

Framing in serious gaming: the effects of initial instruction and prior gaming experience on
learning outcomes and motivation

Master thesis

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Abstract

As previous literature remained indecisive about which is the best condition to promote learning and boost motivation, telling students to play a serious game or to learn with it, we framed both playing and learning in two different ways. These four frames were learning as much as possible, learning for a test, playing for fun, or playing to perform. We extended the previous work done on the subject by including prior gaming experience with commercial games, as we believed this could moderate the relationship between condition and learning or motivation. We found a significant effect of framing condition on learning gain, however posthoc tests were not significant. Given the absolute scores, the best learning instructions appear to be "*try to learn as much as possible*", as well as "*play and have fun*", with which we may have united the conclusions of previous research. If learning or playing is the better learning condition depends on the actual frame. Instructing students to play and perform well in the game is not recommended. Equal to other studies, no effect on motivation was found. Prior gaming experience could not serve as a moderator, therefore conclusions about this variable's effect could not be drawn.

Keywords: framing, serious games, learning outcomes, motivation

Framing in serious gaming: the effects of initial instruction and prior gaming experience on learning outcomes and motivation

Serious games (SGs) emerged at the beginning of this century and are becoming more popular (Breuer & Bente, 2010). SGs are digital games used or designed to foster learning and motivation (Arnab et al., 2012; Gee, 2003; Prensky, 2003). By combining learning with gameplay, SGs were believed to evoke a revolutionary improvement of student engagement (Rigby & Przybylski, 2009; Shaffer, Squire, Halverson, & Gee, 2005). However, the beneficial effects on motivation have not yet been convincingly shown (Wouters, Van Nimwegen, Van Oostendorp, & Van der Spek, 2013). Metareviews comparing teaching with SGs to regular classroom teaching have reported positive effects on learning outcomes (Clark, Tanner-Smith, & Killingsworth, 2016; Connolly, Boyle, MacArthur, Hainey, & Boyle, 2012; Wouters et al., 2013).

Wouters et al. (2013) found larger learning effect sizes when SGs were combined with other instruction methods, played in groups, or played repeatedly. These contextual factors or *non-game factors* are important next to a good game design, (as opposed to *in-game factors* such as feedback, adaptivity to the learner's level, interactivity, a clear goal, and game rules; Wouters et al., 2013).

Erhel and Jamet (2013) mention the playing context (home or school) of SGs as such. In a classroom setting, the exact words teachers use upon instruction, or the type of *framing*, could be an important non-game factor in enhancing learning effectiveness and motivation with SGs. Framing makes manipulation easier as it does not involve technical game modification in order to study effects. In psychology, framing refers to the presentation of information in such a way that it affects one's judgement of a phenomenon or one's choice (Levin, 1987). Frames emphasize certain features and hide others, thereby providing mental access to different thoughts (Kahneman, 2003) and activating other cognitive scripts

(Lieberoth, 2015). Since SGs unite learning and playing, these are two apparent frames, believed to cause different mindsets.

Whether a learning or playing frame is favorable stays yet unclear, as few works are published and contradict each other (Erhel & Jamet, 2013, 2016; Hawlitschek & Joeckel, 2017). In practice, teachers should be able to rely on scientific proof and select the most effective frame for learning or motivation, which may differ. This study wants to add to the body of knowledge on how framing of SGs affects both learning outcomes and motivation. Moreover, it will add student's *prior gaming experience* (PGE) with commercial games since it may affect their attitude towards and motivation to play SGs as well as their learning outcomes (Frank, 2012; Nebel, Schneider, Schledjewski, & Rey, 2017).

Theoretical framework

Prior gaming experience

High prior gaming experience (PGE) may lead to contrasting expectations of SGs. The mainstream opinion states that high PGE motivates students to play SGs in school (Connolly et al., 2012). However, students will mainly judge a SGs' entertainment value (Arnab et al., 2012). When expecting equally high game standards as in commercial games, students might uncover SGs as disguised assignments, and become demotivated (Habgood & Ainsworth, 2011).

Frequent gamers focus attention solely on game stimuli and overlook the educational content (Lee & Heetert, 2017). When not reminded of learning, they might not learn anything. Framing a SG as learning instead of playing material might help. However, Frank (2012) noticed that some students forgot their learning instruction during gameplay and slipped in and out of *gamer mode*, which hampered learning. One could also hypothesize that frequent gamers will more quickly master game features than less experienced players, leaving more learning time. However, Lee and Heetert (2017) found that gaming experts

indeed freed more space in working memory, that they used to process additional game elements instead of learning content.

Thus, according to literature, PGE might either improve or diminish motivation, and frequent gamers might most likely be worse learners with SGs. So, high PGE may lead to different outcomes for learning and motivation than low PGE.

Framing, learning and motivation

Learning. *Intuitive learning* is a recognized problem with SGs. Students process learning material superficially when playing SGs (Wouters et al, 2013), instead of *deep learning*: actively connecting it to prior learned concepts, which will foster memorization and transfer of knowledge onto unfamiliar situations (Kester, Kirschner, & Corbalan, 2007). A learning frame might remind students of active knowledge acquisition and will probably be more beneficial for learning than a playing frame. Thus, a learning frame aims at mastering the learning content or deep learning. Contrary to this, the playing frame will focus student's attention on the game elements and triggers performance, like winning or achieving a highscore.

Similarly, *achievement goals* can influence someone's learning outcomes and engagement (Erhel & Jamet, 2016). A *mastery goal* indicates willingness to learn new skills and knowledge, whereas a *performance goal* refers to the desire to show one's competence to others (Ames & Archer, 1988; Senko, Hulleman, & Harackiewicz, 2011). Erhel and Jamet (2016) showed that achievement goals could be manipulated by framing of SGs, just as in previous studies on written texts. They concluded that a learning frame induced a mastery goal and enhanced learning.

Motivation. SGs exist because playing was associated with high *intrinsic motivation* (Habgood & Ainsworth, 2011). Intrinsic motivation refers to enjoying activities for their own sake, as opposed to *extrinsic motivation* or being moved by rewards (like grades) and

punishment (Ryan & Deci, 2000). A playing frame would then foster intrinsic motivation best, because playing is rewarding. However, it is possible that mastery goals, or learning frames, are equally well motivating through pure enjoyment of learning.

Framing and goal setting in previous studies

Erhel and Jamet (2013, study 1) were the first to investigate learning outcomes and motivational differences by asking participants in the game's introduction screen to either play or learn with it. The authors assumed that the playing and learning condition evoked a performance goal and a mastery goal, respectively. They reported superior transfer in the learning condition, but no differences for memorization and no support for higher intrinsic motivation in the playing condition. In 2016, Erhel and Jamet framed performance (demonstrating success by collecting more points than others in a language game) and mastery (learning a language rule through the game) instructions explicitly. Motivation was not considered. As expected, the mastery frame elicited deeper learning. Remarkably, in both of their studies, both instructions told students to play and simultaneously learn. Manipulation was shaped by emphasizing one of the mentioned goals. From a framing point of view, this might be confusing for participants. In another study, Nebel et al. (2017) built a Minecraft game to compare three conditions: a performance goal (reach the church by train, proceeding required learning), a mastery goal (learn everything about gates and logic circuits) and an especially designed goal-free condition (play and have fun). The goal-free condition confused participants as they expected a goal, upon which 68% adopted a mastery goal. They hypothesized that the goal-free condition would be least beneficial for motivation and best for transfer of learning. The authors included PGE as a moderator in their design but found no significant differences between conditions for learning outcomes and motivation due to low statistical power. Despite this, its set-up suggests that instead of looking just at contrasting

playing and learning, we should involve goal setting when trying to explain which condition is the better.

Contrary to Erhel and Jamet (2013), Hawlitschek and Joeckel (2017) reported that in their field study, the learning instruction had lower learning outcomes compared to the play-and-have-fun condition. They expected an explicit learning frame to decrease motivation compared to the playing frame, but no significant differences were found. So, they concluded that there was no additional need for explicit learning instructions in SGs. Their playing condition was an unambiguous instruction to play and have fun, like the goal-free condition in Nebel et al. (2017). Nevertheless, their learning condition seems to mix two goals: a mastery goal by asking to learn as much as possible and a second goal by asking students to answer history questions afterwards. Students in this condition might have focused on the test part of the instruction instead of the mastering part, making it an externally induced performance goal instead of a mastery goal.

Thus, possibly, two types of performance goals can be framed: in-game by achieving high game scores, or outside the game (external) by stressing to learn for a test. The first goal is a playing goal whereas the second is a learning goal. Table 1 concludes this section with an overview of frames and goals found in the discussed literature.

Table 1

Overview of Frames and Compared Goals in Previous Research

Authors	Emphasized goal		
	Goal-free	Performance	Mastery
	In-game	External	
E&J, 2013	SG called game		SG called learning module*
E&J, 2016	play & earn points		learn & master*
Nebel et al., 2017	play & have fun	play & earn points	learn & master
H&J, 2017	play & have fun*		learn for test ¹ learn & master ¹

Note. E&J = Erhel & Jamet, H&J = Hawlitschek & Joeckel.

* Significantly better learning outcomes than in the other condition. Motivation results were not on significance level or not measured (Erhel & Jamet, 2016).

¹ Combined goal

The present study

The article by Hawlitschek and Joeckel (2017) raises three questions. First, if the two learning goals, when split into separate frames, would generate different learning outcomes. Secondly, how these two learning goals relate to the playing goals. Third, if splitting will also affect motivation outcomes. In this study we assume that different use of words will lead to the pursuit of different achievement goals. Frames should therefore be unambiguously formulated. Thus, we will compare four framing conditions with only one goal per frame derived from Table 1: (a) playing for fun (PF, goal-free), (b) playing to perform (PP, in-game performance goal), (c) learning for a test (LT, external performance goal), and (d) learning as much as possible (LA, mastery goal). After Nebel et al. (2017), we will include PGE as a

possible moderator. Then, the combined research question is: Does the type of framing a SG affect learning outcomes and intrinsic motivation, and are these effects moderated by PGE?

The hypotheses originate in the results of other authors for learning outcomes, and in their hypotheses for motivation:

H1: Different learning frames give different results.

H1.1 LT renders worse learning outcomes than PF (Hawlitschek and Joeckel, 2017).

H1.2 The mastery goal LA gives highest learning outcomes (Erhel & Jamet, 2016).

H1.3 LA is superior over PP (Erhel & Jamet, 2013, 2016).

H2: Motivational outcomes vary between conditions (Erhel & Jamet, 2013).

H2.1 Playing frames are more motivating than learning frames (Hawlitschek and Joeckel, 2017).

H2.2 LT is least motivating (Hawlitschek and Joeckel, 2017), and PP most as this resembles playing a commercial game (Frank, 2012)

For PGE, hypotheses follow the most likely influential direction, giving the following additional hypotheses:

H3: PGE negatively moderates the relationship between framing condition and learning outcomes.

H4: PGE positively moderates this relationship.

Method

Participants

A convenience sample of 10-14 years old Dutch school children ($N = 42$, 27 boys, 15 girls, $M_{age} = 11.91$, $SD = 1.34$) was recruited via parents and teachers in the author's social network. Parents received an information letter about the research set-up and confidentiality of data collection, plus a digital consent form (see Appendices C and D). Registration of their child(ren) occurred by returning the completed form via e-mail. Participants were

alternately assigned to different framing conditions (in the order: PF, PP, LA, LT, PF, etcetera) with corresponding participant codes in the return order of the consent forms. In this way, 51 participant codes were distributed, of which 45 were used. Children indicated their voluntary participation via online consent after being informed about their rights and the purpose of the research. Three participants quit midway. None of the final 42 participants had prior experience with Python or the related JavaScript coding language. Sixteen participants had no programming experience at all, 23 had coded with coding blocks before, and three knew another programming language.

Table 2 shows the composition of each group. As can be seen, the PF condition has fewer participants, due to higher nonresponse in this group.

Table 2

Participant Numbers, Gender and Age per Framing Condition

Condition	Number of participants			M_{age}	SD
	Total	Boys	Girls		
Playing for fun (PF)	9	7	2	12.67	1.12
Playing to perform (PP)	11	8	3	12.36	1.43
Learning for the test (LT)	11	5	6	10.91	1.22
Learning as much as possible (LA)	11	7	4	11.82	0.98

Note. $N = 42$.

Before the experiment, we performed a power analysis, using a *power* of .80 and an effect size of .40 (see Appendix B). The calculated sample size was 73 participants, or at least

19 participants per condition. Due to the current Corona crisis, these numbers were lower, as the research could not take place in schools.

Research design

This study used a quasi-experimental design which aimed at finding relationships between the independent group variable *framing condition* and the two dependent variables *learning gain* (posttest – pretest) and *intrinsic motivation gain* (posttest – pretest). Framing condition included four conditions: (a) playing for fun, (b) playing to perform, (c) learning for the test, and (d) learning as much as possible. The design included the moderating variable prior gaming experience (PGE), see Figures 1 and 2.

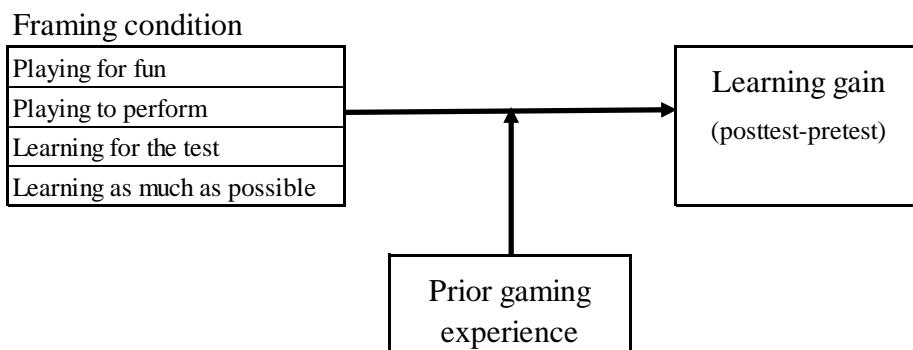


Figure 1. Schematic overview of the supposed relationships between framing condition, PGE and learning gain.

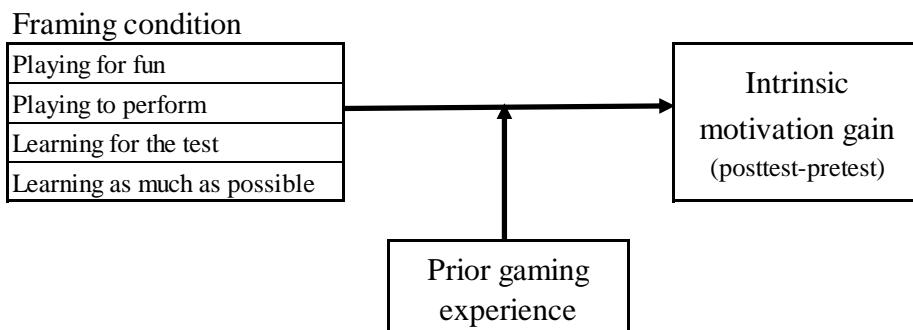


Figure 2. Schematic overview of the supposed relationships between framing condition, PGE and intrinsic motivation gain.

Materials and measures

The serious game. Participants played the free Dutch version of ‘*CodeCombat*’ (CodeCombat, 2020a) and learned how to control their hero using the coding language Phyton. The game’s purpose is to navigate your hero out of the dungeons by using suitable code, beat ogres and munchkins and collect diamonds. Diamonds serve to buy the hero new gear, needed to complete some levels. However, most levels are about writing good code. For level completion, they earn experience points (XP).

In the game, participants first learn to use *basic syntax* (commands to move their ‘hero’ around), *arguments* (notations for multiple actions). Moreover, they learn about *strings* (meaningful text in commands, like whom to attack), *comments* (remarks not belonging to the code) and *while true loops* (repeating code endlessly) and *variables* (input that changes). Appendix I shows in which game level specific commands are introduced. Python commands make use of English, although instructions are in Dutch.

A yellow arrow gives participants in-game directions, see Figure 3. When starting a level, in the left part of the screen, the hero shows which route to walk.



Figure 3. Overview of “The dungeon of Kithgard”, showing the yellow guiding arrow.

Assignments appear in the screen's upper left corner, see Figure 4.



Figure 4. Overview of the game screen, showing the map with hero and goals on the left, and the editor on the right, and coding instructions in the blue box.

These include programming assignments like “*use less than 5 lines of code*”, as well as game assignments like “*beat every ogre and stay alive*”. Participants write their code in the editor on the right. Modelling and completion examples are included in the editor, to learn and improve coding skills. A completed code needs execution (via the run command button) to check if it works. If so, the player gathers XP and continues to the next level. If the code contains errors, red marks indicate what parts need repair. In-game programming *hints* are available via the button in the upper right corner.

Motivation questionnaire. Intrinsic motivation resulting from doing the SG was measured with Ryan and Deci's Intrinsic Motivation Inventory (IMI), translated and validated for this age group in Dutch by Meijer and Van Eck (2008). We used only the 7-item interest/enjoyment subscale for both pretest and posttest, as according to the Center for Self-Determination Theory (2020) this is the original, pure intrinsic motivation self-report scale. Participants recorded their answers on a 7-point Likert scale, on which a score of 1 means

“*not at all true for me*”, 4 means “*somewhat true*” and 7 means “*very true for me*”. An example of how to answer a question on this scale was included. The instruction for the pretest was “*think about the game you are about to do. That is why some questions will sound strange now. Try to imagine how it will be and choose the most likely answer*”. After reversing negatively posed items (see Appendix G) we calculated the average intrinsic motivation scores. Both the pretest and the posttest had sufficient reliability (Van Berkel, 2017) with Cronbach’s α ’s of 0.77 and 0.85, respectively. Subtraction of average scores in posttest and pretest gave the average motivation gain on a scale from -6 to 6.

Subject knowledge questionnaire. A subject knowledge questionnaire containing 12 multiple choice questions was constructed and administered before and after gaming, see appendix H. Pretest and posttest had a different question order. First, a test matrix was created to ensure coverage of all commands (see Appendix I). We used objectives per level taken from CodeCombat (2020b). We then created memorization/understanding and application questions according to the matrix. The pretest instruction explicitly stated that not knowing anything yet was no shame. Afterwards, we calculated the total score per participant (1 point per correct answer). Subtraction of posttest and pretest scores per participant gave the score (0–12) for learning gain. Posttest reliability calculation gave Cronbach’s $\alpha = .48$, which is considered insufficient for a summative test (Van Berkel, 2017).

Prior gaming experience questionnaire. The PGE questionnaire was based on Orvis’ 1-item scale (Brusso & Orvis, 2013) and adjusted to the target audience by dividing into weekdays and weekends (when children supposedly spend more time gaming). We changed game examples to match their experience and added the expression ‘yourself’ to avoid counting watching others as playtime. Moreover, examples of the game platform were mentioned. The first item read “*On a typical weekday, about how many hours per day do you play digital video games (like Fortnite, Grand Theft Auto, Super Mario Kart, Minecraft,*

Subway Surfer, MovieStar Planet) yourself, online or on a computer / tablet or game console (like Wii, XBOX, Playstation, Nintendo)?” The second item asked the same for weekends.

Reliability of this 2-item scale was good, Cronbach’s $\alpha = .73$. The total score for PGE was calculated in hours per week.

Online questionnaires and instruction booklet. Above questionnaires were incorporated in a start (motivation and subject knowledge pretests, plus PGE) and final (motivation and subject knowledge posttests) online questionnaire, created with LimeSurvey software (LimeSurvey, n.d.). Additionally, the start questionnaire contained control questions about gender, age and programming experience. Next to both posttests, the final questionnaire contained control questions about gaming time, level reached, goal recall (or correct memorization of the initial instruction, after Nebel et al., 2017) and adherence to instructions (after Frank, 2012).

The self-paced questionnaires referred to an instruction booklet, comprising of practical instructions, a checklist to sign off each procedural step, and the assigned frame (see appendix F). All texts consistently avoided mentioning *playing* or *learning from* a game, but used the neutral phrase *doing* a game. After testing the understandability of the online questionnaires, the booklet and the game by a 15-year-old, uncertainties in instructions were adjusted and additional gaming tips included.

Frames. The instruction booklet contained the participant’s assigned frame. Both playing frames started with the instruction “*You are going to play the game CodeCombat*”, followed by “*Have fun!*” for the PF condition or “*Make sure that you earn as much XP as possible by beating the Ogres and Munchkins and to grab the diamonds!*” in the PP frame. Both learning frames stated: “*You are going to do the game CodeCombat to learn the Python programming language.*” These frames still avoided the word *playing*. The LA instruction

ended with “*Learn as much Python code as possible!*”, whereas the LT frame mentioned “*You will get a test afterwards!*”.

Procedure

After returning the consent form, parents received the instruction booklet, their child’s participant code and access links to the questionnaires and the SG. Siblings got different frames. Parents received instructions only to print the booklet, help to start up the questionnaire and game if necessary, but not assist with coding or answering questions (see appendix E). Participants could choose a convenient time for questionnaire completion and gaming within 3.5 weeks. Parents received a reminder of the deadline during the last week.

Participants took part in this online study from their homes in their spare time on their own laptop or computer. They were asked to read the preparative instructions in the instruction booklet first (have two hours available, gather a pen, headphone and timer).

The study’s set-up consisted of three phases. During the first phase, after giving online consent, participants filled in the start questionnaire. The second phase started with reading the frame from the instruction booklet. As level numbers did not appear on the game map, participants had to locate level 17 first. Then, they set a timer as a reminder for the estimated one hour of playtime. The second phase consisted of playing levels 1–17 from “The dungeon of Kithgard” without creating an account, to make sure everybody played the same levels. When the timer went off, participants indicated whether they were beyond, at, or not yet at level 17. Those who did not yet finish could play for another 15 minutes and then stop gaming regardless if they were ready or not. Alternatively, they could stop after one hour when they found the game too difficult. The third phase consisted of filling out the final online questionnaire directly after gaming. Hereafter, a debriefing text told participants that the ‘test’ from the LT condition was the posttest. The total procedure took approximately

1,5–2 hours. Although not part of the procedure, some parents e-mailed to describe their child's experience or to correct errors, which we filed.

Data Analysis

Both start and final questionnaires were downloaded into SPSS25 for analysis and combined based on participant code. Three participants filled out the first questionnaire a second or third time, but we only used the first entry.

We calculated the total game time in hours per week from the weekday and weekend questions to conceptualize PGE. The dependent variable motivation gain was defined as posttest motivation - pretest motivation, which preserved the 1–7 scale. The dependent variable learning gain was operationalized as the increase in correct answers on the Python test by posttest score - pretest score. Data for motivation, subject knowledge and PGE were examined and checked on significant group differences between conditions. Control variables were reviewed. We checked assumptions for AN(C)OVA, and if violated, performed appropriate non-parametric tests, if possible. After the omnibus test, Bonferroni adjusted posthoc tests were performed to test hypotheses for motivation and learning gain. In all analyses, a significance level of .05 was applied.

Results

Descriptive statistics

Motivation. Average motivation scores in the total dataset ranged from 1.4 to 6.7 in the pretest ($M = 5.11$, $SD = 1.01$) and from 2.7 to 7.0 in the posttest ($M = 5.22$, $SD = 1.26$). Table 3 shows the results for the derived dependent variable motivation gain.

Table 3

Descriptives for Motivation Gain

Framing condition	<i>M</i>	Minimum score	Maximum score	<i>SD</i>	<i>N</i>
PF	0.21	-2.9	2.0	1.39	9
PP	-0.07	-2.3	1.1	1.13	11
LT	0.23	-2.3	1.7	1.33	11
LA	0.09	-1.4	1.1	0.74	11
Total	0.11	-2.9	2.0	1.13	42

Note. Motivation gain is measured on a scale of -6 to 6.

Learning. Average subject knowledge scores changed from $M = 6.10$ ($SD = 1.95$) in the pretest to $M = 7.71$ ($SD = 1.85$) in the posttest. Descriptives for the dependent variable learning gain are in Table 4.

Table 4

Descriptives for Learning Gain

Framing condition	<i>M</i>	Minimum score	Maximum score	<i>SD</i>	<i>N</i>
PF	2.67	0	7	1.43	9
PP	0.64	-2	3	2.12	11
LT	1.09	-1	3	1.45	11
LA	2.27	0	5	1.85	11
Total	1.62	-2	7	1.85	42

The histogram of the pretest revealed significant negative skew and kurtosis ($z_{\text{skewness}} = -3.32$, $z_{\text{kurtosis}} = 4.38$, $p < .05$). Posttest distributions did not look normally distributed on the

histograms, as two distinct peaks seem to be present, but z -scores for skewness and kurtosis and normality tests looked fine.

PGE. After calculation of PGE per week, one participant seemed to game for 112 hours a week. Close examination of source data showed an impossible entry of 20 hours per weekday. As the number for weekends was a reasonable number for one day, it was concluded that this was a typo and not the total weekly amount. The sum variable was corrected to 24 using an entry of 2 hours for weekdays. PGE ranged from 0 to 53 hours per week ($M = 17.19$, $SD = 11.88$) in the total sample. Table 5 displays the results per condition.

Table 5

Descriptives for Prior gaming Experience per Week

Framing condition	<i>M</i>	Minimum score	Maximum score	<i>SD</i>	<i>N</i>
PF	23.56	9	49	13.27	9
PP	22.36	7	53	12.70	11
LT	10.91	0	27	7.93	11
LA	13.09	2	34	9.26	11
Total	17.19	0	53	16.00	42

End level, goal recall and adherence to instruction. Of all 42 participants, 27 reached or finished the 17th level and six of them used the extra 15 minutes. Thus, 15 participants did not reach level 17, of whom seven quit after the first hour. Throughout conditions, we observed peaks in the histograms around levels 8-10 and 15-17. The last subject started at level 15, meaning that only 29 children studied all test subjects. This group consists of 33% of the 10-year-olds in the total dataset, 67% of all 11-year-olds, 62% of all

12-year-olds and 100% of the 13- and 14-year-olds. According to some 10- and 11-year-olds' parent's comments, the game was too difficult for their child.

Correct goal recall and adherence to the instruction varied per condition, see Tables 6 and 7.

Table 6

Goal Recall After Gaming

Framing condition	Recalled condition				
	PF	PP	LT	LA	Forgotten
PF	66.7 (6)	11.1 (1)	-	-	22.2 (2)
PP	-	72.7 (8)	-	27.3 (3)	-
LT	18.2 (2)	-	9.1 (1)	18.2 (2)	54.5 (6)
LA	-	-	-	63.6 (7)	36.4 (4)

Note. Correct recall is displayed in bold font. Numbers are percentages of participants per condition, with absolute numbers between brackets.

Table 7

Reported Adherence to Instruction per Framing Condition after Correct Goal Recall

Framing condition	Adherence to instruction		
	All the time	Forgot it sometimes	No, started gaming and forgot it
PF	50.0 (3)	16.7 (1)	33.3 (2)
PP	75.0 (6)	25.0 (2)	-
LT	-	100.0 (1)	-
LA	57.1 (4)	42.9 (3)	-

Note. Numbers are percentages of participants who recalled their condition correctly, with absolute numbers between brackets.

Analysis

Check of assumptions. Prior to analysis, assumptions for ANOVA were checked. Independent samples could be assumed, as different participants contributed. However, siblings may have assisted each other. Linear relationships between the variables exist. For all dependent variables, error bars and Levene's test indicated that the assumption of homoscedasticity was met. Outliers were not present in the dataset. Because of the small sample sizes per condition, we examined multiple parameters to judge normality of data. Visually judging the histograms and Q-Q plots of the four conditions was complemented with calculating z-scores for skewness and kurtosis and normality tests (Field, 2018; Kim, 2013). Shapiro-Wilk tests were used as they have most power for small samples (Gashemi & Zahediasl, 2012). With sample size $N < 50$, z-scores outside the $+/- 1.96$ range imply a significant deviation from normality (Field, 2018; Kim 2013). Based on these parameters, the assumption of normality and normally distributed errors was met for learning gain. However, normality was doubtful for motivation gain, as histograms and Q-Q plots of the LT and PP conditions deviated visually from normality, although all calculated values indicated otherwise. PGE was not normally distributed, as the histograms of the LA and PP conditions showed significant deviations ($z_{\text{skewness, LA}} = 2.02$, $z_{\text{skewness, PP}} = 1.97$, $z_{\text{kurtosis, PP}} = 2.26$).

For ANCOVA, the additional assumption of equal distribution of the covariate across conditions exists, as covariate and treatment effect should be independent (Field, 2018). Visual examination showed a violation of this assumption for PGE. A Kruskal-Wallis independent-samples test confirmed the existence of significant differences in PGE between conditions, $H(42) = 9.45$, $p = .02$. Therefore, we could not examine the moderating role of PGE on the relationship between framing condition and motivation or learning gain.

Effect of framing condition on motivation. First, we examined the effect of the intervention on the total sample. As normality of pretest motivation scores could not be

assumed, a Wilcoxon matched-pairs test was performed, which did not show a significant difference between the average scores before ($M = 5.11, SD = 1.01$) and after ($M = 5.22, SD = 1.26$) gaming, $Z = -1.13, p = .26$. So, motivation did not change by gaming. ANOVA showed nonsignificant differences between conditions for motivation, $F(3, 38) = 0.15, p = .93$. When normality is doubtful, Field (2018) recommends a sensitivity test (to check a parametric test with a nonparametric equivalent). The result from ANOVA was confirmed by a Kruskal-Wallis independent-samples test, $H(3) = 0.95, p = .81$. Thus, on average, every participant gained an equal amount of motivation through gameplay, regardless of the frame.

Effect of framing condition on learning. ANOVA showed that group differences in Python knowledge did not yet exist before playing the SG, $F(3,38) = 0.32, p = .81$. In order to see if the intervention was successful, we carried out a paired-samples *t*-test, which showed a significant positive effect of playing the SG on the results of the subject knowledge test between pretest ($M = 6.10, SD = 1.95$) and posttest ($M = 7.71, SD = 1.85$), $t(41) = 5.68, p < .001, d = 0.83$. This qualifies as a large effect (Cohen, 1992). Another ANOVA showed that learning gain is significantly influenced by framing condition, $F(3, 38) = 3.21, p = .03, \eta^2 = .20$. Despite a medium effect size, Bonferroni adjusted posthoc tests did not result in significant group differences. Nevertheless, the visual distance between mean group scores may give directions, see Figure 5.

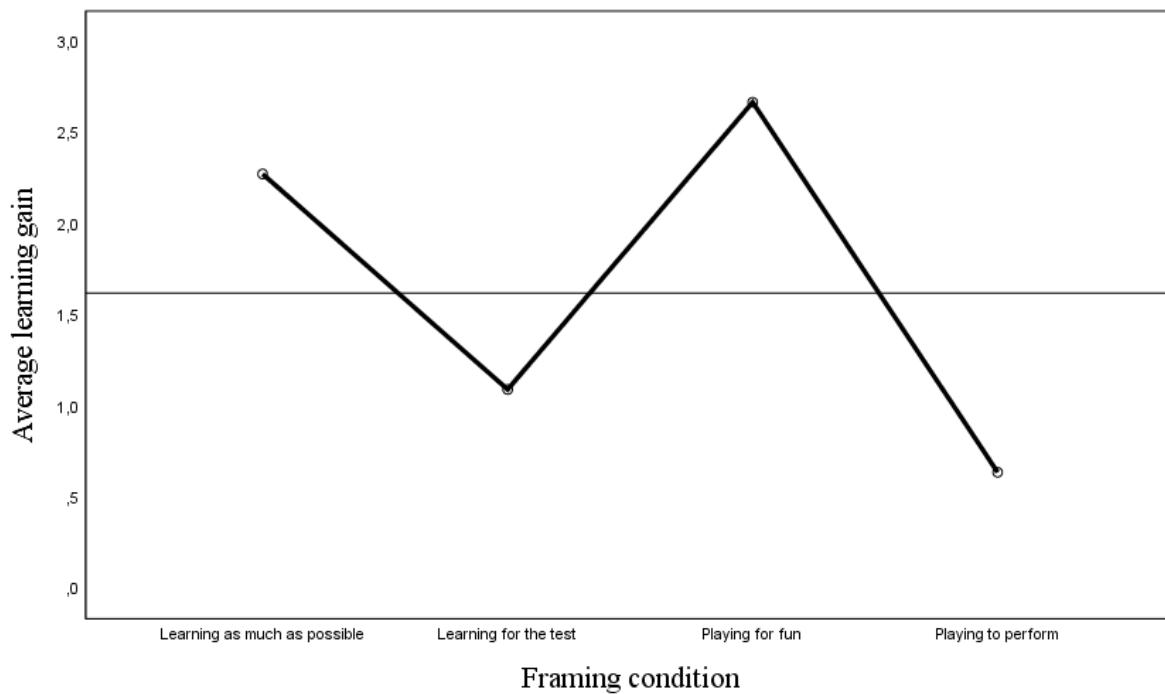


Figure 5. Comparison of average learning gain per framing condition. The horizontal line displays the mean learning gain ($M = 1.62$) in the total sample ($N = 42$).

According to Figure 5, participants in the PF condition seem to have gained more knowledge of Python than the participants in the PP condition. Participants in the LA and PF conditions gained similar amounts of Python knowledge. Condition LT scores in between.

However, not all participants managed to study all subjects within time. The control variable that measured the level participants reached could not be used as a moderator to control for this, due to severe nonnormality of data.

Discussion

This study compared two playing and two learning frames derived from previous literature to answer the research question: “Does the type of framing a SG affect learning outcomes and intrinsic motivation, and are these effects moderated by PGE?” The following sections will first discuss the findings resulting from these analyses, compare these to the conclusions of other authors, and discuss implications per variable. After that, general implications, limitations and recommendations of the study will be discussed.

Motivation

Results for motivation did not support H2. Thus, the type of framing a SG was not better or worse for participants' motivation according to this study. Therefore H2.1 and H2.2 should also be rejected. Probably, the small sample size, that lacks the power to detect an effect, accounts for this. This result supports the outcomes of Erhel and Jamet (2013) and Hawlitschek and Joeckel (2017), who did not find an effect of condition on motivation either, despite larger samples ($N = 46$ and 150, respectively). This indicates that either there is no effect, or we should design experiments for a smaller effect.

However, there may be other explanations, like the nature of the intrinsic motivation measurements. All related research used self-report scales for intrinsic motivation and could not detect differences. Wouters et al. (2013) suggest two things in this respect. First, that there might be better methods than questionnaires to measure motivation related constructs. For instance, measuring engagement through physiological parameters like skin conductance or eye movements. Secondly, that the timing of the motivation measurement afterwards, instead of during gameplay, may lead to the attenuation of self-reported scores. Hence, the method could explain why many studies, including ours, do not find differences between conditions.

A last reason might be that framing and goal setting interfere with one's natural *achievement goal orientation* (Wolters, 2004). Students with a *mastery goal orientation* like the learning process itself (not its outcome) and invest effort in acquiring new knowledge, whereas students with a *performance goal orientation* like to be seen by others as competent, fear failure and strive for success and high outcomes (Ames & Archer, 1988; Senko, Hulleman, & Harackiewicz, 2011). If participants with a particular achievement goal orientation are placed in an opposite-goal condition, what will happen to their intrinsic motivation? For example, will motivation outcomes within a mastery frame change

depending on whether a student likes learning as an activity (mastery orientation) or wants to outperform others (performance orientation)? In the former case, the frame matches their own beliefs, whereas in the latter case the frame conflicts with their natural tendency. This implies that achievement goal orientation might moderate the relationship between framing condition and motivation. If achievement goal orientation leads to opposite effects within conditions, it might have contributed to the fact that we did not obtain significant results for the group comparisons.

Learning

The results for learning gain support H1, meaning that the type of framing influenced learning outcomes. Although ANOVA showed an effect of framing condition, posthoc tests failed to detect differences between conditions, leaving H1.1, H1.2 and H1.3 unanswered. An explanation for this can be that the total sample size was large enough with sufficient power to demonstrate the presence of an omnibus effect with ANOVA, meaning that at least one condition deviates from the rest. Yet, separate conditions contained too few cases to find significant differences in pairwise comparisons. The necessary corrections to probabilities for type I errors in pairwise comparisons are usually conservative, and a correction for unequal group size could also play a role (XLSTAT by Addinsoft, 2019). Thus, the results with our sample size were likely on the edge of significance.

Nevertheless, Figure 5 indicates the higher and lower scoring conditions for learning. Although not on significance level, the goal-free condition PF and mastery condition LA seem equally well suited for achieving higher learning gains. Both differ visually from the in-game performance condition PP with the lowest score and the other performance condition LT with an intermediate score. We will deliberate on the score of the LT frame, followed by comparing the scoring pattern with other authors' findings.

The first remark about this scoring pattern entails the LT score. Given the fact that only 9.1% of the participants in this condition remembered their instruction (see Table 6), we doubt that the manipulation was successful. Nebel et al. (2017) suggest that with an unclear goal, students will adopt other goals. Table 6 shows that other goals were adopted (18.2% LA and 18.2% PF). Additionally, 54% said they forgot their goal and just played as usual, which could count as adopting a playing-to-perform goal. Goal adoption would mean that the intermediate LT score results from a mixture of conditions rather than representing a learning-for-the-test score. We should thus be careful when comparing LT scores to other scores.

However, the question remains why this goal was unclear and why students forgot only this particular instruction to this extent? Unclearness of the goal might be linked to the nature of the test. Sungur (2007) contends that--although tests can function as external motivators for learning--good performance in nonconsequential tests (like here), depends mainly on having a mastery orientation. Possibly, the notion of being tested only resonated with mastery oriented participants, and was meaningless to others. Again, achievement goal orientation appears as a possible moderator. Another reason could be that students choose other strategies and invest less effort in learning for nonconsequential tests than in consequential (graded) tests (Sundre & Kitsantas, 2004). We wonder if less effort investment also implies easier forgetting the instruction during gameplay.

On the other hand, forgetting the instruction does not have to equal unsuccessful manipulation. The approach to learning theory entails that persons choose an approach to learning when *starting* a task, which includes intention, next to learning strategy (Baeten, Kyndt, Struyven, & Dochy, 2010). If participants started with the intention to learn for a test, the manipulation could have been successful, despite forgetting the instruction later or self-

reporting having adhered to another goal. In this case, the score could be considered a learning-for-the-test score.

Secondly, to try to interpret the indicative scoring pattern, we visually compared it to the results of other authors in Figure 6. Then, we see that our nonsignificant scoring pattern appears to explain the opposite findings from previous articles.

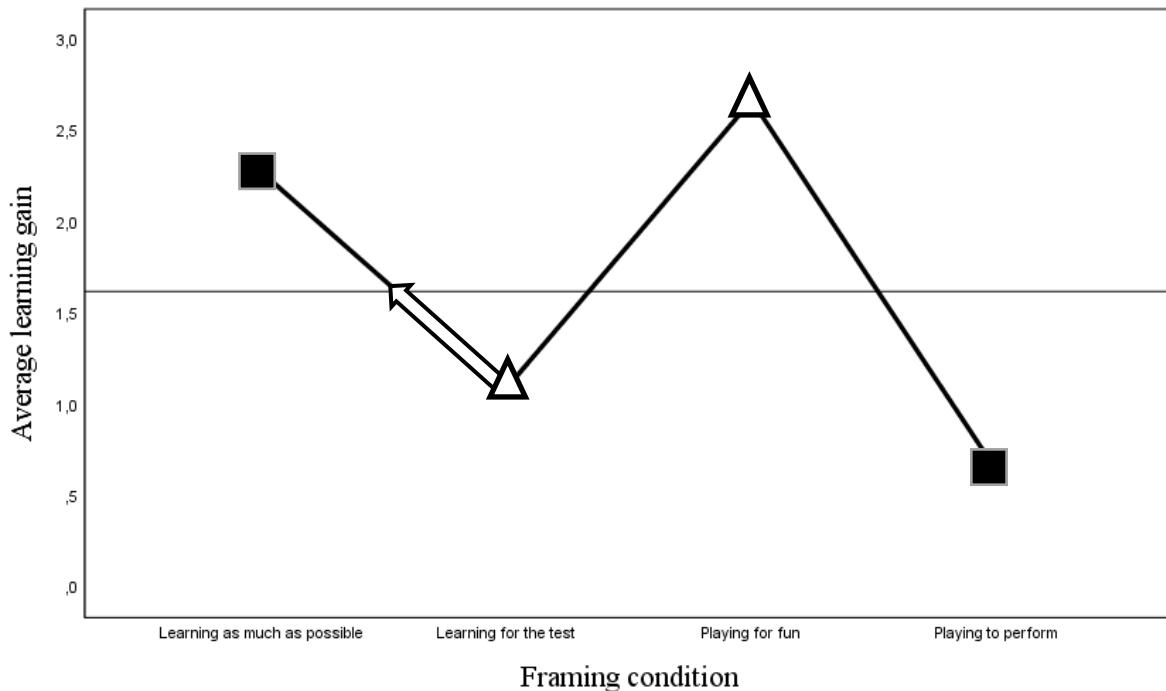


Figure 6. Visual comparison of frames found in different articles. Black squares symbolize conditions used by Erhel and Jamet (2013, 2016), white triangles indicate conditions in Hawlitschek and Joeckel (2017).

Hawlitschek and Joeckel compared a mixed LA/LT instruction with a goal-free condition like PF. Although the left white triangle might be located somewhere on the line between LA/LT because of the mixed frame (indicated by the arrow in Figure 6), of these two frames, PF gave higher results, as seems the case in our study. Erhel and Jamet (2013; 2016) gave their participants elaborate instructions with several goals (like earning points in a learning condition) but emphasized one goal per condition. In both their 2013 and 2016 studies, the stressed goal resembled our LA and PP condition. They concluded that LA or the mastery

condition gives higher learning outcomes than PP or the performance condition. The absolute scores of our study corroborate their findings, marked by black squares in Figure 6, as the learning gain for the LA condition in our study is also highest. Although appearing to be opposites at first glance, this means that both a learning frame and a playing frame can serve learning outcomes, but it depends on the frames compared.

On the other hand, in comparison of results to results from other studies, one should wonder whether differences can be due to other factors outside or inside the game instead of emerging from the frames. From the above comparison it looks like the setting does not matter for the outcome pattern, as results were obtained in a school (Hawlitschek & Joeckel, 2017), a laboratory (Erhel & Jamet, 2013) or a home setting. Although the domains differed, the types of knowledge taught were declarative and procedural in all discussed articles. A crucial factor might be the games' quality. Both the non-game factor of framing and superior or inferior in-game features of other games may cause differences in results. Erhel and Jamet's second study (2013) illustrates this point. Including just-in-time feedback on test questions in the game caused superior comprehension in the playing frame, whereas their first study without additional feedback favored the learning instruction. Moreover, to distinguish between these causes will be especially hard, because an objective standard for judging game quality and effectiveness is lacking (Mayer et al., 2014). So, results obtained with different games by different authors may be hard to compare because in-game and non-game factors interact. Therefore, results might not extend beyond the game used.

Our results and the comparisons to the other studies imply that the type of framing matters for learning outcomes. A theoretical implication that follows from this study is that just contrasting learning with playing is not sufficient, as we can still frame both conditions in various ways. Researchers should be aware of the formulation of instructions when they set up a study as well as when they read and interpret results of others. Moreover, when

promoting learning outcomes in class, teachers should choose the right words when instructing students.

Prior gaming experience as a moderator

As we have not been able to test the effect of PGE on learning gain and motivation gain with the current data set and thus to support or reject hypotheses H3 and H4, this could still be a factor that influences results, at least for learning gain. In both learning conditions, PGE was comparably low, and in both playing conditions, PGE was equally high, making it possible to tentatively compare scores pairwise. So, this indicates that at least children with low PGE possibly achieve more when told to learn as much as possible from a SG, and that frequent gamers appear to do well in a playing-for-fun condition when it comes to learning. A playing-to-perform goal is not preferred for frequent gamers. Supposedly, the PP frame actively reminded participants with high PGE of their usual thing: gaming, thereby activating a cognitive script that does not include learning (Lee & Heetert, 2017). Forgetting to learn does not seem to happen when students' instruction is to play and have fun. How that is possible, especially outside a school setting without mentioning learning, is not yet understood. They might need less working memory to process game elements but use this to process learning content, contrary to the findings of Lee and Heetert (2017). So, possibly a goal-free frame that emphasizes fun could help in overcoming the intuitive learning problem with frequent gamers.

Limitations and recommendations

The first limitation of this study is the small sample size that lacks power. We did not manage to recruit the desired number of participants due to the Corona crisis. An artefact of this small sample is that using the return order of the consent forms for assignment to conditions did not result in the expected randomization of age and PGE. Therefore, results

should be verified by new research with larger samples, in which participant's assignment to conditions takes someone's PGE into account.

Besides this, the home setting was a practical limitation for this study in two ways. First, the researcher was not present to explain tasks, like interpreting the motivation scale, or provide support when stuck in a level because of game features. As schools were closed, a pilot project was impossible. We had to anticipate all difficulties beforehand and include solutions in the instruction booklet. Secondly, at home, environmental conditions and distractions differ per participant, which may have resulted in bias despite all written instructions. To improve reliability of results, future researchers should carry out their projects in schools, where environmental factors vary less across participants, and the understanding of their materials can also be tested.

Not being able to perform a pilot project has also resulted in a low-quality subject knowledge test. The test was custom-made but could not be validated beforehand. Its internal consistency was low with Cronbach's alpha of .48. We should mention that this includes participants that did not study all subjects in time and had to guess answers. According to Van Berkel (2017), tests with alpha below .60 are only suitable as formative, low-stake measurements. Although we applied a formative test, we created it to draw conclusions about learning outcomes and thus a higher-stake purpose. Strikingly, none of the other articles reported on test quality. Hence, subject knowledge test reliability requires more attention in future studies, as its quality might affect outcomes.

A theoretical limitation is that the present study does not consider that for both motivation and learning gain, goal setting and participant's achievement goal orientation might conflict or reinforce each other. To add to already given arguments, Wolters (2004) showed that a mastery goal combined with a mastery orientation, gave the best learning outcomes. Similarly, Seijts, Latham, Tasa and Latham (2004) found a positive correlation

between a learning goal and a mastery orientation for task performance in a digital business game, and a negative correlation for a performance goal with a performance orientation.

Inclusion of this variable as a moderator in future research is therefore strongly advised, as it may lead to other conclusions.

A second theoretical limitation arises from the question what we measured with the IMI interest/enjoyment scale, that was designed to measure self-assessed intrinsic motivation for a certain activity (Center for Self-Determination Theory, 2020). Because of the importance of words when researching the frames' effect, *learning with* the game had to be avoided in the questionnaires, as it could impair the effect of condition. We described the activity unspecifically as *doing the game*, making it hard to define which construct we actually measured. Did children judge their motivation for gaming with CodeCombat, learning or programming with it, or more generally judged the amount of fun? Also, Arnab et al. (2012) argue that students who play a SG outside a school context in their spare time will mainly judge the game's *entertainment value*, which could mean we measured fun or engagement instead of intrinsic motivation to learn with the game. So, future researchers should find a way to unite framing and motivation measurements for learning with games.

As an additional recommendation to the above, interviews with participants could improve our understanding of the factors that play a role with motivation and learning in SGs. When interviewed after gaming, for instance, participants' motives for quitting the research, not finishing within time, or forgetting about instructions can be revealed. Now, the researcher is left puzzled about the incentives. Causes may include the game's subject, features, difficulty or ease of gaming in relation to one's PGE, or the use of English commands. To gain insight in participant's motives and improve future studies on motivation and learning with SGs, it could be valuable to use a mixed-method design.

To be able to find differences for motivation with SGs, we recommend to design studies to fit the construct better, which also entails taking motivation instead of learning as a starting point when setting up a study. This includes anticipating on a small effect size, using technical devices to measure motivation real-time during gameplay, or measuring more than one motivational constructs.

In order to tackle the problem of game comparison, the development of a quality standard, which authors should apply in their publication in order to facilitate comparisons among studies, is advisable. According to Vargas, García-Mundo, Genero, and Piattini (2014) SGs that combine game-playing design and educational rules from the start of the design process would be of high quality. In this respect, games with this so-called *intrinsically integrated* design seem promising candidates for studying the effects of framing in future.

Conclusion

In conclusion, this study showed that the type of framing affects learning outcomes, but not motivation, and that the role of PGE stays unknown. The framing of a SG seems to be a promising addition to the set of in-game possibilities researchers have when trying to improve learning outcomes. This study has also indicated that just comparing instructions to play or learn with SGs is insufficient to explain effects. Different use of words in instructions can direct students towards other goals inside or outside the game. Which instruction we should emphasize needs further clarification. However, future researchers on the topic should carefully formulate their conditions and teachers should keep in mind that the words they use when introducing SGs do influence, at least, their students' learning outcomes.

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Appendices

- A: FETC Form
- B: Power analysis
- C: Information letter
- D: Parental consent form
- E: Instruction mail to parents
- F: Instruction booklet
- G: Dutch IMI questionnaire
- H: Subject knowledge test
- I: Test Matrix

Appendix A: FETC form

Section 1: Basic Study Information

1. Name student:

Marjolijne Luijt

2. Name(s) of the supervisor(s):

Pieter Wouters (p.j.m.wouters@uu.nl)

3. Title of the thesis (plan):

Framing in serious gaming

4. Does the study concern a multi-center project, e.g. a collaboration with other organizations, universities, a GGZ mental health care institution, or a university medical center?

No

5. Where will the study (data collection) be conducted? If this is abroad, please note that you have to be sure of the local ethical codes of conducts and permissions.

In the Netherlands on primary schools

Section 2: Study Details I

6. Will you collect data?

Yes

Yes → Continue to question 11 (?)

No → Continue to question 7

7. Where is the data stored?

The data will be stored in YODA.

Paper questionnaires will be scanned, securely uploaded to YoDa, and the originals will be confidentially destroyed directly afterwards.

8. Is the data publicly available?

No

If yes: Where?

9. Can participants be identified by the student? (e.g., does the data contain (indirectly retrievable) personal information, video, or audio data?)

No, only gender and age will be asked, no names. Participant numbers will be provided that they should use on their forms.

If yes: Explain.

10. If the data is pseudonymized, who has the key to permit re-identification?

N.A.

Section 3: Participants

11. What age group is included in your study?

Primary school children, age 10-12, which was complemented with secondary school children 12-14 years

12. Will be participants that are recruited be > 16 years? No

13. Will participants be mentally competent (wilsbekwam in Dutch)? No

14. Does the participant population contain vulnerable persons?
(e.g., incapacitated, children, mentally challenged, traumatized,
pregnant) Yes

15. If you answered ‘Yes’ to any of the three questions above: Please provide reasons to justify why this particular groups of participant is included in your study.

At this age, children typically start using social media and internet. The skills and knowledge to handle this safely, are being taught with the serious game I will use (Diploma Veilig Internet, target school groups 5-8).

16. What possible risk could participating hold for your participants?

It may generate some stress in one condition, because this involves learning for a test.

17. What measures are implemented to minimize risks (or burden) for the participants?

The children will be debriefed after the experiment that the test was part of the experiment and they will not be actually graded.

18. What time investment and effort will be requested from participants?

Instruction and pre-test questionnaires: 20 minutes, Gameplay: 30-60 minutes , Post-test questionnaires: 20 minutes, Introduction and debriefing: 5 + 5 minutes
Total maximum 1,5-2 hours

19. Will be participants be reimbursed for their efforts? If yes, how? (financial reimbursement, travelling expenses, otherwise). What is the amount? Will this compensation depend on certain conditions, such as the completion of the study?

No.

20. How does the burden on the participants compare to the study's potential scientific or practical contribution?

The burden will not differ much from normal school work. The teacher will loose some school time but in turn the children will learn about a relevant topic, and the results of the study will contribute to the knowledge about how teachers should introduce serious games to promote learning and motivation.

21. What is the number of participants? Provide a power analysis and/or motivation for the number of participants. The current convention is a power of 0.80. If the study deviates from this convention, the FERB would like you to justify why this is necessary.

(Note, you want to include enough participants to be able to answer your research questions adequately, but you do not want to include too many participants and unnecessarily burden participants.)

G*Power generates a number of participants of 73, which makes this at least 19 participants per group ($d = 0.4$, number of groups = 4, moderator = 1, $df = 3$, $\alpha = .05$). Previous research did not provide effect sizes, but learning results could be discriminated with 22 participants per condition. Therefore, 1 school class per condition will be used (20-30 children). See appendix C for output.

22. How will the participants be recruited? Explain and attach the information letter to this document.

Primary school director and class teachers will be asked to participate. If they agree, children's parents will receive an informed consent letter via the teacher/school. Parents from the researcher's social network, received forms directly from the researcher by e-mail, after registration of their child. Or, when schools sent out the information, the forms were included and sent back to the researcher when the child was willing to take part.

23. How much time will prospective participants have to decide as to whether they will indeed participate in the study?

Minimum of 1 week for the parents' informed consent.

24. Please explain the consent procedures. Note, active consent of participants (or their parents) is in principle mandatory. Enclose the consent letters as attachments. You can use the consent forms on Blackboard.

Active informed consent is necessary because participants are too young to decide. Children over 12 years might be present and should also sign the consent form. **All children signed for consent electronically, parents on paper.**
The consent form is included behind the last question.

25. Are the participants fully free to participate and terminate their participation whenever they want and without stating their grounds for doing so? Explain.

Yes for participation. Parents will not be pushed. Children that don't want to participate do not have to, and if they want to quit during the experiment that's okay (data will be destroyed immediately). They will stay with a teacher during the experiment together with children without parental consent.

No, for termination afterwards. If children or their parents want to withdraw from the experiment after data collection took place, this will only be possible if they remember their participant code, because no names or birthdates will be collected. They will be informed about this in the informed consent.

26. Will the participants be in a dependent relationship with the researcher?

No

If yes: Explain.

27. Is there an independent contact person or a general email address of a complaint officer whom the participant can contact?

Independent contact person: a.vanleeuwen@uu.nl

Complaint officer: klachtenfunctionaris-fetcsocwet@uu.nl

28. Is there an independent contact person or a general email address of a complaint officer whom the participant can contact in case of complaints?

klachtenfunctionaris-fetcsocwet@uu.nl

Section 4: Data management

29. Who has access to the data and who will be responsible for managing (access to) the data?

The researcher, the researcher's supervisor, the course coordinator

30. What type of data will you collect or create? Please provide a description of the instruments.

Informed consent forms, see appendix D
Prior game experience questionnaire
Intrinsic motivation questionnaires (pre- and posttest, IMI questionnaire)
Subject knowledge questionnaires (pre- and posttest)
Questionnaires will be collected on paper. => electronically without registration of IP adress

31. Will you be exchanging (personal) data with organizations/research partners outside the UU?

No
If yes: Explain.

32. If so, will a data processing agreement be made up?

N.A.
If yes: Please attach the agreement.
If no: Please explain.

33. Where will the data be stored and for how long?

The raw data will be stored for 10 years on the secured servers of the faculty of behavioural sciences (named YoDa). They will only be exchanged via a secure server connection (not by e-mail).

Paper questionnaires will be scanned and the originals will be confidentially destroyed directly afterwards.

34. Will the data potentially be used for other purposes than the master's thesis? (e.g., publication, reporting back to participants, etc.)

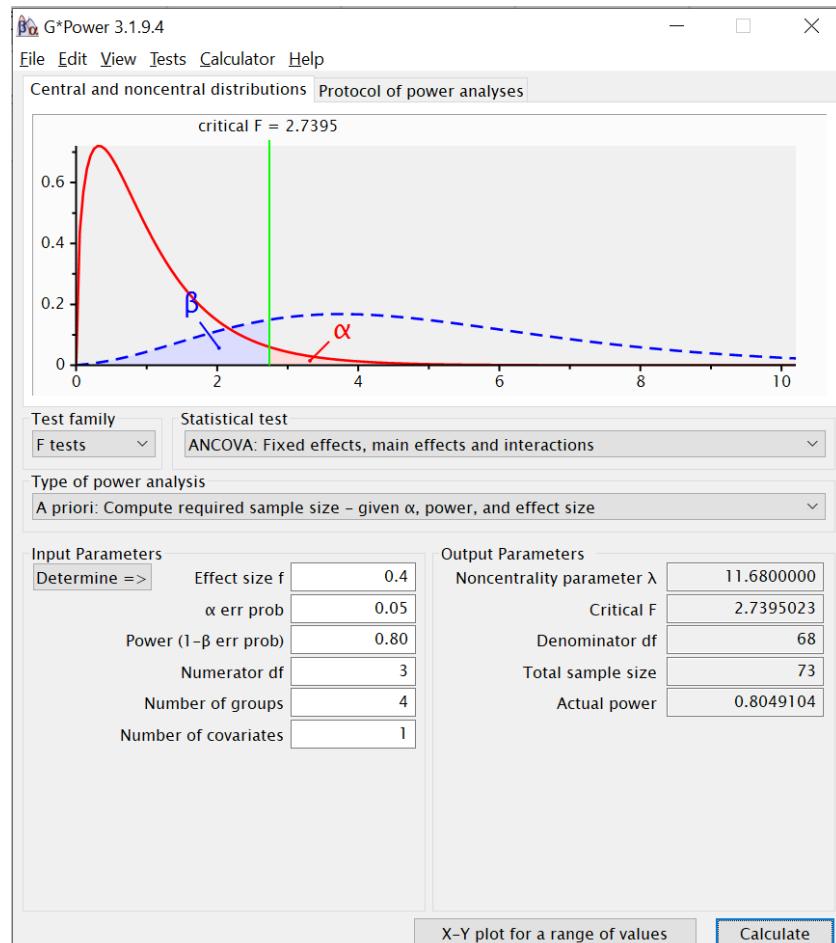
Yes, possibly for publication purposes

35. Will the data potentially be used for other purposes than the master's thesis? (e.g., publication, reporting back to participants, etc.)

See 34.

Appendix B: Power analysis results

Before the experiment, we performed a power analysis with the tool G*Power 3.1 (Faul, Erdfelder, Lang, & Buchner, 2007), with $\beta = .80$ and expecting a large effect size of $f = 0.4$ (Cohen, 1992):



- ⇒ 73 Participants in total mean at least 19 participants per condition
- ⇒ To compare: Erhel and Jamet (2013, 2016) had 22-24 participants in each condition and reported significant differences, indicating that $f = 0.4$ is reasonable.
- ⇒ 1 school class contains usually 25-30 children, so 1 class per condition (taking into account not all parents will give their consent).

Appendix C: Information Letter

Ouderinformatie voor deelname aan (sociaal)-wetenschappelijk onderzoek

Titel onderzoek: Framing in serious gaming (game-based leren)

Den Haag, 8 april 2020

Geachte ouder/verzorger,

Mijn naam is Marjolijne Samwel-Luijt en ik ben masterstudent Onderwijswetenschappen aan de Universiteit Utrecht. Daarnaast ben ik moeder van 3 tieners van 12, 15 en 17. Via deze brief wil ik uw toestemming vragen om uw kind online te laten deelnemen aan mijn afstudeeronderzoek "*Framing in serious gaming: The effects of initial instruction and prior game experience on learning outcomes and motivation*". Serious games zijn computerspellen met leerinhoud. In dit onderzoek gebruik ik een game waarin uw kind de basisbeginselen leert van de programmeertaal Python.

Achtergrond, doel en uitvoering van het onderzoek

Er is al veel bekend over hoe serious games leerresultaten en leermotivatie kunnen beïnvloeden. Leren met games blijkt er in veel gevallen voor te zorgen dat de lesstof beter wordt verwerkt en langer wordt onthouden. Daarnaast vinden kinderen dit meestal een leuke manier van leren in de klas. De meeste onderzoeken bestuderen verschillen tussen leren met games en gewone lessen, of het effect van aanpassingen binnen een game. In de klas speelt de leerkraft echter een belangrijke rol. De wetenschappelijke literatuur is er niet over uit of het beter is om het spelelement of het leerdoel te benadrukken. Daarom onderzoek ik wat een leerkraft uiteindelijk precies zou moeten zeggen ('framing') om de kinderen via de game het meeste te laten leren en te motiveren.

Er zijn daarom vier verschillende introductieteksten voorafgaand aan de game, die random aan de kinderen worden toegekend. Broers en zussen kunnen dus een andere instructie krijgen! Uw kind zal voor en na het gamen een vragenlijst met meerkeuzevragen invullen over het onderwerp (om kennis te meten) en hun leermotivatie. Zo hoop ik te kunnen bepalen van welke introductiewijze kinderen het meeste leren en welke hen het meeste motiveert. Daarnaast vullen kinderen in hoeveel uur per dag ze computerspellen spelen, omdat dit mogelijk invloed kan hebben op de resultaten. Het beantwoorden van de vragen en het zelfstandig spelen van de game zal circa 1,5 uur duren.

Mogelijke voor- en nadelen van deelname aan het onderzoek

Bij het zelf programmeren wordt spelenderwijs aangeleerd om een probleem in kleine stappen uiteen te rafelen. Daarbij leert uw kind een aantal basiscommando's van de programmeertaal Python. Naderhand kan uw kind deze game zelfstandig verder spelen als u uw kind een account laat aanmaken (dit is niet noodzakelijk voor deelname). In één van de instructies wordt de laatste kennisvragenlijst 'toets' genoemd, wat enige zenuwen zou kunnen opwekken. Na afname van het onderzoek wordt echter benadrukt dat de 'toets' geen echte toets was.

Anonimiteit en vertrouwelijkheid verwerking gegevens

In dit onderzoek wordt expres niet naar de naam van uw kind gevraagd. In plaats daarvan krijgt u na het invullen en terugmailtoen van de toestemmingsverklaring een anonieme deelnemerscode voor uw kind. Om de onderzoeksvergiffenis goed te kunnen beantwoorden, worden leeftijd en geslacht van uw

kind (persoonsgegevens) gevraagd. Ik wil benadrukken dat ik met de combinatie deelnemerscode-leeftijd-geslacht (bijvoorbeeld PF005 - 11 jaar - meisje) niet kan achterhalen wie wie is. Ik gebruik de uitkomsten uit de vragenlijsten alleen om de gemiddelde uitkomsten per soort instructie te vergelijken. In (wetenschappelijke) publicaties komen dus geen gegevens van individuele kinderen voor.

De Universiteit Utrecht hanteert strenge normen voor omgang met persoonsgegevens, zoals gescheiden opslag van persoons- en onderzoeksgegevens op verschillende computers. De computer met persoonsgegevens is volgens de hoogste normen beveiligd. Alleen betrokken onderzoekers hebben hier toegang toe en persoonsgegevens worden zo kort mogelijk bewaard. De gegevens zijn ook beveiligd d.m.v. een beveiligingscode. De bewaartermijn van verzamelde onderzoeksgegevens is volgens de daartoe bestemde richtlijnen van de VSNU minimaal 10 jaar. De verzamelde gegevens kunnen mogelijk hergebruikt worden in ander onderzoek. Meer informatie over privacy kunt u lezen op de website van de Autoriteit Persoonsgegevens:

<https://autoriteitpersoonsgegevens.nl/nl/onderwerpen/avg-europese-privacywetgeving>

Vrijwilligheid deelname

Deelname aan dit onderzoek is vrijwillig. Omdat uw kind minderjarig is, dient u als ouder/verzorger officieel toestemming te geven via bijgesloten verklaring. U kunt uw toestemming gedurende de looptijd van het onderzoek zonder opgave van reden intrekken en uw kind kan op elk gewenst moment, zonder reden of nadelige gevolgen, stoppen. Omdat ik na de uiterste deeldatum het bestand met uw e-mailadressen en deelnamecodes wis, kan ik daarna alleen reeds verzamelde gegevens verwijderen als u de deelnemerscode bewaart. Zonder code worden ook na eventuele intrekking van de toestemming de gegevens alsnog gebruikt.

Onafhankelijk contactpersoon en klachtenfunctionaris

Als u vragen of opmerkingen over het onderzoek heeft, kunt u contact opnemen met de onafhankelijke docent/contactpersoon Anouschka van Leeuwen (a.vanleeuwen@uu.nl). Bij een officiële klacht over het onderzoek, kunt u een mail sturen naar de klachtenfunctionaris van de Universiteit Utrecht via klachtenfunctionaris-fetcsocwet@uu.nl.

Als u na het lezen van deze informatiebrief besluit dat uw kind mag deelnemen aan het onderzoek verzoek ik u om een scan of foto van de ingevulde en ondertekende toestemmingsverklaring terug te mailen. U ontvangt dan na Pasen de deelnemerscode. Deelnemen kan t/m vrijdag 1 mei. Deze brief kunt u bewaren voor uw eigen administratie.

Met vriendelijke groeten,

Marjolijne Samwel-Luijt (hoofdonderzoeker, bij vragen: m.luijt@students.uu.nl of 06-27852328)

Pieter Wouters (afstudeerdocent p.j.m.wouters@uu.nl)

Appendix D: Parental consent form

Toestemmingsverklaring:

Graag een digitale kopie (scan, foto o.i.d) van het ingevulde formulier retour via
m.luijt@students.uu.nl.

U ontvangt dan voor elk kind de deelnemerscode en de links naar de vragenlijsten van het onderzoek per e-mail.

Hierbij verklaar ik de informatiebrief m.b.t. het onderzoek "Framing in serious gaming: *The effects of initial instruction and prior game experience on learning outcomes and motivation*" gelezen te hebben en op de hoogte te zijn van het onderzoeksdoel en de omgang met verzamelde gegevens. Ik weet dat ik mijn toestemming weer in kan trekken voordat het onderzoek is uitgevoerd en dat mijn kind vrij is om te stoppen met het onderzoek zonder opgave van redenen.

Ik ga akkoord met deelname aan het onderzoek door mijn kind(eren).

Indien mogelijk graag een handtekening van beide ouders/verzorgers.

Ik geef hiermee _____ kind(eren) op van 10 t/m 14 jaar.

Plaats: _____

Datum: _____ 2020

Naam ouder/verzorger 1: _____

Naam ouder/verzorger 2: _____

Handtekening ouder/verzorger 1

Handtekening ouder/verzorger 2

Appendix E: Parental instruction mail

Beste ouder,

U kunt de volgende deelnemerscode aan uw kind(eren) geven.

Print ook het/de bijgevoegde draaiboek(en) uit:

Kind 1 _____

Kind 2 _____

Kind 3 _____

Met deze code kan hij/zij kan inloggen op de start- en eindvragenlijst in het onderzoek via de links hieronder.

LET OP: de instructies verschillen per kind ! Als meer dan 1 kind meedoet, geef dan aan elk kind een matchende deelnemerscode/draaiboek combinatie: deze beginnen dan allebei met **dezelfde 2 letters**. Welk kind welke code krijgt, maakt niet uit.

Zorg dat de benodigde tijd van 1,5 uur beschikbaar is, omdat het voor mijn onderzoek belangrijk is om achter elkaar door te gaan.

Afhankelijk van de leeftijd, zelfstandigheid en digitale vaardigheden van uw kind kan het zijn dat u enige hulp bij de structuur (juiste volgorde en opstarten van vragenlijst en game) zou moeten verlenen. Help uw kind echter niet met de inhoud van de vragenlijsten of de game! Dat beïnvloedt namelijk de resultaten van het onderzoek.

De volgorde van het onderzoek staat als afstreeplijst op pagina 3 van het draaiboek. In principe heeft uw kind alleen de link naar de Startvragenlijst nodig, want daarin staan aan het einde de andere 2 links genoemd. Voor de zekerheid staan ze hier alle drie genoemd:

- Startvragenlijst : <https://survey1.fss.uu.nl/index.php/682338?lang=nl>
- Game CodeCombat (open deze in nieuw venster): <https://codecombat.nl/spel>
 - Sluit het browservenster van de game niet voordat het kind klaar is; de voortgang gaat anders verloren!
 - NB de game wordt **zonder account** gespeeld, zodat ieder kind hetzelfde kan in de game (met account heeft extra tussenlevels). Na level 17 kan uw kind als hij/zij zelfstandig verder wil kunnen – met uw toestemming – alsnog een account aanmaken zodat zijn/haar voortgang wordt opgeslagen. Dit laat ik aan u.
- Eindvragenlijst: <https://survey1.fss.uu.nl/index.php/534631?lang=nl>

Stuur deze mail door aan uw kind, zodat hij/zij de links naar de vragenlijsten kan aanklikken. Meedoen kan t/m vrijdag 1 mei.

Met vriendelijke groet,

Marjolijne Samwel-Luijt

(Bij problemen te bereiken onder: 06-27852328)

Appendix F: Instruction Booklet for participating children

Each child received an instruction booklet dedicated to their condition. So, four versions of the booklet were made, 1 for every condition.

However, for convenience reasons, the instruction booklet shown in this appendix contains **all 4** instructions

Draaiboek voor deelnemers

Voor deelnemerscodes startend met de letters: **[afkorting conditie hier]**

Hallo,

Welkom bij mijn afstudeeronderzoek over leren met games.

Dit draaiboek hoort bij deelnemerscodes die beginnen met de letters [code van de framing condition stond hier].

Check of dat klopt met de code die je hebt gekregen!

Klopt dat niet? Kijk of je broer/zus per ongeluk jouw instructie heeft gehad.

Klopt het nog niet of heb je geen deelnemende broer/zus, stuur me dan een mailtje op m.luijt@students.uu.nl of bel 06-27852328.

1. Met het onderzoek ben je ongeveer 1,5 tot 2 uur bezig. Zorg dus ook dat je de tijd hebt als je er aan begint, want het is wel de bedoeling dat je alles achter elkaar uitvoert. Natuurlijk kun je tussendoor wel wat drinken pakken of naar de wc. Maar zorg er dan voor dat je de webpagina waarin je bezig bent niet afsluit.
2. De volgorde van het onderzoek is heel belangrijk. Daarom staat op de volgende bladzijde een formulier waarop je kunt afvinken en noteren wat je al hebt gedaan. Zorg dus voor een pen of potlood.
3. Vergeet vooral niet je **instructie** te lezen voor je aan de game begint. Dat is namelijk waar mijn onderzoek om draait.
4. Het invullen van de vragenlijsten duurt ongeveer 15-20 minuten per lijst. Met de knop rechtsboven kun je zo nodig de vragenlijsten opslaan om later verder te gaan via dezelfde link. Doe dit liever niet, zie punt 1.
5. De game die je straks gaat doen, heeft 42 levels. Dat zou veel te lang duren.... Voor het onderzoek speel je daarom t/m **level 17** (deze heet: "Alle namen mogelijk" en deze vind je rechtsboven in het gameoverzicht). Ik denk dat dat in ongeveer een uur (misschien ietsje langer) moet lukken.
Omdat de game niet zelf een seintje geeft na level 17, vraag ik je daarom om ook een timer te zetten op 60 minuten. Zorg dus dat je ook je telefoon of een kookwekker bij de hand hebt.
6. Op de laatste bladzijden vind je ook uitleg over het spel. De uitleg in de game zelf is in het Nederlands, maar de programmeercommando's zijn namelijk in het Engels.

Draaiboek invulformulier Onderzoek “Framing in serious gaming”

Stap	Instructie	Gedaan/ antwoord
1	Zorg dat je 2 uur de tijd hebt en niet afgeleid wordt. Schrijf je deelnemerscode hiernaast op, die heb je straks 2x nodig.	
2	Ga op een <u>laptop of computer</u> naar de Startvragenlijst en vul die in. Het handigst gaat dat via de link in de e-mail. Het webadres is anders: https://survey1.fss.uu.nl/index.php/682338?lang=nl <i>Vul alle vragen naar waarheid in. Je kunt geen vragen overslaan.</i>	
3	Lees jouw INSTRUCTIE op pagina 5 goed voordat je start met de game.	
4	Start de game CodeCombat IN EEN NIEUW VENSTER door de link in de Startvragenlijst te kopiëren of daar codecombat.nl/spel in te typen. Klik dan middenin het venster op <i>Speel het spel</i> . Dit stuurt je door naar de Nederlandse versie. Laat de Startvragenlijst open staan, je komt daar straks terug.	
5	Bekijk de pagina's 6 en 7 hierachter en zoek op de screenshots: <ul style="list-style-type: none">• het speaker-symbooltje om het geluid uit te doen. Gebruik een koptelefoon als je geluid wilt.• de boodschap over de schoolversie. Deze klik je weg bij het kruisje• level 17 op screenshot 2	
6	Zet de timer op 60 minuten als geheugensteuntje.	
7	Klik bij “De kerkers van Kithgard” op SPELEN (links in je scherm). <ul style="list-style-type: none">• Vind eerst level <i>Alle namen mogelijk</i> (level 17) rechtsboven door je muis over de levels te bewegen. Onthoud waar dit is.• Als je binnen de 60 minuten met dit level klaar bent, mag je meteen door naar punt 8.	
8	<u>Begin de game en volg steeds de gele pijl.</u> Kom je er niet uit, of heb je tips nodig? Probeer “Hints” rechtsboven in het scherm of kijk op pagina 7) <i>LET OP: Sluit het gamevenster niet af voordat je klaar bent, anders moet je opnieuw beginnen!</i>	
9	Je timer gaat af!! Check het lijstje met levelnamen voor het levelnummer (zie pagina 4) en schrijf dat hier op. Nu zijn er meerdere mogelijkheden: <ol style="list-style-type: none">1. Je bent al voorbij level 17 => zie punt 102. Je bent bij level 17. Maak deze nog af en ga dan naar punt 10.3. Je bent nog niet bij level 17 => speel nog 15 minuten extra (zet weer de timer). Noteer hier waar je daarna gebleven bent, en ga door met punt 10.4. Je bent nog niet klaar, maar vindt het écht te moeilijk => Noteer hier waar je gebleven bent. Wil je wel nog de eindvragenlijst invullen (punt 10)?	
10	Ga terug naar het venster van de startvragenlijst. Klik op de onderste link en vul de vragen van de Eindvragenlijst zo goed mogelijk in. Het webadres is: https://survey1.fss.uu.nl/index.php/534631?lang=nl	

Lijst met levels en namen

level	naam
1	De kerkers van Kithgard
2	Schat in het diepe
3	Schaduwwachter
4	Vijandelijke mijne
5	Ware namen
6	Cel commentaar
7	Kithgardse bibliothecaresse
8	De gevangene
9	Vuurdansen
10	Spookachtige kithdoolhof
11	Verder afdalen
12	Gevreesde deur
13	Hak en ontsnap
14	Kasten van kithgard
15	Bekende vijand
16	Meester der namen
17	Alle namen mogelijk

Jouw instructie

Lees jouw instructie aandachtig door en probeer te onthouden wat je moet doen.

Houd je tijdens de game zo goed mogelijk aan de instructie.

LA

Je gaat de game CodeCombat doen om de Python programmeertaal te leren.

Leer zo veel mogelijk Python code!

Or: LT

Je gaat de game CodeCombat doen om de Python programmeertaal te leren.

Naderhand krijg je hierover een toets!

Or: PF

Je gaat de game CodeCombat spelen.

Veel plezier daarbij!

Or: PP

Je gaat de game CodeCombat spelen.

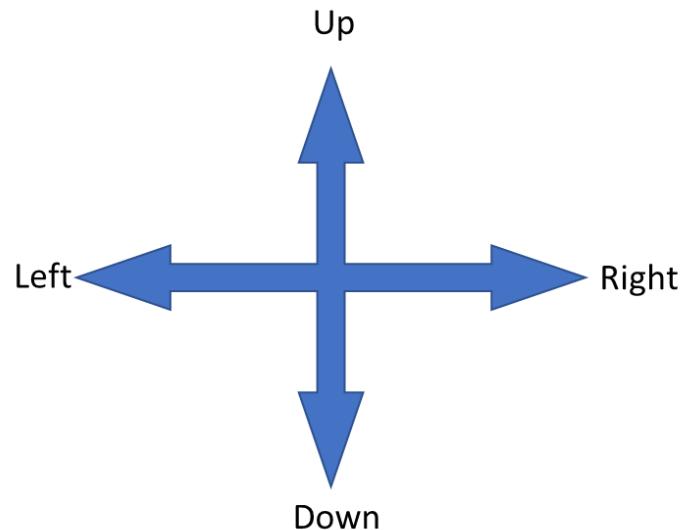
Zorg ervoor dat je zoveel mogelijk XP verdient door de Ogres en Munchkins te verslaan en de edelstenen te pakken!



De wereldbol leidt je terug naar het beginscherm (1^e plaatje). Klik je hier per ongeluk op, klik dan weer op SPELEN. Je voortgang is nog bewaard!

Tips bij het gamen:

- Je kunt je held veranderen bij “Verander held of taal”. Dit kan ook halverwege de game. Alleen de eerste 4 helden zijn beschikbaar in deze gratis versie.
- Verander NIET de taal. Laat deze staan op PYTHON.
- Kijk goed naar de pijl en de aanwijzingen. Als je die volgt, gaat alles lukken.
- Het beginscherm van elk level bevat aanwijzingen over de te programmeren route.
- Je kunt de kaart van het level (links op je scherm) ook vergroten (en soms verschuiven) om te zien waar je heen moet.
- Je kunt in het codeervak (de ‘editor’, rechts op de scherm in een level) rechtsboven op HINTS klikken als je aanwijzingen wilt om het level te halen.
- Als je een letter typt in de editor, komt er een lijst met commando’s tevoorschijn. Je hoeft dus niet per se alles zelf in te typen.
- Onder ‘Doelen’ vind je de opdrachten voor het level
- Middenin je beeldscherm onder het kopje “methodes” staan de commando’s die je kunt gebruiken.
- De game was oorspronkelijk in het Engels. Af en toe staat er per ongeluk nog een Engelse aanwijzing. Stoor je daar niet aan, het maakt niet uit.
- De commando’s in Python zijn Engelstalig. Spreek je nog geen Engels, dan is hier de vertaling:
 - Hero = held
 - Move = verplaats, beweeg
 - Right = rechts
 - Left = links
 - Up = omhoog
 - Down = omlaag
 - Attack = val aan
 - Say = zeg
 - While True = zolang dit waar is



Hints per level:

- Voor alle levels: Je kunt van je verzamelde edelstenen een betere uitrusting kopen voor je held (Dit is soms nodig om een level te halen en niet te blijven doodgaan!).
- level 8: “De gevangene”
 - Hint 1: Gebruik je wel het juiste commando om de deur te openen?
 - Hint 2: je hoeft niet alles alleen te doen, de bevrijde gevangene is er voor hulp! Of: deze Ogre is héél erg sterk, sterker dan andere Ogres.
- Level 16 “Meester der namen”
 - # **Wijst het resultaat van hero.findNearestEnemy() toe aan de variabele enemy1:** Betekent: Vertel je hero dat hij de dichtstbijzijnde vijand moet vinden, en wie dat is (enemy1), dus je schrijft op:
 - enemy1 = hero.findNearestEnemy()
 - Val dan aan door de variabele te gebruiken, en herhaal dat voor enemy2 en enemy3 (zie HINTS pagina 4 bij dit level).

Appendix G: Dutch IMI questionnaire

Er komen nu een aantal uitspraken. Geef bij iedere uitspraak aan in hoeverre deze bij jou past. De betekenissen zijn: 1 = past helemaal niet bij mij 4 = past een beetje bij mij 7 = past helemaal bij mij. Ook de getallen ertussen mag je gebruiken. Hoe hoger, je score, hoe meer je dus vindt dat het bij jou past.

Voorbeeld: *Ik vind spruitjes lekker.*

Als je niks viezer vindt dan spruitjes (zoals ik), kies dan een 1.

Als je spruitjes niet lekker vindt, maar wel eet, kies je een 2 of een 3.

Als je spruitjes niet vies en niet lekker ("neutraal") vindt, kies dan 4.

Als je het best lekker vindt, kies je 5 of 6.

Als je het heel lekker vindt, is het 7.

Instructie pretest:

Bij al deze vragen moet je denken aan de game die je zometeen gaat doen. Daarom klinken sommige vragen nu misschien best gek. Probeer je er iets bij voor te stellen en kies dan een antwoord dat zou kunnen kloppen.

Instructie posttest:

Geef bij iedere uitspraak hieronder aan in hoeverre deze bij jou past. Al deze vragen gaan over de game die je zojuist hebt gedaan.

Geef bij iedere uitspraak hieronder aan in hoeverre deze bij jou past.

1	Deze game is leuk om te doen
2(R)	Ik vind het doen van deze game vervelend
3	Tijdens het doen van de game bedacht ik me hoe leuk ik het vind
4	Ik zou het doen van deze game omschrijven als erg interessant
5(R)	Ik vind het doen van deze game saai
6(R)	Ik kan mijn aandacht niet bij de game houden
7	Ik vind het doen van deze game plezierig

(R) Antwoord moet omgekeerd worden in de analyse

Appendix H: Subject knowledge questionnaire

Note: Correct answers are displayed in green

Vraag 1 [herkennen/reproduceren argument]

Hoe noem je in het commando hero.moveUp(2), het deel dat tussen de haakjes staat?

- a) String
- b) Argument
- c) Comment

Vraag 2 [herkennen/reproduceren betekenis comment]

Wat is een omschrijving van een *comment*?

- a) Een manier om een heel blok code oneindig te herhalen
- b) Tekst die je toevoegt, maar niet bij de code hoort
- c) Een manier om de code korter op te schrijven

Vraag 3 [begrijpen while loop]

Alle code die je NA een while-true loop schrijft,

- a) Wordt herhaald door de loop
- b) Zorgt ervoor dat de loop niet werkt
- c) Wordt nooit uitgevoerd

Vraag 4 [toepassen while loop]

Hoe kan je deze code:

```
hero.moveLeft( )  
hero.moveUp( )  
hero.moveLeft( )  
hero.moveUp( )
```

oneindig herhalen?

- a) Dat kan niet
- b) hero.moveLeft(1000)
hero.moveUp(1000)
- c) while True:
 hero.moveLeft()
 hero.moveUp()

Vraag 5 [toepassen string]

In welk antwoord is de string juist geschreven in Python-code?

- a) hero.attack(Brak)
- b) hero.attack("Brak")
- c) hero.attack("brak")

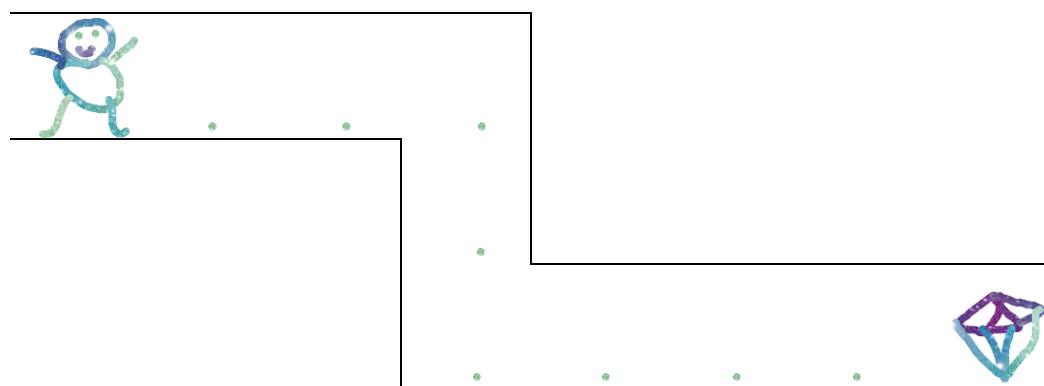
Vraag 6 [herkennen variabele (verschillen)]

Bij welke code hieronder staat er een variabele tussen de haakjes?

- a) hero.say("Open de deur!")
- b) hero.findNearestEnemy(vijand1)
- c) hero.moveLeft(3)

Vraag 7 [toepassen basis syntax]

De hero (het poppetje) wil de diamant pakken, zie plaatje. Met welk stuk code lukt dat?



- a) hero.moveRight(3)
hero.moveDown(2)
hero.moveRight(4)
- b) hero.moveRight(7)
hero.moveDown(2)
- c) hero.moveLeft(4)
hero.moveUp(2)
hero.say("Diamant")

Vraag 8 [begrijpen string/ toepassen basis syntax en while loop]

```
# Ga 2 passen naar links
# Versla de vijand die "Brum" heet
```

Met welke code werkt bovenstaande opdracht?

- a) while True:
 hero.moveLeft(2)
 hero.attack(2)

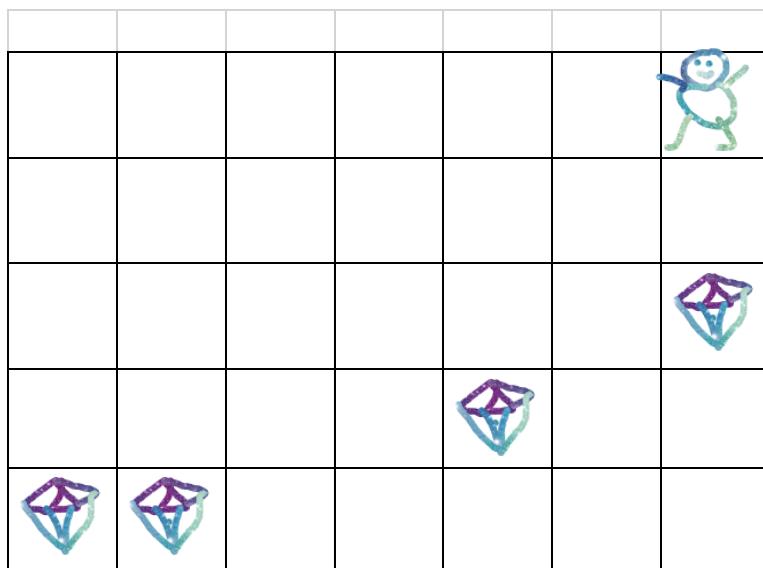
- b) hero.moveLeft("Brum")
 hero.moveLeft("Brum")

- c) hero.moveLeft(2)
 while True:
 hero.attack("Brum")

Vraag 9 [toepassen basis syntax + while loops]

De hero wil alle diamanten pakken in zo min mogelijk coderegels.

Wat is daarvoor de beste code?



- a) hero.moveDown()
 while True:
 hero.moveDown()
 hero.moveLeft(2)

- b) hero.moveDown(4)
 hero.moveLeft(6)

- c) hero.moveDown(2)
 hero.moveDown()
 hero.moveLeft(2)
 hero.moveDown()
 hero.moveLeft(4)

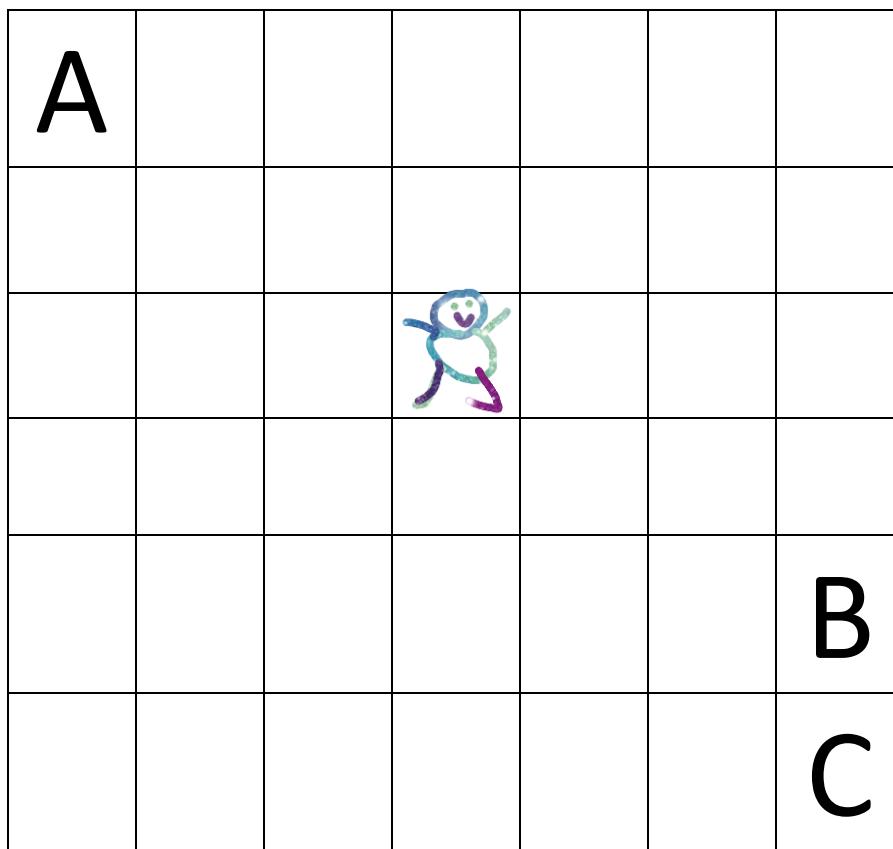
Vraag 10 [begrijpen variabele]

Als je het volgende codeert om Brak aan te vallen:

```
hero.attack(vijand1)
hero.attack(vijand1)
```

wat ontbreekt er dan nog aan de code?

- a) Er ontbreekt niks, want Brak is de vijand
- b) # deze vijand heeft de naam "Brak"
- c) vijand1 = "Brak"

Vraag 11 [toepassen basis syntax en while loops]

Als je deze code volgt, bij welke letter loopt het poppetje (de hero) dan het rooster uit?

```
hero.moveLeft(3)
hero.moveUp(2)
while True:
    hero.moveRight(2)
    hero.moveDown(2)
```

- a) Bij punt A
- b) Bij punt B
- c) Bij punt C

N.B. Antwoord 11c matcht met antwoordoptie A2 in Limesurvey, antwoord b is antwoord A3 in Limesurvey! Ze werden wel in dezelfde volgorde als hier getoond in de antwoordlijst (dus A1, A3, A2)

Vraag 12 [begrijpen while loops]

Waar is de hero na 4 loops als je het volgende codeert?

while True:

```
hero.moveDown(3)  
hero.moveUp(2)
```

- a) De hero is weer op de startplek, want hij komt met een loop niet vooruit
- b) De hero is 4 plekken lager dan waar hij was voor de loop startte
- c) De hero liep op het scherm op en neer en eindigt 4 plekken hoger

Appendix I: Test matrix

level	naam	onderwerpen	nieuwe inhoud	toets
1	De kerkers van Kithgard	in dit level leer je hoe je je held moet bewegen door code te schrijven. Je held zal het lezen en de instructies volgen. Spreek hem aan als hero.	code editor uitvoeren/run basis syntax: moveDown(), moveRight(), moveUp(), moveLeft()	basis syntax gebruiken
2	Schat in het diepe	verzamel snel alle edelstenen, Meerdere keren bewegen is de actie 2x opschrijven of gebruik argumenten in combinatie met beweeg commando's	code 2x opschrijven of argument gebruiken	argument herkennen en gebruiken
3	Schaduwwachter		geen nieuwe	argument herkennen en gebruiken
4	Vijandelijke mijns	gebruik argumenten in combinatie met beweeg commando's	strings attack("Naam")	string toepassen
5	ware namen	Gebruik de attack methode om een vijand aan te vallen bij zijn naam ("Name").	strings i.c.m. basis syntax	string toepassen
6	Cel commentaar	comments lezen om het wachtwoord te vinden	strings comments (#) say	comment herkennen, string toepass
7	Kithgardse bibliothecare	hints gebruiken om achter het wachtwoord te komen	basis syntax en strings	string toepassen, game hints lezen
8	de gevangene	bevrijd de gevangene, hij kan je helpen de ogre te verslaan (je bent misschien niet sterk genoeg om de ogre meteen te verslaan)	strings en basis syntax en argumenten	string toepassen
9	vuurdansen		while loops introductie	while-true loop
10	spookachtige kithdoolhol	patronen herkennen voor gebruik while	while loops (zelf maken)	
11	verder afdalen		while loops met argumenten	
12	gevreesde deur		while loop met andere dan basis syntax	
13	hak en ontsnap		while loop met code ervoor combineren	while true loop begrijpen
14	kasten van kithgard	je kunt iedere actie gebruiken voorafgaand aan de while true loop	while loop met code ervoor combineren	
15	bekende vijand		variabele	
16	meester der namen	variabele gebruiken ipv string bij findNearestEnemy	variabele findNearestEnemy()	
17	alle namen mogelijk	flesjes die je een naam moet geven;	variabele	

level	Bloom ww	niveau	Performance objective	in toets
				-
1	toepassen	3	past de basis syntax toe	
2	onthouden	2	weet wat een argument is	1, (2c)
3	toepassen	3	kan argumenten gebruiken in basis syntax	7, (9 b/c)
4	onthouden	1	herkent een string	(1a)
	toepassen	3	kan verschillende commando's combineren om tot een resultaat te komen	7
5	toepassen	3	kan in commando's een string juist toepassen	5
6	onthouden	1	herkent een comment	(1c), 2
7	toepassen	3	<i>kan in commando's een string juist toepassen (herhaling)</i>	
8	toepassen	3	<i>kan basis syntax met argumenten gebruiken (herhaling) kan in commando's een string juist toepassen (herhaling)</i>	
9	onthouden	1	kent een while true loop	(2a)
	begrijpen	2	begrijpt hoe een while-true loop werkt	12
10	toepassen	3	kan een while-true loop toepassen	4, 11
11	toepassen	3	kan basis syntax met argumenten in een while-true loop gebruiken	(9a), 11
12	toepassen	3	kan basis syntax met argumenten en andere commando's in een while-true loop gebruiken	8
13	toepassen	3	kan while true loops in de juiste volgorde met andere commando's combineren	8
14	begrijpen	2	begrijpt waarom je geen commando's achter while true loops kan zetten maar wel daarvoor	3, (8a)
15	onthouden	1	herkent een variabele	6
16	begrijpen	2	begrijpt het verschil tussen een variabele en een string	(5)
17	begrijpen	2	begrijpt dat je een variabele moet definieren	10