UNIVERSITY OF CALIFORNIA

Santa Barbara

Training Transferable Knowledge with Games

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy
in Psychological and Brain Sciences

by

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September 2016

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May 2016

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ACKNOWLEDGMENTS

I would like to thank the members of my dissertation committee for their invaluable feedback and guidance: Richard Mayer, Mary Hegarty, Michael Miller, and Russell Revlin. Special thanks to Richard Mayer for his extraordinary mentoring and for chairing my dissertation committee.

This work was made possible by excellent undergraduate research assistants, including Kelsey James, Garrison Wells, Angela Hung, Brooke Boehmer, and Shany Bloch.

Financial support from the National Science Foundation, Office of Naval Research, and American Psychological Association.

Thanks to Logan Fiorella, Deanne Adams, Alexander Swan, and the members of the Monday research group. Thanks to Philip Vieira and Stampy. Thanks to the Pilegard family, including Cris, Stella, Cole, Ryan, Carly, Willow, and Grandma Martha.

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ABSTRACT

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Despite popular enthusiasm for using video games in the classroom, a review of the research reveals a frequent lack of meaningful (i.e., transferable) learning outcomes resulting from video game play (Mayer, 2014). One explanation could be that video game environments are fast and forward-moving, whereas learning that leads to transfer is reflective, effortful, and requires integrating new information with prior knowledge. What can be added to computer games to facilitate transferable learning? In Experiments 1 and 2, participants played a computer game called *Cache 17,* which has a narrative cover story about finding stolen art as well as instructional information about electromechanical devices. The player's main goal can be to win (i.e., find the art) rather than to understand the instructional content (i.e., understand how a wet cell battery works). In Experiments 1 and 2, students who filled out worksheets about the devices in *Cache 17* during game play outperformed a control group without worksheets on a transfer test, demonstrating that the worksheets helped students focus their limited cognitive resources on the learning material. Experiments 3a and 3b investigated how to train transferable spatial skills with *Tetris*. Previous research shows that *Tetris* experts are better than non-experts at mentally rotating *Tetris* shapes, but nothing else. In Experiment 3a, participants completed a series of lessons and worksheets on *Tetris* problem-solving with the goal of building a declarative knowledge base to use as a basis for transferable spatial skills. One group completed these lessons and played *Tetris* and one group played *Tetris* only. Pre- and post-training tests measured spatial and cognitive skills related to *Tetris* play. Experiment 3b added an inactive control condition that took the pre- and post-training measures but did not complete any training. The results of Experiments 3a and 3b indicated no benefit of playing Tetris with or without additional training on gains in any spatial or cognitive measure when controlling for pre-training performance. This research helps develop principles for how transferable learning can be facilitated with games.

Table of Contents

Chapter I: Introduction

When students progress through a computer game, they develop in-game knowledge and skills that help them perform well in the game. Is the knowledge that students gain specific to the game context, or can elements of that knowledge be useful in new situations? The experiments in this dissertation examine methods to help train transferable knowledge with computer games. Experiments 1 and 2 investigate training transferable conceptual knowledge with a narrative game, and Experiments 3a and 3b investigate training transferable procedural knowledge with a puzzle game.

Games are unique learning environments. The essential characteristics of games include the following qualities (Mayer, 2014a; Mayer & Johnson, 2010): games are *rule-based*, in that they operate with a limited set of rules that the player can learn; games are *responsive*, in that a player can act in the game environment and the player's actions are reflected by changes in the game environment; games are *cumulative*, in that a player's previous behaviors are reflected by the state of the game environment, allowing the player to track progress toward goals; games are *challenging*, in that games pose difficult tasks for the player to complete; and games are *inviting*, in that games are enjoyable and appealing to the player. These features of games may contribute to an immersive quality and fluency that facilitate sustained gameplay.

Fluency, however, is not a characteristic typically associated with training transferable conceptual or procedural knowledge. Conceptual knowledge is characterized by structured semantic information with interrelations among its parts. The transfer of conceptual knowledge, such as when learners use concepts they learned to solve novel problems, requires learners to engage in active sense-making during learning (Mayer, 2009).

Procedural knowledge, including cognitive skills, is implicit knowledge characterized by cognitive guidance at early stages of learning, and increased automaticity at later stages of learning. Fluency leads to gains during the training of procedural knowledge but does not lead to the transfer of procedural knowledge, such as when training in one skill increases performance in another skill or situation (Schmidt & Bjork, 1992). Perhaps as a result, recent analyses have revealed an ambivalent research base on achieving transfer from games (Mayer, 2014a; National Research Council, 2011; O'Neil & Perez, 2008; Tobias & Fletcher, 2011). Game environments therefore offer both opportunities and challenges as educational tools. One such challenge is the focus of this study—what can be added to games to promote cognitive processing that leads to transferable knowledge?

Theoretical Overview

In order to define what transferable knowledge is, it is useful to first define what it is not. Transferable knowledge is qualitatively different than rote knowledge (Mayer, 2002). Rote knowledge is gained when a learner is able to reproduce the information in a lesson, but is not able to use the information in meaningful ways (Wertheimer, 1945). Colloquially, this type of learning is referred to as *memorizing*. A learner who gains rote knowledge would perform well on a retention test, which requires remembering the information in a lesson.

In contrast, transferable knowledge is gained when a learner makes connections within the information in the lesson they are learning, as well as between the information in the lesson and their prior knowledge. Colloquially, this is the type of learning is referred to as *understanding.* The cognitive processing required to achieve this type of knowledge is referred to as *generative processing* (Mayer, 2009, 2014b; Wittrock, 1990), *germane load*

(Sweller, Ayres, & Kalyuga, 2011), or *productive thinking* (Wertheimer, 1945). Transfer tests require learners to use the information from a lesson in a novel way, such as using the information to solve problems (Mayer & Wittrock, 2006).

Transfer can take a variety of forms depending on the domain of interest. The two types of transfer that are the focus of this study are the transfer of conceptual knowledge and the transfer of cognitive skills. Conceptual information has an underlying structure and interrelations among its parts (Krathwohl, 2002; Mayer, 2008). In order to understand conceptual information, students must construct a coherent mental representation of the information (Kintsch, 1998; van Dijk & Kintsch, 1983). For example, science content is often characterized by conceptual information. Because science information consists of many processes, the underlying structure of science information is often a cause-and-effect chain (Cook & Mayer, 1988; Meyer & Freedle, 1984; Meyer, 1975). Examples of such cause-and-effect chains are how a solar cell works (i.e., free electrons from a top layer of n-type silicon bond to empty sites in a bottom layer of p-type silicon; photons from sun knock electrons loose from the top layer, causing chain of electron displacement resulting in current), or the life cycle of fungi (i.e., environmental conditions cause mycelium to grow mushrooms above ground; mushrooms release spores; spores germinate in threads of hyphae which join to form mycelium). Transfer questions require learners to have a structural (rather than surface-level) understanding of the learning material.

Mayer (2009) classifies four types of transfer questions for conceptual knowledge: redesign questions, which ask students to change something in a system in order to achieve a particular outcome; troubleshooting questions, which ask students to explain what could cause a system to fail; prediction questions, which ask students to explain how elements of the system affect the system as a whole; and conceptual questions, which require students to explain underlying principles in the lesson. For example, a troubleshooting transfer question about how solar cells work is "It's a sunny day but there is no power coming from the solar cell. Why not?" A prediction transfer question about the life cycle of fungi is, "Mark does not like the mushrooms growing in his backyard so he kicks them over whenever they pop up. Is this an effective strategy? Why or why not?" Neither of these questions have answers that are directly addressed in the lessons. However, if students have an understanding of the cause-and-effect chain taught in the lesson, they will be able to use their conceptual knowledge to generate answers.

In the domain of skill learning, transfer occurs when practice in one skill leads to improvement on another skill (Singley & Anderson, 1989). A skill is an acquired procedural memory and thus a form of implicit, nondeclarative memory (Schacter & Tulving, 1994; Squire & Zola, 1996). New skills are acquired in a series of three stages from a high degree of conscious control in the first stage to no conscious control necessary in the third stage (Fitts & Posner, 1967). Skills are distinct from other types of learning in that they do not require conscious guidance to be performed once the third stage of skill acquisition is reached. A cognitive skill is characterized by cognitive operations, rather than motor output (Schacter & Tulving, 1994). Cognitive skills are learned in three stages analogous to the stages of general skill acquisition: a declarative stage, a knowledge compilation stage, in which knowledge is translated from declarative to procedural, and a procedural stage (Anderson, 1982). Cognitive skill learning is often discussed in reference to experts with unique training (Anderson, 1982; Ericsson & Smith, 1991), however normal adults demonstrate expert performance in a variety of common skills (Posner, Digirolamo, & Fernandez-Duque, 1997).

The *transfer of cognitive skills* occurs when an individual practices and improves performance in one skill, and this improvement leads to increased performance on another task. The transfer of cognitive skills can be elusive, and as a result a number of researchers in the history of psychology have questioned whether it is possible. In response to a Victorian era notion that learning certain skills (such as Latin or geometry, the primary subjects in 19th century Latin schools) would foster "proper habits of mind" (Mayer, 2008, p. 27), Thorndike and Woodworth (1901) demonstrated that training specific skills had no effect on generalized abilities. Thorndike and Woodworth described the mind as a highly specific machine, with transfer between tasks only occurring when the tasks share *identical elements*. Early critics of Thorndike's view suggested that broader transfer is possible if instruction is meaningful (Wertheimer, 1945) or focuses on *cultivating generalization* (Judd, 1908).

Modern conceptualizations of the transfer of cognitive skill define three ways to think of transfer: specific transfer, such as that described by Thorndike and Woodworth, in which learning one skill will only help learning a second skill to the extent that the two skills have identical elements in common; general transfer, such as the Victorian notion of "proper habits of mind," in which learning a specific skill helps the functioning of the mind in general; and specific transfer of general principles, in which a learner gains one skill, and if there are general principles underlying that skill that apply to a second task, they will facilitate learning the second task (Mayer & Wittrock, 1996; Salomon & Perkins, 1989). While modern research does not support the existence of general transfer, there is support for the view that under appropriate circumstances learning a cognitive skill in one context can facilitate the use of that skill in a second context (i.e., specific transfer of general skill) (Mayer & Wittrock, 2006).

Can transferable knowledge be acquired while playing a computer-based game? The rise of educational games has been met with abundant enthusiasm (Mayer, 2014a). However, the field of educational games remains under-researched, and the existing results paint an ambivalent picture of the effectiveness of games as educational tools (Mayer, 2014a; National Research Council, 2011; O'Neil & Perez, 2008; Tobias & Fletcher, 2011). The goal of this set of experiments is to investigate conditions that help facilitate transferable knowledge—both transfer of conceptual knowledge in a narrative adventure game and transfer of cognitive skill in spatial puzzle game.

Chapter II: Experiment 1

The goal of Experiment 1 is to determine whether adding worksheets to a narrative game can promote transfer of the conceptual knowledge learned in the game. A narrative game (or adventure game) is a game that has a cover story that poses goals for the player. For example, as exemplified in Figure 1, in *Cache 17* (Koenig, 2008), the player views a cut scene showing that the player's goal is to recover stolen artwork that is hidden in a WWII bunker system, and along the way the player must build a wet-cell battery that can open a stuck door. As summarized in Table 1, in narrative games for learning there can be an inherent conflict between the goal of the game based on the narrative and the goal of the game based on the instructional objective. In the case of *Cache 17*, for example, the narrative theme suggests that the goal is to recover stolen artwork, whereas the instructional goal is to help students learn about electromechanical devices. The narrative theme is intended to prime player motivation and can be expressed through persistence and intensity of game play, whereas the instructional material is intended to prime appropriate cognitive processing such as attending to the relevant information and trying to make sense of it.

Although narrative games for learning may prime the player's motivation, there is danger that the player's main goal can be to win the game rather than to understand the instructional content that is encountered in the game. For example, Adams, Mayer, MacNamara, Koenig, and Wainess (2012) found that students learned better about electromechanical devices from a PowerPoint presentation than from playing *Cache 17*. This result also reflects a larger pattern in research on active learning: teaching methods that promote behaviorally active learning do not necessarily promote cognitively active learning (Mayer, 2004; Mayer & Wittrock, 2006). While students are ostensibly engaging with educational material by playing an educational game, this does not necessarily mean that they are engaging with the cognitive processing necessary for meaningful learning. In the case of narrative games, players may engage with the narrative and the game mechanics instead of engaging in the effortful processing it takes to make sense of the educational material (Adams et al., 2012).

The value-added approach to research on educational games (Mayer, 2011) seeks to identify features that enhance learning by comparing a base version of a game to a version with an added feature. Recent meta-analyses by Mayer (2014a) have identified promising features such as adding hints and advice throughout the game, using conversational wording rather than formal wording, using spoken text rather than printed text, prompting students to explain the material to themselves as they learn, and providing pre-training. Although these guidelines can help game designers build effective games, features that require modifying the game itself can be prohibitive for educators using off-the-shelf games. The addition of simple adjunct materials to games, such as paper-based worksheets (Fiorella & Mayer, 2012) or instructional slides (Erhel & Jamet, 2013), is therefore a more practical domain of investigation.

The goal of the present study is to examine a low-cost technique intended to focus the narrative game player on cognitive processing relevant to the instructional goal, namely the use of pre-game and in-game worksheets. The pre-game worksheet is a sheet of paper that asks the player to write an explanation of how a wet-cell battery works, thereby drawing attention to the major instructional goal in the game. The in-game worksheet is a sheet of paper that asks the player to fill in answers concerning how to build a wet-cell battery during the game.

Theory and Predictions

The rationale for using simple pre-game and in-game worksheets is to increase appropriate cognitive processing aimed at the instructional objective, while still allowing the narrative game player to maintain motivation. A theoretical goal is to determine whether a simple device such as adjunct worksheets can encourage players to focus their limited cognitive resources on understanding the instructional material. The goal of the adjunct worksheets is to reduce the amount of cognitive resources dedicated to information outside the instructional goal [i.e., what Mayer (2009) calls *extraneous processing* in the Cognitive Theory of Multimedia Learning (CTML), or what Sweller, Ayres, & Kalyuga (2011) call *extraneous load* in Cognitive Load Theory (CLT)] in order to allow the learner to dedicate more resources to processing relevant information (i.e., *essential processing* in CTML or *intrinsic load* in CLT) and to making sense of the relevant information (i.e., *generative processing* in CTML or *germane load* in CLT). These processes can be inferred through enhanced retention of key information on a comprehension test and improved performance on a problem-solving transfer test. The primary prediction of this experiment is that adding pre-game and in-game worksheets that require students to reflect on the educational material in the game would help learners reduce the extraneous processing caused by the narrative element of *Cache 17*, and dedicate more cognitive processing to understanding the material. Therefore, participants who receive the worksheets are predicted to perform better on a transfer test and on retention of the instructional material. This is the worksheet hypothesis.

Method

Participants and design. The participants were 62 undergraduates at the University of California, Santa Barbara. Participants were recruited from the Psychology

Subject Pool and fulfilled a course requirement by participating in the experiment. There were 28 men and 34 women, and the mean age was 19.1 (*SD* = 1.3). Twenty-three participants indicated that they do not play video games, 13 typically played less than 30 minutes per week, 16 played 1 to 5 hours per week, 10 played 5 to 10 hours per week, and none played more than 10 hours per week. The mean score on a self-report scale of prior knowledge of electricity was 2.1 out of 5 (*SD* = 1.0), which is considered low. Thirty participants served in the worksheet condition and thirty-two participants served in the control condition.

Materials.

Paper-based materials. The paper-based materials consisted of a consent form, demographic questionnaire, game instructions, pre-game worksheet, in-game worksheet, post-game worksheet, transfer test sheet, comprehension test sheet, post-game questionnaire, and debriefing slip.

The demographic questionnaire asked for basic demographic information (e.g., gender and age), time spent playing video games per week, and prior knowledge about electricity. Time spent playing video games was assessed by the item, "How much time per week do you typically play video games?" with five response options: "I do not play video games"; "Less than 1 hour per week"; "1 to 5 hours per week"; "5 to 10 hours per week"; and "More than 10 hours per week". Prior knowledge about electricity was measured with a question asking participants to rate their knowledge of how electricity works on a scale from 1 ("very low") to 5 ("very high") and to complete a checklist of 13 electricity-related experiences. The checklist read, "Please place a check mark next to the items that apply to you: ___I own a book of basic electrical/electronic repair; ___ I enjoy watching documentaries about science on the Discovery Channel; ___ I have rewired an electrical device; ___ I have used rechargeable batteries; ___ I have built an electrical circuit; ___ I know the difference between AC and DC; ___ I have used a multi-meter to measure amperage, voltage, or resistance; ___ I know the formula to calculate Wattage; ___ I have soldered a circuit board; ___ I know Ohm's Law; ___ I have "jumped" a dead car battery; ___ I have installed a new light switch or electrical outlet; ___ My father/mother pursues a professional career in electricity/electronics."

The game instructions were printed double-sided on a single sheet of paper. They included instructions for how to play *Cache 17*, such as how to navigate in the environment, how to pick up objects, and how to use the tools and resources in the game.

The pre-game worksheet was a single sheet of paper including text at the top of sheet stating, "In this experiment you will be playing a game called *Cache 17*. The purpose of this game is to teach you about electric circuits. As you play, you will learn about different concepts related to electric circuits, such as how a wet-cell battery works. Before you begin the game, we would like you to write an explanation of how a wet-cell battery works. It's ok if you don't know much about how they work now. As you play the game, make sure to pay attention to information that will help you write a better explanation after playing." Below that text were instructions that read, "Please write a paragraph explaining how wet-cell batteries work, and label the diagram of a wet-cell battery below." These instructions were followed by blank space to write an explanation. At the bottom of the page there was a fill-in-the-blank diagram of a wet-cell battery (as shown in Figure 2). The post-game worksheet was identical to the pre-game worksheet, but omitted the initial text at the top of the sheet.

The in-game worksheet was a single sheet of paper with three questions and space to write in answers. The instructions at the top told participants to fill in the worksheet while playing the game and complete the worksheet before finishing the game. The three questions asked, “What are the parts of a wet-cell battery?,” “How do you choose metals for a wet-cell battery?,” and “How do you put the parts of a wet-cell battery together?”

The transfer test included four questions, each on a separate sheet of paper. The four questions reflected the four classes of transfer questions laid out by Mayer (2009): troubleshooting (“Two metals are submerged in a liquid and connected to a light bulb, but the bulb is not lit up. Why not? Name as many reasons as you can think of.”), redesign (“What could you do to increase the voltage of a wet cell battery?”), prediction (“What would happen if you used two of the same metal to build a wet cell battery? Why?”) and conceptual (“What does a brine solution have to do with wet cell batteries?”). At the bottom of each sheet was the statement: “Please keep working until you are told to stop.”

The comprehension test consisted of 17 questions intended to assess participants’ comprehension of the learning material in the game. Eight of the questions referred to wet-cell batteries (task 1 in the game), five referred to electric generators (task 2 in the game), and four referred to series and parallel circuits (task 3 in the game). The 8 questions about wet cell batteries are considered a test of intentional learning (i.e., target information) because the intervention in this study deals only with the wet-cell battery portion of the learning material; the other 9 questions are labeled *incidental* learning (i.e., non-target information). The test questions were developed by Koenig (2008) for use as an embedded test in the original version of the game.

The post-game questionnaire asked four questions: "How difficult was the game you just played?" with 7 Likert-type responses from "Extremely easy" to "Extremely difficult"; "What level of effort did you put into the game you just played?" with 7 Likert-type responses from "Extremely low" to "Extremely high"; "Please rate your agreement: 'I would like to play more games like this one.'" with 7 Likert-type responses from "Strongly disagree" to "Strongly agree"; and "Please rate your agreement: 'I thought the game was fun.'" with 7 Likert-type responses from "Strongly disagree" to "Strongly agree."

The debriefing slip informed participants of the purpose of the experiment, told them to ask the experimenter if they had questions, and thanked them for their participation.

Cache 17. *Cache 17* is a 3-D, first person, narrative discovery learning game designed to teach concepts related to electric circuits. The game was developed by Koenig (2008) and intended for play on a desktop computer. Figure 1 shows screenshots from the game. *Cache 17* begins with a 5-minute cut scene that sets up the story—a male insurance investigator named Alex is investigating a stolen painting with his partner, Kate. Their investigation leads them to a bunker where the game begins. The player navigates the bunker as Alex.

Although the cover story sets the goal of the game as recovering stolen artwork from a bunker system, the instructional goal is to learn how electromechanical devices work such as a wet-cell battery, which is used to open a stuck door. Throughout the game, the player has resources available via a menu bar at the bottom of the screen: a map of the bunker, a multimeter to measure the voltage of devices, a *Notes* tab with goal of their current mission, and a personal digital assistant (PDA). The PDA contains educational information that can help the player complete the tasks in the game, such as information about electric circuits, the galvanic series of metals, and electric motors and generators. Players navigate the PDA through a drop-down menu.

The first task the player encounters in the game is to create a wet-cell battery in order to power a door panel and open a door. This task is completed by selecting from a variety of metals in a storage room, placing the correct metals in a brine solution, and connecting the metals to the door panel with jumper cables. There were 30 possible combinations of metals in the storage room but only 2 would generate the voltage to open the door panel. Behind the door is a prisoner that gives information about a vault to be opened, as well as materials required to complete the next two tasks. The second task is to charge a dead battery using a Stirling engine and an electric motor. The third task is to connect the recharged battery with another battery in series to open a vault.

After opening the vault, the player learns that Kate was a double agent and they do not retrieve the painting. The game is completed when they exit the bunker through an escape hatch.

Apparatus. The apparatus consisted of five Dell desktop computers with 20-inch color monitors and Panasonic headphones, situated on tables in individual cubicles.

Procedure. The experiment took place in a laboratory with up to five participants per session. Participants were randomly assigned to a condition by session. Each participant was seated in an individual cubicle, facing a computer station, without visual access to the other participants. First, following a brief introduction from the experimenter, participants signed a consent form and filled out the demographic questionnaire. Participants in the control group were then given the game instruction sheet and told they would be playing an educational computer game called *Cache 17*. After a few simple instructions about the game, the experimenter asked for questions, turned on each computer screen, and instructed participants to wear their headphones and begin the game.

Participants in the worksheet group received the same procedure as those in the control group but completed the pre-game worksheet before playing the game and the in-game worksheet during the game. The experimenter read the instructions to them aloud and told them they had four minutes to complete the worksheet. After four minutes the worksheet was collected. Participants in the worksheet group were also given the in-game question worksheet and told to complete the worksheet while playing the game.

Participants were given 75 minutes to complete the game. If a participant did not finish the game in 75 minutes, they were instructed to stop playing. As participants finished they were given the first worksheet of the postgame test—the post-game explanation worksheet. They were given four minutes to complete the worksheet. Participants were then given two and a half minutes to complete each of the four transfer questions one at a time. This was followed by the comprehension test and post-game questionnaire, both of which were untimed. Participants were excused after reading a debriefing sheet with information about the experiment.

Results

Data source. Participants who did not complete all three tasks in the game within the allotted time were eliminated from the analysis. As a result, 23 participants remained in the worksheet group and 28 in the control group. There was not a significant difference between groups in the number of eliminated participants, $X^2(N = 62) = 1.25$, $p > .05$. All analyses reported in the results section refer to this subset of the participants.

Are the groups equivalent on basic characteristics? A preliminary step is to determine whether the groups are equivalent on basic demographic characteristics. Individual *t*-tests revealed no significant differences (at $p < .05$) between the worksheet and control groups on age, $t(49) = -0.53$, $p > .05$, or prior knowledge of electricity, $t(49) = -0.43$, $p > .05$. A chi-square test found no significant differences between the groups on proportion of men and women, $X^2(N = 51) = 0.17$, $p > .05$, or time spent playing video games per week, $X^2(N = 51) = 3.03$, $p > .05$. There were therefore no significant differences between the groups on basic characteristics.

Does adding worksheets affect in-game experience? Time to finish the game was based on the number of minutes it took players to reach the end of the game; time to finish was set at 75 minutes for students who completed all three tasks but did not complete the game within the 75-minute deadline. As expected, the worksheet group ($M = 56.93$, $SD = 10.44$) took significantly more time to finish the game (in minutes) than the control group ($M = 49.17$, $SD = 11.70$), $t(49) = 2.47$, $p = .02$. There was no significant difference between worksheet and control group on post-game ratings of difficulty, $t(49) = -0.63$, $p > .05$; effort, $t(49) = 1.39$, $p > .05$; liking the game, $t(49) = 1.13$, $p > .05$; or thinking the game was fun, $t(49) = 0.21$, $p > .05$. Thus, asking students to complete a worksheet during the game caused game play to take longer but did not affect other aspects of in-game experience.

Does adding worksheets affect the quality of explanations? The participant's explanation of the wet-cell battery on the post-game worksheet was scored for the number of correct idea units in the paragraph and diagram. Correct idea units were separated into conceptual idea units (i.e., ideas corresponding to the concept of how wet-cell batteries work such as "the metals must differ in voltage"; 15 possible) and verbatim idea units (i.e., ideas corresponding only to the specific wet-cell battery example in the game and not wet-cell batteries in general, such as "use copper and aluminum"; 5 possible). Participant responses were scored by a scorer blind to experimental condition. A second independent rater scored a subset of the tests. Inter-rater agreement on scores was high for conceptual idea units (r = 0.89) and verbatim idea units (r = 0.88). Analyses are based on the first rater's scores.

If the worksheets help learners process the academic material about wet-cell batteries more deeply, the worksheet group will outperform the control group on producing conceptual idea units but not on producing verbatim idea units. The top two lines in Table 2 shows the means and standard deviations for each group on the conceptual and verbatim parts of the explanation, respectively. As predicted, the worksheet group generated significantly more conceptual idea units than the control group, $t(49) = 3.25$, $p = .002$, $d = 0.92$; and the worksheet group generated significantly fewer verbatim idea units than the control group, $t(40.76) = -2.49$, $p = 0.02$, $d = -0.68$. A one-way repeated measures ANOVA revealed a significant interaction between condition (worksheet or control) and response type (conceptual or verbatim), $F(1,49) = 12.74$, $p = .001$. Overall, the worksheet group recalled more of the conceptual information regarding wet-cell batteries than the control group, whereas the control group recalled more of the game-specific information regarding wet-cell batteries than the worksheet group.

Does adding worksheets affect performance on the comprehension test? Students received one point for each correct answer on the comprehension test. Comprehension performance was divided into two scores: one for the 8 multiple choice questions about wet-cell batteries (i.e., intentional learning score) and another for the 9 multiple choice questions about electrical generators and series and parallel circuits (i.e., incidental learning score). If the worksheets help learners process the academic material about wet-cell batteries more deeply, the worksheet group will outperform the control group on intentional items but not incidental learning. The next two lines of Table 2 show the means and standard deviations for the worksheet and control group on the intentional items and the incidental items of the comprehension test. As predicted, the worksheet group performed significantly better than the control group on the intentional items, $t(49) = 2.32$, $p = .03$, $d = 0.67$, and there was no difference between the two groups in incidental items (i.e., those referring to electric generators or series and parallel circuits), $t(49) = -0.20$, $p > .05$, $d = -0.07$. The worksheets therefore improved comprehension performance for the targeted learning material but did not affect comprehension performance for the other material in the game.

Does adding worksheets affect transfer performance? Students received one point for each acceptable answer on each of the four transfer questions, based on a rubric listing possible answers. Participants who produced multiple acceptable answers for a question could earn more than one point. There was no defined maximum number of correct solutions a participant could generate. Transfer test score was determining by combining the points from all four transfer questions. Participant responses were scored by a scorer blind to experimental condition. A second independent rater scored a subset of the tests. Inter-rater agreement on scores was high, $r = 0.91$. Analyses are based on the first rater's scores.

If the worksheets cause learners to process the academic content about wet-cell batteries more deeply, then the worksheet group should outperform the control group on generating creative answers on the transfer test. The bottom line in Table 2 shows the mean and standard deviation for each group on the transfer test. As predicted, the worksheet group performed significantly better than the control group on transfer, $t(49) = 2.32$, $p = .02$, $d = 0.74$. Thus, the data suggest that completing the worksheets led to significantly better performance on a problem-solving transfer test.

Each of the significant differences for dependent variables in Table 2 (explanation-conceptual, explanation-verbatim, comprehension-intentional, and transfer) remained significant when ANCOVAs were conducted with condition (worksheet vs. control) as a fixed factor and time to finish the game as a covariate.

Discussion

Adding pre-game and in-game worksheets to *Cache 17*, a narrative game for learning, enhanced key learning outcomes, including writing conceptual explanations of how wet-cell batteries work, answering multiple-choice comprehension questions on the topic targeted by the worksheets, and solving transfer problems that require using the learning material in novel situations. Importantly, this intervention improved learning outcomes without affecting students' reported enjoyment of the game.

These results are consistent with the idea, based on the Cognitive Theory of Multimedia Learning and Cognitive Load Theory, that the worksheets helped students focus their limited cognitive resources on the educational aspect of *Cache 17*. Evidence that the worksheets reduce extraneous processing and encourage essential processing is reflected in enhanced conceptual explanations with reduced verbatim intrusions, and enhanced comprehension performance. Evidence that the worksheets helped learners engage in generative processing is reflected in enhanced transfer performance.

A practical implication of this study is that simple materials, such as paper-based worksheets, can be added to games to enhance learning without requiring modifications to the game itself. Further work is needed to identify which aspects of worksheets assist students in attending to and reflecting on the target educational information in narrative games. One aspect of the worksheets – their administration before or during game play – is the subject of the next experiment.

Chapter III: Experiment 2

Experiment 1 demonstrated that adding a pre-game and in-game worksheet to *Cache 17* can significantly improve several learning outcomes. Experiment 2 was designed to extend the results of Experiment 1 to determine whether the in-game or pre-game worksheet alone could affect learning. As a result, one group in the experiment was given only the pre-game worksheet, one group was given only the in-game worksheet, and a control group did not receive either worksheet.

The results of this experiment can help clarify the results found in Experiment 1. Instructions and goals can influence the type of information a learner attends to in a learning situation (Erhel & Jamet, 2013; Flavell, 1979; van den Broek, Lorch, Linderholm, & Gustafson, 2001). The pre-game worksheet asks participants to reflect on their prior knowledge, and tells them that they should pay attention to information in the game that will help them write a better explanation after the game. These instructions are intended to prime appropriate cognitive processes such as selecting information relevant to wet-cell batteries, organizing that information into an explanation, and integrating that information with what they knew before the game. If instructions and goal-setting before the game helps participants select, organize, and integrate information learned in the game, then the pre-game worksheet group will outperform the control group on transfer and retention tests. This is the pre-game worksheet hypothesis.

However, learners have limited cognitive capacity. Front-loading a game with instructions and goals to be remembered throughout the game may exceed the limits of the learner's cognitive system. Previous research shows that under conditions of high cognitive load, presenting a learning prompt too early may be as helpful as not providing a prompt at all (Helsdingen, van Gog, & van Merriënboer, 2011). In contrast, having a worksheet available during game-play may be more practical for focusing the learner's attention during the appropriate parts of game playing (van Merriënboer, Kirschner, & Kester, 2003). The in-game worksheet asks participants several questions about how wet-cell batteries work. These questions are intended to prime appropriate cognitive processes such as selecting information relevant to wet-cell batteries, reorganizing that information into relevant responses, and integrating the information with prior knowledge. If the simple in-game worksheet questions help participants select, organize, and integrate information learned in the game, then the in-game worksheet will outperform the control group on transfer and retention tests. This is the in-game worksheet hypothesis.

Method

Participants and design. One hundred sixty-one undergraduates from the University of California, Santa Barbara participated in the experiment. Participants were recruited from the Psychology Subject Pool and fulfilled a course requirement by participating in the experiment. There were 53 men, 105 women, and 3 participants who declined to state a gender. The mean age was 19.4 (*SD* = 2.7). The majority of participants reported playing video games for less than 1 hour per week and 10 participants played more than 10 hours per week. The mean score on a self-report scale of prior knowledge of electricity was 2.3 out of 5 (*SD* = 1.0), which is considered low. Fifty-six participants served in the pre-game worksheet condition, fifty participants served in the in-game worksheet condition, and fifty-five participants served in the control condition.

Materials. The materials in this experiment were identical to the materials used in Experiment 1.

Procedure. The procedure in this experiment was the same as Experiment 1, with the following exceptions: participants in the pre-game worksheet group received only the pre-game worksheet and did not receive the in-game worksheet; participants in the in-game worksheet group received only the in-game worksheet and did not receive the pre-game worksheet. The control group procedure was the same as Experiment 1.

Results

Data source. Participants who did not complete all three tasks in the game within the allotted time were eliminated from the analysis. As a result, 43 participants remain in the pre-game worksheet group, 42 participants remain in the in-game worksheet group, and 40 participants remain in the control group. There was not a significant difference among the groups in the number of eliminated participants, $X^2(N = 161) = 1.28$, $p = 0.529$. All analyses reported in the results section refer to this subset of the participants.

Are the groups equivalent on basic characteristics? A preliminary step is to determine whether the groups are equivalent on basic demographic characteristics. Individual ANOVAs revealed no significant differences (at $p < .05$) among the groups on age, $F(2,121) = 0.99$, $p = 0.375$, or prior knowledge of electricity, $F(2,121) = 0.38$, $p = 0.682$. A chi-square test found no significant differences among the groups on proportion of men and women, $X^2(N = 124) = 6.64$, $p = 0.156$. A Kruskall-Wallis test revealed no significant difference among groups on time spent playing video games per week, $X^2(N = 124) = 0.32$, $p = .851$. Therefore, the groups were not different on basic characteristics measured.

Does adding worksheets affect in-game experience? Time to finish the game was based on the number of minutes it took players to reach the end of the game; time to finish was set at 75 minutes for students who completed all three tasks but did not complete the game within the 75-minute deadline. Unlike Experiment 1 there was no effect of worksheets on play time (in minutes), with no significant difference among the pre-game worksheet group (M = 51.88, SD = 14.67), the in-game worksheet group (M = 52.24, SD = 14.71), and the control group (M = 50.70, SD = 13.36), $F(2,121) = 0.13$, $p = 0.879$.

There was no significant difference among the groups on post-game rating of difficulty, $F(2,121) = 2.18$, $p = 0.117$. There was a marginally significant difference among groups on liking the game, $F(2,121) = 2.66$, $p = 0.074$. A Tukey's HSD post hoc test revealed that the in-game worksheet group (M = 4.39, SD = 1.60) liked the game marginally more than the pre-game worksheet group (M = 3.49, SD = 1.99). Neither group differed significantly from the control group (M = 4.08, SD = 1.85). There was a significant difference among groups on thinking the game was fun $F(2,121) = 3.12$, $p = 0.048$. A Tukey's HSD post hoc test revealed that the in-game worksheet group (M = 4.71, SD = 1.59) rated the game as significantly more fun than the pre-game worksheet group did (M = 3.81, SD = 1.79). Neither group differed significantly from the control group (M = 4.25, SD = 1.52). Thus, asking students to complete a worksheet before or during the game did not affect game play time or perceived difficulty. However, participants who completed an in-game worksheet liked the game marginally more than participants who completed a pre-game worksheet. Participants who completed an in-game worksheet also thought the game was significantly more fun than participants who completed a pre-game worksheet did. Overall, there is no evidence that adding worksheets diminished players' enjoyment of the game.

Does adding worksheets affect the quality of explanations? The participants' explanation of the wet-cell battery on the post-game worksheet was scored in the same manner as Experiment 1. Participant responses were scored by a scorer blind to experimental condition. A second independent rater scored a subset of the tests. Inter-rater agreement on scores was high for conceptual idea units ($r = 0.87$) and verbatim idea units ($r = 0.83$). Analyses are based on the first rater's scores.

The top two lines in Table 3 shows the means and standard deviations for each group on the conceptual and verbatim parts of the explanation, respectively. The groups performed significantly differently on the number of conceptual idea units generated, $F(2,121) = 3.24$, $p = 0.043$. A Tukey's HSD post hoc test revealed that the in-game worksheet group significantly outperformed the control group on writing conceptual idea units, $p = 0.035$. No other pairwise differences were significant. There was no significant difference between the groups on number of verbatim idea units generated. Overall, the in-game worksheet group recalled more of the conceptual information regarding wet-cell batteries than the control group, and no other group differences were significant.

Does adding worksheets affect performance on the comprehension test? The comprehension test was separated in to intentional questions and incidental questions and scored in the same manner as Experiment 1. The next two lines of Table 3 show the means and standard deviations for each group on the intentional items and the incidental items of the comprehension test, respectively. There were no significant differences among groups on intentional items $F(2,121) = 1.08$, $p = 0.343$, or incidental items, $F(2,121) = 0.69$, $p = 0.505$. Therefore, the worksheets did not affect comprehension performance for the targeted learning material or for the other material in the game.

Does adding worksheets affect transfer performance? Transfer questions were scored in the same manner as Experiment 1. Participant responses were scored by a scorer blind to experimental condition. A second independent rater scored a subset of the tests. Inter-rater agreement on scores was high, $r = 0.80$. Analyses are based on the first rater's scores. The bottom line in Table 3 shows the mean and standard deviation for each group on the transfer test. An ANOVA revealed a significant difference among groups on transfer performance, $F(2,121) = 4.42$ $p = 0.014$. A Tukey's HSD post hoc test revealed that the in-game worksheet group scored significant higher than the control group on the transfer test, $p = 0.010$. No other group differences were significant. Therefore, completing an in-game worksheet led to significantly better performance on a problem-solving transfer test compared to the control group.

Discussion

This experiment helps tease out the results found in Experiment 1. In Experiment 1, participants who received a pre-game and in-game worksheet and played *Cache 17* outperformed a group that played *Cache 17* alone on explanation, comprehension questions, and transfer tests. In Experiment 2, participants who completed only the in-game worksheet performed better than the control group on an explanation and a transfer test. Therefore, the in-game worksheet hypothesis was supported for those learning outcomes. Participants who completed only the pre-game worksheet did not differ significantly from either group on any of the learning outcomes. Therefore, the pre-game worksheet hypothesis was not supported.

The performance of the pre-game worksheet group suggests that attempting to write an explanation of how a wet-cell battery works and setting the goal of being able to write a better explanation after playing the game does not significantly improve learning when done in the absence of an in-game intervention. It is interesting to note that even though this group was explicitly informed of, and given practice on, the explanation test, they still did not outperform the control group. This result is consistent with the idea of *just-in-time information presentation* laid out by van Merriënboer, Kester, and Kirschner (2003), although it involves prompting to attend to conceptual information rather than procedural information. Information such as learning goals can overwhelm a learner's cognitive capacity when presented too early. Instead, giving learners just-in-time information, such as a worksheet they complete during the game, can help them direct their limited cognitive resources to the goal without causing cognitive overload. The post-game survey also helps explain this effect. Participants in the pre-game worksheet group found the game significantly less fun and liked the game marginally less than participants in the in-game worksheet group. It is possible that having a goal in mind (i.e., learning how wet-cell batteries work), but being prevented from working toward that goal by the fast-paced game mechanics, led to a less enjoyable in-game experience.

The results of this experiment have theoretical significance. Research on metacognition and goal-setting emphasizes the importance of knowing what you intend to learn (i.e., setting goals) when engaging with a learning environment. However, goal-setting may be limited in immersive environments such as narrative games, when there is nothing in the game explicitly reminding learners to be working toward their goal. Research on cognitive load, on the other hand, suggests that providing just-in-time prompts to learners can help encourage appropriate cognitive processing, as well-timed prompts can help direct learners' attention without requiring that they keep instructions in mind throughout a learning experience. The results of the current experiment support this latter view.

One limitation of this experiment is that there was no condition that included both the pre-game worksheet and the in-game worksheet. This decision was made largely for efficiency and power. Experiment 1 addressed the effect of adding both in-game and pre-game worksheets, and the primary question in Experiment 2 was to see how the worksheets affect learning independently. In order to maximize power to address this primary question, the second experiment was limited to three groups.

Another important limitation of these experiments is that only participants who completed all three tasks in Cache 17 were included in the analysis. This was necessary because the comprehension test includes questions from all three tasks, so participants who were not exposed to all of the learning material in the game had to be excluded. In Experiment 1, 11 out of 62 participants (18%) did not finish the required tasks. In Experiment 2, 37 out of 161 participants (23%) did not finish the required tasks. Chi-square analyses revealed that participants who did not finish the game were significantly more likely to be women than participants who did finish the game in both experiments, and they were less likely to have video game experience than participants who did finish the game in Experiment 2. There was no significant difference in prior knowledge of electricity. Low video game experience could slow a player down in Cache 17 as the mechanics of navigation (i.e., coordinating between the mouse, which rotates the player's perspective, and keyboard buttons, which move the player through space) can be difficult to learn for inexperienced players. Negative affect toward video games and low video game self-efficacy could also be contributing factors, although they were not measured in the current experiments. Therefore, students who are not able to perform well in an educational game may need additional or alternative instruction in order to be exposed to all of the learning material the game provides.

Future work is needed to identify which aspects of worksheets encourage students to attend to and reflect on the target educational information in narrative games. For example, the in-game worksheet in this study was designed to be as simple as possible in order to encourage completion, but further work is needed to investigate whether asking questions that are more conceptual could facilitate beneficial forward transfer or test expectancy effects (Sagerman & Mayer, 1987; Thiede, Wiley & Griffin, 2011). Future research is also needed to investigate the effect of adding educational worksheets to other computer games.

Chapter IV: Experiment 3a

Experiments 1 and 2 tested the effect of adding instructional support to narrative adventure videogames on transfer of conceptual learning. In order to expand on using instructional supports to foster transferable knowledge, Experiment 3a shifts to the issue of whether instructional supports can foster transfer of cognitive skills in spatial puzzle games. Can adding instruction to a spatial puzzle game help broaden the learner's cognitive skills (i.e., allow the transfer of cognitive skills to non-game contexts)? Experiment 3a employs a value-added approach, which involves comparing a base version of a game to a version with a feature added, to enhance learning of spatial skills with videogame experience. The base game in this experiment is *Tetris*, and the added feature in this experiment is a series of model-based *Tetris* lessons and worksheets.

The manipulation in the current experiment – lessons and worksheets focusing on problem-solving strategies in *Tetris* – was intended to facilitate the transfer of spatial skills trained in *Tetris* to non-*Tetris* tests. Specifically, this manipulation was intended to keep *Tetris* players in the declarative stage of skill acquisition longer in order to facilitate the transfer of cognitive skills used in-game to non-game contexts. The logic of this experiment will be explained in four major sections. First, I discuss the distinction between procedural and declarative knowledge, and review theories and evidence suggesting that a declarative knowledge base can facilitate the transfer of cognitive skills. Second, I relate those theories to the training of spatial skills. Third, I discuss the relevant research base on training cognitive skills with video games. Finally, I propose a set of spatial skills underlying *Tetris* playing that could be facilitated with training.

Declarative Knowledge as a Basis for Transfer in Skill Learning

Declarative and procedural knowledge. Declarative knowledge, also called explicit knowledge, involves conscious awareness, whereas procedural knowledge, also called implicit, nondeclarative, or skill knowledge, does not require conscious remembering. How can declarative, explicit information affect procedural, implicit knowledge? The foundations of the explicit/implicit dissociation come from studies of amnesic patients who cannot form any new long-term declarative memories, but who improve with practice (sometimes at rates similar to normal controls) on implicit learning tasks such reading backwards text (Cohen & Squire, 1980), completing the Tower of Hanoi puzzle task (Cohen, Eichenbaum, Deacedo, & Corkin, 1985), and acquiring artificial grammars (Knowlton, Ramus, & Squire, 1992). As a result, memory-based frameworks for cognition state that the acquisition systems for declarative and procedural knowledge are fully separate (Schacter & Tulving, 1994; Squire & Zola, 1996).

While the separability of these two systems is not in dispute, such a separability does not preclude important interactions between the two knowledge systems in normal cognition. Reber (1989) noted that "in the real world nearly all complex skills are acquired with a blend of the explicit and the implicit" (p. 224). For example, explicit instructions can enhance performance in artificial grammar learning (Reber, Kassin, Lewis, & Cantor, 1980) and dynamic control tasks (Stanley, Mathews, Buss, & Kotler-Cope, 1989). In the skill acquisition theories developed by Fitts and Posner (1967) and Anderson (1982), three stages of skill acquisition are proposed: a *cognitive* or *declarative* phase, in which the learner has to think consciously about skill performance, an *associative* or *knowledge compilation* stage, in which the parts of the skill become linked to one another but require conscious guidance and feedback, and an *autonomous* or *procedural* stage, in which the skill requires little conscious thought to perform. It is therefore foundational to dominant skill acquisition paradigms that skills are acquired through a process in which declarative knowledge is gained first and then, through practice, compiled into production rules (Fitts & Posner, 1967; Anderson, 1982).[1] Alternative theories of skill acquisition that posit a reverse process – skills are learned implicitly and declarative rules extracted after the skill is learned – also include an important role for declarative knowledge feeding back into skill performance (Sun, Merrill, & Peterson, 2001). Sun, Slusarz, and Terry (2005) suggest a synergistic relationship between explicit and implicit processes of skill learning, such that explicit knowledge can facilitate faster response times, improved performance, and transfer of learned skills.

Transfer of skill learning. Inflexibility is one of the defining aspects of procedural knowledge (Schacter & Tulving, 1994; Squire & Zola, 1996). In general, learned skills are assumed to be highly specific and bound to the context of training. Training designed to foster expert skilled performance, such as deliberate practice, leads to outcomes that are specific and automatic (Anderson, 1982; Ericsson & Kintsch, 1995). However, under certain training conditions, transfer can be demonstrated with both simple and complex skills (Singley & Anderson, 1989). Therefore, training must be adapted when the goal is to foster outcomes that are more flexible. Although researchers have been interested in how to train transferable skills for over a century, no clear set of principles for training transferable skills exists.

[1] The requisite declarative knowledge can exist in working memory, and does not necessarily need to be stored in long-term memory, explaining the ability of amnesic patients to acquire new skills (Anderson & Fincham, 1994).

An important foundational concept in training for transfer is that training performance and transfer performance behave differently. Schmidt and Bjork (1992) suggest that one common pitfall of skill training is a focus on optimizing performance during training rather than optimizing performance during transfer. They argue that methods that optimize training performance can actually harm a trainee's ability to transfer the trained skill to new situations, and that training for trainsfer may slow the acquisition of a skill but lead to long-term benefits in performance. Doane, Sohn, and Schreiber (1999) relate this concept to Duncker's (1945) idea of *functional fixedness*: prior experience with knowledge can limit an individual's ability to apply that knowledge to a new situation. In order to facilitate transfer, training must give the learner tools that allow them to use their knowledge in new situations.

Declarative knowledge and transfer of skill learning. One proposal for the facilitation of transfer in skill learning is to increase the amount of task-relevant declarative processing the learner performs during learning (Sun, Merrill, & Peterson, 2001; Sun, Slusarz, and Terry, 2005; van Merrienboer, Jelsma, & Paas, 1992) in order to keep the learner in the declarative stage of skill acquisition longer (Hesketh, 1997). While inflexibility is characteristic of procedural knowledge, declarative knowledge is more flexible, and not bound to the learning context in the same way as procedural knowledge. The goal of training for expertise is typically to move through the declarative stage quickly in order to move on to automatization and training improvements (Hesketh, 1997). However, this approach to training likely creates domain-specific learning that undermines transfer outcomes. An alternative approach is to increase the amount of time a learner spends developing declarative knowledge about the task. This approach to training sets the learner off in the right direction before implicit learning takes over (Sun, Merril, & Peterson, 2001) by taking advantage of the flexibility in interpreting rules afforded by early stages of skill acquisition (Ahlum-Heath & Di Vesta, 1986). This gives learners an opportunity to extract rules and schemas that can be used as a basis for later transfer, allowing them to develop *reflective expertise* (van Merrienboer, Jelsma, & Paas, 1992). Declarative processing also allows the learner to develop metacognitive skills for self-assessment and dealing with errors (Hesketh, 1997). In summary of this position, Hesketh (1997) argued that "maximising the chance of developing transferable expertise… requires a lengthening of the time during skill acquisition when analytic processing is involved" (p. 321).

Evidence suggesting that spending more time in the declarative stage of skill acquisition can facilitate transfer began with a seminal study by Charles Judd. Judd (1908) found that subjects who were given a lesson on light refraction improved more quickly on a task throwing darts at underwater targets than those who did not get the lesson. Modern experiments find similar results. For example, in a reaction time task, participants who developed explicit knowledge about the sequence of stimuli demonstrated better transfer than those who did not develop explicit knowledge (Willingham, Nissen, & Bullemer, 1989). A study using the Tower of Hanoi task and Katona card problem found that participants do not spontaneously focus on the process of finding a solution, but when they are forced to do so with explicit prompting to describe their problem-solving process, transfer effects are positive (Berardi-Coletta, Buyer, Dominowski, & Rellinger, 1995). Similarly, participants who verbalized while completing the 3-disk Tower of Hanoi task showed better transfer to the 6-disk Tower of Hanoi task, although the effect was eliminated if participants were first given practice without verbalization (Ahlum-Heath & Di Vesta, 1986). Participants completing a minefield navigation task in a dual task condition that suppressed verbalization showed worse transfer performance than participants in a single task condition (Sun, Merril, & Peterson, 2001). Concurrent and post-task verbalization on concrete version of Wason selection task improved transfer to abstract version of task compared to no verbalization (Berry, 1983). Novice players demonstrated better transfer of a chess endgame when they gave self-explanations of errors during a learning phase (de Bruin, Rikers, Schmidt, 2007).

These findings run contrary to theories that call for a strong separation between declarative interventions and skill performance. Instead, these findings suggest that the transfer of a cognitive skill can be facilitated by building declarative knowledge about task-related processes. It is important to be clear that declarative knowledge should not be expected to enhance performance in every situation. In some situations, including inappropriate timing, explicit instructions intended to help performance in an implicit learning task actually hinder performance (see Sun, Slusarz, and Terry, 2005 for review), although it is worth noting that hindered performance on a trained task can be a positive indicator for transfer (Schmidt & Bjork, 1992). Also, some procedural skills are apparently impervious to declarative information (Reed, McLeod, & Dienes, 2010).

Training spatial skills

Do principles for training cognitive skills also apply to training spatial skills? The range of proficiencies that fall under the umbrella category of cognitive skills ranges from low-level perceptual discriminations to complex math abilities to analogical problem-solving to spatial transformations to artificial grammars. Theories of cognitive skills tend to discuss a range of tasks as evidence and assume common underlying principles for acquisition and transfer. It is possible that a common psychological mechanism underlies the acquisition and training of diverse cognitive skills. Rosenbaum, Carlson, and Gilmore (2001) demonstrated that even distinctions between cognitive skills and perceptual-motor skills break down quickly. Regardless, the extent to which a principle that applies to one class of cognitive skills will also apply to another is unclear, and class boundaries are similarly undefined. Thus, it is important to investigate the extent to which phenomena that apply to cognitive skills also apply to spatial skills.

The idea that spatial skills can be trained in a transferable way is only recently established. While most work had suggested a high degree of specificity and limited room for transfer in spatial skill learning (National Research Council, 2006; Sims & Mayer, 2002), a recent meta-analysis suggests that spatial skills may be exceptionally malleable. Uttal and colleagues (2013) reviewed the effect of spatial training on transfer and found an overall effect size of $g = 0.48$. That effect size is remarkably large considering how elusive transfer effects typically are. While their review did not analyze what experimental factors moderated the transfer effect, the authors speculated that the effect size was large because studies that intend to measure transfer tend to use more heavy-handed manipulations (such as prolonged training regimens) in order to maximize the chances of an effect. Thus, while evidence suggests that spatial skills can be trained in a transferable way, specific guidelines for doing so have not been established.

It is also important to note that the cognitive architecture underlying spatial skills is a matter of debate. Spatial skills are multifaceted, and the processes underlying spatial skills include the speed and power of spatial processing, strategy use, the quality with which one can create mental images, spatial working memory span, and executive processes (Hegarty & Waller, 2005). When spatial skills improve with training, the question of what specific component process or processes improved is nontrivial, and many standard tests of spatial skills make fine-grained analysis impossible. For example, Wright, Thompson, Ganis, Newcombe, and Kosslyn (2008) found an effect of transfer on a mental rotation task and a mental paper-folding task after participants were trained with one task or the other. Errors and reaction time data on these tasks can be broken down into slope, which is thought to indicate spatial transformations, and intercept, which could include a number of other relevant processes including stimulus encoding, strategy selection, and the initiation of spatial transformation, as well as less relevant processes such as initiating a response. Wright et al. (2008) found no effect for slope, but did find a transfer effect for the harder-to-interpret intercept. While the decomposition of these processes is beyond the scope of the current study, these limitations of interpretation are important to keep in mind.

Video Game Training

An ongoing question in the domain of cognitive skills training is whether playing video games trains cognitive skills that can be applied outside of the game context. Experiments in this field are concerned with the *cognitive consequences* of playing off-the-shelf videogames (Mayer, 2011). The overall picture of videogames as a vehicle for training cognitive skills remains murky (Mayer, 2014a). For example, training with the videogame *Tetris* has been shown to lead to very specific benefits—Tetris players are significantly better than non-players at mentally rotating *Tetris* shapes and *Tetris*-like shapes, but do not improve on any other measure of spatial ability (Sims & Mayer, 2002).

Diverse attempts to facilitate transfer of cognitive skills trained in-game to non-game contexts have not generally been made. However, some success has been demonstrated by taking a *contextual interference* approach, in which skills are practiced in multiple contexts in an interleaved manner (Battig, 1972; Shea & Morgan, 1979). For example, research has shown that combining spatial skills training with *Tetris* training improves spatial skill performance better than combining spatial skill training with a non-spatial game (Terlecki, Newcombe, & Little, 2008). This suggests that transferable spatial skills can be developed when practice is *decontextualized*, (i.e., not bound to the game context) by using the skill in more than one way. The strongest and most well established effect of using videogames to train cognitive skills is the effect of playing first-person shooting games on perceptual attention skills (see Mayer, 2014a for review). These results may be explained by the contextually dynamic nature of first-person shooters: first-person shooters require the player to practice their visual attention skills in an ever-changing context (Green & Bavelier, 2003).

The current study attempts to facilitate the transfer of skills used in *Tetris* to non-game contexts by giving participants lessons that require a declarative approach to *Tetris* problem-solving. The following section examines the skills underlying *Tetris* play.

Cognitive Skills and *Tetris*

Tetris was selected for the current experiments because it is the most studied computer game (Mayer, 2014). While no formal cognitive task analysis of *Tetris* playing has been completed, connections between *Tetris* operations and cognitive operations can be proposed. Some cognitive skills that may be utilized during *Tetris* play are mental rotation, spatial visualization, perceptual speed, useful field of view, and visuospatial working memory.

Mental rotation. Mental rotation may be used during *Tetris* play when a player needs to know how a piece will be configured when turned to a different orientation. This mental rotation can then be translated to action within the game. Additionally, a player can offload the cognitive effort required by mental rotation by taking action within the game (by pressing a key on the keyboard) to rotate the shape on the screen. Kirsh and Maglio (1994) refer to onscreen rotations intended to reduce the need for mental operations as *epistemic action*. These moves can appear superfluous, such as when a player uses rotations beyond what is necessary to get a piece into position, but they serve a purpose by obviating mental rotation. In contrast to epistemic actions are *pragmatic actions*, which are onscreen rotations meant to get the player closer to their in-game goals. *Tetris* players can use a combination of mental rotation, epistemic onscreen rotations, and pragmatic onscreen rotations strategically during play. Players with greater skill in *Tetris* are more likely than lower-skilled players to use epistemic actions (Maglio & Kirsh, 1996). Previous research has yielded mixed results regarding the relationship between mental rotation and *Tetris* play, depending on the object of the mental rotation task (see Mayer, 2014a for review). *Tetris* training does seem to lead to improvement on 2-D mental rotation of *Tetris* shapes and *Tetris*-like shapes (Boot et al., 2008; Okagaki & Frensch, 1994; Sims & Mayer, 2002), but has mixed effects on 2-D card rotation (De Lisi & Wolford, 2002; Okagaki & Frensch, 1994; Sims & Mayer, 2002), and no apparent effect on 3-D mental rotation (Terlecki, Newcombe, & Little, 2008).

Visualization. Spatial visualization may be used during Tetris play when a player needs to imagine, for example, how a piece will fit in with the existing board, what pieces are necessary to clear lines on a board, or what the board could look like several moves ahead. It is conceivably possible for a player to circumvent some mental visualization with in-game actions, such as by letting a piece fall close to the board pieces before deciding where it will go, or adjusting the location of a piece after it falls onto the board but before it locks into position. It is also possible for a *Tetris* player to avoid mental visualization by not planning ahead. There is not strong evidence for a relationship between visualization abilities and *Tetris* playing task (see Mayer, 2014a for review). Sims and Mayer (2002) did not find a significant difference in a form board test between high-skilled a low-skilled *Tetris* players, although the effect size may suggest a trend, $d = 0.40$. Experiments that involve Tetris training have yielded null-to-moderate effects on form board tests with non-*Tetris* and *Tetris*-like shapes (Okagaki & Frensch, 1994; Sims & Mayer, 2002) and moderate-to-large effects on visualization tests involving Tetris-like situations (Okagaki & Frensch, 1994).

Perceptual speed. Perceptual speed may be related to *Tetris* playing. Every 10 lines a player clears in the game, the level advances and the pieces fall at a faster rate. This requires the player to make increasingly fast decisions. The perceptual requirements of the game include identifying the shape of each new piece and the shape of the ever-changing board configuration. Advanced Tetris players may use game mechanics to assist with some of the perceptual requirements of *Tetris*, such as rotating pieces as soon as they appear in order to help identify their shape (Kirsh & Maglio, 1994; Maglio & Kirsh, 1996). There was no effect of *Tetris* training on perceptual speed in the only published study of this effect found in the literature (Okagaki & Frensch, 1994; see Mayer, 2014a for review).

Useful field of view. Another skill that may relate to *Tetris* playing is a measure of visual attention called useful field of view (UFOV). UFOV refers to the space in an individual's visual field over which they can extract information without changing their eye fixation. There appears to be a consistent positive relationship in the literature between action video game experience and measures of visual attention such as UFOV (see Mayer, 2014a for a review). Researchers in this area propose that action video games require players to maintain a large attention field in order to accommodate shifting task demands (Anderson & Bavelier, 2011). *Tetris* requires players to attend to information at the top and bottom of the screen, including noticing when a new shape has appeared, noticing the configuration of the board, and noticing the next shape in the line-up. However, *Tetris* differs from action videogames in that the locations requiring attention are much more predictable in terms of both space and time. There was a negative effect of *Tetris* training on useful field of view in the only published study of this effect found in the literature, and no effect on other measures of perceptual attention (Boot et al., 2008; see Mayer, 2014a for review).

Visuospatial working memory. Finally, *Tetris* playing may be related visuospatial working memory capacity. *Tetris* players may store and manipulate visual and spatial information in a variety of situations, such as imagining how an object will rotate while maintaining a mental representation of the configuration of the board. Players may use in-game actions such as rotating pieces or looking back at the board configuration in order to offload some demands on working memory. The one study of the effect of *Tetris* training on working memory has revealed a null-to-negative relationship for training on tests of visual working memory and Corsi block-tapping (Boot et al., 2008; see Mayer, 2014a for review).

Current Study and Predictions

The goal of the present study is to investigate the effect of adding declarative practice with *Tetris* problem-solving to *Tetris* training on the transfer of spatial skills to non-game contexts. The training program designed for this purpose is similar to cognitive apprenticeship programs (Bloom & Broder, 1950; Collins, Brown, & Holum, 1991) or worked examples (Renkl, 2011, 2014). In both of these contexts, an expert gives novice learners guidance in how to perform a task with a particular focus on the expert's thought processes. Our training program focuses on helping participants reflect on strategies that will help them play *Tetris* successfully. The training required participants to practice *Tetris* with a focus on planning and visualization skills in simulated game situations. The goal of this experiment is to facilitate specific transfer of general skills, that is, to help learners apply the skills practiced while playing *Tetris* to non-game contexts that require the same skills. The main question addressed in Experiment 3a is: Can adding modeling that encourages participants to play *Tetris* in a more reflective way help participants transfer skills learned in *Tetris* to new situations?

Participants were tested on a set of spatial skills that may be related to *Tetris* performance, as described in the previous section. In the current study, training in reflecting on the processes underlying gameplay is proposed to promote specific transfer of general skills learned in playing *Tetris*. Therefore, participants who receive model-based training alongside practice playing *Tetris* are predicted to show greater gains in the spatial skills required by *Tetris* in non-game contexts than students who practice playing *Tetris* alone. This is the training hypothesis.

Method

Participants and design. The participants were 59 undergraduate students (12 male, 47 female) from the University of California, Santa Barbara recruited through the Paid Psychology Subject Pool and through signs posted in the Psychology building. Participants were required to be non-video game players, which was defined as playing less than one hour per week of any type of video game. Participants received $10/hour in compensation for their participation. Twenty-nine participants served in the enhanced Tetris condition, and 30 served in the Tetris only condition.

Materials.

Game. *Meta-T* is a highly configurable version of the classic arcade game *Tetris* (Lindstedt & Gray, 2013). Figure 3 shows a screenshot of the game. Shapes made up of four square blocks fall from the top of the screen at a steady rate into a game area 10 blocks wide and 20 blocks high. The player uses the arrow keys on a standard keyboard to rotate the shapes clockwise or counterclockwise (counterclockwise rotation requires holding the shift key while pressing the up arrow) and to move the shapes left and right to control where they fall. The player must place the shapes efficiently, as leaving spaces between blocks causes the screen to fill up with blocks. When a horizontal row is filled with blocks, that line disappears and any blocks above that row fall down as a group. Multiple rows can be cleared at once if more than one row is completed with the placement of a single shape. The game level increases for every 10 rows cleared. The shapes fall with increasing speed as level increases. The game ends when the stack of blocks hits the top of the screen. The *Meta-T* software collects highly granular data on game play and strategy, including the number of rotations and translations (i.e., movements right and left) a player uses for each block and a frame-by-frame record of the board configuration.

Lessons and worksheets. Four lessons on problem-solving in *Tetris* were developed, as well as corresponding worksheets. The lessons were designed similarly to a worked-out example. Participants first watched a video slideshow with audio narration of an expert describing the way she would complete a *Tetris* problem-solving task (see Figure 4). The expert described her thought process as she completed each move. For example, the expert demonstrated how to think ahead about what types of board configurations allow more flexibility in future movements. Each video slideshow took between four and five minutes to watch. See Appendix A for the full script of each lesson.

At the end of the lesson, participants were given an instruction sheet (see Figure 5) and a worksheet with an in-progress Tetris board and four Tetris shapes (see Figure 6). The problem was similar in complexity and required strategy as the example just viewed, although the given board and available shapes were not identical. The instruction sheet directed participants to use the four shapes in any order to fill in the board as efficiently as possible, instructed the participant to show their work, and gave a worked example of showing work. Participants were given five minutes to complete the worksheets. See Appendix B for an example of an efficient solution for each of the four worksheets.

Cognitive tests. There were pre-training and post-training versions of each of the cognitive tests. Three paper-based tests were administered: a card rotation test, a form board test, and a test of perceptual speed. Three computer-based tests were administered: a 2-D Tetris rotations task, a useful field of view task, a visuospatial working memory task.

Card rotation test. The card rotation test is a 2-D mental rotation test from Educational Testing Service (Ekstrom et al., 1976). A target figure is presented to the left of a line, and 8 figures are presented to the right of the line. The figures at the right of the line are either the same as the figure at the left, but rotated around in the picture plane, or they are different, in that they are mirror-reversed versions of the figure at the left. Participants were given 3 minutes to make 80 same/different judgments for different figures on the pre-training and post-training tests.

Form board test. The form board test was also from Educational Testing Service (Ekstrom et al., 1976), and requires participants to decide what combination of five smaller shapes can be combined to form a final shape. Any number of smaller shapes, from two to five, can be used to complete the final shape. Participants were given 8 minutes to complete 24 items on the pre-training and post-training tests.

Perceptual speed test. The perceptual speed test is the number comparison test from Educational Testing Service (Ekstrom et al., 1976). Participants see a two-column list with a space in between the columns. If the numbers on either side of the space are the same, participants do nothing. If the numbers are different, participants mark the space with an X. Participants were given 90 seconds to make 48 comparisons on the pre-training and post-training tests.

2-D Tetris rotations. The 2-D Tetris rotations task included four Tetris shapes (S, Z, L, and J pieces) and four Tetris-like shapes. Participants see a base shape and a shape that is either the same, just rotated around in the picture plane, or flipped and rotated. Participants use arrow keys to indicate whether the two shapes are the same or different. Participants completed 2 practice trials and 112 test trials. The pre-training test was identical to the post-training test, except for the order of trials, which was randomized. The test was administered through DirectRT.

Useful field of view (UFOV) test. The UFOV test requires participants to identify the radial location of a rapidly-flashed target followed by a visual mask. The target could occur at one of 8 radial locations, at three distances from the fixation point at the center of the screen. The target could therefore appear in one of 24 onscreen positions. The participant used the keyboard number pad to indicate the direction from the center that the target occurred. In the no-distractor block the target is presented alone for 16.7 ms. In the distractor block the target is presented in an array of distractors for 33.3 ms. The display times for the distractor and non-distractor block were determined following pilot testing in order to avoid ceiling or floor effects. Display times were constrained by the refresh rate of the computer monitors (16.7 ms frame duration; 60 Hz refresh rate).

In a typical UFOV task the visual angle of the targets is tightly controlled (usually 10, 20, and 30 degrees away from fixation) by providing participants with a chin rest close to the screen. In this experiment, participants were not given a chin rest. Participants were instructed to tuck their chair in, sit up, and not lean toward or away from the screen for this task. The monitors were placed in the same position on the desk for each participant. For a research assistant who is 5'6" tall and following task instructions, the three levels of visual angle were 9.5°, 14.3°, and 18.9°. Visual angle varied among participants based on their specific height, posture, and sitting position. The pre-training test was identical to the post-training test, except for the order of trials, which was randomized in blocks. The test was administered through DirectRT.

Corsi block-tapping test. For the test of visual working memory capacity, the Corsi block-tapping task was used. In this task a staggered array of nine colored squares is displayed. A sequence of squares lights up, and participants click on the squares to repeat the sequence. Participants complete two practice trials followed by twelve test trials: two trials per sequence length starting with three squares and continuing to eight squares. This test was administered through Psychology Experiment Building Language (PEBL; Mueller & Piper, 2014).

Questionnaire. A questionnaire asked participants to indicate demographic information: age, gender, year in school, and major. It also asked participants to indicate their video game experience with the question, "How many hours a week do you typically play video games?," followed by the response options, "More than 10 hours per week," "5 to 10 hours per week," "1 to 5 hours per week," "less than 1 hour per week," and "I do not play video games." A final item asked, "Have you ever played Tetris before? Y/N."

Procedure. The experiment was completed over the course of 6 sessions. In the first session, participants completed a consent form and questionnaire, and then completed the pre-training version of the six cognitive tests in the following order: card rotation test, number comparison test, form board test, Corsi block-tapping test, Tetris mental rotation test, and UFOV test. They then played a game of *Meta-T* with the instructions to try to get as high of a score as possible. Participants who lost their first game of *Meta-T* in less than five minutes were instructed to play a second game. For the participants who played two games, their highest score was used as their pre-training high score.

In the subsequent four sessions, participants in the enhanced Tetris group first watched the slideshow and then completed a worksheet. The slideshow took approximately four minutes to watch and participants were given 5 minutes to complete the worksheet. After completing the worksheet participants were given one minute to compare their completed worksheet to an efficiently solved worksheet. They then played *Meta-T* for the remainder of an hour. Participants in the Tetris only group played *Meta-T* for a full hour.

In the final session, participants completed the post-training version of the six cognitive tests. They then played a game of *Meta-T* with the instructions to try to get as high of a score as possible. Participants who lost their first game of *Meta-T* in less than five minutes were instructed to play a second game. For the participants who played two games, their highest score was used as their post-training high score.

Results

Data source. If a participant missed any session they were instructed not to return for subsequent sessions. Of the 59 participants who started the experiment, 49 completed all six sessions. As a result, 24 participants remained in the worksheet group and 25 participants remained in the Tetris-only group. There was not a significant difference between groups in number of eliminated participants, X^2(N = 59) = 0.003, p = .95. All subsequent analyses refer to this subset of participants.

Are the groups equivalent on basic characteristics? A preliminary step is to determine whether the groups are equivalent on basic demographic characteristics. The two groups were compared for differences on age, proportion of men and women, time spent playing videogames per week, and Tetris performance on day 1. An independent samples t-test revealed no difference between the groups on age $t(47) = 0.16$, p = 0.88. A chi-square test found no significant differences between the groups on proportion of men and women, X^2(N = 49) = 0.004, p = .95. A Kruskal-Wallis H test revealed no significant difference between groups on time spent playing video games per week, X^2(N = 48) = 0.026, p = .87. A chi-square test revealed a significant difference between groups in having previously played *Tetris*, X^2(N = 49) = 4.22, p = .04, with 2 participants in the enhanced Tetris condition who had never played *Tetris* before, compared to 8 participants in the Tetris only group. Thus, the groups did not differ on basic characteristics, except for the proportion of participants who had never played *Tetris*.

Does training affect gains in cognitive skills? Descriptive statistics for pre-training and post-training performance on each of the cognitive skills tests are reported in Table 4. The two groups were compared for differences on the cognitive skills tests. Individual ANCOVAs for each skill used pre-training performance as a covariate and pre-training to post-training gains as the outcome variable. If the training hypothesis is supported, participants in the enhanced Tetris group would demonstrate greater gains on the measures of spatial skills.

Card rotation test. Scores on the card rotation test were determined by subtracting the number of incorrect responses form the number of correct responses. Incorrect responses were responses that were marked incorrectly, not blank responses. There was no significant difference in pre-training to post-training gains on the card rotation test between the enhanced Tetris group ($M = 6.45$, $SD = 14.53$) and the Tetris only group ($M = 9.84$, $SD = 13.72$) when controlling for pre-training performance (effect of covariate: $F(1,46) = 35.74$, $p < 0.001$; effect of condition: $F(1,46) = 0.33$, $p = 0.57$). These results indicate that enhanced Tetris training did not improve card rotation performance compared to the Tetris only condition.

Form board test. Scores on the form board test were determined by the number of complete correct problems (i.e., all five response items in a problem had to be marked correctly; 24 possible). There was no significant difference in pre-training to post-training gains on the form board test between the enhanced Tetris group ($M = 1.54$, $SD = 6.76$) and the Tetris only group ($M = 0.16$, $SD = 4.78$) when controlling for pre-training performance (effect of covariate: $F(1,46) = 16.83$, $p < 0.001$; effect of condition: $F(1,46) = 0.08$, $p =$

0.77). These results indicate that that enhanced Tetris training did not improve form board performance compared to the Tetris only condition.

Perceptual speed test. Scores on the perceptual speed test were determined by the number of correct responses (i.e., lines checked or left blank that should have been checked or left blank, respectively) minus the number of incorrect responses (i.e., lines checked or left blank that should have been left blank or checked, respectively). There was no significant difference in pre-training to post-training gains on perceptual speed test between the enhanced Tetris group ($M = 4.50$, $SD = 5.08$) and the Tetris only group ($M = 4.56$, $SD = 7.25$) when controlling for pre-training performance (effect of covariate: $F(1,46) = 9.17$, $p = 0.004$; effect of condition: $F(1,46) = 0.45$, $p = 0.50$). These results indicate that enhanced Tetris training did not improve perceptual speed performance compared to the Tetris only condition.

2-D Tetris rotations. 2-D Tetris rotation performance was scored on both overall accuracy (i.e., number of correct responses divided by number of total responses) and overall reaction time (i.e., average reaction time across rotation angles in milliseconds). There was no significant difference in pre-training to post-training gains on 2-D Tetris rotation accuracy between the enhanced Tetris group ($M = 0.003$, $SD = 0.03$) and the Tetris only group ($M = -0.001$, $SD = 0.03$) when controlling for pre-training performance (effect of covariate: $F(1,46) = 4.39$, $p = 0.042$; effect of condition: $F(1,46) = 0.31$, $p = 0.58$). There was no significant difference in pre-training to post-training gains on 2-D Tetris rotation reaction time between the enhanced Tetris group ($M = -584.10$, $SD = 490.00$) and the Tetris only group ($M = -512.48$, $SD = 516.39$) when controlling for pre-training performance (effect of covariate: $F(1,46) = 61.43$, $p < 0.001$; effect of condition: $F(1,46) = 0.33$, $p =$

0.57). These results indicate that enhanced Tetris training did not improve overall accuracy or reaction time on the 2-D Tetris mental rotation test compared to the Tetris only condition.

Further analyses were conducted to determine if Tetris mental rotation strategies differed between groups. Performance on this task was analyzed for slope (i.e., average increase in reaction time divided by increase in angle of disparity) and intercept (i.e., value of the rotation function when degree of angular disparity is 0) for each participant. Lower slopes correspond to faster rates of mental rotation, and lower intercepts correspond to other cognitive processing such as faster encoding and comparison of the stimuli, and/or faster initiation of a response. There was no significant difference in pre-training to post-training gains on 2-D Tetris rotation slope (ms/degree) between the enhanced Tetris group (M = -3.48, SD = 7.10) and the Tetris only group (M = -2.80, SD = 4.23) when controlling for pre-training performance (effect of covariate: $F(1,46) = 94.96$, $p < 0.001$; effect of condition: $F(1,46) = 1.62$, $p = 0.21$). There was no difference in pre-training to post-training gains on 2-D Tetris rotation intercept (ms) between the enhanced Tetris group (M = -199.48, SD = 200.21) and the Tetris only group (M = -219.43, SD = 429.10) when controlling for pre-training performance (effect of covariate: $F(1,46) = 101.31$, $p < 0.001$; effect of condition: $F(1,46) = 1.96$, $p = 0.17$). These results indicate that enhanced Tetris training did not improve 2-D Tetris mental rotation strategy compared to the Tetris only condition.

Useful field of view (UFOV) test. Scores on the UFOV test were determined by the overall accuracy (correct responses divided by total responses) including both the distractor absent and distractor present trials. There was no significant difference in pre-training to post-training gains on the UFOV test between the enhanced Tetris group (M = 3.62, SD = 9.90) and the Tetris only group (M = 2.60, SD = 9.20) when controlling for pre-training performance (effect of covariate: $F(1,46) = 21.73$, $p < 0.001$; effect of condition: $F(1,46) = 0.11$, $p = 0.97$). These results indicate that enhanced Tetris training did not improve UFOV performance compared to the Tetris only condition.

Corsi block-tapping test. Scores on the Corsi block-tapping test were determined by the total number of correct responses in the correct serial order across trials. There was a significant difference in pre-training to post-training gains on the Corsi block-tapping test between the enhanced Tetris group ($M = 0.83$, $SD = 7.15$) and the Tetris only group ($M = 3.68$, $SD = 6.94$) when controlling for pre-training performance (effect of covariate: $F(1,46) = 23.45$, $p < 0.001$; effect of condition: $F(1,46) = 7.03$, $p = 0.011$), in which the Tetris only group showed greater gains on the Corsi block-tapping test than the enhanced Tetris group. In this case, enhanced Tetris training did not improve Corsi block-tapping performance compared to the Tetris only condition.

Does training affect Tetris performance? The two groups were compared for their pre-training to post-training gains on several indicators of *Meta-T* performance. Individual ANCOVAs for each indicator of Tetris performance used pre-training performance as a covariate and pre-training to post-training gains as the outcome variable. If the training improves *Tetris* performance, then participants in the enhanced Tetris group will achieve higher overall performance, as evidenced by their final score, and demonstrate higher efficiency in their game play, as evidenced by fewer unnecessary rotations and translations of *Tetris* pieces, which are recorded with the in-game measures in *Meta-T*. There was no significant difference in pre-training to post-training gains on Tetris high score between the enhanced Tetris group ($M = 12191.46$, $SD = 7705.76$) and the Tetris only group ($M = 16296.48$, $SD = 18136.17$) when controlling for pre-training performance (effect of covariate: $F(1,46) = 0.332$, $p = 0.57$; effect of condition: $F(1,46) = 1.06$, $p = 0.31$). There was no significant difference in pre-training to post-training gains on Tetris efficiency (i.e., average number of unnecessary rotations and translations per piece) between the enhanced Tetris group ($M = -0.33$, $SD = 0.57$) and the Tetris only group ($M = -0.51$, $SD = 1.16$) when controlling for pre-training performance (effect of covariate: $F(1,46) = 4.14$, $p = 0.048$; effect of condition: $F(1,46) = 0.25$, $p = 0.62$). Enhanced Tetris training did not improve Tetris performance in high score or efficiency compared to the Tetris only condition.

Discussion

In this experiment, participants completed four training sessions that consisted either of only Tetris (Tetris only group), or Tetris plus lessons and worksheets designed to help them reflect on the skills used in the game (enhanced Tetris group). Pre-training and post-training tests measured performance on relevant spatial and cognitive skills. Analyses revealed no benefit of training on any of the spatial or cognitive skills tested. These results do not support the training hypothesis, as giving participants enhanced training that focused on reflecting on the skills used in Tetris did not increase their gains in spatial and cognitive skills compared to a group that only played Tetris. This experiment is unique in the way it takes a value-added approach to investigating the effect of videogames on cognitive skills, rather than the usual cognitive consequences approach. This approach allows researchers to take a controlled, theory-driven approach to asking questions about the effect of video games on cognitive skills. This experiment is also unique in adding cognitive modeling, an approach in which an expert explains their thought processes to a novice, to videogame training. This method has been successful in training cognitive skills in other contexts such a problem solving.   This experiment adds to the limited research base on the effect of playing video games on spatial and cognitive skills.

The results of this experiment also have theoretical implications. The features that characterize games contribute to an immersive, fluent experience. However, fluency in training procedural knowledge does not lead to the transfer of that knowledge. The goal of the intervention in this experiment was to increase participants' declarative knowledge base about the skills used in Tetris, thus providing a stronger basis for transfer. This experiment does not provide support for the idea that increasing one's declarative knowledge base about a spatial or cognitive skill will increase the likelihood of transfer of that skill.

Limitations and future directions. Several limitations could have contributed to the lack of a significant effect in this experiment. That is, it is possible that the lack of significant results in this study is due to the theoretical foundations (i.e., increasing one's declarative knowledge base about a skill may not necessarily increase the likelihood of transferring that skill), the manipulation (i.e., the lessons and worksheets failed to increase participants' declarative knowledge base), the dosage (i.e., the training was not long enough in duration to effect a significant change), the vehicle (i.e., Tetris may not utilize the targeted skills enough to produce an appreciable change in them), another limitation, or some combination. These possibilities are discussed below.

Further work is required to determine the validity of the theory that declarative knowledge about a spatial skill will increase the one's ability to use that skill in a novel situation. While this idea has been tested in the broader cognitive skills literature, it has not been similarly scrutinized in the domain of spatial skills. It remains possible that spatial skills are less affected by declarative knowledge than the other types of cognitive skills that have been tested in support of this framework. Future, more direct tests of this idea could mimic the methodology of experiments in the cognitive skills literature, such as prompting metacognitive or if-then verbalizations while completing a skill task (Berardi-Coletta et al., 1995).

It is possible that the manipulation in this experiment did not have the intended cognitive effect—that is, it is possible that completing the lessons and worksheets in addition to Tetris training did not increase participants' declarative knowledge base about the target skills. Many previous experiments in this literature attempt to manipulate participants' development of declarative knowledge more directly, such as by requiring the participant to describe their thought processes out loud (Ahlum-Heath & Di Vesta, 1986; Berardi-Coletta et al., 1995), or by suppressing declarative knowledge formation in a dual-task paradigm (Sun, Merrill, & Peterson, 2001). The manipulation in the current experiment may not have achieved the same effect as these more direct manipulations. The effect of the current manipulation could be tested by surveying participants about their declarative knowledge surrounding the skills tested. Alternatively, the manipulation could be redesigned following principles for training transferable spatial skills as they emerge.

Another possible explanation for the current results is that the dosage in this current experiment (i.e., amount of training) was insufficient to detect any existent effect. Wright et al. (2008) assert that to train transferable spatial skills, "training should be intensive enough to produce large gains, to maximize potential transfer effects" (p.764). In their meta-analysis of spatial training studies, Uttal and colleagues (2013) agree that "demonstrating transfer often requires intensive training" (p.365), including a large number of trials or training over a large amount of time. However, specific definitions of *intensive enough* do not seem to exist in the literature, especially as simply giving participants a spatial task a second time can lead to large gains in performance (Uttal et al., 2013). Further, the relationship between amount of training and positive transfer effects is not always clear-cut. Indeed, some published studies show significant spatial transfer effects following an hour or less of training (e.g., de Lisi & Cammarano, 1996; Wiedenbauer & Jansen-Osmann, 2008). It seems clear that both quantity and quality of training are important when transfer is the target outcome. This presents a logistic problem for researchers, as prolonged training studies are highly resource-intensive. Future work could use a greater amount of training or train subjects to asymptote in order to increase the possibility of transfer.

Finally, these results may demonstrate a more general failure of Tetris as a vehicle for training spatial skills. While the participants in this experiment were recruited on the bases of being non-gamers, pre-training Tetris ability was quite varied. It is possible that at low levels of game play, Tetris simply does not sufficiently tax any underlying spatial or cognitive skills to be a useful vehicle for training. As Tetris skill increases and participants reach higher, more difficult levels of the game, they may simultaneously gain strategies that allow them to obviate many mental operations including mental rotation and visualization (i.e., epistemic actions; Kirsh and Maglio, 1994). Therefore, Tetris may simply be a weak tool for training spatial or cognitive skills. Further, it is possible that if the manipulation in this study had involved a non-Tetris context, then transfer may have been more likely. For example, Terlecki, Newcome, and Little (2008) found a significant benefit of Tetris playing on spatial task performance paired Tetris training with repeated spatial tests. It is possible that because the manipulation in the current experiment was entirely Tetris- based, participants were not able to effectively decontextualize any skills used in Tetris.

Experiment 3b is intended to investigate whether there is any effect of the Tetris training (either enhanced Tetris or Tetris only) on gains on the target skills compared to an inactive control group.

Chapter V: Experiment 3b

In Experiment 3a, there was no benefit of the enhanced Tetris training compared to the Tetris only training on any of the cognitive tests. In Experiment 3b, an inactive control group was added that took both the pre-training tests and the post-training tests, but did not play *Tetris* or receive any training in between. This experiment investigates whether there was an effect of playing *Tetris* – in either the enhanced training condition or the Tetris only condition – on any of the skills tested.

Method

Participants and design. The participants were 25 undergraduate students (11 male, 14 female) from the University of California, Santa Barbara recruited through the Paid Psychology Subject Pool (which is the same as in Experiment 3a). Participants were required to be non-video game players, which was defined as playing less than one hour per week of any type of video game. Participants received $10/hour in compensation for their participation. All participants in Experiment 3b served in the inactive control condition.

Materials.

Game. The game used in this experiment was the same version of *Meta-T* used in Experiment 3a.

Cognitive tests. The pre-training and post-training tests used in this experiment were the same as those used in Experiment 3a: card rotation, form board, perceptual speed, 2-D Tetris rotations, useful field of view, and Corsi block-tapping.

Questionnaire. The same questionnaire was used as in experiment 3a.

Procedure. The experiment was completed over the course of 2 sessions. The first session was the same as the first session of Experiment 3a: participants completed a consent form, a questionnaire, and a series of cognitive tests.

Unlike Experiment 3a, there were no laboratory sessions in between the first and final session.

The second (and final) session took place five weeks after the first session, in order the match the interval between the first and final sessions in Experiment 3a. The second session was the same as the sixth session of Experiment 3a: participants took a series of cognitive tests and then played *Meta-T.*

Results

Data source. If a participant did not return for the second session their data were excluded from the analyses. Of the 25 participants who started the experiment, 17 completed both sessions. All subsequent analyses refer to this subset of participants as the inactive control group. This group is compared against the enhanced Tetris and Tetris only groups from Experiment 3a. Comparing these three groups required comparing across Experiments 3a and 3b. Although participants were recruited separately for these two experiments, the participants were all recruited from the same Paid Psychology Subject Pool with advertisements that used the same wording except for the number of sessions specified (6 sessions in Experiment 3a, 2 sessions in Experiment 3b). The procedure for the two sessions of Experiment 3b was identical to the first and sixth sessions of Experiment 3a, and participants from both experiments were instructed not to play video games outside of the laboratory for the duration of the experiment. These steps were taken in order to minimize the likelihood of population differences between the two experiments. The subsequent two sections test for differences among the groups on basic characteristics and pre-training task performance.

Are the groups equivalent on basic characteristics? A preliminary step is to determine whether the participants in Experiment 3b are equivalent to the participants in Experiment 3a on basic demographic characteristics. The groups were compared for differences on age, proportion of men and women, time spent playing videogames per week, and Tetris performance on day 1. An ANOVA revealed no significant difference between the groups on age $F(2,63) = 1.23$, $p = 0.30$. A chi-square test found no significant differences between the groups on proportion of men and women, $X^2(\mathrm{N} = 66) = 2.72$, $p = 0.26$. A Kruskal-Wallis H test revealed a significant difference between groups on time spent playing video games per week, $X^2(\mathrm{N} = 65) = 6.96$, $p = .03$, with the inactive control group having a higher mean rank ($M = 23.29$) than the enhanced Tetris ($M = 36.80$) or Tetris only groups ($M = 36.10$). No participants in the inactive control group indicated playing more than one hour of video games per week, which was the criterion for inclusion in the experiment. A chi-square test revealed no difference between groups in having previously played *Tetris*, $X^2(\mathrm{N} = 66) = 4.18$, $p = .14$. These results indicate that the groups were not different on basic characteristics, except for amount of video game play each week.

Are the groups equivalent on pre-training task performance? A further step taken to justify the comparing the participant sample from Experiment 3a to the sample from Experiment 3b is to test whether participants Experiment 3b are equivalent to the participants in Experiment 3a on pre-training performance. ANOVAs revealed no significant difference between the groups on pre-training performance for the card rotation test, $F(2,63) = 2.07$, $p = 0.14$, form board test, $F(2,63) = 2.15$, $p = 0.13$, perceptual speed test,

$F(2,63) = 1.05$, $p = 0.35$, 2-D Tetris mental rotation accuracy, $F(2,63) = 0.35$, $p = 0.70$, useful field of view test, $F(2,63) = 12.39$, $p = 0.10$, or Corsi block-tapping test, $F(2,63) = 2.18$, $p = 0.12$. There was a marginal difference between the groups on Tetris mental rotation reaction time, $F(2,63) = 2.48$, $p = 0.09$. These results indicate that the groups were not different on pre-training performance, except for a marginal difference in Tetris mental rotation reaction time. As with the analyses of Experiment 3a, all subsequent analyses of pre-training to post-training gains include pre-training performance as a covariate.

Does training affect gains in cognitive skills? Descriptive statistics for pre-training and post-training performance on each of the cognitive skills tests are reported in Table 4. The three groups were compared for differences on the cognitive skills tests. Individual ANCOVAs for each skill used pre-training performance as a covariate and pre-training to post-training gains as the outcome variable. All tests were scored in the same manner as Experiment 3a.

Card rotation test. There was a significant difference in pre-training to post-training gains on the card rotation test between the inactive control group ($M = 10.23$, $SD = 13.39$), the enhanced Tetris group ($M = 6.45$, $SD = 14.53$), and the Tetris only group ($M = 9.84$, $SD = 13.72$) when controlling for pre-training performance (effect of covariate: $F(1,62) = 57.21$, $p < 0.001$; effect of condition: $F(2,62) = 3.33$, $p = 0.042$). A Bonferonni-corrected posthoc analysis revealed that the inactive control group showed greater gains on the test than the enhanced Tetris group ($M_{diff} = 8.23$, SE = 3.27, $p = 0.043$). No other group comparisons were significant ($\alpha = 0.05$). These results indicate that there was no benefit of either type of Tetris training on card rotation gains, with the inactive control group significantly outperforming the enhanced Tetris group.

Form board test. There was no significant difference in pre-training to post-training gains on the form board test between the inactive control group (M = 0.12, SD = 3.64), the enhanced Tetris group (M = 1.54, SD = 6.76) and the Tetris only group (M = 0.16, SD = 4.78) when controlling for pre-training performance (effect of covariate: $F(1,62) = 16.38$, $p < 0.001$; effect of condition: $F(2,62) = 0.15$, $p = 0.86$). These results indicate that playing Tetris did not improve form board performance.

Perceptual speed test. There was a significant difference in pre-training to post-training gains on the perceptual speed test between the inactive control group (M = 9.06, SD = 6.41), the enhanced Tetris group (M = 4.50, SD = 5.08), and the Tetris only group (M = 4.56, SD = 7.25) when controlling for pre-training performance (effect of covariate: $F(1,62) = 10.46$, $p = 0.002$; effect of condition: $F(2,62) = 3.98$, $p = 0.024$). A Bonferonni-corrected posthoc analysis revealed that the inactive control group showed greater gains on the test than the enhanced Tetris group ($M_{\text{diff}} = 5.10$, $SE = 1.87$, $p = 0.025$), and marginally greater gains than the Tetris only group ($M_{\text{diff}} = 4.05$, $SE = 1.86$, $p = 0.099$). The difference between the two Tetris training groups was not significant ($\alpha = 0.05$). These results indicate that there was no benefit of either type of Tetris training on perceptual speed gains, with the inactive control group significantly outperforming the enhanced Tetris group.

2-D Tetris rotations. There was no significant difference in pre-training to post-training gains on 2-D Tetris rotation accuracy between the inactive control group (M = -0.01, SD = 0.04), enhanced Tetris group (M = 0.003, SD = 0.03), and the Tetris only group (M = -0.001, SD = 0.03) when controlling for pre-training performance (effect of covariate: $F(1,62) = 7.97$, $p = 0.006$; effect of condition: $F(2,62) = 0.43$, $p = 0.66$). There was no difference in pre-training to post-training gains on 2-D Tetris rotation reaction time between the inactive control group (*M* = 402.13, *SD* = 531.45), enhanced Tetris group (*M* = -584.10, *SD* = 490.00) and Tetris only group (*M* = -512.48, *SD* = 516.39) when controlling for pre-training performance (effect of covariate: $F(1,62) = 118.12$, $p < 0.001$; effect of condition: $F(2,62) = 1.03$, $p = 0.36$). These results indicate that playing Tetris did not improve 2-D Tetris mental rotation accuracy or reaction time.

Further analyses were conducted to determine if Tetris mental rotation strategies differed between groups. Participants' response patterns were tested against eight models of mental rotation from Sims and Mayer (2002). Model 1 is similar to the classic mental rotation results from Shepard and Metzler (1971) in which reaction time increases linearly for every increment of rotation from 0 to 180, and then decreases linearly for every increment from 225 to 315. Model 2 represents a bias for clockwise mental rotation, in which reaction time increases linearly for every increment of rotation from 0 to 225, and then decreases linearly at 270 and 315 degrees. Model 3 is similar to Model 2, but is adjusted so that counterclockwise rotations take an equal amount of time as the counterclockwise rotations (i.e., 315 degrees is the same as 45 degrees; 270 degrees is the same as 90 degrees). Model 4 represents no mental rotation, in which 0 degrees of rotation has the fastest reaction time, and all other increments of rotation are equal. Model 5 represents participants who have stored the Tetris shapes at orientations used in Tetris (i.e., 0, 90, 180, and 270 degrees) and mentally rotated the shapes from one of those orientations to the non-Tetris orientations (i.e., 45, 135, 225, and 315 degrees). Model 6 is a modification of Model 5 that adjusts for faster reaction times for mental rotations from 0 degrees (i.e., 45 and 315 degrees) than from mental rotations from the other stored orientations (i.e., 135 and 225 degrees). Model 7 represents mentally rotating clockwise for all increments of rotation (i.e., reaction time linearly increases from 0 to 315 degrees). Model 8 represents mentally rotation counterclockwise for all increments of rotation (i.e., reaction time is lowest at 0 degrees, highest at 45 degrees, and linearly decreases from 45 to 315 degrees). Only trials using L and J pieces were used for this analysis, as the S and Z pieces rotate into themselves at 180 degrees.

The mean reaction time at each increment of rotation was calculated for each participant. Each participant's pattern of reaction times was correlated with the pattern predicted by each model. The model that yielded the highest correlation was designated as the best fitting model. Table 5 shows the number of participants who best fit with each model for the pre-training and post-training tests for each group. A chi-square test revealed no significant differences among the groups on best fit mental rotation strategy for pre-training performance, X^2(N = 66) = 11.30, p = 0.66, or post-training performance, X^2(N = 66) = 13.87, p = 0.18. These results suggest that there was no significant difference among groups in the way participants performed mental rotation of Tetris objects before or after training.

Useful field of view (UFOV) test. There was no difference in pre-training to post-training gains on the UFOV test between the inactive control group (M = 0.00, SD = 10.81), the enhanced Tetris group (M = 3.62, SD = 9.90), and the Tetris only group (M = 2.60, SD = 9.20), when controlling for pre-training performance (effect of covariate: F(1,62) = 22.82, p < 0.001; effect of condition: F(2.62) = 0.002, p = 0.998). These results indicate playing Tetris did not improve UFOV performance.

Corsi block-tapping test. There was a significant difference in pre-training to post-training gains on the Corsi block-tapping test between the inactive control group (M = -0.71, *SD* = 6.59), the enhanced Tetris group (*M* = 0.83, *SD* = 7.15), and the Tetris only group (*M* = 3.68, *SD* = 6.94) when controlling for pre-training performance (effect of covariate: $F(1,62) = 30.89$, $p < 0.001$; effect of condition: $F(2,62) = 3.71$, $p = 0.03$). A Bonferonni-corrected posthoc analysis revealed that the Tetris only group showed greater gains on the test than the enhanced Tetris group ($M_{\text{diff}} = 5.10$, $SE = 1.87$, $p = 0.025$). No other group comparisons were significant ($\alpha = 0.05$). The Tetris only group showed greater gains on the Corsi block-tapping test than the enhanced Tetris group. Playing Tetris did not result in improvements on the Corsi block-tapping task compared to the inactive control group.

Does training affect Tetris performance? There was a significant difference in pre-training to post-training gains on Tetris high score between the inactive control group (*M* = 5940.35, *SD* = 14244.39), the enhanced Tetris group (*M* = 12191.46, *SD* = 7705.76), and the Tetris only group (*M* = 16296.48, *SD* = 18136.17) when controlling for pre-training performance (effect of covariate: $F(1,62) = 2.51$, $p = 0.12$; effect of condition: $F(2,62) = 3.44$, $p = 0.038$). A Bonferonni-corrected posthoc analysis revealed that the Tetris only group showed greater gains on the test than the inactive control group ($\mathrm{M}_{\text{diff}} = 11679.11$, SE = 4456.01, $p = 0.033$). No other group comparisons were significant ($\alpha = 0.05$). There was no difference in pre-training to post-training gains on Tetris efficiency (i.e., average number of unnecessary rotations and translations per piece) between the inactive control group (*M* = -0.15, *SD* = 0.60) enhanced Tetris group (*M* = -0.33, *SD* = 0.57), the Tetris only group (*M* = -0.51, *SD* = 1.16) when controlling for pre-training performance (effect of covariate: $F(1,62) = 10.08$, $p = 0.002$; effect of condition: $F(2,62) = 0.47$, $p = 0.63$). These results indicate that the Tetris only group improved their Tetris score significantly more than the inactive control group. There was no difference in gains between the group in Tetris efficiency.

Do the combined Tetris groups differ from the inactive control group? In order to test whether there was an overall effect of Tetris training compared to the inactive control group, additional analyses were conducted with the enhanced Tetris and Tetris only groups combined to form a combined Tetris group. The combined Tetris group (N = 49) and inactive control group (N = 17) were compared for differences on the cognitive skills tests and on Tetris performance. Individual ANCOVAs for each skill used pre-training performance as a covariate and pre-training to post-training gains as the outcome variable.

Card rotation test. There was a significant difference in pre-training to post-training gains on the card rotation test between the inactive control group ($M = 10.23$, $SD = 13.39$) and the combined Tetris group ($M = 8.18$, $SD = 14.08$), when controlling for pre-training performance (effect of covariate: $F(1,63) = 58.81$, $p < 0.001$; effect of condition: $F(1,63) = 6.47$, $p = 0.014$). These results indicate that there was no benefit of Tetris training on card rotation gains, with the inactive control group significantly outperforming the combined Tetris group.

Form board test. There was no significant difference in pre-training to post-training gains on the form board test between the inactive control group ($M = 0.12$, $SD = 3.64$), the combined Tetris group ($M = 0.84$, $SD = 5.82$) when controlling for pre-training performance (effect of covariate: $F(1,63) = 17.45$, $p < 0.001$; effect of condition: $F(1,63) = 0.13$, $p = 0.72$). These results indicate that playing Tetris did not improve form board performance.

Perceptual speed test. There was a significant difference in pre-training to post-training gains on the perceptual speed test between the inactive control group ($M = 9.06$, $SD = 6.41$) and the combined Tetris group ($M = 4.53$, $SD = 6.22$) when controlling for pre-training performance (effect of covariate: $F(1,63) = 10.19$, $p = 0.002$; effect of condition:

$F(1,63) = 7.66$, $p = 0.007$). These results indicate that there was no benefit of Tetris training on perceptual speed gains, with the inactive control group significantly outperforming the combined Tetris group.

2-D Tetris rotations. There was no significant difference in pre-training to post-training gains on 2-D Tetris rotation accuracy between the inactive control group ($M = -0.01$, $SD = 0.04$) and combined Tetris group ($M = 0.00$, $SD = 0.03$) when controlling for pre-training performance (effect of covariate: $F(1,63) = 7.95$, $p = 0.006$; effect of condition: $F(1,63) = 0.56$, $p = 0.46$). There was no difference in pre-training to post-training gains on 2-D Tetris rotation reaction time between the inactive control group ($M = -402.13$, $SD = 531.45$) and the enhanced Tetris group ($M = 547.56$, $SD = 499.68$) when controlling for pre-training performance (effect of covariate: $F(1,63) = 119.56$, $p < 0.001$; effect of condition: $F(1,63) = 1.68$, $p = 0.20$). These results indicate that playing Tetris did not improve 2-D Tetris mental rotation accuracy or reaction time.

Further analyses were conducted to determine if Tetris mental rotation strategies differed between groups. The same 8 models of Tetris mental rotation strategies were tested as in the analysis of all three groups. A chi-square test revealed no significant differences among the groups on best fit mental rotation strategy for pre-training performance, $X^2(\text{N} = 66) = 5.47$, $p = 0.60$, or post-training performance, $X^2(\text{N} = 66) = 5.80$, $p = 0.33$. These results suggest that there was no significant difference among groups in the way participants performed mental rotation of Tetris objects before or after training.

Useful field of view (UFOV) test. There was no difference in pre-training to post-training gains on the UFOV test between the inactive control group ($M = 3.10$, $SD = 9.46$) and the combined Tetris group ($M = 3.62$, $SD = 9.90$) when controlling for pre-training performance (effect of covariate: $F(1,63) = 23.36$, $p < 0.001$; effect of condition: $F(1,63) = 0.003$, $p = 0.96$). These results indicate playing Tetris did not improve UFOV performance.

Corsi block-tapping test. There was no difference in pre-training to post-training gains on the Corsi block-tapping test between the inactive control group ($M = -0.71$, $SD = 6.59$) and the combined Tetris group ($M = 1.92$, $SD = 7.20$) when controlling for pre-training performance (effect of covariate: $F(1,63) = 25.83$, $p < 0.001$; effect of condition: $F(1,63) = 0.09$, $p = 0.76$). Playing Tetris did not result in improvements on the Corsi block-tapping task compared to the inactive control group.

Tetris performance. There was a significant difference in pre-training to post-training gains on Tetris high score between the inactive control group ($M = 5940.35$, $SD = 14244.39$) and the combined Tetris group ($M = 14285.86$, $SD = 14043.20$) when controlling for pre-training performance (effect of covariate: $F(1,63) = 2.43$, $p = 0.12$; effect of condition: $F(1,63) = 5.73$, $p = 0.02$). There was no difference in pre-training to post-training gains on Tetris efficiency (i.e., average number of unnecessary rotations and translations per piece) between the inactive control group ($M = -0.15$, $SD = 0.60$) and the combined Tetris group ($M = -0.42$, $SD = 0.91$) when controlling for pre-training performance (effect of covariate: $F(1,63) = 10.68$, $p = 0.002$; effect of condition: $F(2,63) = 0.80$, $p = 0.37$). These results indicate that the combined Tetris group improved their Tetris score significantly more than the inactive control group. There was no difference in gains between the group in Tetris efficiency.

Overall, across multiple cognitive measures, there is no evidence that Tetris playing resulted in improvements in cognitive skills.

Discussion

Experiment 3b is a cognitive consequences study showing no benefit of playing Tetris on spatial skills. The inactive control group took the same pre-training and post-training measures with the same elapsed time in between as the groups in Experiment 3a. The participants in Experiment 3b did not complete any training in the weeks between these tests. This addition allows for comparisons to determine whether there was any effect of enhanced Tetris training or Tetris only training on gains in cognitive skills compared to gains from simply taking the tests a second time after a five-week delay. The results of this experiment revealed no benefit of the enhanced Tetris training or Tetris only training on gains in any of the spatial or cognitive skills measured when controlling for pre-training performance. The inactive control group actually outperformed the enhanced training group on two measures, indicating that training may have even suppressed gains on the card rotation and number comparison tasks. The only observed benefit of training was that participants in the Tetris only condition showed significantly greater gains in Tetris performance than participants in the inactive control condition.

This experiment reveals the cognitive consequences of enhanced Tetris training and Tetris only training. These results support a narrow view of transfer of procedural skill, as participants were not able to use any of the skills that may underlie Tetris performance outside of a game context. The current experiment therefore supports a *specific transfer* view of the scope of transfer, rather than a *specific transfer of general skills* view, as none of the skills that may be involved in Tetris play were improved outside of the game context. Participants who played the most Tetris (i.e., the Tetris only condition) improved more on their Tetris performance than the inactive control condition, revealing that the Tetris training was sufficient to improve performance on the trained task compared to simply retesting, but that improvement did not transfer to any other task. These results give additional evidence to the domain-specificity of Tetris expertise (Sims & Mayer, 2002).

Limitations. Similar to Experiment 3a, the results of this experiment may have been affected by insufficient dosage (i.e., participants in the training conditions did not complete enough training to effect a significant change) and the vehicle (i.e., Tetris may not sufficiently challenge any of the examined skills to see any improvement).

Chapter VI: General Discussion

The experiments described in this dissertation all investigate ways of achieving transfer from computer games. Despite widespread excitement over incorporating computer games into the classroom, relatively little research has demonstrated how to do so in a way that facilitates transfer. One explanation for why transfer is hard to come by in video game research is that in-game experiences tend to be fast and fluent, while transferable learning experiences tend to be slow and deliberate.

Experiments 1 and 2 used a video game to teach concepts related to electromechanical devices. While previous research has demonstrated that the narrative content of a game can distract from learning, the results of Experiment 1 show that adding worksheets that focus on the instructional objective of the game can facilitate learning and transfer. Participants who received pre-game and in-game worksheets that focused on the educational aspect of Cache 17 performed better than a control group on a written explanation, a comprehension test, and a transfer test. Experiment 2 disentangled the factors involved in Experiment 1 by investigating whether adding worksheets only before or only during game play can facilitate learning. Participants who received only the in-game worksheet performed better on a written explanation and a transfer test than a control group, although the effect was neither as large nor as nuanced (i.e., no effect for comprehension test; no effect for verbatim information on explanations) as the combination of pre-game and in-game worksheets in Experiment 1. Participants who received the pre-game worksheet only did not differ significantly from either group on any learning outcomes. In both experiments learning outcomes were improved without affecting enjoyment of the game.

Experiment 3a added cognitive modeling-based lessons and worksheets to Tetris training, and measured whether that training helps participants transfer skills used in the game to tests of spatial and cognitive skills. Judd (1908) advocated facilitating transfer by teaching skills in a generalizable way, and this experiment represents such an attempt. While Uttal et al. (2013) found a medium effect size on average for transfer of spatial skill training, the conditions that facilitate transfer are not well defined. This experiment used methods that successfully facilitate transfer in other domains of cognitive skill acquisition in an attempt at establishing a way to achieve transfer of spatial skills from playing a computer game. There was no observed benefit of the enhanced Tetris training on any of the spatial or cognitive skills measured compared to a control condition that played Tetris only. In Experiment 3b, there was no evidence for cognitive benefits resulting from either type of Tetris training compared to an inactive control group. Tetris appears to be a particularly poor choice for improving spatial skills, even though it is the most studied game in the literature on learning from games.

The results of these experiments add to the limited research base on achieving transfer from educational computer games. These experiments examine techniques intended to increase the generalizability of knowledge targeted in computer games by including opportunities to reflect and describe the knowledge being used. In Experiments 1 and 2, participants were given worksheets to help them reflect on the information about electric circuits that they used to navigate the game, with the goal of instilling transferable conceptual knowledge. In Experiment 3a, participants viewed interactive training lessons and completed worksheets to help them play *Tetris* in a reflective way, with the goal of instilling transferable procedural knowledge. While this experiment did not support the use of such an intervention, it is possible that this approach could be useful with redesigned lessons or a different game. Experiment 3b tested whether standard or enhanced Tetris training improved performance compared to an inactive control group, and also found no benefit of training. The results of these experiments help shape guidelines for instructors who are interested in how to use computer games in the classroom, especially when the goal is training transferable conceptual or procedural knowledge. The results of these experiments also indicate that there may be more potential in using games for science learning rather than for improving spatial skills.

Further research is needed to develop principles how to design adjunct materials to enhance transfer outcomes from games, such as those used in Experiments 1 and 2. Future research could also test whether delivering the adjunct materials in-game, such as prompting students with specific questions about the learning material, could similarly increase learning outcomes. Further research is also needed in order to determine how or if transferable spatial skills can be trained in game environments. The responsive, cumulative nature of games makes them attractive options for training skills, but the available evidence suggests they may be weak vehicles for transferable learning. There is currently limited evidence in or out of game environments for how transferable spatial skills can be trained. Understanding how transferable spatial skills can be facilitated can help guide principles for future training studies. It is also important to identify existing games or build new games that appropriately tax the targeted skills.

These experiments demonstrate the value of applying psychological science to the domain of educational games, which has been the subject of strong claims based on weak evidence (Mayer, 2014, National Research Council, 2011, O'Neil & Perez, 2008; Tobias &

Fletcher, 2011). The results of these experiment suggest that, like the Latin schools of the 19th century, video games alone do not lead to a general improvement of the mind. However, with theory-driven experiments and evidence-based design principles for training transferable knowledge with games, video games may have a place in the classroom of the future.

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Tables and Figures

Table 1

Experiment 1: Two Competing Goals of Narrative Games for Learning

Game feature	Goal	Mechanism
Narrative theme	Recover stolen artwork	Motivational processes
Instructional material	Build a wet-cell battery	Cognitive processes

Table 2

Experiment 1: Performance on Post-game Tests of Learning Outcome

Measure of learning outcome	Worksheet group		Control group		Effect size
(Total possible)	*M*	*SD*	*M*	*SD*	*d*
Explanation: Conceptual (15)	6.30*	1.82	4.64	1.81	0.92
Explanation: Verbatim (5)	0.22*	0.42	0.68	0.86	-0.68
Comprehension: Intentional (8)	7.61*	0.58	7.07	0.98	0.67
Comprehension: Incidental (9)	4.61	2.04	4.71	1.67	-0.07
Transfer	4.59*	1.50	3.43	1.64	0.74

Note. Asterisk (*) indicates significant difference from control group at $p < .05$. There were four transfer questions with no defined limit to the number of correct solutions possible.

Table 3

Experiment 2: Performance on Post-game Tests of Learning Outcome

Measure of learning outcome (total possible)	Pre-game WS group	In-game WS group	Control group	Pre-game vs. in-game	Pre-game vs. control	In-game vs. control
Explanation: Conceptual (15)	*M* = 4.84 *SD* = 2.16	*M* = 5.54 *SD* = 2.07	*M* = 4.43 *SD* = 1.69	*d* = -0.33	*d* = 0.21	*d* = 0.59*
Explanation: Verbatim (5)	*M* = 0.49 *SD* = 0.86	*M* = 0.51 *SD* = 0.78	*M* = 0.73 *SD* = 0.85	*d* = -0.03	*d* = -0.26	*d* = -0.28
Comprehension: Intentional (8)	*M* = 7.00 *SD* = 1.31	*M* = 7.37 *SD* = 0.83	*M* = 7.18 *SD* = 1.22	*d* = -0.33	*d* = -0.14	*d* = 0.18
Comprehension: Incidental (9)	*M* = 4.44 *SD* = 1.72	*M* = 4.46 *SD* = 1.40	*M* = 4.80 *SD* = 1.49	*d* = -0.01	*d* = -0.22	*d* = -0.23
Transfer	*M* = 3.79 *SD* = 1.71	*M* = 4.32 *SD* = 1.62	*M* = 3.23 *SD* = 1.62	*d* = -0.32	*d* = 0.34	*d* = 0.67*

Note. Asterisk (*) indicates significant difference at $p < .05$ (Tukey's HSD post hoc test). There were four transfer questions with no defined limit to the number of correct solutions possible.

Table 4

Experiments 3a and 3b: Performance on Pre-training and Post-training Measures

Test (total possible)	Enhanced Tetris Condition		Tetris Only Condition		Inactive Control Condition	
	Pre	Post	Pre	Post	Pre	Post
Card rotation (80)	*M* = 45.2 *SD* = 16.5	*M* = 51.7 *SD* = 10.0	*M* = 42.2 *SD* = 17.8	*M* = 52.1 *SD* = 15.6	*M* = 53.3 *SD* = 18.5	*M* = 63.5 *SD* = 11.5
Tetris Mental rotation (RT, ms)	*M* = 2366.4 *SD* = 660.1	*M* = 1782.3 *SD* = 423.7	*M* = 2336.1 *SD* = 708.0	*M* = 1823.6 *SD* = 470.9	*M* = 1900.2 *SD* = 815.3	*M* = 1498.1 *SD* = 364.8
Paper form board (24)	*M* = 7.9 *SD* = 3.3	*M* = 9.4 *SD* = 5.3	*M* = 9.1 *SD* = 4.2	*M* = 9.2 *SD* = 4.8	*M* = 10.3 *SD* = 3.4	*M* = 10.4 *SD* = 4.6
Number compar. (48)	*M* = 19.2 *SD* = 5.3	*M* = 23.7 *SD* = 5.6	*M* = 21.8 *SD* = 6.8	*M* = 26.3 *SD* = 7.7	*M* = 20.6 *SD* = 6.7	*M* = 29.6 *SD* = 7.7
Useful field of view (128)	*M* = 69.3 *SD* = 16.5	*M* = 72.9 *SD* = 12.8	*M* = 72.1 *SD* = 10.3	*M* = 74.7 *SD* = 9.9	*M* = 76.5 *SD* = 12.7	*M* = 78.5 *SD* = 12.9
Corsi block-tapping (66)	*M* = 47.7 *SD* = 6.5	*M* = 47.8 *SD* = 5.9	*M* = 49.0 *SD* = 6.9	*M* = 52.7 *SD* = 6.6	*M* = 51.9 *SD* = 5.2	*M* = 51.2 *SD* = 5.6
Tetris High Score (n/a)	*M* = 5836.3 *SD* = 6715.8	*M* = 18027.8 *SD* = 8900.4	*M* = 5456.6 *SD* = 7406.1	*M* = 21753.1 *SD* = 20997.5	*M* = 9187.5 *SD* = 9732.3	*M* = 15127.9 *SD* = 20487.9

Table 5

Experiments 3a and 3b: Number of participants whose Tetris mental rotation performance best fit each model for pre-training and post-training tests

	Pre-training							
	Model							
Condition	1	2	3	4	5	6	7	8
Enhanced Tetris	13	1	5	3	0	1	1	0
Tetris only	16	2	3	1	1	1	0	1
Inactive control	11	3	2	0	0	0	0	1
Total	40	6	10	4	1	2	1	2

	Post-training							
	Model							
Condition	1	2	3	4	5	6	7	8
Enhanced Tetris	8	4	8	3	0	1	0	0
Tetris only	17	2	3	2	0	0	0	1
Inactive control	10	1	1	2	0	1	0	2
Total	35	7	12	7	0	2	0	3

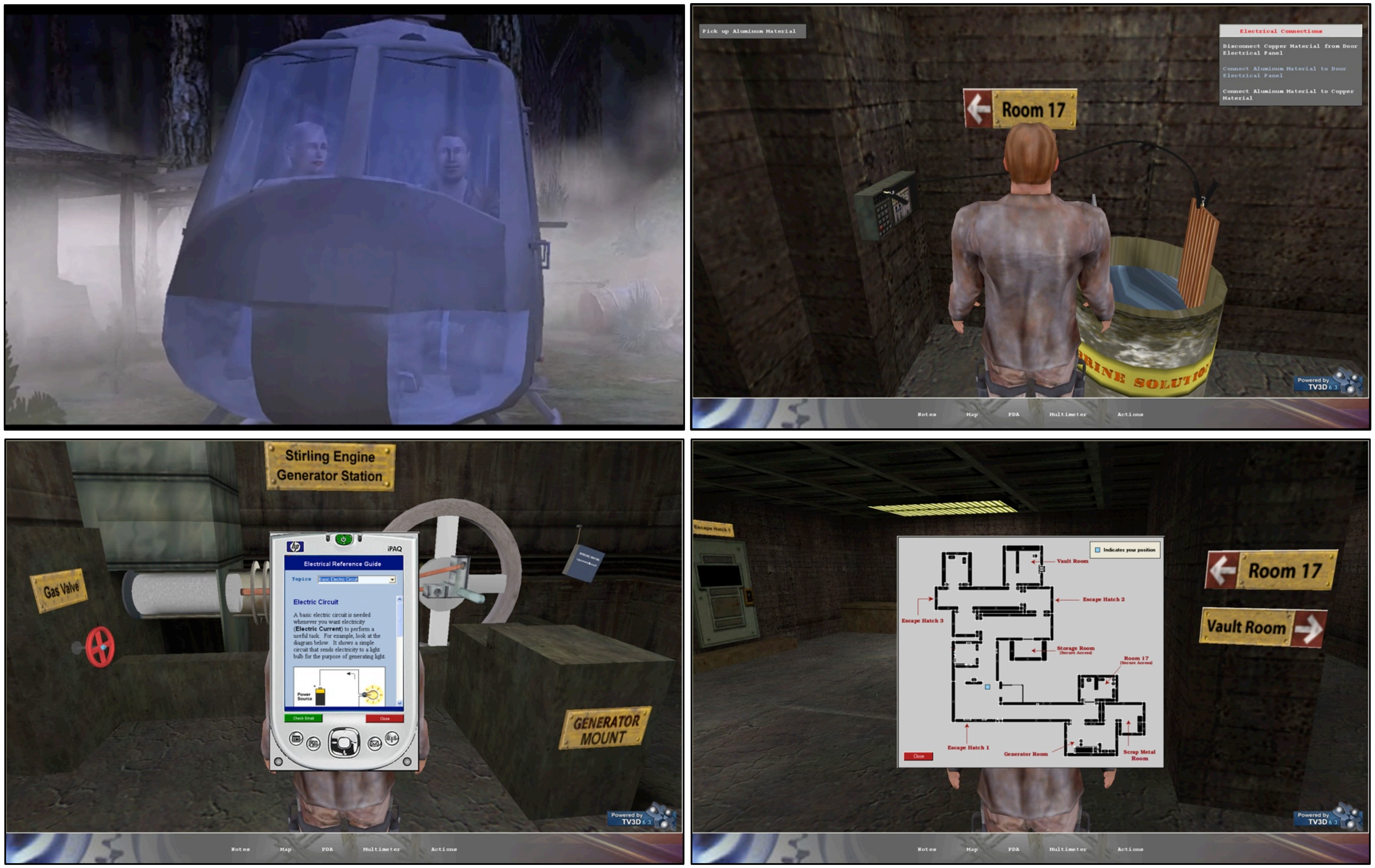


Figure 1. Screenshots from *Cache 17*. Clockwise from top left: Kate and Alex arrive at the bunker during the introductory cut scene; Alex in front of the barrel of brine for the wet-cell battery task; Viewing the PDA; Viewing the map of the bunker.

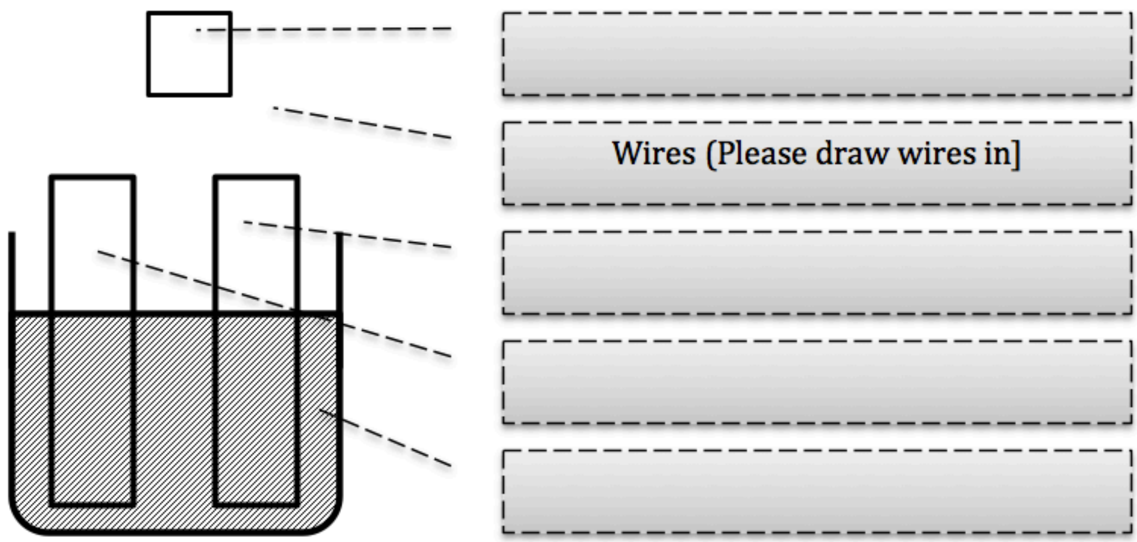


Figure 2. Fill-in-the-blank diagram on the pre- and post-game explanation worksheets.

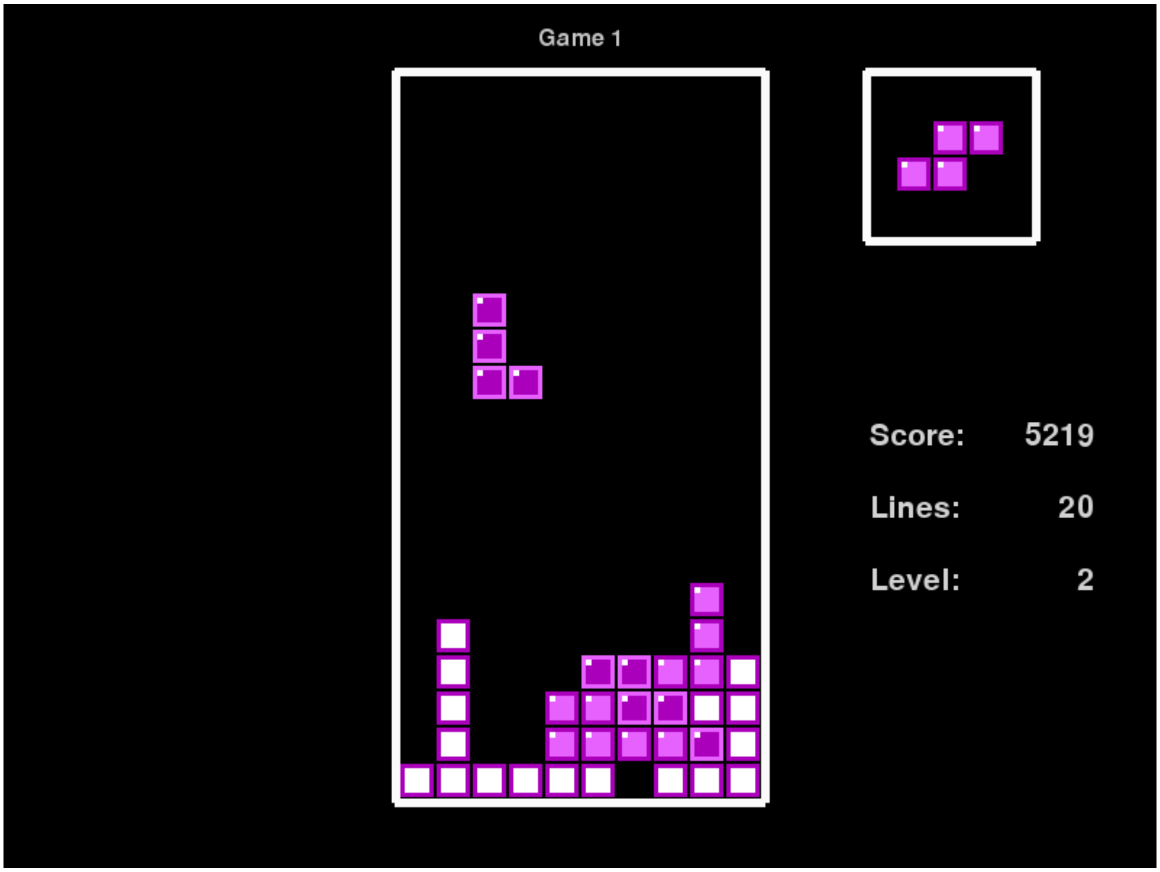


Figure 3. Screenshot from *Meta-T*.

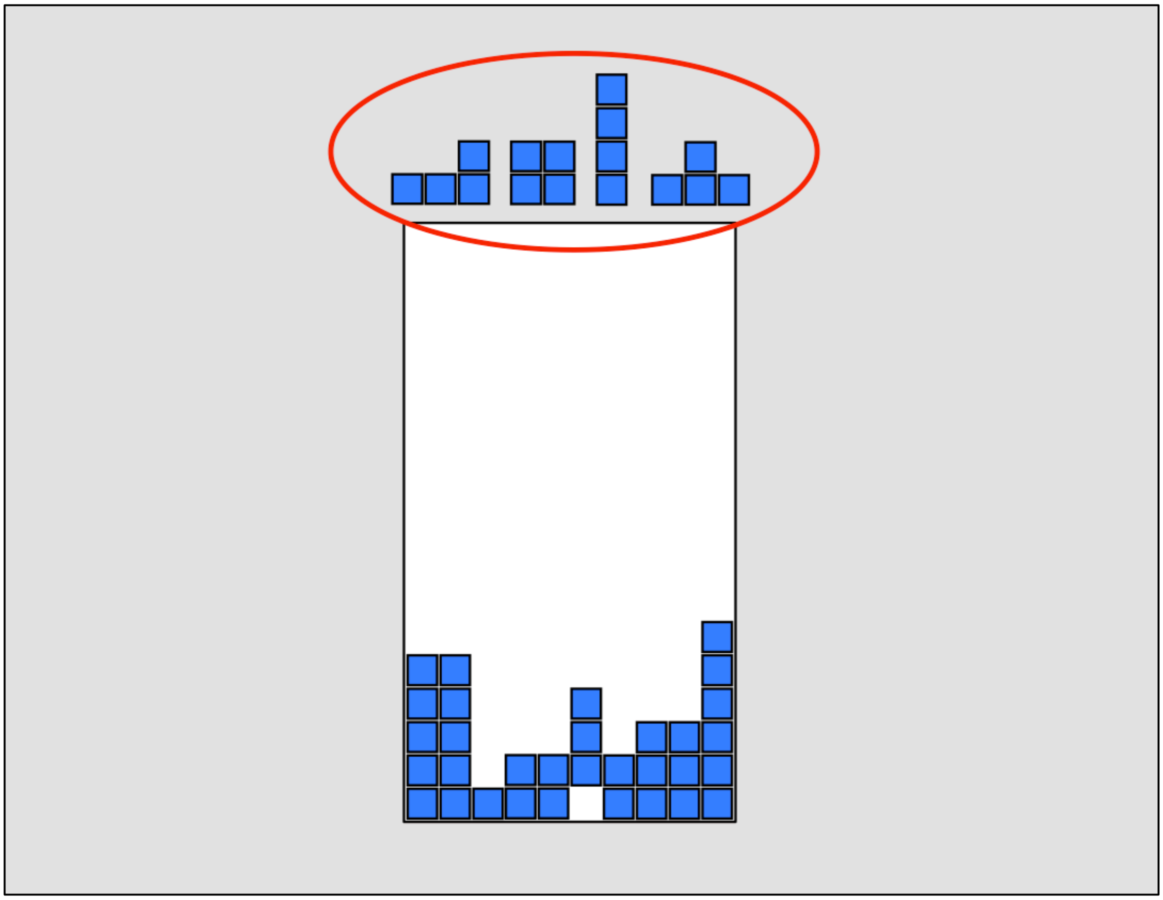
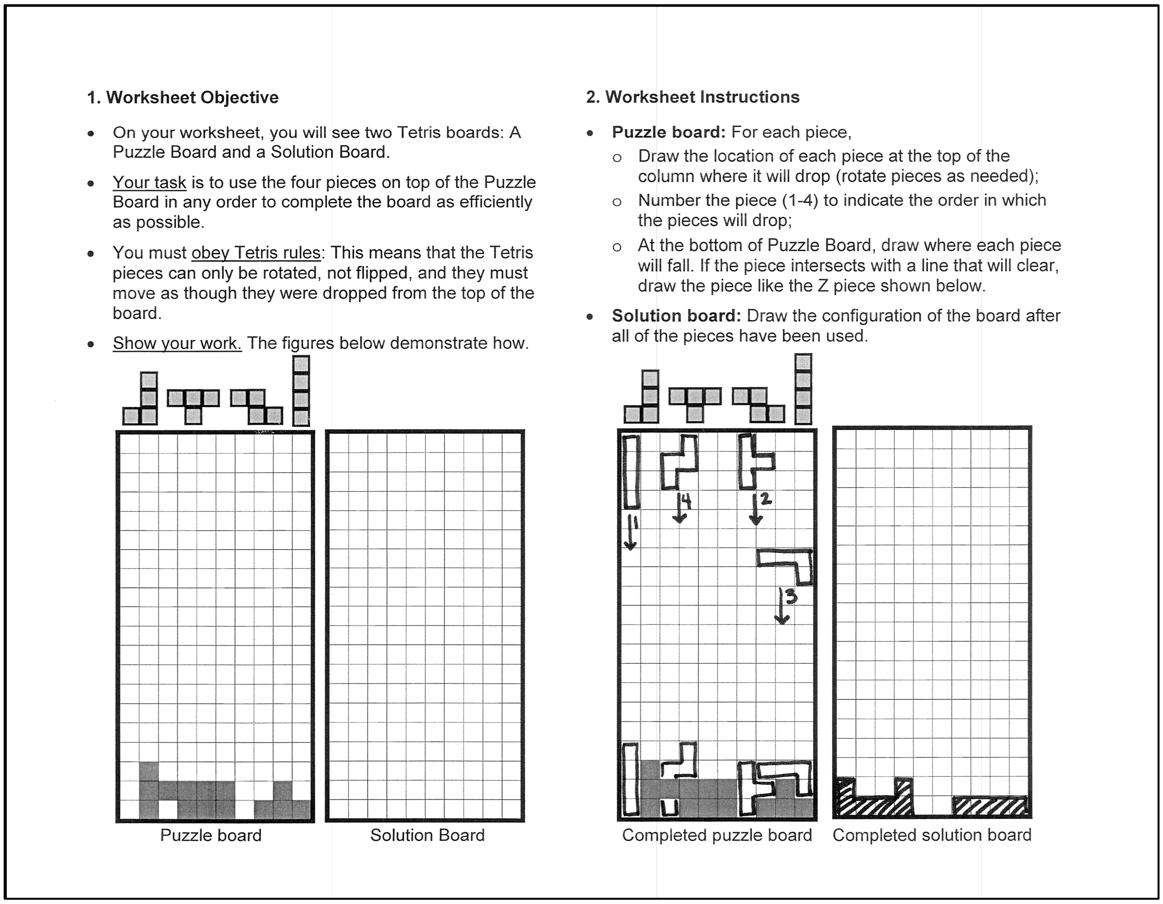


Figure 4. Screenshot from modeling slideshow.


1. Worksheet Objective

- On your worksheet, you will see two Tetris boards: A Puzzle Board and a Solution Board.
- Your task is to use the four pieces on top of the Puzzle Board in any order to complete the board as efficiently as possible.
- You must obey Tetris rules: This means that the Tetris pieces can only be rotated, not flipped, and they must move as though they were dropped from the top of the board.
- Show your work. The figures below demonstrate how.

2. Worksheet Instructions

- **Puzzle board:** For each piece,
  - Draw the location of each piece at the top of the column where it will drop (rotate pieces as needed);
  - Number the piece (1-4) to indicate the order in which the pieces will drop;
  - At the bottom of Puzzle Board, draw where each piece will fall. If the piece intersects with a line that will clear, draw the piece like the Z piece shown below.
- **Solution board:** Draw the configuration of the board after all of the pieces have been used.

Figure 5. Instruction sheet for worksheets in Experiment 3.

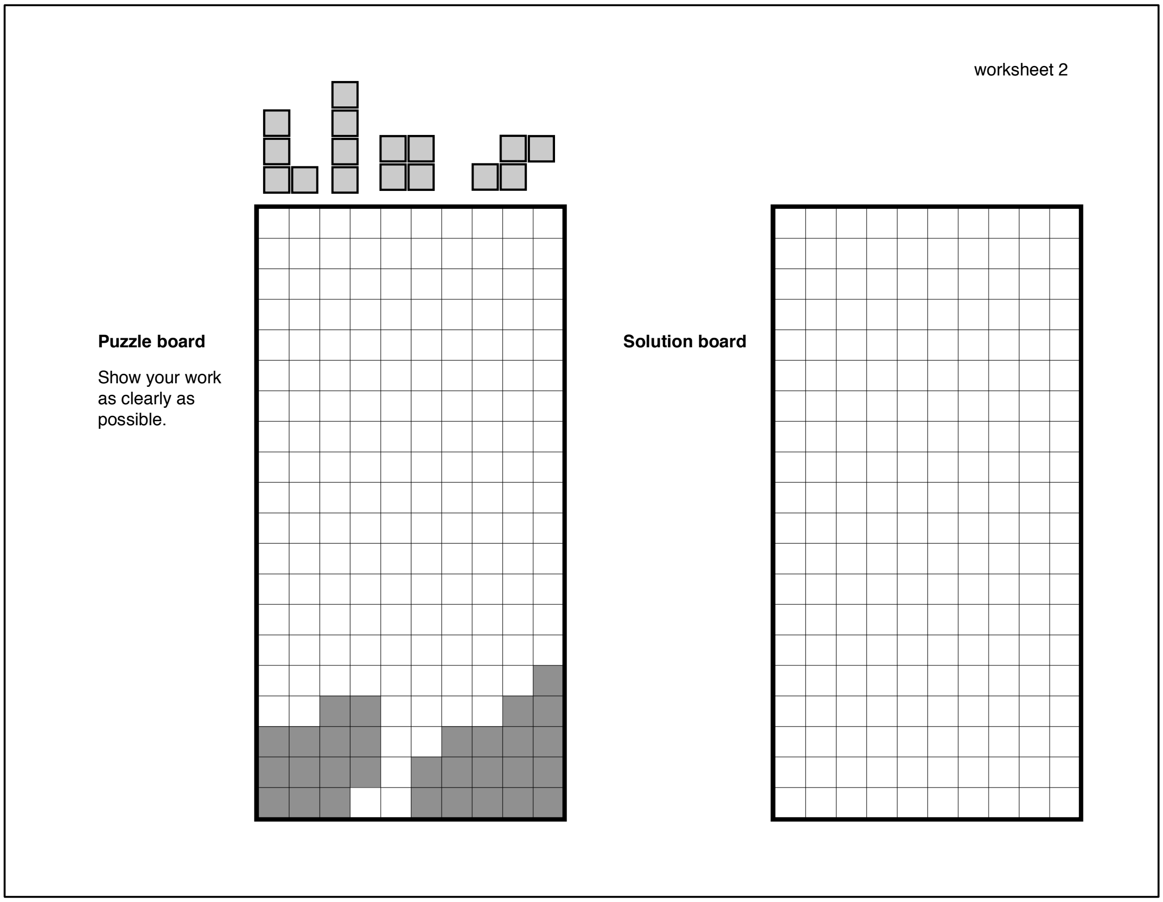


Figure 6. Example worksheet from Experiment 3.

Appendices

Appendix A

Narration scripts for Tetris lessons

Lesson 1 Narration Script

Before we start playing today we're going to practice solving Tetris problems. I'll complete the first one and explain my thought processes to you on the way. Then you'll complete the next one.

On top, there are four Tetris shapes. On the bottom is a Tetris board.

Our task is to use the four Tetris shapes on top in any order we choose, to complete the Tetris board as efficiently as possible. This means that we want to have as few squares left on the board as possible, so we're left with an open board ready for whatever pieces come our way.

Our first step is to evaluate the board [fade out pieces], so take a look at the blocks stacked up on the bottom of the screen. What should we focus on? What kind of pieces do we need?

One thing that stands out on this board is that one column is open all the way to the bottom row. If we can fill the open space on the bottom row, then that row will clear. To fill this space, we need a piece with a tail at least two blocks long. The only pieces with tails that long are the I piece, the L piece, and the J piece. In this case, we need either an I piece or a J piece, because the L piece goes the wrong way and would not fit in this space.

Let's keep looking across the board. Here's another space that requires a piece with a tail that's at least two blocks long. To fill this space, we need either an I piece or a rotated L piece, because here, the J piece goes that wrong way and would not fit in this space.

Finally, let's look at this space on the left side of the board. There are a lot of combinations that could work to fill this space. This requires a piece with two boxes on the bottom, and that can accommodate this raised box here. On the far left, the space could be filled in with an I piece, or any piece that can have a tail on the left side, like a T piece or a Z piece, but not an S piece.

Now let's look at the pieces we have. We have two spaces that need pieces with long tails, and we have two pieces that fit the bill. The L piece can only go in this space here, so let's put it there, and put the I piece, which is more flexible, in the deeper space. This will clear the bottom row.

Now we have a Z piece and a square left. The Z piece could go in the flat open space, but that leaves nowhere for the square to go. So let's put the Z piece on the far left and the Z piece in the open space. This clears two more rows, and leaves a nice open board for the next pieces to fit into.

Lesson 2 Narration Script

Before we start playing today we're going to practice solving Tetris problems. I'll complete the first one and explain my thought processes to you on the way. Then you'll complete the next one.

On top, there are four Tetris shapes. On the bottom is a Tetris board.

Our task is to use the four Tetris shapes on top in any order we choose, to complete the Tetris board as efficiently as possible. This means that we want to have as few squares left on the board as possible, so we're left with an open board ready for whatever pieces come our way.

Our first step is to evaluate the board [fade out pieces], so take a look at the blocks stacked up on the bottom of the screen. What should we focus on? What kind of pieces do we need?

One thing that stands out on this board is the hole on the bottom row. It's important to fill holes on the board, because if you let holes build up on the board then the board will fill quickly and you'll lose the game. Filling holes allows lines to clear and keeps the board from filling up.

Because there are three blocks above the hole, if we want to patch this hole, we'll have to fill in and clear the three lines above it.

Now look at the shape of the open spaces we need to fill. We need something that can drop a tail on the left. This means no square or S pieces, and backwards Js are not ideal either. If we look at the pieces we have, either the L, I, or T piece can rotate to fill in these spaces.

Let's start with the L piece. This piece can go in either of the spaces we just looked at. The space on the right is more shallow than the space on the left, so let's put this piece on the right side and use our taller pieces for the left.

Next let's place the I piece. This piece is tall, so let's place it in the deepest spot on the board. That will clear a row, and leave a nice open space where we can put the square.

When we place the square, we clear two more rows. This means that the hole on the board is no longer enclosed.

We can now use the T piece to fill the bottom row. The T piece can fit a few different ways, but I'll put it down flat so that the board is as versatile as possible for the next Tetris pieces we get.

Lesson 3 Narration Script

Before we start playing today we're going to practice solving Tetris problems. I'll complete the first one and explain my thought processes to you on the way. Then you'll complete the next one.

On top, there are four Tetris shapes. On the bottom is a Tetris board.

Our task is to use the four Tetris shapes on top in any order we choose, to complete the Tetris board as efficiently as possible. This means that we want to have as few squares left on the board as possible, so we're left with an open board ready for whatever pieces come our way.

Our first step is to evaluate the board [fade out pieces], so take a look at the blocks stacked up on the bottom of the screen. What should we focus on? What kind of pieces do we need?

One thing that stands out on this board is the hole on the bottom row. Remember, it's important to fill holes on the board in order to make your games last longer. Filling holes allows lines to clear and keeps the board from filling up.

Because there is one block above the hole, if we want to patch this hole, we'll have to fill in and clear that line. Once that line clears, we will need a piece that can drop a tail at least two blocks long on the right. The only pieces with tails that can fit are the I piece or the L piece, since the J piece goes the wrong way and would not fit in this space.

Now look at the open space on the right half of the board. We need to fill at least three bottom rows. On the far right, we need something that can drop a tail two boxes long. A J piece would be ideal, because it could also fill in the third row in the column to the far right. On the left side of this open space, we also need something with a tail at least two pieces long. Either an I piece or a J piece would fit. In the middle, either a T, Z, or S piece could work, depending on the other pieces.

On top of the board, we have an J piece, an I piece, a T piece, and an L piece. Remember that a J piece was ideal on the far right, so let’s put that there. That leaves two pieces with long tails. Remember that we could put either an I or an L in the area to the far left, and either an I or a J in the left side of the open space. That means we should use the I for the open space, since we only have an I and an L left. The T fits in nicely to clear the third row. This means that the hole in the board is not longer enclosed.

Finally, let’s place the L piece. That will clear two more rows. The board is now open for whatever pieces come next.

Lesson 4 Narration Script

Before we start playing today we’re going to practice solving Tetris problems. I'll complete the first one and explain my thought processes to you on the way. Then you'll complete the next one.

On top, there are four Tetris shapes. On the bottom is a Tetris board.

Our task is to use the four Tetris shapes on top in any order we choose, to complete the Tetris board as efficiently as possible. This means that we want to have as few squares left on the board as possible, so we're left with an open board ready for whatever pieces come our way.

Our first step is to evaluate the board [fade out pieces], so take a look at the blocks stacked up on the bottom of the screen. What should we focus on? What kind of pieces do we need?

Two things that stand out on this board are the two holes on the board. Remember, it's important to fill holes on the board in order to make your games last longer. Filling holes allows lines to clear and keeps the board from filling up.

Because there are two holes on this board, we will have to think ahead about how the board will change as we clear lines. In order to patch the hole on the bottom of the board, we have to clear the third row. In order to patch the hole higher up on the board, we have to clear the fifth row. How should we begin? We could start in the center of the board, but on the far right is the deepest open space on the board. Notice that if we fill this space, we will clear the third row and open up the hole on the bottom of the board. We can fill this space with an I piece or an L piece. An L piece would be nice because it fills in this space in the second column as well.

Now we have to imagine ahead what the board would look like if the third row were cleared. The pieces would fall down, leaving an open space two boxes deep in the middle of the board. We will have to fill in four rows in order to open the space above the other hole on the board.

To fill in this space, we will need a piece that can drop a tail at least two blocks long on the right. The only pieces with tails that can fit are the I piece or the L piece, since the J piece goes the wrong way and would not fit in this space. There are a few different configurations that could fill the space in order to fill these four rows. Once the row clears to open this final hole in the board, we will need something with a tail to patch the hole.

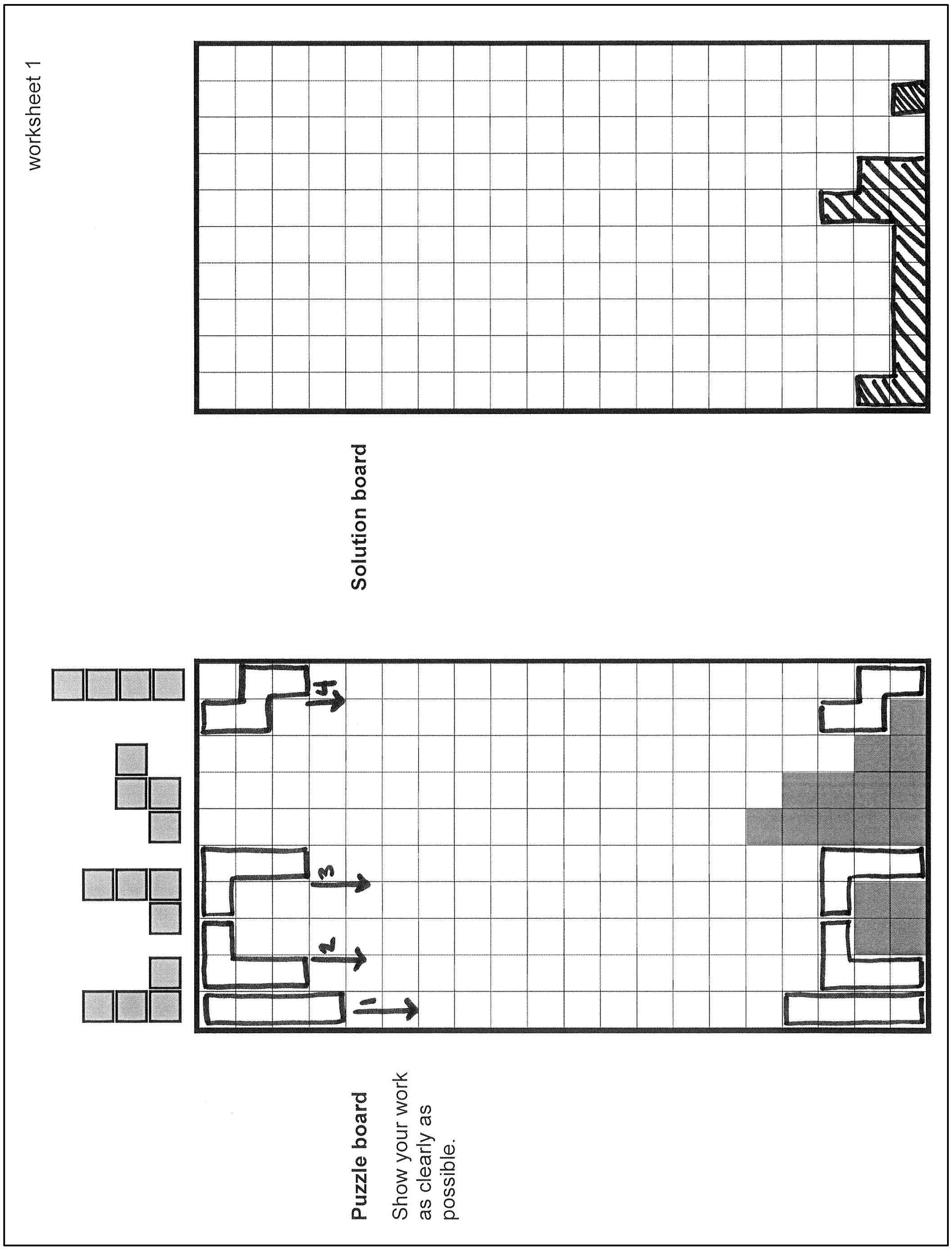
Back to the original board. On top of the board, we have an J piece, an L piece, an I piece, and an S piece. Remember that a L piece was ideal on the far right, so let's put that there. That clears the third row and opens up the hole in the middle of the board. The only piece left that fits in the deepest open space on the board is the I piece, so let's place that there to clear two more rows. The S piece fits nicely to clear another row, and this means that the hole in the board is not longer enclosed.

Finally, let's place the J piece. That will clear one more row. The board is now open for whatever pieces come next.

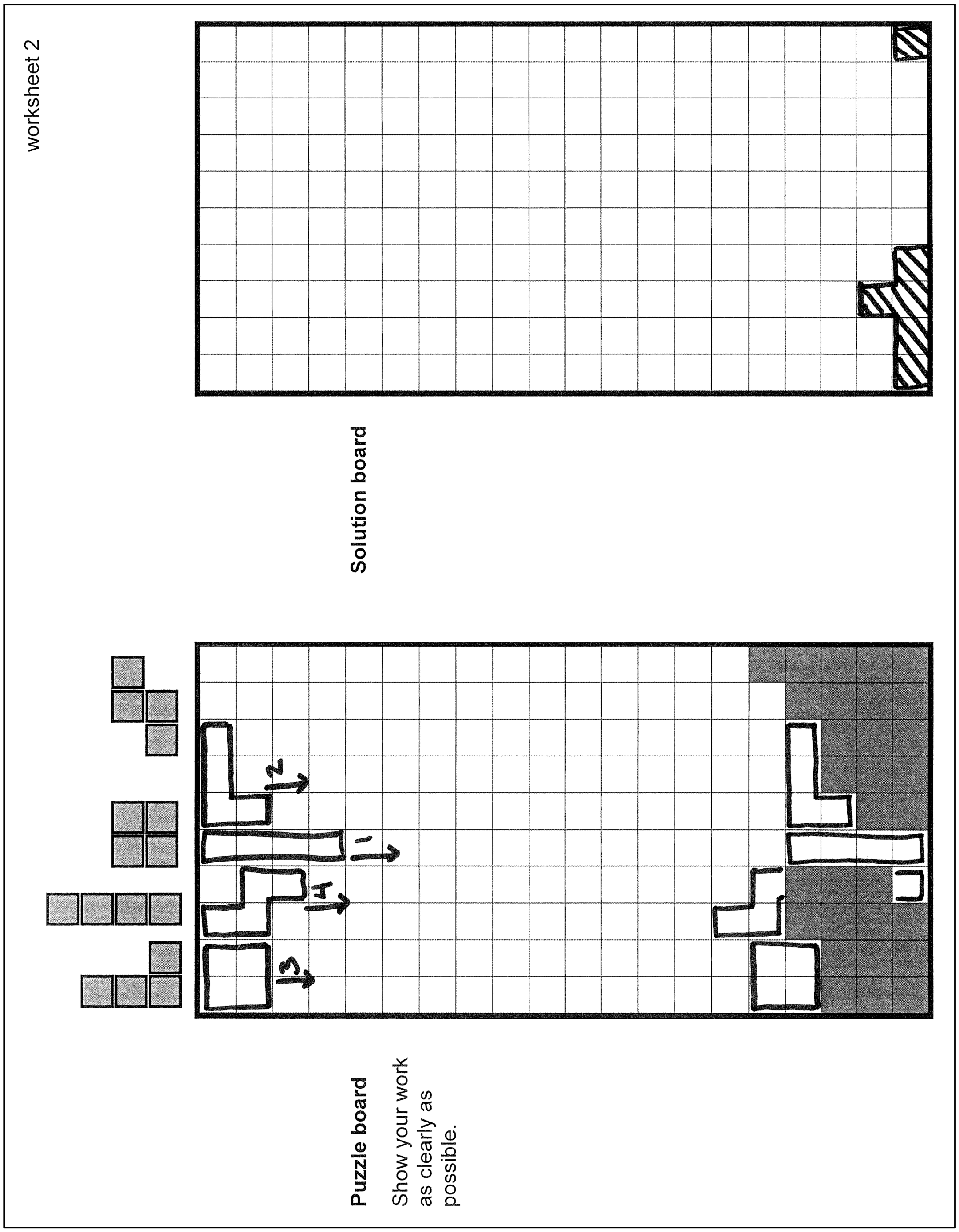
Appendix B

Examples of Completed Worksheets

Worksheet 1

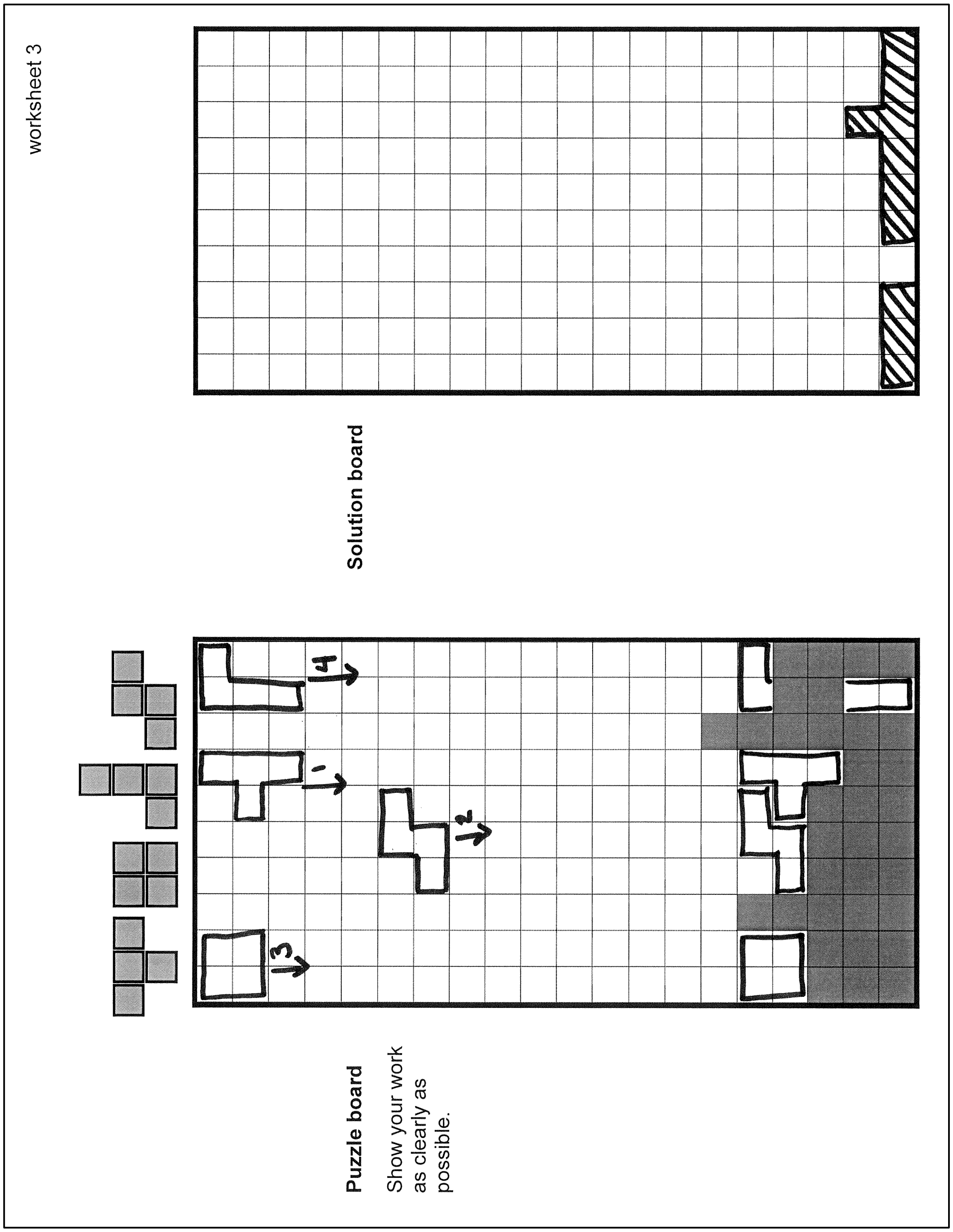


Worksheet 2

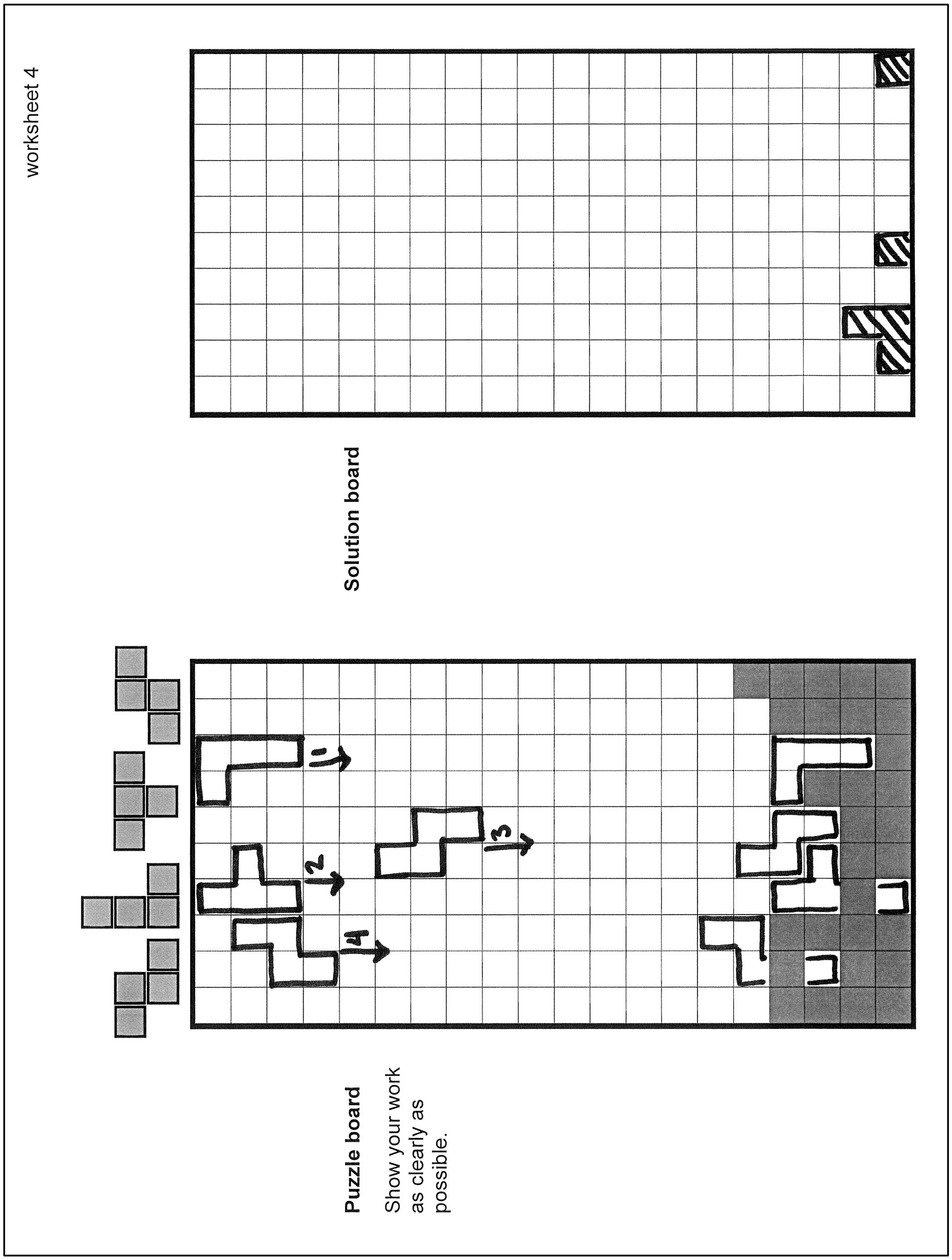


worksheet 2
Solution board
Puzzle board
Show your work as clearly as possible.

Worksheet 3


worksheet 3
Solution board
Puzzle board
Show your work as clearly as possible.

Worksheet 4



worksheet 4
Solution board
Puzzle board
Show your work as clearly as possible.