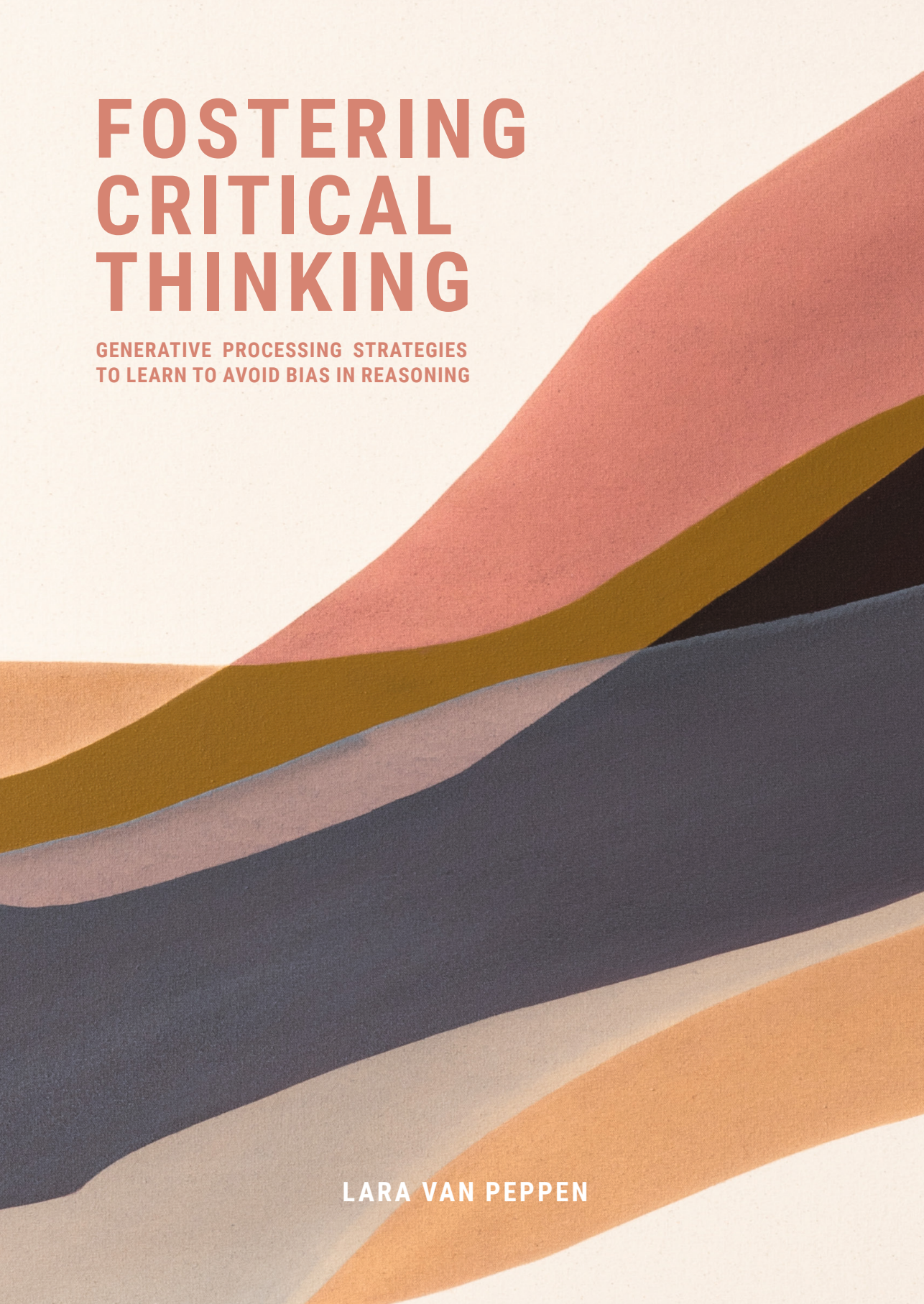
FOSTERING CRITICAL THINKING

GENERATIVE PROCESSING STRATEGIES TO LEARN TO AVOID BIAS IN REASONING



LARA VAN PEPPEN

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Colophon

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Fostering Critical Thinking: Generative processing strategies to learn to avoid bias in reasoning

Bevorderen van kritisch denken: generatieve verwerkingsstrategieën om systematische redeneerfouten te leren vermijden

Proefschrift

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Contents

Chapter 1

General introduction

Introduction

Every day, we make many decisions that are based on previous experiences and existing knowledge. This happens almost automatically as we rely on a number of heuristics (i.e., mental shortcuts) that ease reasoning processes (Tversky & Kahneman, 1974). Heuristic reasoning is typically useful, especially in routine situations. But it can also produce systematic errors (i.e., biases; this concept will be discussed in more detail later). Let us consider the following example:

> If someone conducts scientific research, s/he works at a university.
> Lara worked at the Erasmus University Rotterdam.
> Therefore, Lara conducted scientific research.

Because of its believability, most people will intuitively judge the conclusion as valid (cf. belief bias: Evans et al., 1983; Markovits & Nantel, 1989; Newstead et al., 1992), but the if-then rule does not state that someone working at a university conducts scientific research. We do know that Lara worked at a university, but we cannot deduce whether she conducted scientific research. Lara might have performed research procedures without scientific purposes, for instance, or she might have performed educational activities or support services. The conclusion would not necessarily follow from the premises and is, therefore, invalid. This syllogistic reasoning task requires replacement of the heuristic response with a response based on formal logic. Although in this example, the negative consequences are limited, heuristic reasoning can also produce biases with far-reaching consequences. To illustrate, a forensic expert who misjudges fingerprint evidence because it verifies his or her preexisting beliefs concerning the likelihood of the guilt of a defendant, displays the so-called confirmation bias, which can result in a misidentification and a wrongful conviction (e.g., the Madrid bomber case; Kassin et al., 2013). Fortunately, we are not doomed to reach wrong conclusions and to make incorrect decisions as in this example. Our primary tool for making better decisions is *critical thinking* (henceforth, in this dissertation, abbreviated as CT).

The importance of CT was already stressed by Socrates over 2,500 years ago and received renewed interest in the beginning of the 20th century. In 1910, John Dewey described the importance of critique and stated that *everyone* should engage in CT. Due to the expanding and changing demands that today's society places on people, the importance of being able to think critically has only increased (Pellegrino & Hilton, 2012). Because CT is essential for succeeding in future careers and to be efficacious citizens, helping students to become critically-thinking professionals is a central aim of higher education (Butler & Halpern, 2020; Davies, 2013; DeAngelo et al., 2009; Elen et al., 2019;

Facione, 1990; Halpern, 2014; Halpern & Butler, 2019; Van Gelder, 2005; Verburgh, 2013).

Consequently, many international (Ananiadou & Claro, 2009; OECD, 2018; Vincent-Lancrin et al., 2019) and national (i.e., Dutch: HBO-raad, 2009; OCW, 2019; Onderwijsraad, 2014a, 2014b, 2017, 2018; Vereniging Hogescholen, 2015) higher education policy documents include objectives to enhance students' CT-skills. To illustrate, around the start of this project, Avans Hogeschool, a Dutch University of Applied Sciences[1], had set explicit CT-aims in the documents detailing the educational ambitions (Avans Hogeschool, 2014a, 2014b) such as "every graduate is curious, shows a critical attitude, and is analytical. Therefore, we are committed to developing student's reflective and critical thinking capacity" (Avans Hogeschool, 2014b, p. 5)[2]. Several large-scale longitudinal studies, however, were quite pessimistic that this laudable goal would be realized merely by following a higher education degree program. These studies revealed that far too many higher education graduates lack the knowledge, beliefs, skills, and strategies required to think critically after four years of college (Arum & Roksa, 2011; Flores et al., 2012; Pascarella et al., 2011; although a more recent meta-analytic study reached the more positive conclusion that students' do improve their CT-skills over college years: Huber & Kuncel, 2016).

Hence, there is a growing body of literature on effective strategies for teaching CT in general (e.g., Abrami et al., 2008, 2014; Angeli & Valanides, 2009; Niu et al., 2013; Tiruneh et al., 2014, 2016) and avoiding reasoning biases in particular (Heijltjes et al., 2014a, 2014b, 2015; Van Brussel et al., 2020). It is well established, for instance, that bringing about learning of CT-skills is conditional upon provision of explicit CT-instructions and practice problems (e.g., Abrami et al., 2008, Heijltjes et al., 2014b). Yet there are still many open questions about optimal instructional designs to further enhance CT, and especially to establish transfer of CT-skills. Transfer refers to the ability to apply acquired knowledge and skills to novel situations (e.g., Barnett & Ceci, 2002; Perkins & Salomon, 1992). It is crucial that students think critically, especially in situations that have not been encountered before and where biases can have serious consequences (e.g., in complex professional environments in which the majority of higher education graduates are employed, such as medicine: Elia et al., 2016; Mamede et al., 2010; Law: Kassin et al., 2013; Koehler et al., 2002; Rachlinski, 2004). Therefore, the overall aim of this dissertation was to acquire more knowledge on effective strategies for fostering both learning and transfer of CT-skills in higher education, focusing specifically on avoiding bias in reasoning and drawing from findings from educational

[1] The Dutch education system distinguishes between research-oriented higher education (i.e., offered by research universities) and profession-oriented higher education (i.e., offered by universities of applied sciences).
[2] Surprisingly, CT is not (yet) explicitly mentioned in the latest ambition plan of Avans Hogeschool (2019).

and cognitive psychology. A brief overview of the history and theories of CT and biased reasoning and current research on teaching CT will serve as a preamble.

What is critical thinking?

CT finds its basis in the thoughts of Socrates, Plato, Aristotle, and other Greek philosophers. The term itself originated from this ancient Greek tradition as well; the word critical derives from the Greek words 'kritikos' (i.e., to judge/discern) and 'kriterion' (i.e., standards). Etymologically, then, CT implies making judgments based on standards. Hundreds of thinkers from different disciplines have subsequently made contributions to the idea of critical thought. John Dewey is considered the progenitor of the modern CT tradition. He described reflective thinking – his homologue to CT – to include "active, persistent, and careful consideration of any belief or supposed form of knowledge in the light of the grounds that support it, and the further conclusion to which it tends" (1910, p. 7). A variety of definitions has been suggested since then. Edward Glaser (1941), for example, expanded Dewey's definition to recognize the role of having certain thinking skills, but also of being disposed to use these skills. He was the first to describe CT as a composite of attitudes, knowledge, and skills. Robert Ennis (1962) took Dewey's definition and transformed it into a more general simplified definition that could provide a basis for research. According to him, CT implies "reasonable reflective thinking focused on deciding what to believe or do". The most accepted definition in the field of educational assessment and instruction, however, has been proposed by an expert Delphi Panel of the American Philosophical Association (APA). They agreed to characterize CT as:

> "purposeful, self-regulatory judgment that results in interpretation, analysis, evaluation, and inference, as well as explanations of the considerations on which that judgment is based… The ideal critical thinker is habitually inquisitive, well-informed, trustful of reason, open-minded, flexible, fair-mined in evaluation, honest in facing personal biases, prudent in making judgments, willing to reconsider, clear about issues, orderly in complex matters, diligent in seeking relevant information, reasonable in the selection of criteria, focused in inquiry, and persistent in seeking results which are as precise as the subject and the circumstances of inquiry permit." (Facione, 1990, p. 2).

Despite the variety of definitions of CT and the multitude of components, there appears to be agreement that one key aspect of CT is the ability to avoid bias in reasoning and decision-making (Baron, 2008; Duron et al., 2006; West et al., 2008), which we will refer to as unbiased reasoning from hereon. This is the aspect of CT on which the research

presented in this dissertation is focused. Bias is said to occur when a reasoning process results in a systematic deviation from ideal normative standards derived from laws of logic and probability (Stanovich et al., 2016; Tversky & Kahneman, 1974). Up to now, a substantial amount of literature has focused on the variety of heuristics and biases that exists. The so-called 'heuristics and biases' approach has generated influential research on CT and is central to this dissertation.

Heuristics and biases

The basic idea of the heuristics and biases approach, launched by Daniel Kahneman and Amos Tversky in the early 1970s, is that people rely on a variety of simple heuristics for judgment under uncertainty (Kahneman & Tversky, 1972, 1973; Tversky & Kahneman, 1974). As alluded to earlier, people resort to heuristics because these can help solve many different problems and make quick decisions, especially with rules and principles that have been practiced to automaticity (i.e., routine circumstances; Kahneman & Frederick, 2002; Kahneman & Klein, 2009; Shiffrin & Schneider, 1977; Stanovich, 2011). Heuristic reasoning allows us to not spend endless amounts of time and effort analyzing every information around us and is, therefore, very functional. For instance, when a medical emergency calls for action, an experienced clinician can use a recognizable pattern of cues to quickly make a diagnosis or size up a situation. However, the use of heuristics can also give rise to biases in reasoning and decision-making, as illustrated at the beginning of this chapter.

Kahneman and Tversky originally classified biases as associated with three such general-purpose heuristics (note that this is not the only classification of heuristics, however): representativeness, availability, and anchoring and adjustment (Tversky & Kahneman, 1974). The representativeness heuristic is characterized by the fact that people often evaluate the probability of an uncertain event by similarity with other events of the same type and causal/correlational beliefs (Chapman & Chapman, 1967; Jennings et al., 1982; Tversky & Kahneman, 1983). Specifically, representativeness concerns the degree of correspondence between an outcome and a model. To illustrate, Tversky and Kahneman (1983) asked undergraduates questions as "is it more probable that someone (selected by chance) has had a heart attack or that someone has had a heart attack and is over 55 years old?" Due to the natural assessment of a strong relation between heart failure and older age, thus high representativeness, the majority of graduates incorrectly perceived the conjunction of a heart attack and the age of 55 as more likely than a heart attack alone. Here, the use of the representativeness heuristic leads to neglect of conjunction rule ($P(A\&B) \leq P(B)$), known as the conjunction fallacy.

In case of the availability heuristic, people evaluate the probability of an event according to the ease with which examples come to mind (Tversky & Kahneman, 1973, 1983).

Events that are easy to retrieve from memory are regarded to be much more frequent and probable than they actually are. This can be the result of high exposure, as is the case, for instance, with terrorist attacks, airplane crashes, or natural disasters. But it can also be due to personal experiences/encounters. For example, if you are asked if it is more likely that the letter K appears in the first or third position of a word in English, you might estimate the first position as more probable. Just because it is much easier to recall words with the letter K in the first rather than the third position, however, the latter is actually more probable. In this case, the use of the heuristic results in availability bias (Tversky & Kahneman, 1973).

When people focus on an initial number or value (anchor) and then render a final estimate towards the anchor, they resort to the anchoring-and-adjustment heuristic (Lichtenstein & Slovic, 1971; Slovic, 1967; Tversky & Kahneman, 1974). For instance, when people were asked whether they would pay $25 (low anchor) or $200 (high anchor) to clean up lakes to protect fish populations and were then asked to estimate the amount the average person would contribute, they gave mean estimates of $14 and $36 with the low and high anchors, respectively. Here, the use of the heuristic leads to anchoring bias (Kahneman & Knetsch, 1993, Tversky & Kahneman, 1974).

Origins of biases

The occurrence of biases in thinking and reasoning can be explained by dual process models, which hold that there are two distinct cognitive systems that underlie thinking, reasoning, and decision-making: Type 1 and Type 2 processing, also referred to by some as System 1 and System 2 (Evans, 2003, 2008; Kahneman & Frederick, 2002; Stanovich, 1999, 2005, 2011). *Type 1 processing* is heuristic-based and operates automatically, autonomously, and rapidly, by means of parallel processing. As such, Type 1 processing is relatively effortless and does not place heavy demands on working memory. It has been shown that Type 1 processing is especially useful and functional during routine circumstances. Even a complex, but standard task can be completed with Type 1 processing (e.g., reading; or, for most Dutch people, cycling). However, in other (non-routine) situations, Type 1 processing might produce biased outcomes (Evans, 2003, 2008). Consider for example a clinician who has read information on a disease in the morning and later that day misdiagnoses a case of a patient who is presented with similar features (which triggered that diagnosis read earlier) but had in fact a different disease. That clinician makes use of the rapid and automatic Type 1 processes (i.e., availability heuristic, leading to availability bias; Schmidt et al., 2014). Thus, although the use of the availability heuristic may lead to efficient (i.e., fast and sound) decision-making in routine situations, it may also open the door to biases that could have been prevented by analytical and reflective reasoning, which is labelled as *Type 2 processing*.

Type 2 processing involves controlled processes that are relatively slow and largely sequential. One of the most crucial functions of Type 2 processing is to override Type 1 processing when this is to our benefit. To override Type 1 processing, one has to recognize the need for Type 2 processing and has to try to switch to this type of processing. This is only possible, however, when Type 1 processing can successfully be inhibited. Furthermore, this will only lead to a more favorable outcome when relevant mindware – consisting of both relevant procedural and conceptual knowledge – is available to provide better alternative responses (Aczel et al., 2015; Aron, 2008; Best et al., 2009; Stanovich, 2011; Zelazo, 2004). Biases occur when people use Type 1 processing when that is not appropriate, do not recognize the need for Type 2 processing, are not willing to switch to Type 2 processing or unable to sustain it (e.g., due to lack of sufficient cognitive capacity or time pressure), or miss the relevant mindware to come up with a better response. Consequently, in order to prevent biased reasoning, it is, first of all, necessary to stimulate people to switch to Type 2 processing. However, that may not be enough if they lack the necessary mindware, so in many cases, mindware has to be taught as well. In the next section, I will review what research has revealed with respect to effective ways of teaching CT in general, and then zoom in on effective methods for teaching students to avoid bias in reasoning.

Current research on teaching critical thinking

Previous research has established that CT-skills in general rarely evolve as a by-product of education; rather, they need to be explicitly taught (Abrami et al., 2008, 2014; Arum & Roksa, 2011; Beyer, 2008). However, there are different views of what the best way is to teach CT; the most well-known debate being whether CT should be taught in a general or content-specific manner (Abrami et al., 2014; Davies, 2013; Ennis, 1989; Moore, 2004). On the one hand, generalists (e.g., Ennis, 1989, 1992) ague that CT is a universal, general skill that can be applied to many contents and, as such, might be best learned separately from regular subject matter adjunct to the standard curriculum (Royalty, 1995; Stanovich & West, 1999). According to specifists (e.g., McPeck, 1990, 1992) on the other hand, CT cannot be separated from the subject matter to which it is applied and, therefore, should be taught in specific academic disciplines (Tsui, 2002). During the last years, this debate has faded away, since most researchers nowadays commonly agree that CT can be seen in terms of both general skills (e.g., sound argumentation, evaluating statistical information, and so on) and specific skills or knowledge used in the context of disciplines (e.g., Davies, 2013; Ikuenobe, 2001; Robinson, 2011; Smith, 2002; Tsui, 2002).

Indeed, it has been shown that the most effective teaching methods combine generic instruction, in which general CT-skills and dispositions are taught separately from subject matter, with the opportunity to integrate the general principles that were taught with domain-specific subject matter through infusion or immersion (i.e., mixed courses; for meta-analyses, see Abrami et al., 2008, 2014). In infusion methods, general CT principles are made explicit and students are encouraged to deal with specific subject matter in a critical way, while immersion methods invite students to reflect and make judgments on specific disciplinary issues without general CT principles made explicit (Ennis, 1989). Merely providing students with generic, infusion, or immersion courses, respectively, seemed less effective for fostering CT than mixed courses (Abrami et al., 2008, 2014). In the same vein, Tiruneh and colleagues (2014), found that both generic and mixed courses resulted in better CT outcomes than infusion and immersion courses.

Teaching for unbiased reasoning

A considerable number of studies on avoiding bias in reasoning has focused on strategies to mitigate specific biases (referred to as debiasing strategies; e.g., Aczel et al., 2015a; Catapano et al, 2019; Herzog & Hertwig, 2009; Kaufmann et al., 2010; Larrick, 2004; Lord et al., 1984). These studies, however, are not concerned with the implementation of these strategies in education. Some studies that did address teaching unbiased reasoning reflect the finding of studies concerned with teaching general CT-skills (Abrami et al., 2008, 2014): combining explicit CT-instruction with the opportunity to apply the principles that were taught on domain-relevant problems seems beneficial for learning of unbiased reasoning (Heijltjes et al., 2014a, 2014b, 2015). In these studies, students participated in a pretest-intervention-posttest design. The intervention consisted of either explicit, implicit, or no CT-instructions that were offered either with or without opportunity to practice in a domain context. Unbiased reasoning was operationalized as performance on classical heuristics-and-biases tasks (Tversky & Kahneman, 1974), in which an intuitively cued heuristic response conflicts normative models of CT as set by formal logic and probability theory.

Although these studies uncovered that a combination of explicit CT-instructions and task practice promotes learning of unbiased reasoning, they also consistently observed that this was not sufficient to establish transfer to novel problem types (and this also applies to CT-skills more generally, see for example, Halpern, 2014; Kenyon & Beaulac, 2014; Lai, 2011; Ritchhart & Perkins, 2005; Tiruneh et al., 2016). The process of transfer involves the application of acquired knowledge or skills to some new context or related materials (e.g., Barnett & Ceci, 2002; Cormier & Hagman, 2014; Druckman & Bjork, 1994; Haskell, 2001; McDaniel, 2007; Perkins & Salomon, 1992). In the educational psychology literature, transfer has been described as existing on a continuum from near to far, with lower degrees of similarity between the initial and transfer situation along the way (e.g.,

Perkins & Salomon, 1992). This lack of transfer is worrisome because it would be unfeasible to train students on each and every type of reasoning bias they will ever encounter (and this also applies to CT-skills more generally, see for example, Halpern, 2014; Kenyon & Beaulac, 2014; Lai, 2011; Ritchhart & Perkins, 2005). Surprisingly, though, it has not yet been investigated what kind of practice activities can promote (far) transfer.

The existing transfer literature suggests that, to establish transfer, instructional strategies should contribute to actively constructing meaning from to-be-learned information, by mentally organizing it in coherent knowledge structures and integrate these principles with one's prior knowledge (i.e., *generative processing*; Grabowski, 1996; Osborne & Wittrock, 1983; Wittrock, 1974, 1990, 1992, 2010). Generative processing can help learners acquire abstractions of the underlying principles behind a problem that are required for transfer of learned skills. If the potential transfer situation presents similar requirements and the learner recognizes them, they may select and apply the same or a somewhat adapted learned procedure to solve the novel problem (e.g., Gentner, 1983, 1989; Mayer & Wittrock, 1996; Reed, 1987; Vosniadou & Ortony, 1989). Indeed, strategies that encourage generative processing have been shown to foster knowledge acquisition and promote transfer of various other cognitive skills (e.g., Fiorella & Mayer, 2015, 2016). Ways to stimulate generative processing are, for instance, encouraging elaboration, questioning, or explanation during practice (e.g., Fiorella & Mayer, 2016; Renkl & Eitel, 2019), creating variability in practice (e.g., Barreiros et al., 2007; Moxley, 1979), stimulating comparison of correct problem solutions with erroneous ones (e.g., Durkin & Rittle-johnson, 2012; Loibl & Leuders, 2018, 2019), or having students repeatedly retrieve to-be-learned material from memory (Butler, 2010; Carpenter & Kelly, 2012; McDaniel et al., 2012, 2013; Rohrer et al., 2010).

Taken together, despite the value placed on teaching CT, it remains a disputed point how to do this more effectively. It has been established that bringing about learning of CT-skills is conditional upon provision of explicit CT-instructions and practice problems, but there are still many open questions about optimal practice activities to further enhance CT, in ways that transfer across tasks/contexts. To properly inform educational practice about optimally tailoring CT courses, further study is therefore required. The studies presented in this dissertation overall aim to gain more knowledge on fostering higher education students' learning and transfer of CT-skills – through instructional interventions that target generative processing – focusing specifically on avoiding bias in reasoning. This leads to the main research questions, which will be discussed in the next section.

Context and overview of this dissertation

This dissertation is one of the results of the broader NWO-funded research project "Investing in Thinking Pays Good Interest: Improving Critical Thinking Skills of Students and Teachers in Higher Professional Education". In this project, a consortium of researchers from Erasmus University Rotterdam and Utrecht University and educational policy advisors, teachers, and researchers from Avans University of Applied Sciences, aimed to generate knowledge on teaching CT that would be scientifically relevant as well as directly relevant for educational practice. The main objective of this project was to improve higher education students' CT-skills, by investigating how to equip teachers with the knowledge and skills needed to effectively teach unbiased reasoning (conducted by Eva Janssen, Utrecht University) and how to further enhance students' skills to avoid bias in reasoning, in such a way that these would also transfer across tasks/contexts.

The studies in **Chapters 2**, **3**, **4**, and **5** are concerned with the main question of whether instructional interventions that are known to foster generative processing and transfer of other cognitive skills, would further facilitate learning and transfer of CT-skills required for unbiased reasoning (i.e., above and beyond effects of instruction and practice). These interventions were administered after initial instruction, during the practice phase. In addition, the study in Chapter 6, experimentally examined what obstacle(s) prevent(s) successful transfer of these CT-skills. An important aspect of this dissertation is that all studies contained or consisted of an experiment conducted in a real educational setting and as part of an existing course (using educationally relevant materials) at a University of Applied Sciences, which increases ecological validity of the studies.

The classroom study presented in **Chapter 2** addressed the question of whether prompting students to self-explain during practice; that is, to generate explanations of a problem-solution to themselves (e.g., Bisra et al., 2018; Chi, 2000; Fiorella & Mayer, 2016) would be effective for fostering (transfer of) unbiased reasoning. Students were provided with instruction on the importance and features of CT, on the skills and attitudes needed to think critically, and on several heuristics-and-biases tasks. Subsequently, they performed practice activities on domain-relevant problems in the task categories they were given instructions on, either with or without self-explanation prompts. Students' performance on heuristics-and-biases tasks (both on instructed/practiced tasks, to assess learning, and on novel tasks that shared underling principles, to assess transfer), perceived mental effort investment, and time-on-test were measured on a pretest, immediate posttest, and two-week delayed posttest. Additionally, it was explored whether the quality of students' self-explanations was related to their performance.

In **Chapter 3**, two experiments (laboratory and classroom) tested whether creating variability during practice through interleaved practice (in which practice task categories vary from trial to trial, as opposed to blocked practice; e.g., Barreiros et al., 2007; Helsdingen et al., 2011; Rau et al., 2013) would be effective for fostering unbiased reasoning. While interleaved practice has been shown to enhance learning (e.g., Helsdingen et al., 2011a, 2011b) it is usually more cognitively demanding than blocked practice, and very high cognitive load may hinder learning (Paas et al., 2003a). Therefore, it was additionally examined whether learners would experience lower cognitive load and benefit more from interleaved practice, when using worked examples as opposed to practice problems (cf. Paas & Van Merriënboer, 1994). Worked examples have been shown to reduce ineffective cognitive load (compared to practice problems; Van Gog et al., 2019). After receiving explicit instruction on CT and specific heuristics-and-biases tasks, students either practiced in an interleaved schedule with worked examples, an interleaved schedule with problems, a blocked schedule with worked examples, or a blocked schedule with problems. Again, students' performance on several heuristics-and-biases tasks (both on instructed/practiced tasks and novel tasks), perceived mental effort investment, and time-on-test were measured on a pretest, immediate posttest, and two-week delayed posttest. Additionally, students' global judgements of learning and experienced cognitive load during practice were explored.

The classroom study reported in **Chapter 4** investigated whether comparing correct and erroneous examples (i.e., contrasting examples) would enhance unbiased reasoning more than studying correct examples only, studying erroneous examples only, and solving practice problems. Students were provided with the CT-instructions and practice on domain-relevant problems, under one of the four conditions. Their performance on heuristics-and-biases tasks (both on instructed/practiced tasks and novel tasks), mental effort investment, and time-on-test were measured on a pretest, immediate posttest, three-week delayed posttest, and nine-month delayed posttest. Furthermore, effects on perceived mental effort and time-on-task during practice were explored.

In **Chapter 5**, a classroom study is described that empirically investigated whether repeated retrieval practice over time (i.e., working on practice tasks in sessions that were weeks apart), would be beneficial for learning to reason in an unbiased manner and whether it can additionally facilitate transfer. Students were instructed on CT and avoiding belief-bias in syllogistic reasoning and practiced with syllogisms on domain-relevant problems, followed by feedback on their performance. Depending on assigned condition, they did not engage in extra practice, practiced a second time (week later), or practiced a second (week later) and third time (two weeks after second time).

Students' performance on heuristics-and-biases tasks (both on instructed/practiced syllogisms and novel tasks that shared similar features with syllogisms), mental effort investment, and time-on-test were measured on a pretest and immediate posttest. Additionally, explorative data on students' global judgements of learning, perceived mental effort during practice, time-on-task during practice, and time spent on worked-example feedback after correct and incorrect retrievals were collected.

Understanding the nuances of transfer is necessary to design courses to achieve it. So, it is crucial to gain insight into the obstacles to transfer of CT. Therefore, the study in **Chapter 6** focused exclusively on identifying whether unsuccessful transfer of CT-skills would be due to a failure to recognize that acquired knowledge is relevant in a new context, to recall that knowledge, or to apply that knowledge to the new context (i.e., the three-step model of transfer; Barnett & Ceci, 2012). In two experiments (classroom and laboratory), students received explicit instructions on CT and avoiding belief-bias in syllogistic reasoning and practiced with syllogisms on domain-relevant problems. Students' performance on heuristics-and-biases tasks (on syllogisms with different story contexts to assess learning, syllogisms in a different format to assess near transfer, and novel tasks that shared similar features with syllogisms to assess transfer) and time-on-test were measured on a pretest and immediate posttest. On the posttest transfer items, students received no support, received recognition support, were prompted to recall the acquired knowledge, or received recall support (cf. Butler et al., 2013, 2017). The effects of support for different steps in the process were compared to infer where difficulties arise for learners. Additionally, it was explored (within the free recall condition) whether students' ability to recall the acquired knowledge was related to their posttest performance on near and transfer items.

Finally, **Chapter 7** provides a summary and discussion of the main findings of Chapters 2 to 6. In addition, this chapter discusses the implications for future research on CT and for educational practice.

Chapter 2

Effects of self-explaining on learning and transfer of critical thinking skills

This chapter has been published as:
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Abstract

Critical thinking is considered to be an important competence for students and graduates of higher education. Yet, it is largely unclear which teaching methods are most effective in supporting the acquisition of critical thinking skills, especially regarding one important aspect of critical thinking: avoiding biased reasoning. The present study examined whether creating desirable difficulties in instruction by prompting students to generate explanations of a problem-solution to themselves (i.e. self-explaining) is effective for fostering learning and transfer of unbiased reasoning. Seventy-nine first-year students of a Dutch Applied University of Sciences were first instructed on two categories of 'heuristics-and-biases' tasks (syllogism and base-rate or Wason and conjunction). Thereafter, they practiced these either with (self-explaining condition) or without (no self-explaining condition) self-explanation prompts that asked them to motivate their answers. Performance was measured on a pretest, immediate posttest, and delayed (two weeks later) posttest on all four task categories, to examine effects on learning (performance on practiced tasks) and transfer (performance on non-practiced tasks). Participants' learning and transfer performance improved to a comparable degree from pretest to immediate posttest in both conditions, and this higher level of performance was retained on the delayed posttest. Surprisingly, self-explanation prompts had a negative effect on posttest performance on practiced tasks when those were Wason and conjunction tasks, and self-explaining had no effect on transfer performance. These findings suggest that the benefits of explicit instruction and practice on learning and transfer of unbiased reasoning cannot be enhanced by increasing the difficulty of the practice tasks through self-explaining.

Introduction

Fostering students' critical thinking (CT) skills is an important educational objective, as these skills are essential for effective communication, reasoning and problem-solving abilities, and participation in a democratic society (Billings & Roberts, 2014). Therefore, it is alarming that many higher education students find it hard to think critically; their level of CT is often too low (Flores et al., 2012) and CT-skills do not seem to improve over their college years (e.g., Arum & Roksa, 2011). As early as 1910, John Dewey described the importance of critique and stated that *everyone* needs to engage in CT. A variety of CT definitions has been suggested since then, the most accepted definition in the field of educational assessment and instruction of which has been proposed by an expert Delphi Panel of the American Philosophical Association (APA; Facione, 1990). They characterized CT as "purposeful, self-regulatory judgment that results in interpretation, analysis, evaluation, and inference, as well as explanation of the evidential, conceptual, methodological, criteriological, or contextual considerations on which that judgment is based" (Facione, 1990, p.2). Despite the variety of definitions of CT and the multitude of components, there appears to be agreement that one key aspect of CT is the ability to avoid biases in reasoning and decision-making (West et al., 2008), which we will refer to as unbiased reasoning from hereon. Bias is said to occur when a reasoning process results in a systematic deviation from a norm when choosing actions or estimating probabilities (Stanovich et al., 2016; Tversky & Kahneman, 1974). As biased reasoning can have serious consequences in situations in both daily life and the complex professional environments (e.g., economics, law, and medicine) in which the majority of higher education graduates end up working, it is essential to teach unbiased reasoning in higher education (e.g., Koehler et al., 2002; Rachlinski, 2004). However, it is still largely unclear how unbiased reasoning can be best taught, and especially how *transfer* can be fostered; that is, the ability to apply acquired knowledge and skills to new situations (e.g., Davies, 2013).

In line with findings of research on teaching CT in general (e.g., Abrami et al., 2014), previous research on unbiased reasoning has shown that providing students with explicit instructions and giving them the opportunity to practice what has been learned, improves performance on the learned tasks, but not transfer (e.g., Heijltjes et al., 2014b). This lack of transfer is a problem, as it is important that students can apply what has been learned to other situations. According to the *desirable difficulties* framework (e.g., Bjork, 1994; Bjork & Bjork, 2011; Fyfe & Rittle-Johnson, 2017; Soderstrom & Bjork, 2015), long-term performance and transfer can be enhanced by techniques that are effortful during learning and may seem to temporarily hold back performance gains. Conditions that support rapid improvement of performance (i.e. retrieval strength) often only support

momentary performance gains and do not contribute to permanent changes needed for learning (Bjork & Bjork, 2011). To enhance long-term retention and transfer of learned skills, storage strength should be increased by effortful learning conditions that trigger deep processing (Yan et al., 2016). The active and deeper processing produced by encountering desirable difficulties can promote transfer to new situations (cf. germane load; Soderstrom & Bjork, 2015). If, however, the difficulties evoke learners to invest additional effort on processes that are not directly relevant for learning or the learners miss the relevant knowledge or skills to successfully deal with them, they become undesirable (McDaniel & Butler, 2010; Metcalfe, 2011).

Although conditions inducing the most immediate and observable signs of performance improvements are often preferred by both teachers and learners because they appear to be effective, it is important for teachers and students alike to search for conditions that confront students with desirable difficulties and thereby facilitate learning and transfer (Bjork et al., 2015). Such conditions include, for example, spacing learning sessions apart rather than massing them together (i.e., spacing effect), mixing practice-task categories rather than practicing one task category before the next (i.e., interleaving effect), and testing learning material rather than simply restudying it (i.e., testing effect; e.g., Weissgerber et al., 2018). Another desirable difficulty is the active generation of an answer, solution, or procedure rather than the mere passive reception of it (i.e., generation effect; for a review see Bertsch et al., 2007). Generative processing of learning materials requires learners to invest additional effort on the learning processes and to be actively involved in these processes, such as encoding and retrieval processes (Yan et al., 2016). Therefore, generative learning activities contribute to the connection and entrenchment of new information from the to-be-learned materials to existing knowledge. As a result, understanding of the materials is stimulated and is more likely to be recallable at a later time or in a different context (Bjork & Bjork, 2011; DeWinstanley & Bjork, 2004; Fiorella & Mayer, 2016; Slamecka & Graf, 1978).

One promising strategy to promote generative learning, and thus to create desirable difficulty in instruction, is *self-explaining* (e.g., Fiorella & Mayer, 2016). Self-explaining involves the generation of explanations of a problem-solution to oneself rather than simply answering tasks passively. Indeed, self-explaining has been shown to foster knowledge acquisition and to promote transfer in a variety of other domains (e.g., Dunlosky et al., 2013; Fiorella & Mayer, 2016; Lombrozo, 2006; Rittle-Johnson & Loehr, 2017; Wylie & Chi, 2014; for a review see Bisra et al., 2018), but the effectivity in CT-instruction is not yet clear. Self-explaining is assumed to lead to the construction of meaningful knowledge structures (i.e., mindware), by investing effort in identifying knowledge gaps or faulty mental models and connecting new information to prior knowledge (e.g., Atkinson et al., 2003; Chi, 2000; Fiorella & Mayer, 2016), and seems

especially effective in domains guided by general underlying principles (Rittle-Johnson & Loehr, 2017). Moreover, self-explaining might stimulate students to stop and think about new problem-solving strategies (Siegler, 2002) with engagement in more analytical and reflective reasoning, labeled as Type 2 processing, as a result. This type of processing is required to avoid biases in reasoning and decision-making. Biases often result from relying on Type 1 processing to solve problems, which is a relatively effortless, automatic, and intuitive type of processing. Although Type 1 processing may lead to efficient decision-making in many routine situations, it may open the door to errors that could have been prevented by engaging in Type 2 processing (e.g., Evans, 2008; Stanovich, 2011). As such, self-explaining might contribute to decoupling prior beliefs from available evidence, which is an essential aspect of unbiased reasoning. It is important to bear in mind, however, that the benefit of self-explaining only applies when students are able to provide self-explanations of sufficient quality (Schworm & Renkl, 2007).

Several studies demonstrated that prompting self-explaining fostered learning and/or transfer of certain aspects of CT-skills, such as argumentation (e.g., Schworm & Renkl, 2007), complex judgments (e.g., Helsdingen et al., 2011b), or logical reasoning (e.g., Berry, 1983). Studies on the effect of self-explanation prompts on unbiased reasoning (Heijltjes et al., 2014a, 2014b, 2015), however, showed mixed findings. One study found an effect on transfer performance on an immediate posttest (Heijltjes et al., 2014a), but this effect was short-lived (i.e., not retained on a delayed posttest) and not replicated in other studies (Heijltjes et al., 2014b; Heijltjes et al., 2015). This lack of (prolonged) effects of self-explaining might have been due to the nature of the final tests, which were multiple-choice (MC) answers only. A study in which students had to motivate their MC-answers suggests that this might provide a better, more sensitive measure of the effects of self-explaining on transfer of unbiased reasoning (Hoogerheide et al., 2014). Therefore, the present study used MC-plus-motivation tests to investigate whether self-explaining is effective for fostering learning and transfer of unbiased reasoning.

Since it seems reasonable to assume, but is as yet unproven, that increasing the desirable difficulty of learning materials through self-explaining might foster learning and transfer of unbiased reasoning, the present study was conducted as part of an existing critical thinking course (i.e., classroom study) to examine the usefulness of this desirable difficulty in a real educational setting. We investigated the effects of self-explaining during practice with 'heuristics-and-biases tasks' (e.g., Tversky & Kahneman, 1974) on learning and transfer, as assessed by final test tasks which required students to motivate their MC-answers. Based on the literature reviewed above, we hypothesized that explicit CT-instructions combined with practice on domain-specific cases would be effective for learning: therefore, we expected performance gains on practiced tasks from pretest to

posttest as measured by MC-answers (Hypothesis 1). The more interesting question, however, is whether self-explaining during practice would lead to higher performance gains on practiced (i.e., *learning*; Hypothesis 2a) and non-practiced tasks (i.e., *transfer*; Hypothesis 2b) than not being prompted to self-explain during practice. As outlined before, we expected that beneficial effects of self-explaining on performance outcomes are more likely to be detected when participants are required to motivate their answer to MC-items. We hypothesized that self-explaining during practice would lead to higher total posttest scores (i.e., MC-plus-motivation) on practiced (i.e., *learning*; Hypothesis 3a) and non-practiced tasks (i.e., *transfer*; Hypothesis 3b). We expected this pattern of results to persist on the delayed posttest.

Furthermore, we explored perceived mental effort investment in the test items to get more insight into the effects of self-explaining on learning (Question 4a) and transfer performance (Question 4b). On the one hand, it can be expected that the acquisition of knowledge of rules and strategies would lower the cognitive load imposed by the task, and therefore participants might have to invest less mental effort on the posttests than on the pretest (Paas et al., 2003a), especially after having engaged in self-explaining. On the other hand, as both our training-phase and the self-explanation prompts were designed to provoke Type 2 processing – which is more effortful than Type 1 processing (Evans, 2011) – participants might have been inclined to invest *more* effort on the posttests than on the pretest, especially on the non-practiced (i.e., transfer) tasks, on which participants had not acquired any knowledge during instruction. Finally, because the quality of self-explanations has been shown to be related to learning and transfer, we explored whether the quality of the self-explanations on the practice tasks correlated with the immediate and delayed posttest performance (Question 5).

Materials and methods

We created an Open Science Framework (OSF) page for this project, where all materials, a detailed description of the procedure, and the dataset of the experiment are provided (osf.io/85ce9).

Participants and design

Participants were all first-year 'Safety and Security Management' students of a Dutch University of Applied Sciences (N = 88). Five participants missed the second session and four participants failed to complete the experiment due to technical problems. Therefore, the final sample consisted of 79 students (M_{age} = 19.16, SD = 1.61; 44 males). Because this study took place in a real educational setting and was part of an existing

course, our sample was limited to the total number of students in this cohort. In response to a reviewer, we added a power function of our analyses using the G*Power software (Faul et al., 2009). The power of our 3×2×2 mixed ANOVAs – under a fixed alpha level of .05, with a correlation between measures of .3, and with a sample size of 79 – is estimated at .36, .99, and > .99 for picking up a small, medium, and large interaction effect, respectively. Regarding our 2×2×2 mixed ANOVAs, the power is estimated at .32, .96, and > .99 for picking up a small, medium, and large interaction effect, respectively. The power of our study, thus, should be sufficient to pick up medium-sized effects, which is in line with the mean weighted medium effect size of self-explaining of previous studies as indicated in a recent meta-analysis (Bisra et al., 2018).

The experiment consisted of four phases: pretest, training-phase (CT-instructions plus practice), immediate posttest, and delayed posttest (see Table 1 for an overview). Participants were randomly assigned to one of two conditions: (1) self-explaining condition (CT-instructions and CT-practice with self-explanation prompts; n = 39) and (2) no self-explaining condition (CT-instructions and CT-practice without self-explanation prompts; n = 40). Of the four task categories tested in the pretest and posttests participants received instruction and practice on two task categories (one involving statistical and one involving logical reasoning, see section CT- skills tests). To ensure that any condition effects would not be due to specific characteristics of the instructed and practiced tasks, half of the participants in each condition got instruction and practice on the first logical and the first probabilistic reasoning task category (i.e., syllogism and base-rate), and the other half on the second logical and the second probabilistic reasoning task category (i.e., Wason and conjunction).

Materials

CT-skills tests

The pretest consisted of eight classic heuristics-and-biases tasks that reflected important aspects of CT across four categories (i.e., two of each category): 1) *Syllogistic reasoning* tasks, which examine the tendency to be influenced by the believability of a conclusion when evaluating the logical validity of arguments (adapted from Evans, 2003); 2) *Wason selection* tasks, that measure the tendency to verify rules rather than to falsify them (adapted from Stanovich, 2011); 3) *Base-rate* tasks, which measure the tendency to overrate individual-case evidence (e.g., from personal experience, a single case, or prior beliefs) and to underrate statistical information (adapted from Fong et al., 1986; Tversky & Kahneman, 1974); and 4) *Conjunction* tasks, that measure to what extent people neglect a fundamental rule in probability theory, that is, the conjunction rule ($P(A\&B) \leq P(B)$) which states that the probability of Event A and Event B both occurring must be lower than the probability of Event A or Event B occurring alone

(adapted from Tversky & Kahneman, 1983). The syllogistic reasoning and Wason selection tasks involve logical reasoning (i.e., Wason selection tasks can be solved by applying modus ponens and modus tollens from syllogistic reasoning) and the base-rate and conjunction tasks involve statistical reasoning (i.e., both require knowledge of probability and data interpretation). The content of the surface features (cover stories) of all test items was adapted to the study domain of the participants. A multiple-choice format with four answer options was used, with only one correct answer, except for one base-rate task where two answers were correct.

The immediate and delayed posttests were parallel versions of the pretest (i.e., structurally equivalent tasks but with different surface features). During the posttests, participants were additionally asked to motivate their MC-answers ("Why is this answer correct? Explain in steps how you have come to this answer.") by typing their motivation in a text entry box below the MC-question. The posttest items on the practiced task categories served to assess differences in learning outcomes, whereas the posttest items on the non-practiced task categories served to assess transfer performance. The transfer task categories shared similar features with the learning categories, namely, one requiring knowledge and rules of logic (i.e., syllogisms rules) and one requiring knowledge and rules of statistics (i.e., probability and data interpretation).

Table 1. Overview of the study design

	Self-explaining		No self-explaining	
	A (*n* = 18)	B (*n* = 21)	C (*n* = 22)	D (*n* = 18)
Pretest	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction
Training phase Instruction and practice (version)	Syllogism and Base-rate	Wason and Conjunction	Syllogism and Base-rate	Wason and Conjunction
Self-explanation prompts during practice (condition)	Yes	Yes	No	No
Immediate posttest	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction
Delayed posttest	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction	Syllogism, Wason, Base-rate, and Conjunction

CT-instructions

The text-based CT-instructions consisted of a general instruction on deductive and inductive reasoning and explicit instructions on two of the four categories from the pretest, including two extensive worked examples (of the tasks seen in the pretest) of each category. Participants received the following hints stating that the principles used in these tasks can be applied at several reasoning tasks: “Remember that these reasoning schemes can be applied in several reasoning tasks” and “Remember that the correct calculation of probabilities is an important skill that can be applied in several reasoning tasks”.

CT-practice

The CT-practice phase consisted of a case (315 words text) – on a topic that participants might encounter in their working-life – and four practice problems, two of each of the two task categories that students were given instructions on. In the self-explanation condition, participants were exposed to a self-explanation prompt after each of these tasks in which they were asked to explain how the answer was obtained: “Why is this answer correct? Explain in steps how you have come to this answer”.

Mental effort

After each test item participants reported how much mental effort they invested in completing that item, on a 9-point rating scale ranging from (1) very, very low effort to (9) very, very high effort (Paas, 1992; Paas & Van Merriënboer, 1993).

Procedure

The study was run during the first two lessons of a CT-course in the Safety and Security Management study program of an institute of higher professional education and conducted in the classroom with an entire class of students present. Participants signed an informed consent form at the start of the experiment. All materials were delivered in a computer-based environment (Qualtrics platform) that was created for this experiment, except for the paper-based case during the CT-instructions. The Qualtrics program randomly assigned the participants to a condition/version. Participants could work at their own pace, were allowed to use scrap paper while solving the tasks, and time-on-task was logged during all phases.

The study consisted of two sessions. In session 1 (during the first lesson of the course, ca. 90 min.), participants first completed the pretest. Subsequently, they had to read the CT-instructions and the case, followed by the practice problems, which differed according to the assigned condition/version. At the end, participants completed the immediate posttest. After two weeks, session 2 (during the second lesson of the course,

ca. 30 min.) was held in which participants completed the delayed posttest. Invested mental effort was rated after each test item on all CT-skills tests. Both the teacher and the experiment leader (first author of this paper) were present during all phases of the experiment.

Scoring

For selecting a correct MC-answer on the three CT-skills tests, 1 point was assigned, resulting in a maximum MC-score of four points on the learning (i.e., instructed/practiced task categories) items and four points on the transfer (i.e., task categories not instructed/practiced) items on each test. On the immediate and delayed posttest, participants were additionally asked to motivate their MC-answers. These motivations were scored based on a coding scheme that can be found on our OSF page. In addition to the MC-score (1 point), participants could earn a maximum of two points per question for the given motivation, resulting in a maximum total score (MC-plus-motivation) of three points per item. Because one syllogism task had to be removed from the tests due to an inconsistent variant in the delayed posttest (i.e., relatively easier form), participants who received instructions on the syllogistic reasoning and base-rate tasks, could attain a maximum total score of nine on the learning items and 12 on the transfer items on each posttest; and vice versa for the participants who received instructions on the Wason and conjunction tasks. For comparability, we computed percentage scores on the learning and transfer items instead of total scores. Two raters independently scored 25% of the immediate posttest. The intra-class correlation coefficient was .952 for the learning test items and .971 for the transfer test items. Because of these high inter-rater reliabilities, the remainder of the tests was scored by one rater.

The quality of participants' explanations was determined on the basis of the self-explanations given during the practice tasks with a maximum of two points per task (cf. posttest explanation-scoring procedure). As there were four practice tasks, the maximum self-explanation score was eight (ranging from 0 to 8). Two raters independently scored 25% of the tasks. Because the inter-rater reliability was high (intra-class correlation coefficient of .899), the remainder of the tasks was scored by one rater.

Results

For all analyses in this paper a *p*-value of .05 was used as a threshold for statistical significance. Partial eta-squared (η_p^2) is reported as a measure of effect size for the ANOVAs, for which .01 is considered small, .06 medium, and .14 large, and Cohen's *d*

is reported for the post-hoc tests, with values of 0.20, 0.50, and 0.80 representing a small, medium, and large effect size respectively (Cohen, 1988).

Preliminary analyses confirmed that there were no significant differences between the conditions before the start of the experiment in educational background, $\chi^2(3) = 2.41$, $p = .493$, gender, $\chi^2(1) = 0.16$, $p = .900$, or performance, time-on-task, and mental effort on the pretest (all Fs < 1, maximum $\eta_p^2 = .01$). An independent-samples t-test indicated, surprisingly, that there were no significant differences in time-on-task (in seconds) spent on practice of the instruction tasks between the self-explaining condition ($M = 409.25$, $SD = 273.45$) and the no self-explaining condition ($M = 404.89$, $SD = 267.13$), $t(77) = 0.07$, $p = .943$, $d = 0.02$.

Test performance

Data are provided in Table 2 and test statistics in Table 3. Regarding the version of the instruction, only main effects of Version or interactions of Version with other factors are reported. The remaining results are provided in Table 3.

Performance gains on MC-answers

To test hypotheses 1, 2a, and 2b, two 3×2×2 mixed ANOVAs were conducted with Test Moment (pretest, immediate posttest, and delayed posttest) as within-subjects factor and Self-explaining (self-explaining and no self-explaining) and Version (syllogism and base-rate: SB, and Wason selection and conjunction: WC) as between-subjects factors.

Test Moment significantly affected *learning* (i.e., performance on practiced tasks): performance was lower on the pretest ($M = 40.40$, $SD = 29.09$) than on the immediate posttest ($M = 78.06$, $SD = 26.22$), $p < .001$, $\eta_p^2 = .65$. Performance on the immediate posttest did not differ significantly from that on the delayed posttest ($M = 79.54$, $SD = 25.17$), $p = .611$, $\eta_p^2 < .01$. Note though, that there was an interaction between Test Moment and Version; participants who received the SB-version showed an immediate to delayed posttest performance gain ($M_{immediate} = 74.16$; $M_{delayed} = 78.28$), whereas the WC-version showed a slight performance drop ($M_{immediate} = 82.54$; $M_{delayed} = 81.45$); however, follow-up tests showed that the gain and drop were non-significant, $F(1, 38) = 13.12$, $p = .001$, $\eta_p^2 = .26$; $F(1, 37) = 0.07$, $p = .794$, $\eta_p^2 = .002$. There was no main effect of Self-explaining nor an interaction between Test Moment and Self-explaining, indicating that prompting self-explanations did not affect learning gains.

There was a main effect of Test Moment on test performance on *transfer* (i.e., non-practiced) items. Performance was lower on the pretest ($M = 36.71$, $SD = 27.07$) than on the immediate posttest ($M = 49.37$, $SD = 30.16$), $p < .001$, $\eta_p^2 = .17$, which in turn was lower than on the delayed posttest ($M = 58.02$, $SD = 29.07$), $p = .004$, $\eta_p^2 = .11$. There

was a main effect of Version: receiving the WC-version resulted in higher transfer performance (M = 57.98, SE = 3.46) than the SB-version (M = 38.47, SE = 3.42), indicating that transfer from WC-tasks to SB-tasks was higher than from SB-tasks to WC-tasks. Moreover, there was an interaction between Test Moment and Version. Follow-up analyses showed an effect of Test Moment for both the SB-version, $F(2, 76) = 10.74$, $p < .001$, $\eta_p^2 = .22$, and the WC-version, $F(2, 74) = 16.58$, $p < .001$, $\eta_p^2 = .31$. The pretest to immediate posttest performance gain was only significant for the SB-version, $F(1, 38) = 16.32$, $p = .001$, $\eta_p^2 = .30$, whereas the immediate to delayed posttest performance gain was only significant for the WC-version, $F(1, 37) = 17.64$, $p < .001$, $\eta_p^2 = .32$. There was no main effect of Self-explaining nor a significant interaction between Test Moment and Self-explaining, indicating that prompting self-explanations did not affect transfer performance.

Effects of self-explaining on learning outcomes (MC-plus-motivation)

To test hypothesis 3a, we analyzed the data of the MC-plus-motivation scores on learning items using a 2×2×2 mixed ANOVA with Test Moment (immediate posttest and delayed posttest) as within-subjects factor and Self-explaining (self-explaining and no self-explaining) and Version (syllogism and base-rate: SB, and Wason selection and conjunction: WC) as between-subjects factors (see Table 2 and 3 for data and test statistics, respectively). Pretest scores were not included in this analysis because the pretest only consisted of MC-questions. There was no main effect of Test Moment. Self-explaining significantly affected performance on learning items. Surprisingly, performance was higher in the no self-explaining condition (M = 64.36, SE = 3.26), compared to the self-explaining condition (M = 54.87, SE = 3.30). Note though, that there was an interaction between Self-explaining and Version. The effect of self-explaining was only found for the WC-version, $F(1, 37) = 7.66$, $p = .009$, $\eta_p^2 = .17$; there was no main effect of self-explaining for the SB-version, $F(1, 38) = 0.01$, $p = .953$, $\eta_p^2 < .01$. We did not find an interaction between Test Moment and Self-explaining.

Effects of self-explaining on transfer performance (MC-plus-motivation)

To test hypothesis 3b, we analyzed the data of the MC-plus-motivation scores on the transfer items using a 2×2×2 mixed ANOVA with Test Moment (immediate posttest and delayed posttest) as within-subjects factor and Self-explaining (self-explaining and no self-explaining) and Version (syllogism and base-rate: SB, and Wason selection and conjunction: WC) as between-subjects factors (see Table 2 and 3 for data and test statistics, respectively). There were no main effects of Test Moment and Self-explaining nor an interaction between Test Moment and Self-explaining. Collectively, the results on the transfer items again suggest that transfer occurred to a comparable extent in the self-explaining condition and the no self-explaining condition. Note though, that there was a main effect of version of instruction. In line with the findings on the MC-scores

data, performance was higher for the WC-version (M = 49.95, SE = 3.31) than the SB-version (M = 25.60, SE = 3.27), indicating that transfer was higher when instructed or practiced with the WC-tasks compared to the SB-tasks.

Mental effort investment

Again, data are provided in Table 2 and test statistics in Table 3. We exploratively analyzed the mental effort data (average mental effort invested per learning item) using two 3×2×2 mixed ANOVAs with Test Moment (pretest, immediate posttest, and delayed posttest) as within-subjects factor and Self-explaining (self-explaining and no self-explaining) and Version (syllogism and base-rate: SB, and Wason selection and conjunction: WC) as between-subjects factors (Question 4a and 4b). Regarding the version of the instruction, only main effects of Version or interactions of Version with other factors are reported. The remaining results are available in Table 3. One participant had more than two missing values and was removed from the analysis.

There were no main effects of Test Moment or Self-explaining on effort invested in *learning* items, nor an interaction between Test Moment and Self-explaining. Note tough, that there was a main effect of version of instruction. Less effort investment on learning items was reported for the WC-version (M = 3.65, SE = 0.17) than the SB-version (M = 4.52, SE = 0.17). Moreover, there was an interaction between Self-explaining and Version. The effect of self-explaining was only found for the WC-version, $F(1, 36) = 5.08$, $p = .030$, $\eta_p^2 = .12$; there was no main effect of self-explaining for the SB-version, $F(1, 38) = 1.26$, $p = .268$, $\eta_p^2 = .03$.

Regarding effort invested in *transfer* items, Mauchly's test indicated that the assumption of sphericity had been violated, $\chi 2$ (2) = 7.45, $p = .024$, and therefore Huynh-Feldt corrected tests are reported ($\varepsilon = .95$). Mental effort was affected by Test Moment. Invested mental effort was lower on the pretest (M = 3.98, SE = 0.14) compared to the immediate posttest (M = 4.63, SE = 0.15), $p < .001$, $\eta_p^2 = .21$, which did not differ from that on the delayed posttest (M = 4.38, SE = 0.17), $p = .160$, $\eta_p^2 = .03$. There was no main effect of Self- explaining nor an interaction between Test Moment and Self-explaining

Quality of self-explanations

Several authors have reported that self-explanations are only beneficial when the quality of the explanations is sufficient (e.g., Schworm & Renkl, 2007). To examine whether we could corroborate this finding, we conducted an exploratory analysis. Based on the quality of the self-explanations in the instruction tasks, we created three groups: (1) highest self-explanation scores (score ≥ 4; 25% of the total group), (2) scores between

2 and 3 (42% of the total group), and (3) lowest self-explanation scores (score ≤ 1; 33% of the total group). We examined whether the quality of the self-explanations was related to performance on the learning (practiced) items by conducting a mixed ANOVA (on participants in the self-explanation condition) with Test Moment (immediate posttest and delayed posttest) as within-subjects factor and Quality of Self-explanations (high, medium, and low) as between-subjects factor. There was no main effect of Test Moment, $F(1, 36) = 0.02$, $p = .881$, $\eta_p^2 < .01$, but there was a main effect of Quality of Self-explanations, $F(2, 36) = 8.79$, $p = .001$, $\eta_p^2 = .33$. The group with the lowest self-explanation scores performed lower on learning items ($M = 36.86$, $SE = 5.38$) than the group with the medium self-explanation scores ($M = 59.55$, $SE = 4.85$), $p < .001$. The group with the medium self-explanation scores did not differ from the group with the highest self-explanation scores ($M = 69.17$, $SE = 6.13$), $p = .226$. No interaction between Test Moment and Quality of Self-explanations was found, $F(2, 36) = 1.26$, $p = .297$, $\eta_p^2 = .06$.

A similar mixed ANOVA was conducted to explore whether the quality of the self-explanations was related to performance on the transfer (non-practiced) items. There was no main effect of Test Moment, $F(1, 36) = 2.73$, $p = .107$, $\eta_p^2 = .07$, no main effect of Quality of Self-explanations, $F(2, 36) = 0.01$, $p = .994$, $\eta_p^2 < .01$, nor an interaction between Test Moment and Quality of Self-explanations, $F(2, 36) = 0.61$, $p = .550$, $\eta_p^2 = .03$.

Table 2. Means (*SD*) of Test performance (multiple-choice % score), Test performance (multiple-choice plus motivation % score), and Mental effort (1–9) per Condition and Version

		Self-explaining			No self-explaining		
		A	B	Total	C	D	Total
Learning items							
Test performance (MC)	Pretest	55.56 (28.01)	26.19 (23.02)	39.74 (29.15)	57.58 (25.58)	20.83 (19.65)	41.04 (29.38)
	Immediate	74.07 (31.43)	76.19 (26.78)	75.21 (28.64)	74.24 (25.05)	88.89 (19.60)	80.83 (23.66)
	Delayed posttest	77.78 (22.87)	72.62 (31.53)	75.00 (27.64)	78.78 (24.22)	90.28 (17.44)	83.96 (21.96)
Test performance (MC-plus-motivation)	Immediate	58.64 (23.43)	51.59 (25.77)	54.84 (24.65)	60.61 (20.35)	68.06 (22.55)	63.96 (21.42)
	Delayed posttest	62.04 (24.53)	47.22 (26.26)	54.06 (23.39)	59.34 (18.50)	69.44 (20.01)	63.89 (19.61)
Mental effort	Pretest	4.30 (1.13)	4.20 (1.27)	4.25 (1.19)	4.52 (1.14)	3.61 (1.10)	4.11 (1.20)
	Immediate	3.98 (1.27)	3.68 (1.29)	3.82 (1.27)	4.80 (1.54)	3.18 (1.00)	4.07 (1.54)
	Delayed posttest	3.91 (1.24)	4.19 (1.78)	4.05 (1.53)	4.02 (1.42)	3.58 (1.49)	3.58 (1.49)
Transfer items							
Test performance (MC)	Pretest	25.00 (24.25)	44.44 (30.43)	35.47 (29.10)	29.55 (23.95)	48.15 (23.49)	37.92 (25.24)
	Immediate	40.28 (28.62)	46.03 (32.45)	43.38 (30.48)	48.86 (27.25)	62.96 (30.01)	55.21 (29.03)
	Delayed posttest	41.67 (30.92)	66.67 (25.82)	55.13 (30.63)	45.45 (25.16)	79.63 (16.72)	60.83 (27.55)
Test performance (MC-plus-motivation)	Immediate	22.69 (21.92)	37.30 (27.22)	30.56 (25.68)	26.89 (21.51)	55.86 (23.45)	39.93 (26.49)
	Delayed posttest	25.93 (23.55)	45.50 (24.95)	36.47 (25.95)	26.89 (19.23)	61.11 (18.86)	42.29 (25.52)
Mental effort	Pretest	4.01 (1.28)	4.20 (1.27)	4.11 (1.26)	4.05 (1.28)	3.61 (1.10)	3.85 (1.21)
	Immediate	4.42 (1.40)	4.47 (1.09)	4.44 (1.23)	5.17 (1.41)	4.37 (1.22)	4.81 (1.37)
	Delayed posttest	4.53 (1.41)	4.67 (1.69)	4.60 (1.48)	4.45 (1.48)	3.81 (1.56)	4.17 (1.53)

Note. Instructional conditions: Version A and C = instructed on and practiced with syllogistic reasoning and base-rate tasks; Version B and D = instructed on and practiced with Wason and conjunction tasks

Table 3. Results mixed ANOVAs

	Performance (MC-only)			Performance (MC+motivation)			Mental Effort		
	F-test (df)	*p**	η_p^2	F-test (df)	*p**	η_p^2	F-test (df)	*p**	η_p^2
Learning									
Test Moment	98.13 (2,150)	<.001*	.57	0.01 (1,75)	.925	<.01	2.67 (2,148)	.073	.04
Self-explaining	1.21 (1,75)	.274	.02	4.19 (1,75)	.044*	.05	0.57 (1,74)	.455	.01
Version	2.82 (1,75)	.097	.04	0.05 (1,75)	.817	<.01	6.46 (1,74)	.013*	.08
Test Moment × Self-explaining	1.57 (2,150)	.212	.02	0.02 (1,75)	.903	<.001	2.20 (2,148)	.115	.03
Test Moment × Version	24.53 (2,150)	<.001*	.25	0.32 (1,75)	.571	<.01	2.03 (2,148)	.135	.03
Self-explaining × Version	0.72 (1,75)	.397	.01	4.52 (1,75)	.037*	.06	5.61 (1,74)	.020*	.07
Test Moment × Self-explaining × Version	1.99 (2,150)	.141	.03	1.34 (1,75)	.250	.02	0.36 (1,148)	.697	.01
Transfer									
Test Moment	23.36 (2,150)	<001*	.24	3.63 (1,75)	.061	.05	7.03 (1.94,148.00)	.001*	.09
Self-explaining	3.00 (1,75)	.088	.04	1.97 (1,75)	.164*	.03	0.33 (1,74)	.565	<.01
Version	16.09 (1,75)	<.001*	.18	27.36 (1,75)	<.001*	.27	1.10 (1,74)	.297	.02
Test Moment × Self-explaining	0.93 (2,15)	.399	.01	0.50 (1,75)	.482	.01	2.90 (1.94,148.00)	.060	.04
Test Moment × Version	4.81 (2,150)	.009*	.06	1.36 (1,75)	.248	.02	0.27 (1.94,148.00)	.760	<.01
Self-explaining × Version	0.33 (1,75)	.569	<.01	2.43 (1,75)	.124	.03	2.48 (1,74)	.119	.03
Test Moment × Self-explaining × Version	0.38 (2,150)	.682	.01	0.00 (1,75)	.974	.000	0.06 (1.94,148.00)	.939	.001

*$p < .05$

Discussion

Previous research has shown that creating desirable difficulty in instruction by having learners generate explanations of a problem-solution to themselves (i.e., self-explaining) rather than simply answering tasks passively, is effective to foster learning and transfer in several domains (Fiorella & Mayer, 2016). Regarding unbiased reasoning, Heijltjes and colleagues (2014a) demonstrated that self-explaining during practice had a positive effect on transfer of unbiased reasoning, but this effect was short-lived and not replicated in other studies (Heijltjes et al., 2014b, 2015). However, these findings were based on MC-answers only, and there are indications that effects of self-explaining on transfer may be detected when more sensitive MC-plus-motivation tests are used (Hoogerheide et al., 2014). With the present experiment, we aimed to find out whether instruction followed by self-explaining during practice with heuristics-and-biases tasks would be effective for learning and transfer, using final tests that required participants to motivate their MC-answers.

Consistent with earlier research, our results corroborate the idea that explicit CT-instruction combined with practice is beneficial for learning to avoid biased reasoning (Hypothesis 1), as we found pretest to immediate posttest gains on practiced tasks, remaining stable on the delayed posttest after two weeks, as measured by performance on the MC-only questions. This is in line with the notion that the acquisition of relevant mindware contributes to an adequate use of Type 2 processing which can prevent biased reasoning (Stanovich et al., 2008). Contrary to earlier findings (e.g., Heijltjes et al., 2014a), our experiment seemed to provide some evidence that these instructions and practice tasks may also enhance transfer. However, this only applied to participants who practiced with the syllogism and base-rate version. For participants who received the other version, transfer performance gains were reached at a later stage. As such, this may mean that either transfer was easier from syllogism and base-rate to Wason and conjunction or, given that this pattern is not consistent across analyses, that our findings may reflect non-systematic variance. Another reason why caution is warranted in interpreting this finding is that the maximum scores differed per version, which, even though we used percentage scores, might be an issue for comparability.

As for our main question, we did not find any indications that prompting self-explanations to increase the difficulty of the practice tasks had a differential effect – compared to the control condition – on learning (Hypothesis 2a) or transfer (Hypothesis 2b) performance gains. Nor did the analyses of the MC-plus-motivation data show a benefit of prompting self-explanations during practice for learning (Hypothesis 3a) or transfer (Hypothesis 3b). Surprisingly, our findings even suggest that self-explaining during practice may

actually be less beneficial for learning: participants who received self-explanation prompts benefitted less from the instructions than those who were not prompted; however, this was only the case for one of the versions, so again, this finding needs to be interpreted with caution.

The findings of the present study are contrary to previous studies that demonstrated that self-explaining is effective for establishing both learning and transfer in a variety of domains (for a review see Fiorella & Mayer, 2016), but they are in line with the studies on unbiased reasoning (which assessed performance only by means of MC-answers) that demonstrated no positive effects (Heijltjes et al., 2014b, 2015) or only a short-lived effect of self-explaining on transfer (Heijltjes et al., 2014a). We did find that learners who gave lower quality self-explanations also performed worse on the learning items on the test (Question 5), which seems to corroborate the idea that a higher quality of self-explanations is related to higher performance (Schworm & Renkl, 2007), but it is possible that this finding reflects a priori knowledge or ability difference rather than an effect of the quality of self-explanations on performance. Thus, this study (with a more extensive performance measure) contributes to a small body of evidence that self-explanation prompts seem to have little or no benefit for acquiring unbiased reasoning skills.

One possible reason for the lack of a self-explanation effect could be the fact that the learners did not receive feedback on their self-explanations given in the practice phase. Providing feedback after students' self-explanations could have contributed to consolidating correct explanations and correcting or elaborating incorrect or incomplete explanations (e.g., Hattie & Timperley, 2007), which is of great importance in the domain of unbiased reasoning—arguably even more so than in other learning domains.

Another possibility might be that the nature of the tasks moderates effects of self-explaining. Contrary to previous studies, transfer on the tasks in the present study relies not only on deep understanding of the domain-specific knowledge involved in the task, but also on the ability to inhibit Type 1 processing and to switch to Type 2 processing. Possibly, prompting students to self-explain did not provoke the 'stop and think' reaction that was needed for transfer above and beyond what the instructions already accomplished. Our findings regarding effort investment support this idea (i.e., higher effort investment on transfer items on the posttests compared to the pretest in both conditions), suggesting that our training-phase provoked Type 2 processing, but there was no (additional) effect of the self-explanation prompts on effort investment.

A strength of the present study worth mentioning, is that – contrary to previous studies (e.g., Chi et al., 1994) – both conditions spent equal time on the practice tasks. Hence, it could be hypothesized that the beneficial effects of self-explaining in these studies are

not direct but caused by mediation: generating explanations usually requires more time and spending more time on subject matter increases performance. According to this hypothesis, the effect of self-explaining should disappear when time-on-task is equated between the conditions. Indeed, Matthews and Rittle-Johnson (2009) observed that solving tasks with self-explanations and solving more tasks without explanations in the same amount of time, resulted in equal final test performance. However, there are mixed results within the few studies that equated time-on-task, with some studies finding beneficial effects of self-explaining, while others did not (e.g., De Bruin et al., 2007; De Koning et al., 2011; McEldoorn et al., 2013; Matthews & Rittle-Johnson, 2009) and most other studies on self-explaining did not (fully) report time-on-task (see Bisra et al., 2018). Thus, there is a definite need for more research that examines the interplay between self-explanation, time-on-task, and final test performance.

Another possibility why we did not find effects of self-explaining on learning of unbiased reasoning skills, however, is that our study was conducted as part of an existing course and the learning materials were part of the exam. Because of that, students of the control condition may have imposed desirable difficulties on themselves, for instance by covertly trying to come up with explanations for the questions. It seems likely that students would be more willing to invest effort when their performance on the learning materials actually matters (intrinsically or extrinsically) for them, which is often the case in field experiments conducted in real classrooms where the learning materials are related to the students' study domain. Therefore, it is possible that effects of desirable difficulties such as self-explaining found in the psychological laboratory – where students participate to earn required research credits and the learning materials are not part of their study program and sometimes even unrelated to their study domain – might not readily transfer to classroom studies. This would explain why previous studies, which are mostly laboratory studies, demonstrated effects of self-explaining and why these effects were mostly absent and in one case only short-lived in the classroom studies on unbiased reasoning (e.g., Heijltjes et al., 2014a, 2014b, 2015). Moreover, this finding suggests a theoretical implication, namely that beneficial effects of creating desirable difficulty in instruction might become smaller when the willingness to invest increases and vice versa.

Future work might investigate why self-explanation prompts as used in the present study seem to have no additional effect after instruction and practice and whether strategies to improve students' quality of self-explanations would have beneficial effects on learning, and especially, transfer performance. Enhancing the quality of the self-explanations could be accomplished by, for example, providing students with a self-explanation training in advance or by providing prompts that include some instructional assistance (cf. Berthold et al., 2009). Moreover, future research could investigate via

classroom studies whether other desirable difficulties would be more beneficial for establishing learning and transfer of unbiased reasoning. In contrast to prompting self-explanations, other desirable difficulties such as creating task variability during practice and spacing of learning sessions apart, may result in beneficial effects since students of the control conditions cannot impose these desirable difficulties themselves (e.g., Weissgerber et al., 2018).

To conclude, based on the findings from the present study in combination with prior studies, prompting to self-explain during practice does not seem to be promising to enhance unbiased reasoning skills. This suggests that the nature of the task may be a boundary condition for effects of self-explaining on learning and transfer. Moreover, this study raises the question whether effects of self-explaining depend on the setting of the study, and thus contribute to knowledge about the usefulness of desirable difficulties in real educational settings. Considerably more research is needed to investigate how unbiased reasoning should be taught and especially how transfer can be fostered. This is important, because biased reasoning can have huge negative consequences in situations in both daily life and complex professional environments.

Chapter 3

Learning to avoid biased reasoning: Effects of interleaved practice and worked examples

This chapter has been submitted as:
Van Peppen, L. M., Verkoeijen, P. P. J. L., Kolenbrander, S. V., Heijltjes, A. E. G., Janssen, E. M., & Van Gog, T. (submitted). *Learning to avoid biased reasoning: Effects of interleaved practice and worked examples*.

Abstract

It is yet unclear which teaching methods are most effective for learning and transfer of critical thinking (CT) skills. Two experiments (laboratory: *N* = 85; classroom: *N* = 117), investigated the effect of practice schedule (interleaved vs. blocked) on students' learning and transfer of an important CT-skill, that is, unbiased reasoning, and whether it interacts with practice-task format (worked-examples vs. problems). After receiving CT-instructions, participants practiced in: (1) a blocked schedule with worked examples, (2) an interleaved schedule with worked examples, (3) a blocked schedule with problems, or (4) an interleaved schedule with problems. In both experiments, learning outcomes improved after instruction and practice. Surprisingly, there were no indications that interleaved practice led to better learning or transfer than blocked practice, irrespective of task format. The practice-task format did matter for novices' learning: worked examples were more effective than practice problems, which demonstrates – for the first time – that the worked-example effect also applies to novices' training of CT-skills.

Introduction

Critical thinking (CT) skills are essential for successful functioning in today's society. They are key to effective communication, problem solving, and decision-making in both daily life and professional environments (e.g., Billings & Roberts, 2014; Darling-Hammond, 2010; Kuhn, 2005). Therefore, it is worrying that many students struggle with several aspects of CT, such as avoiding biases in reasoning and decision-making (e.g., Flores et al., 2012; West et al., 2008), hereafter referred to as unbiased reasoning. Bias is said to occur when people rely on heuristics (i.e., mental shortcuts) during reasoning processes prior to choosing actions and estimating probabilities that result in systematic deviations from rational norms (Kahneman & Tversky, 1972, 1973; Tversky & Kahneman, 1974). Consequently, people who have difficulty with CT are more susceptible to making illogical and biased decisions that can have serious consequences, particularly in complex professional environments in which the majority of higher education graduates are employed (e.g., medicine: Ajayi & Okudo, 2016; Elia et al., 2016; Mamede et al., 2010; Law: Koehler et al., 2002). Hence, it is not surprising that helping students to become critically-thinking professionals is a major aim of higher education. However, it is not yet clear what teaching methods are most effective, especially to establish *transfer* (e.g., Heijltjes et al., 20141, 2014b, 2015; Van Peppen et al., 2018), which refers to the ability to apply acquired knowledge and skills in new situations (Halpern, 1998; Perkins & Salomon, 1992).

Contextual interference in instruction

According to the contextual interference effect, greater transfer is established when materials are presented and learned under conditions of high contextual interference (Schneider et al., 2002). High contextual interference can be created by varying practice-tasks from trial to trial (e.g., Battig, 1978). This task variability induces reflection on to-be-used procedures and can help learners to recognise distinctive characteristics of different problem types (i.e., inter-task comparing) and to develop more elaborate cognitive schemata that contribute to selecting and using a learned procedure when solving similar problems (evidencing learning) and new problems (evidencing transfer; Barreiros et al., 2007; Moxley, 1979).

High contextual interference can be achieved by interleaved practice as opposed to blocked practice. Whereas blocked practice involves practicing one task-category at a time before the next (e.g., AAABBBCCC), interleaved practice mixes practice of several categories together (e.g., ABCBACBCA). It has been suggested that reflection on the to-be-used procedures is what causes the beneficial effect of interleaved practice (e.g. Barreiros et al., 2007; Rau et al., 2010). Therefore, distinctiveness between task

categories should be high enough to reflect what strategy is required, but, on the other hand, should not be too high because learners then immediately recognise what procedure to apply. Research on interleaved practice has frequently demonstrated positive learning effects (for a recent meta-analysis, see Brunmair & Richtler, 2019), for example in laboratory studies with troubleshooting tasks (De Croock & Van Merriënboer, 2007; De Croock et al., 1998; Van Merriënboer et al., 1997, 2002); drawing tasks (Albaret & Thon, 1998); foreign language learning (Abel & Roediger, 2016; Carpenter & Mueller, 2013; Schneider et al., 2002); category induction tasks (Kornell & Bjork, 2008; Sana et al., 2018; Wahlheim et al., 2011); and learning of logical rules (Schneider et al., 1995). Furthermore, several classroom experiments found positive effects of interleaved practice in mathematics learning (e.g., Rau et al., 2013; Rohrer et al., 2014, 2015, 2019), and in astronomy learning (Richland et al., 2005).

The effect of interleaved practice on performance on reasoning tasks has received scant attention in the literature. However, it has been demonstrated with complex judgment tasks that interleaved practice enhanced not only learning but also transfer performance (Helsdingen et al., 2011a, 2011b). As this type of task seems similarly complex to reasoning tasks, considering that both rely on evaluation and interpretation of cues for making appropriate judgments, interleaved practice may have similar effects on learning and transfer of unbiased reasoning.

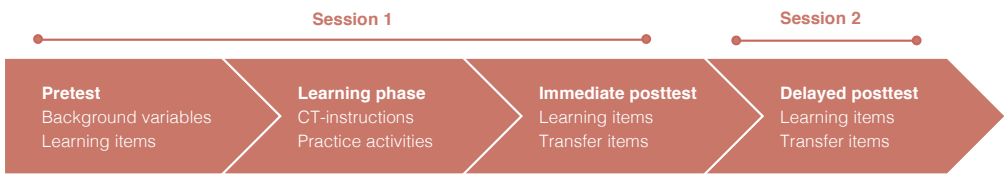
It is important to note, however, that interleaved practice is usually more cognitively demanding than blocked practice, that is, it places a higher demand on limited working memory recourses. Given that it also usually results in better (long-term) learning, interleaved practice seems to impose *germane* cognitive load (Sweller et al., 2011), or ‘desirable difficulties’ (Bjork, 1994). Nevertheless, there is a risk that learners, and especially novices, will experience excessively high cognitive load when engaging in interleaved practice, which may hinder learning (Likourezos et al., 2019). Using a practice-task format that reduces unnecessary cognitive load, like worked examples (i.e., step-by-step demonstrations of the problem solution; Paas et al., 2003a; Renkl, 2014; Sweller, 1988; Van Gog & Rummel, 2010; Van Gog et al., 2019) may help novices benefit from high contextual interference. The high level of guidance during learning from worked examples provides learners with the opportunity to devote attention towards processes – stimulated by interleaved practice – that are directly relevant for learning. As such, learners can use the freed up cognitive capacity to reflect on to-be-used procedures and develop cognitive schemata that contribute to selecting and using a learned procedure when solving similar and novel problems (Kalyuga, 2011; Renkl, 2014). Paas and Van Merriënboer (1994) indeed found that high variability during practice produced transfer test performance benefits (geometrical problem solving) when students studied worked examples, but not when they solved practice problems.

Moreover, students who studied worked examples perceived that they invested less mental effort in solving the transfer tasks than did the students who had solved practice problems.

The present study

The aim of the present study was to investigate whether there would be an effect of interleaved practice with 'heuristics-and-biases tasks' on experienced cognitive load, learning outcomes, and transfer performance (e.g., Tversky & Kahneman, 1974) and whether this effect would interact with the format of the practice-tasks (i.e., worked examples or practice problems). We simultaneously conducted 2 experiments: Experiment 1 was conducted in a laboratory setting with university students and Experiment 2 served as a conceptual replication conducted in a real classroom setting with students of a university of applied sciences[3]. Participants received instructions on CT and heuristics-and-biases tasks, followed by practice with these tasks. Figure 1 displays an overview of the study design: performance was measured as performance on practiced tasks (learning) and non-practiced tasks (transfer), and on a pretest, immediate posttest, and delayed posttest (two weeks later).

Figure 1. Overview of the study design. The four conditions differed in practice activities during the learning phase.



In line with previous findings (Heijltjes et al., 2014a, 2014b, 2015; Van Peppen et al., 2018), we hypothesised that students would benefit from the CT-instructions and practice activities, as evidenced by pretest to immediate posttest gains in performance on practiced items (i.e., *learning*; Hypothesis 1). Regarding our main question (see schematic overview in Table 1), we expected a main effect of interleaved practice, indicating that interleaved practice would require more effort during the practice phase (Hypothesis 2), but would also lead to larger performance gains on practiced items (i.e., *learning*; Hypothesis 3a) and higher performance on non-practiced items (i.e., *transfer*;

[3] The Dutch education system distinguishes between research-oriented higher education (i.e., offered by research universities) and profession-oriented higher education (i.e., offered by universities of applied sciences).

Hypothesis 3b) than blocked practice. We also expected a main effect of practice-task format: conform the worked example effect, we expected that studying worked examples would be less effortful during the practice phase (Hypothesis 4) and would lead to larger performance gains on practiced items (i.e., *learning*; Hypothesis 5a) and higher performance on non-practiced items (i.e., *transfer*; Hypothesis 5b) than solving problems. Finally, we expected an interaction effect, indicating that the beneficial effect of interleaved practice would be larger with worked examples than practice problems, on both practiced (i.e., *learning*; Hypothesis 6a) and non-practiced (i.e., *transfer*; Hypothesis 6b) items. A delayed (two weeks later) posttest was included, on which we expected these effects (Hypotheses 1-6) to persist. As effects of generative processing (relative to non-generative learning strategies) sometimes increase as time goes by (Dunlosky et al., 2013), they may be even greater after a delay.

Despite not having specific expectations, the mental effort during test data can provide additional insights into the effects of interleaved practice and worked examples on learning (Question 7a/8a) and transfer (Question 7b/8b). As people gain expertise, they can often attain an equal/higher level of performance with less/equal effort investment, respectively. As such, an effort investment decrease in instructed and practiced test items would indicate higher cognitive efficiency (Hoffman & Schraw, 2010; Van Gog & Paas, 2008).[4]

Table 1. Schematic overview of hypotheses 2-6

	Mental effort during learning	Test performance	
		Learning items	Transfer items
Practice schedule	Interleaved > Blocked (hypothesis 2)	Interleaved > Blocked (hypothesis 3a)	Interleaved > Blocked (hypothesis 3b)
Practice-task format	Examples < Problems (hypothesis 4)	Examples > Problems (hypothesis 5a)	Examples > Problems (hypothesis 5b)
Interaction Practice schedule and Practice-task format		Effect Interleaved over Blocked: Examples > Problems (hypothesis 6a)	Effect Interleaved over Blocked: Examples > Problems (hypothesis 6b)

[4] We also exploratively analyzed students' global judgments of learning (JOLs) after practice to gain insight into how informative the different practice types were according to the students themselves; however, these analyses did not have much added value for this paper, and, therefore, are not reported here but provided on our OSF-page.

Experiment 1

Materials and methods

We created an Open Science Framework (OSF) page for this project, where detailed descriptions of the experimental design and procedures are provided and where all data and materials (in Dutch) can be found (osf.io/a9czu).

Participants

Participants were 112 first-year Psychology students of a Dutch university. Of these, 104 students (93%) were present at both experimental sessions (see the procedure subsection for more information), and only their data were analysed. Participants were excluded from the analyses when test or practice sessions were not completed or when instructions were not adhered to, that is, when more than half of the practice tasks were not read seriously. Based on the fact that fast readers can read no more than 350 words per minute (e.g., Trauzettel-Klosinski & Dietz, 2012) – and the words in these tasks additionally require understanding – we assumed that participants who spent less than 0.17 seconds per word (i.e., 60 seconds/350 words) did not read the instructions seriously. This involved more participants from the worked examples conditions than the practice problems conditions and resulted in a final sample of 85 students (M_{age} = 19.84, *SD* = 2.41; 14 males). Based on this sample size, we have calculated a power function of our analyses using the G*Power software (Faul et al., 2009). The power of Experiment 1 – under a fixed alpha level of 0.05 and with a correlation between measures of 0.3 (e.g., Van Peppen et al., 2018) – is estimated at .24 for detecting a small interaction effect (η_p^2 = .01), .96 for a medium interaction effect (η_p^2 = .06), and > .99 for a large interaction effect (η_p^2 = .14). Thus, the power of our experiment should be sufficient to pick up medium-sized interaction effects, which is in line with the moderate overall positive effect of interleaved practice of previous studies as indicated in a recent meta-analysis (*g* = 0.42; Brunmair & Richter, 2019).

Design

The experiment consisted of four phases (see Figure 1): pretest, learning phase (CT-instructions plus practice), immediate posttest, and delayed posttest. A 3×2×2 design was used, with Test Moment (pretest, immediate posttest, and delayed posttest) as within-subjects factor and Practice Schedule (interleaved and blocked) and Practice-task Format (worked examples and practice problems) as between-subjects factors. After completing the pretest on learning items (i.e., instructed and practiced during the learning phase), participants received instructions and were randomly assigned to one of four practice conditions: (1) Blocked Schedule with Worked Examples Condition (*n* = 18); (2) Blocked Schedule with Practice Problems Condition (*n* = 28); (3) Interleaved

Schedule with Worked Examples Condition (n = 17); and (4) Interleaved Schedule with Practice Problems Condition (n = 22). Subsequently, participants completed the immediate posttest and two weeks later the delayed posttest on learning items (i.e., instructed and practiced during the learning phase) and transfer items (i.e., not instructed and practiced during the learning phase).

Materials

All materials were delivered in a computer-based environment (Qualtrics platform) that is created for this study.

CT-skills tests

All CT-skills tests consisted of nine classic heuristics-and-biases items across three categories (e.g., West et al., 2008) which we refer to as learning items as (isomorphs of) these items were instructed and practiced during the learning phase, (example-items in Appendix): (1) Base-rate items which measured the tendency to overweigh individual-case evidence (e.g. from personal experience, a single case, or prior beliefs) and to undervalue statistical information (Stanovich & West, 2000; Stanovich et al., 2016; Tversky & Kahneman, 1974); (2) Conjunction items that measured to what extent the conjunction rule (P(A&B) ≤ P(B)) is neglected—this fundamental rule in probability theory states that the probability of Event A and Event B both occurring must be lower than the probability of Event A or Event B occurring alone (adapted from Tversky & Kahneman, 1983); (3) Syllogistic reasoning items that examined the tendency to be influenced by the believability of a conclusion when evaluating the logical validity of arguments (Evans, 2003). To minimalize possible influences of distinctiveness between categories, we combined lower distinctive task categories (i.e., only requiring knowledge and rules of statistics: base-rate vs. conjunction) with higher distinctive task categories (i.e., requiring knowledge and rules of statistics and logic: base-rate vs. syllogistic reasoning and conjunction vs. syllogistic reasoning).

The immediate and delayed posttest contained parallel versions of the nine pretest learning items across three categories (base-rate, conjunction, and syllogism) that were designed as structurally equivalent but with different surface features. In addition, the immediate and delayed posttests also contained four items of two task-categories that were transfer items as these were not instructed and practiced during the learning phase. The transfer items shared similar features with the learning items, namely, requiring knowledge and rules of logic (i.e., syllogisms rules) or requiring knowledge and rules of statistics (i.e., probability and data interpretation), respectively: (1) Wason selection items which measured the tendency to confirm a hypothesis rather than to falsify it (adapted from Evans, 2002; Gigerenzer & Hug, 1992); and (2) Contingency

items measured the tendency to judge information given in a contingency table unequally, based on already experienced evidence (Heijltjes et al., 2014a; Stanovich & West, 2000; Wasserman, Dorner, & Kao, 1990).

The content of the surface features (cover stories) of all test items was adapted to the study domain of the participants. A multiple-choice (MC) format with different numbers of alternatives per item was used, with only one correct alternative for each task that evidences unbiased reasoning.

CT-instructions

The video-based instruction consisted of a general instruction on CT and explicit instructions on three heuristics-and-biases tasks. In the general instruction, the features of CT and the attitudes and skills that are needed to think critically were described. Thereafter, participants received explicit instructions on how to avoid base-rate fallacies, conjunction fallacies, and biases in syllogistic reasoning. These instructions consisted of a worked example of each category that not only showed the correct line of reasoning but also included possible problem-solving strategies. The worked examples provided solutions to the tasks seen in the pretest, which allowed participants to mentally correct initially erroneous responses.

CT-practice

The CT-practice phase consisted of nine practice tasks across the three task categories, in random order, of the pretest and the explicit instructions: base-rate (Br), conjunction (C), and syllogistic reasoning (S). Depending on the assigned condition, participants had to practice either in an interleaved (e.g. Br-C-S-C-S-Br-S-Br-C) or blocked schedule (e.g. Br-Br-Br-C-C-C-S-S-S), and either with worked examples or practice problems. Participants in the practice problems conditions were instructed to read the tasks thoroughly and to choose the best answer option. They received a prompt after each of the tasks in which they were asked to explain how the answer was obtained. After that, participants received correct-answer feedback. Participants in the worked examples conditions were instructed to read each worked-out example thoroughly. The worked examples consisted of a problem statement and a solution to this problem. The line of reasoning and underlying principles were explained in steps, sometimes clarified with a visual representation.

Mental effort

Invested mental effort was measured with the subjective rating scale developed by Paas (1992). After each practice-task and after each test item, participants reported how

much mental effort they invested in completing that task or item, on a 9-point scale ranging from (1) very, very low effort to (9) very, very high effort.

Procedure

The study was run in two sessions that both took place in the computer lab of the university. Participants signed an informed consent form at the start of the experiment. Before participants arrived, A4-papers were distributed among all cubicles (one participant in each cubicle) containing some general rules and a link to the Qualtrics environment of session 1, where all materials were delivered. Participants could work at their own pace and time-on-task was logged during all phases. Furthermore, participants were allowed to use scrap paper during the practice phase and the CT-tests.

In session 1 (ca. 75 min.), participants first filled out a demographic questionnaire and then completed the pretest. After each test item, they had to indicate how much mental effort they invested in it. Subsequently, participants entered the learning phase in which they first viewed the video (10 min.) including the general CT-instruction and the explicit instructions. Thereafter, the Qualtrics program randomly assigned the participants to one of the four practice conditions. Participants rated after each practice task how much mental effort they invested. After the learning phase, participants completed the immediate posttest and again rated their invested mental effort after each test item. The second session took place two weeks later and lasted circa 20 minutes. Participants again received an A4-paper containing some general rules and a link to the Qualtrics environment of session 2. This time, participants completed the delayed posttest and again reported their mental effort ratings after each test item. One experiment leader (first or third author of this paper) was present during all phases of the experiment.

Data analysis

Of the nine learning items, seven items were MC-only questions (with more than two alternatives) and two items were MC-plus-motivation questions (with two MC alternatives; one conjunction and one base-rate item) to prevent participants from guessing. The transfer items consisted of two MC-only and two MC-plus-motivation questions (two contingency items). Performance on the pretest, immediate posttest, and delayed posttest was scored by assigning 1 point to each correct alternative on the MC-only questions (i.e., referring to unbiased reasoning) and 1 point for the correct explanation, 0.5 point for a partially correct explanation, and 0 points for an incorrect explanation for all MC-plus-motivation questions (score form developed by the first author). As a result, participants could earn a maximum score of 9 on the learning items and a maximum total score of 4 on the transfer items. Two raters independently scored 25% of the explanations on the open questions of the immediate posttest. The intra-class correlation coefficient was .991 for the learning test items and .986 for the transfer test items.

Because of the high inter-rater reliability, the remainder of the tests was scored by one rater (the first author) and this rater's scores were used in the analyses.

For comparability, we computed percentage scores on the learning and transfer items instead of total scores. The mean score on the posttest learning items was 59.9% (*SD* = 20.22) and reliability of these items (Cronbach's alpha) was .24 on the pretest, .57 on the immediate posttest, and .51 on the delayed posttest. The low reliability on the pretest might be explained by the fact that a lack of prior knowledge requires guessing of answers. As such, inter-item correlations are low, resulting in a low Cronbach's alpha. Moreover, caution is required in interpreting these reliabilities because sample sizes as in studies like this do not seem to produce sufficiently precise alpha coefficients (e.g., Charter, 2003). The mean score on the posttest transfer items was 36.2% (*SD* = 22.31). Reliability of these items was low (Cronbach's alpha of .25 on the posttest and .43 on the delayed posttest), which can probably partly be explained by floor effects at both tests for one of our transfer task categories (i.e., Wason selection). Therefore, we decided not to report the test statistics of the analyses on transfer performance in the text but to report descriptive statistics only.

Results

In all analyses reported below, a significance level of .05 was used. Partial eta-squared (η_p^2) is reported as a measure of effect size for the ANOVAs for which .01 is considered small, .06 medium, and .14 large (Cohen, 1988). On our OSF-project page we presented the intention-to-treat (i.e., all participants who entered the study) analyses, which did not reveal noteworthy differences with the compliant-only (i.e., all participants who have met the criterion of spending more than 0.17 seconds per word for at least half of the practice tasks) analyses reported below.

Check on condition equivalence and time-on-task

Following the drop-out of some participants, we checked our conditions on equivalence. Preliminary analyses confirmed that the conditions did not differ in educational background, $\chi^2(15) = 15.68$, $p = .403$; performance on the pretest, $F(3, 81) = 1.68$, $p = .178$; time spent on the pretest, $F(3, 81) = 1.75$, $p = .164$; and average mental effort invested on the pretest items, $F(3, 81) = 0.78$, $p = .510$. We found a gender difference between the conditions, $\chi^2(3) = 11.03$, $p = .012$. However, gender did not correlate significantly with learning performance (minimum $p = .108$) and was therefore not a confounding variable.

A 2 (Practice Schedule: interleaved vs. blocked) × 2 (Practice-task Format: worked examples vs. practice problems) factorial ANOVA showed no significant differences on

time-on-task during practice between the interleaved and blocked conditions, $F(3, 81) = 3.05$, $p = .085$, $\eta_p^2 = .04$, but there was a significant difference between worked examples conditions ($M = 577.48$, $SE = 37.93$) compared to the practice problems conditions ($M = 737.61$, $SE = 31.96$), $F(3, 81) = 10.42$, $p = .002$, $\eta_p^2 = .11$. If it turns out that the practice problems conditions outperformed the worked examples conditions, this finding should be taken into account. No significant interaction between Practice Schedule and Practice-task Format was found, $F(3, 81) = 1.00$, $p = .320$, $\eta_p^2 = .01$.

Performance on learning items

Performance data are presented in Table 2 and all omnibus test statistics can be found in Table 3. A 3×2×2 mixed ANOVA on the items that assessed learning, with Test Moment (pretest, immediate posttest, and delayed posttest) as within-subjects factor and Practice Schedule (interleaved and blocked) and Practice-task Format (worked examples and practice problems) as between-subjects factors, showed a main effect of Test Moment. In line with Hypothesis 1, repeated contrasts revealed that participants performed better on the immediate posttest ($M = 61.07$, $SE = 2.10$) than on the pretest ($M = 24.30$, $SE = 1.46$), $F(1, 81) = 267.66$, $p < .001$, $\eta_p^2 = .77$. There was no significant difference between performance on the immediate and delayed posttest ($M = 63.76$, $SE = 1.93$), $F(1, 81) = 2.90$, $p = .092$, $\eta_p^2 = .04$.

In contrast to Hypothesis 3a (see Table 1 for a schematic overview of the hypotheses), we did not find a significant main effect of Practice Schedule or an interaction between Practice Schedule and Test Moment on performance on learning items. However, the analysis did reveal a main effect of Practice-task Format, with worked examples resulting in better performance ($M = 54.56$, $SE = 2.21$) than practice problems ($M = 44.87$, $SE = 1.86$). This was qualified by an interaction effect between Practice-task Format and Test Moment: in line with Hypothesis 5a, repeated contrasts revealed that there was a higher pretest to immediate posttest performance gain for worked examples ($M_{pre} = 23.82$, $SE = 2.23$; $M_{immediate} = 68.66$, $SE = 3.21$) than for practice problems ($M_{pre} = 24.78$, $SE = 1.88$; $M_{immediate} = 53.48$, $SE = 2.70$), $F(1, 81) = 12.90$, $p = .001$, $\eta_p^2 = .14$. Contrary to Hypothesis 6a, there was no interaction between Practice Schedule and Practice-task Format, nor an interaction between Practice Schedule, Practice-task Format, and Test Moment.

Performance on transfer items

To reiterate, we decided not to report the test statistics of the analyses on transfer performance due to low reliability of these items, but to report descriptive statistics only. Descriptive statistics showed that interleaved practice resulted in a numerically lower mean score on transfer items ($M = 35.20$, $SE = 3.00$) than blocked practice (M = 42.35, $SE = 2.80$). Furthermore, descriptive statistics showed that studying worked examples resulted in a numerically higher mean score ($M = 39.72$, $SE = 3.13$) than solving

problems (M = 37.84, SE = 2.64) and that interleaved practice resulted in a numerically higher mean score with worked examples (M = 36.03, SE = 4.50) than with practice problems (M = 34.38, SE = 3.96).

Mental effort during learning

Mental effort data are presented in Table 2 and all omnibus test statistics can be found in Table 3. Contrary to hypotheses 2 and 4 respectively, a 2 (Practice Schedule: interleaved and blocked) × 2 (Practice-task Format: worked examples and practice problems) factorial ANOVA on the mental effort during practice data revealed no main effects of Practice Schedule and Practice-task Format. Moreover, no interaction between Practice Schedule and Practice-task Format was found.

Mental effort during test

We exploratory analysed the mental effort during test data with a 3×2×2 mixed ANOVA on mental effort invested on learning items and a 2×2×2 mixed ANOVA on mental effort invested on transfer items (transfer items were not included in the pretest). Mental effort data during test is presented in Table 2 and all test statistics can be found in Table 3.

Regarding effort invested in the learning items, there was no main effect of Practice Schedule (Question 7a). However, there was a main effect of Practice-task Format (Question 8a); less invested effort on learning items was reported in the worked examples conditions (M = 3.57, SE = .13) compared to practice problems conditions (M = 3.92, SE = .11), and an interaction effect between Test Moment and Practice-task Format. Repeated contrasts revealed an effort investment increase over time with a significant difference between immediate and delayed posttest for the practice problems conditions ($M_{pretest}$ = 3.74, SE = .11; $M_{immediate}$ = 3.89, SE = .14; $M_{delayed}$ = 4.14, SE = .13), $F(1,48) = 6.08$, $p = .017$, $\eta_p^2 = .11$, and no significant differences for the worked examples conditions, $F(2,66) = .38$, $p = .683$, $\eta_p^2 = .01$. The results did not reveal a main effect of Test Moment and interaction effects.

Regarding invested mental effort in the transfer items, the results revealed a main effect of Practice Schedule (Question 7b), with higher effort investment when practiced in an interleaved schedule (M = 4.78, SD = .15) compared to a blocked schedule (M = 4.33, SD = .14). Furthermore, there was an effect of Practice-task Format (Question 7b): higher effort investment was reported by the practice problems conditions (M = 4.80, SD = .13) compared to worked examples conditions (M = 4.31, SD = .16). No main effect of Test Moment and interaction effects were found.

Interim summary

Taken together, there were no indications that interleaved practice – either in itself or as a function of task-format – contributed to better learning. However, interleaved practice resulted in higher effort investment on transfer items than blocked practice, which may indicate that interleaved practice stimulated analytical and effortful reasoning (i.e., Type 2 processing, e.g., Stanovich, 2011) more than blocked practice yet without resulting in replacement of the incorrect intuitive response (i.e., Type 1 processing) with the more analytical correct response. Alternatively, this finding may indicate a lower cognitive efficiency (Hoffman & Schraw, 2010; Van Gog & Paas, 2008) of interleaved practice as opposed to blocked practice. Furthermore, in line with the worked example effect (e.g., Sweller et al., 2011), studying worked examples was more effective for learning than solving problems, as well as more efficient (i.e., higher test performance reached in less practice time and less mental effort investment during the test phase; Van Gog & Paas, 2008). We will further elaborate on and discuss the findings of Experiment 1 in the General Discussion.

Table 2. Means (*SD*) of Test performance (multiple-choice % score) and Invested Mental Effort (1–9) per condition of Experiment 1

		Instructional conditions			
		Blocked Schedule Worked Examples	Blocked Schedule Practice Problems	Interleaved Schedule Worked Examples	Interleaved Schedule Practice Problems
Test performance					
Learning items	Pretest	23.46 (13.14)	29.37 (13.60)	24.18 (11.94)	20.20 (13.56)
	Immediate posttest	65.43 (23.15)	55.95 (18.27)	71.90 (18.89)	51.01 (15.96)
	Delayed posttest	68.86 (19.53)	59.13 (17.12)	73.86 (17.98)	53.54 (15.58)
Transfer items	Immediate posttest	43.06 (22.37)	40.63 (22.21)	36.03 (19.71)	26.70 (22.26)
	Delayed posttest	47.22 (24.08)	45.54 (18.07)	39.71 (28.03)	50.00 (18.90)
Mental effort during test					
Learning items	Pretest	3.47 (0.99)	3.73 (0.66)	3.84 (0.63)	3.76 (0.89)
	Immediate posttest	3.28 (1.23)	3.97 (0.99)	3.80 (0.58)	3.80 (0.90)
	Delayed posttest	3.25 (1.01)	4.09 (0.97)	3.80 (0.88)	4.20 (0.88)
Transfer items	Immediate posttest	4.14 (1.38)	4.81 (1.10)	4.85 (0.72)	4.81 (0.97)
	Delayed posttest	3.81 (1.45)	4.57 (0.80)	4.46 (0.98)	5.01 (0.94)
Mental effort during learning		3.51 (0.26)	4.05 (0.21)	4.20 (0.26)	4.11 (0.23)

Table 3. Results Mixed ANOVAs Experiment 1

	Test performance			Mental Effort		
	F-test (df)	*p**	η_p^2	F-test (df)	*p**	η_p^2
Learning items						
Test Moment	242.29 (2,162)	<.001*	.75	1.15 (1.837,148.825)	.315	.01
Test Moment × Practice Schedule	0.88 (2,162)	.417	.01	0.35 (1.837,148.825)	.689	.00
Test Moment × Practice-task Format	10.62 (2,162)	<.001*	.12	3.55 (1.837,148.825)	.035*	.04
Test Moment × Practice Schedule × Practice-task Format	0.01 (2,162)	.981	.00	0.40 (1.837,148.825)	.654	.01
Practice Schedule	0.17 (1,81)	.680	.00	2.11 (1,81)	.150	.03
Practice-task Format	11.30 (1,81)	.001*	.12	4.74 (1,81)	.032*	.06
Practice Schedule × Practice-task Format	3.47 (1,81)	.066	.04	2.28 (1,81)	.135	.03
Practice tasks	-	-	-	2.41 (1,81)	.125	.03
Practice-task Format	-	-	-	0.88 (1,81)	.352	.01
Practice Schedule × Practice-task Format	-	-	-	1.72 (1,81)	.194	.02

*p < .05

Experiment 2

As promising interventions sometimes fail in realistic settings (e.g., Hulleman & Cordray, 2009), we conducted a replication experiment in a classroom setting to assess the robustness of our findings and to increase ecological validity. All test and practice items were the same but, if necessary, adapted to the domain of the participants to meet the requirements of the study program.

Materials and methods

Participants and design

The design of Experiment 2 was the same as that of Experiment 1. Participants were 157 second-year 'Safety and Security Management' students of two locations of a Dutch university of applied sciences. Students from the first location had some prior knowledge as they had participated in a study that included similar heuristics-and-biases tasks in the first year of their curriculum that was followed by some lessons on this topic (n = 83), while students of the second location (n = 74) had not. Since the level of prior knowledge may be relevant (Likourezos et al., 2019), the factor Site will be included in the main analyses. Of the 157 students, 117 students (75%) were present at both sessions. As a large number of students missed the second session, we decided to conduct two separate analyses on performance and mental effort on learning items (transfer items were only included in the immediate and delayed posttest): pretest to immediate posttest analyses for all students present during session 1 and immediate posttest to delayed posttest analyses for all students present at both sessions. As in Experiment 1, participants who did not read the instructions seriously were excluded of the analyses. This resulted in a final subsample of 117 students (M_{age} = 20.05, SD = 1.76; 70 males; 60 higher knowledge) for the pretest-immediate posttest analyses and a final subsample of 89 students (M_{age} = 19.92, SD = 1.78; 46 males; 51 higher-knowledge) for the immediate posttest-delayed posttest analyses. Participants were randomly assigned to the Blocked Schedule with Worked Examples (n = 20; n = 15); Blocked Schedule with Practice Problems (n = 43; n = 33); Interleaved Schedule with Worked Examples (n = 15; n = 8); and Interleaved Schedule with Practice Problems (n = 39; n = 32) conditions.

Materials, procedure, and scoring

All data, materials, and detailed descriptions of the procedures and scoring are provided at the OSF-page of this project. The same materials were used as in Experiment 1 but the content of the surface features (cover stories) was adapted to the domain of the participants when the original features did not reflect realistic situations for these participants to keep the level of difficulty approximately equal to Experiment 1 and to

meet the requirements of the study program (i.e., the final exam was based on these materials).

The main difference with Experiment 1 was that Experiment 2 was run in a real education setting, namely during the lessons of a CT-course. Experiment 2 was conducted in a computer classroom at the participants' school with an entire class of students present. Participants came from eight different classes (of 25 to 31 participants) and were randomly distributed among the four conditions within each class. The two sessions of Experiment 2 took place during the first two lessons and between these lessons no CT-instruction was given. In advance of the first session, students were informed about the experiment by their teacher. When entering the classroom, participants were instructed to sit down at one of the desks and read the A4-paper containing some general instructions and a link to the Qualtrics environment of session 1 where they first signed an informed consent form. Again, participants could work at their own pace and could use scrap paper and time-on-task was logged during all phases. Participants had to wait (in silence) until the last participant had finished the posttest before they were allowed to leave the classroom. The experiment leader and the teacher of the CT-course (first and third author of this paper) were both present during all phases of the experiment and one of them explained the nature of the experiment afterwards.

The same test-items and score form for the open questions were used as in Experiment 1. Again, participants could attain a maximum score of 9 on the learning items and a maximum total score of 4 on the transfer items and we computed percentage scores on the learning and transfer items instead of total scores. Two raters independently scored 25% of the open questions of the immediate posttest. Because the intra-class correlation coefficient was high (.931 for learning test items; .929 for transfer test items), the remainder of the tests was scored by one rater (the third author) and this rater's scores were used in the analyses.

The mean score on the posttest learning items was 62.5% (*SD* = 19.06) and reliability of these items was .36 on the pretest, .45 on the posttest and .52 on the delayed posttest (Cronbach's alpha). Again, the low reliability on the pretest might be explained by the fact that a lack of prior knowledge requires guessing of answers, resulting in low inter-item correlations and subsequently a low Cronbach's alpha. Moreover, caution is warranted in interpreting these reliabilities because a sample size as in our study does not seem to produce precise alpha coefficients (e.g., Charter, 2003). The mean score on the posttest transfer items was 32.2% (*SD* = 25.55) and reliability of these items was .36 on the posttest and .30 on the delayed posttest (Cronbach's alpha). In view of this low reliability, which can probably partly be explained by floor effects at both tests for

one of our transfer task categories (i.e., Wason selection), we decided not to report the test statistics of the analyses on transfer performance but only the descriptive statistics.

Results

In all analyses reported below, a significance level of .05 was used. Partial eta-squared (η_p^2) is reported as a measure of effect size for the ANOVAs for which .01 is considered small, .06 medium, and .14 large. On our OSF-project page we presented the intention-to-treat (i.e., all participants who entered the study) analyses, which did not reveal noteworthy differences with the compliant-only analyses. As it might have been of influence that half of the students had some prior knowledge as they participated in a study that included similar heuristics-and-biases tasks in the first year of their curriculum, we included the factor Site in all analyses.

Check on condition equivalence and time-on-task

Preliminary analyses confirmed that there were no significant differences between the conditions in educational background, $\chi^2(9) = 10.00$, $p = .350$; gender, $\chi^2(3) = .318$, $p = .957$, or performance on the pretest, time spent on the pretest, and mental effort invested on the pretest items (maximum $F = 1.30$, maximum $\eta_p^2 = .03$). A one-way ANOVA indicated that there were no significant differences in time-on-task (in seconds) spent on practice of the instruction tasks, $F(3, 116) = 1.73$, $p = .165$, $d = 0.016$.

Performance on learning items

Performance data are presented in Table 4 and omnibus test statistics in Table 5. The data on learning items were analysed with two 2×2×2×2 mixed ANOVAs with Test Moment (analysis 1: pretest and immediate posttest; analysis 2: immediate posttest and delayed posttest) as within-subjects factor and Practice Schedule (interleaved and blocked), Practice-task Format (worked examples and practice problems), and Site (low prior knowledge and higher prior knowledge learners) as between-subjects factors. In line with Hypothesis 1, the pretest-immediate posttest analysis showed a main effect of Test Moment on learning outcomes: participants performed better on the immediate posttest ($M = 61.40$, $SE = 1.49$) than on the pretest ($M = 46.13$, $SE = 1.59$).

Contrary to Hypothesis 3a (see Table 1 for a schematic overview of the hypotheses), the results did not reveal a significant main effect of Practice Schedule, nor an interaction with Test Moment. We did find an interaction effect between Test Moment and Practice-task Format: in line with Hypothesis 5a, there was a higher pretest to immediate posttest performance gain for worked examples ($M_{pre} = 38.79$; $M_{immediate} = 71.96$) than for practice problems ($M_{pre} = 41.71$; $M_{immediate} = 58.24$), $F(1, 109) = 22.18$, $p < .001$, $\eta_p^2 = .17$. In contrast to Hypothesis 6a, the results did not reveal an interaction between Practice

Schedule and Practice-task Format, nor an interaction between Practice Schedule, Practice-task Format, and Test Moment. However, there was a main effect of Site, with higher-knowledge learners performing better (M = 60.95, SE = 2.00) than low-knowledge learners (M = 44.39, SE = 1.97). Moreover, we found an interaction between Test Moment and Site, with a higher increase in learning outcomes for low-knowledge learners (M_{pre} = 29.36, SE = 2.25; $M_{immediate}$ = 59.43, SE = 2.31) compared to higher-knowledge learners (M_{pre} = 51.14, SE = 2.38; $M_{immediate}$ = 70.77, SE = 2.34). Interestingly, our results revealed an interaction between Test Moment, Practice-task Format, and Site. Follow-up analyses revealed that low-knowledge learners showed a larger increase in learning outcomes when they practiced with worked examples (M_{pre} = 27.58, SE = 2.83; $M_{immediate}$ = 70.30, SE = 4.28) compared to practice problems (M_{pre} = 31.14, SE = 2.63; $M_{immediate}$ = 48.55, SE = 2.94), $F(1, 53) = 22.17$, $p < .001$, $\eta_p^2 = .30$. For higher-knowledge learners, the differences in learning gains between the worked examples and practice problems conditions were no longer significant, $F(1, 56) = 3.00$, $p = .089$, $\eta_p^2 = .05$.

The second analysis – to test whether our results are still present after two weeks – showed a significant main effect of Test Moment: participants' performance on learning items improved from immediate (M = 63.13, SE = 2.19) to delayed (M = 67.71, SE = 2.31) posttest. In contrast to Hypotheses 3a, 5a, and 6a respectively, there was no main effect of Practice Schedule, no main effect of Practice-task Format, no interaction between Practice Schedule and Practice-task Format, nor interactions with Test Moment. Again, there was a main effect of Site: higher-knowledge learners performed higher on learning items (M = 72.73, SE = 2.49) than low-knowledge learners (M = 58.11, SE = 3.26). Furthermore, an interaction between Practice Schedule, Practice-task Format, and Site was found. Follow-up analyses revealed that, for low-knowledge learners practice in a blocked schedule worked best with worked examples compared to practice problems (M_{WE} = 69.14, SE = 5.78; M_{PS} = 47.57, SE = 4.34), while in an interleaved schedule practice problems were more beneficial (M_{WE} = 52.78, SE = 12.27; M_{PS} = 62.96, SE = 5.01), $F(1, 35) = 4.43$, $p = .043$, $\eta_p^2 = .11$. There was no significant interaction between Practice Schedule and Practice-task Format for higher-knowledge learners, $F(1, 45) = 1.87$, $p = .178$, $\eta_p^2 = .04$. No other interaction effects were found.

Performance on transfer items

Descriptive statistics showed that blocked practice resulted in a numerically higher mean score on transfer items (M = 31.71, SE = 2.86) than interleaved practice (M = 27.04, SE = 4.00). Moreover, studying worked examples (M = 28.92, SE = 4.35) did not result in a numerically higher mean score than solving problems (M = 29.82, SE = 2.29) and interleaved practice with worked examples (M = 24.33, SE = 7.27) did not result in a higher numerically mean score than with practice problems (M = 29.74, SE = 3.31). However, the descriptive statistics showed that for low-knowledge learners practice

problems resulted in a descriptively higher mean score (M = 30.01, SE = 3.46) than worked examples (M = 27.26, SE = 7.09), while for higher-knowledge learners worked examples did not result in a higher numerically mean score (M = 30.58, SE = 5.04) than practice problems (M = 29.63, SE = 2.99). For both low-knowledge learners (M_{WE} = 21.88, SE = 12.82; M_{PS} = 30.73, SE = 5.24) and higher-knowledge learners (M_{WE} = 26.79, SE = 6.85; M_{PS} = 28.75, SE = 4.06), interleaved practice with worked examples did not result in a numerically higher mean score than with practice problems.

Mental effort during learning

Mental effort data are presented in Table 4 and omnibus test statistics in Table 5. Contrary to Hypotheses 2 and 4, respectively, a 2 (Practice Schedule: interleaved and blocked) × 2 (Practice-task Format: worked examples and practice problems) × 2 (Site: low prior knowledge learners and higher prior knowledge learners) factorial ANOVA on the mental effort during practice data revealed no main effects of Practice Schedule and Practice-task Format, nor an interaction between Practice Schedule and Practice-task Format was found. Moreover, no main effect of Site nor interactions between Practice Schedule, Practice-task Format, and Site were found.

Mental effort during test

Our pretest-immediate posttest analyses on effort invested on learning items showed no main effects of Practice Schedule (Question 7a) and Practice-task Format (Question 8a), nor an interaction between Practice Schedule and Practice-task Format. The results did reveal a significant interaction between Test Moment, Practice Schedule, and Site, but follow-up analyses revealed no significant interactions between Test Moment and Practice Schedule for both sites (maximum F = 3.47, maximum η_p^2 = .06). No main effects of Test Moment and Site, nor other significant interactions were found.

Our second analysis – to test whether our results were still present after two weeks – showed no main effects of Practice Schedule (Question 7b) and Practice-task Format (Question 8b), nor an interaction between Practice Schedule and Practice-task Format. However, a three-way interaction between Test Moment, Practice Schedule, and Practice-task Format was found. Follow-up analyses revealed that interleaved practice with worked examples resulted in an immediate posttest – delayed posttest increase in effort investment ($M_{immediate}$ = 3.58; $M_{delayed}$ = 3.97) and with practice problems in an immediate posttest – delayed posttest decrease in effort investment ($M_{immediate}$ = 4.45; $M_{delayed}$ = 4.07), $F(1, 36) = 4.21$, p = .047, η_p^2 = .11. There was no significant difference in immediate posttest – delayed posttest effort investment between the practice-task format conditions when practiced in a blocked schedule, $F(1, 43) = 2.74$, p = .105, η_p^2 = .06. No main effects of Test Moment and Site, nor other interactions were found.

Our analyses on effort invested in transfer items revealed no main effects of Practice Schedule, Practice-task Format, Test Moment, or Site. Moreover, there were no significant interaction effects.

Interim summary

The results of Experiment 2 provide converging evidence with Experiment 1. Again, we did not find any indications that interleaved practice would be more beneficial than blocked practice for learning, either in itself or as a function of task format. There was again a benefit of studying worked examples over solving problems, but – as was to be expected – this was limited to participants who had low prior knowledge (i.e., had not participated in a study that included similar heuristics-and-biases tasks in the first year of their curriculum).

Table 4. Means (*SD*) of Test performance (multiple-choice % score) and Invested Mental Effort (1–9) per condition and analysis of Experiment 2

		Instructional conditions			
		Blocked Schedule Worked Examples	Blocked Schedule Practice Problems	Interleaved Schedule Worked Examples	Interleaved Schedule Practice Problems
Analysis 1					
Test performance					
Learning items	Pretest	35.56 (20.58)	41.09 (20.65)	40.00 (20.91)	43.59 (27.55)
	Immediate posttest	68.33 (15.83)	56.85 (21.17)	75.56 (15.83)	60.68 (16.49)
Mental effort during test					
Learning items	Pretest	3.81 (0.99)	4.01 (0.87)	3.97 (1.09)	4.23 (1.08)
	Immediate posttest	3.78 (1.10)	3.86 (1.09)	3.78 (1.10)	4.36 (0.95)
Analysis 2					
Test Performance					
Learning items	Immediate posttest	68.15 (16.19)	58.25 (21.70)	72.22 (18.78)	62.50 (14.87)
	Delayed posttest	71.85 (16.19)	63.64 (22.95)	70.83 (19.64)	70.14 (13.37)
Transfer items	Immediate posttest	30.83 (22.04)	27.65 (22.04)	26.39 (19.21)	30.86 (26.56)
	Delayed posttest	35.83 (19.97)	32.20 (21.43)	33.33 (20.84)	28.13 (22.67)
Mental effort during test					
Learning items	Immediate posttest	3.80 (1.11)	3.83 (0.99)	3.65 (1.65)	4.42 (0.97)
	Delayed posttest	3.83 (1.23)	4.16 (1.01)	3.90 (1.62)	4.03 (1.18)
Transfer items	Immediate posttest	4.74 (1.10)	4.88 (1.06)	4.69 (2.25)	5.44 (1.35)
	Delayed posttest	4.27 (1.50)	5.18 (1.18)	5.00 (2.07)	5.21 (1.24)
Mental effort during learning		3.84 (1.10)	4.05 (1.11)	3.97 (1.05)	4.48 (0.85)

Table 5. Results mixed ANOVAs on performance on learning items in Experiment 2

		Test performance			Mental effort		
	ANOVA	F-test (df)	p*	η_p^2	F-test (df)	p*	η_p^2
Pretest – Immediate Posttest	Test Moment	198.07 (1,109)	<.001*	.65	0.55 (1,108)	.459	.01
	Test Moment × Practice Schedule	1.05 (1,109)	.308	.01	0.00 (1,108)	.971	.00
	Test Moment × Practice-task Format	22.18 (1,109)	<.001*	.17	0.81 (1,108)	.370	.02
	Test Moment × Practice Schedule × Practice-task Format	0.35 (1,109)	.558	.00	3.34 (1,108)	.070	.03
	Test Moment × Site	8.73 (1,109)	.004*	.07	2.50 (1,108)	.117	.02
	Test Moment × Site × Practice Schedule	0.30 (1,109)	.584	.00	5.58 (1,108)	.020*	.05
	Test Moment × Site × Practice-task Format	6.04 (1,109)	.016*	.05	1.27 (1,108)	.262	.01
	Test Moment × Site × Practice Schedule × Practice-task Format	0.97 (1,109)	.326	.01	1.37 (1,108)	.244	.01
	Practice Schedule	1.42 (1,109)	.236	.01	0.78 (1,108)	.378	.01
	Practice-task Format	3.70 (1,109)	.057	.03	2.54 (1,108)	.114	.02
	Practice Schedule × Practice-task Format	0.06 (1,109)	.806	.00	1.01 (1,108)	.316	.01
	Site	34.79 (1,109)	<.001*	.24	2.18 (1,108)	.143	.02
	Site × Practice Schedule	2.27 (1,109)	.135	.02	0.03 (1,108)	.855	.00
	Site × Practice-task Format	1.73 (1,109)	.191	.02	0.72 (1,108)	.398	.01
	Site × Practice Schedule × Practice-task Format	1.12 (1,109)	.292	.01	0.63 (1,108)	.430	.01
Immediate – Delayed Posttest	Test Moment	6.07 (1,80)	.016*	.07	0.65 (1,79)	.422	.01
	Test Moment × Practice Schedule	0.01 (1,80)	.943	.00	0.62 (1,79)	.432	.01
	Test Moment × Practice-task Format	1.29 (1,80)	.260	.02	1.15 (1,79)	.286	.01
	Test Moment × Practice Schedule × Practice-task Format	0.58 (1,80)	.450	.00	7.50 (1,79)	.008*	.09
	Test Moment × Site	0.49 (1,80)	.485	.00	3.13 (1,79)	.081	.04
	Test Moment × Site x Practice Schedule	0.80 (1,80)	.375	.00	0.11 (1,79)	.744	.00
	Test Moment × Site x Practice-task Format	0.02 (1,80)	.898	.01	0.87 (1,79)	.354	.01
	Test Moment × Site x Practice Schedule × Practice-task Format	0.59 (1,80)	.444	.01	0.13 (1,79)	.718	.00
	Practice Schedule	0.00 (1,80)	.984	.00	0.16 (1,79)	.693	.00
	Practice-task Format	1.29 (1,80)	.260	.02	1.27 (1,79)	.264	.02
	Practice Schedule × Practice-task Format	1.50 (1,80)	.225	.02	0.24 (1,79)	.623	.00
	Site	12.72 (1,80)	.001*	.14	0.17 (1,79)	.686	.00
	Site × Practice Schedule	0.19 (1,80)	.891	.00	0.01 (1,79)	.909	.00
	Site × Practice-task Format	0.07 (1,80)	.800	.00	0.02 (1,79)	.878	.00
	Site × Practice Schedule × Practice-task Format	7.01 (1,80)	.010*	.08	0.14 (1,79)	.715	.00
Practice tasks	Practice Schedule	-	-	-	1.34 (1,109)	.250	.01
	Practice-task Format	-	-	-	2.34 (1,109)	.129	.02
	Practice Schedule × Practice-task Format	-	-	-	0.69 (1,109)	.409	.01
	Site	-	-	-	1.11 (1,109)	.294	.01
	Site × Practice Schedule	-	-	-	0.15 (1,109)	.698	.00
	Site × Practice-task Format	-	-	-	0.32 (1,109)	.572	.00
	Site × Practice Schedule × Practice-task Format	-	-	-	0.62 (1,109)	.431	.01

*$p < .05$

General discussion

Previous research has demonstrated that providing students with explicit CT-instructions combined with practice on domain-relevant tasks is beneficial for learning to reason in an unbiased manner (e.g., Heijltjes et al., 2015) but not for transfer to new tasks. Therefore, the present experiments investigated whether creating contextual interference in instruction through interleaved practice – which has been proven effective in other and similar domains – would promote both learning and transfer of reasoning skills.

In line with our expectations and consistent with earlier research (e.g., Heijltjes et al., 2015; Van Peppen et al., 2018), both experiments support the finding that explicit instructions combined with practice improves learning of unbiased reasoning (Hypothesis 1), as we found pretest to immediate posttest gains on practiced tasks in all conditions, which remained stable on the delayed posttest after two weeks. This is in line with the idea of Stanovich (2011) that providing students with relevant mindware and stimulating them to inhibit incorrectly used intuitive responses (i.e., Type 1 processing, e.g., Evans, 2008; Kahneman & Klein, 2009; Stanovich, 2011, 2016) and to replace them with more analytical and effortful reasoning (i.e., Type 2 processing) is useful to prevent biases in reasoning and decision-making. Furthermore, the performance gain on practiced tasks suggests that having learners repeatedly retrieve to-be-learned material (i.e., repeated retrieval practice: e.g., Karpicke & Roediger, 2007) may be a promising method to further enhance learning to avoid biased reasoning.

Contrary to our hypotheses, however, we did not find any indications that interleaved practice would improve learning more than blocked practice (Hypothesis 3a), regardless of whether they practiced with worked examples or problem-solving tasks (Hypothesis 6a). These findings are in contrast to previous studies that demonstrated that interleaved practice is effective for establishing both learning and transfer in other domains and with other complex judgment tasks (e.g., Likourezos et al., 2019). Moreover, they are contrary to the finding of Paas and Van Merriënboer (1994) that high variability during practice with geometrical problems produced test performance benefits when students studied worked examples, but not when they solved practice problems. Unfortunately, we were not able to test our hypotheses regarding transfer performance (Hypothesis 3b/6b). Therefore, it is unknown whether interleaved practice – either in itself or as a function of task-format – would be beneficial for transfer of unbiased reasoning. However, given that the transfer scores were overall rather low, we can assume the overall effect of instruction and practice (if present at all) would seem to be limited.

One of the more interesting findings to emerge from this study, however, is that the worked example effect (e.g., Paas & Van Gog, 2006; Renkl, 2014) also applies to CT-tasks. Moreover, this was found even though the instructions that preceded the practice tasks already included two worked examples. As most of the studies on the worked example effects used pure practice conditions or gave minimal instructions prior to practice, these examples could have helped students in the problem-solving conditions perform better on the practice problems; nevertheless, we still found a worked example effect.

To the best of our knowledge, the results of Experiment 1 demonstrated for the first time in CT-instruction a benefit of studying worked examples over solving problems on learning outcomes, reached with less effort during the tests (i.e., more effective and efficient, Van Gog & Paas, 2008). Experiment 2 replicated the worked example effect (i.e., more effective than solving problems) and demonstrated that this was the case for novices, but not for learners with relatively more prior knowledge. This observation supports findings regarding the expertise reversal effect (e.g., Kalyuga, 2007; Kalyuga et al., 2003, 2012), which shows that while instructional strategies that assist learners in developing cognitive schemata are effective for low-knowledge learners, they are often not effective (or may even be detrimental) for higher-knowledge learners. Moreover, as far as we know, our second experiment was the first to actually vary both level of guidance (i.e., practice-task format) and level of expertise along with practice schedule and, thus, our study provides a first step in exploring the interactions between these three factors. It would be interesting in future research to manipulate students' level of expertise to actually demonstrate a causal relationship between expertise and the effect of studying worked examples on learning outcomes in CT-instruction. Finally, one could argue that the unequal cell distribution (i.e., higher exclusion in worked examples conditions compared to practice problems conditions based on reading time of instructions) may indicate that students' motivation may have been the basis for the worked example effect. However, our intention-to-treat analyses still revealed a worked example effect and, therefore, this possible explanation does not seem convincing. Yet, this points to another remarkable finding, that is, that worked examples were more beneficial for learning than problems, even if the examples were minimally read; possibly, students quickly located and processed the relevant information in the examples.

A possible explanation for the absence of an interleaved practice effect on learning outcomes might lie in the distinctiveness between the task categories, which may have been greater than in previous studies. Effects of interleaved practice only occur if task categories differ and require different problem-solving procedures. However, as reflection on the to-be-used procedures is what causes the beneficial effect of

interleaved practice (e.g., Barreiros et al., 2007; Rau et al., 2010), distinctiveness between categories should not be too high because learners then immediately recognise what procedure to apply. It seems possible that the task categories used in the present study were the same at a high level but that the mindware needed for each category differed too much. If so, determining the nature of each task was relatively easy and intertask comparing was not necessary. It should be noted, though, that this was not expected in advance and that arguing that the distinctiveness between task categories was too high after we know the results is risky, because of hindsight bias (Fischhoff, 1975).

Another possible explanation for the absence of an interleaving effect on learning outcomes, might be that the surface characteristics within two of the practice-task categories were so different (i.e., base-rate and conjunction) that students in the blocked practice condition did not realise that strategies could be reused in subsequent tasks of that category. As such, they might have been stimulated as much as students in the interleaved practice condition to stop and think about new problem-solving strategies. It seems possible that interleaved practice is useful for practice within a task category in which surface characteristics are similar to each other and problem-solving procedures differ slightly (e.g., syllogistic reasoning tasks), but further research should be undertaken to investigate this.

Moreover, it should be noted that the relatively low reliabilities, implying high amounts of measurement error, of our learning test items might have played a crucial role as it largely decreased the power to detect intervention effects (Cleary et al., 1970; Kanyongo et al., 2007; Schmidt & Hunter, 1996). Although sample sizes as in studies like this do not seem to produce sufficiently precise alpha coefficients (e.g. Charter, 2003), the possibility that the items were not sufficiently related or that students do not see the overlap between the items should be taken into account. Future research, therefore, would need to find ways to improve CT measures (i.e., decrease random measurement error) or should utilise measures known to have acceptable levels of reliability (LeBel & Paunonen, 2011). The latter option seems challenging, however, as multiple studies report rather low levels of reliability of tests consisting of heuristics-and-biases tasks (Aczel et al., 2015b; Bruine de Bruin et al., 2007; West et al., 2008) and revealed concerns with the reliability of widely used standardised CT tests, particularly with regard to subscales (Bernard et al., 2008; Ku, 2009; Leppa, 1997; Liu et al., 2014).

To conclude, the present experiments provide evidence that worked examples can be effective for novices' learning to avoid biased reasoning. However, there were no indications that practice in an interleaved schedule – with worked examples or practice problems – enhances performance on heuristics-and-biases tasks. These findings

suggest that the nature or the combination of the task categories may be a boundary condition for effects of interleaved practice on learning and transfer. Further research should be undertaken to investigate what the exact boundary conditions of effects of interleaved practice are and to provide more insight into the expertise-reversal effect in CT-instruction. Moreover, future research could investigate whether other types of (generative) activities would be beneficial for establishing learning and transfer of unbiased reasoning and whether it is feasible at all to teach students to inhibit Type 1 processing and to recognise when Type 2 processing is needed. It is important to continue the search for effective methods to foster transfer, because biased reasoning can have huge negative consequences in situations in both daily life and complex professional environments.

Appendix

Example of each category of tasks included in the critical thinking tests including the correct answer and explanation.

Base-rate item

Imagine you work at the Human Recourses department of a large oil company. The director is considering a reorganisation, but she is afraid that this can have negative consequences for the atmosphere at work. She asks you for advice. You know few reorganisations are successful, that is, in few situations both the company and the employees are satisfied with the end result. You decide to search for success factors and found out that many successful reorganisations are supported by external consultancy.

What information would you want to have in order to estimate the probability that the reorganisation at your company will be successful given that it is supported by external consultancy? Below are four pieces of information that may or may not be relevant for determining the probability. Please indicate what information is needed to make a good estimate of the probability that the reorganisation will proceed successfully when the director consults external consultancy. Choose one or more of the alternatives, but only those that are necessary to make the estimate.

1) Probability of companies that were supported by external consultancy and had a successful reorganisation
2) Marginal probability of successful reorganisations
3) Marginal probability of unsuccessful reorganisations
4) Probability of companies that were supported by external consultancy and had an unsuccessful reorganisation

Correct answer: 1+2+4 or 1+3+4

Explanation: The explanation of this item is illustrated by the crosstab below. You are asked for A/(A+C). People should not overrate the first piece of information (A/total) as this probability only gives information about companies that were supported by external consultancy and had a successful reorganisation. This probability is uninformative for the probability estimation requested if you do not have the fourth piece of information, that is, the probability of companies that were supported by external consultancy and had an unsuccessful reorganisation (C/total). Additionally, you need the second piece of information, that is, the marginal probability of successful reorganisations ((A+B))/total). Since the marginal probability of successful reorganisations could be derived from the marginal probability of unsuccessful reorganisations, you could also choose for the third piece of information ((C+D)total).

	External consultancy		Total
Reorganisation	Yes	No	
Successful	A	B	A+B
Unsuccessful	C	D	C+D
Total	A+C	B+D	A+B+C+D (total)

Syllogistic reasoning item

Below, you will find two premises that you must assume are true. Please indicate whether the conclusion follows logically from the premises.

Premise 1. If a safety product is of good quality, people are willing to pay a high price for it.
Premise 2. People are willing to pay a high price for the Vimtag security camera.
Conclusion: The Vimtag security camera is a product of good quality.

a) Conclusion follow logically from the premises
b) Conclusion does not follow logically from the premises

Correct answer: b

Explanation: This assignment requires that participants do not confuse logical validity of the conclusion with the believability of the conclusion. The conclusion is (presumably) believable for participants due to their prior knowledge or real-world knowledge. If the first part of premise 1 (if a safety product is of good quality) is met, then the second part (people are willing to pay a high price for it) automatically follows. The second premise states that people are willing to pay a high price for the Vimtag security camera. But this does not necessary mean that the camera is a product of good quality. There might be another reason.

Conjunction item

Gerard is 51 years old. He is professor at the Erasmus University in Rotterdam and his research focusses on crisis communication. In recent years he has won several science awards, published many articles, and was asked to present on many conferences. Please rank the following eight statements on the 9-point rating scales in terms of probability.

a) Gerard has studied architecture and is afraid of heights
b) Gerard has studied Communication
c) Gerard has studied Safety Management
d) Gerard has presentation anxiety
e) Gerard has a fear of flying
f) Gerard has studied Psychology
g) Gerard has studied Safety Management and has presentation anxiety
h) Gerard has a fear of illness

Correct answer: estimated probability of (g) < estimated probability of (c) and estimated probability of (g) < estimated probability of (d).

Explanation: People should neglect the tendency to judge the conjunction of attributes (representative 'Safety Management' and unrepresentative 'presentation anxiety') as more probable than the less representative constituent ('presentation anxiety') of each conjunction.

Contingency item

Imagine you are an entrepreneur and your company is on the brink of bankruptcy. Your neighbour tells you about Corporate Fixer: a company that specialises in solving business problems. 'They do fan-tas-tic work', he says, 'the company of a good friend of mine became extremely successful after their help!' You visit their website and find out that the services of Corporate Fixer are quite pricey. You are prepared to pay the price, provided that you have a better chance of solving your business problems with their help than without any help. On an independent comparison website, you see that (a) 188 companies received help from corporate fixer and solved their business problems, (b) 95 companies did not receive help and solved their problems, (c) 90 companies received help without solving their business problems, and (d) 25 companies did not receive help and did not solve their problems:

	Help from Corporate Fixer	No help from Corporate Fixer
Business problems solved	188	95
Business problems unsolved	90	25

Based on this information, would you hire the help from Corporate fixer or not?

a) Yes
b) No

Correct answer: b

Explanation: The information given in a 2x2 contingency table should be evaluated equally instead of the tendency to focus on the large number in cell A. More specifically, you have to compare the probability of business problems solved given help from Corporate Fixer and the probability of business problems solved given no help from Corporate Fixer. The first probability is lower than the second and, therefore, you would not hire the help from Corporate Fixer.

Wason selection item

The Dutch airport Schiphol is keen on a substantial flight expansion. 'No problem at all', the airport claims. But is that true? The additional flights may not produce negative environmental effects. Schiphol has had the following rule for years:

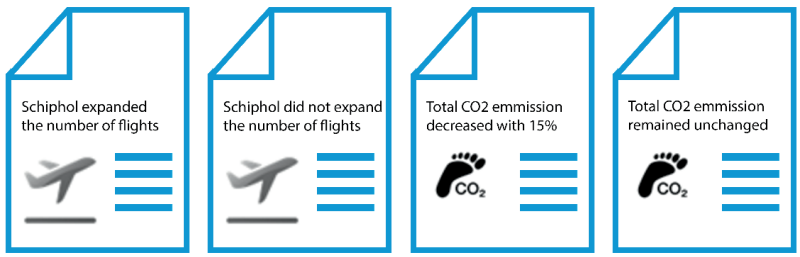
> *If Schiphol increases the number of flights, the total CO_2 emissions from air Traffic must have decreased by 10% compared to the previous years.*

In a presentation about the importance of Schiphol as 'engine of the Dutch economy', it is expertly explained that the aircraft of today fly even more economically and that the total CO_2 emissions from air traffic decreased by 20% compared to last year.

Assume that you are an official at the Dutch Ministry of Infrastructure and Environment and – based on the above rule – you have to decide whether Schiphol can realise the intended flight expansion. If the statements from the presentation are true, Schiphol would be allowed to continue the intended expansion. However, there are also indications that the claims of Schiphol are incorrect and that the airport has attenuated the negative environmental effects of air traffic. You decide to have an independent researcher chart whether Schiphol has complied with the rule in the past. The researcher investigates four random years and draws up four separate reports. There are only two findings in each report:

1. The total CO_2 emissions from air traffic compared to the previous years did / did not decrease
2. Schiphol expanded / did not expand the number of flights that year

Below you see only one of the two findings from each of the four annual reports. You will have to read the entire report to find the second finding of the year in question. Which annual report(s) should you read to check whether Schiphol has complied with the rule in the past? Choose one or more of the options below, but only choose the options that are necessary to check if Schiphol had complied with the rule.



a b c d

Correct answer: a + d

Explanation: This assignment requires that participants do not only seek to confirm the rule but also look for falsification of the rule. By reading the report with the finding that Schiphol expanded the number of flights, you can test whether the rule is violated: if the total CO_2 emissions have not decreased, the rule is violated. The same for reading the report with the finding that the total CO_2 emission remained unchanged: if that report also states that Schiphol expanded the number of flights, the rule is violated. Because if Schiphol expanded the number of flights, the total CO_2 emissions should have decreased. People who choose other options than the combination of 'Schiphol expanded the number of flights' + 'the total CO_2 emission remained unchanged' probably fail to apply logical principles, verify rules rather than to falsify them, or demonstrate matching bias by selecting options explicitly mentioned in the conditional statement.

Chapter 4

Enhancing students' critical thinking skills: Is comparing correct and erroneous examples beneficial?

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Abstract

There is a need for effective methods to teach critical thinking (CT). One instructional method that seems promising is comparing (or *contrasting*) correct and erroneous examples. The aim of the present study, therefore, was to investigate the effect of comparing correct and erroneous examples on learning and transfer of CT-skills, focusing on avoiding biased reasoning. Students (N = 170) received instructions on CT and avoiding biases in reasoning tasks, followed by: (1) contrasting examples, (2) correct examples, (3) erroneous examples, or (4) practice problems. Performance was measured on a pretest, immediate posttest, three-week delayed posttest, and nine-month delayed posttest. Our results revealed that participants' reasoning task performance improved from pretest to immediate posttest, and even further after a delay. Surprisingly, there were no differences in learning gains or transfer performance between the four practice conditions. Our findings raise questions about the preconditions of contrasting examples effects. Moreover, how learning and transfer of CT-skills can be fostered remains an important issue for future research.

Introduction

Every day, we reason and make many decisions based on previous experiences and existing knowledge. To do so we often rely on a number of heuristics (i.e., mental shortcuts) that ease reasoning processes (Tversky & Kahneman, 1974). Usually, these decisions are inconsequential but sometimes they can lead to *biases* (i.e., deviating from ideal normative standards derived from logic and probability theory) with severe consequences. To illustrate, a forensic expert who misjudges fingerprint evidence because it verifies his or her preexisting beliefs concerning the likelihood of the guilt of a defendant, displays the so-called confirmation bias, which can result in a misidentification and a wrongful conviction (e.g., the Madrid bomber case; Kassin et al., 2013). Our primary tool for reasoning and making better decisions is *critical thinking* (CT), which is generally characterized as "purposeful, self-regulatory judgment that results in interpretation, analysis, evaluation, and inference, as well as explanation of the evidential, conceptual, methodological, criteriological, or contextual considerations on which that judgment is based" (Facione, 1990, p.2).

Because CT is essential for successful functioning in one's personal, educational, and professional life, fostering students' CT has become a central aim of higher education (Davies, 2013; Halpern, 2014; Van Gelder, 2005). However, several large-scale longitudinal studies were quite pessimistic that this laudable aim would be realized merely by following a higher education degree program. These studies revealed that CT-skills of many higher education graduates are insufficiently developed (e.g., Arum & Roksa, 2011; Flores et al., 2012; Pascarella et al., 2011; although a more recent meta-analytic study reached the more positive conclusion that students' do improve their CT-skills over college years: Huber & Kuncel, 2016). Hence, there is a growing body of literature on how to teach CT (e.g., Abrami et al., 2008, 2014; Angeli & Valanides, 2009; Niu et al., 2013; Tiruneh et al., 2014, 2016; Van Peppen et al., 2018). It is well established, for instance, that explicit teaching of CT combined with practice improves learning of CT-skills required for unbiased reasoning (e.g., Abrami et al., 2008, Heijltjes et al., 2014b). However, while some effective teaching methods have been identified, it is as yet unclear under which conditions *transfer* of CT-skills can be promoted, that is, the ability to apply acquired knowledge and skills to novel situations (e.g., Barnett & Ceci, 2002). As it is important for CT-skills acquired in higher education to transfer to other domains and on-the-job, it is crucial to acquire more knowledge on how transfer of these skills can be fostered (and this also applies to CT-skills more generally, see for example, Halpern, 2014; Kenyon & Beaulac, 2014; Lai, 2011; Ritchhart & Perkins, 2005). One instructional method that seems promising is comparing (or contrasting) correct and erroneous worked examples (e.g., Durkin & Rittle-Johnson, 2012).

Benefits of studying examples

Over the last decades, a large body of research has investigated learning from studying worked examples as opposed to unsupported problem solving. Worked examples consist of a problem statement and an entirely and correctly worked-out solution procedure (in this paper referred to as correct examples; Renkl, 2014; Renkl et al., 2009; Sweller et al., 1998; Van Gog et al., 2019). Typically, studying correct examples is more beneficial for learning than problem-solving practice, especially in initial skill acquisition (for reviews, see Atkinson et al., 2000; Renkl, 2014; Sweller et al., 2011; Van Gog et al., 2019). Although this worked example effect has been mainly studied in domains such as mathematics and physics, it has also been demonstrated in learning argumentation skills (Schworm & Renkl, 2007), learning to reason about legal cases (Nievelstein et al., 2013) and medical cases (Ibiapina et al., 2014), and novices' learning to avoid biased reasoning (Chapter 3).

The worked example effect can be explained by cognitive load imposed on working memory (Paas et al., 2003a; Sweller, 1988). Cognitive Load Theory (CLT) suggests that – given the limited capacity and duration of our working memory – learning materials should be designed so as to decrease unnecessary cognitive load related to the presentation of the materials (i.e., extraneous cognitive load). Instead, learners' attention should be devoted towards processes that are directly relevant for learning (i.e., germane cognitive load). When solving practice problems, novices often use general and weak problem-solving strategies that impose high extraneous load. During learning from worked examples, however, the high level of instructional guidance provides learners with the opportunity to focus directly on the problem-solving principles and their application. Accordingly, learners can use the freed up cognitive capacity to engage in generative processing (Wittrock, 2010). Generative processing involves actively constructing meaning from to-be-learned information, by mentally organizing it into coherent knowledge structures and integrating these principles with one's prior knowledge (i.e., Grabowski, 1996; Osborne & Wittrock, 1983; Wittrock, 1974, 1990, 1992, 2010). These knowledge structures in turn can aid future problem solving (Kalyuga, 2011; Renkl, 2014; Van Gog et al., 2019).

Regarding unbiased reasoning, a recent study showed that studying correct examples after initial instruction was more beneficial than problem-solving practice on isomorphic tasks on a final test, but not on transfer tasks (Chapter 3). The latter finding might be explained by the fact that students sometimes process worked examples superficially and do not spontaneously use the freed up cognitive capacity to engage in generative processing needed for successful transfer (Renkl & Atkinson, 2010). Another possibility is that these examples did not sufficiently encourage learners to make abstractions of the underlying principles and explore possible connections between problems (e.g.,

Perkins & Salomon, 1992). It seems that to fully take advantage of worked examples in learning unbiased reasoning, students should be encouraged to be actively involved in the learning process and facilitated to focus on the underlying principles (e.g., Van Gog et al., 2004).

The potential of erroneous examples

While most of the worked-example research focuses on correct examples, recent research suggests that students learn at a deeper level and may come to understand the principles behind solution steps better when (also) provided with erroneous examples (e.g., Adams et al., 2014; Barbieri & Booth, 2016; Booth et al., 2013; Durkin & Rittle-Johnson, 2012; McLaren et al., 2015). In studies involving erroneous examples, which are often preceded by correct examples (e.g., Booth et al., 2015), students are usually prompted to locate the incorrect solution step and to explain why this step is incorrect or to correct it. This induces generative processing, such as comparison with internally represented correct examples and (self-)explaining (e.g., Chi et al., 1994; Renkl, 1999; McLaren et al., 2015). Students are encouraged to go beyond noticing surface characteristics and to think deeply about *how* erroneous steps differ from correct ones and why a solution step is incorrect (Durkin & Rittle-Johnson, 2012). This might help them to correctly update schemas of correct concepts and strategies and, moreover, to create schemas for erroneous strategies (Durkin & Rittle-Johnson, 2012; Große & Renkl, 2007; Siegler, 2002; Van den Broek & Kendeou, 2008; VanLehn, 1999), reducing the probability of recurring erroneous solutions in the future (Siegler, 2002).

However, erroneous examples are typically presented separately from correct examples, requiring learners to use mental resources to recall the gist of the no longer visible correct solutions (e.g., Große & Renkl, 2007; Stark et al., 2011). Splitting attention across time increases the likelihood that mental resources will be expended on activities extraneous to learning, which subsequently may hamper learning (i.e., temporal contiguity effect: e.g., Ginns, 2006). One could, therefore, argue that the use of erroneous examples could be optimized by providing them side by side with correct examples (e.g., Renkl & Eitel, 2019). This would allow learners to focus on activities directly relevant for learning, such as structural alignment and detection of meaningful commonalities and differences between the examples (e.g., Durkin & Rittle-Johnson, 2012; Roelle & Berthold 2015). Indeed, studies on comparing correct and erroneous examples (referred to as contrasting examples) revealed positive effects in math learning (Durkin & Rittle-Johnson, 2012; Kawasaki, 2010; Loibl & Leuders, 2018, 2019; Siegler, 2002).

The present study

We already indicated that it is still an important open question, which instructional strategy can be used to enhance transfer of CT skills. Contrasting examples, especially when presented side-by-side with correct examples, seem to hold a considerable promise with respect to promoting generative processing and transfer. Hence, the purpose of the present study was to investigate whether contrasting examples of fictitious students' solutions on 'heuristics-and-biases tasks' (a specific sub-category of CT skills: e.g., Tversky & Kahneman, 1974) would be more effective to foster learning and transfer than studying correct examples only, studying erroneous examples only, or solving practice problems. The study was conducted at the start of an existing first-year CT-course (i.e., classroom study). Participants received video-based instructions on the importance of CT and on reasoning tasks, followed by (1) contrasting examples, (2) correct examples, (3) erroneous examples, or (4) practice problems (control condition). Performance was measured on a pretest, immediate posttest, three-week delayed posttest, and nine-month delayed posttest, to examine effects on learning and transfer.

Based on the literature presented above, we hypothesized that studying correct examples would impose less cognitive load (i.e., lower investment of *mental effort during learning*) than solving practice problems (i.e., worked example effect: e.g., Chapter 3; Renkl, 2014). Whether there would be differences in cognitive load between contrasting examples, studying erroneous examples, and solving practice problems, however, is an open question. That is, it is possible that these instructional formats impose a similar level of cognitive load, but originating from different processes: while practice problem solving may impose extraneous load that does not contribute to learning, generative processing of contrasting or erroneous examples may impose germane load that is effective for learning (Sweller et al., 2011). As such, it is important to consider (experienced) cognitive load (i.e., invested mental effort) in combination with learning outcomes. In sum, we predict the following pattern of results regarding invested mental effort during learning (Hypothesis 1): correct examples < contrasting examples ≤ erroneous examples ≤ practice problems.

Secondly, we hypothesized that students in all conditions would benefit from the CT-instructions combined with the practice activities, as evidenced by pretest to immediate posttest gains in performance on instructed and practiced items (i.e., *learning*: Hypothesis 2). Furthermore, based on cognitive load theory, we hypothesized that studying correct examples would be more beneficial for learning than solving practice problems (i.e., worked example effect: e.g., Chapter 3; Renkl, 2014). Based on the aforementioned literature, we expected that studying erroneous examples would promote generative processing more than studying correct examples. Whether that generative processing would actually enhance learning, however, is an open question.

This can only be expected to be the case if learners can actually remember and apply the previously studied information on the correct solution, which arguably involves higher cognitive load (i.e., temporal contiguity effect) than studying correct examples or comparing correct and erroneous examples. As contrasting can help learners to focus on key information and thereby induces generative processes directly relevant for learning (e.g., Durkin & Rittle-Johnson, 2012), we expected that contrasting examples would be most effective. Thus, we predict the following pattern of results regarding performance gains on learning items (Hypothesis 3): contrasting examples > correct examples ≥ erroneous examples ≥ practice problems.

Furthermore, we expected that generative processing would promote transfer. Despite findings of previous studies in other domains (e.g., Paas, 1992), we found no evidence in a previous study that studying correct examples or solving practice problems would lead to a difference in transfer performance (Chapter 3). Therefore, we predict the following pattern of results regarding performance on non-practiced items of the immediate posttest (i.e., *transfer*, Hypothesis 4): contrasting examples > correct examples ≥ erroneous examples ≥ practice problems.

We expected these effects (Hypotheses 3 and 4) to persist on the delayed posttests. As effects of generative processing (relative to non-generative learning strategies) sometimes increase as time goes by (Dunlosky et al., 2013), they may be even greater after a delay.

Method

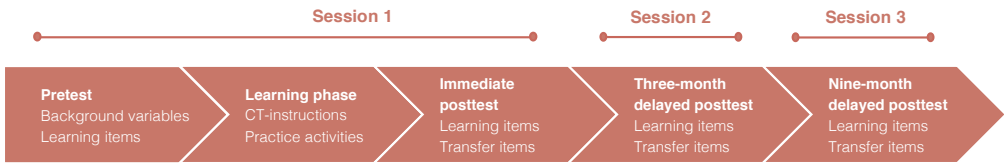
We created an Open Science Framework (OSF) page for this project, where all materials, the dataset, and all script files of the experiment are provided (https://osf.io/8zve4/).

Participants and design

Participants were 182 first-year 'Public Administration' and 'Safety and Security Management' students of a Dutch University of Applied Sciences (i.e., total number of students in these cohorts). Of these, 173 students (95%) completed the first experimental session (see Figure 1 for an overview) and 158 students (87%) completed both the first and second experimental session. Additionally, 83 of these students (46%) of the Safety and Security Management program completed the nine-month delayed posttest.

We defined a priori that participants would be excluded in case of excessively fast reading speed. Considering that even fast readers can read no more than 350 words per minute (e.g., Trauzettel-Klosinski & Dietz, 2012), we excluded participants who spent less than 0.17 seconds per word (i.e., 60 seconds/350 words). This resulted in a final sample of 170 students (M_{age} = 19.54, *SD* = 1.93; 57 female) for the pretest to posttest analyses, a subsample of 155 students for the immediate to three-week delayed posttest analyses (M_{age} = 19.46, *SD* = 1.91; 54 female), and a subsample of 82 students (46%) for the immediate to nine-month delayed posttest (M_{age} = 19.27, *SD* = 1.79; 25 female). We calculated a power function of our analyses using the G*Power software (Faul et al., 2009) based on these sample sizes. The power for the crucial Practice Type × Test Moment interaction – under a fixed alpha level of 0.05 and with a correlation between measures of 0.3 (e.g., Van Peppen et al., 2018) – for detecting a small (η_p^2 = .01), medium (η_p^2 = .06), and large effect (η_p^2 = .14) respectively, is estimated at .42, > .99, and 1.00 for the pretest to posttest analyses; .39, > .99, and 1.00 for the immediate to three-week delayed posttest analyses; and .21, .90, and > .99 for the immediate to nine-month delayed posttest. Thus, the power of our study should be sufficient to pick up medium-sized interaction effects.

Figure 1. Overview of the study design. The four conditions differed in practice activities during the learning phase.



Students participated in a pretest-intervention-posttest design (see Figure 1). After completing the pretest on learning items (i.e., instructed and practiced during the learning phase), all participants received succinct CT instructions and two correct worked examples. Thereafter, they were randomly assigned to one of four conditions that differed in practice activities during the learning phase: they either (1) compared correct and erroneous examples ('contrasting examples', *n* = 41; *n* = 35; *n* = 20); (2) studied correct examples (i.e., step-by-step solutions to unbiased reasoning) and explained why these were right ('correct examples', *n* = 43; *n* = 40; *n* = 21); (3) studied erroneous examples (i.e., step-by-step incorrect solutions including biased reasoning) and explained why these were wrong ('erroneous examples', *n* = 43; *n* = 40; *n* = 18); or (4) solved practice problems ('practice problems', *n* = 43; *n* = 40; *n* = 23). Immediately after the learning phase and after a three-week delay, participants completed a posttest

on learning items (i.e., instructed and practiced during the learning phase) and transfer items (i.e., not instructed and practiced during the learning phase). Additionally, some students took a posttest after a nine-month delay. Further CT-instructions were given in courses in-between the second session of the experiment and the nine-month follow up. Thus, these data were exploratively analyzed and need to be interpreted with caution.

Materials

CT-skills pretest

The pretest consisted of six classic heuristics-and-biases tasks that reflected important aspects of CT, across two categories (see Appendix A for an example of each category): syllogistic reasoning (i.e., logical reasoning) and conjunction (i.e., statistical reasoning) tasks. Three *syllogistic reasoning items* measured students' tendency to be influenced by the believability of a conclusion that is inferred from two premises when evaluating the logical validity of that conclusion (adapted from Evans, 2002). For instance, the conclusion that cigarettes are healthy is logically valid given the premises that all things you can smoke are healthy and that you can smoke cigarettes. Most people, however, indicate that the conclusion is invalid because it does not align with their prior beliefs or real-world knowledge (i.e., belief bias, Evans et al., 1983). Three *conjunction items* examined to what extent the conjunction rule ($P(A\&B) \leq P(B)$) – which states that the probability of multiple specific events both occurring must be lower than the probability of one of these events occurring alone – is neglected (Tversky & Kahneman, 1983). To illustrate, people have the tendency to judge two things with a causal or correlational link, for example advanced age and occurrence of heart attacks, as more probable than one of these on its own.

The posttests consisted of parallel versions (i.e., structurally equivalent but different surface features) of the six pretest items which were instructed and practiced and, thus, served to assess differences in *learning* outcomes. Additionally, the posttests contained six items across two non-practiced categories that served to assess differences in *transfer* performance (see Appendix A for an example of each category). Three *Wason selection items* measured students' tendency to disprove a hypothesis by verifying rules rather than falsifying them (i.e., confirmation bias, adapted from Stanovich, 2011). Three *base-rate items* examined students' tendency to incorrectly judge the likelihood of individual-case evidence (e.g., from personal experience, a single case, or prior beliefs) by not considering all relevant statistical information (i.e., base-rate neglect, adapted from Fong et al., 1986; Stanovich & West, 2000; Stanovich et al., 2016; Tversky & Kahneman, 1974). These transfer items shared similar features with the learning categories, namely, one category requiring knowledge and rules of logic (i.e., Wason selection tasks can be solved by applying syllogism rules) and one category requiring

knowledge and rules of statistics (i.e., base-rate tasks can be solved by appropriate probability and data interpretation).

The cover stories of all test items were adapted to the participants' study domain. A multiple-choice (MC) format with different numbers of alternatives per item was used, with only one correct alternative for each task. One point was assigned for each correct answer (see data-analysis subsection), resulting in a maximum total score of six points on the pretest and six points on the posttests.

CT-instructions

All participants received a 12 minutes video-based instruction that started with emphasizing the importance of CT in general, describing the features of CT, and explaining which skills and attitudes are needed to think critically. Thereafter, explicit instructions on how to avoid biases in syllogistic reasoning and conjunction fallacies followed, consisting of two worked examples that showed the correct line of reasoning. The purpose of these explicit instructions was to provide students with knowledge on CT and to allow them to mentally correct initially incorrect responses on the tasks seen in the pretest.

CT-practice

Participants performed practice activities on the task categories that they were given instructions on (i.e., syllogistic reasoning and conjunction tasks). The CT-practice consisted of four practice tasks, two of each of the task categories. Each practice task was again adapted to the study domain and started with the problem statement (see Appendix B for an example of a practice task of each condition). Participants in the *correct examples* condition were provided with a fictitious student's correct solution and explanation to the problem, including auxiliary representations, and were prompted to explain why the solution steps were correct. Participants in the *erroneous examples* condition received a fictitious student's erroneous solution to the problem, again including auxiliary representations. They were prompted to indicate the erroneous solution step and to provide the correct solution themselves. In the *contrasting examples*, participants were provided fictitious students' correct and erroneous solutions to the problem and were prompted to compare the two solutions and to indicate the erroneous solution and the erroneous solution step. Participants in the *practice problems* condition had to solve the problems themselves, that is, they were instructed to choose the best answer option and were asked to explain how the answer was obtained. Participants in all conditions were asked to read the practice tasks thoroughly.

Mental effort

After each test item and practice-task, participants were asked to report how much effort they invested in completing that task/item on a 9-point subjective rating scale ranging from (a) very, very low effort to (9) very, very high effort (Paas, 1992). This widely used scale in educational research (for overviews, see Paas et al., 2003b; Van Gog & Paas, 2008), is assumed to reflect the cognitive capacity actually allocated to accommodate the demands imposed by the task or item (Paas et al., 2003a).

Procedure

The study was run during the first two lessons of a mandatory first-year CT-course in two Security and Governance study programs. Participants were not given CT-instructions in between these lessons. They completed the study in a computer classroom at the participants' university with an entire class of students, their teacher, and the experiment leader (first author) present. When entering the classroom, participants were instructed to sit down at one of the desks and read an A4-paper containing some general instructions and a link to the computer-based environment (Qualtrics platform). The first experimental session (ca. 90 minutes) began with obtaining consent from all participants. Then, participants filled out a demographic questionnaire and completed the pretest. Next, participants entered the learning phase in which they first viewed the video-based CT-instructions and then were assigned to one of the four practice conditions. Immediately after the learning phase, participants completed the immediate posttest. Approximately three weeks later, participants took the delayed posttest (ca. 20 minutes) in their computer classrooms. Additionally, students of the Safety and Security Management program took the nine-month delayed posttest during the first mandatory CT-lesson of their second study year[5], which was exactly the same as the three-week delayed posttest. During all experimental sessions, participants could work at their own pace and were allowed to use scrap paper. Time-on-task was logged during all phase and participants had to indicate after each test item and practice-task how much effort they invested. Participants had to wait (in silence) until the last participants had finished before they were allowed to leave the classroom.

Data analysis

All test items were MC-only questions, except for one learning item with only two alternatives (conjunction item) that was a MC-plus-motivation question to prevent participants from guessing. Items were scored for accuracy, that is, unbiased reasoning; 1 point for each correct alternative on the MC-only questions and a maximum of 1 point

[5] Due to practical reasons, students of the Public Administration program were not administered to the nine-month delayed posttest.

(increasing in steps of 0.5) for the correct explanation for the MC-plus-motivation question using a coding scheme that can be found on our OSF-page. Because two transfer items (i.e., one Wason selection task and one base-rate item) appeared to substantially reduce the reliability of the transfer performance measure, presumably as a result of low variance due to floor effects, we decided to omit these items from our analyses. As a result, participants could attain a maximum total score of 6 on the learning items and a maximum score of 4 on the transfer items. For comparability, learning and transfer outcomes were computed as percentage correct scores instead of total scores. Participants' explanations on the open questions of the tests were coded by one rater and another rater (the first author) coded 25% of the explanations of the immediate posttest. Intra-class correlation coefficients were .990 for the learning test items and .957 for the transfer test items. After the discrepancies were resolved by discussion, the primary rater's codes were used in the analyses.

Cronbach's alpha on the learning items was .21, .42, .58, and .31 on the pretest, immediate posttest, three-week delayed posttest, and nine-month delayed posttest, respectively. The low reliability on the pretest might be explained by the fact that a lack of prior knowledge requires guessing of answers. As such, inter-item correlations are low, resulting in a low Cronbach's alpha. Cronbach's alpha on the transfer items was .31, .12, and .29 on the immediate, three-week delayed, and nine-month delayed posttest, respectively. However, caution is required in interpreting these values because sample sizes as in studies like this do not seem to produce sufficiently precise alpha coefficients (e.g., Charter, 2003). There was no significant difference on pretest performance between participants who stayed in the study and those who dropped out after the first session, $t(181) = -0.37$, $p = .711$, and those who dropped out after the second session $t(181) = 0.14$, $p = .890$.

Additionally, the explanations given during learning were coded for explicit relations to the principles that were communicated in the instructions (i.e., principle-based explanations; Renkl, 2014). In each condition, participants could attain a maximum score of 2 points (increasing in steps of 0.5) for the correct answer on each problem, resulting in a maximum total score of 8. Participants' explanations were coded by the first author and another rater independently coded 25% of the explanations. Intra-class correlation coefficients were .941, .946, and .977 for performance in the correct examples, erroneous examples, and practice problems conditions respectively (contrasting examples consisted of MC-only questions). After a discussion between the raters about the discrepancies, the primary rater's codes were updated and used in the exploratory analyses.

Results

For all analyses in this paper, a *p*-value of .05 was used a threshold for statistical significance. Partial eta-squared (η_p^2) is reported as an effect size for all ANOVAs (see Table 2) with $\eta_p^2 = .01$, $\eta_p^2 = .06$, and $\eta_p^2 = .14$ denoting small, medium, and large effects, respectively (Cohen, 1988). Cramer's *V* is reported as an effect size for chi-square tests with (having 2 degrees of freedom) *V* = .07, *V* = .21, and *V* = .35 denoting small, medium, and large effects, respectively.

Preliminary analyses

Check on condition equivalence

Before running any of the main analyses, we checked our conditions on equivalence. Preliminary analyses confirmed that there were no a-priori differences between the conditions in educational background, $\chi^2(15) = 15.57$, $p = .411$, $V = .18$; gender, $\chi^2(3) = 1.21$, $p = .750$, $V = .08$; performance on the pretest, $F(3, 165) = 0.42$, $p = .739$, $\eta_p^2 = .01$; time spent on the pretest, $F(3, 165) = 0.16$, $p = .926$, $\eta^2 < .01$; and mental effort invested on the pretest, $F(3, 165) = 0.80$, $p = .498$, $\eta^2 = .01$.

Check on time-on-task

The Levene's test for equality of variances was significant, $F(3, 166) = 9.57$, $p < .001$. Therefore, a Brown-Forsythe one-way ANOVA was conducted. This analysis revealed a significant time-on-task (in seconds) difference between the conditions during practice, $F(3, 120.28) = 16.19$, $p < .001$, $\eta^2 = .22$. Pairwise comparisons showed that: erroneous examples ($M = 862.79$, $SD = 422.43$) = correct examples ($M = 839.58$, $SD = 298.33$) > contrasting examples ($M = 512.29$, $SD = 130.21$) = practice problems ($M = 500.41$, $SD = 130.21$), all *p*'s < .001. This should be considered when interpreting the results on effort and posttest performance.

Main analyses

Descriptive and test statistics are presented in Table 1 and 2, respectively[6].

Performance during learning

As each condition received different prompts during learning, performance during learning could not be meaningfully compared between conditions and, therefore, we

[6] We also exploratively analyzed invested mental effort and time-on-task data on the posttest; however, these analyses did not have much added value for this paper and, therefore, are not reported here but will be provided on our OSF-project page.

decided to report descriptive statistics only to describe the level of performance during the learning phase per condition (see Table 1). Descriptive statistics showed that participants earned more than half of the maximum total score on explanations while studying correct examples or engaging in contrasting examples. Participants who studied erroneous examples or solved practice problems performed worse during learning.

Mental effort during learning

A one-way ANOVA revealed a significant main effect of Practice Type on mental effort invested in the practice tasks. Contrary to hypothesis 1, a Tukey post hoc test revealed that participants who solved practice problems invested significantly less effort (M = 4.28, SD = 1.11) than participants who engaged in contrasting examples (M = 5.08, SD = 1.29, p = .022) or studied erroneous examples (M = 5.17, SD = 1.19, p = .008). There were no other significant differences in effort investment between conditions.

Test performance

The data on learning items were analyzed with two 2×4 mixed ANOVAs with Test Moment (pretest and immediate posttest / immediate posttest and three-week delayed posttest) as within-subjects factor and Practice Type (correct examples, erroneous examples, contrasting examples, and practice problems) as between-subjects factor. Because transfer items were not included in the pretest, the data on transfer items were analyzed by a 2×4 mixed ANOVA with Test Moment (immediate posttest and three-week delayed posttest) as within-subjects factor and Practice Type (correct examples, erroneous examples, contrasting examples, and practice problems) as between-subjects factor.

Performance on learning items

In line with Hypothesis 2, the pretest-immediate posttest analysis showed a main effect of Test Moment on performance on learning items: participants' performance improved from pretest (M = 27.26, SE = 1.43) to immediate posttest (M = 49.98, SE = 1.87). In contrast to Hypothesis 3, the results did not reveal a main effect of Practice Type, nor an interaction between Practice Type and Test Moment. The second analysis (N = 154) – to test whether effects are still present after three weeks – showed a main effect of Test Moment: participants performed better on the delayed posttest (M = 55.54, SE = 2.16) compared to the immediate posttest (M = 50.95, SE = 2.00). Again, contrary to our hypothesis, there was no main effect of Practice Type, nor an interaction between Practice Type and Test Moment.

Performance on transfer items

The results revealed no main effect of Test Moment. Moreover, in contrast to Hypothesis 4, the results did not reveal a main effect of Practice Type, nor an interaction between Practice Type and Test Moment.

Table 1. Means (*SD*) of Performance during learning (1–8), Mental effort during learning (1–9), Performance on learning items and transfer items (% correct score) per Instructional condition

	Instructional conditions			
	Contrasting examples	Correct examples	Erroneous examples	Practice Problems
Performance during learning	5..05 (1.84)	5.14 (2.33)	3.50 (2.07)	3.00 (1.50)
Mental effort during learning	5.08 (1.29)	4.98 (1.45)	5.17 (1.19)	4.28 (1.11)
Performance on learning items				
Pretest	25.81 (19.53)	25.97 (18.38)	27.91 (19.83)	29.46 (16.90)
Immediate posttest	47.56 (24.45)	50.58 (25.48)	46.90 (22.42)	56.81 (24.20)
Immediate posttest	48.57 (26.16)	51.50 (25.35)	47.50 (23.05)	56.25 (24.37)
Three-week delayed posttest	52.62 (27.84)	55.77 (26.08)	51.88 (27.25)	61.88 (25.87)
Three-week delayed posttest	44.58 (22.83)	51.98 (20.90)	56.48 (26.44)	60.14 (26.47)
Nine-month delayed posttest	60.00 (27.52)	67.86 (14.02)	59.26 (19.15)	64.86 (18.11)
Performance on transfer items				
Immediate posttest	15.95 (16.09)	20.63 (15.79)	17.71 (15.24)	17.71 (12.40)
Three-week delayed posttest	21.43 (15.30)	21.46 (14.97)	16.25 (13.33)	22.08 (16.18)
Three-week delayed posttest	19.30 (15.23)	19.84 (14.55)	15.28 (13.48)	22.57 (14.22)
Nine-month delayed posttest	25.88 (16.41)	26.59 (14.58)	20.83 (14.64)	26.04 (14.60)

Table 2. Results mixed ANOVAs on Mental effort during learning and Test performance

	N	ANOVA	F-test (df)	*p**	η_p^2
Mental effort during learning		Practice-type	4.37 (3, 168)	.005*	.07
Performance on learning items					
Pretest – immediate posttest	170	Test moment	126.48 (1,166)	<.001*	.43
		Practice type	1.05 (3,166)	.373	.02
		Test moment × Practice type	0.64 (3,166)	.592	.01
Immediate – three-week delayed posttest	154	Test moment	8.58 (1,150)	.004*	.05
		Practice type	1.24 (3,150)	.300	.02
		Test moment × Practice type	0.05 (3,150)	.984	.00
Three-week – nine-month delayed posttest	82	Test moment	21.36 (1,78)	<.001*	.22
		Practice type	0.97 (3,78)	.412	.04
		Test moment × Practice type	2.69 (3,78)	.052	.09
Performance on transfer items					
Immediate – three-week delayed posttest	155	Test moment	3.20 (1,151)	.076	.02
		Practice type	0.76 (3,151)	.520	.02
		Test moment × Practice type	1.52 (3,151)	.211	.03
Three-week – nine-month delayed posttest	82	Test moment	9.53 (1,78)	.003*	.11
		Practice type	0.98 (3,78)	.409	.04
		Test moment × Practice type	0.19 (3,78)	.901	.01

*$p < .05$

Exploratory analyses

Participants from one of the study programs were tested again after a nine-month delay. Regarding performance on learning items, a 2×4 mixed ANOVA with Test Moment (three-week delayed posttest or nine-month delayed posttest) as within-subjects factor and Practice Type (correct examples, erroneous examples, contrasting examples, and practice problems) as between-subjects factor revealed a main effect of Test Moment (see Table 1): participants' performance improved from three-week delayed posttest (*M* = 53.30, *SE* = 2.69) to nine-month delayed posttest (*M* = 63.00, *SE* = 2.24). The results did not reveal a main effect of Practice Type, nor an interaction between Practice Type and Test Moment.

Regarding performance on transfer items, a 2×4 mixed ANOVA with Test Moment (three-week delayed posttest and nine-month delayed posttest) as within-subjects factor and Practice Type (correct examples, erroneous examples, contrasting examples, and practice problems) as between-subjects factor revealed a main effect of Test Moment (see Table 1): participants performed lower on the three-week delayed test (*M* = 19.25, *SE* = 1.60) than the nine-month delayed test (*M* = 24.84, *SE* = 1.67). The results did not

reveal a main effect of Practice Type, nor an interaction between Practice Type and Test Moment.

Discussion

Previous research has demonstrated that providing students with explicit instructions combined with practice on domain-relevant tasks was beneficial for learning to reason in an unbiased manner (Heijltjes et al., 2014a, 2014b, 2015), and that practice consisting of worked example study was more effective for novices' learning than practice problem solving (Chapter 3). However, this was not sufficient to establish transfer to novel tasks. With the present study, we aimed to find out whether contrasting examples – which has been proven effective for promoting transfer in other learning domains – would promote learning and transfer of reasoning skills.

Findings and implications

Our results corroborate the finding of previous studies (e.g., Heijltjes et al, 2015; Van Peppen et al., 2018) that providing students with explicit instructions and practice activities is effective for learning to avoid biased reasoning (Hypothesis 1), since we found pretest to immediate posttest gains on practiced items. Moreover, our results revealed that participants' performance improved even further after a three-week and a nine-month delay, although the latter finding could also be attributed to the further instructions that were given in courses in-between the three-week and nine-month follow up. In contrast to our expectations, we did not find any differences among conditions on either learning or transfer (Hypothesis 3). It is surprising that the present study did not reveal a beneficial effect of studying correct examples as opposed to practicing with problems, as this worked example effect has been demonstrated with many different tasks (Renkl, 2014; Van Gog et al., 2019), including heuristics-and-biases tasks (Chapter 3).

Given that most studies on the worked example effect use pure practice conditions or give minimal instructions prior to practice (e.g., Van Gog et al., 2019), whereas the current study was preceded by instructions including two worked examples, one might wonder whether this contributed to the lack of effect. However, these instructions were also provided in a previous study in which a worked example effect was found in two experiments (Chapter 3). A major difference between that prior study and this one, however, is that in the present study, participants were prompted to self-explain while studying examples or solving practice problems. Prompting self-explanations, however, seems to encourage students to engage in deep processing during learning (Chi et al.,

1994), especially for students with sufficient prior knowledge (Renkl & Atkinson, 2010). In the present study, this might have interfered with the usual worked-example effect. However, the quality of the self-explanations was higher in the correct example condition than in the problem-solving condition, making the absence of a worked example effect even more remarkable.

Another potential explanation might lie in the number of practice tasks, which differed between the prior study (nine tasks: Chapter 3) and present study (four tasks), and which might moderate the effect of worked examples. The mean scores on the pretests as well as the performance progress in the practice problem condition was comparable with the previous study, but the progress of the worked example condition was considerably smaller. As it is crucial for a worked example effect that the worked-out solution procedures are understood, it might be that the effect did not emerge in the present study because participants did not get sufficient worked examples during practice.

This might perhaps also explain why contrasting examples did not benefit learning or transfer in the present study. Possibly, students first need to gain a better understanding of the subject matter with heuristics-and-biases tasks before they are able to benefit from aligning the examples (Rittle-Johnson et al., 2009). Potentially, having contrasting examples preceded by a more extensive instruction phase to guarantee a better understanding of logical and statistical reasoning would enhance learning and establish transfer. Another possibility would be to provide more guidance in the contrasting examples, as has been done in previous studies by explicitly marking the erroneous examples as incorrect and prompting students to reflect or elaborate on the examples (e.g., Durkin & Rittle-Johnson, 2012; Loibl & Leuders, 2018, 2019). It should be noted though, that the lower time on task in the contrasting condition might also be indicative of a motivational problem; whereas the side-by-side presentation was intended to encourage deep processing, it might have had the opposite effect that students might have engaged in superficial processing, just scanning to see where differences in the examples lay, without thinking much about the underlying principles. It would be interesting in future research to manipulate knowledge gained during instruction to investigate whether prior knowledge indeed moderates the effect of contrasting examples and to examine the interplay between contrasting examples, reflection/elaboration prompts, and final test performance.

The present study raises further questions about how transfer of CT-skills can be promoted. Although several studies have shown that to enhance transfer of knowledge or skills, instructional strategies should contribute to storage strength by effortful learning conditions that trigger active and deep processing (*desirable difficulties*; e.g., Bjork & Bjork, 2011), the present study – once again (Chapter 3; Heijltjes et al., 2014a, 2014b,

2015; Van Peppen et al., 2018) – showed that this may not apply to transfer of CT-skills. This lack of transfer could lie in inadequate recall of the acquired knowledge, recognition that the acquired knowledge is relevant to the new task, and/or the ability to actually map that knowledge onto the new task (Barnett & Ceci, 2002). Following this, a further study should elucidate what the underlying mechanism(s) is/are to shed more light on how to promote transfer of CT-skills.

Limitations and strengths

One limitation of this study is that our measures showed low levels of reliability. Under these circumstances, the probability of detecting a significant effect – given one exists – are low (e.g., Cleary et al., 1970; Rogers & Hopkins, 1988), and subsequently, the chance that Type 2 errors have occurred in the current study is relatively high. In our study, the low levels of reliability can probably be explained by the multidimensional nature of the CT-test, that is, it represents multiple constructs that do not correlate with each other. Future research would need to find ways to improve CT measures (i.e., decrease measurement error), for instance by narrowing down the test into a single measurable construct, or should utilize measures known to have acceptable levels of reliability (LeBel & Paunonen, 2011). The latter option seems challenging, however, as multiple studies report rather low levels of reliability of tests consisting of heuristics-and-biases tasks (Aczel et al., 2015b; West et al., 2008) and revealed concerns with the reliability of widely used standardized CT tests, particularly with regard to subscales (Bernard et al., 2008; Bondy et al., 2001; Ku, 2009; Leppa, 1997; Liu et al., 2014; Loo & Thorpe, 1999).

A strength of the current study is that it was conducted in a real educational setting as part of an existing critical thinking course. Despite the wealth of worked examples research, classroom studies are relatively rare. Interestingly, (multi-session) classroom studies have also failed to find the worked example effect, although – in contrast to the present study – worked examples often did show clear efficiency benefits compared to practice problems (Van Loon-Hillen et al., 2012; McLaren et al., 2016). In line with our finding, a classroom study by Isotani and colleagues (2011) indicated that (high prior knowledge) students did not benefit more from studying erroneous examples than from correct examples or practice problems.

Conclusion

To conclude, based on the findings of the present study, comparing correct and erroneous examples does not seem to be a promising instructional method to enhance learning and transfer of specific – and specifically tested – CT skills. Consequently, our findings raise questions about the preconditions of contrasting examples effects. Further

research on the exact boundary conditions is therefore recommended. Moreover, this study highlights the difficulty of designing instructions to enhance transfer of CT-skills.

Appendix A. Example items critical thinking tests

Below we translated an example item of each task category administered in the critical thinking tests and the explanation.

Syllogistic reasoning item

Below, you will find two premises that you must assume are true. Indicate whether the conclusion follows logically from the given premises.

Premise 1. No safety instrument leads to a decrease in incidents.
Premise 2. Some risk inventories and evaluations (RIE's) lead to a decrease in incidents.
Conclusion: Some RIE's are no safety instruments.

a) Conclusion follow logically from the premises
b) Conclusion does not follow logically from the premises

Explain briefly why you chose this answer:

Correct answer: a

Explanation: This assignment requires that participants do not confuse logical validity of the conclusion with the believability of the conclusion. The conclusion is (presumably) unbelievable for participants due to their prior knowledge or real-world knowledge (RIE's are well-known safety instruments in the domain of Safety and Security). For more information, see Evans (2002).

Conjunction item

The Dutch national police have investigated crime in the major cities of the Netherlands. The city Rotterdam was part of the research and was selected by chance from the list of cities. Which of the following statements is most likely? (Choose one answer).

a) The Rotterdam police had to cut off staff and the number of street robberies in Rotterdam has increased.
b) The number of street robberies in Rotterdam has increased.

Explain briefly why you chose this answer:

Correct answer: a

Explanation: This assignment requires that participants do not violate the conjunction rule that states that the probability of a conjunction cannot be more probable that one of its constituents. For more information, see Tversky and Kahneman (1983).

Wason selection item

The Dutch airport Schiphol is keen on a substantial flight expansion. 'No problem at all', the airport claims. But is that true? The additional flights may not produce negative environmental effects. Schiphol has had the following rule for years:

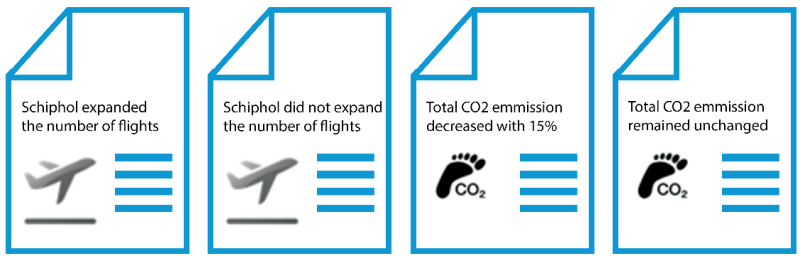
> *If Schiphol increases the number of flights, the total CO_2 emissions from air Traffic must have decreased by 10% compared to the previous years.*

In a presentation about the importance of Schiphol as 'engine of the Dutch economy', it is expertly explained that the aircraft of today fly even more economically and that the total CO_2 emissions from air traffic decreased by 20% compared to last year.

Assume that you are an official at the Dutch Ministry of Infrastructure and Environment and – based on the above rule – you have to decide whether Schiphol can realise the intended flight expansion. If the statements from the presentation are true, Schiphol would be allowed to continue the intended expansion. However, there are also indications that the claims of Schiphol are incorrect and that the airport has attenuated the negative environmental effects of air traffic. You decide to have an independent researcher chart whether Schiphol has complied with the rule in the past. The researcher investigates four random years and draws up four separate reports. There are only two findings in each report:

1. The total CO_2 emissions from air traffic compared to the previous years did / did not decrease
2. Schiphol expanded / did not expand the number of flights that year

Below you see only one of the two findings from each of the four annual reports. You will have to read the entire report to find the second finding of the year in question. Which annual report(s) should you read to check whether Schiphol has complied with the rule in the past? Choose one or more of the options below, but only choose the options that are necessary to check if Schiphol had complied with the rule.



a b c d

Correct answer: a + d

Explanation: This assignment requires that participants do not only seek to confirm the rule but also look for falsification of the rule. By reading the report with the finding that Schiphol expanded the number of flights, you can test whether the rule is violated: if the total CO_2 emissions have not decreased, the rule is violated. The same for reading the report with the finding that the total CO_2 emission remained unchanged: if that report also states that Schiphol expanded the number of flights, the rule is violated. Because if Schiphol expanded the number of flights, the total CO_2 emissions should have decreased. People who

choose other options than the combination of 'Schiphol expanded the number of flights' + 'the total CO_2 emission remained unchanged' probably fail to apply logical principles, verify rules rather than to falsify them, or demonstrate matching bias by selecting options explicitly mentioned in the conditional statement.

Base-rate item

Imagine you work at the Human Recourses department of a large oil company. The director is considering a reorganisation of the company, but she is afraid that this can have negative consequences for the atmosphere at work. She asks you for advice. You know few reorganisations are successful, that is, in few situations both the company and the employees are satisfied with the end result. You decide to search for success factors and found out that many successful reorganisations are supported by external consultancy.

What information would you want to have in order to estimate the probability that the reorganisation at your company will be successful given that it is supported by external consultancy? Below are four pieces of information that may or may not be relevant for determining the probability. Please indicate what information is needed to make a good estimate of the probability that the reorganisation will proceed successfully when the director consults external consultancy. Choose one or more of the alternatives, but only those that are necessary to make the estimate.

1) Probability of companies that were supported by external consultancy and had a successful reorganisation
2) Marginal probability of successful reorganisations
3) Marginal probability of unsuccessful reorganisations
4) Probability of companies that were supported by external consultancy and had an unsuccessful reorganisation

Correct answer: 1+2+4 or 1+3+4

Explanation: The explanation of this item is illustrated by the crosstab below. You are asked for A/(A+C). People should not overrate the first piece of information (A/total) as this probability only gives information about companies that were supported by external consultancy and had a successful reorganisation. This probability is uninformative for the probability estimation requested if you do not have the fourth piece of information, that is, the probability of companies that were supported by external consultancy and had an unsuccessful reorganisation (C/total). Additionally, you need the second piece of information, that is, the marginal probability of successful reorganisations ((A+B))/total). Since the marginal probability of successful reorganisations could be derived from the marginal probability of unsuccessful reorganisations, you could also choose for the third piece of information ((C+D)total).

	External consultancy		Total
Reorganisation	Yes	No	
Successful	A	B	A+B
Unsuccessful	C	D	C+D
Total	A+C	B+D	A+B+C+D (total)

Appendix B. Example items practice conditions

Below, we translated an example of a practice task of each practice condition.

Example item contrasting examples condition

Tom and Laura were presented with the following assignment. Below the assignment, you can see their step-by-step solution to the problem. Please, study the assignment and the solutions of Tom and Laura carefully.

Assignment 1

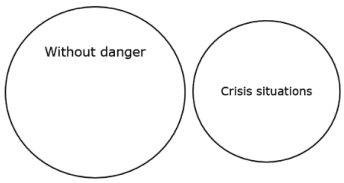
Below, you will find two premises that you must assume are true.
Indicate whether the conclusion follows logically from the given premises.

Premise 1. No crisis situation is without danger.
Premise 2. Some transport accidents are without danger.
Conclusion: Some transport accidents are no crisis situation.

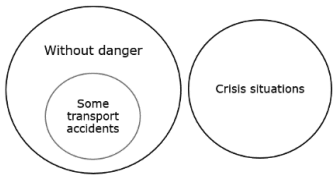
a) The conclusion follows logically from the premises
b) The conclusion does not follow logically from the premise

Tom's solution

Step 1. According to premise 1, no crisis situation is without danger and therefore the circle crisis situations should be placed outside the circle without danger.

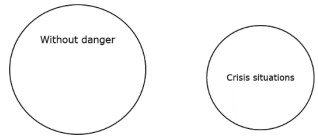


Step 2. According to statement 2, some transport accidents are without danger and therefore the circle some transport accidents should be placed within the circle without danger.

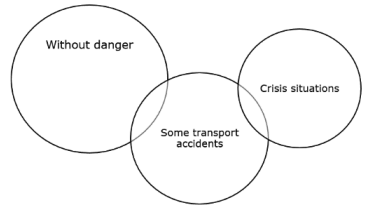


Laura's solution

Step 1. According to premise 1, no crisis situation is without danger and therefore the circle crisis situations should be placed outside the circle without danger.



Step 2. According to statement 2, some transport accidents are without danger.



Step 3. There is no overlap between some transport accidents transport and crisis situations. Therefore, I can say with certainty that some transport accidents are no crisis situation and I choose answer a.

a) The conclusion follows logically from the premises

b) The conclusion does not follow logically from the premises

Step 3. The fact that some transport accidents are without danger does not mean that some other transport accidents are no crisis situations. You cannot know for sure whether some transport accidents are no crisis situation and therefore I choose answer b.

a) The conclusion follows logically from the premises

b) **The conclusion does not follow logically from the premises**

Compare the solutions of Tom and Laura. Which solution is incorrect?

- o Tom's solution is incorrect
- o Laura's solution is incorrect

At which step the solution is incorrect?

- o Step 1
- o Step 2
- o Step 3

Example item correct examples condition

Tom was presented with the following assignment. Below the assignment, you can see his step-by-step solution to the problem. Please, study the assignment and Tom's solution carefully.

Assignment 1

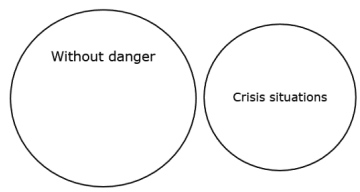
Below, you will find two premises that you must assume are true.
Indicate whether the conclusion follows logically from the given premises.

Premise 1. No crisis situation is without danger.
Premise 2. Some transport accidents are without danger.
Conclusion: Some transport accidents are no crisis situation.

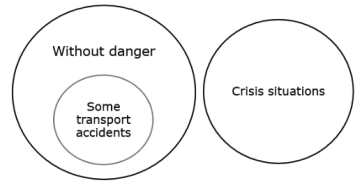
a) The conclusion follows logically from the premises
b) The conclusion does not follow logically from the premises

Tom's solution

Step 1. According to premise 1, no crisis situation is without danger and therefore the circle crisis situations should be placed outside the circle without danger.



Step 2. According to statement 2, some transport accidents are without danger and therefore the circle some transport accidents should be placed within the circle without danger.



Step 3. There is no overlap between some transport accidents and crisis situations. Therefore, I can say with certainty that some transport accidents are no crisis situation and I choose answer a.

a) **The conclusion follows logically from the premises**
b) The conclusion does not follow logically from the premises

Tom's solution is correct. Indicate for each step what is correct about his reasoning:

Step 1 ☐
Step 2 ☐
Step 3 ☐

Example item erroneous examples condition

Laura was presented with the following assignment. Below the assignment, you can see her step-by-step solution to the problem. Please, study the assignment and Laura's solution carefully.

Assignment 1

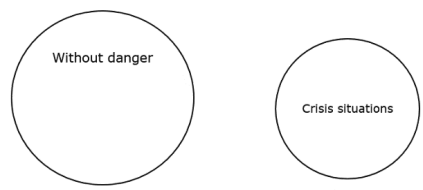
Below, you will find two premises that you must assume are true.
Indicate whether the conclusion follows logically from the given premises.

Premise 1. No crisis situation is without danger.
Premise 2. Some transport accidents are without danger.
Conclusion: Some transport accidents are no crisis situation.

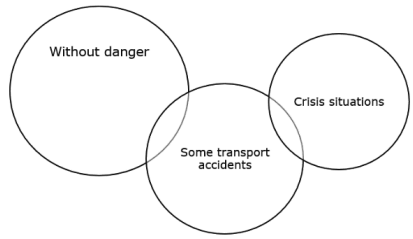
a) The conclusion follows logically from the premises
b) The conclusion does not follow logically from the premises

Laura's solution

Step 1. According to premise 1, no crisis situation is without danger and therefore the circle crisis situations should be placed outside the circle without danger.



Step 2. According to statement 2, some transport accidents are without danger.



Step 3. The fact that some transport accidents are without danger does not mean that some other transport accidents are no crisis situations. You cannot know for sure whether some transport accidents are no crisis situation and therefore I choose answer b.

a) The conclusion follows logically from the premises
b) **The conclusion does not follow logically from the premises**

Laura's solution is incorrect. At which step the solution is incorrect? What would be thecorrect reasoning?

- o Step 1
- o Step 2
- o Step 3

Example item practice problems condition

Below you will find assignment, followed by two answer options. Please study the assignment carefully and choose the best answer option.

Assignment 1

Below, you will find two premises that you must assume are true.
Indicate whether the conclusion follows logically from the given premises.

Premise 1. No crisis situation is without danger.
Premise 2. Some transport accidents are without danger.
Conclusion: Some transport accidents are no crisis situation.

- o The conclusion follows logically from the premises
- o The conclusion does not follow logically from the premises

Explain briefly why you chose this answer:

Chapter 5

Repeated retrieval practice to foster students' critical thinking skills

This chapter has been submitted as:
Van Peppen, L. M., Verkoeijen, P. P. J. L., Heijltjes, A. E. G., Janssen, E. M., & Van Gog, T. (submitted). *Repeated retrieval practice to foster students' critical thinking skills.*

Abstract

There is a need for effective methods to teach critical thinking (CT). Many studies on other skills have demonstrated beneficial effects of distributed practice that induces retrieval processes (repeated retrieval practice). The present experiment investigated whether repeated retrieval practice is effective for fostering CT-skills, focusing on avoiding biased reasoning. Seventy-five students were instructed on CT and avoiding belief-bias in syllogistic reasoning and practiced with syllogisms. Depending on assigned condition, they (1) did not engage in extra practice; (2) practiced a second time (week later); or (3) practiced a second (week later) and third time (two weeks after second time). Performance on practiced (learning) and non-practiced (transfer) tasks was measured on a pretest and posttest (two/three days after last practice-session). Results revealed no significant difference between pretest and posttest learning performance as judged by total performance (MC-answers + justification), but this comparable level of posttest-performance was attained in less time than pretest-performance. So, surprisingly, although performance during practice numerically improved with more repetitions, repeated retrieval did not lead to better test performance. Exploring performance on MC-answers-only suggested that students did benefit from instruction/practice but may have been unable to justify their answers. Unfortunately, we were unable to test effects on transfer due to a floor effect. Further research should focus on determining the preconditions of repeated retrieval practice effects for this type of tasks. To the best of our knowledge, this is the first study that addresses repeated retrieval practice effects in the CT-domain. Findings highlight the difficulty of establishing transfer of CT-skills.

Introduction

One of the most valued and sought after skills that higher education students are expected to learn is critical thinking (CT). CT is key to effective thinking about difficult issues, weighing evidence, determining credibility, and acting rationally, which is essential for succeeding in future careers and to be efficacious citizens (Billings & Roberts, 2014; Davies, 2013; Halpern, 2014; Van Gelder, 2005). The concept of CT can be expressed in a variety of definitions, but at its core, CT is "good thinking that is well reasoned and well supported with evidence" (Butler & Halpern, 2020, p. 152). One key aspect of CT is the ability to avoid biases in reasoning and decision-making (e.g., West et al., 2008), referred to as *unbiased reasoning*. Bias is said to occur when people rely on heuristics (i.e., mental shortcuts) during reasoning prior to choosing actions and estimating probabilities that result in systematic deviations from ideal normative standards (i.e., derived from logic and probability theory: Stanovich et al, 2016; Tversky & Kahneman, 1974). As biased reasoning can have serious consequences in both daily life and complex professional environments, it is essential to teach CT in higher education (e.g., Koehler et al., 2002).

Not surprisingly, therefore, there is a growing body of literature on how to teach CT, including unbiased reasoning (e.g., Abrami et al., 2014; Heijltjes et al. 2014a, 2014b, 2015; Janssen et al., 2019a, 2019b; Kuhn, 2005; Sternberg, 2001; Van Peppen et al., 2018). It is well established, for instance, that explicit teaching of CT combined with practice improves learning of CT-skills required for unbiased reasoning. Nonetheless, while some effective interventions for learning CT have been identified, it is still unclear which methods are most effective in supporting the ability to transfer what has been learned (Halpern & Butler, 2019; Heijltjes et al., 2014a, 2014b, 2015; Ritchhart & Perkins, 2005; Tiruneh et al., 2014, 2016; Van Peppen et al., 2018). Transfer is the process of applying one's prior knowledge or skills to related materials or some new context (e.g., Barnett & Ceci, 2002; Cormier & Hagman, 2014; Haskell, 2001; Perkins & Salomon, 1992; Salomon & Perkins, 1989). There are some insights into fostering transfer of CT-skills to isomorphic tasks (in this study referred to as learning; e.g., Heijltjes et al., 2014a), but not into transfer to novel tasks that share underlying principles but have not been previously encountered (e.g., Heijltjes et al., 2014a, 2015; Van Peppen et al., 2018). As it is crucial that students can successfully apply the CT-skills acquired at a later time and to novel contexts/problems, more knowledge is needed into the conditions that not only yield learning of CT-skills but also transfer.

Previous research has demonstrated that to establish learning *and* transfer, learners have to develop abstract and rich knowledge structures and have to practice in applying

that knowledge (Bassok & Holyoak 1989; Fiorella & Mayer, 2016; Gick & Holyoak, 1983; Holland et al, 1986; Wittrock, 2010). One of the strongest learning techniques known to promote the construction of well-developed knowledge structures, is having students repeatedly retrieve to-be-learned material from memory, known as repeated retrieval practice (e.g., Dunlosky et al., 2013; Fiorella & Mayer, 2015, 2016; Roediger & Butler, 2011).

Repeated retrieval practice

Ever since the work of Ebbinghaus (1885/1964), it has been established that the more times learning material is presented, the more accurate are its recognition and recall (for a review, see Roediger & Karpicke, 2006). The guiding assumption of several theories on repetition effects is that an extra study opportunity increases the number of retrieval cues encoded with a stimulus' memory trace (Cull, 2000; Hintzman, 2010; Hintzman & Block, 1971; Lansdale & Baguley, 2008; Logan, 1988; McClelland & Chappell, 1998; Melton, 1976, 1970; Murdock et al., 2001). However, presenting learning materials repeatedly may only result in simple memorization (i.e., rote learning; Kintsch, 1994; Mayer, 2002). To establish deep learning, it is essential that study opportunities are distributed over time (spacing) rather than all in immediate succession (i.e., spacing effect; e.g., Benjamin & Tullis, 2010; Toppino & Bloom, 2002; Verkoeijen et al., 2004, 2005). The spacing effect, however, is conditional upon retrieving knowledge from memory, termed as retrieval practice (also referred to as the testing effect: for reviews, see Roediger & Butler, 2011; Rowland, 2014); just restudying to-be learned materials is less effective.

Indeed, many studies have firmly established distributed practice and retrieval practice effects to be extremely robust (for reviews on distributed practice, see Cepeda et al., 2006; Janiszewski et al., 2003; Hintzman, 1976; for reviews on retrieval practice, see Carpenter, 2012; Delaney et al., 2010; Pan & Rickard, 2018; Rickard & Pan, 2017; Roediger & Butler, 2011). The advantages, for both learning and transfer, are evident with different kinds of materials and test formats (Butler, 2010; Carpenter & Kelly, 2012; McDaniel et al., 2012, 2013; Rohrer et al., 2010). Despite the potential of repeated retrieval for learning, its impact has not been investigated in research on CT. Therefore, the present study sought to determine whether repeated retrieval practice is beneficial to foster learning of CT-skills as well, and whether it can additionally facilitate transfer. Findings from cognitive psychology suggest that practicing twice will lead to a large learning gain, with diminishing returns for practicing three times and four times (e.g., Greene, 1989; Rawson & Dunlosky, 2011). While the majority of studies were conducted in laboratory settings, the current study was conducted as part of an existing CT-course using educationally relevant practice sessions (multiple practice tasks within a session)

and retention intervals (days/weeks). To the best of our knowledge, this is the first study that investigated the effects of repeated retrieval practice in the CT-domain.

The present study

Participants first completed a pretest including syllogistic reasoning tasks (for an overview of the study design, see Table 1), which examined their tendency to be influenced by the believability of a conclusion when evaluating the logical validity of arguments. Thereafter, they received instructions on CT in general and on syllogisms in particular. Subsequently, they practiced with these tasks on domain-specific problems. Depending on condition, participants (1) did not engage in extra practice with these tasks (practice once); (2) practiced a second time (one week later; practice twice); or (3) practiced a third time (two weeks after second time; practice thrice). All participants completed a posttest including practiced tasks (i.e., syllogistic reasoning tasks; measure of *learning*) and non-practiced tasks (i.e., Wason selection tasks; measure of *transfer*) two or three days after their last practice session. Participants had to indicate after each test and practice item how much effort they invested on that item and time-on-task was logged during all phases. Furthermore, they were asked after each practice session to assess how well they thought they understood the practice problems (i.e., global judgment of learning; JOL) to gain insight into the added value of extra practice according to the students themselves. Previous research has demonstrated that students' JOLs are related to their learning strategies and study time (i.e., monitoring learning processes; e.g., Koriat, 1997; Nelson et al.,1994; Zimmerman, 2000) and, thus, may indirectly contribute to performance enhancement.

Table 1. Study design per instructional condition

	Week 1		Week 2		Week 3	Week 4	
Practice once	Pretest, Instructions Practice 1	Posttest	Practice 2*	-	-	Practice 3*	-
Practice twice	Pretest, Instructions Practice 1	-	Practice 2	Posttest	-	Practice 3*	-
Practice thrice	Pretest, Instructions Practice 1	-	Practice 2	-	-	Practice 3	Posttest

* Participants of the practice once and practice twice condition practiced with the extra materials after completing the posttest conform the ethical guidelines of the institute of higher professional education (see procedure subsection).

We hypothesized that explicit CT-instructions combined with retrieval practice would be effective for *learning*: thus, we expect pretest to posttest performance gains on learning items in all conditions (Hypothesis 1). Furthermore, we expect that practicing twice would lead to a higher pretest to posttest performance gain on learning items (Hypothesis 2a) and a higher posttest performance on transfer items (Hypothesis 3a) than practicing retrieval once. We expected that retrieval thrice would lead to a higher pretest to posttest performance gain on learning items (Hypothesis 2b) and a higher posttest performance on transfer items (Hypothesis 3b) than practicing retrieval twice. However, as cognitive psychology research has revealed that practicing twice leads to a large learning gain with diminishing returns for more repetitions, we expected these differences to be smaller than the differences between practicing retrieval once and twice.

To get more insight into the effectiveness and efficiency of repeated retrieval practice on learning and transfer, we explored the invested mental effort, time-on-test, and JOLs. Thus, we exploratively compared the practice conditions on invested mental effort on test items, time-on-test, and JOLs.

Method

The hypotheses and complete method section were preregistered on the Open Science Framework (OSF). All data, script files, and materials (in Dutch) are available on the project page that we created for this study (https://osf.io/pfmyg/).

Participants and design

Participants were all first-year 'Safety and Security Management' students attending a Dutch University of Applied Sciences (N = 103). Eleven students did not complete the posttest and two students completed the posttest a week late and therefore were excluded from the analyses (as this may have influenced the results). Seventeen participants were excluded because of non-compliance, i.e., when more than half of the practice tasks during one of the essential practice sessions were not read seriously[7]. Due to a technical problem, one class of students (i.e., 24 students) did not receive the demographic questionnaire and the pretest. Together, this resulted in a final sample of 75 students for the posttest-only analyses (i.e., completed all essential sessions, excluding the demographic questions and pretest) and a subsample of 51 students

[7] Fast readers (i.e., maximum reading speed of 0.17 seconds per word; e.g., Trazettel-Klosinkski & Dietz, 2012), taken as a limit.

(68%) for the pretest to posttest analyses (i.e., completed all essential sessions: M_{age} = 19.47, *SD* = 1.64; 25 female).

We calculated power functions of our analyses using the G*Power software (Faul et al., 2009). The power of our one-way ANOVAs – under a fixed alpha level of .05 and with a sample size of 75 – is estimated at .11, .47, and .87 for picking up a small (η_p^2 = .01), medium (η_p^2 = .06), and large (η_p^2 = .14) effect. Regarding the crucial interaction between number of practice sessions and test moment – again calculated under a fixed alpha level of .05, but with a sample size of 51 and a correlation between measures of .64 – the power is estimated at .27, .95, and >.99 for picking up a small, medium, and large interaction effect, respectively. Thus, our sample size under the above assumptions should be sufficient to pick up medium-large effects, and previous studies on repeated (retrieval) practice mainly demonstrated medium-large effects (e.g., Roediger & Karpicke, 2006).

The educational committee of the university approved on conducting this study within the curriculum. In week 1, all participants first completed the CT-skills pretest, followed by the CT-instructions and practice session one (see Table 1 for an overview). Participants were randomly assigned to one of three conditions. They either (1) did not practice extra with the tasks (practice once condition, posttest only: *n* = 26; both tests: *n* = 16), (2) practiced a second time in week 2 (practice twice condition, *n* = 25; *n* = 16), or (3) practiced a second time in week 2 and a third time in week 4 (practice thrice condition, *n* = 24; *n* = 19). Participants completed the CT-skills posttest two or three days after their last practice session.

Materials

CT-skills tests

The content of the surface features of all items was adapted to participants' study domain. The pretest consisted of 16 syllogistic reasoning items across two categories (i.e., conditional and categorical syllogisms, see the Appendix for an example with explanation of each category), which were used to measure *learning*, as these were instructed and practiced during the training phase. All of the items included a belief bias (i.e., when the conclusion aligns with your prior beliefs or real-world knowledge but is invalid or vice versa; Evans et al., 1983; Markovits & Nantel, 1989; Newstead et al., 1992) and examined the tendency to be influenced by the believability of a conclusion when evaluating the logical validity of arguments (Evans, 2003, 1977). These types of tasks are frequently used to measure people's ability to avoid biases (e.g., Stanovich et al., 2016).

Our tests consisted of 3 × affirming the consequent of a conditional statement (if *p* then *q*, *q* therefore *p*; invalid); 3 × denying the consequent of a conditional statement (if *p* then *q*, not *q* therefore not *p*; valid); 2 × affirming the antecedent of a conditional statement (if *p* then *q*, *p* therefore *q*; valid); 2 × denying the antecedent of a conditional statement (if *p* then *q*, not *p* therefore not *q*; invalid); 3 × categorical syllogism 'no A is B, some C are B, therefore some C are not A' (valid); and 3 × categorical syllogism 'no A is B, some C are B, therefore some A are not C' (invalid). Participants had to indicate for each item whether the conclusion was valid or invalid and to explain their multiple-choice (MC) answer to check their understanding (on the MC-answers they might be guessing). They could earn 1 point for the correct MC-answer and 1 point for a correct and 0.5 point for a partially correct explanation (see data-analysis subsection). The MC and explanation scores were sum-scored and, thus, the maximum total score on the learning items was 32 points.

The posttest was identical to the pretest but, additionally, six Wason selection items were added that measured the tendency to confirm a hypothesis rather than to falsify it (see the Appendix for two examples with explanations; e.g., Dawson et al., 2002, Evans, 2002; Stanovich, 2011). These items measured *transfer* as they were not instructed/practiced but shared similar features with the four types of conditional syllogisms. Our test consisted of 3 abstract versions and 3 versions including study-related context. A MC-format with four answer options was used in which only a specific combination of two selected answers was the correct answer. One point was assigned for each correct answer (see data-analysis subsection), resulting in a maximum total score of six points on the transfer items.

CT-instructions

The video-based CT-instructions (15 min.) consisted of a general CT-instruction (i.e., features of CT and attitudes/skills needed to think critically) and explicit instructions on belief-bias in syllogisms that consisted of a worked example of each of the six types in the pretest. The worked examples showed the correct line of reasoning and included possible problem-solving strategies, which allowed participants to mentally correct initially erroneous responses. At the end, participants received a hint stating that the principles used in these examples can be applied with several other reasoning tasks.

CT-practice

Participants could practice retrieval on the six types of syllogisms on topics that they might encounter in their working-life. Participants were instructed to read the problems thoroughly and to choose the best MC-answer option. After each practice-task, they received correct-answer feedback and were given a worked example in which the line

of reasoning was explained in steps and clarified with a visual representation. The second and third practice sessions were parallel versions of the first one (i.e. structurally equivalent problems but with different surface features).

Mental effort

After each test item and after each CT-practice problem, participants were asked to indicate how much effort they invested on completing that task, on a 9-point scale ranging from (1) very, very low effort to (9) very, very high effort (Paas, 1992).

Global judgments of learning (JOL)

At the end of each practice session, participants made a JOL on how well they thought they understood the CT-practice problems on a 7-point scale ranging from (1) very poorly to (7) very well (Koriat et al., 2002; Thiede et al., 2003).

Procedure

The study was run during the first four weeks of a CT-course in the Integral Safety and Security Management study program of an institute of higher professional education. The CT-skills pretest and first practice session were conducted during the first lesson in a computer classroom at the participants' university with an entire class of students and their teacher present. The extra practice sessions and the posttest were completed entirely online (cf. Heijltjes et al., 2014b). Participants came from four different classes and within each class, students were randomly assigned to one of the conditions. All materials were delivered in a computer-based environment (Qualtrics platform). Participants could work at their own pace, were allowed to use scrap paper while solving the tasks, and time-on-task was logged during all phases.

In advance of the first lesson, the students were informed by their teacher about the experiment (i.e., procedure and time window). When entering the classroom in *week 1*, participants were instructed to sit down at one of the desks and read the A4-paper containing some general instructions and a link to the Qualtrics environment where they first had to sign an informed consent form. Thereafter, they had to fill in a demographic questionnaire and complete the pretest. After each test item, they had to indicate how much mental effort they invested. Subsequently, participants entered the practice phase in which they first viewed the video-based CT-instructions (15 min), followed by the practice tasks. At the end of the practice phase, participants had to indicate their JOL. Participants had to wait (in silence) until the last participant had finished before they were allowed to leave the classroom.

One day before each online session (i.e., practice session 2 and 3 and posttest), participants received an e-mail with a reminder and the request to reserve time for this mandatory part of their CT-course. One hour before participants could start, they received the link to the Qualtrics environment. They were given a specific time window (8 am to 10 pm that day) to complete these sessions. Two or three days after session 1, participants of the practice once condition had to complete the posttest. In the beginning of *week 2*, all participants had to complete the second practice session. Since the content of our materials was part of the final exam of this course and the ethical guidelines of the institute of higher professional education state that all students should have been offered the same exam materials, participants of the practice once condition practiced with the extra practice materials but they were no longer included in the experiment. Two or three days after session 2, participants of the practice twice condition had to complete the posttest. Due to practical reasons (i.e., one-week school holiday), the procedure of week 2 was repeated in *week 4*; all participants had to complete the third practice session but students in the practice once and twice conditions were no longer partaking in the experiment and those in the practice thrice condition had to complete the posttest after three days. Participants who did not complete either the posttest or one of the extra practice sessions received an e-mail the day after the specific time-window with the message that they could complete it that day as a last opportunity.

Data analysis

Items were scored for accuracy; 1 point for each correct MC-alternative and a maximum of 1 point (increasing in steps of 0.5) for the correct explanation on the learning items (coding scheme can be found on our OSF-page). Unfortunately, one transfer item had to be removed from the test due to incorrectly offered MC-answer options. As a result, participants could attain a maximum total score of 32 points on the learning items and five points on the transfer items. For comparability, learning and transfer outcomes were computed as percentage correct scores instead of total scores. Two raters independently scored 25% of the explanations on the learning items of the posttest. Intraclass correlation coefficient (two-way mixed, consistency, single-measures; McGraw & Wong, 1996) was 0.996, indicating excellent interrater reliability (Koo & Li, 2016). The remainder of the tests was scored by one rater. Cronbach's alpha was .74 on the learning items on the pretest, .71 on the learning items on the posttest and .79 on the transfer items.

Boxplots were created to identify outliers (i.e., values that fall more than 1.5 times the interquartile range above the third quartile or below the first quartile) in the data. If any, we first conducted the analyses on the data of all participants and reran the analyses on the data without outliers. If outliers had influence on the results, we reported the data of

both analyses. If not, we only reported the results on the full data set. In case of severe violations of the assumption of normality for our analyses, we conducted appropriate non-parametric tests.

Results

For all analyses in this paper, a p-value of .05 was used as a threshold for statistical significance. Partial eta-squared (η_p^2) is reported as an effect size for all ANOVAs with ηp^2 = .01, η_p^2 = .06, and ηp^2 = .14 denoting small, medium, and large effects, respectively (Cohen, 1988). Cramer's *V* is reported as an effect size for chi-square tests with (having 2 degrees of freedom) *V* = .07, *V* = .21, and *V* = .35 denoting small, medium, and large effects, respectively.

Check on condition equivalence

Before running any of the main analyses, we checked our conditions on equivalence. Preliminary analyses confirmed that there were no a-priori differences between the conditions in age, $F(2, 50) = 0.46$, $p = .634$, $\eta_p^2 = .02$; educational background, $\chi^2(8) = 12.69$, $p = .12$, $V = .35$; performance on the pretest, $F(2, 47) = 0.24$, $p = .790$, $\eta_p^2 = .01$; time spent on the pretest, $F(2, 47) = 0.74$, $p = .481$, $\eta_p^2 = .03$; mental effort invested on the pretest, $F(2, 47) = 0.82$, $p = .445$, $\eta_p^2 = .03$; performance on practice problems session one, $F(2, 74) = 0.12$, $p = .889$, $\eta_p^2 < .01$; time spent on practice problems session one, $F(2, 74) = 0.89$, $p = .417$, $\eta_p^2 = .02$; effort invested on practice problems session one, $F(2, 74) = 0.47$, $p = .629$, $\eta_p^2 = .01$; and global JOL, $F(2, 74) = 0.36$, $p = .701$, $\eta_p^2 = .01$. We found a gender difference between the conditions, $\chi^2(2) = 6.23$, $p = .043$, $V = .35$. However, gender did not correlate significantly with any of our performance measures (minimum $p = .669$) and was therefore not a confounding variable.

Planned analyses

We conducted pretest to posttest analyses on the data of participants who completed all essential experimental sessions ($n = 51$) and posttest-only analyses on the data of participants who missed the demographic questions and pretest ($n = 75$). Because of a floor effect on transfer performance, analysis of the transfer data would unfortunately not be very meaningful, and we therefore report only descriptive statistics on those data. Together with the descriptives of the other dependent variables, these can be found in Table 2.

Performance on learning items

In contrast to Hypotheses 1 and 2a, a 2×3 mixed ANOVA with Test Moment (pretest, posttest) as within-subjects factor and Condition (practice once, practice twice, practice thrice) as between-subjects factor on performance on learning items revealed no main effects of Test Moment, $F(1, 48) = 3.05$, $p = .087$, $\eta_p^2 = .06$, and Condition, $F(2, 48) = 0.24$, $p = .788$, $\eta_p^2 = .01$. Furthermore, there was no interaction between Test Moment and Condition, $F(2, 48) = 0.01$, $p = .991$, $\eta_p^2 < .01$. A one-way ANOVA with the full sample on the posttest data only, did not reveal an effect of Condition either, $F(2, 72) = 0.06$, $p = .945$, $\eta_p^2 < .01$.

Mental effort

A 2×3 mixed ANOVA on invested mental effort on the learning items, with Test Moment (pretest, posttest) as within-subjects factor and Condition (practice once, practice twice, practice thrice) as between-subjects factor showed a main effect of Test Moment, $F(1, 48) = 8.41$, $p = .006$, $\eta_p^2 = .15$; less effort was invested on learning items on the pretest ($M = 3.93$, $SD = 1.24$) than the posttest ($M = 4.32$, $SD = 1.30$). There was no main effect of Condition, $F(2, 48) = 0.67$, $p = .515$, $\eta_p^2 = .03$, nor an interaction between Test Moment and Condition, $F(2, 48) = 0.85$, $p = .435$, $\eta_p^2 = .03$. A one-way ANOVA with the full sample on the posttest data only, did not reveal an effect of Condition either, $F(2, 72) = 0.28$, $p = .754$, $\eta_p^2 = .01$.

Time-on-test

Because the data was not normally distributed, we conducted a Kruskal-Wallis H test with Condition (practice once, practice twice, practice thrice) as between-subjects factor on pretest-posttest differences in time spent on learning items. The results showed that there was no significant difference between conditions in pretest-posttest time spent on learning items, $\chi^2(2) = 1.54$, $p = .464$, $\eta_p^2 = .01$. A Kruskal-Wallis H test on the posttest-only data with Condition (practice once, practice twice, practice thrice) as between-subjects factor, showed that there was no significant difference in time spent on posttest learning items between conditions, $\chi^2(2) = 4.54$, $p = .103$, $\eta_p^2 = .04$. In addition to the results of the analysis on the full data, a 2×3 mixed ANOVA on the data without five outliers with Test Moment (pretest, posttest) as within-subjects factor and Condition (practice once, practice twice, practice thrice) as between-subjects factor did reveal a significant effect of Test Moment, $F(1, 42) = 39.34$, $p < .001$, $\eta_p^2 = .48$; more time was spent on the pretest ($M = 73.84$, $SD = 17.55$) than the posttest ($M = 49.26$, $SD = 21.14$).

Table 2. Means (*SD*) of Test performance on learning items (% correct score), Test performance on transfer items (% correct score), Invested mental effort during test (1–9), Time-on-task during test (in seconds), and Global Judgment of Learning (1-7) after the last practice session per Instructional condition

			Instructional conditions		
		N	Practice once	Practice twice	Practice thrice
Test performance					
Learning items	Pretest	51	43.85 (18.22)	47.36 (21.07)	45.23 (17.25)
	Posttest	51	47.17 (14.46)	51.37 (18.94)	49.01 (14.94)
	Posttest	75	47.06 (15.88)	48.56 (17.85)	47.92 (14.22)
Transfer items	Posttest	75	7.69 (19.66)	5.60 (17.81)	1.67 (8.16)
Mental effort					
Learning items	Pretest	51	4.32 (1.13)	3.65 (1.28)	3.82 (1.27)
	Posttest	51	4.48 (1.25)	4.22 (1.47)	4.22 (1.47)
	Posttest	75	4.64 (1.19)	4.60 (1.19)	4.39 (1.38)
Transfer items	Posttest	75	4.23 (1.43)	4.35 (1.48)	4.01 (1.68)
Time on test					
Learning items[a]	Pretest	46	74.48 (17.13)	75.63 (21.25)	74.43 (18.57)
	Posttest	46	58.82 (31.86)	51.35 (22.85)	45.80 (23.36)
	Posttest	70	59.64 (21.89)	58.82 (26.08)	51.94 (28.50)
Transfer items	Posttest	75	38.04 (19.11)	37.70 (25.47)	29.04 (16.75)
Global JOL		75	4.04 (1.64)	4.76 (1.17)	4.63 (1.47)

[a] Means (SD) of the data excluding outliers.

Global judgments of learning

Finally, we examined differences in global JOLs using a one-way ANOVA. The results revealed no main effect of Condition, $F(2, 74) = 1.82$, $p = .170$, $\eta_p^2 = .05$.

Exploratory analyses

To gain more insight into the effects of repeated retrieval practice, we explored participants' level of performance during practice session one, two, and three[8]. Descriptive statistics showed that on average, performance increased with increasing

[8] This concerns all participants who engaged in the relevant practice sessions (i.e., all conditions in practice session one, practice twice and thrice in session two, and practice thrice in session three).

practice opportunities: mean percentage correct during practice session one was 58.67% (*SD* = 21.29; *n* = 75), during session two 65.31% (*SD* = 19.20; *n* = 49), and during practice three 69.44% (*SD* = 16.79; *n* = 24)[9]. Since the transfer items of the tests shared similar features with the four types of conditional syllogisms, we additionally explored participants' level of performance during learning on these types only. Again, descriptive statistics showed that performance increased: mean percentage correct during practice session one was 55.33% (*SD* = 24.42; *n* = 75), during practice session two 63.78% (*SD* = 25.55; *n* = 49), and during practice session three 69.79% (*SD* = 19.48; *n* = 24).

Additionally, we explored whether performance on MC-questions only on the syllogism (learning) items improved after instruction and practice, using a 2×3 mixed ANOVA with Test Moment (pretest, posttest) as within-subjects factor and Condition (practice once, practice twice, practice thrice) as between-subjects factor. The results indeed revealed a main effect of Test Moment, $F(1, 47) = 20.26$, $p < .001$, $\eta_p^2 = .30$; performance was better on the posttest ($M = 68.66$, $SE = 2.30$) than the pretest ($M = 57.42$, $SE = 2.60$). There was, however, no significant main effect of Condition, $F(2, 47) = 0.50$, $p = .613$, $\eta_p^2 = .02$, nor an interaction between Test Moment and Condition, $F(2, 47) = 0.01$, $p = .990$, $\eta_p^2 < .01$.

Finally, we explored how much time participants spent on the worked-example feedback after correct and incorrect retrievals. Both test and descriptive statistics (see Table 3) showed that participants spent – with almost all practice tasks – more time on the worked-example feedback after incorrect retrievals than after correct retrievals. Although participants generally spent less time on the worked-example feedback as they practiced more often (i.e., during a later practice session), this pattern is found during each of the three practice sessions.

Addressing potential power issues

Due to a technical problem, our final sample was considerably smaller than predetermined and might have been insufficient to detect a small-medium interaction effect. Since adding participants to an already completed experiment will increase the Type 1 rate (alpha) and conducting a second identical experiment (i.e., in the context of an actual course) would be resource-demanding, we decided to exploratory apply whether or not that would be worthwhile, using a sequential stopping rule (SSR: see, for

[9] We additionally tested within the practice thrice condition ($n = 24$) whether there was a significant difference in performance during practice session one, two, and three. Performance increased on average with increasing practice opportunities ($M_1 = 60.42\%$, $M_2 = 65.97\%$, $M_3 = 69.44\%$), but these differences (possibly due to the small sample size) were not significant, $F(2, 46) = 1.94$, $p = .155$, $\eta_p^2 = .08$.

example Arghami & Billard, 1982, 1991; Botella et al., 2006; Doll, 1982; Fitts, 2010; Pocock, 1992; Ximénez & Revuelta, 2007). SSRs make it possible to stop early when statistical significance is unlikely to be achieved with the planned number of participants.

One SSR that is simple, efficient, and appropriate to this experiment is the COAST (composite open adaptive stopping rule; Frick, 1998). The COAST allows to stop testing participants and reject the null hypothesis if the *p*-value is less than a lower criterion of .01; to stop testing participants and retain the null hypothesis if the *p*-value is greater than an upper criterion of .36; and to test more participants if the *p*-value is between these two values. In the present study, the *p*-values of our main analyses (i.e., on performance measures) were obviously larger than the high criterion of .36. Hence, there was no hint of an existing effect of repeated retrieval practice in the present study and, thus, we decided not to add additional participants.

Discussion

The current study investigated whether repeated retrieval practice is beneficial to foster learning of CT-skills and whether it can additionally facilitate transfer. Contrary to our expectations, we did not reveal pretest to posttest performance gains on learning items. Thus, we did not replicate the finding that participants' performance improves after explicit instructions combined with retrieval practice on domain-specific problems (Hypothesis 1: e.g., Heijltjes et al, 2015; Van Peppen et al., 2018). It should be noted, however, that this comparable level of posttest performance was attained in less time than pretest performance (i.e., prior to instruction/practice). Moreover, our exploratory findings on performance on MC-questions only, suggest that students did benefit from instructions and retrieval practice. This difference in outcomes when looking at MC-answers and total scores (i.e., MC + justification) could mean that participants did learn what the right answer was, but may have been unable to justify their answers sufficiently. In that case, however, our intervention only resulted in simple memorization (i.e., rote learning; Mayer, 2002) instead of a deeper understanding of the subject matter. This might perhaps also explain the occurrence of a floor effect on performance on transfer items, as transfer of knowledge or skills depends on how well-developed the knowledge structures are that are formed during initial learning (e.g., Perkins & Salomon, 1992).

In line with previous repeated retrieval findings (e.g., Roediger & Butler, 2011), average performance scores during practice seemed to increase with more repetitions. However, repeated retrieval practice did not have a significant effect – compared to practice once – on performance on the final test (i.e., on learning items; Hypotheses 2a/2b). Unfortunately, we were unable to test whether repeated retrieval practice would enhance

transfer (Hypotheses 3a/3b) due to a floor effect. Because the power of our study was only sufficient to pick up medium-to-large effects of repeated retrieval, it could be that additional retrieval practice had an unidentifiable small effect. In the current study, each practice session consisted of multiple practice tasks (instead of one as in most studies) and it could, therefore, be argued that practice once in this study can already be seen as repeated practice, which possibly explains the absence of substantial effects of repeated retrieval.

Another potential explanation for the lack of effect of additional retrieval practice, might lie in the feedback that was provided after each retrieval attempt. While many studies only show a retrieval practice effect when feedback is provided (for an overview, see Van Gog & Sweller 2015) and others show that elaborative feedback can enhance effects of retrieval practice (e.g., Pan et al., 2016; Pan & Rickard, 2018), findings from recent research suggest that the feedback after each retrieval attempt may have eliminated the repeated retrieval effect (Kliegl et al., 2019; Pastötter & Bäuml, 2016; Storm et al., 2014). According to the bifurcation model (Halamish & Bjork, 2011; Kornell et al., 2011), feedback only strengthens knowledge that is not successfully retrieved, whereas knowledge that is successfully retrieved is hardly affected by subsequent feedback. As such, it may be that participants in the condition that merely practiced once (i.e., lowest performance during practice) processed the feedback better and, therefore, performed equally well on the final test as participants in the other conditions. Moreover, it may be that participants' motivation to learn the correct answer was higher when they were unable to provide the correct answer during retrieval practice than when they were able to do so (e.g., Kang et al., 2009; Potts & Shanks, 2019). Our findings regarding time spent on worked-example feedback after correct/incorrect retrievals support this idea (i.e., more time spent after incorrect than correct retrievals). The possible elimination of a lag effect on learning problem-solving skills by providing feedback after each retrieval attempt is an interesting issue for future research.

Although participants achieved a considerably high level of performance during retrieval practice (approx. 60–70 percent correct), which was comparable to previous studies that did demonstrate beneficial effects of repeated retrieval practice (e.g., Butler, 2010; Roediger & Karpicke, 2006), a floor effect on performance on transfer items had arisen. Since the practice tasks consisted of MC-questions only, this finding again supports the idea that students do benefit from instructions and retrieval practice but may have been unable to justify their answers on the tests sufficiently. Another likely cause for this floor effect may be that participants lacked profound in-depth understanding of the structural overlap between syllogisms and Wason selection tasks (i.e., measure of transfer). During practice, participants could earn one point for each correctly solved syllogism. Each transfer item, however, required recall and application of all four conditional syllogism

principles to solve it correctly and, thus, to earn one point. Future studies on to-be-transferred problem-solving procedures as in the current study, should guarantee sufficient understanding of structural features of tasks and complete recall of the procedure during retrieval practice. It may be helpful to provide more guidance in identifying how tasks are related. Potentially, practicing retrieval until all retrievals are successful and complete might be a solution for complete recall of procedures (i.e., successive relearning: e.g., Bahrick, 1979; Rawson et al., 2013). Given that transfer of CT skills from trained to untrained tasks remains elusive (as our current results also underline), there is an urgent need to determine the exact obstacles to the transfer of CT-skills, which could lie in a failure to recognize that the acquired knowledge is relevant to the new task, inadequate recall of the acquired knowledge, and/or difficulties in actually applying that knowledge onto the new task (i.e., three-step process of transfer; Barnett & Ceci, 2002).

To the best of our knowledge, this is the first study that investigated the effects of repeated retrieval practice in the CT-domain. Moreover, while the majority of research on repeated retrieval practice has been conducted in laboratory settings, the current was conducted as part of an existing CT-course – using educationally relevant practice sessions and retention intervals. As such, it adds to the small body of literature on what instructional designs are (or are not) efficient and effective for CT-courses aiming at learning and transfer of CT-skills, which is relevant for both educational science and educational practice.

Appendix

Below, we translated an example item of each task category administered in the critical thinking tests and the explanation.

Learning tasks

Conditional syllogism

Below, you will find two premises that you must assume are true. Indicate whether the conclusion follows logically from the given premises.

Premise 1. If citizens are involved in improving the safety of a neighborhood, the number of home burglaries decreases.
Premise 2. The number of home burglaries in the Princenhage district has decreased.
Conclusion: In the Princenhage district, citizens are involved in improving the safety of their neighborhood.

a) The conclusion follows logically from the premises
b) The conclusion does not follow logically from the premises

Explain briefly why you chose this answer:

Correct answer: b

Explanation: This assignment requires that participants do not confuse logical validity of the conclusion with the believability of the conclusion. The conclusion is (presumably) believable for participants due to prior beliefs or real-world knowledge. If the first part of premise 1 is met, then the second part automatically follows. The second premise states that the number of home burglaries in the Princenhage district has decreased. However, this does not necessarily mean that it is caused by the involvement of citizens in improving the safety of the neighborhood. There might be another cause. For more information, see Evans (2003).

Categorical syllogism

Below, you will find two premises that you must assume are true.
Indicate whether the conclusion follows logically from the given premises.

Premise 1. No safety instrument leads to decrease in incidents.
Premise 2. Some risk inventories and evaluations (RIE's) lead to a decrease in incidents.
Conclusion: Some RIE's are no safety instruments.

a) The conclusion follows logically from the premises
b) The conclusion does not follow logically from the premises

Correct answer: a

Explanation: This assignment requires that participants do not confuse logical validity of the conclusion with believability of the conclusion. The conclusion is (presumable) unbelievable for participants due to prior beliefs or real-world knowledge (RIE's are well-known safety instruments in the domain of Safety and Security). There is no overlap between some RIE's and a decrease in incidents. For more information, see Evans (2003).

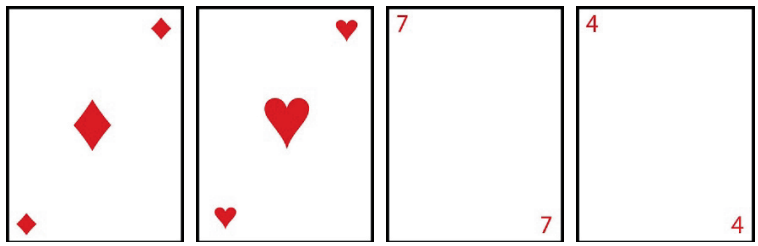
Transfer tasks

Wason selection task (abstract)

Each of the four cards below has an image on one side and a number on the other side. The following rule applies to the cards:

> *If there is a heart on one side, then there is a 7 on the other side.*

Which card(s) should you turn over to check if the rule is violated? Choose one or more of the options below, but only choose the card(s) that is/are necessary to check if the rule is violated.



a b c d

Correct answer: b + d

Explanation: This assignment requires people to not only seek to confirm the rule but also look for falsification of the rule. By turning over the card with a heart, you can test whether the rule is violated: if there is no 7 on the other side, the rule is violated. The same for turning over the card with a 4: if that card has a heart on the other side, the rule is violated. Because if there is a heart on the one side, there should be a 7 on the other side. People who choose other options than the combination of the card with a heart and the card with a 4, verify rules rather than to falsify them, or demonstrate matching bias by selecting options explicitly mentioned in the conditional statement (see Stanovich, 2011).

Wason selection task (study-related context)

Each of the four passports below has a nationality on one side and a surname on the other side. The following rule applies to the passports:

> *If the Dutch nationality is on one side of the passport, the surname Janssen is on the other side of the passport.*

Which passport(s) should you turn over to check if the rule is true? Choose one or more of the options below, but only choose the passport(s) that is/are necessary to decide whether the rule is true.

a b c d

Correct answer: a + d

Explanation: This assignment requires that participants not only seek to confirm the rule but also look for falsification of the rule. By turning over the passport with the Dutch nationality on the one side, you can test whether the rule is violated: if the surname Janssen is not on the other side, the rule is violated. The same for turning over the passport with the surname Peeters on the one side: if that passport also has the Dutch nationality, the rule is violated. Because if the Dutch nationality is on the one side, the surname Janssen should be on the other side. People who choose other options than the combination of 'Dutch' + 'Peeters' probably fail to apply logical principles, verify rules rather than to falsify them, or demonstrate matching bias by selecting options explicitly mentioned in the conditional statement. For more information, see Stanovich (2011).

Chapter 6

Identifying obstacles to transfer of critical thinking skills

This chapter has been submitted as:
Van Peppen, L. M., Van Gog, T., Verkoeijen, P. P. J. L., & Alexander, P. (submitted). *Identifying obstacles to transfer of critical thinking skills.*

Abstract

This study investigated whether unsuccessful transfer of critical thinking (CT) would be due to recognition, recall, or application problems (cf. three-step model of transfer). In two experiments (laboratory: N = 196; classroom: N = 104), students received a CT-skills pretest (including learning, near transfer, and far transfer items), CT-instructions and practice problems, and a CT-skills posttest. On the posttest transfer items, students received no support, received recognition support, were prompted to recall the acquired knowledge, or received recall support. Results showed that CT could be fostered through instruction and practice: we found learning, near transfer, and (albeit small) far transfer performance gains and a reduction in test-taking time. There were no significant differences between conditions, however, suggesting that the difficulty of transfer of CT-skills lies in problems with application/mapping acquired knowledge onto new tasks. Additionally, exploratory results on free recall data suggested suboptimal recall can be a problem as well.

Introduction

Every day, we have to make a multitude of quick but sound judgments and decisions. Since our working-memory capacity and duration are limited and we cannot process all the information around us, we have to resort to heuristics (i.e., mental shortcuts) that ease reasoning processes (Tversky & kahneman, 1974). Usually heuristic reasoning is very functional and inconsequential – think, for example, of where you decide to sit in a train – but it also makes us prone to illogical and biased decisions (i.e., deviating from ideal normative standards derived from logic and probability theory) that can have significant impact. To illustrate, a forensic expert who misjudges fingerprint evidence because it verifies his or her preexisting beliefs concerning the likelihood of the guilt of a defendant, displays the so-called confirmation bias, which can result in a misidentification and a wrongful conviction (e.g., the Madrid bomber case; Kassin et al., 2013).

To reduce or eliminate biased decisions and to successfully function in today's society, one should engage in *critical thinking* (CT: e.g., Dewey, 1910; Pellegrino & Hilton, 2012). In the field of educational assessment and instruction, CT is generally defined as "purposeful, self-regulatory judgment that results in interpretation, analysis, evaluation, and inference, as well as explanation of the evidential, conceptual, methodological, criteriological, or contextual considerations on which that judgment is based" (APA: Facione, 1990, p.2). It is not surprising that educational researchers, practitioners, and policymakers agree that CT is one of the most valued and sought-after skills that higher education students are expected to learn (Davies, 2013; Facione, 1990; Halpern, 2014; Van Gelder, 2005). Consequently, there is a substantial body of research on teaching CT-skills (Abrami et al., 2008, 2014) including reducing biases in reasoning (e.g., Flores et al., 2012; Heijltjes et al., 2014a, 2014b, 2015; Janssen et al., 2019; Kuhn, 2005; Sternberg, 2001; Van Peppen et al., 2018). It is well established, for instance, that explicit teaching of CT combined with practice improves learning of CT-skills required for unbiased reasoning. However, transfer to similar tasks that were not instructed or practiced is very hard to establish (Heijltjes et al. 2014a, 2014b, 2015; Van Peppen et al., 2018). As it would be unfeasible to train students on each and every type of reasoning bias they will ever encounter, there is increased concern as to how to promote *transfer* of these skills (and this also applies to CT-skills more generally, see for example, Halpern, 2014; Kenyon & Beaulac, 2014; Lai, 2011; Ritchhart & Perkins, 2005).

The process of transfer

Transfer is the process of applying one's prior knowledge or skills to some new context or related materials (e.g., Barnett & Ceci, 2002; Cormier & Hagman, 2014; Druckman &

Bjork, 1994; Haskell, 2001; McDaniel, 2007; Perkins & Salomon, 1992). Transfer involves gradients of similarity between the initial and novel situation, so that transfer between situations that have less in common occurs less often than transfer between closely related situations (e.g., Barnett & Ceci, 2002; Dinsmore et al., 2014). In the educational psychology literature, transfer is usually subdivided into near and far transfer, differentiating in degree of similarity between the initial task or situation and the transfer task or situation (e.g., Perkins & Salomon, 1992). Transferring knowledge or skills to a very similar situation, for instance problems in an exam of the same kind as that have been practiced during the lessons, refers to 'near' transfer. By contrast, transferring between situations that share similar structural features but, on appearance, seem remote and alien to one another is considered 'far' transfer. It is important to realize, however, that near and far transfer occur on a continuum and do not imply any precise codification of closeness (Salomon & Perkins, 1989), for instance because people differ considerably in their ability to identify similarities between different problem situations. In their attempt to bring clarity to the literature on transfer of knowledge, Barnett and Ceci (2002) developed a taxonomy in which they conceptualized transfer as a three-step process in which learners need to (a) recognize that acquired knowledge is relevant in a new context, (b) recall that knowledge, and (c) apply that knowledge to the new context.

Previous research has shown that to promote successful (far) transfer of learning, instructional strategies should contribute to permanent changes, by creating effortful learning conditions that trigger active and deep processing (i.e., *generative processing*; e.g., Fiorella & Mayer, 2016; Wittrock, 2010). More specifically, it is important that learners explore similarities and/or differences between different problem types to acquire better mental representations of the structural features of the different types of problems (i.e., schemas; Bassok & Holyoak 1989; Fiorella & Mayer, 2016; Holland et al, 1986; Wittrock, 2010). Ways to stimulate this are, for instance, creating variability in practice (e.g., Barreiros et al., 2007; Moxley, 1979) or encouraging elaboration, questioning, or explanation during practice (e.g., Fiorella & Mayer, 2016; Renkl & Eitel, 2019). Taken together, transfer of learning can occur when a learner acquires an abstract action schema responsive to the requirements of a problem. If the potential transfer situation presents similar requirements and the learner recognizes them, they may apply (or map) the same or a somewhat adapted action schema to solve the novel problem (e.g., Gentner, 1983, 1989; Mayer & Wittrock, 1996; Reed, 1987; Vosniadou & Ortony, 1989).

When interventions that encourage generative processing are applied to CT-skills, however, it is often found that they promote learning but not transfer; the effects hardly seem to transfer across tasks or domains (Halpern & Butler, 2019; Ritchhart & Perkins,

2005; Tiruneh et al., 2014, 2016). Research that focused on teaching unbiased reasoning has uncovered that a combination of instruction and task practice enhances transfer to isomorphic problems, i.e., same structural features/problem type but different superficial features, meaning other values or story contexts; in this study we refer to the ability to solve such problems after instruction as evidence of *learning* (e.g., Heijltjes et al., 2014b). However, it was shown that CT-skills required for unbiased reasoning consistently failed to transfer to novel problem types that have different structural features yet share underlying principles, i.e., far transfer, even when using instructional methods that proved effective for fostering transfer in various other domains. These methods, administered after initial instruction, were encouraging students to self-explain during practice (Heijltjes et al., 2014a, 2014b, 2015; Van Peppen et al., 2018) and offering variable as opposed to blocked practice with examples or problems (i.e., interleaved practice; Chapter 3). Other methods involved comparing correct and erroneous worked out examples (Chapter 4) and repeated retrieval practice (i.e., testing effect; Chapter 5). Additionally, a recent study with teachers who were trained on (teaching) CT in three sessions and engaged in effortful learning activities (i.e., designing a CT-task; Janssen et al., 2019), found no evidence of transfer to novel problems.

These findings raise the question what obstacle(s) underlie(s) the lack of transfer of CT-skills required for unbiased reasoning. According to the three-step process of transfer (Barnett & Ceci, 2002), the lack of transfer in previous studies could lie in a recognition, recall, or application problem. As mentioned above, understanding the obstacle(s) underlying (un)successful transfer is crucial to design courses to achieve it and, moreover, is relevant for theories of learning and transfer.

The present study

In the current study, we therefore investigated different conditions during the final test procedure that support the recognition, recall, and application steps in the transfer process (cf. Butler et al, 2013, 2017, in which a two-step procedure was adopted because recognition was unlikely to be a problem). By comparing the effects of support for different steps in the process, we infer where difficulties arise for learners. We simultaneously conducted two experiments: Experiment 1 in a laboratory setting and Experiment 2 in a classroom setting (i.e., conceptual replication). Participants first completed a pretest and, thereafter, received video-instructions on CT and on specific CT-tasks. Subsequently, they practiced with these tasks on domain-specific problems, followed by correct-answer feedback and a worked example. Finally, participants completed a posttest—including learning (i.e., same problem type but different story contexts), near transfer (i.e., same problem type but offered in a different/less abstract

format), and far transfer (i.e., similar principles but different problem types: see method section for more information) items.

The experimental intervention took place during the posttest. Participants were randomly allocated to one of four conditions, in which they completed the near and far transfer posttest items: (1) without receiving support (no support condition), (2) while receiving hints that the information provided in the learning phase is relevant for these items (recognition support condition), (3) while receiving hints that the information provided in the learning phase is relevant and being prompted to recall the acquired knowledge (free recall condition), or (4) while receiving hints that the information provided in the learning phase is relevant and receiving a reminder of the paper-based overview of that information that they received prior to the transfer tasks (recall support condition).

Table 1 provides a schematic overview of the logic behind the procedure. If the lack of transfer is only due to participants' ability to *recognize* that the acquired knowledge is relevant to the new task, then receiving a hint that the knowledge is relevant should be sufficient to establish transfer. Thus, if inadequate recognition underlies the problem, we expected greater performance gains on transfer items in all conditions compared to the no support condition. (Hypothesis 1: no support < recognition support = free recall = recall support). If, however, participants are able to recognize the relevance but have problems *recalling* the exact rules of logic, then presenting these rules while completing the transfer items would lead to greater performance gains on transfer items than the no support, recognition support, and free recall condition. If participants are not able to recall any of the information, we expected no differences in transfer performance gains between the free recall and recognition support condition (Hypothesis 2a: no support = recognition support = free recall < recall support). But if they can retrieve some of the relevant information, we expected higher transfer performance gains in the free recall condition compared to the recognition support condition (Hypothesis 2b: no support = recognition support < free recall < recall support). If, within the free recall condition, participants' ability to recall the acquired knowledge positively correlates with their performance on transfer items, that would provide further evidence for the assumption that suboptimal recall underlies the lack of transfer. Finally, if difficulties in *applying* the relevant knowledge onto the new task underlie the lack of transfer – while participants are able to recognize that the acquired knowledge is relevant and to recall that knowledge – there would be no differences in transfer performance gains between conditions (Hypothesis 3: no support = recognition support = recall support = free recall).

Table 1. The logic behind the procedure used.

Problem/step in the transfer process	Performance on posttest transfer items
Recognition-only	No support < Recognition support = Free recall = Recall support
Suboptimal recall	No support = Recognition support = Free recall < Recall support *or* No support < Free recall < Recall support Within free recall: positive correlation with retrieved information
Application scaffold	No support = Recognition support = Recall support = Free recall

Experiment 1

Method

The hypotheses, planned analyses, and method section were preregistered on the Open Science Framework (OSF). Detailed descriptions of the design and procedures and all data/script files and materials (in Dutch) are publicly available on the project page we created for this study (osf.io/ybt5g).

Participants

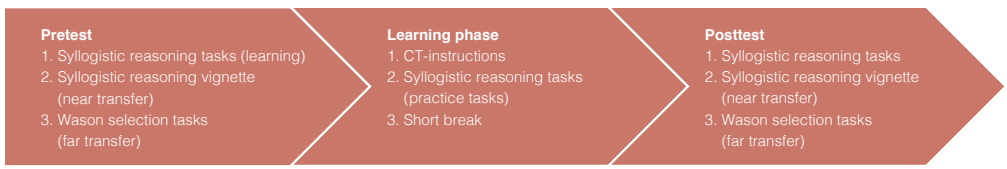
Participants were 196 first-year and second-year Psychology students attending a Dutch University. Of these, two students were unable to complete the free recall due to an experimenter error and six students did not adhere to instructions (i.e., they copied information from the CT-instructions). They were therefore excluded from the analyses and this resulted in a final sample size of 188 students (M_{age} = 20.59, *SD* = 2.53; 69 males). Four students who were originally allocated to the recall support condition did not receive the reminder of the information provided in the learning phase and were therefore automatically assigned to the recognition support condition (i.e., they only received the recognition support).

Based on the sample size of 188 students, a power function for mixed ANOVAs with a single within-subjects factor (two levels) and a single between-subjects factor (four levels) using the G*Power software (Faul et al., 2009), shows that the power of our study – under a fixed alpha level of 0.05 and with a correlation between measures of 0.3 – is estimated at .47, >.99, and >.99 for detecting a small (η_p^2 = .01), medium (η_p^2 = .06), and large (η_p^2 = .14) interaction effect, respectively. Thus, the power of our study should be sufficient to at least pick up medium-sized interaction effects.

Design

The experiment consisted of three phases (see Figure 1 for an overview) and had a 2 (Test Moment: pretest and posttest) × 4 (Condition: no support, recognition support, free recall, recall support) design, with Test Moment as within-subjects factor and Condition as between-subjects factor. Dependent variables were performance on learning, near transfer, and far transfer items. Participants first completed the CT-skills pretest and then received video-based instructions on CT in general and on specific CT-tasks. Subsequently they practiced with these tasks on domain-specific problems, followed by correct-answer feedback and a worked example that showed the correct line of reasoning. After a short break of four minutes, participants completed a posttest including learning, near transfer, and far transfer items (for more information see materials subsection). They started with the learning items and were thereafter randomly allocated to one of four conditions. Depending on assigned condition, they completed the near and far transfer items: (1) without receiving support (no support condition, n = 47), (2) while receiving hints that the information provided in the learning phase is relevant for these items (recognition support condition, n = 55), (3) while receiving hints that the information provided in the learning phase is relevant and being prompted to recall the acquired knowledge (free recall condition, n = 44), or (4) while receiving hints that the information provided in the learning phase is relevant and receiving a reminder of the paper-based overview of that information that they received prior to the transfer tasks (recall support condition, n = 42). Time-on-task was logged during all phases.

Figure 1. Overview of the study design. The four conditions differed in amount of support received while completing the near and far transfer items of the posttest.



Materials

All materials were administered as an online survey with a forced response-format using Qualtrics Survey Software (Qualtrics, Provo, UT; http://www.qualtrics.com).

CT-skills tests

In line with previous research on avoiding bias in reasoning and decision-making, we used several heuristics-and biases tasks as measures of CT (e.g., Stanovich et al., 2016;

Tversky & Kahneman, 1974; West et al., 2008). The CT-skills pretest and posttest addressed three types of tasks in a fixed order: general syllogistic reasoning tasks (i.e., learning items), syllogistic reasoning in vignettes (i.e., near transfer items), and Wason selection tasks (i.e., far transfer items). Example items of each task category are provided in Appendix B. For the sake of comparability, the content of the surface features (cover stories) of all test items was the same for both experiments and was based on the study domain of participants of Experiment 2 (because that experiment was conducted as part of an existing course), namely 'Biology and Medical Laboratory Research' and 'Chemistry'.

Learning items

Each test contained eight conditional syllogistic reasoning items that measured learning (hence, hereafter referred to as learning items), as these were instructed and practiced during the learning phase. All items included a belief bias, that is, when the conclusion aligns with your prior beliefs or real-world knowledge is invalid or vice versa (Evans et al., 1983; Markovits & Nantel, 1989; Newstead et al., 1992) and examined the tendency to be influenced by the believability of a conclusion when evaluating the logical validity of arguments (Evans, 2003; Evans et al., 1983). Conditional syllogisms consist of a premise including a conditional statement and a premise that either affirms or denies either the antecedent or the consequent. Our tests contained 2 × affirming the consequent of a conditional (if *p* then *q*, *q* therefore *p*; invalid but believable); 2 × denying the consequent of a conditional (if *p* then *q*, not *q* therefore not *p*; valid but unbelievable); 2 × affirming the antecedent of a conditional (if *p* then *q*, *p* therefore *q*; valid but unbelievable); and 2 × denying the antecedent of a conditional (if *p* then *q*, not *p* therefore not *q*; invalid but believable). Participants had to indicate for each item whether the conclusion is valid or invalid. Thereafter, they were asked to explain their multiple-choice answer. The forced response-format of these items required them to guess if they did not know the answer.

Near transfer items

For each test, we constructed six short vignettes (about 100 words) to assess whether students are able to evaluate the logical validity of arguments in a written news item or article on a topic that participants might encounter in their working life. Each vignette contained a logically invalid but believable conclusion or a logically valid but unbelievable conclusion from two given premises (i.e., conditional syllogisms). These items reflected near transfer items as they were offered in a different format/situation compared to the learning phase. Participants were instructed to read the text thoroughly, to indicate whether the conclusion in the text is valid or invalid, and to provide an explanation.

Far transfer items

Each test contained six Wason selection items that measured the tendency to confirm a hypothesis rather than to falsify it (adapted from Evans, 2002; Gigerenzer & Hug, 1992). These items reflected far transfer items as they were not explicitly instructed and practiced during the learning phase but shared similar features with the four forms of conditional syllogistic reasoning (i.e., each item required recall and application of all four conditional syllogism principles to solve it correctly). For each of the two forms of Wason selection items (abstract or concrete, with the latter being study-related), there were three test items. A multiple-choice forced-response format with four answer options was used (cf. four forms of conditional syllogistic reasoning) in which only a specific combination of two selected answers was the correct answer. Thereafter, participants were asked to explain their multiple-choice answer. Again, all correct answers were related to reasoning strategies and incorrect answers were related to biased reasoning.

Supporting prompts

Depending on assigned condition, participants received different levels of support while completing the near and far transfer items of the posttest. Participants in the no support condition completed the near and far transfer items without receiving additional support. In the recognition support condition, participants received a prompt that emphasized the relevance of the information provided in the learning phase: “To solve this task, you can use the rules of logic explained in the instructions”. In the free recall condition, participants were first asked to recall the rules of logic explained in the instruction and to write them down on the blank paper they received. Then participants completed each near and far transfer item while receiving the following prompt: “To solve this task, you can use the rules of logic explained in the instructions that you tried to recall beforehand. Take that paper to solve the task.”

In the recall support condition, participants were requested to pick up a paper from the experiment leader and they received a prompt that emphasized the relevance of the information provided in the learning phase and that indicated where they could find this information: “To solve this task, you can use the rules of logic explained in the instructions. You can find these rules in the overview on the paper that you have received. Take that paper to solve the task.” For the detailed description of the supporting prompts and the rules of logic that participants in the recall support condition receive, see Appendix A.

CT-instructions

The video-based CT-instructions (15 minutes) consisted of a general instruction on CT and explicit instructions on avoiding belief-bias in syllogistic reasoning. In the general instruction, the features of CT and the attitudes and skills that are needed to think

critically were described. These were followed by the explicit instructions on avoiding belief-bias in syllogistic reasoning that consisted of a worked example of each form of syllogistic reasoning included in the pretest. The worked examples not only showed the rationale behind the solution steps but also included possible problem-solving strategies which allowed participants to mentally correct initially erroneous responses. At the end of the video-based instruction, participants received a hint stating that the principles used in these examples can be applied to several other reasoning tasks.

CT-practice

After the video-based instruction, participants practiced with the four types of syllogistic reasoning problems of the pretest and explicit instructions, on topics that they might encounter in their working-life. Participants were instructed to read the problems thoroughly, to choose the best multiple-choice answer option, and to give a written explanation of how the answer was obtained in a text entry box below the multiple-choice question. After each practice-task, participants received correct-answer feedback (e.g., "You gave the following answer: conclusion follows logically from the two premises. This answer is incorrect.") and were given a worked example that consisted of the problem statement and a correct solution to this problem. The line of reasoning and the underlying principles were explained in steps and clarified with a visual representation. Again, participants were asked to read the worked examples thoroughly before they continued to the next problem. The content of the surface features (cover stories) of all practice items was adapted to the study domain of participants of Experiment 2 (i.e., Biology and Medical Laboratory Research/Chemistry), because that experiment was conducted in a classroom setting as part of an existing course.

Procedure

Experiment 1 was run in the computer lab of the university and lasted circa 90 minutes. One experiment leader (first author of this paper or research assistant) was present during all phases of the experiment. Participants were seated in individual cubicles, where A4-papers were distributed before they arrived. These papers contained some general rules, a link to the Qualtrics environment where all materials were delivered, and a blank page that was only needed for participants in the free recall condition. The experiment leader first introduced herself and provided some basic information about the experiment. Afterwards, she instructed participants to read the A4-paper containing some general instructions and a link to the Qualtrics environment where they first signed an informed consent form.

Next, participants filled out a short demographic questionnaire and completed the pretest. Thereafter, participants entered the learning phase in which they viewed the video (15 min.) including the general CT-instruction and the explicit instructions, followed

by the four practice problems. Immediately after the learning phase, they took a short break of four minutes in which they could relax or move about. Next, participants completed the learning items of the posttest. Subsequently, the Qualtrics program randomly assigned the participants to one of the four conditions. Depending on assigned condition, participants received different levels of support while completing the near and far transfer items of the posttest (see supporting prompts subsection). Participants could work at their own pace and time-on-task was logged during all phases. Furthermore, participants could use scrap paper during the practice phase and the CT-tests.

Data analysis

Unbiased reasoning items were scored for accuracy based on multiple-choice responses and explanations, using a coding scheme that can be found on our OSF-page. Specifically, each correct multiple-choice answer was worth 0.5 point and a correct explanation was worth 1 point, a partially correct explanation received 0.25 – 0.5 point, and an incorrect explanation was awarded 0 points. The scores were summed, resulting in a maximum score of 12 points on the learning items, 9 points on the near transfer items, and 9 points on the far transfer items. Unfortunately, one near transfer item had to be removed because it was inconsistent in difficulty between test moments, as the belief bias was less effective in the pretest compared to the posttest, making it relatively easier on the pretest. As a result, a total score of 7.5 points could be gained on near transfer items. Two raters independently scored 25% of the posttest. Intra-class correlation coefficients were 0.985 for the learning test items, 0.989 for the near transfer test items, and 0.977 for the far transfer items. After the discrepancies were resolved by discussion, the remainder of the tests was scored by one rater.

To explore whether participants' ability to recall the acquired knowledge underlies difficulties with transfer, free recall was scored, using another coding scheme (see OSF-page). Participants in the free recall condition could earn a maximum of 1 point per rule of logic correctly retrieved (in steps of 0.5), resulting in a maximum total score of 4 points on retrieved information. The two raters independently scored all free recall data. Intra-class correlation coefficients were .963 (nothing written down coded as no recall) and .998 (nothing written down coded as missing value).

Reliability (Cronbach's alpha) of the learning items was .56 on the pretest and .75 on the posttest, reliability of the near transfer items was .51 on the pretest and .71 on the posttest, and reliability of the far transfer items was .74 on the pretest and .92 on the posttest. It was expected that participants would have very limited knowledge relative to these tasks at the outset, and therefore were unable to generate coherent explanations

(and may even have had to guess), leading to low variability and low alphas at pretest. Posttest alphas are thus more indicative of the reliability of these tasks when respondents are presumed to have some knowledge or exposure to the content being assessed.

Results

In all analyses reported below, a *p*-value of .05 was used as a threshold for statistical significance. Partial eta-squared (η_p^2) is reported as a measure of effect size for all ANOVAs with $\eta_p^2 = .01$, $\eta_p^2 = .06$, and $\eta_p^2 = .14$ denoting small, medium, and large effects, respectively (Cohen, 1988). Cohen's *d* is reported as a measure of effect size for all t-tests, with values of 0.20, 0.50, and 0.80 representing small, medium, and large effects, respectively (Cohen, 1988). Furthermore, Cramer's *V* is reported as an effect size for chi-square tests with (having 2 degrees of freedom) $V = .07$, $V = .21$, and $V = .35$ denoting small, medium, and large effects, respectively.

We created boxplots to identify outliers (i.e., values that fall more than 1.5 times the interquartile range above the third quartile or below the first quartile) in the data. If there were any, we first conducted the analyses on the data of all participants who completed the experiment (i.e., including outliers) and reran the analyses on the data without outliers. If outliers influenced on the results, we reported the results of both analyses. If the results were the same, we only reported the results on the full data.

Before addressing our hypotheses, preliminary analyses were conducted to assess whether the four conditions were comparable before the start of the manipulation. Results confirmed that there were no a-priori differences between the conditions in educational background, $\chi^2(12) = 16.50$, $p = .17$, $V = .17$; gender, $\chi^2(3) = 0.41$, $p = .938$, $V = .05$; age, $F(3, 184) = 0.98$, $p = .406$, $\eta_p^2 = .02$; performance on near transfer items of the pretest, $F(3, 184) = 0.60$, $p = .616$, $\eta_p^2 = .01$; time-on-task on near transfer items of the pretest, $F(3, 184) = 0.33$, $p = .804$, $\eta_p^2 = .01$; performance on far transfer items of the pretest, $F(3, 184) = 0.20$, $p = .895$, $\eta_p^2 < .01$; time-on-task on far transfer items of the pretest, $F(3, 184) = 0.36$, $p = .782$, $\eta_p^2 = .01$; performance on practice tasks, $F(3, 184) = 2.30$, $p = .079$, $\eta_p^2 = .04$; and time-on-task on practice tasks, $F(3, 184) = 0.41$, $p = .746$, $\eta_p^2 = .01$. Figures 2 – 4 provide Violin plots in which the full distribution per condition and test moment is visualized for each dependent variable.

Figure 2 – 4 Violin plots with the full distribution per condition and test moment (i.e., pretest and posttest) on performance on learning items (maximum total score of 12; Figure 2), performance on near transfer items (maximum total score of 7.5; Figure 3), and performance on far transfer items (maximum total score of 9; Figure 4) in Experiment 1.

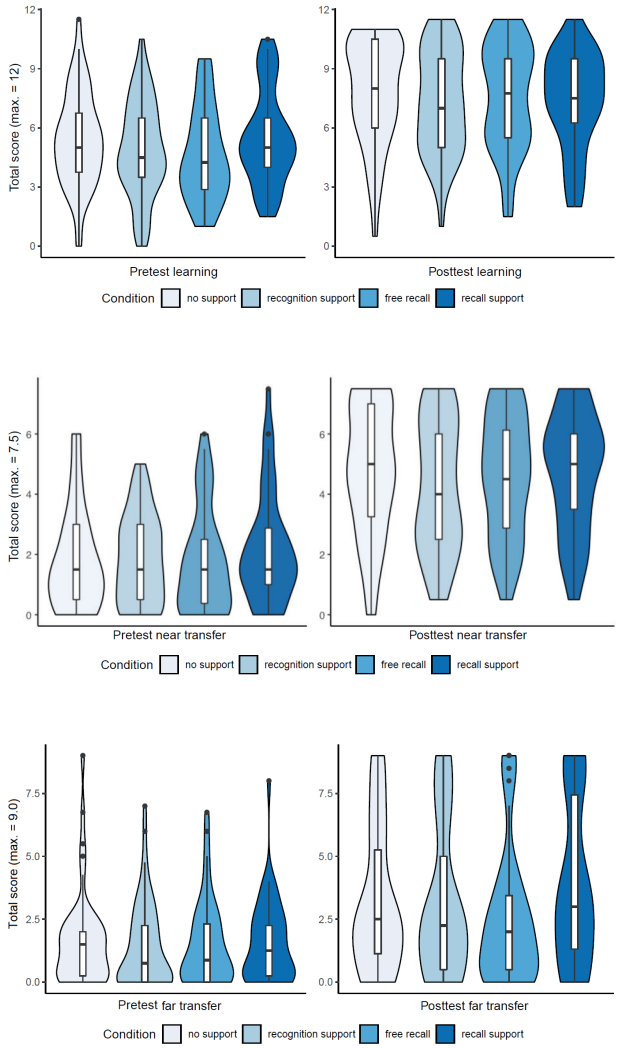


Figure 5. Graphical representation of the relationship between retrieved information during free recall and posttest near transfer performance in Experiment 1. Two measures of retrieved information were used: nothing written down was either coded as no recall or as missing value.

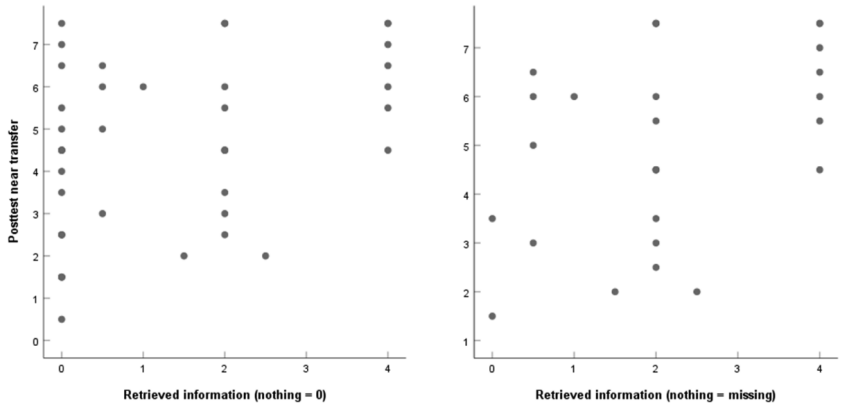
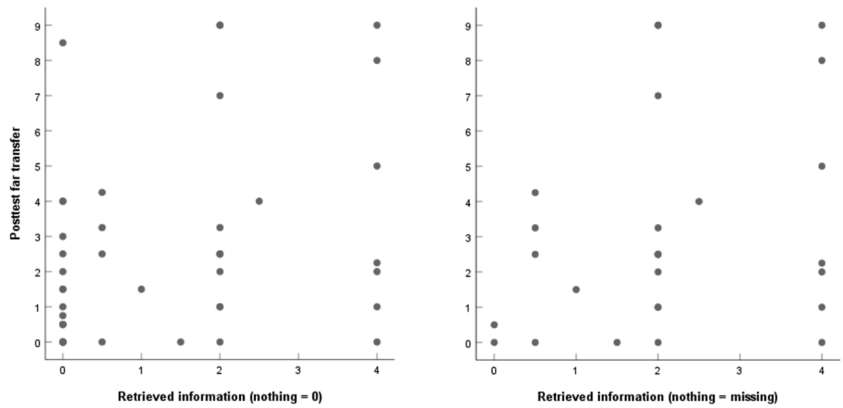


Figure 6. Graphical representation of the relationship between retrieved information during free recall and posttest far transfer performance in Experiment 1. Two measures of retrieved information were used: nothing written down was either coded as no recall or as missing value.



Performance on learning items

Performance scores on the pretest and posttest per condition are presented in Table 2. To test if we could replicate the finding from prior research that providing students with explicit instructions and practice activities is effective for learning to avoid biased reasoning, we conducted a paired samples t-test with Test Moment (pretest and posttest) as within-subjects factor on performance on learning items. In line with previous findings, the results revealed an overall pretest (M = 5.04, SD = 2.38) to posttest (M = 7.83, SD = 2.76) performance gain on learning items, $t(188) = -13.53$, $p < .001$, $d = 1.07$.

Performance on near and far transfer items

Again, performance scores on the pretest and posttest per condition are presented in Table 2. To test our main question what obstacle(s) underlie(s) the lack of transfer what has been learned to new – but related – tasks requiring CT-skills, we conducted a 2×4 mixed ANOVA with Test Moment (pretest and posttest) as within-subjects factor and Level of Support (no support, recognition support, free recall, and recall support) as between-subjects factor. On *performance on near transfer items*, this revealed a main effect of Test Moment, $F(1, 184) = 261.75$, $p < .001$, $\eta_p^2 = .59$: mean performance was higher on the posttest (M = 4.56, SD = 2.07) compared to the pretest (M = 1.90, SD = 1.66). However, there was no significant main effect of Level of Support, $F(3, 184) = 0.61$, $p = .613$, $\eta_p^2 = .01$, nor an interaction between Test Moment and Level of Support, $F(3, 184) = 0.66$, $p = .576$, $\eta_p^2 = .01$.

On *performance on far transfer items*, results revealed a main effect of Test Moment, $F(1, 184) = 77.31$, $p < .001$, $\eta_p^2 = .30$: mean performance was higher on the posttest (M = 3.18, SD = 2.97) compared to the pretest (M = 1.52, SD = 1.71). However, there was no significant main effect of Level of Support, $F(3, 184) = 0.85$, $p = .469$, $\eta_p^2 = .01$, nor an interaction between Test Moment and Level of Support, $F(3, 184) = 1.74$, $p = .161$, $\eta_p^2 = .03$.

Finally, to explore whether participants' ability to recall the acquired knowledge underlies difficulties with transfer, we computed Pearson correlations on the data of participants within the free recall condition, between retrieved information and posttest performance on near transfer items and between retrieved information and performance on far transfer items (see Figures 5 and 6 for a graphical representation of the relationship between the variables). Retrieved information was positively related to posttest performance on near transfer items, $r(44) = .41$, $p = .005$, as well as to posttest performance on far transfer items, $r(44) = .34$, $p = .023$. When nothing written down during free recall was coded as missing value instead of no recall, retrieved information was still positively related to

posttest performance on near transfer performance, $r(27) = .41$, $p = .033$, but not with posttest performance on far transfer items, $r(27) = .29$, $p = .139$.

Table 2. Experiment 1: mean (*SD*) of test performance (number of items correct) on learning (0 – 12), near transfer (0 – 7.5), and far transfer items (0 – 9) and mean (SD) of time-on-task (in seconds) on learning, near transfer, and far transfer items per condition.

		Level of support			
		No support	Recognition support	Free recall	Recall support
Test performance					
Learning	Pretest	5.38 (2.39)	4.90 (2.42)	4.61 (2.35)	5.45 (2.39)
	Posttest	8.34 (2.85)	7.65 (2.79)	7.75 (2.77)	7.69 (2.66)
Near transfer	Pretest	1.83 (1.70)	1.85 (1.45)	1.76 (1.73)	2.20 (1.83)
	Posttest	4.77 (2.14)	4.25 (2.17)	4.60 (2.03)	4.70 (1.96)
Far transfer	Pretest	1.65 (1.87)	1.40 (1.68)	1.49 (1.70)	1.56 (1.62)
	Posttest	3.17 (2.84)	3.14 (3.01)	2.56 (2.73)	3.87 (3.23)
Time-on-task					
Learning	Pretest	80.46 (37.74)	82.02 (38.62)	81.12 (40.81)	79.07 (32.79)
	Posttest	51.05 (18.93)	52.82 (24.97)	57.32 (22.02)	49.83 (19.88)
Near transfer	Pretest	107.97 (48.51)	101.01 (48.41)	101.83 (46.72)	109.12 (55.26)
	Posttest	77.95 (32.49)	76.27 (28.38)	91.16 (31.13)	95.94 (31.05)
Far transfer	Pretest	84.45 (37.17)	83.77 (38.04)	89.01 (42.77)	91.26 (46.35)
	Posttest	46.92 (18.28)	49.25 (30.88)	57.43 (26.13)	62.08 (36.77)

Time-on-test

We also explored differences over time and among conditions in the time spent on test items (in seconds). Descriptive statistics are provided in Table 2. A paired samples t-test with Test Moment (pretest and posttest) as within-subjects factor on time spent on *learning items* revealed that the mean time was lower for the posttest items ($M = 52.76$, $SD = 21.77$) than the pretest items ($M = 80.76$, $SD = 37.43$), $t(187) = 11.98$, $p < .001$, $d = .91$.

We conducted 2×4 mixed ANOVAs on the time spent on transfer items with Test Moment (pretest and posttest) as within-subjects factor and Level of Support (no support, recognition support, free recall, and recall support) as between-subjects factor. On time spent on *near transfer items*, this revealed a main effect of Test Moment, $F(1, 184) = 30.20$, $p < .001$, $\eta_p^2 = .14$: participants spent less time on average on the posttest items

(M = 84.57, SD = 31.58) compared to the pretest items (M = 104.75, SD = 49.40). There was no significant main effect of Level of Support, $F(3, 184) = 1.47$, $p = .225$, $\eta_p^2 = .02$, nor an interaction between Test Moment and Level of Support, $F(3, 184) = 1.64$, $p = .181$, $\eta_p^2 = .03$.

On time spent on *far transfer items*, results revealed a main effect of Test Moment, $F(1, 184) = 173.78$, $p < .001$, $\eta_p^2 = .49$: again, participants spent less time on average on the posttest items (M = 53.45, SD = 29.11) compared to the pretest items (M = 86.84, SD = 40.73). There was no significant main effect of Level of Support, $F(3, 184) = 1.35$, $p = .260$, $\eta_p^2 = .02$, nor an interaction between Test Moment and Level of Support, $F(3, 184) = 0.49$, $p = .684$, $\eta_p^2 = .01$.

Experiment 2

We simultaneously conducted a replication experiment in a classroom setting to assess the robustness of our findings and to increase ecological validity. The educational committee of the university approved on conducting this study within the curriculum. The design and materials were the same as that of Experiment 1.

Methods

Participants

Participants were 104 third-year 'Biology and Medical Laboratory Research' and 'Chemistry' students of a University of Applied Sciences. Of these, three students did not complete the complete study due to technical problems and four students did not adhere to instructions (i.e., they copied information from the CT-instructions). They were therefore excluded from the analyses and this resulted in a final sample size of 97 students (M_{age} = 20.39, SD = 1.67; 23 males).

Because the experiment took place in classroom setting as part of an existing course, our sample size was limited to the total number of students in this cohort. The power of our mixed ANOVAs – under a fixed alpha level of .05, with a correlation between measures of 0.3, and with a sample size of 97 – is estimated at .25, .95, and <.99 for picking up a small ($\eta_p^2 = .01$), medium ($\eta_p^2 = .06$), and large ($\eta_p^2 = .14$) interaction effect, respectively. Therefore, our sample size should be sufficient to pick up medium-to-large interaction effects.

Procedure

The main difference with Experiment 1 was that Experiment 2 was run in a real education setting, namely during the lessons of a CT-course. The experiment was conducted in a computer classroom at the participants' university with an entire class of students present. Participants came from five different classes (of 17 to 23 participants) and were randomly distributed among the four conditions within each class. In advance of the experiment, students were informed about the experiment by their teacher. The experiment leader (first author) and the teacher of the CT-course were present during the experiment. When entering the classroom, participants were instructed to sit down at one of the desks. The experiment leader first introduced herself and provided some basic information about the experiment. Afterwards, she instructed participants to read a sheet of paper containing some general instructions and a link to the Qualtrics environment where they first signed an informed consent form. Again, participants could work at their own pace and time-on-task was logged during all phases. Furthermore, participants could use scrap paper during the practice phase and the CT-tests. Participants had to wait (in silence) until the last participant had finished the posttest before they could leave the classroom.

Data analysis

The same coding schemes were used as in Experiment 1. Again, a total score of 12 points could be earned on learning items, of 7.5 points on near transfer items, and of 9 points on far transfer items. Again, two raters independently scored all free recall data. Intra-class correlation coefficients were .987 (nothing written down coded as no recall) and .971 (nothing written down coded as missing value).

Reliability (Cronbach's alpha) on the pretest and posttest, respectively, of the learning items were .45 and .68; of the near transfer items were .32 and .67; and of the far transfer items .77 and .89. While these low reliabilities on the pretest might again be explained by lack of prior knowledge, they are substantially lower in experiment 2 than in experiment 1, and under these circumstances, the probability of detecting a significant effect (given one exists) is low (e.g., Cleary, Linn, & Walster, 1970; Rogers & Hopkins, 1988), and therefore, the chance that Type 2 errors may have occurred in the current study is relatively high. Therefore, we conducted alternative analyses (see Results section), as preregistered.

Two participants had two missing near transfer answers on the posttest, which were replaced by their series mean. One participant did not fill in the far transfer items of the posttest, so data for this participant were not included in the analyses involving the respective measure.

Results

Again, a *p*-value of .05 was used as a measure of statistical significance in all analyses reported below. Partial eta-squared (η_p^2) is reported as a measure of effect size for the ANOVAs for which .01 is considered small, .06 medium, and .14 large (Cohen, 1988). If outliers influenced the results, we reported the results of the analysis on the data of all participants who completed the experiment (i.e., including outliers) and the analysis on the data without outliers. If the results were the same, we only reported the results on the full data.

Preliminary analyses confirmed that there were no a-priori differences between the conditions in educational background, $\chi^2(12) = 8.90$, $p = .712$, $V = .18$; gender, $\chi^2(6) = 3.97$, $p = .681$, $V = .14$; age, $F(3, 97) = 1.08$, $p = .361$, $\eta_p^2 = .03$; performance on near transfer items of the pretest, $F(3, 93) = 1.76$, $p = .159$, $\eta_p^2 = .05$; time-on-task on near transfer items of the pretest, $F(3, 93) = 0.70$, $p = .552$, $\eta_p^2 = .02$; time-on-task on far transfer items of the pretest, $F(3, 93) = 0.21$, $p = .888$, $\eta_p^2 = .01$; performance on practice tasks, $F(3, 96) = 0.39$, $p = .762$, $\eta_p^2 = .01$; and time-on-task on practice tasks, $F(3, 96) = 1.59$, $p = .196$, $\eta_p^2 = .05$. However, the conditions differed in performance on far transfer items of the pretest, $F(3, 93) = 4.17$, $p = .008$, $\eta_p^2 = .12$. If it turns out that the conditions would differ significantly in performance gains on far transfer items, this finding should be taken into account. Figures 7 – 9 provide Violin plots in which the full distribution per condition and test moment is visualized for each dependent variable.[10]

Performance on learning items

Performance scores on the pretest and posttest per condition are presented in Table 3. Because Cronbach's Alpha on the pretest was very low, we conducted a one-sample t-test on posttest performance on learning items, in which we compared the average on the posttest of the entire sample against the reference value of the average on the pretest ($M = 4.59$, $SD = 2.43$). In line with Experiment 1, the results revealed an overall pretest to posttest ($M = 7.79$, $SD = 2.69$) performance gain on learning items, $t(97) = -11.73$, $p < .001$, $d = 1.25$.

Performance on near and far transfer items

Performance scores on the pretest and posttest per condition are presented in Table 3. To test our main question what obstacle(s) underlie(s) the lack of transfer what has been learned to novel tasks requiring CT-skills, we conducted a one-way ANOVA (due to low reliability on the pretest, see preregistration where we reported what analyses would be

[10] We also conducted some exploratory analyses regarding students' study background and the time participants spent on the CT-instructions. However, these analyses did not have much added value for this paper, and, therefore, are not reported here but provided on our OSF-page.

Figure 7 – 9 Violin plots with the full distribution per condition and test moment (i.e., pretest and posttest) on performance on learning items (maximum total score of 12; Figure 7), performance on near transfer items (maximum total score of 7.5; Figure 8), and performance on far transfer items (maximum total score of 9; Figure 9) in Experiment 1.

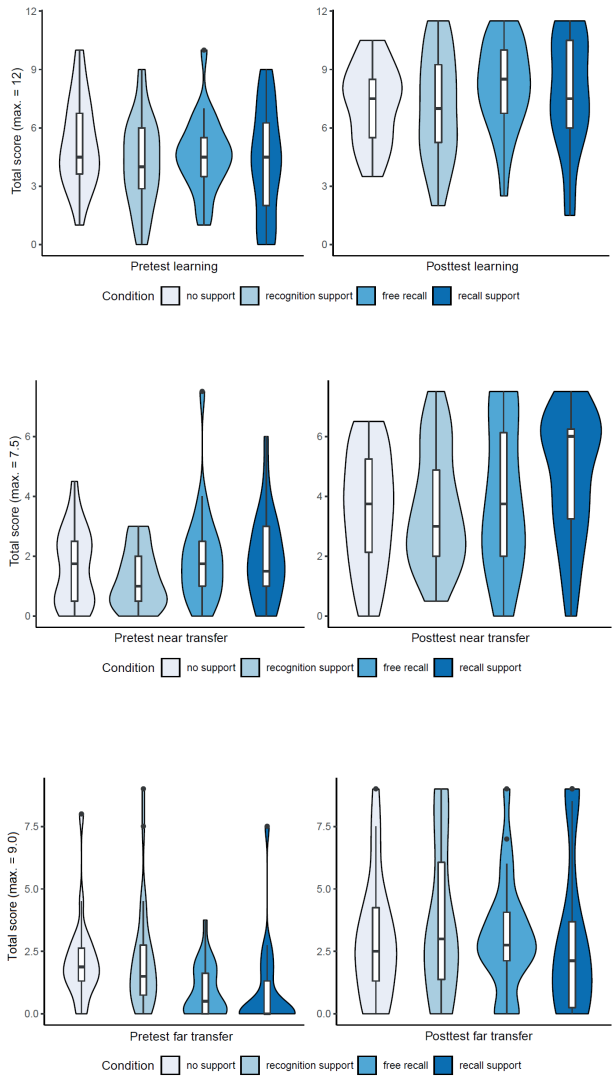


Figure 10. Graphical representation of the relationship between retrieved information during free recall and posttest near transfer performance in Experiment 2. Two measures of retrieved information were used: nothing written down was either coded as no recall or as missing value.

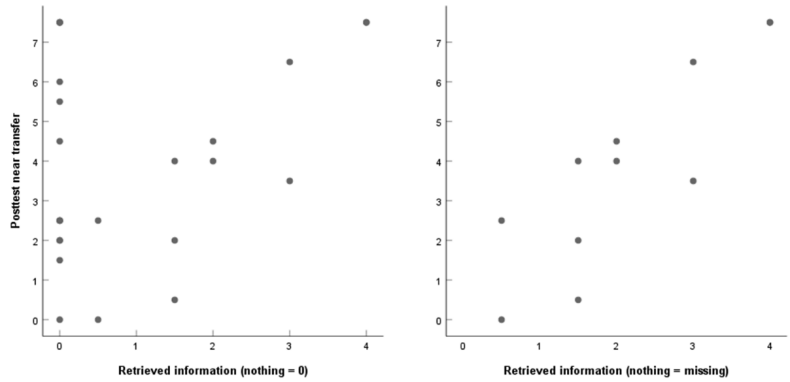
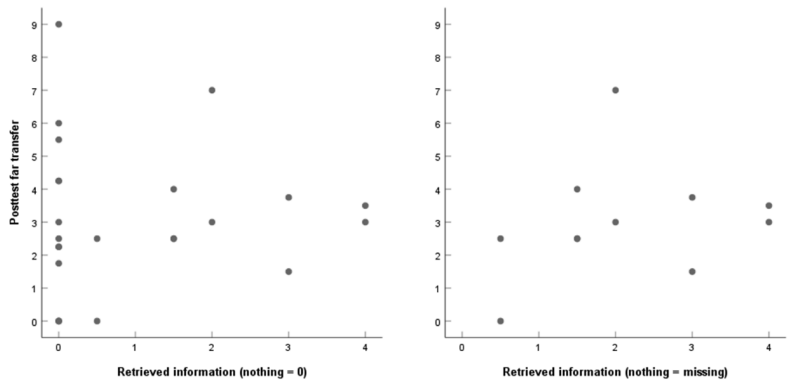


Figure 11. Graphical representation of the relationship between retrieved information during free recall and posttest far transfer performance in Experiment 2. Two measures of retrieved information were used: nothing written down was either coded as no recall or as missing value.



performed if Cronbach's Alpha on the pretest turned out to be low) with Level of Support (no support, recognition support, free recall, and recall support) as between-subjects factor on *performance on near transfer items*. The results revealed no significant main effect of Level of Support, $F(3, 93) = 1.36$, $p = .259$, $\eta_p^2 = .06$. In addition to the planned analysis, we decided to conduct a one-sample t-test on posttest performance on near transfer items, compared to the reference value of the average on the pretest ($M = 1.66$, $SD = 1.35$). The results revealed an overall pretest to posttest ($M = 3.91$, $SD = 2.16$) performance gain on near transfer items, $t(96) = 10.21$, $p < .001$, $d = 1.25$.

Additionally, we conducted a 2×4 mixed ANOVA with Test Moment (pretest and posttest) as within-subjects factor and Level of Support (no support, recognition support, free recall, and recall support) as between-subjects factor on *performance on far transfer items.* The results revealed a main effect of Test Moment $F(1, 92) = 43.91$, $p < .001$, $\eta_p^2 = .32$: mean performance was higher on the posttest ($M = 3.20$, $SD = 2.74$) compared to the pretest ($M = 1.55$, $SD = 1.80$). However, there was no significant main effect of Level of Support, $F(3, 92) = 1.39$, $p = .250$, $\eta_p^2 = .04$, nor an interaction between Test Moment and Level of Support, $F(3, 92) = 1.48$, $p = .226$, $\eta_p^2 = .05$.[11]

Finally, to explore whether participants' ability to recall the acquired knowledge underlies difficulties with transfer, we computed Pearson correlations on the data of participants within the free recall condition, between retrieved information and posttest performance on far transfer items and between retrieved information and performance on near transfer items (see Figures 10 and 11 for a graphical representation of the relationship between the variables). Retrieved information was not positively related to posttest performance on near transfer items, $r(24) = .33$, $p = .114$, nor with posttest performance on far transfer items, $r(24) = .06$, $p = .787$. When nothing written down during free recall was coded as missing value instead of no recall, however, retrieved information was positively related to posttest performance on near transfer performance, $r(11) = .87$, $p = .001$, but not with posttest performance on far transfer items, $r(11) = .26$, $p = .443$.

Time-on-test

We exploratory analyzed the time spent on test items (in seconds). Descriptive statistics are provided in Table 3. A paired samples t-test with Test Moment (pretest and posttest) as within-subjects factor on time spent on *learning items* revealed that the mean time spent on posttest items ($M = 52.76$, $SD = 21.77$) was lower than on pretest items ($M = 80.76$, $SD = 37.43$), $t(97) = 9.88$, $p < .001$, $d = 1.11$.

[11] Because of severe violations of the normality assumption, we additionally conducted a Kruskal-Wallis H Test (nonparametric alternative of ANOVA); however, the results did not differ from the parametric analyses and, therefore, are not reported in this paper but provided on our OSF-page.

We conducted 2×4 mixed ANOVAs on the time spent on transfer items with Test Moment (pretest and posttest) as within-subjects factor and Level of Support (no support, recognition support, free recall, and recall support) as between-subjects factor. On time spent on *near transfer items*, that revealed a main effect of Test Moment, $F(1, 93) = 37.59$, $p < .001$, $\eta_p^2 = .29$: participants spent less time on the posttest items ($M = 84.32$, $SD = 26.87$) compared to the pretest items ($M = 108.78$, $SD = 40.27$). There was no significant main effect of Level of Support, $F(3, 93) = 1.85$, $p = .143$, $\eta_p^2 = .06$, nor an interaction between Test Moment and Level of Support, $F(3, 93) = 2.18$, $p = .096$, $\eta_p^2 = .07$.

On time spent on *far transfer* items, results revealed a main effect of Test Moment, $F(1, 92) = 151.39$, $p < .001$, $\eta_p^2 = .62$: again, participants spent less time on the posttest items ($M = 62.84$, $SD = 25.23$) compared to the pretest items ($M = 104.41$, $SD = 35.49$). There was no significant main effect of Level of Support, $F(3, 92) = 0.63$, $p = .595$, $\eta_p^2 = .02$, nor an interaction between Test Moment and Level of Support, $F(3, 92) = 1.21$, $p = .309$, $\eta_p^2 = .04$.

Table 3. Experiment 2: mean (*SD*) of test performance (number of items correct) on learning (0 – 12), near transfer (0 – 7.5), and far transfer items (0 – 9) and mean (*SD*) of time-on-task (in seconds) on learning, near transfer, and far transfer items per condition.

		Level of support			
		No support	Recognition support	Free recall	Recall support
Test performance					
Learning	Pretest	5.16 (2.25)	4.20 (2.26)	4.90 (2.33)	4.20 (2.78)
	Posttest	7.43 (2.23)	7.52 (2.87)	8.29 (2.38)	8.28 (3.13)
Near tansfer	Pretest	1.64 (1.26)	1.21 (0.97)	1.94 (1.60)	1.96 (1.48)
	Posttest	3.50 (1.98)	3.61 (1.97)	3.92 (2.52)	4.65 (2.11)
Far transfer	Pretest	2.11 (1.69)	2.16 (2.13)	0.92 (1.04)	0.89 (1.74)
	Posttest	2.95 (2.46)	3.65 (3.05)	3.08 (2.23)	3.00 (3.18)
Time-on-task					
Learning	Pretest	90.02 (21.27)	80.10 (27.34)	82.85 (21.55)	90.84 (39.81)
	Posttest	63.42 (19.17)	53.08 (18.98)	57.64 (20.88)	59.51 (22.58)
Near transfer	Pretest	115.02 (39.28)	101.69 (33.06)	106.63 (36.83)	113.69 (40.27)
	Posttest	73.21 (23.68)	76.50 (19.88)	92.52 (27.54)	95.91 (30.35)
Far transfer	Pretest	107.40 (33.89)	101.53 (36.51)	103.32 (36.16)	106.26 (37.14)
	Posttest	58.73 (17.41)	63.38 (22.82)	56.07 (21.04)	73.63 (35.06)

General discussion

The present study aimed to identify obstacles to transfer of CT-skills required for unbiased reasoning. Prior studies observed a lack of transfer of these CT-skills (e.g., Heijltjes et al., 2014a, 2014b, 2015; Van Peppen et al., 2018), and we examined whether this would be due to (a) failure to recognize that the acquired knowledge is relevant to the new task, (b) inability to recall the acquired knowledge, or (c) difficulties in actually mapping that knowledge onto the new task (cf. the three-step model of transfer: Barnet & Ceci, 2012).

Benefits of instruction and practice

In line with our expectations and consistent with earlier research (e.g., Abrami et al., 2014; Heijltjes et al., 2014b), we found that providing students with explicit instructions and practice leads to a performance gain in unbiased reasoning and a reduction in test-taking time in two experiments. These results further support the idea of Stanovich (2011) that acquisition of relevant knowledge structures and stimulating students to engage in CT, is useful to prevent biased reasoning.

Interestingly, our experiments demonstrated that these instructions and practice activities may also enhance transfer (both to similar tasks in a different format and to novel task types) to some extent: students showed better performance on posttest transfer tasks, and, again, with reduced test-taking time. As one would expect (Barnett & Ceci, 2002; Bray, 1928; Dinsmore et al., 2014), transfer between closely related situations occurred more often than transfer between situations that had less in common: performance gains were highest on learning items (i.e., same problem type but different story contexts), followed by near transfer items (i.e., same problem type but offered in a different/less abstract format), and thereafter far transfer items (i.e., similar principles but applied to novel problem types).

It is particularly promising that participants improved noticeably on near transfer items after a relatively short instruction and practice phase. These items consisted of belief biases in written news items or articles on topics that participants might encounter in other courses and/or their working life. The few studies that investigated effects of instruction/practice on transfer of CT-skills, and failed to find evidence of transfer, only examined tasks reflecting far transfer (Heijltjes et al., 2014a, 2015; Van Peppen et al., 2018). We even observed some increase in performance on far transfer items in the present study. Other studies did not include these items on the pretest (Chapters 3 to 5) and were, therefore, not able to detect transfer *gains*.

Thus, our findings are promising as they seem to support the idea that instruction/practice can be beneficial for near and far transfer of CT-skills. However, the scores were still rather low, so there was a lot of room for improvement, yet students did not seem to benefit from the support conditions, as we will discuss in the next section.

Obstacles to successful transfer of CT-skills

As for our main question regarding the obstacles to successful transfer of CT-skills, our findings suggest that participants were able to recognize that the acquired knowledge was relevant to the new task and to recall that knowledge: they did not benefit from recognition and recall support (i.e., there were no significant differences among conditions). Thus, our findings suggest that students may have had difficulties in *applying* the relevant knowledge on the new tasks (Hypothesis 3).

However, findings from the free recall condition do not fully support the idea that it is only an application/mapping problem. Most participants did not retrieve all relevant information and exploratory results pointed to moderate-to-large positive correlations between participants' retrieved knowledge and their performance on near transfer (in both experiments) and far transfer (only in Experiment 1 when nothing written down was coded as no recall) items. This may suggest that suboptimal recall underlies unsuccessful transfer as well (Hypothesis 2b). Descriptive statistics support this idea: participants who received recall support numerically outperformed the other conditions on far transfer items at posttest in Experiment 1 and on near transfer items at posttest in Experiment 2. Because the power of our study was only sufficient to pick up medium-to-large interaction effects and it may be that providing recall support had a small effect on transfer, a further study with a more powerful design (e.g., a larger sample size) is suggested.

Limitations and future directions

One potential limitation of this study concerns the short training duration. While it is interesting to see that this relatively brief intervention already had beneficial effects on learning and near transfer, gaining deep understanding of the underlying principles of the subject matter, required for far transfer, might need more extensive training. Even though our results indicate that participants learned to solve abstract CT-tasks (i.e., syllogisms), their subject-matter knowledge may have been insufficient for identifying structural overlap between problems and, consequently, for solving more complex or novel CT-tasks.

Given that multiple studies reported rather low levels of reliability of tests consisting of heuristics-and-biases tasks (Aczel et al., 2015b; West et al., 2008) and revealed

concerns with the reliability of widely used standardized CT tests, particularly with regard to subscales (Bernard et al., 2008; Bondy et al., 2001; Ku, 2009; Leppa, 1997; Liu et al., 2014; Loo & Thorpe, 1999), we aimed to increase reliability of our measures. Therefore, we included multiple items of one CT-task category to narrow down the test into a single measurable construct and, thereby, to decrease measurement error (LeBel & Paunonen, 2011), which resulted – except on the pretest – in quite reliable measures. However, because of this, we focused on only one (though very important) aspect of CT, namely overturning belief-biased responses when evaluating the logical validity of arguments (De Chantal et al., 2019; Evans, 2003). Relevant next steps would be to investigate gains in performance on transfer tasks with other types of reasoning biases, for instance those involving probabilistic reasoning.

A noteworthy strength of this study was that we simultaneously conducted a replication experiment in a classroom setting to assess the robustness of our findings and to increase ecological validity. As promising interventions sometimes fail in realistic settings (e.g., Hulleman & Cordray, 2009) and classroom studies aimed at fostering transfer of CT-skills are relatively rare, this study provides valuable new insights for educational practice. To wit, that transfer of CT-skills from abstract tasks to domain relevant texts and to novel task types can be established with a relatively short instruction and practice phase. However, there is still a lot of room for improvement in bringing about far transfer, and for that, obstacles such as suboptimal recall and application should be countered. Considerably more studies, preferably including direct or conceptual replications to increase robustness of findings, are needed to develop a full picture of effective ways to teach (far) transfer of CT-skills.

Conclusion

To conclude, the present study established that it is possible to foster learning and transfer of CT-skills to different formats/situations and novel task types through a relatively simple intervention. Our findings imply that instructional interventions aimed at transfer of CT-skills should focus on the recall and application steps in the transfer process. As far as we know, our study was the first to systematically vary gradients of similarity between the initial CT-task and the transfer task (i.e., learning, near transfer, and far transfer) and, thus, adds to the small body of literature on whether instruction/practice can foster students' CT, which is relevant for both educational science and educational practice.

Appendix A. Overview of the supporting prompts

Below, we provided an overview per condition of the supporting prompts (translated from Dutch) that participants received at the start of the posttest transfer items and with each posttest transfer item.

No support condition

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Recognition support condition

To solve the following problems, you can use the rules of logic explained in the instructions.

Hint: To solve this task, you can use the rules of logic explained in the instructions.

Free recall condition

To solve the following problems, you can use the rules of logic explained in the instructions. Try to recall these rules and write them on the paper that you have received (Paper 2).

Hint: To solve this task, you can use the rules of logic explained in the instructions that you tried to recall beforehand. Take that paper to solve the task.

Recall support condition

To solve the following problems, you can use the rules of logic explained in the instructions. You can find these rules in the overview on the paper that you just have received.

Hint: To solve this task, you can use the rules of logic explained in the instructions. You can find these rules in the overview on the paper that you have received. Take that paper to solve the task.

Affirming the antecedent Statement 1: If P, then Q Statement 2: P Conclusion: Therefore Q (valid)	Affirming the consequent Statement 1: If P, then Q Statement 2: Q Conclusion: Therefore P (invalid)
Denying the antecedent Statement 1: If P, then Q Statement 2: Not P Conclusion: Therefore not Q (invalid)	Denying the consequent Statement 1: If P, then Q Statement 2: Not Q Conclusion: Therefore not P (valid)

Appendix A. Example items critical thinking tests

Below, we translated an example item of each task category administered in the critical thinking tests and the correct answer with an explanation.

Learning task (syllogistic reasoning)

Below, you will find two premises that you must assume are true. Indicate whether the conclusion follows logically from the given premises.

Premise 1. If a disease is caused by parasites, then it is an infectious disease.
Premise 2. Malaria is an infectious disease.
Conclusion: Malaria is caused by parasites.

a) Conclusion follow logically from the premises
b) Conclusion does not follow logically from the premises

Explain briefly why you chose this answer:

Correct answer: b

Explanation: This assignment requires to not confuse logical validity of the conclusion with the believability of the conclusion, which presumably seems believable to participants due to their prior beliefs or real-world knowledge. If the first part of premise 1 (if a disease is caused by parasites) is met, the second part (then it is an infectious disease) automatically follows. The second premise states that Malaria is an infectious disease. But this does not necessarily mean that it is caused by parasites. There might be another cause.

Near transfer task (syllogistic reasoning in a vignette)

An article by the Netherlands Forensic Institute (NFI) about the essence of forensic hair analyses states: Forensic hair analyses can provide important information in solving crimes. If the aim of forensic hair analyses is to identify the donor of the hair sample, then hair comparisons are performed. The investigator compares the hair sample that is found at the crime scene with reference samples of a suspect, victim, and/or person involved. In a recent investigation including forensic hair analyses, no hair comparisons are performed and, thus, the aim was not to identify the donor of the hair sample.

a) Conclusion follows logically from the premises
b) Conclusion does not follow logically from the premises

Explain briefly why you chose this answer:

Correct answer: a

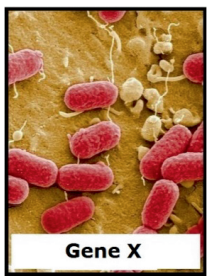
Explanation: This assignment requires to not confuse logical validity of the conclusion with the believability of the conclusion, which presumably seems unbelievable to participants due to their prior beliefs or real-world knowledge. According to the statement in the second sentence 'if the aim of forensic hair analyses is to identify the donor of the hair sample' (P) is met, then 'hair comparisons are performed' (Q) automatically follows. In the last sentence it can be read that hair comparisons are not performed in a recent investigation, so Q is denied. Therefore, P is not present. Because if P had been present, Q would have always followed.

Far transfer task (Wason selection)

Below, you can see four bacterial strains. Each bacterial strain has two characteristics: (1) it contains gene X or gene Y and (2) it is resistant to antibiotics or not. Of the four bacterial strains, you only see one of the two characteristics. You will have to test the bacterial strain to find out the second characteristic.

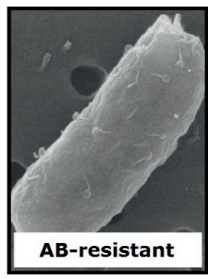
The rule is 'if the bacterial strain contains gene X, then it is resistant to antibiotics (AB)'.

Which bacterial strains do you need to test to check if the rule is correct? Choose one or more from the options, but only choose the option(s) that is/are necessary to check whether the rule is correct:

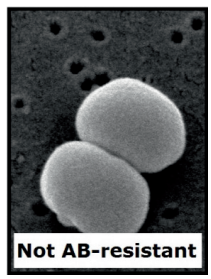


a

b



c



d

Explain briefly why you chose this answer:

Correct answer: a + d

Explanation: This assignment requires to not only confirm the rule but also look for falsification of the rule. By testing the bacterial strain with Gene X, you can test whether the rule is violated: if it is not AB-resistant, the rule is violated. The same for testing the bacterial strain that is not AB-resistant: if it contains gene X, the rule is violated. Because if it contained gene X, then it should have been resistant to antibiotics. People who choose other options than the combination of bacterial strain gene X + bacterial strain not AB-resistant probably fail to apply logical principles, verify rules rather than to falsify them, or demonstrate matching bias by selecting options explicitly mentioned in the conditional statement.

Chapter 7

Summary and general discussion

As outlined in the introduction, it is essential that higher education students are trained to become critically-thinking professionals. Critical thinking (CT) is crucial for succeeding in future careers and, moreover, is an important life skill (Davies, 2013; Facione, 1990; Halpern, 2014; Van Gelder, 2005). More specifically, students should be able to avoid biases in their reasoning and decision-making, even in situations that have not been encountered before, because especially in (professional) situations, reasoning biases can have serious consequences. However, it would be unfeasible to train students on each and every type of reasoning bias they will ever encounter. The challenge for educational practitioners, therefore, is to design instruction/practice so that students acquire the necessary resources to enhance CT in such a way that it would also transfer across tasks and contexts.

The overarching purpose of the research presented in this dissertation was to acquire more knowledge on how higher education students' learning and transfer of CT-skills can be fostered, focusing specifically on one important aspect of CT: the ability to avoid bias in reasoning. The studies in Chapters 2 to 5 examined whether instructional interventions that are known to foster generative processing and transfer of other cognitive skills, would further enhance learning and transfer of CT-skills required for unbiased reasoning (i.e., above and beyond effects of instruction and practice). These interventions were administered after initial instruction, during the practice phase. Through generative processing, learners actively construct meaning from to-be-learned information, by mentally organizing it in coherent knowledge structures and integrating these principles with existing knowledge (Grabowski, 1996; Osborne & Wittrock, 1983; Wittrock, 1974, 1990, 1992, 2010), which is required for transfer of learned skills. In addition, the study presented in Chapter 6 experimentally examined what obstacle(s) prevent(s) successful transfer of these CT-skills. In this final chapter, the main findings of the studies are presented and positioned in the broader literature first. Subsequently, the implications for research and educational practice are discussed, along with potential directions for future research.

Summary of main findings

The main question addressed in this dissertation, was to investigate how (i.e., which instructional strategies could be used) to further enhance learning of unbiased reasoning and to establish transfer to novel problem types. Instructional strategies that encourage generative processing seem to hold a considerable promise with respect to deep learning and transfer of other cognitive skills (e.g., Fiorella & Mayer, 2016; Wittrock, 2010), but their effects on the acquisition of CT-skills had hardly been investigated. The generative processing strategies investigated in the studies presented in this

dissertation were: prompting students to self-explain during practice (Chapter 2), creating variability in practice (Chapter 3), stimulating comparison of correct problem solutions with erroneous ones (Chapter 4), and having students repeatedly retrieve to-be-learned material from memory (Chapter 5). In all of these studies, students participated in a pretest-intervention-posttest design. During the intervention, students were provided with instruction on the importance and features of CT, on the skills and attitudes needed to think critically, and on specific heuristics-and-biases tasks. Subsequently, they performed practice activities on domain-relevant problems in the task category/categories they were given instructions on, either with or without the respective generative processing strategies. Unbiased reasoning has been operationalized as performance on classic heuristics-and-biases tasks (Tversky & Kahneman, 1974), in which an intuitively cued heuristic response conflicts normative models of CT as set by formal logic and probability theory. Students' performance (both on task categories that were part of the practice phase to assess learning and novel task categories that share underlying principles to assess transfer) and perceived mental effort were measured on a pretest and posttest. Additionally, Chapters 2 to 4 included delayed posttests.

The classroom study presented in **Chapter 2** addressed the question of whether prompting students to self-explain during practice; that is, to generate explanations of a problem-solution to themselves (e.g., Bisra et al., 2018; Chi, 2000; Fiorella & Mayer, 2016) would be effective for fostering (transfer of) unbiased reasoning. Students were provided with instruction on the importance and features of CT, on the skills and attitudes needed to think critically, and on several heuristics-and-biases tasks. Subsequently, they performed practice activities on domain-relevant problems in the task categories they were given instructions on, either with or without self-explanation prompts. Results revealed that learning outcomes improved after instruction/practice (i.e., from pretest to posttest) and remained stable after a two-week delay. In contrast to previous findings in a variety of domains (for a review see Bisra et al., 2018), however, prompting self-explanations had no differential effect – compared to the control condition that did not receive prompts – on learning gains or transfer performance. Remarkably, mental effort investment did not differ across conditions. That raises the possibility that students in the control condition had also engaged in generative processing, for instance by covertly trying to come up with explanations for the questions. Additionally, it was explored whether the quality of students' self-explanations was related to their performance. Results indicated that this was the case: learners who gave lower quality self-explanations also performed worse on the learning (but not on transfer) items on the test, which seems to corroborate the idea that a higher quality of self-explanations is related to higher performance (Schworm & Renkl, 2007). It is possible, however, that this finding

reflects a priori knowledge or ability difference rather than an effect of the quality of self-explanations on performance.

In **Chapter 3**, two experiments (laboratory and classroom) tested whether creating variability during practice through *interleaved practice* (in which practice task categories vary from trial to trial, as opposed to blocked practice; e.g., Barreiros et al., 2007; Helsdingen et al., 2011; Rau et al., 2013) would be effective for fostering unbiased reasoning. While interleaved practice has been shown to enhance learning (e.g., Helsdingen et al., 2011a, 2011b) it is usually more cognitively demanding than blocked practice, and a very high cognitive load may hinder learning (Paas et al., 2003a). Therefore, it was additionally examined whether learners would experience lower cognitive load and benefit more from interleaved practice, when using worked examples as opposed to practice problems (cf. Paas & Van Merriënboer, 1994). Worked examples have been shown to reduce ineffective cognitive load (compared to practice problems; Van Gog et al., 2019). After receiving explicit instruction on CT and specific heuristics-and-biases tasks, students either practiced in an interleaved schedule with worked examples, an interleaved schedule with problems, a blocked schedule with worked examples, or a blocked schedule with problems. In both experiments, learning outcomes again improved after instruction/practice (i.e., from pretest to posttest). However, contrary to expectations and previous findings (e.g., Barreiros et al., 2007; Likourezos et al., 2019; Moxley, 1979), there were no indications that interleaved practice led to better learning or transfer than blocked practice, irrespective of task format. Interestingly, the laboratory experiment demonstrated a benefit of studying worked examples over solving problems on learning outcomes, reached with less effort during the tests (i.e., more effective and efficient: Hoffman & Schraw, 2010; Paas & Van Merriënboer, 1993; Van Gog & Paas, 2008). The classroom experiment replicated this worked example efficiency and demonstrated that this was the case for novices, but not for learners with relatively more prior knowledge. Hence, these experiments were the first to show that the worked example effect also applies to novices' training of CT-skills (e.g., Paas & Van Gog, 2006; Renkl, 2014; Van Gog et al., 2019). The observation from the second (i.e., classroom) experiment also supports findings regarding the expertise reversal effect (e.g., Kalyuga et al., 2003, 2012), which shows that while instructional strategies that assist learners in developing cognitive schemata are effective for low-knowledge learners, they are often not effective for higher-knowledge learners.

The classroom study reported in **Chapter 4** investigated whether comparing correct and erroneous examples (i.e., contrasting examples) would enhance unbiased reasoning more than studying correct examples only, studying erroneous examples only, and solving practice problems. Students were provided with the CT-instructions and practice

on domain-relevant problems, under one of the four conditions. Results revealed that students' learning outcomes again improved from pretest to posttest. Moreover, their performance improved even further after a three-week and nine-month delay, although the latter finding could also be attributed to the further instructions that were given in courses in-between the three-week and nine-month follow up. Unexpectedly, however, results did not reveal any differences among conditions on either learning outcomes or transfer performance and, thus, differ from findings of previous studies (e.g., Durkin & Rittle-johnson, 2012; Kawasaki, 2010; Loibl & Leuders, 2018, 2019; Siegler, 2002). Moreover, it is surprising that this study did not reveal a beneficial effect of studying correct examples as opposed to practicing with problems (i.e., worked example effect), which is contrary to the finding in Chapter 3 and findings of previous studies on many other tasks (e.g., Renkl, 2014; Van Gog et al., 2019).

In **Chapter 5**, a classroom study was described that empirically investigated whether repeated retrieval practice over time (i.e., working on practice tasks in sessions that were weeks apart), would be beneficial for learning to reason in an unbiased manner and whether it can additionally facilitate transfer. Students were instructed on CT and avoiding belief-bias in syllogistic reasoning and practiced with syllogisms on domain-relevant problems. After each practice-task, they received correct-answer feedback and were given a worked example. Depending on assigned condition, they did not engage in extra practice, practiced a second time (week later), or practiced a second (week later) *and* third time (two weeks after second time). Consistent with previous repeated retrieval findings (e.g., Butler, 2010; McDaniel et al., 2012, 2013; Roediger & Butler, 2011), results revealed that average performance scores *during* practice sessions increased with more repetitions. However, repeated retrieval practice did not have a significant effect, compared to practicing just once, on learning outcomes on the final test, as judged by total scores (MC-answers plus justification). Exploring performance on MC-answers only revealed pretest to posttest learning gains, suggesting that students did benefit from instruction/practice but may have been unable to justify their answers. Effects on transfer could not be tested due to a floor effect. It seems possible that the feedback after each practice task eliminated the effects of repeated retrieval, in line with findings from recent research (Kliegl et al., 2019; Pastötter & Bäuml, 2016; Storm et al., 2014), since students spent more time on worked-example feedback after incorrect than correct retrievals.

The study in **Chapter 6** focused exclusively on identifying whether unsuccessful transfer of CT-skills would be due to a failure to recognize that acquired knowledge is relevant in a new context, to recall that knowledge, or to apply that knowledge to the new context (i.e., the three-step model of transfer; Barnett & Ceci, 2012). In two experiments

(classroom and laboratory), students received explicit instructions on CT and avoiding belief-bias in syllogistic reasoning and practiced with syllogisms on domain-relevant problems. This time, students' performance was measured on syllogisms with different story contexts (to assess learning), syllogisms in a different format (to assess near transfer), and novel tasks that shared similar features with syllogisms (to assess far transfer) both on a pretest and immediate posttest. On the posttest transfer items, students received no support, received hints that the information provided in the learning phase is relevant for these items (recognition support), received hints that the information provided in the learning phase is relevant and were prompted to recall the acquired knowledge (free recall), or received hints that the information provided in the learning phase is relevant and receiving a reminder of the paper-based overview of that information that they received (recall support). The effects of support for different steps in the process were compared to infer where difficulties arise for learners (cf. Butler et al, 2013, 2017). Additionally, it was explored (within the free recall condition) whether students' ability to recall the acquired knowledge was related to their posttest performance on near and far transfer items. Over the two experiments, learning and near transfer outcomes improved after instruction/practice (i.e., from pretest to posttest). Results even showed some increase on far transfer items, but the far transfer scores were overall rather low, so there was still a lot of room for improvement. Interestingly, students did not benefit from recognition and recall support while solving transfer tasks (i.e., there were no significant differences among conditions). This finding suggests that students were able to recognize that the acquired knowledge was relevant to the new task and to recall that knowledge, but had difficulties in applying the relevant knowledge to the new tasks. However, findings from the free recall condition do not fully support the idea that it is only an application/mapping problem. Most students did not retrieve all relevant information from memory, and exploratory analyses pointed to moderate-to-large positive correlations between students' retrieved knowledge and their performance on near and far transfer items. This may suggest that suboptimal recall is at least partially responsible for unsuccessful transfer as well. Descriptive statistics support this idea: students who received recall support had higher (though not significantly higher) scores than the other conditions on far transfer items at posttest in the laboratory study and on near transfer items at posttest in the classroom study.

Discussion of main findings

Together, the studies in this dissertation seem to corroborate findings of previous studies on teaching CT in general (Abrami et al., 2014, 2018) and unbiased reasoning in particular (Heijltjes et al., 2014a, 2014b, 2015) that providing students with explicit CT-instruction and the opportunity to practice with domain-relevant problems improves learning outcomes. Although we did not include a no-instruction/practice control condition, students did show pretest to posttest performance gains on practiced/instructed items, and their performance remained stable or improved even further after a delay of (several) weeks (Chapters 2 to 4). Regarding the effect of instruction/practice on transfer, the study in Chapter 6 showed a noticeable progress on near transfer from pretest to posttest, that is, after instructions and practice activities[12]. However, there were no or very limited indications of progress on far transfer (Chapters 2 and 6, respectively)[13], which is in line with findings of previous studies that examined effects on *far* transfer (Heijltjes et al., 2014a, 2014b, 2015). Taken together, this research extends prior research on teaching for transfer of CT-skills and confirms that transfer between closely related situations occurred more often than transfer between situations that had less in common (Barnett & Ceci, 2002; Bray, 1928; Dinsmore et al., 2014).

Remarkably, the generative processing strategies did not work as expected: Chapters 2 to 5 found no indications that these strategies – be it self-explaining during practice, interleaved practice, comparing correct and erroneous examples, or repeated retrieval practice – further improved learning or transfer of CT-skills. It has been well established that encouraging generative processing fosters knowledge acquisition and transfer of various cognitive skills (e.g., Fiorella & Mayer, 2016; Wittrock, 2010). As such, it is somewhat surprising that generative processing strategies did not seem beneficial for fostering CT-skills.

There are several possible explanations for this absence of differential effects of generative processing strategies on learning and transfer. The possible strategy-specific explanations and preconditions have been addressed in the respective chapters, so I will not repeat them here, but instead, I will focus on the overarching issues. First, it seems possible that the CT-instructions, which included worked examples, already had a substantial effect on learning unbiased reasoning, making it difficult to find differential effects of different types of practice activities. Most studies on the effects of generative processing strategies with other types of cognitive tasks use pure practice conditions or

[12] Near transfer items were only included in the tests of the study presented in Chapter 6.

[13] The studies presented in Chapters 3, 4, and 5 did not include transfer items in the pretest and were, therefore, not able to detect transfer gains.

give minimal instructions prior to practice (e.g., Fiorella & Mayer, 2016). Thus, the effects are usually not investigated in a context in which elaborate processing of instructions precedes practice, as in the studies in this dissertation.

Second, the absence of differential effects of generative processing on learning may be related to the affective and attitudinal dimension of CT. Being able to think critically relies on the extent to which one possesses the requisite skills and is able to use these skills, but also on whether one is inclined to use these skills (i.e., thinking dispositions; Perkins et al., 1983). It is possible, for instance, that generative processing would only benefit students who score high on thinking dispositions (such as need for cognition, Cacioppo & Petty, 1982, or actively open-minded thinking, Stanovich & West, 2007). A possible interaction between generative processing strategies and thinking dispositions could not be investigated in this dissertation, however, because thinking dispositions were not assessed.

Third, in some studies, the classroom setting might explain why there were no differential effects of generative processing. In Chapter 2, I already pointed to the possibility that because the study was conducted in an existing CT-course (as in all classroom studies part of this dissertation), students' willingness to invest effort in their performance may have been higher than generally in psychological laboratory studies. The learning materials from the study were also relevant for the course/exam and their performance actually mattered (intrinsically or extrinsically) to them. Not so much on the posttest of this study, which did not have consequences for their exam grade, but on such tasks in general. As such, students in the control condition may have engaged in generative processing themselves, for instance by covertly trying to come up with explanations for the questions. It is therefore possible that effects of generative processing strategies such as self-explaining found in the psychological laboratory – where students participate to earn required research credits and the learning materials are not part of their study program – might not readily transfer to field experiments conducted in real classrooms. This could be a possible explanation for the lack of effects of contrasting examples (Chapter 4) as well, in which the control conditions may have tried to compare the given correct (or erroneous) examples with internally represented erroneous (or correct) solutions. I will discuss recommendations for future research based on this assumption later in this chapter. It should be noted though, that the above argument probably cannot fully explain the absence of differential effects of interleaved practice (Chapter 3) and repeated retrieval practice (Chapter 5), where motivational aspects are less crucial. To illustrate, in Chapter 3, students in the control condition practiced in a blocked schedule and could not easily engage in interleaved practice themselves. Moreover, Chapter 3 included both a classroom *and* laboratory study and consistently demonstrated a lack of differential effects and, therefore, the classroom setting argument

cannot fully explain the absence of differential effects of this generative processing strategy.

A possible reason for the lack of transfer to novel problem types in general, might be related to the duration or extensiveness of the practice activities. Even though substantial evidence is provided that students learned to solve abstract heuristics-and-biases tasks (Chapters 2 to 6) and tasks closely related to those instructed (Chapter 6), their subject-matter knowledge may have been insufficient for solving more complex or novel CT-tasks. That might explain the considerably low levels of performance on far transfer items in all chapters. As such, it can be argued that establishing transfer to novel problem types needs longer or more extensive practice. Additionally, Chapter 6 implies that instructional interventions aimed at far transfer of CT-skills should focus on recall of the acquired knowledge and application of that knowledge onto novel tasks, since students seem to have most difficulty with these steps in the transfer process (for the three-step model of transfer, see Barnett & Ceci, 2012). These explanations are not mutually exclusive and should be investigated further in future research. I will elaborate on this when giving suggestions for future work. Nonetheless, the series of studies presented in this dissertation do show – contrary to the assertion made by Halpern and Butler (2019) that teaching CT-skills explicitly with multiple examples from different contexts will facilitate transfer to novel contexts – that establishing transfer of CT-skills to novel *problem types* is no easy feat, at least with regard to skills required for unbiased reasoning.

Taken together, providing students with explicit CT-instruction and opportunities to practice with domain-relevant problems is beneficial for learning unbiased reasoning, but what kind of practice activity does not seem to matter. The latter finding may be explained by the magnitude of the effect of the CT-instruction itself, the nature of the practice tasks (i.e., heuristics-and-biases tasks), and/or the setting of the experiments. Furthermore, findings suggest that these instructions and practice opportunities may also enhance near transfer, but are not sufficient to establish further transfer. As such, it can be suggested that bringing about far transfer needs longer or more extensive practice, in which obstacles such as suboptimal recall and application should be countered.

Methodological issues

Several methodological issues need to be discussed. Again, I will focus on the overarching issues as study specific issues have been addressed in each chapter. First, the measures in Chapters 2 to 4 showed low levels of reliability. Reliability issues are

quite common in research using tests consisting of heuristics-and-biases tasks (Aczel et al., 2015b; Bruine de Bruin, 2007; Janssen et al., 2019a; West et al., 2008) and multiple studies revealed concerns with the reliability of widely used standardized CT tests, particularly with regard to subscales (Bernard et al., 2008; Bondy et al., 2001; Janssen et al., 2020; Ku, 2009; Liu et al., 2014; Leppa, 1997; Loo & Thorpe, 1999; Rear, 2019). Low levels of reliability decrease statistical power and, thereby, reduce the chance of detecting true effects (e.g., Cleary et al., 1970; Rogers & Hopkins, 1988). Furthermore, given that the point estimates of the crucial interaction effects appeared to be very small, these may have been difficult to detect.

In this dissertation, the low levels of reliability can probably be explained in terms of multidimensionality of the tests encompassing several heuristics-and-biases tasks, a factor often ignored in current research. That is, when tests represent multiple constructs that do not correlate with each other. As alluded to earlier, performance on such tasks depends not only on the extent to which that task elicits a bias (resulting from heuristic reasoning), but also on the extent to which one possesses the requisite mindware. Thus, systematic variance in performance on such tasks can either be explained by a person's use of heuristics or his/her available mindware. If it differs per item to what extent a correct answer depends on these two aspects, there may not be a common factor explaining all interrelationships between the measured items. In that case, the theoretical assumption of unidimensionality is violated.

In the research presented in this dissertation, the general reliability issue may have increased even more since multiple task types were included in the CT-skills tests, requiring different types of mindware (e.g., rules of logic or probability). Hence, I have attempted to increase reliability of the measures in Chapters 5 and 6, by constructing tests with multiple items of one task category to narrow down the tests into single measurable constructs and, thereby, to decrease measurement error (LeBel & Paunonen, 2011). Indeed, these compositions led to quite reliable measures. However, even though biased reasoning is a very important aspect of CT, it is already a rather restricted operationalization and this focus on one task category narrowed it even further. To achieve further progress in research on instructional methods for teaching CT, more knowledge on the construct validity of CT in general and unbiased reasoning is needed, and reliable (aspect-specific) tests of CT should be developed. That seems challenging, however, especially given that for practical use in educational contexts, tests cannot be overly long.

Along with the issues raised, it should be considered to what extent the tests in the research reported in this dissertation and previous research by others, accurately assessed CT as it is practised in the real world, that is, outside education. I would argue

that the current findings do provide valuable insights into how people reason, given that heuristics-and-biases tasks represent how people judge under uncertainty and in various contexts; heuristics and biases appear in newspapers, books, courses, and applications of many kinds. Especially since in this dissertation – contrary to standardized CT-tests and most research on heuristics-and-biases tasks – CT was assessed at the level of individual study domains (i.e., content of the tasks was adapted to specific study domains) and could, therefore, be evaluated within authentic contexts. To illustrate, in Chapter 6, students' ability to evaluate the logical validity of arguments in a written news item or article on a topic that they might encounter in their working life, was assessed. Hence, performance on these tasks could presumably predict everyday reasoning, as has already been assumed in various studies (see for example, Gilovich et al., 2002).

A strength of the research presented in this dissertation, is that it follows the standards of an open research culture by using open practices. Open practices are designed to make scientific processes and results more transparent and accessible to others than the researchers involved (e.g., Nosek et al., 2015). It includes making complete research materials, designs, and data freely available to anyone, which makes it easier to replicate and evaluate scientific findings (for instance because both null results and statistically significant results are accessible). Although transparency and openness are readily recognized as disciplinary norms and values, scientific practice often fails to adhere these valued features (Ioannidis et al., 2014; John et al., 2012; Open Science Collaboration, 2012). Practicing open science has been central to the research reported in this dissertation. The study presented in Chapter 2 has already been published open access[14] and, for all studies, important aspects of the research design and data analyses are publicly available on the online repository 'Open Science Framework' (OSF).

Furthermore, this dissertation is strengthened by the fact that some of the studies have been preregistered on the OSF repository (Chapters 5 and 6), with specific details such as the hypotheses, planned analyses, and rules for data exclusions recorded prior to the data-analyses. The practice of pre-registration was introduced in response to some serious issues in academic publishing. These included, for instance, the use of 'questionable research practices' by individual researchers, such as manipulating statistics to obtain significant effects (*p*-hacking) and hypothesizing after the results are known (HARKing; John et al., 2012; Simmons et al., 2011). Both open practices and pre-registrations help to more accurately assess the evidence base for phenomena and are, therefore, imperative to increase confidence in scientific findings.

[14] In time, the studies presented in the other chapters will be publicly available as well, through publications in open access journals or preprints on the OSF-repository.

Implications for practice and future directions

Educational practice and future research could benefit from the findings presented in this dissertation both from a theoretical and practical point of view. The findings clearly indicate that providing students with explicit CT-instruction and the opportunity to practice with domain-relevant problems has the potential to improve learning. It is important to emphasize, however, that there is no one-size-fits-all recommendation in terms of best practice activity. These acquired insights advocate for CT integration in higher education curricula and explicit CT objectives at course level (i.e., CT as important learning outcome), for instance through explicit CT-courses. Acquisition of requisite mindware was particularly central to this dissertation, but perhaps instructional designs should pay more attention to changing students' thinking dispositions. To illustrate, a student who masters CT-skills but is unwilling to put in the mental effort to use these skills on complex or novel CT-tasks, will be no better off than a student without these CT-skills. Investigating the exact role of students' thinking dispositions in fostering unbiased reasoning and developing an approach aimed at improving *both* aspects will be a fruitful area for further work. Enhancing thinking dispositions may require building a certain culture of thinking in the classroom, in which students are exposed to models of thinking of fellow students, supported in cultural interaction, and provided with direct instructions on thinking dispositions (cf. enculturation model; Tishman et al., 1993). Also, future research could explore whether changes as complex as these may be realized through personalized approaches, such as personalized feedback (Marsh & Eliseev, 2019). It is important, then, to ensure that students believe in and process that feedback (Rich et al., 2017), which, however, is often left to the discretion of students.

To specifically address development of deep learning of CT-skills, instructional design studies as in this dissertation could be preceded by research that identifies the exact factors that help or hinder learning of that explicit CT-skill. Chapter 6 is a useful example of how to design such a study. This chapter provided initial insights into the obstacles that prevent successful transfer of overturning belief-biased responses when evaluating the logical validity of arguments; students seem to have most difficulty with recall of the acquired knowledge and application of that knowledge onto novel tasks. Future studies should therefore focus on the recall and application/mapping steps in the transfer process. However, it could not be determined from this study *why* students have difficulties with these steps in the transfer process, which should be addressed in future investigations. Furthermore, the question of how to facilitate transfer of CT-skills remains of interest. Assuming that unsuccessful transfer of CT-skills can be attributed to recall and application/mapping problems, the challenge for researchers and educational practitioners (e.g., consultants, teachers) in the CT-domain is to develop instructional

designs that focus on these steps in the transfer process. A possible direction could be to provide exemplars of knowledge application while gradually remove scaffolding (cf. four-component instructional design model; Van Merriënboer et al., 1992) or while fading from concrete-to-abstract situations (i.e., concreteness fading; McNeil & Fyfe, 2012).

More broadly, a key challenge of classroom studies is to prevent noisy or incomplete data produced by these realistic settings (e.g., Hulleman & Cordray, 2009), which would make it more difficult to detect any (small) effect. Issues as these can be (at least partially) addressed by using large sample sizes and collecting multiple data points per participant. Moreover, to increase the impact, transfer, and translation of education research into improved practice, it seems promising to additionally conduct instructional design research within even more realistic settings than in this dissertation (e.g., through education design research; McKenney & Reeves, 2018). Education design research blends empirical investigation with systematic development and implementation of solutions, such as improved instructional designs, for educational problems. To establish transfer of CT-skills, a longer, but carefully structured, intervention based on principles derived from prior fundamental research may be needed. A comprehensive CT-course (that fosters the cultivation of both CT-skills and thinking dispositions) can possibly meet these needs, which takes long-term studies in realistic settings to test its effectiveness.

All of this assumes, of course, that those who teach CT are equipped with the knowledge and skills needed to effectively teach unbiased reasoning (e.g., Elen et al., 2019; Klassen & Tze, 2014). The challenge is for educators to know what is needed, and when. Furthermore, for educators to teach CT, they need to consider teaching CT as relevant (e.g., Eccles & Wigfield, 2002; Elen et al., 2009) and should have confidence in their ability to teach CT (Janssen et al., 2019b). To achieve this ambitious goal, we can facilitate educators by including (teaching/explaining) CT in professional development programs (e.g., Janssen et al., 2019a) and sharing CT resources that they could use in their own courses (e.g., Dutch online platform Kritisch Leren Denken: https://kritischdenkenhbo.nl/).

Conclusion

This dissertation sheds light on fostering higher education students' learning and transfer of CT-skills, focusing specifically on avoiding bias in reasoning. The evidence presented highlights the importance of explicit CT-instruction and practice opportunities for learning of these skills. It also demonstrated that generative processing is not a panacea for all kinds of learning tasks: it does not seem to improve learning and transfer of CT-

skills required for unbiased reasoning. This dissertation again underlines the great difficulty encountered when seeking to enhance CT-skills in such a way that these would also transfer across tasks/domains. All things considered, to help students become good critical thinkers in the sense they can apply the acquired skills to a variety of tasks and contexts, it seems valuable to develop longer CT interventions or comprehensive CT courses. To conclude, I believe further progress in this area will come from instruction designs that are grounded in solid laboratory and classroom studies.

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Samenvatting

Summary in Dutch

Elke dag nemen we allerlei beslissingen en vellen we oordelen. Wanneer je de trein in stapt, maak je bijvoorbeeld een besluit waar je gaat zitten. En hoor je in die trein iemand veelvoudig niezen, dan ga je er momenteel al snel van uit dat diegene een virus opgelopen heeft. Doordat we vaak beperkt zijn in de tijd en in de hoeveelheid informatie die we tot onze beschikking hebben, maken we in ons denken gebruik van *heuristieken* (ofwel vuistregels). Heuristieken helpen ons om de grote hoeveelheid informatie die we dagelijks tegenkomen aan te kunnen en om redeneerprocessen te vereenvoudigen. Daardoor kunnen we relatief snel beslissingen nemen, vaak zonder dat we ons er bewust van zijn. Maar heuristieken maken ons ook vatbaar voor systematische redeneerfouten, die ook wel *biases* worden genoemd (Tversky & Kahneman, 1974). Wanneer men bijvoorbeeld gevraagd wordt om in te schatten of het waarschijnlijker is dat iemand overlijdt aan het coronavirus of dat iemand overlijdt aan het coronavirus én ouder is dan 70 jaar, dan zal de intuïtieve reactie van de meeste mensen zijn dat de tweede optie waarschijnlijker is. Doordat de relatie tussen overlijden aan het coronavirus en het hebben van een hogere leeftijd vaak genoemd wordt en dus herkenbaar is, hebben we de neiging om de kans op deze combinatie te overschatten: we maken in dit geval gebruik van de *representativiteitsheuristiek* (Tverksy & Kahneman, 1983). De kans dat een bepaalde combinatie van gebeurtenissen voorkomt is echter áltijd kleiner dan de kans dat slechts een van deze gebeurtenissen voorkomt. Het is dus waarschijnlijker dat iemand overlijdt aan het coronavirus, dan dat diegene ook nog ouder is dan 70 jaar. In dit geval leidt het gebruik van een heuristiek dus tot een systematische redeneerfout.

Het gebruik van heuristieken kan leiden tot redeneerfouten met ernstige gevolgen. Zeker in de complexe beroepssituaties waarin de meeste afgestudeerden in het hoger onderwijs terecht komen, zoals in de medische, economische of juridische sector. Denk bijvoorbeeld aan het verkeerd toedienen van medicatie, het geven van onjuist financieel advies of het onterecht veroordelen van een verdachte voor een strafbaar feit. Het tegengaan van systematische redeneerfouten vereist dat een intuïtieve reactie wordt onderdrukt en wordt vervangen door een rationele reactie, die gebaseerd is op redeneerregels of -strategieën (uit de logica en waarschijnlijkheidstheorie; Kahneman & Tversky, 1972, 1973; Tversky & Kahneman, 1974). Ofwel, dat je kritisch kunt denken. Kritisch denken betekent, kort gezegd, dat je “redeneert en reflecteert voordat je een standpunt inneemt of een besluit neemt hoe te handelen en dat je kunt verklaren waarop dat standpunt of het besluit is gebaseerd”[15]. Het is dus van belang dat studenten in het hoger onderwijs worden opgeleid tot kritisch denkende professionals (Davies, 2013;

[15] Deze werkdefinitie is gebaseerd op de toonaangevende definitie van kritisch denken voor onderwijs en onderzoek, die is opgesteld door een panel van deskundigen: “kritisch denken wordt beschouwd als het vermogen om doelgericht, zelfregulerend te oordelen, resulterend in interpretatie, analyse, evaluatie en gevolgtrekking, alsook het verklaren waarop dat oordeel is gebaseerd in termen van bewijzen, concepten, methodes, criteria en contextuele overwegingen” (APA: Facione, 1990, p.2).

Facione, 1990; Halpern, 2014; Van Gelder, 2005). Een belangrijk kenmerk van een kritisch denkende professional is dat hij/zij in staat is om onbevooroordeeld te redeneren en beslissingen te nemen, zonder systematische redeneerfouten te maken. Er bestaan echter tal van systematische redeneerfouten en het is niet haalbaar om studenten te trainen in het vermijden van elk type redeneerfout. Daarom is het de uitdaging leeractiviteiten zó te ontwerpen dat de kritisch-denken-vaardigheden van studenten niet alleen verbeteren op de getrainde redeneertaken in een gegeven context, maar dat de geleerde vaardigheden ook tot verbetering leiden op andersoortige redeneertaken of tot een verbetering van de getrainde redeneertaken in een andere context. Met andere woorden, het doel van kritisch-denken-instructie is onder meer dat er *transfer* optreedt van de geleerde vaardigheden naar nieuwe taken en situaties. De vraag die in dit proefschrift centraal stond, was dan ook hoe kritisch-denken-instructie het beste kan worden vormgegeven om ervoor te zorgen dat studenten in het hoger onderwijs (1) leren om systematische redeneerfouten te vermijden en (2) het geleerde kunnen toepassen op nieuwe redeneertaken en in nieuwe situaties (transfer).

Om nieuwe leerstof langere tijd te onthouden en te kunnen toepassen in nieuwe situaties, moet de leerstof actief verwerkt worden. Zogenoemde 'generatieve verwerkingsstrategieën' (Engels: *generative processing strategies*) kunnen hieraan bijdragen: ze vereisen van studenten dat zij extra inspanningen leveren tijdens het leren (bijvoorbeeld door het genereren van verklaringen of vergelijkingen) en zorgen ervoor dat betekenis wordt gegeven aan de leerstof. Generatieve verwerking helpt om informatie in het geheugen te organiseren in samenhangende kennisstructuren en te integreren met reeds aanwezige kennis (Grabowski, 1996; Osborne & Wittrock, 1983; Wittrock, 1974, 1990, 1992, 2010). Bovendien kan het studenten helpen om de onderliggende principes van een probleem te identificeren en te leren welke oplossingsprocedure voor dit type probleem nodig is. Als een nieuw probleem vervolgens hetzelfde onderliggende principe heeft en de student herkent dit, dan kan hij/zij de aangeleerde procedure gebruiken om het nieuwe probleem op te lossen. Er treedt dan transfer op. Generatieve verwerkingsstrategieën zijn effectief gebleken voor het leren en de transfer van diverse vaardigheden (zie bijv. Fiorella & Mayer, 2016; Wittrock, 2010), maar het was nog onduidelijk of ze ook helpen bij het leren om systematische redeneerfouten te vermijden. In de studies in hoofdstuk 2 tot en met 5 werd daarom onderzocht of generatieve verwerkingsstrategieën eveneens het leren en de transfer van kritisch-denken-vaardigheden verder verbeteren (bovenop de effecten van instructie en oefening). In de studie in hoofdstuk 6 is daarnaast onderzocht welke factoren succesvolle transfer van kritisch-denken-vaardigheden belemmeren.

De generatieve verwerkingsstrategieën die in dit proefschrift zijn onderzocht, waren: studenten aansporen om aan zichzelf hun redeneerproces uit te leggen tijdens het

oefenen, ook wel 'zelfverklaren' genoemd (hoofdstuk 2); variatie aanbrengen in taaktypen tijdens oefening, waarmee vergelijkingen tussen taken maken (impliciet) wordt aangemoedigd (hoofdstuk 3); studenten stimuleren om correcte en incorrecte 'uitgewerkte voorbeelden' te vergelijken (hoofdstuk 4); en op meerdere momenten oefentaken aanbieden aan studenten, zodat zij die informatie herhaaldelijk uit hun geheugen moeten ophalen (hoofdstuk 5). De effecten van deze strategieën, ofwel interventies, werden getest in experimenten die plaatsvonden in de praktijk van het hoger onderwijs. Zo werd in de studie in hoofdstuk 2 één groep studenten aangezet tot zelfverklaren. In elke studie was er minstens één controlegroep, die een andere of geen interventie kreeg. Eerst werd bij alle groepen een voormeting (pretest) afgenomen. Vervolgens kregen ze instructies over kritisch denken (het belang en de kenmerken van kritisch denken en de vaardigheden en houding die nodig zijn om kritisch te denken) en over specifieke heuristics-and-biases taken, vergelijkbaar met het 'coronavirusvoorbeeld' aan het begin van dit hoofdstuk. Daarna werd er geoefend, al dan niet met extra interventie. Direct na het oefenen werd er een nameting (posttest) afgenomen. In elk hoofdstuk werd dus op tenminste twee momenten de mate van onbevooroordeeld redeneren en de mentale inspanning gemeten (pretest en posttest). Onbevooroordeeld redeneren werd in kaart gebracht door prestatie op heuristics-and-biases taken van de test te bepalen, zowel voor taakcategorieën die deel uitmaakten van de oefenfase (hiermee werd het *leren* gemeten) als voor nieuwe taakcategorieën met dezelfde onderliggende principes (hiermee werd *transfer* gemeten). In hoofdstuk 2 en 4 werden studenten tevens op een later moment, tussen de twee weken en negen maanden, getest (verlate posttest).

De hoofdbevindingen

In hoofdstuk 2 werd onderzocht of het aanzetten van studenten tot zelfverklaren, ofwel het aan zichzelf uitleggen van hun redeneerproces (Bisra et al., 2018; Chi, 2000; Fiorella & Mayer, 2016) tijdens het oefenen, effectief zou zijn voor het leren en de transfer van de vaardigheid om systematische redeneerfouten te vermijden. De studie werd uitgevoerd in de context van een hbo-vak. De studenten ontvingen eerst de instructies over kritisch denken. Vervolgens oefenden ze met een aantal taken in de context van domeinrelevante problemen. Dat wil zeggen dat de taken realistische problemen bevatten uit het studiedomein van de studenten, in dit geval Integrale Veiligheidskunde. Tijdens het oefenen werd de helft van de studenten gevraagd om zelfverklaringen te genereren. Uit de resultaten bleek dat de prestaties van studenten op de leertaken verbeterden van pretest naar posttest en dat dit prestatieniveau na twee weken nog even hoog was. Dat wil zeggen dat studenten na de instructie en het oefenen beter in staat waren om systematische redeneerfouten te vermijden op de leertaken. Er was echter geen verschil in prestaties op de leer- en transfertaken tussen de groep die werd aangezet tot het genereren van zelfverklaringen en de controlegroep die niet werd

aangezet tot zelfverklaren. Bovendien was het opmerkelijk dat de mentale inspanning die studenten leverden tijdens het maken van de taken niet verschilde tussen de twee groepen. Mogelijk hebben de studenten in de controlegroep ook generatieve verwerkingsprocessen gebruikt, bijvoorbeeld door spontaan zelfverklaringen te genereren. In dit hoofdstuk is daarnaast onderzocht of de kwaliteit van de zelfverklaringen van de studenten gerelateerd was aan hun prestaties op de leer- en transfertaken. Dit was inderdaad het geval: de studenten die verklaringen van hogere kwaliteit gaven, presteerden ook beter op de leertaken op de posttest (maar niet op de transfertaken). Eenvoudiger gezegd, de studenten die beter waren in het aan zichzelf uitleggen van hun redeneerproces, presteerden ook beter. Deze correlatie duidt wellicht op een causaal verband: door aan zichzelf hun eigen redeneerproces uit te leggen, gaan studenten beter presteren. Het kan echter ook zo zijn dat studenten met een hoger algemeen kennis- of vaardigheidsniveau betere zelfverklaringen genereren en beter presteren dan mensen met een lager kennisniveau.

In hoofdstuk 3 werd onderzocht of het creëren van variatie in oefening effectief zou zijn voor het bevorderen van leren en transfer. Er werden twee experimenten uitgevoerd; een met universitaire studenten in het laboratorium en een met hbo-studenten in de context van een vak. De variatie in oefening werd gecreëerd door de oefentaken af te wisselen die betrekking hadden op verschillende redeneerfouten (Engels: *interleaved practice*; bijv. Barreiros et al., 2007; Helsdingen et al., 2011; Rau et al., 2013) in plaats van de oefentaken gegroepeerd per redeneerfout aan te bieden (Engels: *blocked practice*). Bij een gevarieerd oefenschema werden de verschillende type oefentaken dus afgewisseld – ABACBCAABC – terwijl bij een gegroepeerd oefenschema blokken met dezelfde type oefentaken werden aangeboden – AAA-BBB-CCC. Afwisseling in taaktypen is belangrijk om studenten te leren verschillende oplossingsprocedures te gebruiken: bij elke taak moet immers het type probleem en een passende oplossing herkend worden. Hoewel dit bijdraagt aan betere prestaties op de langere termijn (bijv. Helsdingen et al., 2011a, 2011b), doet het een groter beroep op het werkgeheugen dan oefenen in een gegroepeerd schema. Omdat een te hoge werkgeheugenbelasting het leren kan belemmeren (Paas et al., 2003a), is tevens onderzocht of studenten meer zouden profiteren van afwisseling in taaktypen, wanneer ze uitgewerkte voorbeelden bestudeerden in plaats van dat ze oefenproblemen oplosten (vgl. Paas & Van Merriënboer, 1994). Het bestuderen van uitgewerkte voorbeelden – dit zijn oefeningen waarvan de oplossing volledig is uitgeschreven – leidt namelijk tot een lagere belasting van het werkgeheugen, terwijl de leerprestaties gelijk blijven of zelfs verbeteren (Van Gog et al., 2019). Nadat de studenten de instructie over kritisch denken ontvingen, volgden zij of (1) een gevarieerd oefenschema met uitgewerkte voorbeelden, of (2) een gevarieerd oefenschema met probleem-oplostaken, of (3) een gegroepeerd oefenschema met uitgewerkte voorbeelden of (4) een gegroepeerd oefenschema met

probleem-oplostaken. In beide experimenten verbeterden de prestaties op de leertaken opnieuw na de instructie en het oefenen. Er waren echter geen aanwijzingen dat een gevarieerd oefenschema tot betere prestaties leidde op de leer- of transfertaken dan een gegroepeerd oefenschema, ongeacht of er geoefend werd met uitgewerkte voorbeelden of probleem-oplostaken. Een interessante bevinding uit het experiment met de universitaire studenten was dat het bestuderen van uitgewerkte voorbeelden tot betere prestaties op de leertaken leidde dan het oplossen van oefenproblemen. Bovendien werden deze prestaties bereikt met minder mentale inspanning tijdens de tests; dat wil zeggen dat de uitgewerkte voorbeelden zowel effectiever als efficiënter waren (Hoffman & Schraw, 2010; Paas & Van Merriënboer, 1993; Van Gog & Paas, 2008). Het tweede experiment met de hbo-studenten repliceerde dit positieve effect van uitgewerkte voorbeelden maar alleen bij beginners (studenten die nog weinig of geen voorkennis hadden) en niet bij meer gevorderden. Deze experimenten toonden daarmee voor het eerst aan dat het effect van uitgewerkte voorbeelden ook van toepassing is op het trainen van kritisch-denken-vaardigheden van beginners. De bevinding uit het tweede experiment is bovendien in lijn met het *expertise reversal effect* (bijv. Kalyuga et al., 2003, 2012), dat stelt dat instructiestrategieën die studenten helpen bij het ontwikkelen van cognitieve schema's effectief zijn wanneer studenten hun eerste stappen zetten in het verwerven van nieuwe kennis of vaardigheden, maar vaak niet als zij al meer gevorderd zijn.

In hoofdstuk 4 stond het vergelijken van correcte en incorrecte uitgewerkte voorbeelden (ook wel contrasterende voorbeelden genoemd) centraal. Er werd onderzocht of (1) het bestuderen van contrasterende voorbeelden zou zorgen voor een grotere verbetering in de vaardigheid om systematische redeneerfouten te vermijden dan (2) het alleen bestuderen van correcte voorbeelden, of (3) het alleen bestuderen van incorrecte voorbeelden of (4) het oplossen van oefenproblemen. De studie werd wederom uitgevoerd in de context van een hbo-vak. De studenten ontvingen eerst de instructies over kritisch denken. Daarna oefenden ze met een aantal taken in de context van domeinrelevante problemen, onder een van de vier bovengenoemde oefencondities. Uit de resultaten bleek dat de prestaties van de studenten op de leertaken wederom verbeterden na de instructie en het oefenen. Bovendien presteerden de studenten zelfs nog beter na drie weken. Er waren echter geen verschillen tussen de vier oefengroepen in prestaties op de leer-en transfertaken. Bovendien waren er in deze studie geen aanwijzingen dat het bestuderen van uitgewerkte voorbeelden tot betere prestaties leidde dan het oplossen van oefenproblemen. Dit in tegenstelling tot de bevinding uit hoofdstuk 3 en de bevindingen uit eerdere studies met vele andere soorten taken (bijv. Renkl, 2014; Van Gog et al., 2019).

De studie in hoofdstuk 5 onderzocht of het herhaaldelijk ophalen van informatie uit het geheugen (Engels: *repeated retrieval practice*) effectief zou zijn voor het leren vermijden van systematische redeneerfouten en of dit bijdraagt aan transfer. Meer specifiek, kregen studenten de mogelijkheid om te oefenen in meerdere sessies die verspreid waren over een periode van een aantal weken. Deze studie werd wederom uitgevoerd in de context van een hbo-vak. De studenten ontvingen eerst de instructies over kritisch denken. Daarna oefenden zij met een aantal taken in de context van domeinrelevante problemen. Na elke oefening werd getoond of het gegeven antwoord juist was en kregen de studenten een uitgewerkt voorbeeld van een goede redenering te zien (feedback). Afhankelijk van de groep waarin de studenten ingedeeld waren, oefenden ze eenmalig, oefenden ze een tweede keer (een week later) of oefenden ze een derde keer (twee weken na de tweede keer). Een verrassende bevinding was dat (herhaald) oefenen geen significant effect had op de prestatie op de leertaken: de drie groepen lieten geen vooruitgang zien na de instructie en het oefenen. Tenminste, dit was het geval wanneer de prestatie werd gemeten aan de hand van zowel de antwoorden op meerkeuzevragen als de onderbouwing van deze antwoorden. Wanneer alleen naar de antwoorden op de meerkeuzevragen werd gekeken, werd er wel bij alle drie de groepen een vooruitgang in prestatie op de leertaken gevonden. Bovendien bleek, zoals werd verwacht op basis van bevindingen uit eerder onderzoek (bijv. Butler, 2010; McDaniel et al., 2012, 2013; Roediger & Butler, 2011), dat de gemiddelde prestatie *tijdens* het oefenen verbeterde naarmate er meer geoefend werd. Het lijkt er dus op dat de studenten wel enigszins profiteerden van herhaald oefenen, maar dat zij niet in staat waren om hun antwoorden goed te onderbouwen. Helaas kon het effect op transfer niet worden vastgesteld omdat de prestatie van studenten op de transfertaken extreem laag was (Engels: *floor effect*). Mogelijk heeft de feedback het effect van herhaald oefenen op leren tenietgedaan. Volgens recent onderzoek is feedback alleen nuttig wanneer studenten niet in staat zijn om tot het goede antwoord te komen en heeft het nauwelijks invloed wanneer zij hier wel toe in staat zijn (Kliegl et al., 2019; Pastötter & Bäuml, 2016; Storm et al., 2014). Het is dus mogelijk dat de groep studenten die maar één keer oefende – en het minst goed presteerde tijdens het oefenen – de feedback beter heeft verwerkt en daardoor even goed presteerde op de posttest als de andere groepen. Dit idee wordt ondersteund door de bevinding dat de studenten meer tijd besteedden aan de feedback na onjuiste antwoorden op de oefentaken dan na juiste antwoorden.

De studie in hoofdstuk 6 richtte zich op het identificeren van belemmeringen voor succesvolle transfer van vaardigheden om kritisch te denken. Wanneer transfer niet succesvol is, dan zou dit kunnen komen doordat studenten niet herkennen dat de aangeleerde kennis relevant is voor het nieuwe probleem, doordat ze de aangeleerde kennis niet uit hun geheugen kunnen ophalen of doordat ze de aangeleerde kennis niet kunnen toepassen op het nieuwe probleem (het driestappenmodel van transfer; Barnett

& Ceci, 2012). Er werden twee experimenten uitgevoerd om vast te stellen waar het transferprobleem uit eerdere onderzoeken van dit proefschrift door veroorzaakt zou kunnen zijn: één experiment met universitaire studenten in het laboratorium en één met hbo-studenten in de context van een vak. De studenten ontvingen eerst instructies over kritisch denken. Daarna oefenden zij met syllogismen – dit zijn redeneertaken waarbij je moet bepalen of een getrokken conclusie geldig is – in de context van domeinrelevante problemen. Deze keer werd de prestatie van de studenten gemeten op syllogismen (hiermee werd het *leren* gemeten), syllogismen in nieuwsberichten of artikelen (hiermee werd *nabije transfer* gemeten) en nieuwe taken met dezelfde onderliggende principes als syllogismen (hiermee werd *verre transfer* gemeten). De studenten werden ingedeeld in vier verschillende groepen en afhankelijk van de groep, ontvingen zij tijdens het maken van de transfertaken op de posttest (1) geen ondersteuning, (2) hints dat de principes uit de instructie relevant waren voor deze taken (ondersteuning in herkenning), (3) hints dat de principes uit de instructie relevant waren voor deze taken en de vraag om de opgedane kennis over die principes op te halen uit het geheugen (kennis ophalen) of (4) hints dat de principes uit de instructie relevant waren voor deze taken en een kort overzicht op papier van deze principes (ondersteuning in ophalen). Kortom, er werd in de condities verschillende ondersteuning voor de verschillende stappen in het transferproces aangeboden. Door de effecten van de condities te vergelijken, valt dan af te leiden waar zich problemen in het bereiken van transfer voordoen bij de studenten (vlg. Butler et al., 2013, 2017). Binnen de 'kennis ophalen' groep werd daarnaast gekeken of het vermogen van de studenten om kennis op te halen uit het geheugen gerelateerd was aan hun prestaties op de transfertaken. In beide experimenten verbeterden de prestaties van studenten op de leer- en nabije transfertaken na de instructie en het oefenen. Er werd zelfs een verbetering op de verre-transfertaken gevonden, maar de prestatie op deze taken was over het algemeen vrij laag dus er was nog veel ruimte voor verbetering. Een interessante bevinding was dat er geen verschillen waren tussen de vier groepen. De studenten waren dus niet geholpen bij de ondersteuning voor herkenning en ophalen. Dit suggereert dat de studenten in staat waren te herkennen dat de aangeleerde kennis relevant was voor de nieuwe taken en dat zij deze kennis konden ophalen uit het geheugen, maar moeite hadden met het toepassen van deze kennis op de nieuwe taken. Echter, de bevindingen uit de 'kennis ophalen' groep ondersteunen het idee dat de studenten alleen problemen hadden met het toepassen van de aangeleerde kennis niet volledig. De meeste studenten haalden namelijk niet alle relevante informatie op uit het geheugen. Bovendien werd er (via verkennende analyses) een matig tot hoog positief verband gevonden tussen de opgehaalde kennis en de prestaties op de transfertaken. Dit wijst erop dat problemen in het ophalen van kennis in elk geval ten dele ook een rol spelen bij niet-succesvolle transfer. Beschrijvende statistieken ondersteunen dit idee: de studenten die ondersteuning in ophalen kregen, presteerden beter (hoewel niet significant beter) dan

de studenten in de andere groepen op de verre-transfertaken van de posttest in het eerste experiment en op de nabije-transfertaken in het tweede experiment.

Conclusie

Dit proefschrift werpt meer licht op de complexiteit van het bevorderen van het leren en de transfer van kritisch-denken-vaardigheden van studenten in het hoger onderwijs. De bevindingen in dit proefschrift benadrukken het belang van expliciete instructie over kritisch denken in combinatie met oefening op domeinrelevante problemen voor het leren van kritisch-denken-vaardigheden. Het lijkt dus waardevol om kritisch denken in te bedden in hoger onderwijs curricula en het expliciet aan bod te laten komen in (kritisch denken) vakken. Tevens werd uit dit proefschrift duidelijk dat generatieve verwerkingsstrategieën geen wondermiddel zijn om leren te verbeteren en transfer te bewerkstelligen. Hoewel ze voor sommige vaardigheden goed werken, lijken ze namelijk niet bij te dragen aan het verder bevorderen van de vaardigheid om systematische redeneerfouten te vermijden (bovenop de effecten van instructie en oefening). Daarnaast maken de studies in dit proefschrift eens te meer duidelijk hoe moeilijk het is om kritisch-denken-vaardigheden zodanig te trainen dat er ook *transfer* optreedt naar nieuwe situaties. Het lijkt zinvol om in (onderzoek naar) onderwijs in kritisch denken meer aandacht te besteden aan factoren die succesvolle transfer van kritisch-denken-vaardigheden kunnen belemmeren, zoals problemen met het ophalen van aangeleerde kennis uit het geheugen en met het toepassen van deze kennis in een nieuwe context. Met het oog op dat laatste, zou het interessant zijn om verder te onderzoeken hoe studenten ondersteund kunnen worden in de toepassing van kritisch-denken-vaardigheden.

Om studenten te helpen goede kritische denkers te worden – in de zin dat zij aangeleerde vaardigheden kunnen toepassen op verschillende taken en in verschillende contexten – lijkt het waardevol om in toekomstig (praktijkgericht) onderzoek de effecten van langere interventies of uitgebreidere cursussen gericht op kritisch denken te onderzoeken. Daarnaast is het van belang om in toekomstig onderzoek ook aandacht te besteden aan de ontwikkeling en de verbetering van de denkhouding van studenten. Want interventies die de kritisch-denken-vaardigheden van studenten verbeteren, zullen in de praktijk weinig zoden aan de dijk zetten wanneer studenten niet de juiste denkhouding hebben en geen mentale inspanning willen leveren om deze vaardigheden te gebruiken. Kortom, de vraag is hoe we studenten uit kunnen dagen om in kritisch denken te investeren.

Curriculum vitae

Curriculum vitae

Lara van Peppen was born in Delft, the Netherlands, on April 28, 1992. After completing her secondary education at the Stanislascollege in Delft in 2010, she started studying Psychology at Leiden University from which she obtained her bachelor's degree in 2013. Subsequently, she enrolled in the master's specialization Applied Cognitive Psychology at Leiden University from which she obtained her degree in 2015. For her master's thesis, which focused on the effects of physical load on cognitive performance, she did a research internship at the Training & Performance Innovations expertise group of the Netherlands Organisation for Applied Scientific Research (TNO). Following her interest in (applied) research, Lara became a PhD candidate at the Department of Psychology, Education and Child Studies at Erasmus University Rotterdam in January 2016, studying how to foster students' critical thinking skills in such a way that these would also transfer across tasks and contexts. Her project was part of the broader NWO-funded research project "Investing in Thinking Pays Good Interest: Improving Critical Thinking Skills of Students and Teachers in Higher Professional Education". During her PhD trajectory, Lara participated in the Brain and Learning research group of Avans University of Applied Sciences and collaborated with educational advisors, teachers, and researchers from Avans University of Applied Sciences and Utrecht University. Further, Lara was a visiting scholar in prof. dr. Patricia Alexander's Disciplined Reading and Learning Research Lab at the University of Maryland in College Park, MD, United States (September – November 2019). Whilst conducting her PhD research, Lara was a member of the Interuniversity Centre for Educational Sciences (ICO), presented her research at various (inter)national conferences and symposia, was the recipient of two best poster awards, worked as an academic teacher/trainer, co-supervised master's thesis projects, and organized several events, symposia, and meetings. Further, she gave workshops on various educational and psychological topics to students and educational professionals. Lara is currently working as educational innovator at the Erasmus University Medical Center in Rotterdam, focusing on blended learning and assessment and feedback in medical education.

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Janssen, E. M., Mainhard, T., Buisman, R. S. M., Verkoeijen, P. P. J. L. Heijltjes, A. E. G., **Van Peppen, L. M.**, & Van Gog, T. (2019). Training higher education teachers' critical thinking and attitudes towards teaching it. *Contemporary Educational Psychology, 58*, 310–322. https://doi.org/10.1016/j.cedpsych.2019.03.007

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Van Peppen, L. M., Verkoeijen, P. P. J. L., Heijltjes, A. E. G., Janssen, E. M., & Van Gog, T. (submitted). *Repeated retrieval practice to foster students' critical thinking skills.* Manuscript submitted for publication.

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Verkoeijen, P. P. J. L., Koppenol-Gonzalez, G. V., Van Peppen, L. M., Broeren, M. M. D. H. J., Heijltjes, A. E. G., Kuijpers, R. E., Nobelen, J. T. L. M., & Tillema, M., & Arends, L. R. *Assessing the generality of a self-administered strategic resource use intervention on academic performance: A multi-site, preregistered conceptual replication of Chen, Chavez, Ong & Gunderson*. Provisionally accepted for publication in *Advances in Methods and Practices in Psychological Science*.

Presentations

Scientific presentations on the studies that have been conducted as part of this research project to educational professionals and researchers at (inter)national conferences and symposia. Presenting author(s) indicated with *.

Van Peppen, L. M.*, Verkoeijen, P. P. L. J., Heijltjes, A. E. G., Janssen, E. M., & Van Gog, T. (2019, August). *Contrasting correct and erroneous examples to enhance students' critical thinking skills.* Oral presentation as part of the symposium 'Critical Thinking in Higher Education: Educational Guidelines and Instructional Interventions' (Organizers: L.M. van Peppen & E. M. Janssen) at the biannual conference of the European Association for Research on Learning and Instruction (EARLI 2019), Aachen, Germany.

Van Peppen, L. M.*, Verkoeijen, P. P. L. J., Heijltjes, A. E. G., Janssen, E. M., & Van Gog, T. (2019, March). *Enhancing students' critical thinking skills: Is contrasting correct and erroneous examples beneficial?* Pitch at the Graduate Research Day of the Department of Psychology Education and Child Studies (GRD DPECS) of the Erasmus University Rotterdam, Rotterdam, the Netherlands.

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Awarded with (1) Best Poster Presentation Award, EARLI SIG 6–7 conference 2018 and (2) Award for PhD Excellence: Best Poster 2018, Erasmus Graduate School of Sciences and the Humanities.

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Van Peppen, L. M.*, Kolenbrander, S. V., Verkoeijen, P. P. L. J., Heijltjes, A. E. G., Janssen, E. M., & Van Gog, T. (2017, May). O*ptimizing critical thinking instruction: What practice schedules and task-types are effective?* Poster presentation at the annual convention of the Association of Psychological Science (APS 2017), Boston, MA, USA.
Nominated for the Award for PhD Excellence: Best Poster 2017, Erasmus Graduate School of Sciences and the Humanities.

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Kolenbrander, S. V.*, Van Peppen, L. M.*, & Janssen, E. M.* (2017, April). *Kritisch onderwijzen [Teaching critically].* Invited evening lecture at the Academielezing of Avans University of Applied Sciences, Breda, the Netherlands.

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Van Peppen, L. M.*, Verkoeijen, P. P. J. L., Heijltjes, A. E. G., Janssen, E. M., Koopmans, D., & Van Gog, T. (2016, Oktober). *Effects of self-explaining on learning and transfer of critical thinking skills.* Poster presentation at the Interuniversity Center for Educational Sciences International Fall School (ICO IFS 2016), Bad Schussenried, Germany.

Workshops and training sessions

Workshops and training sessions on various educational and psychological topics to students, teachers, educational practitioners, and researchers. Presenting author(s) indicated with *.

Van Peppen, L. M.*, & Janssen, E. M.*, (2019, November). "*Van mening veranderen is een teken van zwakte": Wat is een kritische denkhouding en (hoe) kun je die bevorderen? ["Changing your opinion is a sign of weakness": What is a critical thinking attitude and (how) can you improve it?]*. Workshop at the symposium 'Laat je (niets) wijsmaken!' over kritisch leren denken in het hbo [symposium on critical thinking in higher professional education], Avans Hogeschool, Den Bosch, the Netherlands.

Raaijmakers, L. H.*, **Van Peppen, L. M.***, Tillema, M.*, & Van Harsel, M. (2019, February). *Motiverend lesgeven [Teaching motivating]*. Workshop at the Academie voor Industrie en Informatica (AI&I) of Avans University of Applied Sciences, Den Bosch, the Netherlands.

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Alles blijft
Alles gaat voorbij
Alles blijft voorbijgaan
— Jules Deelder