other reasons for publication bias such as the file drawer problem.

Limitations of the Two Pools of Studies

The studies included in both meta-analyses are limited with regard to the cognitive outcomes under investigation. On the one hand, our meta-analyses suggest that reading fiction is associated with small-sized benefits for cognition. On the other hand, they offer only weak support for the assumption that reading fiction is not linked with adverse effects on cognition as discussed in the Introduction section. This is because, in Meta-Analysis 1, only 19.68% of effect sizes concerned any undesirable types of cognition, for example, negative perceptions of robots (Mara & Appel, 2015), sexual prejudice (Orellana, 2019, Experiment 1), or pejorative attitudes toward autistic individuals (Stern, 2020; these effect sizes were reverse coded, so that negative effect sizes indicate cognitive detriments). Importantly, a number of experiments on acquiring misinformation (but, importantly, also accurate information) from fiction were excluded since they did not include a comparison condition not involving reading fiction (e.g., Appel & Richter, 2007; Butler et al., 2012; Eslick et al., 2011; Fazio et al., 2013; Gerrig & Prentice, 1991; Marsh & Fazio, 2006; Marsh et al., 2003; Prentice et al., 1997; Rapp et al., 2014; Wheeler et al., 1999). In Meta-Analysis 2, only 11.09% of effect sizes concerned undesirable types of cognition, for instance, unrealistic perceptions of romantic relationships (Stern et al., 2019), egocentric bias (Castano et al., 2020), or psychological essentialism (Castano et al., 2021). Like

all of the cognitive outcomes under investigation, however, these variables include a broad range of cognitions such as attitudes (e.g., sexual prejudice, pejorative attitudes toward autistic individuals) and cognitive biases (e.g., egocentric bias, psychological essentialism). In sum, further research (with appropriate control conditions wherever applicable) is needed to assess whether reading fiction is associated with unwanted cognitive outcomes.

A further limitation concerns underlying textual mechanisms. Regarding Meta-Analysis 1, the fiction reading stimuli used within experiments were typically narrative fictions, in which effects of narrativity and fictionality are conflated. Therefore, we do not know whether effects apply to nonnarrative fictions such as certain types of poetry, even if the latter are in reality less often read than narrative fictions (V. Jackson, 2008). In addition, researchers generally did not describe the literariness of the reading material in sufficient detail or did not mention the literariness of their reading stimuli at all. Consequently, like Dodell-Feder and Tamir (2018), we were not able to explicitly consider literariness as a moderator variable in our meta-analysis. Relatedly, study authors usually did not address the extent to which their reading stimuli represent the textual genre of interest. Thus, it is impossible to know whether the current results pertain to the reading stimuli used within experiments or expand to the entire fiction genre (see Liberman, 2013). Similarly, in Meta-Analysis 2, the measures of fiction exposure under investigation typically assess the frequency with which narrative fictions are read but do not touch on nonnarrative fictions. So again, we do not know whether the overall meta-analytic effect generalizes to nonnarrative fictions. Even though approximately 9% of studies collected exposure to especially literary fictions, the vast majority did not address literariness. In sum, we could not take into account literariness as a moderator.

There remain open questions regarding the underlying textual mechanisms of fiction reading effects. In other words, whether the effects summarized here are due to the content of fiction (e.g., because works of fiction contain predominantly accurate or inaccurate information that is acquired during reading) or due to engagement with fictions (e.g., because reading fictions draws on cognitive processes that are strengthened as a result; see also Mar, 2018a, who distinguishes between a process- and a content-based route to learning from narratives). The original research reports included in Meta-Analysis 1 do not typically provide an in-depth analysis of either the reading materials' content or the cognitive processes engaged in during reading. For the original work synthesized in Meta-Analysis 2, such an analysis seems almost impossible given that those studies are about associations of accumulated reading experiences with cognitive outcomes assessed offline. Hence, neither of the present meta-analyses can inform whether the presently observed cognitive benefits and disadvantages are related to the content of fictions or to the cognitive processes engaged in during reading.

Furthermore, the quality of the evidence was mixed in both Meta-Analyses: In Meta-Analysis 1, 44% of effect sizes harbored a high risk of bias in that they assessed the outcome via self-report; furthermore, sample sizes lacked sufficient power to identify an effect of the size observed—fiction reading groups had a median sample size of 51, control groups a median sample size of 48. According to a post hoc power analysis (using G*Power; Faul et al., 2007), this sample size had a power of 11% to detect an effect of the size observed (i.e., $d = 0.14$) at the standard 5% significance

level in a two-tailed t test for independent samples. In Meta-Analysis 2, both print exposure and the outcome were assessed via self-report for only 6.46% of effect sizes, yet 49.37% assessed either print exposure or the outcome via self-report, so that neither print exposure nor the outcome was assessed via self-report for just approximately half of the effect sizes. Beyond this, sample sizes suffered from a similar power issue as sample sizes of Meta-Analysis 1. Studies worked with a median sample size of 194. A post hoc power analysis (again using G*Power) revealed that this sample size had insufficient power (59%) to detect a small-sized correlation coefficient of $r = .16$, as observed here, at the standard 5% significance level (two-tailed). Also, the median sample size is below the target of $n = 250$ at which correlation coefficients have been found to stabilize (Schönbrodt & Perugini, 2013).

Similarities and Differences With Previous Syntheses

The aggregate effect of reading fiction in Meta-Analysis 1 ($g = 0.14$) was almost identical to that reported by Dodell-Feder and Tamir's (2018) previous synthesis ($g = 0.15$). Similarly, the total effect of lifetime fiction reading in Meta-Analysis 2 ($r = .16$) was comparable to observations in Mumper and Gerrig's (2017) previous synthesis. These authors did not calculate an overall effect for social cognition but separate scores for empathy, $r = .07$, and mentalizing, $r = .21$. The present synthesized effect falls in between these coefficients. Differences between the current and both previous meta-analyses pertain to moderators and publication bias. The fact that the earlier syntheses did not yield significant outputs in either regard whereas the present ones did—at least for some analyses—may be related to differences in power. More precisely, Meta-Analysis 1 involved 5 times the number of studies included in Dodell-Feder and Tamir's (2018) work, while Meta-Analysis 2 covered 3.8 times the number of investigations involved in Mumper and Gerrig's (2017) synthesis; hence, power was much improved for the present meta-analyses.

Summary and Conclusions

Having discussed the findings of each meta-analysis in turn, we close with a comparative summary. Both meta-analyses yielded small-sized aggregate effects, suggesting that short fiction reading assignments cause small cognitive benefits and that lifetime exposure to written fiction is related to small cognitive enhancements, respectively. Overall effects of both meta-analyses were robust, so they were neither the result of individual outliers nor driven by assessments based on self-report. Given that undesirable outcomes were rarely investigated, the two meta-analyses provide only minimal evidence for the assumption that reading fiction is not linked with cognitive detriments. Thus, there is uncertainty about any negative outcomes that may accrue to those who make a steady diet of reading fiction, and so the possible existence of negative effects remains a crucial question. Results of both meta-analyses also converged regarding the role of reading fiction for the aggregate effects: Both projects showed that reading in general makes a significant contribution, with fiction reading having a particular impact.

In terms of the outcomes associated with fiction exposure, a significant effect of reading fiction on moral cognition did not arise in Meta-Analysis 1, and was even more strikingly missing in Meta-Analysis 2, in which moral cognition was the only cognitive

outcome that was not significantly associated with reading fiction. This pattern conflicts with some prevailing philosophical theories of morality such as moral sentimentalism (Kauppinen, 2022), which posit that most of the content (i.e., what is reasoned about or decided upon) in moral cognition is essentially emotional in nature. This philosophical position has been supported by neuroscientific evidence as well (e.g., Young & Koenigs, 2007). Given that empathy is defined by an emotional resonance or connection between an observer and an object of observation (typically another person), it is noteworthy that reading fiction would not have some pronounced effect on moral reasoning.

Beyond this, publication bias is a concern for the two meta-analyses. This bias does not seem to be associated with *p* hacking but to reflect the file drawer problem (Rothstein, 2008). Further limitations shared by both meta-analyses include the risk of bias linked with self-report assessments, underpowered sample sizes, and insufficient consideration of narrativity and literariness as potential mechanisms.

The patterns of results for the two meta-analyses differed in two major ways. Firstly, the percentage of female participants was a significant moderator of correlational (Meta-Analysis 2), but not of experimental effects (Meta-Analysis 1). It is possible that males have a greater potential for fiction-related cognitive benefits since they have a lower preference for written fictions than females (Hu et al., 2023). Alternatively, the gender effect seen in Meta-Analysis 2 might reflect confounds, due to differences between student and community samples, such as varying incentives for research participation. In any case, the fact that participant characteristics did not moderate effects in Meta-Analysis 1 suggests that short fiction-reading assignments can overrule the influence of reader attributes. Secondly, the meta-analyses yielded diverging results regarding the cognitive outcomes associated with reading fiction. Experimental effects of reading fiction were significant for indicators of social cognition only (i.e., empathy and mentalizing), but correlations with lifetime fiction exposure were significant for all cognitive outcomes except for moral cognition. In fact, the effects of empathy and mentalizing were surpassed by those of verbal and general cognitive abilities and were not stronger than the effects of remaining outcomes.

If we assume a causal impact of reading fiction across both meta-analyses, this could indicate that short fiction-reading assignments merely prime social cognitive skills and that these priming effects consolidate over time without growing in size. The other cognitive outcomes that correlated with lifetime exposure to print fiction may not be immediately primed during reading, at least not to a measurable extent, but may still accumulate over time. Alternatively, the pattern could be interpreted as evidence against a causal impact of reading fiction: If effects for verbal and general cognitive abilities show up only in correlational studies, which cannot confirm causal relationships, but do not become evident in experiments, this may suggest that reading fiction does not cause sustainable cognitive benefits. In that case, the aggregate effect obtained in Meta-Analysis 1 may reflect a transient priming response, and the overall effect yielded in Meta-Analysis 2 may reflect differences in fiction-reading preferences between people high in verbal and/or general cognitive abilities. Finally, third variables, such as education level, could underlie the association between lifelong fiction reading and cognition. This explanation is not unlikely given the very small effect sizes in Meta-Analysis 2 (and in the earlier meta-analysis by Mumper & Gerrig, 2017) and the fact that we relied on raw correlations (not

partial correlations controlling for the impact of third variables). Lifetime exposure to written fiction might then not be the cause of the cognitive benefits observed.

To conclude, we have presented a comprehensive and preregistered set of meta-analyses that provide robust evidence for a small positive relationship between reading fiction and cognitive benefits. Whether this relation represents a sustainable change caused by reading fiction remains unknown. Further research needs to (a) examine the underlying textual mechanisms and potential for adverse outcomes of reading fiction, (b) implement stronger manipulations of reading volume, (c) assess variables based on behavioral performance wherever possible, (d) recruit well-powered samples, and (e) consider publication of results independent of statistical significance, an invitation also directed to publication outlets.

Constraints on Generality

For most studies from both meta-analyses, samples were younger adults between approximately 20 and 34 years of age, with the percentage of female participants varying between approximately 53% and 77%. Hence, results are representative of younger adult groups with a rather high percentage of females, whereas children/adolescents, older adults, and also males, are underrepresented. Future research with these understudied groups would be desirable.

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