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Original Article

Procedural Arguments of Persuasive Games

An Elaboration Likelihood Perspective

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Abstract: Studies into the effects of persuasive games – games designed to change players' attitudes – have not yet yielded insight into the psychological processes involved in persuasion through procedural rhetoric. As a type of nonverbal argument embedded in game systems, it is an open question whether procedural rhetoric leads players to elaborate on a message in the same way as traditionally delivered arguments do. The current study tested 241 participants in a 2 (rhetoric strength) × 2 (level of cognitive load) between-subjects experiment, using game stimuli generated through analytical game design. Results indicate that procedural rhetoric strength meaningfully added to persuasive effects. Participants in high-cognitive-load conditions were not driven to process the games' message differently. We outline the empirical process required to further investigate effects of procedural rhetoric on elaboration, and conclude how the current study contributes to conceptions of arguments borne out through gameplay.

Keywords: persuasive games, effects, procedural rhetoric, elaboration, attitudes

As games specifically made to persuade their players, persuasive games inherently share a common goal that is nevertheless difficult to realize. Based on current knowledge around persuasive messages in other media and interpersonal communication (Perloff, 2014), changing individual players' attitudes after a single, brief exposure is an uphill struggle. At the same time, this is the format most persuasive games are published in, whether they intend to promote a brand (Lee & Youn, 2008; Roettl, Waiguny, & Terlutter, 2016) or highlight prosocial issues (e.g., Gerling, Birk, & Mandryk, 2014). Moreover, studies are showing that many (although not all) persuasive games are indeed changing their players' attitudes (Peng, Lee, & Heeter, 2010; Ruggiero, 2015; van't Riet, Meeuwes, van der Voorden, & Jansz, 2018). With the tentative conclusion that persuasive games work as intended, the focus can shift to how they work and to how the medium contributes to persuasion in general. The current article is intended to start this more specific process by applying a generalized persuasion model to a persuasive mechanism that is unique to games.

We work from the assumption that persuasive games differ from other media on their ability to carry procedural rhetoric (Bogost, 2007), a form of persuasive argument embedded in game systems and rulesets that can be unpacked by players through observing and responding to these rules – in essence, players are playing with(in) game systems. Naturally, persuasive games are not all reliant on procedural rhetoric; game developers have access to multiple persuasive dimensions through which games can convey messages (de la Hera Conde-Pumpido, 2015). As these dimensions hinge on, for example, narratives (Steinemann et al., 2017) and emotional appeals (Perloff, 2014), many of them have been explored in other persuasive media, while procedural rhetoric has not yet been empirically established. Finding proof for the influence of procedural rhetoric on player attitudes therefore builds up the case for persuasive games as a different type of persuasion, rather than simply an alternative. At the same time, knowing how gameplay elements can affect persuasion can generate insight into the psychological processes elicited by game-borne persuasion.

The question is therefore whether procedural rhetoric persuades players in the same way as verbally delivered arguments affect their listeners. In line with argument-focused persuasion research (Schreiner, Appel, Isberner, & Richter, 2018), we present a study that tested differences in procedural rhetoric not in terms of the rhetoric presence or absence – something that could arguably be done through the manipulation of interactivity (Peng et al.,

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2010) but that might have deleterious effects on the study's validity – but in terms of argument strength. For this investigation we drew on the elaboration likelihood model (ELM; Petty & Cacioppo, 1986b), a comprehensively validated model that gives a nuanced psychological view on how persuasion takes place in a wide range of contexts (Perloff, 2014). Our investigation was guided by the following research question: Do common results with verbal argument strength under the ELM replicate when arguments are procedurally embedded in a persuasive game? We first discuss the ELM before proposing how procedural rhetoric might map onto this model.

Elaboration and Game Persuasion

Under the ELM (Petty & Cacioppo, 1986b), verbal arguments are typically considered as strong or weak. Strong arguments make a solid, sensible point in favor of an attitude while weak arguments do not make sense or are easily countered with information that is readily available. These outcomes rely on the receiver's ability and motivation to elaborate – to consider the message consciously and actively weigh its arguments in one's mind. Seen in this way, the ELM describes the persuasive effects on individuals' attitudes by not just considering the message, but also the attention the central arguments of a message are given by receivers. Under conditions of full attention (i.e., when the receiver is able and willing to attend to a message), message recipients process the message through the central route, leading messages with strong arguments to be more persuasive than those with weak arguments (Petty & Cacioppo, 1986a). In fact, weak arguments can even lead to counter-attitudinal persuasion, whereby message recipients' attitudes shift toward the opposite of the message source's position (Perloff, 2014). If the receiver's ability and/or motivation to elaborate is low, messages only affect recipients through a peripheral processing route. Persuasion can still lead to attitude change under peripheral processing, but it is dependent on (contextual) factors outside of the strength of the message's arguments. Among others, these factors consist of presentation style, the sheer number of arguments, or the source of the message (Perloff, 2014). Attitude change resulting from peripheral processing is often short-lived compared with its centrally processed counterpart, as it is not anchored in personally held beliefs but instead in, for example, positive associations or source credibility.

Dual-process theories like the ELM have been of use in explaining previously anomalous effects in other fields. They can also help to explain surprising findings in studies on persuasive games, such as the sleeper effect (Priester, Wegener, Petty, & Fabrigar, 1999) the game *Spent* had on attitudes toward the homeless; even though *Spent* players could not be distinguished from participants who only read about this topic directly after exposure, they evinced continued attitude reinforcement in a post-test after 3 weeks, while readers returned to their previously held attitudes (Ruggiero, 2015). This finding implies sustained elaboration of the game's message. In fact, dual-process models have already been applied to brand recall in advergames (games that advertise a brand or product), leading to the conclusion that peripheral processing of a brand is preferred for these games (Peters & Leshner, 2013), at least for certain groups of players (Cicchirillo & Mabry, 2016). With the knowledge that peripheral processing can be elicited by suppressing message recipients' ability and motivation to elaborate, Peters and Leshner's (2013) conclusion has been applied to the development of advergames. One of the most common ways in which the ability to elaborate has been manipulated in previous research has been to increase receivers' cognitive loads (the information an individual is processing simultaneously). Players can be overloaded either with sensorial stimuli (Vyvey, Núñez Castellar, & Van Looy, 2018) or with cognitively highly demanding tasks (Evans & Hoy, 2016). In either scenario, players are distracted from scrutinizing the arguments in favor of a brand or product, opening players up for transfer of positive affect from the game to the brand (Cauberghe & De Pelsmacker, 2010).

The conclusion that advergames should invite peripheral processing to have the desired effects on players implies that these games offer weak arguments (or none at all) in favor of the brands they advertise. This line of thinking should not be extended to the medium as a whole, however. In a large-scale survey, Malliet and Martens (2010) correlated motivation to elaborate with knowledge acquisition from commercial (shooter) games. Although their study was not designed for causal inferences, their findings support the notion that games can persuade players who are able and willing to absorb whatever messages they offer. The sleeper effect found by Ruggiero (2015) supposes that even one-off persuasive game experiences elicit central processing routes. The advergames included in effect studies thus far might not emphasize the same range of persuasive strategies as advergames in general do (de la Hera Conde-Pumpido, 2017). With the goal of determining the argumentative power of games on an empirical level, we investigated a persuasive strategy unique to the medium: procedural rhetoric.

Operationalizing Procedural Rhetoric

Procedural rhetoric is a term coined by Bogost (2007) to describe how a developer can model certain aspects of the real world in a game to proffer a persuasive message. This concept is primarily based on simulations: Designers simulate elements of a real-world topic in gameplay systems and rules to persuade players on how the world works. *September 12th*, for example, offers a narrow simulation of the military strategy implemented by the United States in the years following the terror attacks on September 11, 2001 (Frasca, 2003). In this game, firing long-distance missiles at terrorists in a Middle Eastern setting inevitably causes collateral damage and grief among civilians, fomenting an increase in the number of terrorists that is nearly always on par with their losses (Jacobs, Jansz, & de la Hera Conde-Pumpido, 2017). By locking players into a Sisyphean cycle of violence, *September 12th* wordlessly paints a harsh indictment of the strategies applied in the US war on terror using only a handful of game mechanics. Players "play" the argument by engaging with gameplay systems, testing what consequences certain actions have. During play, they develop a mental model (Wasserman & Banks, 2017) of the game system in order to advance or understand the game. The nonverbal representation of the relationships and links between actors and objects in the game would then lead players to draw real-world conclusions about the simulated process.

Although the experience of procedural rhetoric by players has not yet been investigated, Sicart (2011) supposed that players may act in unpredictable ways that transcend the intended procedures, for instance by misunderstanding the processes or appropriating the simulation to play in ways the designer did not intend. Going beyond Sicart's concerns, game rules are intricately interconnected, and changing individual design parameters can and often does produce "ripple effects" that alter the interpretation of related procedures. Similarly, the context of a persuasive game, such as its mode of distribution and the intended play setting, tangibly influence the players' interpretation and their attentiveness toward the intended meanings (De Grove, van Looy, Neys, & Jansz, 2012; Deterding, 2016). While it is important to acknowledge these constraints, procedural rhetoric is a useful concept in this stage of researching the effects of persuasive games as it provides an analytical framework within which we can make reasonably well-defined changes and interpret the corresponding impact on players.

Considering procedural rhetoric from an elaboration-likelihood perspective generates some concrete predictions. Although the persuasive actuator of procedural rhetoric is nonverbal, it is not likely to act (solely) as a peripheral cue. Its nature as a simulation of real-world processes requires players' reflective capacity to relate the two, building mental models with the game that then inform thinking about real-world issues. Such a transfer process requires elaboration, which often includes relating message arguments to relevant personal experiences (Perloff, 2014), as a part of central route processing. At the same time, procedural rhetoric is often tied to progression within the game, in that players who understand a game's systems are likely to progress more easily than players who do not. This has two implications. Firstly, any player who finishes a game is more likely to have (on some level) processed its procedural arguments, and in-game progress could therefore act as an indicator of player understanding – something which cannot be assumed for written or spoken arguments. Combining this with Juul's (2013) contention that players' goals are aligned with those of their in-game avatar at least as long as a play session lasts lends credence to the idea that playing a game with procedural rhetoric does more than merely enable reflective self-persuasion; rather, it seems to facilitate it and even require it (to some degree) to complete the game. Secondly, the relationship of procedural rhetoric to ability and motivation to elaborate could be different than that of traditionally delivered arguments. Even leaving out the motivational effects of playing games that reflect the real world in some way (Malliet & Martens, 2010), the ability to elaborate could be affected by the complexity of the same simulated systems that require this elaboration. Put simply, the gameplay needs to strike a balance between the depth of the simulation and the cognitive limits of players for the procedural rhetoric to be persuasive.

The dearth of empirical evidence on procedural rhetoric might be explained by the difficulty inherent in isolating and manipulating this factor in the context of a behavioral science study. Nearly any gameplay system *could* say something about the real world, and these systems are intertwined with the other persuasive dimensions often emphasized in persuasive games, such as their narratives (de la Hera Conde-Pumpido, 2017). Changing one part of a game's design could affect other mechanisms, making it harder to differentiate them through experimental manipulations. By the same token, interactivity is a prerequisite for procedural rhetoric, as it affords players the agency to play within the system. Removing interactivity can adversely affect the impact of a persuasive game (Peng et al., 2010), but this change affects more than just procedural rhetoric, as this persuasive mechanism hinges on a discernible linking of the game systems and a real-world topic. Many branded puzzle advergames (e.g., the M&M-branded match-three game tested by Redondo, 2012) clearly are not intended to exert a message through their systems, and so could not be said to include procedural rhetoric despite being interactive. To summarize, interactivity is a necessary but not sufficient condition for procedural rhetoric, but manipulating it would cause too many changes to safeguard methodological validity.

In this study we propose to manipulate the strength, rather than inclusion, of procedural rhetoric in an existing persuasive game. This minimizes the ripple effects on the other experiential aspects of playing a persuasive game, as the interactivity, narrative, and presentation are unaffected, while at the same time bringing the study in line with elaboration research for written and spoken arguments. The strength manipulation was achieved by tweaking game system parameters to affect the link to the process the system aimed to model. Weak procedural rhetoric would reflect the process poorly (e.g., exaggerating it beyond credibility or changing causal links), while strong rhetoric would provide a consistent, believable model. We followed the process of analytical game design (Werning, 2019) to plan these tweaks to be minimal and carefully controlled so that other parts of the game experience unrelated to the systems were not affected.

If procedural rhetoric is processed by players in the same way as verbal arguments, we expect to replicate traditional ELM studies (Petty & Cacioppo, 1986a, 1986b). When respondents in these original studies were able and willing to attend to a message's arguments, persuasive effects were largest for strong arguments and smallest for weak arguments. Participants who were not in a capacity to elaborate (through a lack of motivation or ability) were still persuaded – although not as strongly – regardless of argument strength. This leads us to formulate the following hypotheses for the current study:

> *Hypothesis 1 (H1)*: Weak procedural rhetoric is less persuasive than strong procedural rhetoric when players are able and motivated to elaborate.

> *Hypothesis 2 (H2)*: Ability and motivation to elaborate moderates the influence of procedural rhetoric strength, whereby the difference between strong and weak rhetoric becomes smaller (or disappears entirely) when players are not able and/or motivated to elaborate.

Method

Ethics Statement

This research was registered with the Institutional Review Board of the Erasmus University Rotterdam. Informed consent was obtained digitally from all participants. The study's design and analysis procedures were pre-registered with the Open Science Framework prior to data collection at: https://osf.io/9ujeq

Design

The current study sought to replicate traditional ELM findings through a 2 (procedural rhetoric strength) × 2 (ability to elaborate) factor between-subject experiment with pre- and post-test attitude scales. An existing persuasive game that was previously validated (Jacobs, 2018) was modified to generate stimuli for these four conditions. Similar to Vyvey et al. (2018), the ability to elaborate on the game's persuasive message was manipulated by overloading participants in relevant conditions with audiovisual stimuli. This was done to distract players and increase cognitive load, in turn reducing their ability to elaborate.

Sample

A total of 241 participants were drawn from an international sample at a European university. Because of the international sample, respondents were asked to choose between Dutch and English versions of the survey part of the study. Most participants (70.5%) completed the survey in English with the remaining respondents participating in Dutch. There were more female (70.1%) than male participants, and the average age in the sample was 21.70 (SD = 2.41), ranging from 18 to 33 years. The sample did not consist of self-styled "gamers": Although 83.8% had a little (33.2%), some (36.1%), or a lot (14.5%) of experience with games, 81.3% did not consider themselves gamers. Survey language, gender, and identification as a gamer did not significantly influence any of the main variables in this study, and randomization ensured a balance of gender and country of origin across this study's conditions. Participants were given a cash participation reward upon completing the study.

Stimuli

The persuasive game *My Cotton Picking Life* (*MCPL*; Rawlings, 2012) served as the object of this study. *MCPL* is a short, simple, English-language game about cotton-picking practices in Uzbekistan that involve child slave labor (Jacobs et al., 2017). As part of a series of "newsgames," *MCPL* aimed to raise awareness and persuade its players about the severity of the problem. Its gameplay consists of repetitively clicking on-screen buttons to pick handfuls of cotton, with the end goal being to gather 50 kg. As each click only yielded approximately 1.5 g, it would take around 6 hr of continuous play to complete the game. Instead, players are expected to give up early and click on a third button that simply reads, "Alright, I've had enough," whereupon they are confronted with how little they would have earned and how Uzbekistani children are unable to choose to stop working. The game's strict limits on player choice, endless and unengaging gameplay actions, and progress feedback together formed the procedural rhetoric it leveraged to change attitudes. Even though an average session with the game lasted for less than 2 min, the game was previously found to be more effective at persuading players with regard to perceptions of the workload involved in cotton picking than a YouTube video on the same topic (Jacobs, 2018).

MCPL was re-created and modified using *Game Maker Studio*, a 2D game engine originally conceived by Mark Overmars for educational purposes that has been developed further by YoYo Games. The redesign process followed the analytical game design framework (Werning, 2019), a practice-based game research heuristic that seeks to explore how games operate as an expressive medium by iterating upon and comparing small-scale playable vignettes. The procedural rhetoric in the original game was regarded as positing a strong argument. In its redesign, the game's message was weakened by changing one parameter of the game's cotton-picking mechanic: Every button clicked in the weak procedural rhetoric condition yielded around 2,000 g (rather than 1.5 g) of cotton. This allowed players to easily gather the 50-kg daily quota. Players reaching this goal received a notification that they were done for the day, whereupon the game would immediately start a second day with another 50 kg to pick. Through this small numerical change, the game system became a worse reflection of the real-world practice of cotton picking while maintaining the same textual and visual message.

A second modification was performed to manipulate players' cognitive load. The modification was limited to audiovisual effects to increase players' cognitive load without affecting the game's primary persuasive actuators. The manipulation consisted of four elements. Primarily, Uzbekistani folk music was added, with the sound quality modulated for a diegetic, AM-radio effect. As music could conceivably affect the experience in other ways, the same score was included in all four conditions, differing only in volume and the amount of white noise that was audible. The test environment's volume level was controlled. Three visual effects were also added: a grainy distortion effect, small birds flying in flocks across the top half of the screen, and pulsating "Pick cotton" and "Alright, I've had enough" buttons. These effects were not seen as affecting the game's experience or persuasive mechanisms, and therefore they were only included in the high-cognitive-load condition. Participants in this condition sometimes indicated during debriefing that they experienced the game (sound and visuals) as "annoying," "loud," and "intense."

Measures

Table 1 lists of the items and scales used for the analyses in this study.

Attitudes

A previous investigation into the effects of *MCPL* applied proprietary attitude scales developed to gauge those attitudes the game and video were aiming to change (Jacobs, 2018). *MCPL* was more effective in changing attitudes related to workload, presumably because of its procedural rhetoric. The four workload-related attitude items (e.g., "Harvesting cotton by hand is hard work") were re-used here, and further items were generated relating specifically to aspects of workload. Items were added on the physical pain ("Harvesting cotton by hand is painful work"), task monotony ("Harvesting cotton by hand is boring"), and futility ("Harvesting cotton by hand feels like an endless task"). This generated an 11-item scale that yielded scores with adequate interreliability (Cronbach's $\alpha_{\text{pre-test}} = .73$, $\alpha_{\text{post-test}} = .81$). To measure those attitudes that the game also affected outside of its procedural rhetoric (primarily through its atmosphere and textual elements), items from the previous study on *MCPL* relating to slavery and empowerment to change it (e.g., "I want to make a change to stop forced labor") were re-applied here, resulting in a 3-item empowerment scale after factor and reliability analyses ($\alpha_{\text{pre-test}} = .72$, $\alpha_{\text{post-test}} = .74$). The difference scores between the pre- and post-test scales were used in the analyses for this study.

Manipulation Checks

Two measures were included to ascertain whether the study's manipulations on cognitive load and procedural rhetoric strength affected participants as predicted. The NASA Task Load Index (TLX) measures subjective workload perceptions (Hart & Staveland, 1988), and has previously been applied to measure cognitive load in research on digital games (Sharek & Wiebe, 2014). As a likely result of the unattainable goal of *MCPL*, one item ("How successful were you in accomplishing what you were asked to do?") did not meaningfully add to the reliability. After dropping this item, the average scores of the 5-item scale displayed adequate interreliability ($\alpha = .72$). As the first experimental effect study to operationalize procedural rhetoric, we sought to include an antecedent of attitude change as a result of procedural argument strength. A 5-item scale was developed, based on Williams and Williams's (2007) concept of *cognitive identification*, which "develops when players identify the structure (design aspects and results) of the simulation they are playing with reality (e.g., they see the game as a true and accurate representation of the way things are)" (Williams & Williams, 2007, p. 3). The scale measured the degree to which the gameplay reflected real-world systems by including items such as "This game shows what it feels like to pick cotton" and "The way this game works reflects what it is like to pick cotton in the real world." Overall, the scale's scores showed adequate interreliability ($\alpha = .75$).

Table 1. Item list of the scales used in this study

Item	*M* (pre)	*SD* (pre)	*M* (post)	*SD* (post)
Workload attitudes (α: pre: .73, post: .81)				
I would be able to pick 50 kg of cotton with little trouble. (R)	5.40	1.48	6.06	1.40
Harvesting cotton by hand is hard work.	6.17	0.76	6.34	0.98
Harvesting 50 kg of cotton by hand takes a long time.	6.21	0.82	6.45	0.99
Cotton harvesters in some countries have to meet large quotas daily.	5.97	1.01	6.34	.92
Cotton harvesters in some countries are not paid according to their efforts.	6.27	1.00	6.40	1.01
Harvesting cotton by hand is good for your physical fitness. (R)	4.64	1.40	5.51	1.48
Harvesting cotton by hand is painful work.	5.39	1.07	6.02	1.04
Harvesting cotton by hand is boring.	5.83	0.96	6.62	0.74
I would go crazy if I had to pick 50 kg of cotton every day.	6.21	1.10	6.56	0.98
Harvesting cotton by hand feels like an endless task.	5.84	1.11	6.55	0.91
Harvesting cotton by hand is work that offers a bright future. (R)	6.00	1.18	6.36	1.07
Empowerment attitudes (α: pre: .72, post: .74)				
It is important to me that people who made the products I use are treated fairly.	6.00	1.03	6.20	1.02
Forced labor is a big problem in the world right now.	6.17	0.84	6.25	0.82
I want to make a change to stop forced labor.	5.50	1.01	5.85	1.02
NASA Task Load Index (α: .72)				
How mentally demanding was the experience?			4.32	5.22
How physically demanding was the experience?			5.31	6.56
How hurried or rushed was the pace of the experience?			9.94	6.02
How hard did you have to work to accomplish your level of performance?			7.84	7.26
How insecure, discouraged, irritated, stressed, and annoyed were you?			11.89	5.79
Cognitive Identification (α: .75)				
This game lets players experience cotton picking in a safe way.			3.84	1.19
This game shows what it feels like to pick cotton.			2.96	1.27
The way this game works reflects what it is like to pick cotton in the real world.			2.56	1.22
This game is meant to be played for its message, not just for fun.			4.63	0.67
The way this game is played makes me think of what it is like to be forced to pick cotton.			4.12	1.04

Note. Means and standard deviations are shown for each item and scale, and Cronbach's α is shown for relevant scales. (R) = Item recoded before analysis.

Procedure

The study was performed in a laboratory setting to avoid potential uncontrolled distractors. Participants followed the study on computers through a web-based survey suite while wearing headphones, while an experimenter was present (although not in sight) throughout. Participants were randomly assigned to the four conditions by the survey software. They were asked not to adjust the volume of their headphones themselves and were instructed to "undergo the experience" for as long as they wished. This was done to allow all participants to engage with the games' procedural rhetoric at their own pace. The first part of the survey consisted of the attitude pre-test, followed by demographic items. Before starting the games themselves, participants were instructed through on-screen text to behave as if they had some time available and found a game while browsing online. Directly after quitting the game, participants filled in the TLX, followed by the post-test attitude scales and the cognitive identification scale. Upon completing the survey, participants were debriefed. Debriefings focused on letting participants talk about the game they played and their perception of it as a persuasive message.

The decision to let participants decide how long they wanted to play each of the games led to differences in stimulus exposure time. A two-way ANOVA for rhetoric strength and cognitive load manipulation on exposure time demonstrated a small effect of rhetoric strength, $F(1, 237) = 4.36$, $p = .038$, $\eta_p^2 = .02$, while a similar effect of level of cognitive load was not significant at .05 level, $F(1, 237) = 3.69$, $p = .056$, $\eta_p^2 = .02$. The strong-rhetoric, high-cognitive-load condition had the longest average exposure time ($M = 537.71$ s, $SD = 413.38$), differing significantly from the condition with the lowest exposure time, the weak-rhetoric, low-cognitive-load condition ($M = 355.69$ s, $SD = 255.36$) on a nonparametric test: Kruskal–Wallis $\chi^2(3) = 9.92$, $p = .019$. The other two conditions did not differ significantly from any other conditions (strong rhetoric/low cognitive load M = 457.32 s, SD = 326.28; weak rhetoric/high cognitive load M = 449.73 s, SD = 393.13). We suspect that higher cognitive load and stronger procedural rhetoric both caused respondents to play for slightly longer.

Results

Confirmatory Analyses

The first hypothesis posited that weak procedural rhetoric would be less persuasive than strong procedural rhetoric when players are able to elaborate, while the second hypothesis predicted an interaction effect of rhetoric strength and ability to elaborate, whereby differences between the procedural rhetoric groups would be less pronounced (or disappear) under high cognitive load. Both hypotheses were tested with a multivariate ANOVA with the workload and empowerment scales as dependent variables and a planned contrast of the two procedural rhetoric groups under low cognitive load. Of the full model, a main effect of rhetoric strength was significant, Wilk's λ = .92, $F(2, 236) = 10.43$, $p < .001$, $\eta_p^2 = .08$, with a medium effect size, while the main effect of cognitive load, Wilk's λ = .99, $F(2, 236) = 1.00$, $p = .370$, and the interaction effect between the two factors, Wilk's λ = 1.00, $F(2, 236) = 0.09$, $p = .916$, did not reach significance. Specifically, procedural rhetoric strength affected the workload attitudes, $F(1, 237) = 20.87$, $p < .001$, $\eta_p^2 = .08$, but not the empowerment attitudes, $F(1, 237) = 0.66$, $p = .419$. The planned contrast for the low-cognitive-load conditions showed this same difference for workload (contrast estimate = 0.34, $p = .001$, 95% CI [0.14, 0.53]) but not for empowerment (contrast estimate = 0.03, $p = .695$, 95% CI [−0.18, 0.12]). The effect size of the difference on the workload attitudes (strong rhetoric/low cognitive load M = 0.64, SD = 0.47; weak rhetoric/low cognitive load M = 0.30, SD = 0.47) was considerable (Hedges' gs = .71). As procedural rhetoric strength affected workload attitudes in the hypothesized direction for the low-cognitive-load conditions, H1 was retained. The absence of an interaction effect for either of the two dependent variables led us to reject H2, as it meant the high-cognitive-load conditions did not have smaller differences in the effect of rhetoric strength. Figure 1 shows the distribution of workload attitude change across the four conditions.

Exploratory Analyses

The confirmatory analyses showed that there were no effects of the cognitive load manipulation on either attitude scale. To follow up on this null result, the TLX scale scores used to indicate cognitive load were entered into subsequent analyses. As we expected TLX scores to be higher for high-cognitive-load conditions without differing between procedural rhetoric strength, we performed a two-way ANOVA with both factors and the TLX as the dependent variable. In this model, procedural rhetoric strength had a significant main effect, $F(1, 237) = 13.29$, $p < .001$, $\eta_p^2 = .05$, while cognitive load did not, $F(1, 237) = 0.86$, $p = .354$. Respondents who played the games with strong procedural rhetoric (M = 9.26, SD = 4.18) reported higher subjective workload than those who played the games with weaker arguments, M = 7.32, SD = 4.11, $t(239) = -3.64$, $p < .001$, Hedges' gs = .47. This means that the cognitive load manipulation very likely failed; participants did not perceive their workload to be higher in the high-cognitive-load conditions. At the same time, stronger procedural rhetoric did result in higher perceived workloads.

It is possible the subjective workload of the task the respondent was performing (as measured by the TLX) was conflated with the workload of picking cotton in a field in Uzbekistan influenced by the games' procedural rhetoric (i.e., the attitude scale applied in this study). To investigate this further, we performed stepwise linear regressions separately for low- and high-cognitive-load conditions. In the first step, the TLX scores were set to predict workload attitudes, with the procedural rhetoric manipulation entered in the second step. Table 2 displays the results of this analysis. Perceived current-task workload predicted cotton-picking workload attitude change only in low-cognitive-load conditions, although it did so independently of procedural rhetoric strength. Moreover, uncontrolled individual differences in perceived workload were about equally strong in their predictive power ($\beta_{TLX} = .32$ and $\beta_{rhetoric} = .29$) under conditions of low cognitive load, while rhetoric strength was the only predictor of attitude change under high cognitive load.

Finally, the cognitive identification scale was analyzed for its viability as an antecedent of attitude change resulting from manipulations in procedural rhetoric. A two-way ANOVA with both manipulated factors indicated that procedural rhetoric had a strong effect on cognitive identification, $F(1, 237) = 37.62$, $p < .001$, $\eta_p^2 = .14$, while cognitive load exerted no such influence directly, $F(1, 237) = 0.03$, $p = .858$, or in interaction with procedural rhetoric strength, $F(1, 237) = 0.37$, $p = .546$. Strong rhetoric conditions showed a large difference in cognitive identification (M = 3.91, SD = 0.66) from weak rhetoric conditions, M = 3.34, SD = .77, $t(239) = -6.15$, $p < .001$, Hedges' gs = .79. To determine whether cognitive identification mediated the relationship between rhetoric strength and workload attitudes, a mediation analysis was performed using PROCESS (Hayes, 2013). Apart from a direct effect on workload attitudes, $F(1, 239) = 21.03$, $p < .001$, $R^2 = .08$, $\beta = .28$, rhetoric strength also

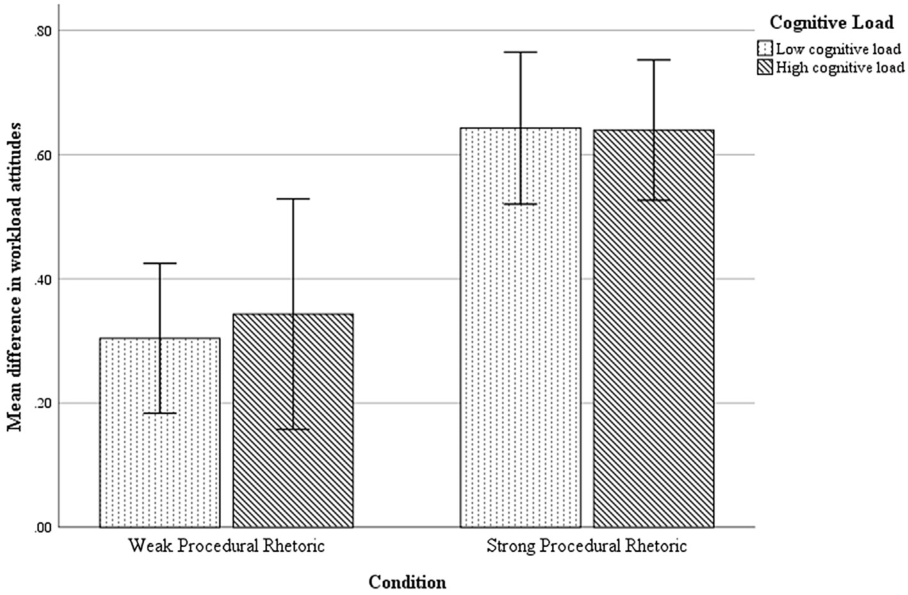

Figure 1. The distribution of average attitude change under strong and weak procedural rhetoric and low and high levels of cognitive load. Error bars denote 95% confidence intervals.

Table 2. Stepwise regression analysis for workload attitude change

	Low distraction			High distraction		
Step and predictors	R^2	ΔR^2	β	R^2	ΔR^2	β
Step 1	.13***			.00		
NASA TLX			.36***			.01
Step 2	.21***	.09**		.06*	.06**	
NASA TLX			.32***			−.07
Procedural rhetoric strength			.29**			.27**

Note. NASA TLX = NASA Task Load Index. *$p < .05$. **$p < .01$. ***$p < .001$.

directly influenced cognitive identification, $F(1, 239) = 37.84$, $p < .001$, $R^2 = .14$, $\beta = .37$. In the total model, the effect of rhetoric strength became smaller, $F(2, 238) = 21.63$, $p < .001$, $R^2 = .15$, $\beta_{cognitive} = .29$, $\beta_{rhetoric} = .18$, showing a small indirect effect through cognitive identification ($R^2_{med} = .05$, 95% CI [.02, .10]). Cognitive identification partially mediated the effect of rhetoric strength on attitude change, and thus functioned as a psychological antecedent of attitude change resulting from procedural persuasion.

Discussion

The current study intended to investigate the way procedurally delivered arguments are processed by players. If strong arguments embedded in the gameplay of a persuasive game would have resulted in the strongest attitude change when players were not distracted and less (or no) change when they were, it would have been proof that players elaborate on procedural rhetoric in much the same way as they do verbally delivered rhetoric. This distinction was not found; although weaker and stronger procedural rhetoric led to robust, noticeable differences in attitude change, players who were in the high-cognitive-load condition did not react differently than players in the low-cognitive-load condition. Although this could have been an indication that elaboration on procedurally delivered arguments occurs in a different way than with verbally delivered arguments, the manipulation check in the form of the NASA Task Load Index provided evidence for a simpler conclusion: The cognitive load manipulation was not successful in preventing players from elaborating. As far as we were able to tell, participants' ability to elaborate on the game's message was not affected by the sensory bombardment they were exposed to.

One surprising difference between the high- and low-cognitive-load conditions remained. The experience of subjective workload did not predict cotton-picking workload attitudes for distracted players as it did for the low-cognitive-load participants. Thus, although discrepancies were not found in cognitive load or attitude change, some part of the experience of the game was different between the two conditions. This difference caused players in the low-cognitive-load condition to display greater attitude change if their individual workload perceptions were higher. Whether high-cognitive-load players were better able to unlink perceptions of their own workload from those of their in-game avatar because of, for example, greater perceived distance or reduced identification or whether another factor was in play cannot be determined with the current dataset. The 88-s average difference in playing time between cognitive load conditions found in this sample might factor into this finding.

Manipulating procedural rhetoric by tweaking a small aspect of the game's design (i.e., the amount of cotton picked per button press) affected the persuasiveness of the overall game. This part of the game's message was chiefly focused on how hard it is to pick the daily quota of cotton, which means the null results found with regard to the empowerment scale are easily interpreted. The manipulation was specific to that part of the message that hinged on the game's procedural rhetoric, and thus the conditions' differences on the workload scale can also be said to have been borne out through gameplay. This study provided proof that procedural rhetoric meaningfully added to the overall persuasive heft of a game. Further support for this contention comes from the (partial) mediating effect of cognitive identification: More participants saw the way the game reflected cotton-picking practices as realistic in the strong procedural rhetoric conditions and their final attitude change was greater as a result.

Limitations and Future Research

Although the current study provided evidence of the efficacy of procedural rhetoric, it was not able to force players into a peripheral processing route. This leads to three considerations for future studies into this topic. Firstly, the choice for *My Cotton Picking Life* as a stimulus was made on the basis of its accessibility and previously found indications of its effects (Jacobs, 2018). One drawback of *MCPL* was that its persuasive heft was predicated on the visualization of intense physical labor. The NASA Task Load Index used in this study gauged the respondents' *own* sensation of workload, but its applicability as an indicator of greater cognitive load meant it needed to be applied directly after playing the game. It is likely this influenced respondents to conflate their own sense of workload with that of their in-game character. Future studies should therefore include a game in which the procedural rhetoric is unrelated to task load. This also problematizes the use of certain other high-profile persuasive games, such as *September 12th* (Frasca, 2003) – discussed in the Introduction – because its message is also constructed from the unwinnable nature of the conflict it portrays (Ruggiero & Becker, 2015). Future investigations into games in which the message cannot be disentangled from cognitive load could consider thought-listing directly following play to act as a direct indicator of players' elaboration on the topic (Tormala, Briñol, & Petty, 2007). Although careful controls are necessary, employing a thought-listing method after some time has passed since exposure to a persuasive game would also shed more light on the strength of the elaboration process that a game could set in motion.

Second, cognitive load elicits peripheral processing only when respondents cannot recover from its effects. By giving players the time to play the game until they felt they were done, a compensation effect might have been created where players first had to adjust to the visual and aural intensity of the high-cognitive-load version of the game before being able to tune it out. Judging from the fact that differences in exposure time were erratic, there is good reason to control this factor. Increasing players' cognitive loads can be done in several ways. First, *MCPL* is a rather sedate game, free of time pressures or action-based gameplay. Including stimuli with more frenetic gameplay or indeed any kind of fail state could rush the players, thereby lowering their ability to elaborate on messages (pushing them into peripheral processing routes). Second, the intensity of the distracting elements could simply be increased. Adding a reaction time task to be performed concurrent with play would at the same time present the chance to elicit peripheral processing by manipulating the interval between prompts (i.e., shorter intervals hinder elaboration more than longer ones) as well as act as an indicator of cognitive load by measuring response latencies (Lang, Bradley, Park, Shin, & Chung, 2006). Alternatively, the games could be tweaked to be more personally relevant, triggering players' elaboration by personalizing the experience for one condition and keeping it generalized for the other (Malliet & Martens, 2010; Perloff, 2014). With any of these solutions, however, care must be taken not to disturb the stimulus game's procedural rhetoric. The interconnected nature of game-borne persuasion means any change is likely to have ripple effects throughout the experience. Fail-states, time pressure, and even concurrent tasks could influence what kind of choices a player can make and affect their mental models beyond the ability to construct them through elaboration. In other words, we urge investigators to prioritize a congruent matching of gameplay modifications with any attempt to manipulate players' abilities and motivation to elaborate.

The cognitive identification scale developed for this study was operationalized from Williams and Williams's multiple identification theory (2007) to provide support for our contention that any attitude difference between conditions was indeed the result of procedural rhetoric. The finding of partial mediation of the procedural rhetoric manipulation on attitude change by cognitive identification supported the use of this scale. We suggest that future endeavors to uncover the additive effects of procedural rhetoric in persuasive games should include a variation on this measure that is specifically tuned toward the rhetorical goals of those games.

Conclusion

On the basis of the results of this study, we can conclude that procedural arguments can be an important part of the way in which games persuade their players. The current result corroborates the long-standing supposition in game studies that procedural rhetoric can act independently of verbal arguments (Bogost, 2007), by letting players act within a system to reach their own conclusions about the real-life processes it simulates. This type of rhetoric is warranted in cases where an agentic perspective is needed. Although the current study cannot bring conclusive results to the concept of elaboration on game-based messages, it was intended to help develop a method by which this goal can be reached in a realistic, reproducible way. Above all, the current study offers further insight into operationalizations and effects of procedural rhetoric, a persuasive dimension that is unique to games, with the end goal of determining what role games can play in the broader landscape of persuasive communication.

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