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## Investigating the Effects of a Persuasive Digital Game on Immersion, Identification, and Willingness to Help

Jonathan van 't Riet<sup>a</sup>, Annika C. Meeuwes<sup>b</sup>, Laura van der Voorden<sup>a</sup>, and Jeroen Jansz<sup>b</sup>

<sup>a</sup>Radboud University; <sup>b</sup>Erasmus University Rotterdam

### ABSTRACT



Recent years have seen a tremendous rise in the development and distribution of persuasive games: digital games that are used to influence players' attitudes and/or behavior. Three studies ( $N_{\text{Study 1}} = 134$ ;  $N_{\text{Study 2}} = 94$ ;  $N_{\text{Study 3}} = 161$ ) tested the effects of a persuasive game on immersion, identification, and willingness to help. The results showed that playing the persuasive game did not result in substantially stronger willingness to help, relative to the control conditions. Video and printed text resulted in more immersion than the digital game, but playing the game resulted in substantially higher perceptions of embodied presence.

Recent years have seen a tremendous rise in the distribution of digital games that aim to change society for the better. The website Games for Change ([www.gamesforchange.org](http://www.gamesforchange.org)) provides information on 173 (as of January 2018) digital games that aim to persuade or influence players in domains as diverse as civics, economics, health, and environmental issues. We refer to these games as *persuasive games*, because they are designed with the primary intention to change or reinforce attitudes with respect to social or political causes (Bogost, 2007; Jacobs, Jansz, & De La Hera, 2017).

Because the intended persuasion can occur in many different domains, persuasive games can take many shapes. The most prominent areas in which persuasive games are used are advertising, health, and the socio-political domain (Bogost, 2007). In the present research, we focus on digital games in the latter category, also sometimes called *social impact games* (Ruggiero, 2015). These can be distinguished from *prosocial games*. The label *prosocial* refers to the internal reality of gameplay that is characterized by players and game characters helping and supporting each other (Gentile et al., 2009). This altruistic behavior has the potential of "spilling over" into the real, nongame world, but this happens more by accident than by design. Persuasive games, by contrast, are

purposefully designed to affect attitudes that apply to the real, nongame world (Jacobs et al., 2017).

Much has been made of the potential for persuasive games to result in positive social change (Barthel, 2013; Bogost, 2007; Ruggiero, 2015). Notwithstanding this potential, however, there is a dearth of research on the effectiveness of persuasive games. As Peng, Lee, and Heeter (2010) remarked, "Dozens of ... games for social change have been developed and played by millions of people, [but] few empirical study ... has been conducted to evaluate how effective these games are" (pp. 723–724). Unfortunately, this observation is still true 8 years after it was made: Research on the effectiveness of persuasive games aimed at social impact is still relatively scarce (Jacobs, 2016; Ruggiero, 2015). Although there is a burgeoning literature on digital games that aims to promote a healthy lifestyle (DeSmet et al., 2014; Van 't Riet, Crutzen, & Lu, 2014) and digital games that are used in education and training contexts (Wouters, van Nimwegen, van Oostendorp, & Van der Spek, 2013), research on the effectiveness of persuasive games aimed at social impact is relatively scarce (Jacobs, 2016; Ruggiero, 2015). The current article aims to contribute to a stronger empirical foundation by studying the effectiveness of *Against All Odds*, a persuasive game that has been designed to increase empathy for refugees.

**CONTACT** Jonathan van 't Riet  [j.vantriet@maw.ru.nl](mailto:j.vantriet@maw.ru.nl)  Behavioural Science Institute, Radboud University, PO Box 9104, 6500 HE, Nijmegen, The Netherlands.

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In addition to investigating *whether* persuasive games are persuasive, more insight is needed about *how* these games work (Peng et al., 2010). Game scholars have analyzed the persuasive properties of games (Bogost, 2007; De La Hera, 2017) and have generally linked these persuasive properties to the medium's interactive nature (Neys & Jansz, 2010). In the present research, this interactivity is studied from the perspective of construal-level theory (Liberman & Trope, 1998). From this perspective, it could be expected that the interactive nature of persuasive games makes the persuasive information more “self-relevant.” This increased self-relevance can be expected to increase *immersion* (Brockmyer et al., 2009; Granic, Lobel, & Engels, 2014) and *identification* (Peng et al., 2010; Van Looy, Courtois, De Vocht, & De Marez, 2012). At present, there is very little research that investigates whether persuasive games are indeed more immersive or more likely to foster identification than traditional media and whether these two factors are related to persuasive outcomes. Therefore, in addition to investigating the effectiveness of *Against All Odds*, the present research also tested the importance of immersion and identification.

### Interactivity and construal level

By nature, digital games are interactive, which means that players cannot passively submit to the events in a game. Interactivity has been referred to as the ability for users to influence or control the form or content of the media (Grodal, 2000; Steuer, 1992). One of the advantages of interactivity may be that it decreases psychological “distance” between the player and the content. According to construal-level theory (Liberman & Trope, 1998), people have distinct psychological associations with events and objects based on perceived temporal and social distance. When people receive information about events that will happen shortly (short temporal distance) or that is directly related to the self (short social distance), this information is processed quickly and attentively (Rogers, Kuiper, & Kirker, 1977). However, when the distance between the user and the information increases (i.e., when it concerns events in the far future, or when the information is not directly related to the self), information processing slows down and is less thorough.

As Ahn (2015) pointed out, digital technology can be used to decrease the distance between the information and the user. In one study, Ahn used an *immersive virtual environment* to simulate the adverse effects of risky health behavior. Rather than describing potential adverse effects, as is common in traditional health

communication, users of the virtual environment could experiment with risky behavior and immediately find out what these adverse effects were. Because the events were not hypothetical but the direct result of the player's actions, the information was directly self-relevant (Ahn, 2015).

Digital games offer similar opportunities. They can challenge players to experiment with several courses of action and explore the consequences of these actions (cf. Bogost, 2007; Jansz, 2005). Because the events in the game have important implications for players' performance in the game, they are highly self-relevant and may be scrutinized thoroughly. Thus, similar to immersive virtual environments, digital games can be used to present persuasive information that is directly relatable to the self and therefore more persuasive than persuasive information presented through traditional information.

Although this rationale seems plausible, and is—in various forms—quite frequently espoused in the literature (see Barthel, 2013; Bogost, 2007; Kampf & Cuhadar, 2014; Peng et al., 2010; Ruggiero, 2015), it is important to note that the empirical evidence that persuasive games are generally more effective than traditional, noninteractive media is not so straightforward. Although strong effects of persuasive games have been found in some studies (Kampf & Cuhadar, 2014; Peng et al., 2010; Ruggiero, 2015), weaker effects have been found in other studies (Barthel, 2013; Gutierrez et al., 2014; Jacobs, 2016; Soekarjo & Van Oostendorp, 2015). Ultimately, however, there are too few empirical studies to derive conclusions on the genre as a whole. A recent review of the literature (Soekarjo & Van Oostendorp, 2015) found 60 persuasive games that were described in academic publications, but only 15 of these descriptions were accompanied with empirical data on the effectiveness of the game in question. Moreover, the majority of these empirical studies suffered from methodological weaknesses, such as the lack of adequate control conditions (Soekarjo & Van Oostendorp, 2015). We agree with Iacovides and Cox (2015) that more methodologically sound research on the persuasive effects of persuasive games is needed.

Sound empirical evidence is all the more important, as there are reasons to be cautious with claims of large persuasive effects. In the adjacent area of video games aimed at improving cognitive performance, a recent review suggests that there is no evidence of enhanced everyday cognition (Simons et al., 2016). Likewise, the literature on video games and aggression offers a cautionary tale. Here, initial strong claims about negative effects of video games on aggressive behavior

(Anderson et al., 2010) have been criticized (Ferguson, San Miguel, Garza, & Jerabeck, 2012), and the results of recent meta-analytic studies suggest that the effects generally are weak (Ferguson, 2015; Furuya-Kanamori & Doi, 2016; Hilgard, Engelhardt, & Rouder, 2017; but for a different view, see Kepes, Bushman, & Anderson, 2017). This is especially noteworthy because, in this literature as well, predictions of large effects are sometimes based on the increased interactivity of video games (e.g., Lin, 2013). So far, however, the importance of interactivity in this regard remains contentious. For instance, Sherry (2001) concluded from his meta-analysis that the effect of playing games on aggression was smaller than the effect of watching TV violence. More recently, Devilly and colleagues (2017) did not find different effects of media violence when they compared the effects of a book, a TV show, and a game.

In sum, there are great expectations about the usefulness of persuasive digital games for bringing positive change to society. The increased interactivity of such games as compared to traditional media provides a powerful theoretical rationale for this. On the other hand, research is scarce, and findings from adjacent fields suggest we should be cautious in our expectations. For this reason, the present research investigated whether a persuasive game would be more persuasive than traditional, noninteractive modes of communication.

RQ1: Will playing a persuasive game result in a stronger persuasive effect than being exposed to a video or printed text with similar persuasive information, or than being exposed to no persuasive information?

In addition to investigating the persuasive potential of persuasive games, the present research aimed to look closer at the processes that mediate a potential persuasive effect. Therefore, in line with the general notion that interactivity increases the self-relevance of the persuasive content, we investigated whether persuasive games result in increased levels of immersion and identification as compared with traditional noninteractive media.

### Immersion and identification as mediators

We just argued that the interactive nature of digital games increases the self-relevance of the game's content. If this is indeed the case, one would expect users of interactive content to be more motivated to engage with the content, pay more attention to the content than recipients of noninteractive content, and be more

emotionally involved and engrossed in the content. In short, one would expect interactivity to result in increased *immersion* (Caroux, Isbister, Le Bigot, & Vibert, 2015; Charlton & Danforth, 2007).

Unfortunately, different authors have conceptualized immersion in different ways. In the academic literature, player-game interactions are described with multiple concepts that are sometimes similar and almost always related (Caroux et al., 2015; Wirth et al., 2007). Examples are engagement (Boyle, Connolly, Hainey, & Boyle, 2012), enjoyment (Jacobs, 2016), presence (Banos et al., 2004), and flow (Schmierbach, Chung, Wu, & Kim, 2014). Resolving this debate was not the aim of the present research, however. Rather, we define immersion as the extent to which the player is motivated to play the game (Crutzen, Van 't Riet, & Short, 2016). In this operationalization, being determined to play and keep playing the game signals high levels of immersion while being able and willing to interrupt or stop playing at any moment signals low levels of immersion. It could be expected that the interactive nature of persuasive games makes the persuasive content more self-relevant and thereby increases motivation to engage with this content (i.e., immersion). To our knowledge, however, this has not been investigated. Jacobs (2016) experimentally compared a persuasive game with a video clip but did not include immersion as an outcome measure. Peng et al. (2010) compared a persuasive game with a video of recorded gameplay but also did not include immersion as an outcome measure.

With regards to the effect of immersion on persuasion, there are a number of indications that this effect is important (Graesser, Chipman, Leeming, & Biedenbach, 2009; Prensky, 2007; Soekarjo & Van Oostendorp, 2015). As Khaled (2007) put it, "It seems that games as persuasive technology hold much promise for changing people's attitudes: games are by nature interactive, and people tend to retain more impressions" (p. 17). Therefore, we assessed immersion as a mediator of the persuasiveness of persuasive games, reasoning that the interactivity of persuasive digital games will lead to increased immersion and that increased immersion will lead to a stronger persuasive effect of playing the game. We formulated the following research questions:

RQ2: Will playing a persuasive game result in more immersion than being exposed to a video or printed text with equivalent persuasive information?

RQ3: Will immersion mediate the persuasive effect of the persuasive game?

A similar line of reasoning could be employed regarding identification. It is important to note that, in many digital games, players take control of an avatar. If, as we have argued, interactivity increases the self-relevance of the content, this also implies that what happens to the avatar feels more relevant to the player.

*Identification* refers to the mental activity of temporarily imagining oneself in the position of another person. According to Cohen (2001), identification occurs not only in real life but also through mediated communication. Many scholars (e.g., Konijn, Nije Bijvank, & Bushman, 2007; Moyer-Gusé, 2008) have emphasized two levels of identification: *wishful identification* and *similarity identification*. Wishful identification can be defined as “the desire to be like or act like the character” (Hoffner & Buchanan, 2005, p. 325). Similarity identification refers to the extent to which the user can identify with the media character because they “share salient characteristics” (Konijn et al., 2007, p. 1039). Van Looy et al. (2012) expanded these theories by adding a third level of identification: embodied presence. Embodied presence refers to “the [feeling] of being embodied in the character” (Van Looy et al., 2012, p. 202). Embodied presence is the highest level of identification. It constitutes an affective process in which the user experiences a real bond with the media character.

In previous work, Peng et al., (2010) have investigated the effects of the persuasive game *Darfur is Dying*, which aims to foster identification with residents of South Darfur hit by war and famine (<http://www.darfurisdying.com/>; see also Peng et al., 2010). In two studies, Peng and colleagues found that participants who played *Darfur is Dying* showed greater willingness to help residents of Darfur than participants who read a text containing information on the Darfur crisis (Study 1) and than participants who watched the game but did not play (Study 2). They also found that identification mediated the effectiveness of the game. One other study, however, found that a printed text about the situation in Darfur resulted in more identification than *Darfur is Dying* (Steinemann et al., 2015). It is therefore currently unclear whether persuasive games can be used to elicit increased levels of identification. In the present study, we attempted to shed more light on this issue using a different persuasive game.

RQ4: Will playing a persuasive game result in more identification than being exposed to a video or text with equivalent persuasive information?

RQ5: Will identification mediate the persuasive effect of the persuasive game?

## The present research

We conducted three studies to investigate the effects of a persuasive game on immersion, identification, and persuasion. Following previous research (Peng et al., 2010), we used willingness to help as the main persuasive outcome measure. The digital game in question was the existing online Adobe Flash game *Against All Odds* (<http://www.playagainstallodds.ca>), designed to increase empathy for refugees. This game was chosen because it was deemed highly relevant in the context of the European Union’s current refugee crisis, which has been described by German chancellor Angela Merkel as one of the European Union’s greatest challenges in recent decades (Barkin, 2015).

In *Against All Odds*, players are faced with the challenge of fleeing an unnamed war-torn country and arriving safely at an unnamed peaceful destination country. A short introduction tells the player that his or her country is plagued by political instability and that a military coup has taken place. The player is also told that he or she could well be targeted by the secret police and should “be careful.” The narrative unfolds from there, taking place in 12 levels: The first four deal with escaping the home country, the next four deal with arriving safely in the destination country, and the final four levels deal with building a new life in the destination country. In the present study, only Levels 1–4 were used. All through Levels 1–4, the player is faced with choices that represent the dilemmas of actual refugees. For instance, (a) during the interview with the secret police, the player has to decide whether to sign declarations of support for the military regime; (b) while getting only 2 min to pack his or her bag, the player has to quickly decide which items to pack; (c) faced with the prospect of crossing the border through a steep mountain path, the player is faced with the dilemma of leaving his or her injured best friend or staying by his side. The game uses a click-and-point-adventure mechanic, in which the player’s choices help the narrative along. However, certain decisions will result in incarceration or death, at which point the level must be played again. For instance, (a) not signing declarations of support for the military regime, (b) taking too much time to pack the bag, and (c) staying with an injured friend will result in incarceration or death, and the player is notified of this through a short text. The only exception is Level 3, in which a short skills-based game is added



and the player has to navigate through his or her village while avoiding the secret police and the army.

We conducted three experiments that most notably differed in terms of comparison conditions. For the sake of external validity, Study 1 used existing media content as stimuli in the comparison conditions. The persuasive game condition was compared with a video condition in which participants viewed an excerpt of a TV program about refugees and with a printed text condition in which participants read a newspaper article about refugees. The drawback was the fact that the experimental conditions differed substantially in the type of stimuli to which participants were exposed. To remedy this, Study 2 employed a printed text condition in which a written description of events in the game served as the experimental stimulus. This way, the information transmitted was held constant across conditions, and only the medium (game vs. printed text) was manipulated. Study 3 employed a video condition in which participants viewed an edited video of recorded gameplay. In Study 3, only interactivity differed between the conditions, whereas the information transmitted and mode of (audiovisual) presentation were held constant. The comparisons in Study 2 and 3 were based on similar comparisons in Peng et al. (2010) previous research and served to increase internal validity.

## Study 1

### Method

#### Participants

One hundred thirty-one undergraduates from the first author's university volunteered to participate in the study in exchange for course credit or a monetary reward of €7.50 (US\$8.84). The average age was 22.49 ( $SD = 5.75$ ), and 105 participants were female. Age did not differ substantially between conditions ( $\eta_p^2 = .015$ ), and neither did the distribution of gender,  $\chi^2(3) = 0.33.0$

#### Design and procedure

The study employed a one-factor (medium: game vs. video vs. print vs. control) between-subjects experimental design. Undergraduate students who agreed to participate in the study were randomized into the four conditions ( $n_{\text{game}} = 34$ ;  $n_{\text{video}} = 33$ ;  $n_{\text{print}} = 36$ ;  $n_{\text{control}} = 28$ ). They were invited into the university's lab and seated in individual soundproof booths, where they took part in the study. Desktop computers were used to present the game, the video, and all

questionnaires. The printed text was provided on paper. (Video and printed text can be found at <https://osf.io/3d8qa/files/>)

Participants randomly assigned to the game condition were instructed how to play the game. They were asked to play Levels 1–4 of *Against All Odds*. In case of failure, they were asked to try again and to keep playing until Level 4 had been finished. Participants in the video and print conditions were simply asked to watch the entire video or to read the entire text. The questionnaire was administered after the experimental manipulation and measured identification, immersion, and willingness to help, as well as demographic information.

#### Stimuli

An existing online Flash game (game condition), a video showing an excerpt of a TV program about refugees (video condition), and a newspaper article containing an interview with two refugees (text condition) were used as the experimental stimuli in this study. The game was *Against All Odds*. Participants played Levels 1–4, which took most participants approximately 15 min.

The video was a 15-min excerpt from a Dutch television program called *Piss Off to Your Own Country*. In this program, a number of Dutch citizens, selected to be a diverse representation of the Dutch population but notably including several people with outspoken negative views of refugees, travel from the Netherlands to Jordan (i.e., the reverse of the route taken by many Syrian refugees nowadays). In the course of their trip, they endure some of the hardships that refugees endure and encounter actual Syrian refugees. The excerpt used was from the original broadcast (January 22, 2015) and was not edited for this study. It showed that the participants had entered Greece, where they encountered some refugees living in Athens, either in refugee shelters or in squatted apartments.

The printed text consisted of an interview with a father and daughter who fled Syria and applied for asylum in the Netherlands. This interview was originally published in the Dutch national newspaper *De Volkskrant* on October 14, 2014. In the interview, father and daughter speak of the increasingly dangerous situation in Syria, their reasons for fleeing, and the hardships and dangers they faced while traveling through Europe. Participants in the print condition spent on average 2.5 min to read the article.

Although there were many differences between the game, video, and text, a common characteristic was that they all showed the grave dangers of refugees'

situation in their home countries and the hardships of traveling. All materials communicated the persuasive message that refugees do not choose to flee but instead are forced to flee.

### Measures

**Immersion.** Immersion was assessed with three items. Two items, taken from a scale used by Whitton (2010), asked participants to indicate their agreement with the statements “I really wanted to finish the game/video/text” and “I was drawn into the game/video/text” on a 7-point scale from 1 (*strongly disagree*) to 7 (*strongly agree*). An additional item, taken from a scale by Brockmyer and colleagues (2009), asked participants to indicate their agreement with the statement “I really got into the game/video/text.” The Immersion scale had good internal consistency (Cronbach’s  $\alpha = .85$ ).

**Identification.** To assess identification, six items by Peng et al. (2010) were used, which in turn were adapted from Cohen’s (2001) Identification scale. In addition, two items were taken from Cohen’s original Identification scale. The items from Peng and colleagues’ previous study asked participants to indicate their agreement with statements such as “I was able to understand the events in a manner similar to that in which the refugee understood them” and “I felt I could really get inside the refugee’s head” using a 7-point scale from 1 (*strongly disagree*) to 7 (*strongly agree*). The two additional items from Cohen’s Identification scale asked participants to indicate their agreement with the statements “I understood why the refugee did what he/she did” and “I felt that I really understood the refugee” also on a 7-point scale.

Peng and colleagues (2010) distinguished identification from role-taking, arguing that the term *identification* is used most frequently in a mediated context. Because a digital game clearly constitutes a mediated context, we use the term *identification* and therefore employed Cohen’s (2001) original Identification scale.

In a factor analysis (Kaiser-Meyer-Olkin [ $KMO$ ] = .83), the Scree test yielded a single factor, explaining 46.21% of variance. Therefore, a single identification score was calculated by averaging all eight items. The role-taking scale had good internal consistency ( $\alpha = .83$ ).

**Willingness to help.** To assess willingness to help, we employed a procedure previously used by Peng and colleagues (2010). For the first two items, participants were asked to use a 7-point scale to rate how likely it

was that they would (a) donate money to help refugees or (b) discuss the refugees’ situation with their friends or family. A third item used by Peng and colleagues, assessing participants’ willingness to forward the message, was omitted, as it could not be used in the control condition. One additional self-constructed item assessed willingness to (c) do volunteer work in a shelter for asylum seekers. Four additional items were included that indicated a willingness to actively oppose the immigration of refugees and that were reverse coded. These items were based on previous research on political participation (Bakker & De Vreese, 2011; Moeller et al., 2014) and asked participants to indicate how willing they would be to (d) argue online against the immigration of refugees, (e) sign a petition against the immigration of refugees, (f) protest against plans to shelter refugees in their neighborhood, and (g) vote for an anti-immigrant political party.

In Peng and colleagues’ (2010) original study, the willingness items were used separately in the analysis. In a replication study using the same materials, however, Steinemann et al. (2015) aggregated the willingness items into a single willingness score, with a borderline acceptable internal consistency of  $\alpha = .69$ . In the present study, we decided to perform a factor analysis and calculate internal consistency to see if the items could be aggregated or should be used separately. In the factor analysis ( $KMO = .70$ ), the Scree test yielded a single factor, explaining 34.75% of variance. The Willingness scale had borderline acceptable internal consistency ( $\alpha = .68$ ). This was considered acceptable, and a single willingness score was therefore calculated by averaging all seven items. In addition, however, all analyses were performed with the separate willingness items, following the original procedure by Peng et al. These analyses lead to similar results and identical conclusions and are not reported for reasons of brevity.

### Data analysis

The first step of data analysis was to investigate the correlation between identification, immersion, and willingness to help. Second, differences in identification and immersion between the three experimental conditions and differences in willingness between all four conditions were investigated by means of univariate analyses of variance (ANOVAs). Partial eta-squared was used as the effect size measure. In case of a comparison between two means, Cohen’s  $d$  was used as the effect size measure. We interpret effect sizes according to the guidelines laid out by Cohen (1988). These analyses were run using IBM SPSS Statistics 23.

Third, potential mediation of the effect of condition on willingness to help through immersion and identification was investigated using linear regression analyses. Outliers were removed casewise for all scales, but including the outliers in the analyses did not affect the significance of the results. In addition to the variables just reported, participants' explicit and implicit attitude toward refugees were assessed, as well as narrative transportation and digital game enjoyment, but the analyses concerning these variables are not reported here for reasons of brevity. The complete data and syntax can be found at <https://osf.io/3d8qa/files/>.

## Results

### Correlations

A correlation analysis revealed that there was a small correlation between immersion and willingness to help ( $r = .12$ ). There were also small correlations between identification and willingness ( $r = -.04$ ) and between immersion and identification ( $r = .17$ ).

### Differences between conditions

To investigate RQ1, an ANOVA was conducted with condition as the independent variable and willingness as the dependent variable. The results revealed that willingness to help did not differ substantially between conditions ( $\eta_p^2 = .025$ ).

To investigate RQ2 and RQ4, two ANOVAs were conducted with condition as the independent variable and immersion and identification, respectively, as the dependent variable. For immersion, substantial differences were found ( $\eta_p^2 = .182$ ). Pairwise comparisons showed that the video and printed text resulted in greater immersion than the game, with no substantial difference between the video and print conditions. Means and standard deviations can be seen in Table 1. The results of the second ANOVA showed no substantial differences between conditions in terms of identification ( $\eta_p^2 = .006$ ).

### Mediation analyses

Surprisingly, the game resulted in lower scores for immersion, and there were no differences for identification and willingness to help. Thus, a mediation model, in which the game would outperform the other conditions in terms of immersion and identification, and these two factors would mediate the game's effects on willingness (RQ3 and RQ5) can be confidently rejected based on the present data.

### Discussion and introduction to study 2

Study 1 revealed no evidence that a persuasive game could be more effective than video and printed text. Of interest, there was an effect of condition on immersion, with the video and print condition resulting in more immersion than the digital game, but immersion was not strongly associated with willingness to help.

A limitation of Study 1, however, was that participants in the game, video, and print conditions were exposed to similar, but not identical, content. Although the game, video, and printed text all dealt with the hardships of refugees, subtle difference of content and style could be responsible for the effect. Most notably, whereas the video game was set in an unnamed country, the video and printed text dealt with refugees from Syria. The video and printed text moreover featured real refugees rather than the fictional avatar in the video game. The content of the video and printed text may therefore have been both more realistic and more specific, making it easier for recipients to empathize. Indeed, to circumvent this issue, Peng and colleagues (2010, Study 1) compared *Darfur is Dying* to a print text that was designed to provide the same information as the information that was provided in the game. Therefore, in Study 2 we again tested the effectiveness of *Against All Odds* but now compared to a print condition in which the content was designed to be as similar as possible to the game's content.

Notwithstanding the differences between experimental stimuli in Study 1, it should be noted that the inclusion of a no-treatment control group enabled us

**Table 1.** Results of the analysis of variance for immersion, identification, and willingness to help in Study 1.

	Game		Video		Print		Control		Test results
	M	SD	M	SD	M	SD	M	SD	
Immersion	5.09 <sub>a</sub>	0.90	5.75 <sub>b</sub>	0.97	6.04 <sub>c</sub>	0.66	—	—	$F = 10.83, \eta_p^2 = .182$
Identification	4.76	0.94	4.81	0.92	4.91	0.65	—	—	$F = 0.29, \eta_p^2 = .006$
Willingness to help	5.13	0.92	5.35	0.80	5.19	0.83	4.94	0.99	$F = 1.10, \eta_p^2 = .025$

Note. Different subscripts within a row indicate differences between conditions of greater than Cohen's  $d = 0.20$ .



to conclude that the game did not substantially affect willingness to help. As such, the inclusion of this no-treatment control group constituted an important strength of Study 1, and the control group was retained in Study 2.

Willingness to help was assessed as the persuasive outcome measures, and immersion was assessed as a potential mediator. Because identification was not substantially affected in Study 1, it was not assessed in Study 2.

## Study 2

### Method

#### Participants

Ninety-four undergraduates volunteered to participate in the study in exchange for course credit. The average age was 19.32 ( $SD=2.12$ ); 83 participants were female. Age did not differ substantially between the conditions ( $\eta_p^2=.003$ ), and neither did the distribution of gender,  $\chi^2(2)=0.12$ .

#### Design and Procedure

The study employed a one-factor (medium: game vs. print vs. control) between-subjects experimental design. Undergraduate students who agreed to participate in the study were randomized into the three conditions, invited into the lab, and seated in individual soundproof booths, where they took part in the study ( $n_{\text{game}}=30$ ;  $n_{\text{print}}=28$ ;  $n_{\text{control}}=36$ ). Desktop computers were used to present the game and all questionnaires. The printed text was presented on paper. Participants in the game condition were instructed how to play the game, as in Study 1. Participants in the print condition were simply asked to read the entire printed text. This took approximately 2.5 min. The questionnaire was administered after the experimental manipulation and measured immersion and willingness to help, as well as demographic information.

#### Stimuli

As in Study 1, *Against All Odds* was used as the persuasive game. For the print condition, a text was constructed that had comparable content as the content provided in the game. Thus, the printed text essentially provided a description of events that could occur in the game (depending on the player's actions).

### Measures

**Immersion.** The same three items used in Study 1 were used to assess immersion in Study 2. The scale had good internal consistency ( $\alpha=.84$ ).

**Willingness to help.** Willingness to help was measured with the same seven items that were used in Study 1. In a factor analysis ( $KMO=.68$ ), the Scree test yielded a single factor, explaining 35.30% of variance. The Willingness scale had borderline acceptable internal consistency ( $\alpha=.67$ ). As in Study 1, and as in the previous study by Steinemann et al. (2015), we decided to aggregate the willingness to help items into a single willingness score. However, analyses were also run with the separate willingness items, which yielded similar results and identical conclusions. These analyses are not reported here for reasons of brevity.

### Data analysis

The same procedure was used as in Study 1. In addition to the variables just reported, participants' attitude with regards to refugees and empathy toward refugees was assessed, as were perceptions of narrative transportation, enjoyment, fun, and appreciation of the game, but the analyses concerning these variables are not reported here for reasons of brevity. The data and syntax can be found at <https://osf.io/3d8qa/files/>.

## Results

### Correlations

A correlation analysis revealed a small correlation between immersion and willingness ( $r=.19$ ).

### Differences between conditions

With regards to willingness to help, there were no substantial differences between the three conditions in terms of willingness to help ( $\eta_p^2=.019$ ). A second ANOVA revealed that there was a substantial effect of condition on immersion ( $\eta_p^2=.280$ ), with higher immersion in the print as compared to the game condition (see Table 2).

### Mediation analyses

As in Study 1, the game resulted in lower scores for immersion than the print condition. A mediation model in which the game would outperform the print condition in terms of immersion, and immersion would mediate the game's effects on willingness (RQ3), can be confidently rejected based on the present data.

**Table 2.** Results of the analysis of variance for immersion and willingness to help in Study 2.

	Game		Print		Control		Test Results
	M	SD	M	SD	M	SD	
Immersion	4.00 <sub>a</sub>	1.34	5.51 <sub>b</sub>	1.11	—	—	$F = 21.76, \eta_p^2 = .280$
Willingness to help	5.28	0.77	5.42	0.86	5.53	0.74	$F = 0.86, \eta_p^2 = .019$

Note. Different subscripts within a row indicate differences between conditions of greater than Cohen's  $d = 0.20$ .

## Discussion and introduction to study 3

Study 2 revealed no evidence that a persuasive game could be more persuasive than a printed text. Of interest, there was an effect of condition on immersion, with the print condition resulting in more immersion than the digital game, but immersion was not strongly associated with increased willingness.

Although the comparison of the persuasive game with a printed text detailing the (possible) events in the game allowed us to manipulate the medium while keeping the communicated information constant, there can still be subtle difference of style that could have influenced the results. Most notably, the conditions differed not only in terms of interactivity but also in terms of audio-visual presentation versus print presentation. This problem was remedied in a third study. In Study 3, which was conducted simultaneously with Study 2, we exposed all participants to the persuasive game but randomized participants into a play condition and a watch condition. In the watch condition, participants watched a video of a recorded gameplay session. Thus, participants in both conditions were exposed to audiovisual materials, communicating the same information. Interactivity was the only difference between the conditions in Study 3.<sup>1</sup>

Another change as compared to Study 1 and 2 was our procedure for assessing identification. In Study 1, there was no substantial difference in identification between the experimental conditions. It is notable, however, that our Identification scale, adopted from Peng et al. (2010), did not differentiate between different levels of identification. As discussed in the Introduction, several scholars make a distinction between wishful identification, similarity identification, and embodied presence (Van Looy et al., 2012). Wishful identification is not very relevant for the present research, as this is unlikely to occur with regards to refugees. Similarity identification and embodied presence, however, are highly relevant for the present purpose and are assessed independently.

Theoretically, it could be argued that similarity identification is equally likely in the game and video condition. It is well documented that stories transmitted by traditional media can result in perceptions of increased similarity between the self and the story's

main characters (Gabriel & Young, 2011) and generally in high levels of identification (see Introduction). With regards to embodied presence, on the other hand, it is possible that digital games have a distinct advantage over traditional, noninteractive media. After all, digital games constitute the only medium for which players are invited to actively step into the avatar's shoes and "the [feeling] of being embodied in the character" (Van Looy et al., 2012, p. 202) is necessary to truly enjoy the experience.

We therefore assessed similarity identification and embodied presence separately in Study 3. Willingness to help was assessed as the persuasive outcome measures. We formulated the following research questions:

RQ6: Will playing a persuasive game result in more similarity identification than being exposed to a video with equivalent persuasive information?

RQ7: Will similarity identification mediate the persuasive effect of the persuasive game?

RQ8: Will playing a persuasive game result in more embodied presence than being exposed to a video with equivalent persuasive information?

RQ9: Will embodied presence mediate the persuasive effect of the persuasive game?

## Study 3

### Method

#### Participants

In total, 161 participants took part in this study. The study took place at two secondary schools in the Netherlands. All respondents in this study were in their 5th year of secondary education and were between 16 and 18 years old. A slight majority was female ( $n = 85$ ); 11 participants failed to indicate their gender. Gender was equally distributed across the two conditions,  $\chi^2(1) = 2.67$ . As a reward for participation, a gift certificate for €7.50 was raffled.

#### Design and procedure

The study employed a Time (Pretest vs. Posttest)  $\times$  Medium (Game vs. Recorded Gameplay) within- and between-subjects design. Secondary school

students who agreed to participate in the study were randomized into the two experimental conditions ( $n_{\text{game}} = 87$ ;  $n_{\text{video}} = 74$ ). The study took place during social science classes. Students were invited into the school's computer lab and seated (with headphones) in front of individual desktop computers on which the game, the video, and all questionnaires were presented. A pretest was administered in which a baseline for willingness to help was assessed, as well as demographic information. Participants randomly assigned to the game condition were then instructed how to play the game. They were asked to play Levels 1–4 of *Against All Odds*. In case of failure, they were asked to try again and to keep playing until Level 4 had been finished. Participants in the video conditions were simply asked to watch the entire video. The posttest questionnaire was administered after the experimental manipulation and measured identification and willingness to help.

### Stimuli

As in Study 1, *Against All Odds* was used as the persuasive game. Playing Levels 1–4 took approximately 15 min for most participants. To be able to make a good and reliable comparison between the digital game and the video, the video contained parts of a recorded gameplay, which was edited for the purposes of this study. In this way, it was made sure that the content and information given to the participants was the same in both conditions. Two videos were made, one in which the main character is male and called Muhammad and one in which the main character is female and called Fatima, to be used for male and female participants, respectively. Both videos had the same content and were 6 min 12 s long. The video was somewhat shorter in length than a full playing session, but this was deemed necessary for two reasons. First, loading screens and nonplaying parts of the game were edited out of the video. Second, included in the full length of the gameplay session were instances in which players failed and had to start a level anew. We included several of such failure experiences in the video and followed these up with a repetition of part of the level in question. We did not repeat the entire level in case of failure, as this would have likely resulted in too much repetition. The videos can be found at <https://osf.io/3d8qa/files/>.

### Measures

**Identification.** To assess identification, two scales were employed, one for similarity identification and one for

embodied presence. For similarity identification, six items from Van Looy et al. (2012) were used. The questions all started with the stem “The main character ...” and ended in the following statements “thinks like me,” “behaves like me,” “is comparable to me,” “shares the same values as me,” “treats other people like I do,” and “looks like me.” A composite similarity identification score was calculated by averaging all six items ( $\alpha = .90$ ).

For embodied presence, six items were used. Three of these items were also adapted from Van Looy and colleagues (2012), asking participants to indicate the extent to which they agreed with the following statements: “I became one with the main character in the game/video,” “It felt like I was the main character in the game,” and “It felt like the movements of the main character in the game/video were my own.” Two of the items were adapted from Green and Brock (2000): “During the game/video, I had no attention for the real world” and “I lost myself in the game.” The final item was based on an item from Hefner et al. (2007): “The goals of the main character in the video/game became my own goals.” All identification items were questioned using a 7-point scale from 1 (*totally disagree*) to 7 (*totally agree*). The embodied presence score was calculated by averaging all six items ( $\alpha = .89$ ).

A factor analysis ( $KMO = .86$ ) showed a two-factor solution, explaining 66.40% of the variance. All six similarity identification items loaded on one factor, whereas the embodied presence items loaded on a second factor.

**Willingness to help.** A new Willingness to Help scale was assembled. Based on the limited internal consistency of the Willingness to Help scale in previous research (Steinemann et al., 2015), which was also observed in Study 1 and 2, we changed the wording of the Willingness items. Four items asked participants to indicate agreement with the statements “I would donate €1 to help refugees, if someone would ask for it”; “I would volunteer to help refugees, if someone would ask for it”; “I want to help refugees if I can”; and “I would be prepared to truly do something to help the refugees in our country.” The first two of these were adapted from Wilson and Lu (2008), and the last two were based on Koster (2007) and Peng et al. (2010). All Willingness items employed a 7-point scale from 1 (*totally disagree*) to 7 (*totally agree*). A composite willingness score was calculated by averaging all four items (pretest,  $\alpha = .88$ ; posttest,  $\alpha = .86$ ).

## Data analysis

We employed the same procedure as in Study 1 and 2. Because pretest and posttest data for willingness to help was available, however, we analyzed the effect on willingness with a repeated-measures ANOVA, using a 2 (time)  $\times$  2 (condition) mixed design. In addition to the variables just reported, participants' knowledge about refugees, perspectives on learning, and perceived persuasiveness of the stimuli were assessed, but the analyses concerning these variables are not reported here for reasons of brevity. The data can be found at <https://osf.io/3d8qa/files/>.

## Results

### Correlations

A correlation analysis revealed that there was a close to medium-sized correlation between similarity identification and embodied presence ( $r = .27$ ). There were no substantial correlations between similarity identification and posttest willingness ( $r = -.01$ ), between embodied presence and posttest willingness ( $r = .11$ ), and between pretest-willingness and similarity identification ( $r = .04$ ), but there was a medium-sized correlation between pretest-willingness and embodied presence ( $r = .32$ ).

### Differences between conditions

To investigate RQ1, a repeated measures ANOVA was conducted with condition and time as the independent variables and willingness to help as the dependent variable. The analysis revealed an interaction between condition and time that was not quite substantial ( $\eta_p^2 = .024$ ). As shown in Table 3, willingness to help increased slightly after watching the video and decreased slightly after playing the game, but these differences were small. Furthermore, similarity identification was higher in the video condition than in the game condition ( $d = -0.41$ ). With regards to embodied presence, there seemed to also to be a small-to-medium difference ( $d = 0.32$ ), this time with higher

embodied presence in the game condition than in the video condition (see Table 3).

### Mediation analysis

To investigate RQ7 and RQ9, mediation analyses are required. With regards to similarity identification, however, the game resulted in lower scores than the video, and so our hypothesized mediation model could be rejected outright. With regards to embodied presence, the game outperformed the video, but the zero-order correlation between embodied presence and posttest willingness was small. To further investigate the relationship between embodied presence and posttest willingness, a regression analysis with condition, similarity identification, and embodied presence as the independent variables, and posttest willingness as the dependent variable was conducted. This analysis revealed a slightly more substantial effect of embodied presence on willingness ( $r_{sp} = .17$ ). However, adding pretest willingness to the model as a covariate resulted in a negligible effect of embodied presence on posttest willingness ( $r_{sp} = -.02$ ). Thus, a mediation model in which the game would outperform the print condition in terms of embodied presence, and embodied presence would mediate the game's effects on willingness (RQ9), can be confidently rejected based on the present data. (The effect of condition on embodied presence was still small-to-medium in size when controlling for pretest willingness,  $\eta_p^2 = .038$ .)

### General discussion

In the present research we investigated whether a persuasive game, *Against All Odds*, would be more effective in changing willingness to help refugees than traditional, nongame messages. We also investigated whether immersion and identification would mediate the increased effectiveness of the persuasive game relative to the nongame messages. On the basis of previous research and construal-level theory (Liberman & Trope, 1998), we expected that persuasive information would be more self-relevant when communicated through digital games than when communicated through

**Table 3.** Results of the analysis of variance for identification and willingness to help in Study 3.

	Pretest				Posttest				Test results
	Game		Video		Game		Video		
	M	SD	M	SD	M	SD	M	SD	
Similarity identification	—	—	—	—	3.49 <sub>a</sub>	1.29	3.99 <sub>b</sub>	1.11	$d = -0.41$
Embodied presence	—	—	—	—	4.45 <sub>b</sub>	1.25	4.04 <sub>a</sub>	1.28	$d = 0.32$
Willingness to help	4.60 <sub>b</sub>	1.30	4.79 <sub>b</sub>	1.49	4.21 <sub>a</sub>	1.52	4.82 <sub>b</sub>	1.53	$F = 3.94, \eta_p^2 = .024$

Note. Different subscripts within a row indicate differences between conditions of greater than Cohen's  $d = 0.20$ .



traditional, noninteractive media (cf., Ahn, 2015). The overall pattern of results in the present three studies lends little support for this notion, however.

The results showed that the persuasive game in question was not more persuasive than a video and printed text in Study 1 and Study 2. In Study 3, moreover, a small negative effect of the persuasive game was found relative to a video: Whereas willingness to help increased from pretest to posttest in the video condition, a small decrease in willingness to help was found in the game condition. It is unwarranted, however, to conclude from this that *Against All Odds* backfired as a persuasive tool. After all, no negative effects were found for the game condition relative to the no-treatment control condition in Studies 1 and 2. It seems clear, however, that there was no evidence for a positive persuasive effect. Although this may be disappointing to some readers, it is not all that surprising in light of research on media effects more broadly. There is, after all, a rich tradition in media psychology and communication science arguing that media effects, even if they are theoretically plausible, are seldom large, automatic, or unqualified (Lang, 2013). Also notable is the perspective of “differential effects” (Valkenburg & Peter, 2013): Media effects may be large but are rarely so for large numbers of people. Perhaps future research on persuasive games should set out to investigate for whom and under which circumstances persuasive games can be effective.

So why did our game fail to persuade players? The results with regards to immersion and identification may provide a partial answer to this question. Studies 1 and 2 found that a video and text resulted in stronger immersion than the game, even when the text was designed to transmit the same information as was transmitted in the game (Study 2). In light of these findings, it cannot be concluded that digital games are always more immersive than traditional, noninteractive media and invariably provide players with self-relevant information. It seems that interactivity as such is not sufficient to decrease the psychological distance between the player and the content. Rather than assuming that digital games will always be highly self-relevant, researchers would be wise to focus on the specific characteristics that can increase or decrease self-relevance.

As an example, Crutzen and colleagues (2016) argued that immersion in persuasive games depends, among other things, on the extent to which the game satisfies the player's need for competence and the extent to which the game provides a captivating narrative. It should be noted that, although traditional media are perfectly capable of communicating

captivating narratives (e.g., Green & Brock, 2000), the potential for satisfying the player's need for competence is still a unique characteristic of digital games (Ryan, Rigby, & Przybylski, 2006). Satisfying players' need for competence may therefore be the key to increasing the persuasiveness of persuasive games. In the present research, it is possible that our digital game of choice, *Against All Odds*, did not sufficiently result in feelings of competence.

As such, the present research's null results may stem from selecting a particularly poor game rather than from a flaw in our reasoning concerning persuasive games, interactivity, and self-relevance. In the end, however, this is an empirical question, and more research on persuasive games is necessary before we arrive at this conclusion. It should also be noted that this explanation for the null findings constitutes a post hoc explanation. We feel that the limitations of *Against All Odds* are not so glaring that any video game scholar or professional would have confidently predicted a failure to find persuasive effects. At the same time, the strengths of *Darfur is Dying* and *My Cotton Picking Life* do not strike us as so apparent that the persuasive successes found by Peng and colleagues (2010) and Jacobs (2016) were inevitable from the outset. Future studies should pay attention to the particular characteristics that make a good persuasive video game.

Another possible reason for the present research's null results may be that participants in our game condition did not engage in particularly thorough information processing. In fact, immersion was higher in the video and text conditions than in the game condition in Studies 1 and 2. Does this mean that participants in those conditions paid closer attention to the transmitted information? Perhaps the experimental task of reading a text/watching a video activated the goal of careful information processing, whereas the experimental task of playing a game activated the goal of having fun. Perhaps also gameplay itself (what to do, which button to press, how to proceed in the game) was an obstacle to thorough information processing. Rather than *immersing* participants in the game, gameplay may well have *distracted* participants from the conveyed information about refugees and the refugee crisis. Unfortunately, this question cannot be resolved with the data at hand. Future research may employ recall and thought-listing measures (as well as perhaps manipulations of information-seeking vs. fun-seeking goals) to address this issue. If gameplay has the potential of distracting players from the information that is supposed to be transmitted, then this could seriously hamper the effectiveness of persuasive games. This may be an important research avenue to pursue.



Of all our findings, only the positive effect of the game on embodied presence (Study 3) is consistent with the notion that interactivity increases perceived self-relevance. It remains a possibility, then, that digital games have a distinct advantage over traditional, non-interactive media in terms of advancing “the [feeling] of being embodied in the character” (Van Looy et al., 2012, p. 202). After all, digital games constitute the only medium in which players are invited to actively step into the avatar’s shoes (Van Looy et al., 2012). Whereas similarity identification relies heavily on a cognitive process in which people *imagine* themselves in someone else’s shoes (Cohen, 2001), embodied presence constitutes a process in which there is a direct subjective experience of actually *standing* in someone else’s shoes (Van Looy et al., 2012). It is possible that this experience makes the persuasive information increasingly self-relevant, although it should be noted that embodied presence did not mediate a positive effect of persuasive games on willingness to help in Study 3. Future research should further investigate the role of embodied presence and the circumstances under which it is likely to facilitate persuasion.

### Limitations and strengths

The contribution of the present research should be seen in the context of its limitations and strengths. First, we used a self-report outcome measure. Future research should investigate effects of persuasive games on relevant behavioral measures. It should be noted, however, that using self-report assessment is likelier to result in an overestimation rather than an underestimation of the effect under investigation. After all, the Hawthorne effect, where participants are inclined to conform to what they think the researchers expect of them (Sedgwick & Greenwood, 2015), would render the present research more likely to overestimate rather than underestimate the effect of the persuasive game, especially when comparing the game condition to the no-treatment control condition in Studies 1 and 2, and the posttest with the pretest in Study 3.

The use of short-term assessment of the outcome measures may represent a bigger problem. A recent study has shown that a persuasive game aimed at influencing attitudes toward the homeless had a non-significant effect at the immediate follow-up but a significant persuasive effect after 3 weeks (Ruggiero, 2015). If *Against All Odds* had a similar effect on our participants, our use of only short-term outcome measures precluded us from finding this effect.

An important strength of the present research was the use of no-treatment control conditions in Studies

1 and 2 and a pretest in Study 3. This enabled a direct assessment of the change in willingness to help that could be expected from playing the game.

### Conclusions

The results of the present research serve as a warning that, notwithstanding their theoretical merit, not every persuasive game will be more persuasive than traditional, noninteractive media. Future research should focus on the game characteristics that contribute to the game’s persuasiveness. Possibly, games that satisfy players’ need for competence to a greater extent may be able to exert a greater effect. More empirical research is needed to confirm this expectation, however.

### Note

1. While the first and third author were collecting data for Study 2, the second and fourth author, at another institution, were collecting data for Study 3. No communication took place between the teams during the design and data collection stages, hence the subtle differences in operationalization between Studies 1 and 2, on one hand, and Study 3, on the other hand. When sharing our work afterward, the similarities between the studies (and the results) were so striking that it seemed only natural to report them together.

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