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Empirical study

Gaining from explaining: Learning improves from explaining to fictitious others on video, not from writing to them

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ABSTRACT

Two experiments investigated whether studying a text with an "explanation intention" and then actually explaining it to (fictitious) other students in writing, would yield the same benefits as previously found for explaining on video. Experiment 1 had participants first studying a text either with the intention to explain it to others or to complete a test, and subsequently restudying vs. explaining in writing. Neither study intention nor explaining affected learning outcomes. Experiment 2 directly compared explaining in writing and on video. Participants studied a text with a test intention followed by restudy, or study with an explanation intention followed by either explaining in writing or on video. Explaining on video, but not in writing, enhanced learning more than restudy. These findings suggest that the benefits of explaining on video are not a result of engaging in explanation per se. Results are discussed in light of feelings of social presence.

1. Introduction

It is well established that explaining is a powerful learning strategy (Dunlosky, Rawson, Marsh, Nathan, & Willingham, 2013; Fiorella & Mayer, 2015a, 2015b; Leinhardt, 2001; Lombrozo, 2012; Ploetzner, Dillenbourg, Preier, & Traum, 1999; Richey & Nokes-Malach, 2015; Wylie & Chi, 2014). Most research on the effects of explaining has focused on explaining instructional materials to oneself (i.e., *self-explaining*) or explaining to others in *interactive tutoring* situations (Ploetzner et al., 1999; Richey & Nokes-Malach, 2015). Recent studies, however, have shown that providing explanations of learned material to *fictitious* other students (i.e., not present, no interaction) is also effective for learning, and even more so than restudying that material (Fiorella & Mayer, 2013, 2014; Hoogerheide, Loyens, and Van Gog, 2014a).

Hoogerheide et al. (2014a) provided students with a text on syllogistic reasoning problems. Students who were instructed to study with the intention to explain the learning material to someone else and then explained it to a fictitious other student by creating a webcam video showed higher learning and transfer performance on an immediate and delayed posttest compared to students who were instructed to study with the intention of performing well on a test and engaged in restudying the material, which is how students normally study. The cognitive schemas acquired by those who explained on video were also more efficient in the sense that higher test performance was attained with equal (perceived) effort investment on the posttest (for elaboration on instructional efficiency in terms of the relation between mental effort and performance, see Van Gog & Paas, 2008). This pattern of results was found across two experiments. In the second experiment, students in the restudy condition engaged in a recall activity prior to restudy to rule out the possibility that the positive effects of explaining on video were simply caused by retrieval practice (inherent to explaining), which has been shown to positively affect learning outcomes (Roediger, Putnam, & Smith, 2011).

Fiorella and Mayer (2013, 2014) obtained similar results in two studies on the effects of studying with the expectation of teaching later on (i.e., a teaching expectancy) and actually teaching by creating a short five-minute video lecture. Their participants studied a text about the Doppler effect. Across both studies, those students who expected to have to teach later on showed enhanced performance on an immediate but not on a delayed comprehension test compared to those studying for a test. Only the students who had actually created a video lesson showed better comprehension scores than those studying for a test on both the immediate and delayed comprehension test. Fiorella and Mayer also explored effects on (perceived) effort investment during learning. They

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found some tentative indications that studying with a teaching expectancy is more effortful than studying with a test expectancy. However, findings were mixed, possibly because effort investment was measured at the end of the experiment rather than directly after the learning phase.

Roscoe and Chi (2008) contrasted explaining learning materials to a fictitious peer student on video (i.e., creating a video lesson) to self-explaining and peer tutoring. In a first session, university students studied a text about the human eye (1025 words) for 30 minutes. One week later, in a second session, they generated explanations for 30 minutes with the materials still being available (at least in the peer tutoring and self-explaining conditions). Although all three strategies were beneficial for learning, explaining on video was less effective relative to peer tutoring and self-explaining. It is unclear how these findings relate to Fiorella and Mayer (2013, 2014) and Hoogerheide et al. (2014a), however. Next to self-explaining and peer tutoring being stronger control conditions than restudy, Roscoe and Chi's study had a very different design (i.e., a delay between sessions, materials available during explaining, the time spent on explaining), and the actual time spent explaining in the three conditions was not reported and therefore may have differed among conditions.

Regardless of what exactly caused explaining on video to be less effective than self-explaining and peer tutoring, the positive effect found by Fiorella and Mayer (2013, 2014) and Hoogerheide et al. (2014a) beg the question of whether there is something specific to the video creation process that promotes learning, or whether it is simply the fact that students engage in explaining that causes beneficial effects on learning outcomes. In case of the latter, one would expect no unique benefit from explaining on video compared to explaining in writing. Instructions to provide written explanations for others would also be easier to implement in the classroom. Therefore, Experiment 1 replicated and extended the study by Fiorella and Mayer (2013, 2014) and Hoogerheide et al. (2014a) by having students explain in writing instead of on video. Experiment 2 made a direct comparison between explaining on video versus explaining in writing. Before introducing the experiments in more detail, we will first review relevant literature on the effects of study intention and teaching expectancy, as well as on the effects of giving explanations on learning outcomes.

1.1. *Effects of studying with the intention to explain*

Studying learning materials with the intention of explaining them to others later on, also referred to as 'teaching expectancy', can be expected to foster effective study processes. For example, studying with an explanation intention may stimulate more active processing (Benware & Deci, 1984), comprehension monitoring (e.g., asking oneself "why" questions; Roscoe, 2014), self-explaining (Chi, Bassok, Lewis, Reimann, & Glaser, 1989; Chi, De Leeuw, Chiu, & LaVancher, 1994; Renkl, 1997, 2002), metacognitive processing (Muis, Psaradellis, Chevrier, Leo, & Lajoie, 2015), and generating deep questions and explanations (Craig, Gholson, Brittingham, Williams, & Shubeck, 2012; Craig, Sullins, Witherspoon, & Gholson, 2006).

Research on studying with a teaching expectancy has led to mixed findings, however. Some studies found positive effects on learning outcomes. For example, in Bargh and Schul (1980), the university students who studied a passage with a teaching expectancy outperformed those who studied with a test expectancy on a subsequent recall and recognition test. Similarly, Nestojko, Bui, Kornell, and Bjork (2014) recently showed that university students recalled more information from a text and recalled more efficiently if they had studied the text with a teaching expectancy. This benefit was also found, albeit less consistently, on the short answer test. Muis et al. (2015) even found that for primary school children, studying with a teaching expectancy fostered the use of metacognitive strategies and learning outcomes. Other studies did not find such positive effects on learning outcomes, however. For example, Renkl (1995) showed that studying learning materials with a teaching expectancy evoked university students to study less superficially than those who studied with a test expectancy, but this did not result in higher learning outcomes. Those who studied with a teaching expectancy even showed less intrinsic motivation and increased levels of anxiety. Higher anxiety was also found by Ross and DiVesta (1976). Finally, Ehly, Keith, and Bratton (1987) found a detrimental effect of teaching expectancy in the sense that high school students performed worse on a test if they studied with a teaching expectancy than if they studied for a test.

Several explanations have been offered for the mixed findings. Regarding immediate vs. delayed tests, Fiorella and Mayer (2013, 2014) suggested that the effect of studying a text with the intention of explaining it later on might be short-lived. On a delayed posttest, this effect would have diminished unless the expectancy had been coupled with actually explaining (on video). However, other studies did not even find beneficial effects of teaching expectancy on an immediate posttest (e.g., Ehly et al., 1987; Renkl, 1995). A potential explanation for the differences in findings with regard to immediate test performance could be that learners might need a certain level of experience with studying with an explanation expectancy before it becomes beneficial for learning. In the study by Hoogerheide et al. (2014a), no effects of an explanation intention were apparent for secondary education students. For university students in a problem-based learning curriculum, who are used to explaining to other students, the explanation intention did positively affect learning both on the immediate and the delayed posttests. Note however that Muis et al. (2015) showed that even primary school children could benefit from studying with a teaching expectancy, and it would seem unlikely that they would have had a lot of experience explaining to each other.

1.2. *Generating explanations*

Generating explanations can be a powerful method for improving learning outcomes (Dunlosky et al., 2013; Fiorella & Mayer, 2015a, 2015b; Leinhardt, 2001; Lombrozo, 2012; Ploetzner et al., 1999; Richey & Nokes-Malach, 2015; Wylie & Chi, 2014). As mentioned above, research on generating explanations has mainly focused on the effects of self-explanations and the effects of explaining to others in tutoring or collaborative learning contexts (Ploetzner et al., 1999; Richey & Nokes-Malach, 2015). As Richey and Nokes-Malach (2015) describe, research on self-explaining has shown that:

> '… encourage learners to identify and elaborate on the critical features of problems, including the underlying principles (Atkinson, Renkl, & Merrill, 2003; Chi & VanLehn, 1991), the conditions for applying those principles (Chi et al., 1989), and the logic and subgoals for applying them (Catrambone, 1998; Crowley & Siegler, 1999). These critical features tend to apply across problems within a domain. By recognizing and understanding these features, a learner is more likely to successfully transfer knowledge to a novel problem (Atkinson et al., 2003).' (p. 203)

These cognitive benefits may in part arise because the process of self-explaining may stimulate metacognitive processes such as monitoring the quality of one's own understanding (i.e., comprehension monitoring; Roscoe & Chi, 2007). However, a caveat to self-explaining is that students may not always generate high quality self-explanations on their own (e.g., Renkl, 1997), and therefore may need self-explanation prompts (e.g., Nokes, Hausmann, VanLehn, & Gershman, 2011) or even an explicit training (e.g., Kurby et al., 2012) before generating self-explanations effectively.

Explaining to others has also been shown to enhance learning outcomes in interactive situations, for instance when tutoring (Cohen, Kulik, & Kulik, 1982) or during small group discussions (Cohen, 1994; Johnson, Johnson, & Smith, 2007). Several studies analyzed the quality of the explanations to identify the benefits of different discourse moves, and these studies typically show that explaining is most effective when the explanations are relevant, coherent, complete, and accurate (Coleman, Brown, & Rivkin, 1997; King, 1994; Roscoe & Chi, 2007; Webb, 1989). Moreover, learners benefit more from generating explanations when they engage in so-called 'reflective knowledge building activities' such as generating inferences, repairing knowledge gaps, elaborating, and comprehension-monitoring. In contrast, learners benefit less when they predominantly engage in 'knowledge-telling', which entails summarizing with little elaboration or monitoring of one's own understanding (King, Staffieri, & Adelgais, 1998; Roscoe, 2014; Roscoe & Chi, 2007, 2008). Studies on tutoring or small group discussions typically do not experimentally control for explaining as a contributing factor (for an exception, see Roscoe & Chi, 2008). Therefore beneficial effects could also, at least partly, be attributed to the fact that in these situations, explanations are aimed at others who are present and can be interacted with. The other students may, for instance, ask questions, point out inconsistencies, or provide explanations themselves, which might contribute to the effectiveness of tutoring or small group learning (Okita & Schwartz, 2013; Ploetzner et al., 1999; Webb, 1989). Indeed, King et al.'s (1998) concept of transactive peer tutoring postulates that the benefits of peer tutoring are for a large part a result of a cognitive partnership in which learning partners and their actions continuously depend on the others' level of understanding and their responses.

When interactive elements are controlled for, a situation remains in which explanations are aimed at instructing someone who is present but merely listens. Ploetzner et al. (1999) suggest that aiming explanations at someone else who is physically present may stimulate more elaborate explanations and monitoring of whether the recipient comprehends the explanations, compared to self-explaining, which may lead to skipping. In line with this view, Coleman et al. (1997) found that engaging in explanations with the aim of instructing another person in the room fostered measures of deep learning more so than self-explaining. These findings suggest that 'social presence', even without interaction, may foster the effectiveness of explaining for learning.[1]

Social presence was originally defined by Short, Williams, and Christie (1976) as the degree to which a person is aware of the presence of another person in a technology-mediated communication or learning setting. The definition was updated more recently to the degree to which someone is perceived as a "real person" in computer-mediated communication or learning (Gunawardena, 1995). It is a key concept in understanding and improving the degree of participation and success in online learning environments (Borup, West, & Graham, 2013; Sung & Mayer, 2013). Placed on a continuum of social presence, engaging in explaining in interactive situations (e.g., tutoring, small group discussions) is on the high end and self-explaining is on the low end because the explanations are directed at oneself (i.e., the student's own understanding). Explaining to present but merely listening (i.e., non-interacting) others and explaining to non-present others fall in between these two on the social presence continuum. Explaining to non-present others may seem odd, but has become quite common in online learning environments nowadays. For instance, people provide explanations to others who may not be online at the same time (and whom they often do not even know) in asynchronous text-based discussion forums (Andresen, 2009) or in demonstration ('how-to') videos (e.g., Spires, Hervey, Morris, & Stelpflug, 2012). Such video demonstrations or lectures are often recorded behind a webcam, or using a digital camera on a tripod, without an audience present.

Being aware of a recipient/listener and perceiving them as real (even if they are not present) may result in "productive agency," that is, the belief that one's actions can affect others (Okita & Schwartz, 2013; Schwartz, 1999; Schwartz & Okita, 2004). Okita and Schwartz argued and showed that collaborative learning and teaching are in part so effective because they foster learners' awareness that their actions can affect the learning of others, which stimulates them to contribute (more) and to keep on doing so in the face of difficulties.

The findings by Fiorella and Mayer (2013, 2014) and Hoogerheide et al. (2014a) have shown that explaining to non-present, fictitious other students on video can be effective for learning. It is unclear, however, whether the same would apply to explaining to non-present, fictitious other students in writing. Explaining in writing would be much easier to implement in the classroom as a learning activity. Moreover, as addressed below, the process of explaining in writing is very different from explaining on video, and therefore may be more or less advantageous relative to reading. This question was addressed in the present study.

1.3. The present study

If it is the act of explaining itself that produces beneficial effects on learning, then explaining to fictitious other students in writing would be expected to be effective compared to restudy, just like explaining on video was found to be (Fiorella & Mayer, 2013, 2014; Hoogerheide et al., 2014a). Indeed, with regard to engaging in *recall*, research conducted in the context of the testing effect (i.e., the finding that engaging in recall after an initial study phase is more effective than restudying; Roediger et al., 2011) has demonstrated that engaging in both oral and written recall of paired associates is effective for learning (Putnam & Roediger, 2013). Although recalling information and explaining it are different processes, prior research has proven written explanations during learning (e.g., Hilbert, Schworm, & Renkl, 2004; Schworm & Renkl, 2006) and during problem-solving (e.g., Aleven & Koedinger, 2002) to be effective compared to not explaining. The effectiveness of explaining in writing is perhaps to be expected, as, compared to reading, writing activities "can support more sophisticated elaboration and organizational strategies by linking new understandings with familiar ones, synthesizing knowledge, exploring relations and implications, and building outlines and conceptual frameworks (Bangert-Drowns, Hurley, & Wilkinson, 2004, p. 32)." Moreover, writing can stimulate various metacognitive strategies, such as deliberate planning and monitoring the quality of the writing (Paris & Paris, 2001; Schraw, 1998).

On the other hand, because the act of writing is very different from the act of speaking in a camera, the form in which explanations are given may matter. Speaking in front of a camera may be higher in perceived social presence than writing. That is, the presence of a camera may give students a stronger feeling that they are communicating information to an actual other person (even though that person is not present at the moment) than writing does. Moreover, speaking allows for a high number of idea units to be expressed in a short amount of time, which is not the case for writing (Grabowski, 2007; Kellogg, 2007). But writing, in contrast to speaking, involves more deliberate planning and may therefore entice learners to think more about what is most important (i.e., key ideas/concepts/procedures) to explain to others. Indeed, it seems that writing results in less irrelevant or distorted idea units being (re)produced than speaking does (Horowitz & Newman, 1964; Kellogg, 2007). Moreover, explaining in writing may better enable learners to monitor whether the information is accurately

[1] Note that the concept of social presence is similar, but not identical, to King et al.'s (1998) concept of transactive peer tutoring. Although both concepts focus on the effects of taking the 'fellow learner(s)' into account, King's theory limits itself to highly interactive situations that allow for continuous interaction.

presented, whereas speaking may impede output monitoring (Grabowski, 2007).

In sum, it is unclear whether explaining in writing would be more, less, or equally effective as explaining on video compared to a restudy control condition. We hypothesize that explaining on video would be more effective than restudying, while it is an open question whether explaining in writing would be more beneficial than restudying and whether there would be differences between explaining in writing and explaining on video. The present study addressed these questions in two experiments. Experiment 1 investigated the effects of explaining in writing compared to restudying, and Experiment 2 compared explaining in writing to explaining on video and restudying.

2. Experiment 1

Experiment 1 aimed to replicate and extend the findings by Hoogerheide et al. (2014a), using the same materials and conditions (plus an additional control condition), but having students explain in writing rather than on video. In a 2 × 2 design, participants studied a text on syllogistic reasoning (of which the content was new to them) with either a test or explanation study intention. Subsequently they either restudied the materials or explained them in writing to other students. Note that students were not told beforehand whether they would restudy or produce written or video explanations, and time on task was kept equal across conditions.

Given the mixed findings on the effects of studying with an explanation study intention (Hoogerheide et al., 2014a) or teaching expectancy (e.g., Bargh & Schul, 1980; Fiorella & Mayer, 2014; Nestojko et al., 2014; Renkl, 1995), we cannot formulate a directional hypothesis regarding the effects of study intention. Explaining is hypothesized to have beneficial effects on learning and transfer compared to restudying, at least when it was preceded by an explanation study intention (cf. the study by Hoogerheide et al., 2014a, in which explaining was always preceded by an explanation study intention).

We also analyzed perceived mental effort invested in the learning phase and in answering questions on the test. Such data, in combination with test performance measures, provide more insight in the learning process and the quality of learning outcomes, respectively (Van Gog & Paas, 2008). Hoogerheide et al. (2014a) investigated only effort invested during the test; they did not explore whether providing explanations is more effortful than restudying. Fiorella and Mayer (2013, 2014) only investigated effort invested in the learning phase, but found a mixed pattern of results, likely because the effort investment measurement was not presented directly after learning, but instead at the end of the experiment. We hypothesize that providing explanations might be more effortful than restudying in the learning phase (cf. germane cognitive load, Paas, Renkl, & Sweller, 2003; Paas & Van Gog, 2006; or desirable difficulties, Bjork & Bjork, 2011), but that this will also lead to higher learning outcomes, evidenced by higher test performance attained with equal or less effort investment on the test.

2.1. Method

2.1.1. Participants

Participants were 123 higher education students (81 female; $M = 20.05$, $SD = 1.90$), enrolled in the first year of a communication and media design ($n = 37$) or primary school teacher training ($n = 86$) program of a Dutch university of applied sciences.

2.1.2. Design

The experiment consisted of five phases: (1) pretest, (2) learning phase I, (3) learning phase II, (4) immediate posttest, and (5) delayed posttest. The experiment had a 2 × 2 design, with Study Intention (Test vs. Explanation; manipulated in learning phase I) and Explaining (No: Restudy vs. Yes: Writing Explanations; manipulated in learning phase II) as between-subject factors. Students were randomly assigned to one of the four conditions: test study intention—restudy ($n = 29$), test study intention—explain in writing ($n = 33$), explanation study intention—restudy ($n = 30$), or explanation study intention—explain in writing ($n = 31$).

2.2. Materials

All the study and test materials were paper-based.

2.2.1. Pretest

The pretest presented eight syllogistic reasoning items and two Wason-selection task items to assess prior knowledge. The syllogistic reasoning items asked participants to assess whether the conclusion that followed from the two premises was logical (i.e., choose one of two answer options: valid or invalid). Two test items were used for each of the four forms of syllogistic reasoning, namely: affirming the antecedent (if *P* then *Q*, *P* therefore *Q*), denying the antecedent (if *P* then *Q*, not *P* therefore not *Q*), affirming the consequent (if *P* then *Q*, *Q* therefore *P*), and denying the consequent (if *P* then *Q*, not *Q* therefore not *P*). One of those items was prone to belief bias, whereas the other was not. Belief bias makes it more difficult to assess whether a conclusion is logically valid because the conclusion is in line with real world knowledge (George, 1995; Newstead, Pollard, Evans, & Allen, 1992).

Wason-selection tasks (Wason, 1966) require combining the two valid forms of syllogistic reasoning (i.e., affirming the antecedent and denying the consequent) to correctly test the validity of a rule. For example, when asked "If there is an A on one side, and a 2 on the other side" by turning two cards out of the four possibilities A, E, 1, and 2, then the correct answer would be to turn A (affirming the antecedent) and 1 (denying the consequent). People are, however, inclined to turn A and 2 instead. Thus, the pretest required participants to select the two correct forms of syllogistic reasoning out of four answer options.

2.2.2. Learning phase I

In the first learning phase, participants received a 1930 words text (the same as used in Hoogerheide et al., 2014a). Participants studied the text for 12 minutes, which was equal to Hoogerheide et al. (2014a) in which this was based on a pilot study. This text addressed when a conclusion logically follows from two premises. After a general introduction, all four forms of syllogistic reasoning were explained using the same recurrent example: "If John sees a clown, then he is afraid. John sees a clown. Conclusion: John is afraid." The last page presented a summary table of all four forms of syllogistic reasoning using the example: "If this is an apple, then it is a fruit." It was also indicated whether each form led to a valid or an invalid conclusion.

Two versions of the study text were used in the present experiment, one for those who studied with a test study intention, and one for those who studied with an explanation study intention. These only differed in the study intention prompt placed on the first page and in the footer of each page: "Can you apply the information from this page to complete a test?" or "Can you explain the information on this page to a fellow student?"

2.2.3. Learning phase II

The second learning phase had a duration of 8 minutes, which is 3 minutes longer than Hoogerheide et al. (2014a) to provide the explanation conditions with sufficient time. Half of all the students restudied the same text as in learning phase I for 8 minutes. The first page of this booklet, however, differed as it instructed participants to engage in a cued recall activity prior to restudying the text to ensure that all conditions engaged in retrieval practice (i.e., to rule out the possibility that beneficial effects of explaining are simply due to retrieving information from long-term memory, which is inherent in explaining). Using the table and the example that was on the last page of the study text (i.e., "If this is an apple, then it is a fruit"), but without the indications of which forms were valid, participants were asked to fill in the gaps in the table from memory. For example, for affirming the antecedent, the correct answer was: "It is an apple, therefore it is a fruit" and for denying the antecedent, the correct answer was: "It is *not* an apple, therefore it is *not* a fruit", etcetera. The other half of the students engaged in the explanation activity by explaining what they had learned in writing as if explaining to a complete novice on the subject, with the help of the same table and example on the last page of the study text. In addition, participants in the explanation group were instructed to explain the error commonly made when judging whether a conclusion is valid (i.e., the belief-bias, although belief-bias was not explicitly mentioned).

2.2.4. Posttests

To assess learning, the immediate and delayed posttests presented eight conditional syllogistic reasoning items (one with and one without the belief-bias for each form) and two Wason selection tasks to assess transfer. An example of a conditional syllogistic reasoning test item (affirming the antecedent with belief-bias) is: If you are a Pokémon, then you belong in a pokeball. Pikachu belongs in a pokeball. Conclusion: Pikachu is a Pokémon. An example of a Wason selection task is: Which two cards would you have to turn to test the rule 'If there is a Y on one side, then there is a 2 on the other side?', with answer options X, Y, 2, and 7. Two parallel versions of the posttest (A and B) were created. These versions were structurally equivalent compared to each other and to the pretest, but different on surface features. On both posttests, participants were not only asked to select the correct answer, but also to explain their answer, making the items on the posttests substantially more difficult and less prone to guessing.

2.2.5. Mental effort

Mental effort was measured after each test item on the pretest and posttests and after the second learning phase, using a subjective 9-point rating scale (Paas, 1992), asking students to rate how much effort they invested in the preceding task, with answer options ranging from (1) very, very low effort to (9) very, very high effort.[2]

2.3. Procedure

The study was run in small groups with approximately 15 students per session, at a university of applied sciences. Within each session students were randomly assigned to one of the four conditions. The first session lasted 60 minutes. Every student received an envelope with four booklets and then received a general introduction. After the introduction, students were instructed to take out the first booklet containing the pretest, and to complete it. After each pretest item participants rated how much mental effort they invested in that item. Participants had 10 minutes to complete the pretest. When time was up, they were instructed to place the first booklet upside down at the corner of their table and to take out the second booklet containing the study text, for which they received 12 minutes. All participants were encouraged to fully use the available time and to learn as much as possible. After the experimenter indicated that the 12 minutes were up, participants placed the second booklet on the corner of their table. Then, they were instructed to take the third booklet out of their envelope and to follow the written instructions. Participants in the restudy conditions were instructed to fill in the gaps in the table from memory, after which they would turn the page and restudy the same text as in the second booklet. Again, the participants were encouraged to use all available time and to learn as much as possible. The writing conditions were instructed to explain what they had just learned (instructions were provided in the booklet, see Section 2.2.3.). Participants had 8 minutes in total for the third booklet, in all conditions. When time was up, the participants first indicated perceived effort investment in restudying or giving explanations, then returned booklet 3 to the corner of their desk and worked on the fourth booklet, which contained the immediate posttest. Half of the participants in each condition received version A as the immediate posttest while the other half received version B. Participants again rated perceived effort investment after every test item. Maximally 25 minutes were available for the immediate posttest. The delayed posttest was to take place one week later, at which participants who received version A as immediate test would now receive version B and vice versa. Unfortunately, however, the delayed test session attendance was very low ($n = 52$) due to a scheduling error that was not under our control. Consequently, the analysis of the delayed posttest data would not be very useful and is not reported.

2.4. Data analysis

Scoring was done using the same coding scheme as was used in the Hoogerheide et al. (2014a) study. The pretest was scored by assigning one point per correctly answered question, resulting in a maximum score of 10 points. The maximum score on the syllogistic reasoning items (i.e., the items that measured learning) on the immediate posttest was 56 points. Each belief bias item (four in total) could result in a maximum of eight points. One point could be earned for the correct choice on the multiple-choice question and seven points for the explanation. These seven points were comprised of: correctly recalling the form of syllogistic reasoning (one point), explaining correctly in abstract terms of p and q (one point), explaining correctly in concrete terms (two points), correctly concluding in the explanation whether a conclusion was valid or invalid (one point), and correctly explaining the belief-bias (two points). Each no belief-bias item (four in total) could result in a maximum of 6 points (scoring as on the other items without the two points for explaining the belief-bias). A total of 18 points could be earned on the Wason selection tasks, maximally 9 points per correctly answered item. These 9 points were comprised of one point for selecting the correct answer and two points per correct explanation for each of the four forms of syllogistic reasoning as applied to the rule in the Wason selection task. Two raters scored 10% of the tests. Because the inter-rater reliability was high (intra-class correlation coefficient of .90), the remainder of the tests was scored by one rater.

Average perceived mental effort investment was computed separately for the syllogistic reasoning items (i.e., learning performance) and the Wason selection tasks (i.e., transfer performance). One participant in the 'explanation intention—explaining' condition was removed from all analyses because of non-compliance with instructions on the immediate posttest. One other participant had a missing mental effort rating on the pretest, which was replaced with the series mean.

3. Results and discussion

Performance data are presented in Table 1, and perceived mental effort data are presented in Table 2. At pretest, there were no

[2] Perceived confidence was also measured (after the effort measures) because the second author, who conducted this study as part of the qualifications for her MSc degree, was interested in exploring that variable. However, because we had no hypothesis about it and did not measure it in the second experiment, those (null) results are not reported here. Details can be obtained from the first author.

Table 1
Mean (*SD*) of learning and transfer test scores per condition in Experiment 1.

	Test intention—restudy	Test intention—explain in writing	Explanation intention—restudy	Explanation intention—explain in writing
Pretest—learning (*range 0–8*)	4.90 (1.45)	5.06 (1.14)	5.33 (2.02)	5.40 (1.48)
Pretest—transfer (*range 0–2*)	0.66 (0.61)	0.76 (0.61)	0.80 (0.66)	0.87 (0.57)
Pretest—total (*range 0–10*)	5.55 (1.43)	5.82 (1.36)	6.13 (1.53)	6.27 (1.41)
Immediate Posttest—Learning (*Range 0–56*)	16.76 (9.18)	15.74 (6.69)	15.98 (7.58)	16.28 (7.18)
Immediate posttest—transfer (*range 0–18*)	2.38 (2.81)	1.44 (2.30)	2.10 (3.32)	2.97 (2.77)

Note: Whereas the pretest consisted of multiple choice items only, the posttest asked students not only to select the correct answer, but also to explain their answer, making the items on the posttest more difficult and less prone to guessing.

differences among conditions, as one would expect after random assignment. An ANOVA showed no significant differences among conditions in pretest performance, $F < 1$, or perceived mental effort investment, $F(3, 118) = 1.51$, $p = .215$, $\eta_p^2 = .037$. The posttest data were analyzed by 2 × 2 ANOVAs with the exception of the learning and transfer results, which were analyzed by 2 × 2 ANCOVAs with students' pretest scores as a covariate. The nature of significant interactions was determined with follow-up Bonferroni-corrected t-tests.

As for the items that measured learning, students' pretest scores were a significant predictor, $F(1, 117) = 9.33$, $p = .003$, $\eta_p^2 = .074$, but there were no main effects of Study Intention or Explaining, nor an interaction effect (all Fs < 1). In a similar vein, students' pretest scores were a significant predictor of performance on the items that measured transfer, $F(1, 117) = 10.73$, $p = .001$, $\eta_p^2 = .084$. There were no main or interaction effects (Study Intention and Explaining: both Fs < 1; interaction: $F(1, 117) = 3.69$, $p = .057$, $\eta_p^2 = .031$).

The analysis of perceived mental effort investment in the second learning phase (booklet 3, restudying or writing explanations) showed a main effect of Explaining, $F(1, 118) = 6.29$, $p = .013$, $\eta_p^2 = .051$. This indicates that participants who gave explanations reported to have invested significantly more mental effort in the learning phase than those who restudied. There was no main effect of Study Intention, $F < 1$, nor an interaction effect, $F < 1$.

On perceived mental effort investment in the posttest items that measured learning, there was no main effect of Study Intention, $F(1, 118) = 1.93$, $p = .167$, $\eta_p^2 = .016$, or Explaining, $F < 1$, nor a significant interaction effect, $F(1, 118) = 3.75$, $p = .055$, $\eta_p^2 = .031$. On perceived mental effort investment in the Wason selection tasks that measured transfer performance, no main effects of Study Intention or Explaining were found (both Fs < 1). There was a significant interaction effect, $F(1, 118) = 5.74$, $p = .018$, $\eta_p^2 = .046$. To explore this interaction effect, two independent samples *t*-tests with Bonferroni-adjusted alpha levels of .025 were conducted that investigated effects of the test intention and explanation intention conditions separately. However, the test intention—restudy condition did not differ significantly (given the alpha-adjustment) from the test intention—explanation condition, $t(60) = 2.17$, $p = .034$, nor did the explanation intention—restudy differ significantly from the explanation intention—explain condition, $t(58) = 1.17$, $p = .247$.

In sum, the results of Experiment 1 showed no benefit of studying a text with an explanation intention compared to a test-taking intention on learning outcomes. Interestingly, we found no evidence that actually providing explanations would be more effective than restudying. Explaining in the learning phase was perceived to be more effortful than restudying, but this additional effort investment did not seem to pay off, as it did not result in higher learning outcomes. Note however that it is possible that the additional effort investment would have been beneficial for learning or transfer measured on a delayed test. Students who explained after studying with an explanation intention did reach the highest transfer test score numerically (see Table 1), but the interaction effect was not statistically significant ($p = .057$). This may suggest that the effort invested in explaining positively affected students' deep comprehension of the material, and it is very well possible that the effects of deep comprehension would only show after a delay (cf. Fiorella & Mayer, 2013, 2014). This makes it even more unfortunate that we were unable to obtain delayed test data in Experiment 1 from a sufficiently large number of students. Alternatively, the beneficial effects of explaining in writing might just be small (when preceded by studying with an explanation intention), which would be similar to the finding that writing-to-learn assignments such as writing summaries or essays typically only yield small benefits (Bangert-Drowns et al., 2004).

Another potential factor contributing to the lack of effect might have been the classroom setting in Experiment 1, which may have made it more difficult for students to concentrate than the individual study and test conditions in the study by Hoogerheide et al. (2014a). Therefore, a second Experiment was conducted in which we (a) made a direct comparison of explaining in writing and explaining on video to a restudy control condition, (b) did include a delayed test, and (c) tested students individually.

4. Experiment 2

In Experiment 2, students either studied a text with a test study intention and then engaged in a short recall activity (i.e., filling in the gaps in the table; see Section 2.2.3) prior to restudying (Test Condition), or studied with an explanation study intention followed by explaining in writing (Explanation—Writing Condition) or followed by explaining on video (Explanation—Video Condition). Based on findings that explaining in front of a camera is more effective than restudy (Fiorella & Mayer, 2013, 2014; Hoogerheide et al., 2014a), we hypothesize that explaining on video is more effective

Table 2
Mean (*SD*) of mental effort ratings (range 1–9) per condition in Experiment 1.

	Test intention—restudy	Test intention—explain in writing	Explanation intention—restudy	Explanation intention—explain in writing
Learning phase 2	5.21 (2.27)	6.09 (2.08)	4.70 (2.53)	5.93 (2.42)
Pretest—learning	3.34 (1.32)	2.91 (1.18)	2.55 (0.91)	3.28 (1.38)
Pretest—transfer	4.21 (1.99)	4.02 (1.63)	4.28 (1.73)	3.57 (1.42)
Pretest—total	3.51 (1.24)	3.13 (1.14)	2.90 (0.93)	3.34 (1.33)
Immediate posttest—learning	3.90 (2.06)	3.28 (1.44)	2.93 (1.27)	3.43 (1.55)
Immediate posttest—transfer	4.31 (2.19)	3.23 (1.73)	3.32 (1.46)	3.83 (1.93)

than restudy on both an immediate and delayed test. Based on the findings from Experiment 1, we expect no differences between the explaining in writing condition and the Restudy Condition on the immediate test. However, it is possible that the beneficial effects of explaining in writing compared to restudy would show on a delayed test. Whether explaining orally on video would be more effective than explaining in writing is an open question. We again measured perceived mental effort investment in Experiment 2 to investigate the efficiency of engaging in explaining. Additionally, because Hoogerheide et al. (2014a) found some tentative indications that explaining on video, although effective for learning and transfer, seemed to *reduce* students' perceived competence compared to restudy, this variable was also explored in Experiment 2. Perceived competence is an important variable to take into account because students' perceptions of their own competence are positively related to factors such as academic motivation and learning outcomes (Bong & Skaalvik, 2003; Harter, 1990; Law, Elliot, & Murayama, 2012; Ma & Kishor, 1997). Whereas studying learning materials may foster the development of students' perceived competence (Hoogerheide, Loyens, & Van Gog, 2014b, 2016), explaining may not be as beneficial because it can confront learners with knowledge gaps, that is, what they do not know (Roscoe & Chi, 2007).[3]

4.1. Participants

Participants were 129 Dutch undergraduate students ($M^{age} = 20.20$, $SD = 3.04$; 99 female) who studied Psychology in a Problem-Based Learning curriculum. Participants received a monetary reward or course credits for their participation.

4.2. Design

Like Experiment 1, Experiment 2 also consisted of five phases: (1) pretest, (2) learning phase I, (3) learning phase II, (4) immediate posttest, and (5) delayed posttest. Participants were randomly allocated to one of three conditions, namely the Test Condition (i.e., test study intention—restudy; $n = 42$), the Explanation—Writing Condition (i.e., explanation study intention—explain in writing; $n = 43$), or the Explanation—Video Condition (i.e., explain study intention—explain on video; $n = 44$).

4.3. Materials and procedure

The materials and procedure in Experiment 2 were almost identical to Experiment 1 with a few exceptions. First, some additional measures were added. Measures of perceived competence were added at the end of the pretest and start of the posttests (cf. Hoogerheide et al., 2014a), using an adapted version of the Perceived Competence Scale for Learning (Williams & Deci, 1996). After the pretest, students were asked to indicate on a scale of 1 (not at all true) to 7 (very true): "I feel confident in my ability to learn an in-depth explanation of the eight items," "I am capable of learning an in-depth explanation of the eight items," and "I feel able to meet the challenge of performing well in learning an in-depth explanation of the eight items." Prior to the posttests they were asked to indicate: "I feel confident in my ability to answer questions on a test," "I am capable of answering questions on a test," and "I feel able to meet the challenge of performing well answering questions on a test." Moreover, we also asked participants to indicate perceived mental effort invested at the end of learning phase I (i.e., booklet 2) to explore whether study intention would already affect effort investment in the learning phase, which could not be inferred from the data from Experiment 1. Finally, because of the differences in writing and video creation, we asked participants to indicate to which degree they felt that they had enough time to explain the four forms of syllogistic reasoning (Explanation—Writing and Explanation—Video Conditions) or to fill in the table on the first page and read the text (Test Condition) on a scale from 1 (to a very small degree) to 9 (to a very large degree).

A second difference with Experiment 1 was that participants in Experiment 2 were all seated in individual cubicles (as in the Hoogerheide et al., 2014a study). Third, in Experiment 1, where they had to write by hand, students had 8 minutes for explaining, whereas in the study by Hoogerheide et al. (2014a) students had only 5 minutes to create a video. In Experiment 2, we allowed students to type their explanations on the computer, which is faster than handwriting not only when copying information but also when writing from memory (even for "two-finger typists"; (Brown, 1988). Nevertheless, we gave them some extra time compared to the Hoogerheide et al. (2014a) study: all three conditions received six minutes to either restudy or generate written or video explanations during learning phase II. Fourth, following Hoogerheide et al., (2014a, Experiment 2), the Explanation Conditions were no longer explicitly instructed to explain the common errors that people tend to make when judging whether a conclusion is valid or invalid (i.e., the belief-bias). This ensures that an increased focus on this bias would not be the cause of the expected benefits of providing explanations.

4.4. Data analysis

Data were scored in the same manner as in Experiment 1. Four participants had to be removed from all analyses: One participant indicated high familiarity with the learning material from partaking in another experiment (Explanation—Video Condition), another failed to make a video (Explanation—Video Condition), the third did not follow instructions provided by the experimenter (Explanation—Writing Condition), and the last one received instructional materials from two conditions due to an experimenter error (Test Condition).

A further eight participants (two from the test condition and Explanation—Video Condition and four from the Explanation—Writing Condition) who did not return for the Delayed Posttest were excluded from the analyses of learning, transfer, mental effort, and perceived competence on the posttests. One participant who did not fill in the mental effort rating after the first learning phase was removed from this analysis. In case of maximally two missing mental effort ratings on the tests, these were replaced with the series mean (two instances on the pretest; nine on the immediate posttest).

5. Results and discussion

The learning and transfer scores can be found in Table 3, and the perceived mental effort investment and perceived competence scores are shown in Table 4. An ANOVA showed no significant differences among conditions in pretest performance or mental effort ratings, both Fs < 1, nor in perceived competence, $F(2, 122) = 1.05$, $p = .353$, $\eta_p^2 = .017$.

With regard to learning, a repeated measures ANCOVA with Test Moment (Immediate vs. Delayed) as within-subjects factor, condition as between-subjects factor, and pretest scores as covariate, showed that students' pretest scores were not a significant predictor of learning, $F < 1$. There was no main effect of Test Moment, $F < 1$, but there was a main effect of Instruction Condition, $F(2, 113) = 3.71$, $p = .027$, $\eta_p^2 = .062$. Bonferroni-corrected post-hoc tests showed that

[3] Note that the construct of perceived competence is similar, but not identical, to the construct of self-efficacy (Hughes, Galbraith, & White, 2011; Rodgers, Markland, Selzler, Murray, & Wilson, 2014). Although both reflect perceptions of one's own abilities, perceived competence focuses on the need to master personally challenging tasks and self-efficacy reflects more situation specific self-confidence.

Table 3
Mean (*SD*) of learning and transfer test scores per condition in Experiment 2.

	Test intention—restudy (*test condition*)	Explanation intention—explain in writing (*explanation—writing condition*)	Explanation intention—explain on video (*explanation—video condition*)
Pretest—learning (*range 0–8*)	5.68 (1.37)	6.00 (1.36)	5.95 (1.29)
Immediate posttest—learning (*range 0–56*)	22.49 (5.90)	24.86 (6.27)	26.01 (5.32)
Delayed posttest—learning (*range 0–56*)	21.53 (4.89)	22.17 (5.95)	24.66 (6.12)
Immediate posttest—transfer (*range 0–18*)	5.51 (3.45)	6.53 (3.08)	5.99 (3.61)
Delayed posttest—transfer (*range 0–18*)	6.22 (3.17)	6.84 (3.35)	6.15 (3.57)

Note: Whereas the pretest consisted of multiple choice items only, the posttests asked students not only to select the correct answer, but also to explain their answer, making the items on the posttests more difficult and less prone to guessing.

the Explanation—Writing Condition ($M = 23.50$; $SD = 5.37$) did not outperform the Test Condition ($M = 22.03$; $SD = 5.39$), $p = .709$, $d = 0.193$, but the Explanation—Video Condition did ($M = 25.33$; $SD = 5.37$), $p = .023$, $d = 0.434$. No significant difference was found between the Explanation—Writing and Explanation—Video Condition, $p = .405$, $d = 0.241$. There were no interaction effects (Test Moment × Pretest scores: $F < 1$; Test Moment × Instruction Condition: $F(2, 113) = 1.68$, $p = .191$, $\eta_p^2 = .029$). With regard to transfer, students' pretest scores were a significant predictor, $F(1, 113) = 9.55$, $p = .003$, $\eta_p^2 = .078$. There was no main effect of Test Moment or Instruction Condition, nor interaction effects, Fs < 1.

As for perceived mental effort invested in the learning phase, an ANOVA showed no significant effect of Instruction Condition, $F < 1$, on mental effort ratings in the first learning phase (booklet 2, test study intention or explanation study intention). There was a significant effect of Instruction Condition on the perceived mental effort investment in the second learning phase (booklet 3; restudying or writing explanations), $F(2, 122) = 21.48$, $p < .001$, $\eta_p^2 = .260$. Students in the Test Condition ($M = 2.90$; $SD = 1.96$) reported having invested significantly less effort in this phase than both the Explanation—Writing Condition ($M = 4.64$; $SD = 1.76$), $p < .001$, $d = 0.933$, and the Explanation—Video Condition ($M = 5.71$; $SD = 2.17$), $p < .001$, $d = 1.960$. Furthermore, the Explanation—Video Condition reported having invested more effort in this phase than the Explanation—Writing Condition, $p = .042$, $d = 0.542$.

Perceived mental effort invested in the test was analyzed with a repeated measures ANOVA with Test Moment (Immediate vs. Delayed) as within-subjects factor and condition as between-subjects factor. On perceived mental effort invested in solving the items measuring learning, a main effect of Test Moment was found, $F(1, 114) = 13.89$, $p < .001$, $\eta_p^2 = .109$. This indicated that participants, on average, reported to have invested less mental effort on the Delayed Posttest ($M = 2.65$; $SD = 1.27$) than on the Immediate Posttest ($M = 3.02$; $SD = 1.42$). There was no main effect of Instruction Condition, $F < 1$, nor a significant interaction, $F < 1$. On the items measuring transfer performance, invested mental effort ratings showed a similar pattern. That is, there was a main effect of Test Moment, $F(1, 114) = 55.37$, $p < .001$, $\eta_p^2 = .327$, with participants reporting less mental effort investment on the Delayed Posttest ($M = 3.63$; $SD = 2.13$) than on the Immediate Posttest ($M = 4.82$; $SD = 2.20$), but no main effect of Instruction Condition, $F < 1$, nor a significant interaction effect, $F(2, 114) = 2.20$, $p = .116$, $\eta_p^2 = .037$.

As for perceived competence, an ANOVA on students' confidence in being able to learn the content of the materials before example study showed no significant effect of Instruction Condition, $F < 1$. As for students confidence in answering questions on a test, a repeated measures ANOVA showed a main effect of Test Moment, $F(1, 114) = 8.46$, $p = .004$, $\eta_p^2 = .069$, indicating higher perceived competence on the Immediate Posttest ($M = 5.88$; $SD = 0.86$) than on the Delayed Posttest ($M = 5.66$; $SD = 0.88$). There was no main effect of Instruction Condition, nor a significant interaction effect (both Fs < 1). So although the study by Hoogerheide et al. (2014a) seemed to indicate that explaining on video might reduce students' confidence in their own capabilities, no such indications were found here.

We also measured to what degree participants felt that they had enough time for the second learning phase (booklet 3; restudying or writing explanations). An ANOVA showed a main effect of Instruction Condition, $F(2, 122) = 69.07$, $p < .001$, $\eta_p^2 = .531$. There was no difference between the Test Condition ($M = 6.66$, $SD = 2.03$) and Explanation—Video Condition ($M = 7.10$, $SD = 1.74$), $p = .826$, $d = 0.232$, and the means suggest these students felt they had sufficient time. However, the Explanation—Writing Condition ($M = 2.86$, $SD = 1.66$) reported much lower scores than the Test Condition, $p < .001$, $d = 2.049$, and the Explanation—Video Condition, $p < .001$, $d = 2.493$. This indicates that students in this condition would have preferred to have more time for explaining. Although this might potentially explain why there was no benefit of the writing condition over the restudy condition, it seems that a more likely

Table 4
Mean (*SD*) of mental effort ratings (effort; range 1–9) and perceived competence (pc; range 1–7) per condition in Experiment 2.

	Test intention—restudy (*test condition*)	Explanation intention—explain in writing (*explanation—writing condition*)	Explanation intention—explain on video (*explanation—video condition*)
Learning phase 1 (effort)	3.78 (1.33)	4.07 (1.76)	4.24 (1.80)
Learning phase 2 (effort)	2.90 (1.96)	4.64 (1.76)	5.71 (2.17)
Pretest (effort)	2.96 (1.17)	3.26 (1.19)	3.12 (1.04)
Learning immediate posttest (effort)	3.10 (1.50)	2.89 (1.23)	3.05 (1.54)
Learning delayed posttest (effort)	2.74 (1.31)	2.68 (1.23)	2.53 (1.29)
Transfer immediate posttest (effort)	4.81 (1.98)	4.66 (2.27)	4.96 (2.36)
Transfer delayed posttest (effort)	3.65 (2.06)	3.88 (2.29)	3.38 (2.05)
Pretest (pc)	6.00 (0.95)	5.90 (1.03)	5.87 (0.99)
Immediate posttest (pc)	5.86 (0.80)	5.78 (1.02)	6.00 (0.74)
Delayed posttest (pc)	5.60 (0.96)	5.68 (0.85)	5.70 (0.84)

explanation lies in the differences between writing and video creation, as we will discuss below.

In sum, Experiment 2 replicated the findings from Experiment 1, showing that providing written explanations was a more effortful activity that did not contribute to learning outcomes compared to restudy. Surprisingly, Experiment 2 failed to replicate prior findings that explaining by making a video with a webcam would have a significant beneficial effect on transfer performance compared to restudy (Hoogerheide et al., 2014a). We did replicate prior findings that explaining on video was more beneficial for learning than restudy (Fiorella & Mayer, 2013, 2014; Hoogerheide et al., 2014a)—although it was not significantly better than explaining in writing. A potential explanation for the fact that explaining on video is more effective for learning than restudying while explaining in writing is not is that explaining in front of a webcam may enhance feelings of social presence. That is, it may give students a stronger feeling that they are communicating information to an actual other person (even though that person is not present at the moment) than writing does. Increased feelings of social presence could be beneficial for learning. Students may, for instance, monitor whether the (imagined) audience will be able to understand the explanation, which would provide a good indicator of how well she understands it.

If this explanation holds true, then we should find more indications of audience-directed utterances (e.g., 'you') in the video explanations than in the written explanations. We explored this by counting the number of times participants used the self-other referential words 'me', 'you', 'us', 'we', 'your', and 'yourself' in their explanation, dividing this by the total number of words they used in their explanation and multiplying the result by 100 to get a percentage score. Counting such pronouns is a common method for assessing social presence in asynchronous computer-based communication and teacher–student interaction as they connote feelings of closeness and association (Rourke, Anderson, Garrison, & Archer, 1999; Sanders & Wiseman, 1990). Data from three participants in the Explanation—Video Condition were unavailable for this analysis as a result of a malfunction in the audio recording software. An independent samples t-test showed that the video explanations indeed contained a significantly higher percentage of those self-other referential words ($M = 5.42\%$, $SD = 1.73\%$) than written explanations ($M = 2.16\%$, $SD = 2.44\%$), $t(80) = 6.98$, $p < .001$, $d = 1.537$.

6. General discussion

This study investigated whether studying a text with the intention to explain learned material to someone else would be more effective than studying to complete a test, and whether explaining to fictitious others in writing would yield the same benefits as explaining to fictitious others on video (Fiorella & Mayer, 2013, 2014; Hoogerheide et al., 2014a). Regarding study intention, we did not find any indications in Experiment 1 that studying a text with an explanation intention would be more effective or efficient than studying with a test intention on an immediate test. Note that prior research has also found mixed results regarding the effectiveness of an explanation study intention that is not followed by actually providing explanations (Bargh & Schul, 1980; Fiorella & Mayer, 2014; Hoogerheide et al., 2014a; Nestojko et al., 2014; Renkl, 1995). All in all, there seems to be little evidence that studying with the intention of explaining the material to others helps learning unless it is actually followed by explaining—but not just any kind of explaining, as our results show.

Our experiments provided no evidence that explaining to a non-present fictitious other student in writing would be more beneficial for learning outcomes than restudy. Explaining in writing was actually less efficient for learning, in the sense that it required more effort than restudy while this additional effort investment did not pay off in terms of improved learning. One could argue in Experiment 1 that this additional effort was probably invested in more elaboration which would lead to deeper learning, the benefits of which might show only after a delay (Fiorella & Mayer, 2014). Experiment 2 did include a delayed test, yet still found explaining in writing to be less efficient than restudy.

Explaining to a non-present fictitious other student on video was also more effortful than restudy and even than explaining in writing, but this additional effort relative to restudy did pay off. That is, it resulted in better learning with a medium effect size (i.e., this effort was invested in processes that were germane to, or effective, for learning, e.g., Paas et al., 2003; Paas & Van Gog, 2006; see also research on desirable difficulties, e.g., Bjork & Bjork, 2011). The question is, then, why is explaining on video more effective than restudy while explaining in writing is not? We hypothesized that this might be due to increased feelings of social presence when explaining in front of a camera compared to producing written explanations. In terms of the social presence definition provided by Gunawardena (1995), producing video explanations might make the potential recipients feel "more real" (this has also been referred to as 'immediacy': Andersen, 1979; Wiener & Mehrabian, 1968). Consequently, students may be more inclined to take the perspective of their (imagined) audience into account while generating explanations, which may evoke several processes that could aid their own learning. For instance, imagining an audience may evoke students to believe that their actions (i.e., the explanations) can affect others (cf. productive agency; (Okita & Schwartz, 2013; Schwartz, 1999). Consequently, they may monitor whether their explanations are comprehensible for their (imagined) audience, which provides a good indicator of how well they understand and explain it.

Moreover, if explaining on video stimulates learners to be aware of their potential audience, their level of arousal may increase (e.g., the Trier Social Stress Test also encompasses speaking for five minutes in front of a camera with the aim of inducing arousal; Kirschbaum, Pirke, & Hellhammer, 1993), which could affect their learning. It is well-established that the presence of an actual audience can affect how well people perform on a task (Aiello & Douthitt, 2001; Bond & Titus, 1983; Zajonc, 1965), and that arousal contributes to this audience effect (Aiello & Douthitt, 2001; Uziel, 2007). Importantly, situations that are located higher on the perceived social presence continuum seem to evoke stronger arousal responses. For example, being led to believe that another person in the room cannot see you decreases arousal levels compared to believing that they can (Myllyneva & Hietanen, 2015), and another person's direct gaze leads to higher arousal levels than a person's averted gaze (Helminen, Kaasinen, & Hietanen, 2011). Interestingly, no arousing effect of direct gaze occurs when pictures of people are presented on a screen (cf. Hietanen, Leppänen, Peltola, Linna-aho, & Ruuhiala, 2008; Pönkänen, Peltola, & Hietanen, 2011). With more credible manipulations, however, an imagined audience can also lead to more arousal. For example, Somerville et al. (2013) found that people who lay in a neuroimaging scanner experience higher arousal levels if they were led to believe that they were being watched by a peer via a camera embedded in the scanner than when they believed that the camera was off.

With regard to the relationship between arousal and learning, it has long been believed that there is an inverted U-shape function for the relationship between arousal and task performance (Salehi, Cordero, & Sandi, 2010; Yerkes & Dodson, 1908). Research indeed seems to indicate that relative to conditions of low or high arousal, moderate arousal levels can foster cognitive processes that are important for learning, such as memory, attention, and alertness (Arnsten, 2009; Diamond, Campbell, Park, Halonen, & Zoladz, 2007; Roozendaal, 2002; Sauro, Jorgensen, & Pedlow, 2003). Interestingly, Okita, Bailenson, and Schwartz (2007) demonstrated the link between social presence and arousal and learning for students learning how the human body deals with a fever. In their study, students who asked questions to a computer-based agent and then received scripted answers showed higher arousal levels when they were led to believe that the agent was controlled by an actual person than when they were led to believe that the agent was computer-controlled. Importantly, those who believed that the agent was an actual person performed better on a posttest, and students' posttest scores and arousal during learning were positively correlated.

In line with our hypothesis that explaining in front of a camera leads to increased feelings of social presence compared to producing written explanations, an explorative analysis of students' utterances showed that video explanations contained 2.5 times more self-other referential expressions (such as 'you' or 'we') than written explanations (and our measure corrected for explanation length). This finding resonates well with findings from research on asynchronous communication. Although an asynchronous communication situation is slightly different from explaining to a fictitious other because learners may know each other (i.e., their audience) and may receive delayed replies, it is similar in the sense that messages are generated with the intention of being shared with others who are not present and cannot respond at that moment. Research on asynchronous communication has shown that social presence can be established using text only (e.g., asynchronous discussion forums; Andresen, 2009), although the lack of visual and vocal cues can make it difficult to do so (Garrison, Anderson, & Archer, 2000; Tu & McIsaac, 2002). The lack of vocal and visual cues associated with written communication has even been proposed as an explanation for the high attrition rate found in online education (Carr, 2000; Patterson & McFadden, 2009), possibly because learners feel isolated when such cues are not present in the learning environment (Palloff & Pratt, 2007). Consequently, asynchronous video communication has been proposed and is more frequently used as a means to increase the richness of communication (Borup et al., 2013). In the field of multimedia learning, the presence of social cues, such as a human voice compared to a machine-generated voice and a conversational speaking style opposed to a more formal one, has indeed been shown to positively affect the quality of learning outcomes. These benefits presumably arise because social cues induce a social response in the learner which leads to an increase in active processing (Mayer, 2014).

A potential limitation of the current study is that we cannot exclude the possibility that writing would also have been more beneficial than restudy if students would have had more time available for explaining. Compared to Fiorella and Mayer (2013, 2014) and Hoogerheide et al. (2014a), however, the time available for explaining had already been increased in the present experiments. Moreover, because the restudy and video condition indicated that they had sufficient time available, giving a writing group more time would result in unequal time spent on the task, and it would become unclear whether any potential benefits of writing would then be due to the explanation activity itself or the increased time on task. A second potential limitation is that we only used one type of learning task. Further research is needed to test whether beneficial effects of explaining on video can be generalized to other domains and types of tasks, although findings by Fiorella & Mayer (2013, 2014) suggest that it would. Their study showed that video explanations fostered comprehension when learning a short text about the Doppler Effect, which is very different from the syllogisms studied by our participants.

Despite these limitations, our findings add to the explanation literature by showing that in addition to providing self-explanations (e.g., Chi et al., 1989, 1994; Renkl, 1997, 2002), explanations to others in interactive situations (e.g., Cohen, 1994; Cohen et al., 1982; Johnson et al., 2007) or explanations to present others in non-interactive situations (Coleman et al., 1997), explaining to non-present, fictitious others, is also effective for learning. This beneficial effect seems to be qualified by the form in which such explanations are provided: Explaining on video was effective, in writing it was not. These findings are also of interest for educational practice. Having students explain in writing is arguably much easier to implement, but it seems to yield little benefit. With cameras becoming ubiquitous (e.g., webcams, cameras in phones, tablets, laptops) and opportunities for storing and sharing video (online) becoming more affordable and accessible, video-based instruction is increasingly being used in educational practice. Moreover, a study procedure similar to the one used in this study can easily be implemented. The effectiveness of producing video explanations may even increase when students get the opportunity to edit and re-do their products (cf. learning by designing hypermedia: Lehrer & Romberg, 1996; Penner, Lehrer, & Schauble, 1998; or by designing "slow animations": Hoban, Loughran, & Nielsen, 2011) or when they collaboratively create the videos (cf. Zahn, Krauskopf, Hesse, & Pea, 2012; Zahn et al., 2014).

To conclude, this study showed that explaining to fictitious others on video can be an effective learning activity compared to restudy, whereas explaining in writing is not. Considering that we found no direct differences between explaining in writing and on video and that we did not replicate the beneficial effect of explaining on video on transfer (cf. Hoogerheide et al., 2014a), it is important that these findings are replicated in future research. Such a replication could go hand in hand with a focus on mechanisms that make explaining on video effective. We hypothesized and provided some tentative evidence that these might lie in feelings of social presence, and qualitative analysis of the explanation process data in future research could perhaps shed more light on this and other mechanisms that make explaining on video more effective than restudy, but not explaining in writing (note that this would require log data of the writing process, instead of just the end product). Moreover, applied future research should investigate the effectiveness of providing video explanations when used in real classroom situations.

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