

The Effects of Surprising Events on Promoting Social Change in Unwinnable Persuasive Games

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Abstract

Surprising events can be beneficial for unwinnable persuasive games, especially since they can evoke players to reflect on their failure to win the game. Despite its presence in some titles, the usage of surprising events still lacks empirical support. This study aims to gain insight into it by comparing the effects of revealing the game's context from the beginning to delaying it until the game ends. In addition, we also examine the interaction effects with playing duration since it is possible that longer playtime will lead to smaller effects for a game with surprising events, whereas longer playtime will result in greater effects for a game without surprising events. To do so, we conducted a 2 x 2 factorial between-subject experiment with an additional no-treatment control group. The results suggest that delaying the revelation to create a surprising event can promote the same level of donation from players, regardless of their playing time. On the other hand, longer playtime is important if players know the context from the beginning. Additional results about the effect of playing duration on donation and willingness to help were also discussed in this paper.

Keywords: Game loss, serious games, games for change, empirical study, playing duration, donation;

1 Introduction

For organizations that primarily advocate humanitarian or environmental issues, one of the main challenges is to change individuals' attitudes or behaviors to support a specific cause or, at least, to raise their awareness. The emergence of persuasive games, special types of serious games that are designed with the intention to change or reinforce certain attitudes of its player [1], introduces new possibilities to support these organizations in achieving their goal. In some cases, persuasive games are deliberately designed to be unwinnable by forcing the players to lose or providing no satisfactory ending [2], [3]. We name this particular type of game that expects the players to give up to get the message across as unwinnable persuasive games.

For instance, in "My Cotton Picking Life", players must pick cotton to meet one day's quota of 50 kg. However, with around 2 grams of cotton picked per button-click, it will take approximately 6 hours to reach the quota. Since the activity is tedious and the game offers no incentives to keep playing, players might quit the game after a few minutes. Interestingly, throughout the play, the game features a "give up" button that practically dissuades players from continuing. By clicking on that button, the game will reprimand the players that not everyone has the option to quit work. Aside from "My Cotton Picking Life", numerous titles have utilized this concept to promote various causes, like



“September 12th”, which focuses on terrorism and “Spent”, which raises awareness about poverty.

It might be counterintuitive to force players to lose a game, especially since one of the enjoyments of playing games comes from overcoming the challenge [4]. However, there are several reasons why losing a game can produce desirable results. If the purpose of the persuasive game is to raise awareness or solicit players to offer some form of help, then winning might give the wrong impression. Instead of helping, players might believe that it is possible for the individuals depicted in the game to overcome the problem with their own strength. Instead, the loss can reinforce the characters’ harsh circumstances to the players [5] or encourage them to reflect on their failure [6]. In addition, Benford et al. [7] stated that a product with carefully and ethically managed uncomfortable interaction can be used for enlightenment. Similarly, Marsh and Costello [8] emphasized the importance of lingering uncomfortable or negative experiences after playing to provoke reflection or encourage behavior changes. Furthermore, presenting the loss from different perspectives or connecting it to a universal human theme can reframe the experience into a more meaningful one [9]. In short, by making the game unwinnable, the game designers aim to show the “dark” or “uncomfortable” sides of an issue to encourage the players to act and prevent it from becoming a reality.

While unwinnable persuasive games were developed with good intentions, it is essential to establish their effectiveness. Validating persuasive games’ effectiveness has been one of the main questions within the research community for the past decade [10], [11]. Initially, scholars followed a black box approach that compares games with other persuasive media, but then they shifted their attention to the role of specific game features [12]. Nowadays, many scholars [12]–[15] have supported the latter because the results can be generalizable to other persuasive games and guide designers to create effective ones. For example, Gerling et al. [16] studied the effects of embodied interaction and found that playing with a special wheelchair-based controller can produce a more sustainable attitude change toward wheelchair users than playing with a traditional controller. In addition, Jacobs, Jansz, and Kneer [17] investigated whether narrative and proceduralism affected players differently and concluded that both persuasion strategies could produce similar results when properly implemented. Furthermore, Lin and Wu [18] explored the effects of graphic realism and found that a game with a more realistic style can evoke greater appreciation from the players than a game with a cartoonish style, which in turn leads to a greater intention to forward the game to others.

Similarly, in unwinnable persuasive games, the main question is whether forcing the players to lose is actually beneficial. However, despite its usage in several titles, the effectiveness of unwinnable persuasive games still lacks empirical support. Neys and Jansz [19] investigated the effects of several political games, including “September 12th”, and found that playing the game can lead to individual and social facilitation. Although one of the games used in the study was an unwinnable persuasive game, the role of game loss on the overall persuasion process was never the focal point. The only study that specifically covers the effect of game loss in persuasive games was conducted by Steinemann et al. [20]. Even then, they only compared the different effects of winning and losing a persuasive game. Since losing is mainly associated with negative experiences like frustration or even rage quit [21], thus these experiences might distract the players from the game’s core message. Given these points, it is inadequate to only know the differences between winning and losing a persuasive game. Because of that, our research focuses on investigating how game loss can be designed to produce positive impacts in promoting social change. Mainly for this paper, we focus on the effect of surprising events.

Surprise is usually associated with individuals’ emotional reactions toward an unexpected event. A surprising event can be created by omitting critical information at the beginning of an event and revealing it later [22]. Once the individuals reach the point where the hidden information is revealed, a surprise reaction can occur. This reaction results from a breakdown of their current mental representation of the event that urges them to integrate the new information so they can reinterpret the whole event and make



sense of it [23]. Surprise can arouse individuals' attention when something fails their expectations and gives them reasons to recall the preceding events to correct or better understand it [24], [25]. The tendency for a surprising event to encourage individuals to re-evaluate their experience can be beneficial to evoke reflection, which is an important factor in facilitating change [26]. Given these points, surprising events can be valuable in unwinnable persuasive games that require players to reflect on their failure to grasp the core message.

The presence of surprising events in unwinnable persuasive games is not entirely novel. For instance, "Spent" challenges players to survive one month with only 1,000 \$ as they are forced to make difficult choices on how to spend the money wisely for essential needs. Interestingly, even if the players survive the month, the game introduces a surprising event by informing them that they will have to pay the rent for the next month. At this point of the game, whatever choice they made beforehand, they will never have enough money. Similarly, "September 12th" challenges players to kill terrorists among civilians by launching a missile at them. While it seems like a simple game of picking the right target, because the terrorists are always standing in a place that is crowded with civilians, it is nearly impossible to only kill the terrorists. What makes the game compelling is that for every civilian's death, other civilians will mourn over them and then turn into new terrorists, providing a surprising event to the players. Finally, "My Cotton Picking Life" gives players the mission to collect 50 kg of cotton using a simple two-button clicking mechanic. However, the game never tells the players that they can only collect around 2 grams per click. They will then realize that it will take hours to fulfill the quota.

Surprisingly, despite its usage in unwinnable persuasive games, the effects of surprising events on the overall effectiveness of the persuasion process still lack empirical support. Therefore, we conducted an experiment to compare the effectiveness of unwinnable persuasive games with a surprising event to another without it. For our experiment, unlike the aforementioned three games, we hid the context until the end of the game to create a surprising event. To put it another way, we never briefed the players about the persuasive intent of the game and just presented it like any other commercial game.

"Spent", "September 12th", and "My Cotton Picking Life" similarly open up with an explanation of the premise of the game. The briefings set the tone of the gameplay and let the players know beforehand that the game is not "just a game". As a result, players might already have an idea that the game is intended to persuade them. Even though they lose the game, they know what to expect and can appreciate the failure. Furthermore, Jacobs [1] showed in his study that having prior knowledge of the game's persuasive intent can be beneficial for the persuasion process.

On the other hand, hiding the context of the game (e.g., the game's purpose, the narrative's backstory, or the character's identity) can have its benefits. The study by Bachen et al. [27] suggested that it is more difficult to empathize with individuals who are perceived as psychologically distant. However, Kaufman and Libby [28] found from two different experiments that delaying the revelation of a character's identity in an experience-taking process can reduce the boundary for it to occur. Based on that, we can infer that hiding the context of the game can reduce the psychological distance. Similarly, Kaufman, Flanagan, and Seidman [29] argued that an explicit persuasion approach could trigger psychological reactance, making individuals more resistant to the message. Instead, one possible approach they proposed is to deliberately conceal the persuasive intent of the game from the beginning. Furthermore, the surprise reaction from knowing the game's persuasive intent afterwards might create a lasting impression on the players, which is advantageous in fully conveying the core message to the players [8].

Aside from comparing the effectiveness of unwinnable persuasive games with and without surprising events, we also examined if playing duration influences the effectiveness of the surprising event. With extended playtime, players can get more accustomed to the game, including its character and narrative. As a result, a relationship



between the players and the game can be established. The relationship can make the game more meaningful to the players [30], which is also supported by Jacobs' study [1], in which players are more supportive of the game's agenda when they play longer. However, for unwinnable persuasive games with surprising events, a wide span between the start of the game and the revelation moment can diminish the effect of the surprise because it might be more difficult to recall the preceding event. Based on that, we hypothesized that playing unwinnable persuasive games with surprising events for a longer duration will have smaller effects on players. Conversely, if the context was informed from the beginning, then playing for a longer duration will have a greater effect on players. For this case, we took an exploratory approach. The results will indicate the benefits or drawbacks of longer playing duration for unwinnable persuasive games with or without surprising events.

To sum it up, our study aims to examine the effects of surprising events and playing duration in unwinnable persuasive games. For the surprising events, we compared the effects of openly revealing the context of the game from the beginning to omitting it until the end of the game. The results of our study can shed some light on if and how surprising events can be designed to be beneficial in unwinnable persuasive games.

2 Method

2.1 Design

Because it is more valuable to examine the effect of specific game properties [12], we followed a value-added approach [31] and compared the outcome of a group that plays a base game to the other group that plays a similar game that only differs in the properties being studied. Because of that, we employed 2 x 2 factorial between-subject designs with surprising events (Surprise x No Surprise) and number of levels (Long x Short) as between-subject factors.

In the Surprise condition, the game's context was revealed after the end of the game. On the other hand, in the No Surprise condition, the context was informed from the beginning of the game. For playing duration, we did not operationalize it as a time limit. The decision was made because the persuasion process in unwinnable persuasive games might rely on the participants making some progress during the play session. With differences in gaming skills, participants might reach different points in the game when the time runs out. For a comparable result between participants in the game groups, they need to reach the same point, regardless of playing time. Therefore, we opted to operationalize playing duration as the number of levels the participants need to pass through before the level we designed to be unwinnable. In other words, participants would encounter a higher number of levels when they were assigned to the Long condition.

While we were mainly interested in understanding how surprising events work in unwinnable persuasive games, it is still important to truly measure the game's impact. Based on that, a single no-treatment control group was included in the study design [12], [32]. In the end, there were five experimental conditions. Participants in the game groups were asked to play the game until they were out of life points, while participants in the control group received no intervention at all. The complete procedure of the experiment will be elaborated more in section 2.5. The percentage of donation made by the participants, willingness to help, and critical reflection were selected as the primary dependent variables to gauge the effectiveness of the persuasion process. In addition, participants' involvement in environmental issues, empathic concern, and gamer dedication were considered to control for possible confounding effects.



2.2 Game setup

We have developed our own game to be used in the experiment. The decision was made to make it easier to control the independent variables. The selected theme for the game was marine debris, an emerging and pressing environmental issue that threatens the local Indonesian oceans and the world's oceans, with a projected 155-265 million metric tonnes by 2060 [33]. Indonesia itself has been a large contributor to marine debris [33], [34]. The immense number of marine debris can have negative biological and ecological impacts, which can lead to economic damage, including fisheries or tourism [35]. As a maritime and the world's largest archipelagic nation, marine debris is a major concern in Indonesia.

Based on the theme, we designed the game to depict how accumulated marine debris at the coast negatively affects the quality and quantity of fish in that area. Because of that, the fishers are forced to sail further, increasing their safety risk and expense. In the game, players take control of the fishers' boat and must catch a fish to meet the weight quota. The playing area is divided into the debris zone and the fishing zone. The former is located near the coast (the starting point) and is depicted as part of the ocean with no fish and littered with marine debris. The latter is located beyond the debris zone and is used by players to catch the fish. If the players sail away from the coast, they will encounter more and bigger fish. However, their boat was only provided with a limited amount of fuel. If it runs out of fuel before meeting the quota, players will lose one life point and must repeat the level. Once they lose three life points, the game is over. The game can be played only by using point and click, a decision we made to lower the barrier for the casual players to master the game control so that they can focus more on the game's core message.

To accommodate surprising events in our game, we divided the game into two parts: the information text and the gameplay. The former contained the game's context, including the core message and the information that participants play as the fishers. The latter was the session during which the participants played the game. By dividing the game into two parts, the surprising events as an independent variable can be manipulated by setting which part of the game will be encountered first. Following Brewer and Lichtenstein's [22] surprise discourse structures, participants in the Surprise condition will play the game first and then read the text once the game is over. Conversely, participants in the No Surprise condition will read the text first before playing the game (Figure 1).

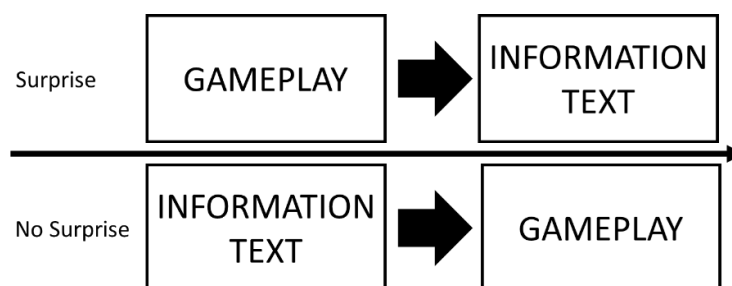


Figure 1. *Surprise and No Surprise condition*

The English-translated version of the information texts used in the experiment can be found in Figure 2.A and Figure 2.B. It is important to note that the text in Figure 2.C were used in both conditions at the end of the game. This text served as a Call to Action to encourage participants to donate money to reduce marine debris.

In addition, the game's aesthetics were designed to not explicitly reveal the game's context. Because of that, we avoid depicting the fishers' character as a man wearing an archetypal fishers' attire. Instead, we depicted the character as a young man with a casual outfit. Similarly, we opted for a modern boat rather than a traditional one. However, we also decided to give hints related to the core message to help participants make sense of the situation once they reach the revelation moment. Subsequently, it will also help the

debriefing process. For that reason, the debris zone was still depicted with scattered marine debris. The aesthetics were retained for the No Surprise condition to ensure that the only difference between the game was the order of the information text and the gameplay.

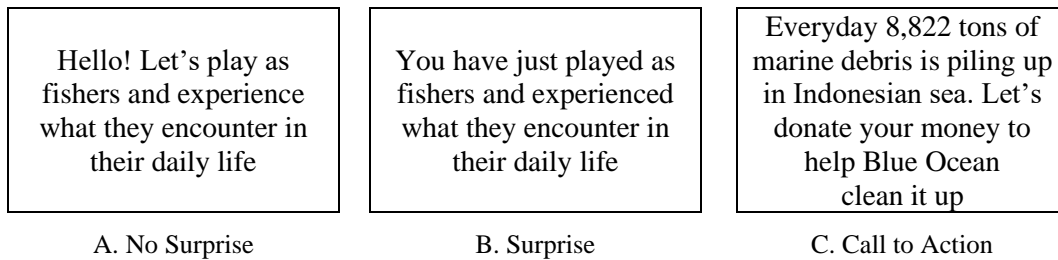


Figure 2. *The information text used in the experiment*

To create the Short condition and the Long condition, we set the number of levels to 3 and 5, respectively. In other words, the third and the fifth level of the game will be designated as the unwinnable level. However, since the unwinnable level was set relative to our research team's gaming skills, there is a likelihood that participants with higher skills will manage to overcome it. In this instance, the game will automatically create a new level with a higher weight quota. As a result, participants will still find the game unwinnable.

2.3 Participants

The participants were recruited by sending an invitation with an attached link to our experiment website through WhatsApp and Telegram, popular instant messaging services in Indonesia, and they participated voluntarily. Initially, the invitations were sent to multiple chat groups consisting of undergraduate students from the first author's university. Following the snowball sampling technique, we encouraged them to forward the invitation to their friends. Since it is possible for the participants to know the author personally, the content of the invitation and the website used a fake identity so that participants remain objective.

Afterwards, the collected data were filtered to exclude participants with incomplete or missing data from analyses. Additionally, we excluded participants who failed to pass the first level, indicating difficulty in understanding how to play the game. In the end, data from 142 participants (100 male and 42 female) remained. On average, participants were 19.94 (SD = 1.78) years old. They were randomly assigned to 5 experimental conditions: 34 in No Surprise x Long, 26 in No Surprise x Short, 27 in Surprise x Long, 25 in Surprise x Short, and 30 in control.

2.4 Measures

Percentage of Donation. Following the concept from Steinemann et al. [15], [20], we measured the percentage of donation made by the participants during the experiment. For this study, we used Rp. 50,000 instead of 1\$ to match with the local currency. At the beginning of the experiment, we promised to give the full money to the participants as a reward for completion. However, after the intervention, we asked them if they would donate a part of it to an organization that helps reduce marine debris. They can choose to donate from RP 0 to Rp 50,000 with 10% increments (i.e., RP 5,000).

Willingness to Help. We followed a similar procedure as Peng et al. [36], Steinemann et al. [15], and van 't Riet et al. [13] to measure willingness to help. However, since the topic of their research was refugees, we adjusted the questions to match our theme of environmental issues. Participants were asked to rate using a 7-point Likert scale about how willing they are to (1) "sign a petition to support the prevention of

environmental pollution”, (2) “discuss environmental pollution issues with their friends or family”, (3) “be active in environmental pollution prevention activities”, (4) “be actively involved in organizations that support the prevention of environmental pollution”, and (5) “share information related to the prevention of environmental pollution with friends”. It is important to note that, in all of the previous works, there was an item asking the participants about their willingness to donate money. We intentionally excluded this item to avoid priming the participants before the part asking them to donate part of their rewards. By doing so, we want to ensure that participants did not make donation because they were influenced by their previous answer.

Critical Reflection. To measure critical reflection, we utilized the Critical Reflection subscale of the Reflection Questionnaire [37]. The sub-scale consisted of four items (Cronbach’s $\alpha = .81$). Participants were asked to rate their level of agreement on several statements using a 5-point Likert scale from Definitely Agree to Definitely Disagree.

Playing Time. Participants’ overall playing time was recorded automatically during their play session. The recording included the time they took to read the information text and to play the game until it ended.

Empathic Tendency. One of the covariates we considered in this study to control for possible confounding effects was the participants’ empathic tendency. We measured it because their tendency to empathize with others might influence the donation and willingness to help. For this study, we utilized Davis’ [38] Empathic Concern subscale from Interpersonal Reactivity (Cronbach’s $\alpha = .61$).

Involvement in Environmental Issues. It is also possible that participants’ interest in environmental issues influences their donation and willingness to help. Therefore, we also considered their involvement in environmental issues as a covariate. We measured it following Peng et al. [36] procedure which also has been used in other study [15]. Since our topic is related to environmental issues, instead of humanitarian issues, we adjusted the wording of the questions. Hence, we used four items to ask participants to indicate their agreement with the statements (1) “I am interested in issues concerning the environment”, (2) “I am interested in issues concerning the environmental pollution”, (3) “I pay attention to news about the environment”, and (4) “I pay attention to news about the environmental pollution”.

Gamer Dedication. We also considered participants’ gaming skills as a covariate since it can affect their playing duration. We focused on gamer dedication because different games require different gaming skills. Hence, we utilized the Gamer Dedication scale created by Ip [39]. It consisted of 15 items (Cronbach’s $\alpha = .90$) which were rated using a 5-point Likert scale from 1 (strongly disagree) to 5 (strongly agree). Each item was scored using different weightings to calculate the final score. While the weightings can be determined without restriction, we decided to follow the weightings used by Ip.

All the scales were translated into Bahasa Indonesia to accommodate the participants’ native language. All the scales were translated by a professional English-Bahasa Indonesia translator. Additionally, if the game is over either before or after the unwinnable level, participants’ data were still collected without any changes.

2.5 Procedure

This study employed a remote unmoderated approach. Figure 3 shows the complete procedure of the experiment. In the beginning, participants arrived at the experiment website by following the link in the invitation. On the welcome page, participants read the consent form that informed them about their rights and the experiment procedure. However, the study’s true nature was only informed at the end of the experiment to avoid any influence on the donation and willingness to help. Furthermore, participants were informed that they would receive real money as a reward for completing the experiment. They can choose between phone or e-money balance. In order to receive it, they must be



willing to fill in their phone number so we can use it to send the reward. If they agreed, they would be randomly assigned to one of the five experimental conditions.

Afterwards, participants in the game groups were informed that they must play the game until they had no more life points or they managed to finish it. However, we never informed them that the game was unwinnable. After the end of the play session, they were asked to fill in the critical reflection questionnaire. Afterwards, they were reminded of their reward and given a chance to donate part or all of it to an organization that helps clean marine debris. In the next step, they were asked to fill in the general questionnaire that measured their willingness to help, empathic tendency, involvement in environmental issues, gamer dedication, gender, and age. Finally, they received their reward based on their previous selection.

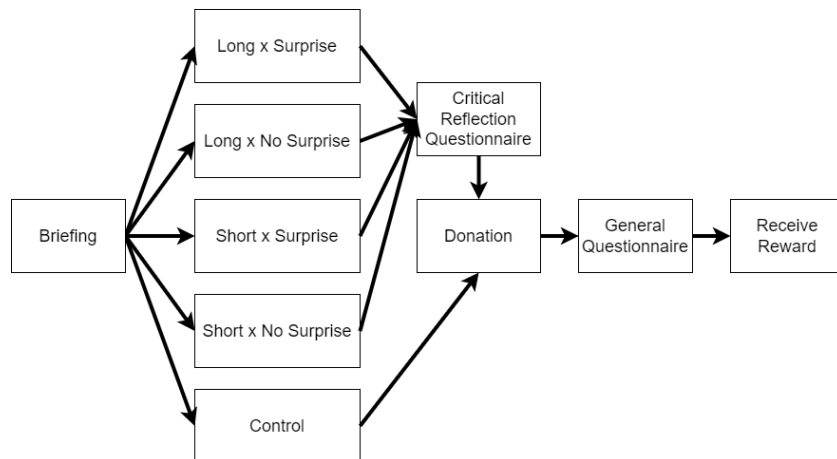


Figure 3. *Experiment procedure*

As for participants in the control group, they received no intervention at all. After voluntarily filling in their phone number on the welcome page, they went straight to the donation part of the experiment. Afterwards, they were asked to fill in the same questionnaires as the game groups, except for the critical reflection questionnaire since they never had the chance to play the game.

3 Result

Before analyzing the main effects, we conducted several data preprocessing. First, we examined the willingness to help and involvement in environmental issues. In the original study by Peng et al. [36], all the questionnaire items were analyzed separately. However, both Steinemann et al. [15] and van 't Riet et al. [13] averaged these items into a single willingness to help score and a single involvement in environmental issues. Following van 't Riet et al., we conducted a factor analysis to determine if aggregation was acceptable. For the willingness to help, the Scree test suggested a single factor ($KMO = .86$), explaining 68.81% of the variance with acceptable internal consistency ($\alpha = .89$). As for the involvement in environmental issues, the same procedure yielded a similar conclusion ($KMO = .84$), accounting for 82.25% of the variance with acceptable internal consistency ($\alpha = .93$). Hence, we averaged all items to produce one willingness to help score and one involvement in environmental issues score.

Afterwards, we examined if the random assignment produced equivalence between all the experimental conditions. Using one-way ANOVA, we could infer that there were no significant differences between groups on age ($\eta_p^2 = .026$), empathic tendency ($\eta_p^2 = .023$), involvement in environmental issues ($\eta_p^2 = .040$), and dedication for game ($\eta_p^2 =$

.004). However, from Chi-square test, we found significant difference between groups on gender ($\chi^2(4) = 10.607, p = .031$). Therefore, gender was included as a covariate in subsequent analyses.

Table 1. *Descriptive statistics for dependent variables and covariates*

	No Surprise		Surprise		Control ^a
	Long M (SD)	Short M (SD)	Long M (SD)	Short M (SD)	M (SD)
Percentage of donation	31.47 (39.01)	10.38 (22.36)	22.59 (39.48)	18.80 (36.67)	27.33 (35.81)
Willingness to help	5.26 (1.25)	5.82 (0.82)	5.37 (1.27)	6.06 (1.12)	4.87 (1.41)
Critical Reflection	8.85 (3.15)	9.96 (3.77)	9.70 (4.30)	8.24 (3.80)	–
Playing Time ^b	1,034.29 (342.90)	731.46 (225.52)	941.93 (326.61)	688.88 (200.60)	–
Empathic tendency	2.84 (0.70)	2.86 (0.65)	2.56 (0.69)	2.74 (0.77)	2.74 (0.63)
Involvement in environmental issues	5.11 (1.29)	5.41 (1.30)	4.86 (1.63)	5.39 (1.30)	4.72 (1.28)
Gamer dedication	68.38 (17.79)	67.92 (16.80)	68.42 (13.17)	70.66 (15.60)	69.96 (17.37)

Note: ^a control group only collected percentage of donation, willingness to help, and covariates

^b measured in seconds

Finally, the Shapiro-Wilk tests were conducted on all the dependent variables to test for normality. The result suggested that all the dependent variables did not come from a normal distribution. Because of that, all analyses on dependent variables were conducted using non-parametric tests.

The descriptive statistics of all the dependent variables and covariates can be seen in Table 1, while the Spearman correlation of the dependent variables can be seen in Table 2. On average, participants spent 864.63 seconds (14-15 minutes) during the play session (SD = 318.56 seconds). In particular, participants in the Short condition spent 710.59 seconds or 11-12 minutes (SD = 40.15), while participants in the Long condition spent 993.41 seconds or 16-17 minutes (SD = 36.71). It needs to be noted that the control group never filled in the critical reflection questionnaire. Hence, only the percentage of donation, willingness to help, and covariates can be collected from them.

Table 2. *Spearman correlation of dependent variables*

	Percentage of Donation	Willingness to help	Critical Reflection
Willingness to help	-.04		
Critical Reflection	.05	-.45**	
Playing Time	.34**	-.11	-.04

Note: * $p < .05$; ** $p < .01$

To examine the main effects of surprising events and number of levels, we conducted two-way ANCOVA analyses on the percentage of donation, willingness to help, and critical reflection with gender as the covariate. Since all the dependent variables did not come from a normal distribution, Aligned Rank Transform [40] was conducted first. Table 3 shows the summary of the result.

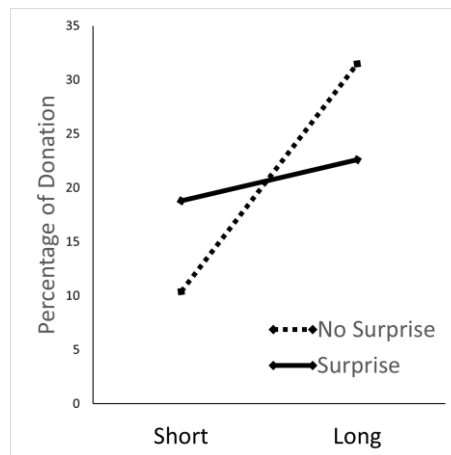


Table 3. Result from two-way ANCOVA with Aligned Rank Transform

	Surprise	Number of Levels	Surprise * Number of Level
	p (η_p^2)	p (η_p^2)	p (η_p^2)
Percentage of Donation	.197 (0.016)	.001** (0.152)	.044* (0.038)
Willingness to help	.228 (0.014)	.011* (0.059)	.636 (0.002)
Critical Reflection	.553 (0.003)	.659 (0.002)	.089 (0.027)

Note: * $p < .05$; ** $p < .01$, gender as covariate

For the percentage of donation, the results suggested that there was a significant interaction effect ($F(1,107) = 4.173$, $p = .044$, $\eta_p^2 = 0,038$). Further pairwise comparisons using the Mann-Whitney U test were conducted to examine any simple effect. The results suggested that there was a significant simple effect of number of levels on the No Surprise condition ($U = 271$, $p = .007$), in which participants in the Long condition ($M = 31.47$, $SD = 39.01$) donated significantly more money compared to participants in the Short condition ($M = 10.38$, $SD = 22.36$). The same results were not found on the Surprise condition ($U = 330.5$, $p = .881$). Similarly, there were no simple effects of surprise on both the Short ($U = 311$, $p = .757$) and the Long ($U = 334.5$, $p = .055$) conditions. Figure 4 shows the mean difference of the percentage of donation in all game conditions.

**Figure 4.** Mean difference of the percentage of donation

In addition to the significant interaction effect, there was a significant main effect of number of levels ($F(1,107) = 19.229$, $p < .001$, $\eta_p^2 = 0,152$). Participants in the Long condition significantly donated more money ($M = 27.54$, $SD = 39.14$) than participants in the Short condition ($M = 14.51$, $SD = 30.22$).

For the willingness to help, results of the two-way ANCOVA suggested that there was a significant main effect of number of levels ($F(1,107) = 6.757$, $p < .011$, $\eta_p^2 = 0,059$). Follow-up analyses suggested that participants in the Short condition had a higher willingness to help ($M = 5.93$, $SD = 0.98$) than participants in the Long condition ($M = 5.31$, $SD = 1.25$).

For the critical reflection, the two-way ANCOVA results suggested that neither the main effect nor the interaction effect of surprising events and number of levels was statistically significant. Despite that, the interaction effect on critical reflection ($F(1,107) = 2.955$, $p = .089$, $\eta_p^2 = 0,027$) seemed to suggest that the effect of surprising events might depend on the number of levels.

Finally, the Mann-Whitney U tests were conducted to examine whether any game groups performed better than the control group on the percentage of donation and willingness to help. For the percentage of donation, the results of analyses suggested that

none of the game groups exceeded the control group significantly. In fact, the results suggested that participants in the control group donated significantly more money than participants in the No Surprise x Short condition ($U = 250.50$ $p = 0.14$). Nevertheless, for the willingness to help, two game groups were significantly higher than the control group: No Surprise x Short ($U = 227.00$, $p = .01$) and Surprise x Short ($U = 172.00$, $p < .01$).

4 Discussion

From the experiment, it can be inferred based on the follow-up pairwise comparisons that there are no simple effects of surprising events on the percentage of donation, neither on the group that plays the shorter version of the game nor the group that plays the longer version. It might suggest that the moment the game's context is revealed has indifferent effects on the donation amount. While the discrepancy between the group in the Short condition (11-12 minutes) and the Long condition (16-17 minutes) is significant when the information text preceded the gameplay, nevertheless, we find out that the difference is not significant when the information is revealed at the end. Given these points, it is likely that surprising events can promote donation on the same level, regardless of the playing duration.

On the other hand, a longer time commitment is essential if players are given the game's context from the beginning. This is in parallel with Jacobs' findings [1], in which players are more supportive of the game's core message when they spend more time (i.e., over 45 seconds in "My Cotton Picking Life"). In addition, Jacobs also mentioned that knowing the game's persuasive intent also positively affects the persuasion process. The results from our experiments further support that longer playtime can lead to greater effects when the game's intention is informed from the beginning. Specifically for our case, playing the game for 16 minutes led to a significantly higher donation than 11 minutes of playtime. While 5 minutes differences are small, it is an interesting finding that such small gap could lead to a significant result. Although it is too early to generalize the result with games played for more than 15 minutes or time difference bigger than 5 minutes, it might be worth repeating the experiment in the future with a more extended playtime (e.g., 30 minutes) or a bigger time gap (e.g., 15 minutes) between conditions.

Aside from the effects of the surprising events, the results suggest that the number of levels has a significant main effect on both the percentage of donation and the willingness to help. Surprisingly, the effects on both variables are different. Higher donation comes from the group that plays the longer version, while higher willingness to help comes from the group that plays the shorter version. In addition, willingness to help does not significantly correlate with the percentage of donation, which does not support previous findings by Steinemann et al. [15].

One possible explanation is that the willingness to help does not necessarily translate to actual behavior to help because of a gap between the intention and the behavior [41]. Several studies in the psychological domain have suggested how intention can determine the likelihood that a behavior will be carried out (e.g., [42], [43]). For a behavior to be performed, a sufficient level of intention is necessary. The results suggest that losing in the shorter version of the game might evoke players' intention to help but is not impactful enough to move them to donate. In addition, all the items in the scale used to measure willingness to help have nothing in common with donating behavior, despite the mutual goal to save the environment. This might explain why the correlation is not significant. Moreover, we have mentioned how we intentionally removed willingness to donate from the scale to avoid priming the participants. This might also explain the gap between the intention to help and the actual act of donation.

One of the reasons why it is so difficult to establish persuasive games as effective means of persuasion stem from the lack of knowledge about how to measure its impact in the real world and how to compare the result between studies without a universal success



indicator [44]. However, if the goal of the game is to evoke behavior change, then the usage of objective measurable behavior to measure its impact is more encouraged than subjective measure. In our opinion, we agree with Steinemann et al. [15] that donation can be a good measure for future studies in a similar domain.

Finally, this study also finds that playing games do not lead to a higher percentage of donation when compared to the control group. The null results support Van 't Riet et al. [13], which shows that the idea of persuasive games as novel persuasive media that outperform traditional types of media (e.g., text or video) might need a more thorough examination. Without disregarding the promising findings from previous studies (e.g., [1], [15], [36], [44]), the null results should encourage future studies to dissect the games to gain more knowledge on how to design persuasive games that can persuade effectively. In addition, since the persuasion process also includes how the players interpret the games [45], thus persuasive games might be effective when played by particular individuals or situated in certain circumstances, something that has been suggested by both Van 't Riet et al. [13] and Jacobs [46].

As for why this study finds null results when compared with the control group, one possible reason might be related to the obstacle of the donation. In our experiment procedure, the control group received no intervention at all and directly filled in the questionnaires, unlike the game groups that were asked to play the game first. Therefore, the control group took a shorter experiment time. It is possible that the short duration makes them feel not entitled to receive the full reward. As a result, their obstacle becomes lower than the game groups, encouraging them to donate more [47]. While the existence of the no-treatment control group is valuable to assess persuasive games' effectiveness [12], [32], future studies might attempt to set a control condition that receives no intervention but takes an approximately similar amount of time to the group that plays the games.

Nevertheless, despite the null results in the percentage of donation, this study finds that all the game groups have a higher willingness to help than the control group, especially those that play the shorter version of the games. These promising findings should encourage further studies to explore how to design unwinnable persuasive games that can support the translation of the intention to help into the actual act of donation.

The study manages to gain insight into the effects of surprising events and the interaction with playing duration in unwinnable persuasive games. However, in addition to setting the control group with an unequal time compared to the game groups, this study has several limitations. First, we created the surprising events simply by delaying the revelation of the game's context after the end of the game. Several works (e.g., [23], [48]) have mentioned that a surprising event is a graded experience. In other words, some events can be more surprising than others, either because they have a lower probability of occurring or are more difficult to comprehend. It is possible that the null results on critical reflection are caused by the players not finding the revelation surprising enough to evoke them to reflect on the experience. The same cause can also explain why there is no main effect of surprising events on the willingness to help and the amount of donation. With that in mind, different degrees of surprising events might influence players differently, and it is encouraging for future studies to explore it and add further insight into the role of surprising events in unwinnable persuasive games.

Another limitation is that we only utilized short text, similar to "Spent", to reveal the context of the game. Other games like "My Cotton Picking Life" and "September 12th" let the players realize the surprise through the gameplay. In persuasive games, designers can use several strategies ranging from signs (e.g., cinematic or linguistic) to context (e.g., tactical or sensorial) [49]. The content of the revelation can also be manipulated, ranging from revealing only a glimpse of the game's context to presenting facts and figures. Given these points, it is interesting for future studies to compare the effects of different revelation methods.



5 Conclusion

From this study, we learn that there are merits for designers to use surprising events in unwinnable persuasive games because delaying the revelation moment of the game's context can invite the same level of donation, regardless of playing duration. However, if the persuasive intent is known from the beginning, designers will benefit more if the playtime is longer. Despite the positive insight, we encourage future studies to further examine the effects of different degrees of surprising events and different methods to reveal the surprise. Interestingly, we also find that playing duration affects intention to help and donating behavior differently. This finding brings us back to the question of how to measure the impact of persuasive games in the real world, especially if there are differences between intention and behavior. We recommend that future studies utilize an objective measurable behavior such as donating behavior to measure the effectiveness of unwinnable persuasive games, especially if the main goal of the game leads to a change in behavior.

Acknowledgement

This research was funded by Kementerian Pendidikan dan Kebudayaan Indonesia (Ministry of Education and Culture Indonesia) through Beasiswa Pendidikan Pascasarjana Dalam Negeri (BPPDN) scholarship program

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