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Can a Video Game with a Fictional Minority Group Decrease Intergroup Biases towards Non-Fictional Minorities? A Social Neuroscience Study

Guillaume P. Pech[a] and Emilie A. Caspar[a,b]

[a]Center for Research in Cognition and Neuroscience, Université libre de Bruxelles, Brussels, Belgium; [b]Social and Moral Brain Lab, Department of Experimental Psychology, Ghent University, Ghent, Belgium

ABSTRACT

A critical scientific and societal challenge involves developing and evaluating interventions that reduce prejudice towards outgroups. Video games appear to be a promising method, but several holes in the current scientific literature prevent fully understanding the sizeable potential impact of video games on reducing prejudice. The present study investigated to what extent a video game designed to reduce prejudice towards minorities in a fictional society has the potential to reduce prejudice towards non-fictional minorities. Participants played either a recently developed game designed to reduce prejudice towards non-fictional minorities (hereafter referred to as the test game) or a control game. After playing at home, participants performed two tasks in a lab context. We observed overall a positive effect of playing the test game compared to the control game on attenuating prejudice towards an outgroup individual. We indeed observed that players of the control game had increased midfrontal theta activity, reflecting a higher degree of cognitive conflict when they acted prosociality towards the outgroup participant and a lower neural response to the outgroup participant's pain than compared to the ingroup participants. These effects were attenuated for players of the test game. We also observed that players of the test game had a higher sense of agency when they decided to help the outgroup participant than when they did not help the outgroup participant, an effect not observable in control game players. These results are promising as they support evidence that using fictional characters in video games may induce positive changes toward non-fictional individuals.

1. Introduction

Developing cooperative social relationships between groups is a fundamental challenge for human societies, especially in the context of mundialization, where people from diverse backgrounds and cultures are brought to live together. Therefore, a critical scientific and societal challenge consists in developing and evaluating interventions that improve social relationships between groups and reduce prejudice towards people considered as "outgroup" (Paluck et al., 2021).

Prejudice can be defined as more negative attitudes toward another group (i.e., the outgroup) and more positive attitudes toward one's own group (i.e., the ingroup) (Greenwald & Pettigrew, 2014). Prejudice can take many forms, ranging from negative thinking, antisocial attitudes, reduced prosociality, social exclusion, hate speech or, in its more extreme forms wars and genocides. The scientific literature has witnessed the development of a high range of different approaches, all aiming to reduce prejudice between groups (Paluck & Green, 2009a). They range from meditation- and mindfulness-based mental training interventions (Berry et al., 2021), contact with the outgroup (Ellison et al., 2011; Miller & Brewer, 1986; Tajfel, 1970), raising consciousness (Blair, 2002), instructions to be empathetic when reading about everyday discrimination (Stephan & Finlay, 1999), or entertainment activities, such as reading (Cameron et al., 2006; Wham & Others, 1996), listening to educational radio soaps (Paluck & Green, 2009b) or playing video games (Adachi et al., 2015).

Among the different intervention methods used, video games appear to be one of the most promising in their potential to reduce prejudice between groups. First, video games are the fastest-growing form of entertainment (e.g., Adachi et al., 2015), with billions of gamers worldwide. Second, several studies have shown that imagining contact with an outgroup is sufficient to reduce prejudice towards those outgroup members (Cameron et al., 2006; Crisp & Turner, 2009). Thus, video games have the potential to be a critical method for fostering contact between individuals from diverse groups and reducing intergroup biases (Stiff & Bowen, 2016; Stiff & Kedra, 2020). Third, young adults are highly vulnerable to xenophobic narratives towards outgroups and violent practices spread by extremists (Jahnke et al., 2020). Young adults are also the largest social media and online game user demographic (Robinson & Whittaker, 2021). On the Internet, violent extremists often use online social networks and platforms to incite violence, recruit

CONTACT Emilie A. Caspar Emilie.Caspar@UGent.be Department of Experimental Psychology, Ghent University, Henri Dunantlaan, 2, Ghent 9000, Belgium

vulnerable individuals and raise funds (Robinson & Whittaker, 2021). For instance, modified versions of games have famously been used for jihadist recruitment, but also right-wing extremist talk is ripe on community websites and among the "guilds" and "crews" of a popular shooting game. By using these similar platforms (i.e., online video games) to promote counter-narratives and stimulating positive behavior change in the audience, hope is raised for providing a powerful tool for counter-radicalization efforts.

Past scientific literature has started to investigate how the intergroup context in video games influences intergroup attitudes and behaviors in experimental set-ups. Saleem and Anderson (2013) observed that playing a violent video game in which the enemy character was an outgroup member (i.e., Arab) increased prejudiced attitudes towards Arabs among American participants. Adachi et al. (2015) found that playing a violent video game cooperatively with an outgroup member for 12 min reduced outgroup prejudices. Stiff and Kedra (2020) showed that playing a casual video game with an outgroup member increased positivity towards members of that group. Playing video games in an intergroup context thus seems to be a promising tool. However, there are several holes in the literature that prevent any complete understanding from being imparted on video games' ability to reduce intergroup biases in diverse human societies.

First, the vast majority of past studies on video games and prejudice used self-reported outcomes, such as questionnaires, to evaluate the intergroup biases (Adachi et al., 2016; Saleem & Anderson, 2013; Stiff & Kedra, 2020), without controlling for social desirability. With self-reported outcomes, participants can exaggerate the influence of the intervention in order to please the experimenter (Paluck et al., 2021). More objective methods, for instance relying on neuro-cognitive measurements, are unfortunately almost inexistent in the current scientific literature (Paluck et al., 2021). Second, most studies lack direct comparison with other video games (Adachi et al., 2015; Stiff & Kedra, 2020), thus precluding isolating the effect of a specific game on intergroup biases or a general effect of playing video games. Third, past studies on the effect of video games on prejudiced attitudes towards outgroup individuals predominantly used gameplay that targeted a real human individual as the outgroup enemy or rivals, such as someone with a different skin color (Vang & Fox, 2014; Yang et al., 2014) or a different religion (Saleem & Anderson, 2013). As video games are used worldwide, using real individuals as the enemy character can negatively influence individuals who are actually part of this "enemy" group, increasing their perception of exclusion and stigmatization (Cary, 2017; Ortiz, 2019). Consequently, using fictional characters to evaluate the sizeable impact of video games in reducing intergroup biases would be more appropriate.

The present study aims to evaluate the effect of the test game, a prototype edutainment intervention designed to reach youth vulnerable to radicalization. It is a Massive Online Battle Arena (MOBA) game that shows a fictional society consisting of two groups, the Etanoreans, the majority group, which owns all the technological advancements, and the Oldarins, the minority group, which the Etanoreans consider as inferior and feeble-minded. When the society experiences an existential energy crisis, an unscrupulous leader convinces the Etanoreans that the Oldarins are responsible for the crisis. To "appease the gods," he makes the two groups confront each other during arena battles. Players play the role of Esp, a young Etanorean who undergoes various experiences leading to the progressive realization that the Oldarins are not responsible for society's degradation. Through this shift in realization, Esp's efforts veer towards the defense of the Oldarins against attacks driven by the discrimination of the Etanoreans. Thus, the game has been conceived to create a fictional scenario where insecurity prompts players to experience scapegoating, leading them into conflicts with the minority group. Then, through the messaging delivered implicitly throughout the game, players start gaining awareness regarding how leaders can exploit their fears for their power without helping them solve their fears' real cause- the goal of reducing prejudiced attitudes. Creating only implicit messaging is crucial here as a strategy employed by the intervention to prevent reactance – a frequent problem encountered by anti-radicalization projects that find that they do not reach the intended target audience when that audience perceives "open" messaging as an attempt by an "opposing" side to change their attitudes. Hence this intervention aims to avoid any feeling of "being lectured to" which could turn the target audience away from playing the intervention.

Participants were invited to play the test game or another control game with a relatively similar gameplay but without any specific messaging, between 2.5 and 4 h at home. This gaming duration was decided based on the shortest and longest timing necessary to end the storyline of the test game. Following this, participants were invited to the lab and informed that they would perform different tasks with three other (fake) participants in different experimental rooms. Purposely, those three other participants had two representants of the majority group where the study was conducted (i.e., European Caucasian) and one representant of a minority group (i.e., Afro-descendant). Using behavioral and electroencephalography (EEG) measurements, we assessed prejudiced attitudes towards the representant of the minority group (=outgroup) compared to the majority group (=ingroup) through two different prosocial tasks previously used in the literature: a costly helping task (Hein et al., 2010) and an intended prosociality task (Pech et al., 2022). Those two tasks differ in the extent to which participants may feel close or distant from the outcomes of their actions, as the tasks respectively involve making decisions that have a direct consequence on someone else (i.e., physical pain) or making decisions that have only imagined consequences on someone else.

Costly helping tasks commonly involve losing a monetary gain or enduring physical pain to prevent someone else from receiving a painful stimulation. In a previous study (Hein et al., 2010), the authors observed that soccer fans were less likely to endure physical pain to prevent the soccer fan of a rival team (=outgroup) from receiving a painful shock compared to another soccer fan supporting the same team (=ingroup). The Magnetic Resonance Imagery (MRI) results showed that empathy was a key neural process to explain such results. Empathy refers to our capacity to imagine and understand what others feel. Previous MRI studies showed that experiencing painful stimulations and empathizing with the same pain delivered to others evoke a brain activation pattern overlapping within the anterior insula (AI) and the anterior cingulate cortex (ACC) (see Lamm et al., 2011; Timmers et al., 2018 for reviews). These results suggest that we can understand and imagine what others feel since we can process that pain within our own pain system. In the study of Hein et al. (2010), the authors observed that activity in the left anterior cingulate (left AI) cortex was less activated when participants saw the outgroup receiving the pain compared to the ingroup. Further, activity in the left AI predicted helping behavior for the ingroup (Hein et al., 2010). This result is consistent with other studies showing a reduced neural response to the pain of outgroup individuals compared to ingroup individuals (Caspar et al., n.d.; Cikara et al., 2014; Han, 2018). Based on the study of Hein et al. (2010), we used in the present study a costly helping task in which participants had to decide to drop a part of their monetary gain to prevent the other (fake) participants from receiving a mildly painful shock on their hand. We predicted less costly helping towards the outgroup than the two ingroup participants, an effect that was potentially attenuated after playing the test game. In the present study, we used electroencephalography (EEG) to record the neural response to the pain of others. In EEG studies, a recent meta-analysis suggested that the P3 and the Late Positive Potential (LPP) over the centro-parietal electrodes are robust in measuring the neural empathic response to the pain of others (Coll, 2018). We predicted that the amplitude of the P3 and LPP would be reduced for the outgroup participant compared to the ingroup participants. This difference could also be attenuated for players of the test game, with fewer differences between outgroup participants and the ingroup participants.

In the costly helping task, we also evaluated to what extent participants feel a sense of agency when they decide to help the outgroup or the ingroup participants. The sense of agency is the feeling that we are the authors, and thus potentially responsible for, our actions and their consequences (Gallagher, 2000). In the literature, researchers have largely used an implicit method relying on time perception (Haggard, 2017) to avoid biases in explicit reports, such as social desirability (Yoshie & Haggard, 2013). In classic time perception tasks, participants have to estimate the duration of a time interval that elapsed between an action (e.g. pressing a button) and its consequences (e.g. hearing the beep it produces). Results typically show that participants tend to estimate time intervals as shorter when the action is performed voluntarily compared to a condition in which the action is performed involuntarily (Moore & Haggard, 2010), an effect referred to as temporal binding. It has been concluded that a sense of agency modulates time perception, by reducing it. A previous study showed that people usually experienced a higher sense of agency, as indexed by a lower estimation of time intervals when they act prosocially and decide not to send a painful shock to another participant compared to sending a painful shock (Caspar et al., 2018). Here, we predicted that our participants would experience less agency when they did not prevent the outgroup participant from receiving the shock compared to the ingroup participants, an effect again potentially attenuated after playing the test game.

Intended prosociality was measured by presenting to our participants three different prosocial scenarios from daily life, such as giving €2 to offer a bus ticket or taking time to comfort someone, based on a method used in another study (Pech et al., 2022). The pictures of the three (fake) participants were presented in pairs whereby participants had to decide as fast as possible who the beneficiary of the prosocial action was. In a previous study conducted in Rwanda on former genocide perpetrators and survivors (Pech et al., 2022), results indicated that participants tend to favor their own ingroup compared to the outgroup. We expected that participants would choose the ingroup participants more frequently than the outgroup participant, an effect attenuated after playing the test game.

A concern has nonetheless been raised in the literature regarding the fact that participants may not be fully sincere in imagined scenarios and show a high prosociality towards outgroup individuals to appear fairer to the experimenter (Amodio et al., 2008). In the neuroscientific literature, a promising approach consists in evaluating the extent to which the selected behaviors implied have a high cognitive conflict (Berns et al., 1997; Nieuwenhuis et al., 2001). Usually, a higher cognitive conflict involves that the selected action was not the most natural to select compared to the other actions (Amodio et al., 2008). In response to this, we further measured the extent to which selecting the outgroup would induce a higher degree of cognitive conflict as opposed to selecting the ingroup. Moreover, we also measured whether playing the test game reduced cognitive conflict when selecting the outgroup. Several methods to measure the intensity of the conflict experienced by participants when selecting an action have been reported in the literature. It has been repeatedly shown that decisions that are more difficult to take involve longer reaction times before selecting the action than easier decisions (Cohen & Donner, 2013; Greene et al., 2004). Past literature using EEG as a measurement modality has demonstrated that stronger cognitive conflict engenders increased activity in the midfrontal theta (4–8 Hz) compared to low conflicts (Cavanagh & Frank, 2014; Cohen & Cavanagh, 2011; Cohen, 2014; Cohen & Ridderinkhof, 2013; Nigbur et al., 2012).

In the literature on prejudice, several studies have shown that electrophysiological measurements of cognitive conflict indicate a higher conflict when participants are required to inhibit prejudice towards outgroups compared to when they are not required to. Hence, changing behaviors may be significantly mediated by the degree to which conflict is experienced or not. Amodio et al. (2008) showed that people with lower conflict tend to change their behaviors toward outgroup members more easily when inhibiting their stereotyping. In another study (Pech et al., 2022), the results indicated that conflict, as measured with midfrontal theta power and reaction times, was higher when participants selected the outgroup instead of the ingroup. In the intended prosociality task, we used reaction times and midfrontal theta activity as indirect measurements of cognitive conflict. This enabled us to evaluate the extent to which the decision to act prosocially toward outgroup members would involve higher cognitive conflict than with an ingroup member. We expected to observe a higher conflict when participants selected the outgroup compared to selecting the ingroup, an effect potentially attenuated for players of the test game.

2. Method

2.1. Participants

We recruited 65 male participants, aged between 18 and 36 years old with no preference for dominance handing. As no previous studies used a similar experimental approach, we used a small-to-medium effect size f of 0.175 to calculate the sample size (Faul et al., 2007). To achieve a power of .90 for this effect size, the estimated sample size was 54 for two groups tested with a 2-level within-subject factor (i.e., Individual) and a 2-level between-subject factor (i.e., Group). We increased the sample size to prevent data loss in the costly helping task, as never helping or always helping would have prevented to compute event-related potentials associated with the visualization of the pain. We recruited participants through advertisements on social media and on job recruitment websites. Inclusion criteria included being able to play video games and having a haircut that fits with electroencephalography (EEG) recordings (i.e., no dreadlocks, not bald). Exclusion criteria were determined prior to the data acquisition. They included: (1) failure to discriminate between time intervals, (2) bad signal-to-noise ratio in EEG, (3) not paying attention to the task, (4) not having enough shock or no shock trials, which would prevent this factor to be taken into account, (5) being an outlier and (6) being Afro-descendant. For time intervals (see exclusion criteria 1), to identify participants for whom the estimated action-tone intervals did not gradually increase with the real action-tone intervals, we performed a linear trend analysis (LTA) with Sieve Bootstrap Based Test for the Null Hypothesis of no Trend (Hess et al., 2001). This was done to ensure that participants' estimations of time intervals followed the real-time intervals and a linear trend (Hess et al., 2001). The data of 7/65 participants were excluded due to a non-significant LTA. Other data (i.e. EEG data, behavioral data) were kept for those participants. Regarding EEG recordings, we excluded 4 participants due to a bad signal-to-noise ratio (i.e., the bad signal on the electrodes of interest or noisy reference electrodes). In the costly helping task, the EEG data of 31 participants were not analyzed: 5 because of too many head artifacts, muscle artifacts and/or sweat artifacts, 25 because they delivered only a small number of shock ($\leq$5/60) or a high number of shocks ($\geq$55/60) to either one or all individuals, and 1 was considered outlier. In the intended prosociality task, the EEG data of 3 participants were considered outliers for the response-locked FMθ and 1 was considered as outlier for the stimulus-locked FMθ. Again, the other data of those participants were kept. One participant was fully excluded due to a technical problem during the recording. For the test game, there were 33 remaining participants (mean age: 23.21, SD = 4.35), and for the control game, there were 31 remaining participants (mean age: 22.45, SD = 2.80). All participants were paid 30 euros for their participation even if they were instructed that they could increase this monetary gain based on their decisions during the task. The study was approved by the local ethical committee of the Université libre de Bruxelles (reference number: 2021/696). Participants gave their consent before starting to perform the tasks, after having received all the necessary information.

2.2. Procedure and material

Participants were randomly assigned to play either the test game or the control game (name withheld for the confidentiality of the intervention but available upon request) at home for 2.5–4 h. They were requested to send us a print screen of their Steam account to confirm that they finished game's storyline or played for the expected duration before coming to the lab for the experiment.

When participants came to the lab 2–4 days after playing the game, we first explained the two different tasks (i.e., the "Costly helping task" and the "Intended Prosociality task," see below) and told them that we had to synchronize the timing of the experiment with three other participants participating in the experiment at the same time. As we sought to evaluate how playing a specific video game would decrease prejudice towards outgroups, we had to include other individuals in the study who would be representative of the ingroup majority in Europe (i.e. Caucasian) or outgroup minorities (i.e., Afro-descendant). To avoid participants guessing that we were evaluating their behaviors towards ingroup or outgroup individuals, we did not include only one Caucasian participant versus a one afro-descendant participant, which would have displayed obvious experimental manipulation. Rather, we included three other individuals, two Caucasian male individuals, and one Afro-descendant male individual. Due to the necessity to ensure that participants believed they were undertaking the same task simultaneously as other participants (fakes), they were informed that they would be placed in different experimental rooms on different floors. This was undertaken under the pretext of anonymity which was highly congruent with how the experimental procedure appeared. Moreover, participants had already been informed in the initial recruitment emails, which instructed them to avoid mixing floors in order to ensure anonymity would be protected. They were also instructed to remain attentive to scheduling and arrive at the exact time for the tasks between different participants to work in synchrony. To increase the authenticity of the procedure, we told

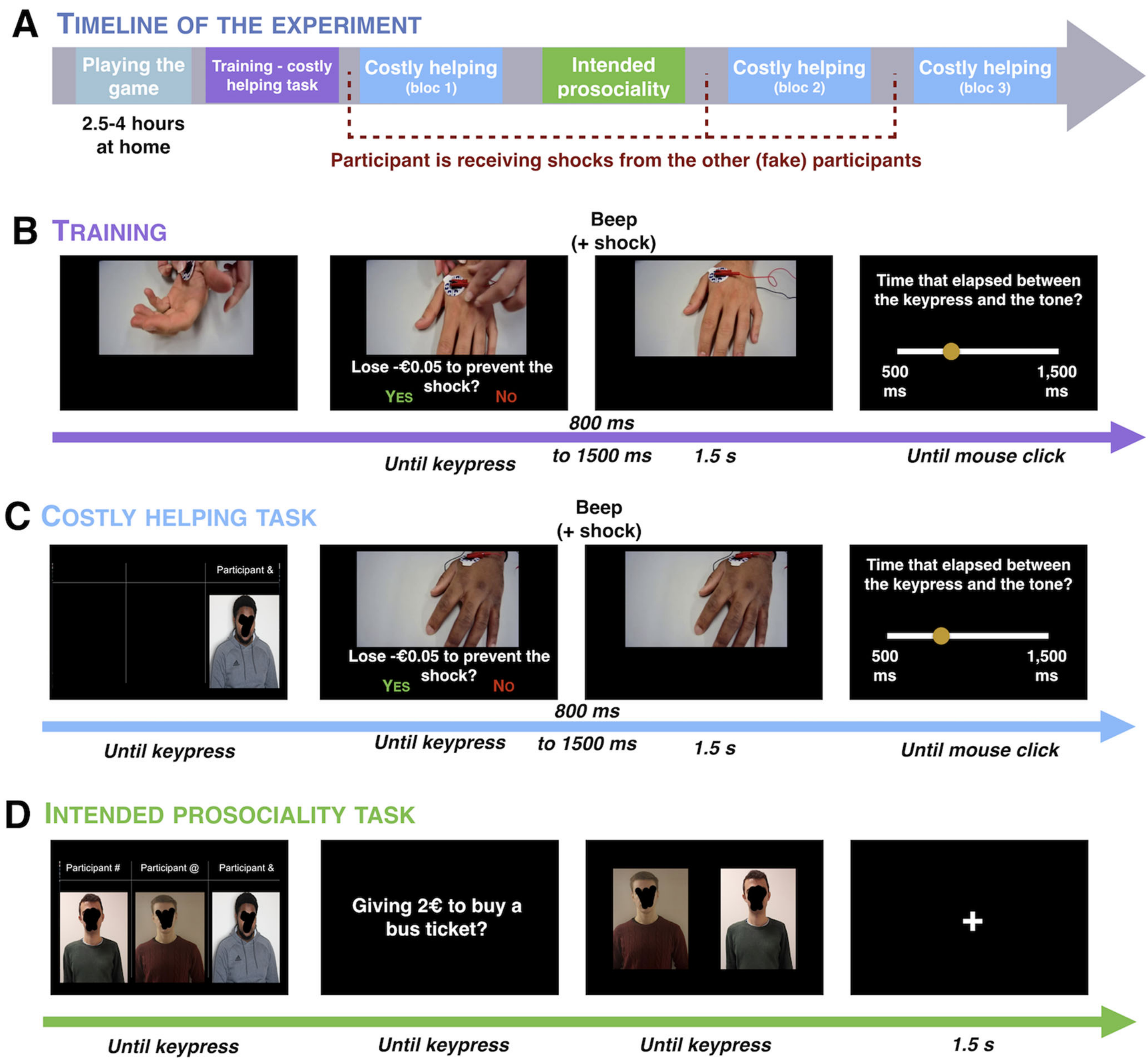

Figure 1. (A) Schematic representation of the timeline of the experiment. (B) Schematic representation of the training. While practicing the interval estimate task, participants could witness the video of the hand of another (fake) participant being installed to increase the reality of the task. (C) Schematic representation of the costly helping task. Participants were presented with one of the three other (fake) participants and had to decide on each trial to help that participant and avoid him to receive a mildly painful shock in exchange for dropping €0.05. (D) Schematic representation of the intended prosociality task. In each trial, participants were presented with pairs of individuals, and they had to select who would benefit from the prosocial action. The picture displays an example of an ingroup *vs* ingroup pair.

participants from time to time to wait for a few minutes before starting a task because we had to wait for the other participants to be ready as well.

After signing the consent forms, we installed the EEG and determined participants' pain threshold for the electrical stimulation, as described in Caspar et al. (2016). Two electrodes were placed on the participants' left hand on the abductor pollicis muscle in order to produce a clear and visible muscle twitch and the threshold was increased by steps of 1 mA until a mildly painful stimulation was achieved. The pain threshold was determined by asking a series of questions to the participants about their pain perception during the calibration (1. « Is it uncomfortable? » – 2. « Is it painful? » – 3. « Could we increase the threshold? »). We told our participants that the same procedure would be applied to the other participants as well and that the pain threshold would remain similar throughout the entire experiment.

Then, participants were provided explanations about the two different tasks. In the "Costly helping task," participants observed through a video displayed on the top of their screen the hand of one of the three other individuals, see Figure 1(C). We told our participants that the videos were displayed in real-time to analyze their brain activity when witnessing the consequences of their actions. We also told them that the other participants would see their own hands when doing the costly helping task. Moreover, a webcam was concurrently installed in front of the participants' hand who were informed to remain still. In reality, the webcam was not connected, and the videos displayed on the screen were pre-recorded. Two of those videos showed the hand of two Caucasian males, and the third video showed the hand of an Afro-descendant male. On the bottom of the screen, below the video, was displayed a sentence stating, "*Do you want to spend 0.05€ to avoid the other participant to receive the shock?.*" Two possible answers were displayed below: a "*YES*" displayed in green and associated with a picture of no shock and a "*NO*" displayed in red and associated with a picture of a shock. The "YES" and "NO" answers were presented randomly on the right or on the left of the screen to avoid motor habituation. Participants answered with the keyboard's left or right arrow to select their answer. We instructed participants that if they answered "YES," they would lose €0.05 but would prevent the other participant from receiving the shock. In the case participants answered "NO," they were instructed to keep the €0.05 and that the other participant would receive the shock. Participants could observe the consequences of their decision through the (fake) real-time video displayed on the screen, which showed a hand connected to two electrodes. The hand showed a visible muscular contraction if a shock was delivered and no muscular contraction if no shocks were delivered. No matter the participant's decision, a tone (550 Hz, 500 ms) was displayed after each keypress at random time interval of 800, 1,100, or 1,400 ms. Participants had to estimate the time interval that elapsed between their keypress and the tone. If a shock was delivered, the shock was displayed exactly at the same time as the tone. We instructed them that the time intervals would vary between 500 and 1,500 ms. An analog scale ranging from 500 to 1,500 ms was displayed on the screen 2 s after the keypress. Participants were told to click with the mouse on the scale according to their time interval estimation. There were 60 trials for each individual presented, thus resulting in a total of 180 trials for the three individuals.

In the "Intended Prosociality task," participants were presented with three different prosocial scenarios in which they had to choose one out of two pictures of a pair of individuals, see Figure 1(D). In those pictures, participants could see the upper part of the body of the other individuals, with their faces blurred, as mentioned in the emails during the advertisement. To justify the fact that we had the pictures of the other participants, we asked our participants to also send us a picture of them with a blurred face and told them that this picture would be uploaded on the computer of the other participants to be used during the experiment. We used three scenarios including different types of prosocial intentions instead of a single one in order to prevent monotony during the task. In one scenario, participants had to decide to which of the two individuals presented they would give 2€ to pay them a bus ticket. In another scenario, participants had to decide which of the two individuals presented they would help to move. In a third scenario, participants had to decide to which of the two individuals presented they would give time to comfort at the emotional level. After being presented with the scenario, participants were presented with six different combinations of picture pairs showing the other individuals. Those combinations included either two different individuals (e.g., the picture of one of the Caucasian male individual VS the picture of the Afro-descendant male individual) (free-choice condition) or two times the same individual (e.g., the picture of the Afro-descendant male individual VS the picture of the Afro-descendant male individual) (forced-choice condition). During the presentation of the picture pairs, participants were instructed to press the left or right arrow as fast as possible for answer selection. The three scenarios were presented seven times each, systematically followed by the six different pair combinations presented in a randomized order. In total, there were 126 trials, with 21 trials for each combination.

The two tasks were presented in a semi-randomized order for each participant, see Figure 1(A). All participants started with training to estimate the intervals between their keypress and the resulting tone, as used in the costly helping task, see Figure 1(B). There were 10 trials during training, but the trials could be repeated until participants could correctly discriminate between the different time intervals. Participants were equally shown the same screen used in the costly helping task during training. However, the video displayed the preparation of electrodes installed on the hand of another participant by a different experimenter. This video was used to reinforce the belief that other participants were being prepared at the same time for the same task. Once the training session ended, the real participant was informed that another participant was ready for the costly helping task and potentially receiving the shocks. We also told our real participant that we would not reveal whom the other participant completing the costly helping task was so not to bias their decisions when they performed the task themselves on the other participants. Our participants received 6 shocks with a random time interval between each shock for 4 min. Each time we told our participants that another participant was performing the costly helping task, there were six shocks sent at a random time interval for 4 min in order to make them believe that another individual was genuinely performing the task. This also avoided creating differences between the three other individuals. The six shocks were decided to maximize the ratio between accepting to drop money to prevent a shock from happening or not based on a pilot study. After receiving the shocks, participants were invited to start the first block of the costly helping task. One of the three other individuals was presented, and randomly selected to perform the task. After the first block of the costly helping task was completed, participants moved on to perform the intended prosociality task. Once this was completed, they received six shocks again at a random time (as we told them that another participant was performing the costly helping task). Following this, participants performed the last two blocks of the costly helping task on the two other individuals, with the same number of shocks received between the two blocks.

2.3. Data analysis

2.3.1. Trials rejection

In the two tasks, trials which were either faster than 350 ms (Semmelmann & Weigelt, 2017) or slower than 20 s were removed. We used longer minimum and maximum Reaction Times (RT) than in previous studies (i.e., 200 ms, Cohen & Donner, 2013) because the prosocial component in our task requires more time to select the answer than in reaction time tasks which involve neutral decisions. We also rejected responses deviating from more than 2.5 Median Absolute Deviation (Leys et al., 2013) in the interval estimation task for each participant.

2.3.2. EEG recordings

EEG data was acquired at 2,048 Hz from 32 channels placed according to the international 10–20 system using Biosemi equipment (see http://www.biosemi.com for hardware details). Four additional electrodes were used to acquire horizontal eye movement and mastoid signals. The Actiview software recorded all the data. To filter and clean the data, we used the Matlab r2018a software and the fieldtrip toolbox (Oostenveld et al., 2011). We applied a bandpass filter between 0.1 Hz and 30 Hz. Different baselines were used depending on the analysis and are further specified below. To re-reference the signal, we subtracted the signal of the two mastoid electrodes to each scalp electrode. Since our participants were not in a Faraday Cage, we performed a spectrum interpolation between the 49 Hz frequency band and the 51 Hz frequency band to remove the 50 Hz noise. To remove artifacts due to eye movements (i.e., saccades and eye blinks), we first ran an Independent Component Analysis (ICA) on 30 components. Then, we removed the remaining artifacts (i.e., muscle artifacts, head movements) based on visual inspection. On average, 2.66 (SD = 0.122) trials were removed in the intended prosociality task, and 3.1 (SD = 0.213) trials were removed in the costly helping task.

2.3.2.1. Event-related potential (ERP) analysis. In the costly helping task, we measured the amplitude of the P3 and the LPP study (Coll, 2018) across Cz and Pz. According to Cheng et al. (2014), we divided the LPP into an early (eLPP) and a late (lLPP) LPP. Based on the visualization of the grand averages, the P3 was measured as the most positive peak between the 250 and 400 ms time-window after the visualization of the shock. The early LPP and the late LPP were measured as the mean amplitude between the 380 and 800 ms time-window and the 800–1,200 ms time-window after the tone, respectively. The baseline was taken from 500 to 300 ms prior to the keypress. We conducted our analyses on the amplitude of the difference between shock and no shock trials (Shock – No Shock) for the P3, the eLPP and the lLPP as an indicator of the neural response to the pain of others.

2.3.2.2. Time-frequency representation (TFR) analysis. Based on previous studies, we measured the frontal midline theta frequency (FMθ) over the Fz, Cz, FC1, and FC2 electrodes. We extracted the time-frequency power of each trial after calculating a Fast Fourier transform (FFT) on our data and an FFT of a complex Morlet wave with the following parameters: frequency from 2 to 20 Hz with 80 bins spaced logarithmically and 4–14 cycles logarithmically spaced. Then, we used the Inversed Fast Fourier Transform (IFFT) method on the multiplication of the computed FFT on our data and the FFT of the complex Morlet Wave. Epoching was performed by taking -2.5 to 1.5 s around the keypress (response-locked time window) and -1.5 to 2.5 around the presentation of the picture (stimulus-locked time window). All power values in the time-frequency representation were normalized to the average pre-stimulus baseline power at each frequency band. We used a decibel (dB) transform for normalization [dB power $= 10 \times \log_{10}$(power/baseline)]. The baseline power was computed as the average power across all experimental conditions, from 1 s to 1.2 s after the keypress when TFR were response-locked and from -500 to -300 ms when TFR were stimulus-locked. Based on visual inspection of the grand average data significatively superior at $p > 0.05$ (see Figure 2), FMθ was extracted from a -420 ms to 0 ms time window before the keypress and from a 100 ms to 580 ms time window after the presentation of the picture. FMθ was extracted from a 2.9 Hz to 6.4 Hz window before the keypress and from a 2.5 Hz to 8.1 Hz window after the presentation of the picture We systematically extracted each participant's mean value in this time/frequency window. In the response-locked time window, we only selected a window prior to the keypress and did not consider the significant activation in the delta band after the keypress as we had no specific predictions.

3. Results

We systematically combined the results of the two ingroup individuals by averaging their respective data as we did not expect differences between them. We systematically checked that there were no differences between them in each task before combining them. For each analysis, participants whose mean deviated more than 3SD based on the mean of all participants were considered outliers and excluded (Seo, 2006). Our hypothesis predicted an overall "outgroup" effect, that is, a higher prejudice towards the outgroup individual than towards the ingroup individuals. We also predicted that this effect would be attenuated for players of the test game compared to players of the control game. A significant interaction between Group (test game, control game) and Individual (outgroup, ingroup) was thus not necessarily expected as we expected attenuation of the outgroup effect for players of the test game. Therefore, we systematically conducted paired comparisons between the ingroup and outgroup individuals for each group of players, irrespective of the interaction value given by the repeated-measures ANOVA.

3.1. Costly helping task

3.1.1. Prosocial behavior

We conducted a repeated-measures ANOVA with Individual (outgroup, ingroup) as a within-subject factor and Group (test game, control game) as a between-subject factor on the number of helping actions. Results indicated a tendency for

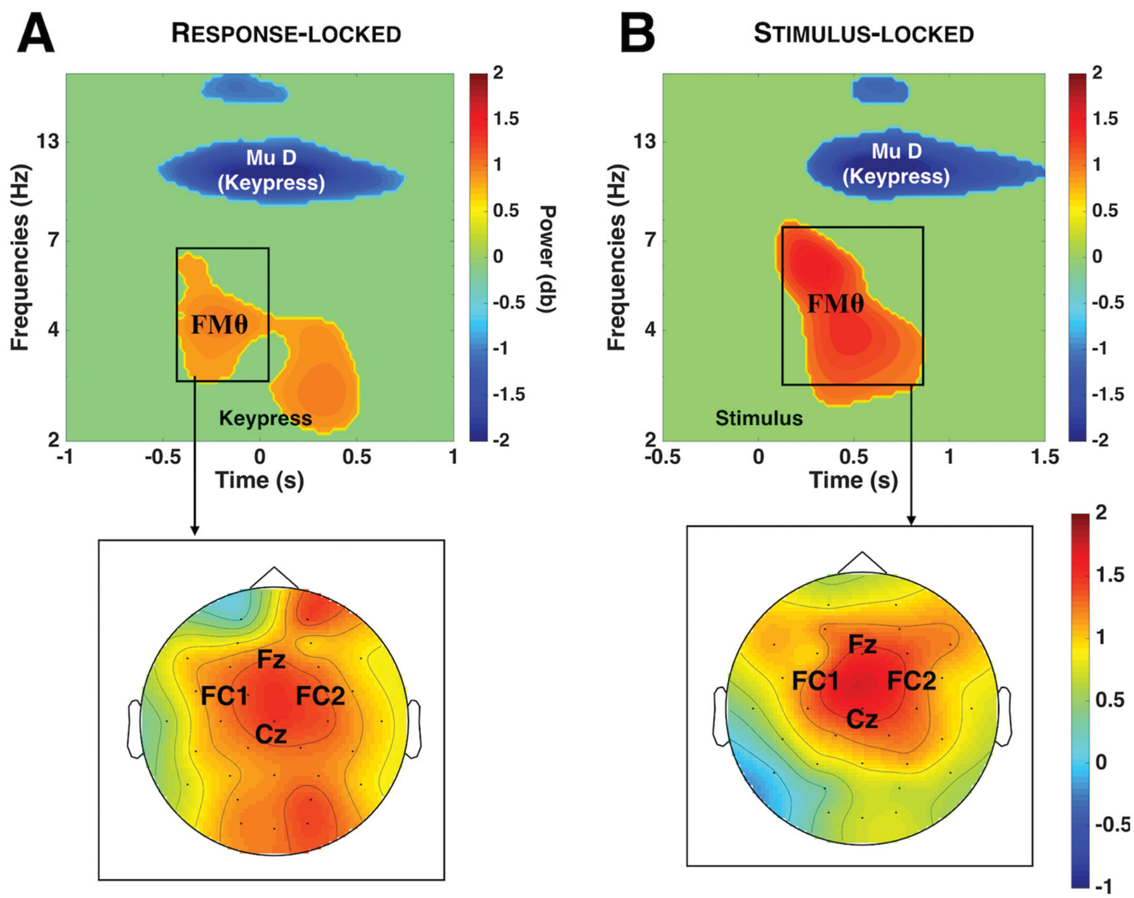

Figure 2. Response-locked (A) and Stimulus-locked (B) time frequency power plots for the four electrodes (Fz, Cz, FC1, FC2). The significance level taken was $p > 0.05$. Mu D represents the desynchronization in the Mu band (8–12 Hz), which is typical of movement execution (i.e., keypress). The Mu D was not analyzed in the present paper and is just presented for visual purpose.

a main effect of Individual ($F_{(1,62)} = 3.331$, $p = 0.073$, $\eta^2_p = 0.051$), no main effect of Group ($p > 0.3$) and no interaction ($p > 0.3$). Paired comparisons indicated that players of the test game helped the outgroup individual more (35.6/60, SD = 16.48) than the ingroup individuals (33.3/60, SD = 17.29, $t_{(32)} = -2.049$, $p = 0.049$, Cohen's $d = -0.357$). This difference was not significant for control game players ($p > 0.5$).

3.1.2. Event-related potentials (ERPs)

We first compared the amplitude of the P3, eLPP and lLPP when participants witnessed a shock on the other individuals' hand to when they did not witness that shock to ensure that those ERPs are sensitive to seeing pain, see Figure 3(A,B). To do so, we averaged the amplitude of each ERP across the individuals presented and the groups of players. Results confirmed that the amplitude was systematically higher when participants visualized a shock being delivered compared to when they did not visualize a shock being delivered for the P3 ($t_{(33)} = 4.769$, $p < 0.001$, Cohen's $d = 0.806$), the eLPP ($t_{(33)} = 13.412$, $p < 0.001$, Cohen's $d = 2.267$) and the lLPP ($t_{(33)} = 9.573$, $p < 0.001$, Cohen's $d = 1.618$). The remaining analyses were conducted on the difference between the amplitude when participants visualized a shock compared to when they did not visualize a shock (Shock-No Shock trials).

We then conducted a repeated-measures ANOVA with Individual (outgroup, ingroup) as within-subject factor and Group (test game, control game) as a between-subject factor on the difference between Shock and No Shock trials. For the P3, neither the main effect of Group $p > 0.4$, nor the main effect of Individual ($p > 0.9$) nor the interaction ($p > 0.5$) were significant. As we expected the effect between ingroup and outgroup to be similar in both groups, but reduced in the test group, we conducted paired comparisons. We conducted paired comparisons but there was not difference between ingroup and outgroup individuals for the two groups (all $ps > 0.4$), see Figure 3(C). For the eLPP, there was a significant main effect of Individual ($F_{(1,32)} = 14.243$, $p = 0.001$, $\eta^2_p = 0.308$), with a higher amplitude for the ingroup individuals (8.80 µv, SD = 3.83) than for the outgroup individual (6.51 µv, SD = 4.25). The main effect of Group ($p > 0.9$) and the interaction ($p > 0.1$) were not significant. Again, based on our hypotheses, we conducted paired comparisons. For players of the test game, paired comparisons indicated that there was no significant difference between the ingroup and the outgroup individuals ($p > 0.09$). For players of the control game, the amplitude was lower for the outgroup individual (5.84 µv, SD = 5.47) than for the ingroup individuals (9.33 µv, SD = 4.21, $t_{(13)} = -3.247$, $p = 0.006$, Cohen's $d = -0.868$), showing a classical intergroup bias. For the lLPP, there was a main effect of Individual ($F_{(1,32)} = 6.707$, $p = 0.014$, $\eta^2_p = 0.173$), with a

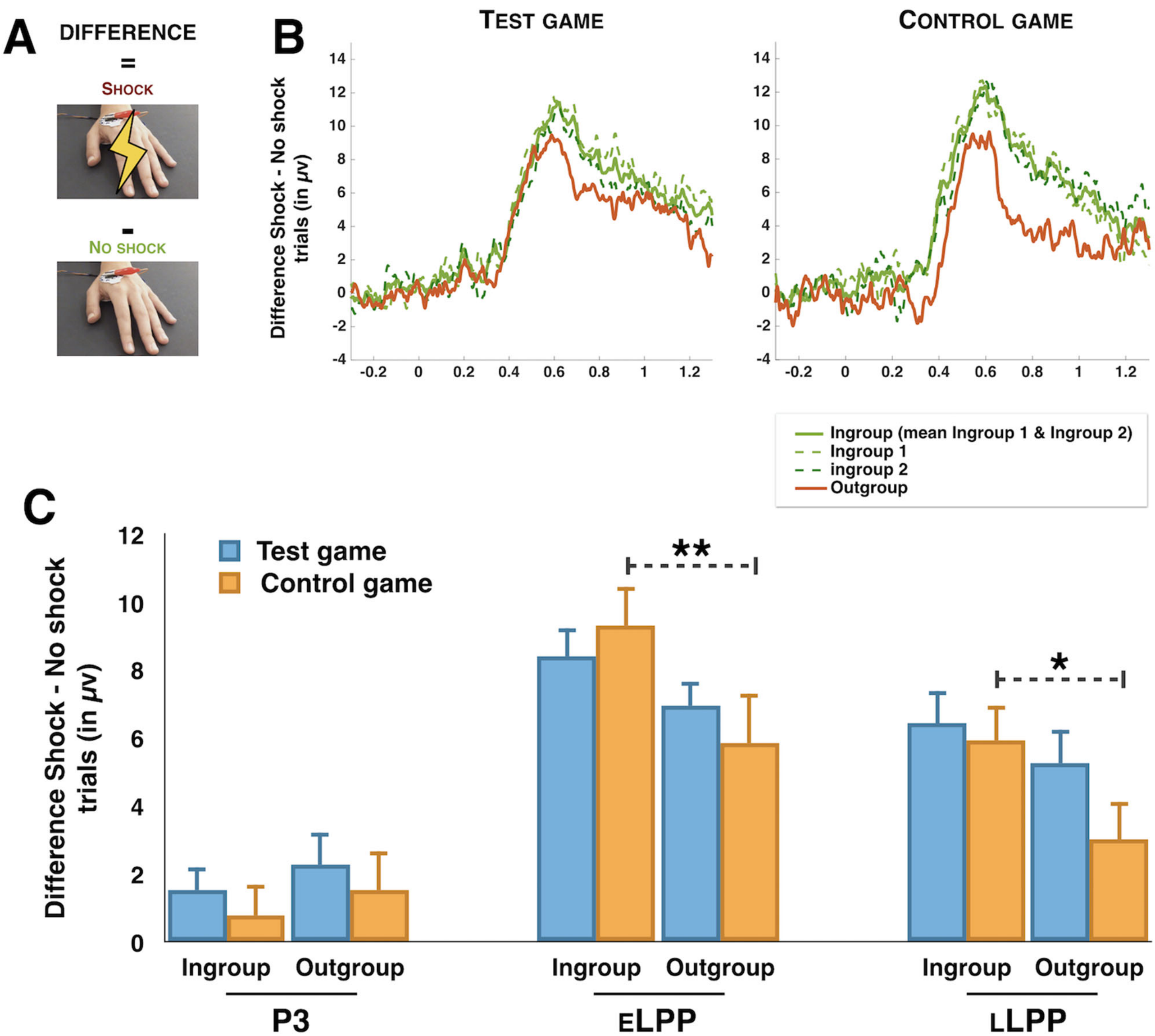

Figure 3. (A) Computed difference Shock trials (=No helping) – No Shock trials (=Helping). (B) Graphical representation of the ERPs for each group and individual. (C) Graphical representation of the amplitude in μV of each ERP for the group of players of the test game (in blue) and players of the control game (in orange). All tests were two-tailed. ** represents $p \geq 0.001$ and ≤ 0.01. * represents $p \geq 0.01$ and ≤ 0.05. Error bars represent the standard errors.

higher amplitude for the ingroup individuals (6.24 μv, SD = 3.96) than for the outgroup individual (4.34 μv, SD = 4.42). The main effect of Group ($p > 0.2$) and the interaction ($p > 0.2$) were not significant. For players of the test game, paired comparisons indicated that there was no significant difference between the ingroup and the outgroup individuals ($p > 0.3$). For players of the control game, the amplitude was lower for the outgroup individual (3.03 μv, SD = 4.05) than for the ingroup individuals (5.93 μv, SD = 3.90, $t_{(13)} = -2.479$, $p = 0.028$, Cohen's $d = -0.663$). The result thus indicated that the "outgroup effect," as indexed by a lower neural response to the pain of the outgroup individual compared to the ingroup individuals, was not present for players of the test game.

3.1.3. Temporal binding

It is known that participants may differ in the way they use the scale to provide an answer, some preferring smaller numbers and others preferring larger numbers.(Caspar et al., 2020; Cravo et al., 2013). We thus transformed the raw interval estimates in z-scored data as they reduce irrelevant inter-subject variability. This was done by subtracting from each interval estimate, the mean estimate for that participant across all trials and by dividing the resulting differences by the standard deviation of all estimates for that participant. We conducted a repeated-measures ANOVA with Individual (outgroup, ingroup) and Helping (helping, no helping) as within-subject factors and Group (test game, control game) as a between-subject factor on the z-scored interval estimates. Only participants with a significant linear trend were taken into account in the analysis. We observed a significant triple interaction Individual*Helping*Group ($F_{(1,42)} = 6.871$, $p = 0.012$, $\eta^2_p = 0.141$), see Figure 4. When participants decided to help or not the ingroup individuals, z-scored interval estimates did not statistically differ for both players of the test game ($p > 0.7$) and players of the control game ($p > 0.3$). When participants decided to help or not the

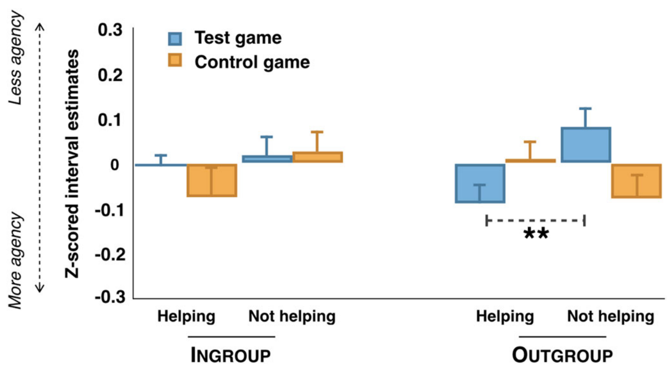

Figure 4. Graphical representation of the z-scored interval estimates for the group of players of the test game (in blue) and players of the control game (in orange). All tests were two-tailed. ** represents $p \geq 0.001$ and ≤ 0.01. Error bars represent the standard errors.

outgroup individual, differences in z-scores emerged. Paired sample t-tests indicated that players of the test game had lower z-scored interval estimates, indicating a higher sense of agency, when they helped the outgroup individual (-0.075, SD = 0.23) compared to when they did not help the outgroup individual (0.042, SD = 0.25, $t_{(25)} = -2.575$, $p = 0.016$, Cohen's $d = -0.505$). The difference was not significant for players of the control game ($p > 0.8$). Other main effects or interactions were not significant (all $ps > 0.072$).

3.2. Intended prosociality task

3.2.1. Prosocial behavior

We conducted a repeated-measures ANOVA with Individual (outgroup, ingroup) as a within-subject factor and Group (test game, control game) as a between-subject factor on the number of intended prosocial actions in the free-choice condition. We observed a main effect of Individual ($F_{(1,61)} = 18.471$, $p < 0.001$, $\eta^2_p = 0.232$), with a higher number of intended prosocial actions towards the outgroup individual (46, SD = 7.34) than towards the ingroup individuals (39.9, SD = 3.67). The main effect of Group ($p > 0.2$) and the interaction ($p > 0.2$) were not significant.

3.2.2. Time-frequency representation (TFR)

We conducted a repeated-measures ANOVA with Individual (outgroup, ingroup) and Condition (free-choice, forced-choice) as within-subject factors and Group (test game, control game) as a between-subject factor on the frontal midline theta frequency (FMθ) power before the keypress (response-locked). None of the main effects or interactions were significant (all $ps > .1$). Paired comparisons indicated that for players of the test game, the power did not differ between the ingroup individuals and the outgroup individual ($p > 0.9$). For players of the control game, the power was higher when they selected the outgroup individual (0.94, SD = 0.79), suggesting more conflict than when they selected the ingroup individuals (0.63, SD = 0.79, $t_{(30)} = 2.047$, $p = 0.050$, Cohen's d = 0.368), see Figure 5(A).

We conducted a repeated-measures ANOVA with Pair (outgroup *VS* ingroup, ingroup *VS* ingroup) and Condition (free-choice, forced-choice) as within-subject factors and Group (test game, control game) as a between-subject factor on the frontal midline theta frequency (FMθ) power after the presentation of the pairs of pictures (stimulus-locked). Three participants were considered as outliers. We observed a significant interaction Pair*Group ($F_{(1,61)} = 4.688$, $p = 0.034$, $\eta^2_p = 0.071$). Paired comparisons indicated that for players of the test game, the power did not differ between the ingroup individuals and the outgroup individual ($p > 0.7$), see Figure 5(B). For players of the control game, the power was higher when they visualized an outgroup *vs* an ingroup individual (1.09, SD = 0.71), suggesting more conflict than when they visualized an ingroup *vs* an ingroup individual (0.70, SD = 0.92, $t_{(30)} = -2.739$, $p = 0.010$, Cohen's $d = -0.492$). We also observed a significant interaction Condition*Group ($F_{(1,61)} = 6.114$, $p = 0.016$, $\eta^2_p = 0.091$), which was not investigated further as not part of our hypothesis. Other main effects or interactions were marginal or not significant (all $ps \geq .09$).

3.2.3. Reaction times

We conducted a repeated-measures ANOVA with Individual (outgroup, ingroup) and Condition (free-choice, forced-choice) as within-subject factors and Group (test game, control game) as a between-subject factor on the reaction times. We observed a main effect of Condition ($F_{(1,58)} = 134.35$, $p < 0.001$, $\eta^2_p = 0.698$), with shorter reaction times in the forced-choice condition (862 ms, SD = 225.1) than in the free-choice condition (1,538 ms, SD = 561.25). We also observed a main effect of Group ($F_{(1,58)} = 6.484$, $p = 0.014$, $\eta^2_p = 0.101$), with players of the test game having longer RT than players of the control game. No other main effects or interactions were significant (all $ps > .2$). None of the paired comparisons indicated an effect of the game on reaction times when selecting the ingroup or the outgroup individuals (all $ps > 0.1$).

3.2.4. Correlations

We also conducted exploratory Pearson correlations between reaction times and midfrontal theta power across the different experimental conditions and the individual (i.e., ingroup, outgroup) selected. We corrected for multiple comparisons with the False Discovery Rate approach (FDR) with the Benjamini and Hochberg method (Benjamini & Hochberg, 1995). None of the correlations were significant (all $ps \geq .2$).

4. Discussion

The present study aimed to investigate the effect of playing a video game designed to reduce prejudiced attitudes towards fictional minorities on prejudice towards non-fictional minorities. Overall, we observed encouraging results between 2.5 and 4 h after playing the game.

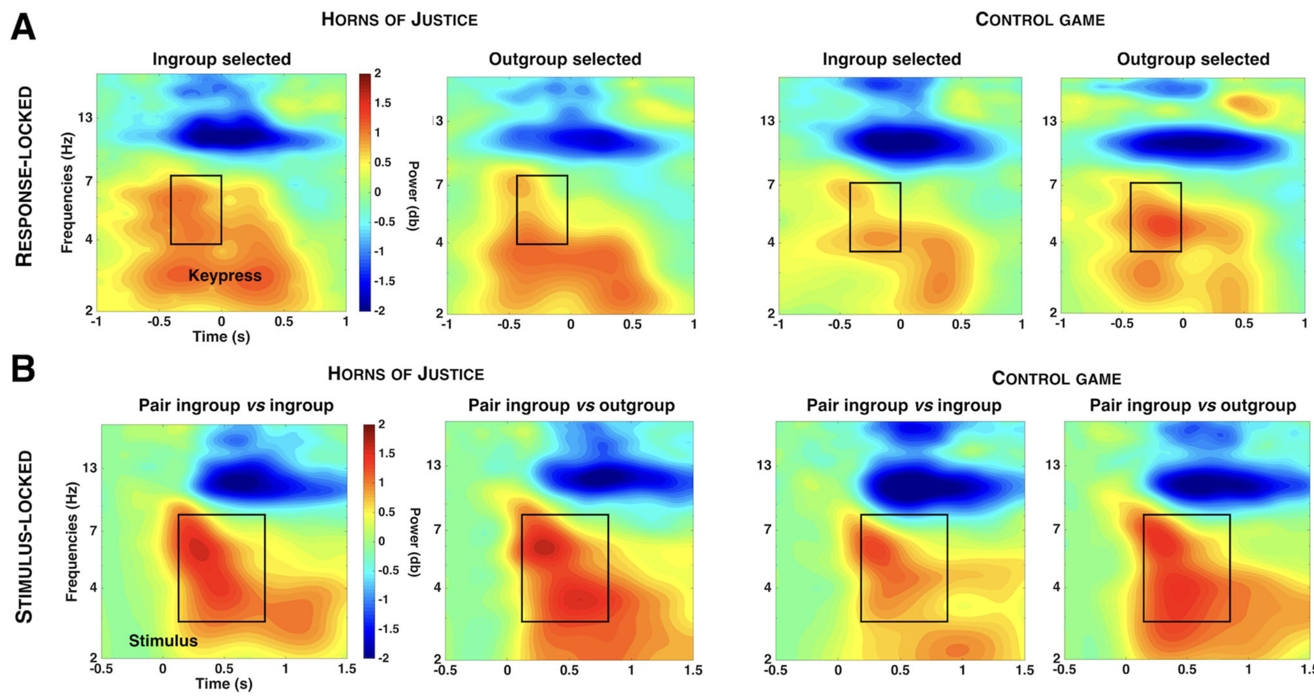

Figure 5. (A) Response-locked midfrontal theta over the Fz, FC1, FC3, Cz electrodes when participants pressed the key. (B) Stimulus-locked midfrontal theta over the Fz, FC1, FC3, Cz electrodes when participants saw the pairs being presented on the screen. The black squares represent the selected time windows based on the significance levels presented on Figure 2.

In the costly helping task, we observed a typical intergroup empathy bias (Caspar et al., n.d.; Cikara et al., 2014; Han, 2018) for players of the control game, with a reduced neural response to the pain of the outgroup individual compared to the ingroup individuals on the eLPP and the lLPP. However, we observed that this intergroup empathy bias was not present for players of the test game. Our results may reflect possible advances indicating the ability to reduce intergroup empathy bias towards non-fictional minorities through playing a game designed to reduce prejudice towards fictional minorities. Even if we slightly increased our sample size to prevent data loss, we nonetheless had to exclude a higher-than-expected number of participants from those analyses as many of them either never helped or helped the other (fake) participants all the time. These extreme behaviors prevented us from computing a reliable neural response to the pain of others. These results would thus benefit from replication with a higher number of participants kept for the analysis.

We also observed that players of the test game had lower z-scored interval estimates, interpreted as a higher sense of agency based on the literature (e.g., Haggard, 2008, 2017; Moore, 2016), when they helped the outgroup individual by preventing him from receiving a painful shock compared to when they did not help him. Interestingly, this effect was not present for players of the control game. Past literature already showed that the sense of agency, as measured through the method of time perception, is higher for actions with positive outcomes for oneself or others compared to actions with negative outcomes (Caspar et al., 2018; Takahata et al., 2012; Yoshie & Haggard, 2013). However, those results have not consistently been replicated (e.g., Caspar et al., 2020; Moreton et al., 2017; Moretto et al., 2011) and may reflect differences in experimental set-up regarding the predictability of the outcome (e.g., Tanaka & Kawabata, 2021) or regarding one's own self-judgment about the positivity of the action (Caspar et al., 2016). The present study was not designed to provide a solution for this ongoing debate on the effect of outcome valence on the sense of agency. However, it may provide additional evidence demonstrating that actions that produce positive outcomes for others, even at the expense of their own well-being (i.e., losing a small monetary gain), can lead to an increase in the sense of agency. Numerous studies conducted in psychology and neuroscientific fields have shown that prosocial actions are rewarding (Drayton & Santos). For instance, people tend to report higher well-being after engaging in prosocial actions such as donations to charity (Thoits & Hewitt, 2001) or spending money on others rather than oneself (Dunn et al., 2008). Neuroscience research has shown that when people make charitable donations, it activates brain regions in the mesolimbic systems associated with reward processing (Moll et al., 2006). It is possible that after playing the test game, participants experienced higher forms of reward helping the outgroup rather than himself. This may be due to the game's storyline which shows the benefits of helping a fictional minority group for the welfare and prosperity of a fictional society. This increased perception of positivity associated with helping the outgroup may have led to an increase in the sense of agency. However, further studies are necessary to confirm this mediating link.

Curiously, we did not replicate the classical pattern of reduced prosociality towards outgroup individuals compared to ingroup individuals as classically reported in previous studies (Amodio & Cikara, 2021; Borinca et al., 2021). In the intended prosociality task, the two groups of players selected the outgroup individual more frequently as the recipient of the intended prosocial actions compared to the ingroup individuals. In the costly helping task, the number of helping actions did not differ between the outgroup individual and the ingroup individuals for players of the control game. However, players of the test game helped the outgroup individual more than the ingroup individuals. These two tasks, the costly helping task, and the intended prosociality task, differed in the extent to which participants may feel close or distant from the outcomes of their actions, as the tasks respectively involve making decisions that have a direct consequence on someone else (i.e., physical pain), or making decisions that only have imagined consequences on someone else. In the intended prosociality task, there were no implications for choosing the outgroup individual more frequently than the ingroup individuals. The process by which people's decisions may be guided by their willingness to appear fair or good to the experimenter (e.g., Franzen & Pointner, 2012) may explain why the two groups displayed a higher prosociality towards the outgroup individual compared to the ingroup individuals. However, in the costly helping task, which involves "real" consequences for oneself and others, only players of the test game displayed a higher prosociality towards the outgroup individual compared to the ingroup individuals. This may be indicative of the notion that test game players were more willing to reduce their perceived prejudice towards outgroup individuals, even at their own expense (i.e., losing a monetary gain). However, as we did not investigate how participants felt after the experience, this is only an assumption. The results on the overall lack of higher prejudice towards outgroup individuals compared to ingroup individuals would nonetheless warrant further investigation from future research. Our participants were recruited in Brussels, a highly-culturally diverse city and home to the headquarters of the European Commission and NATO. Due to this increased form of diversity, people living in Brussels frequently interact with people coming from different cultural and ethnic backgrounds. According to the contact hypothesis (Pettigrew, 1986), contacts with outgroup minorities favors the reduction of prejudice towards outgroup individuals, which may broadly explain why we did not observe high rates of prejudice towards outgroup individuals. However, to confirm this hypothesis, the same study should be replicated on individuals coming from less diverse environments. It would also provide the opportunity for research to determine the sizeable impact of the game on different population samples.

Interestingly, even though we observed in the intended prosociality task that players of the control game selected the outgroup individual more frequently as the recipient of the intended prosocial actions, we observed that it nonetheless led to a higher cognitive conflict to select the outgroup individual compared to selecting the ingroup individuals. We indeed observed a higher midfrontal theta power prior to the keypress when they selected the outgroup individual compared to the ingroup individuals, an effect that was not present for players of the test game. Previous studies have outlined that stronger cognitive conflict elicits higher activity in the midfrontal theta (4–8 Hz) compared to low conflicts (Cohen & Cavanagh, 2011; Cohen, 2014; Cohen & Ridderinkhof, 2013; Nigbur et al., 2012). This may suggest that players of the control game experienced more conflict when choosing the outgroup over the ingroup and that selecting the outgroup was a less natural behavior. These findings are congruent with previous research showing increased theta activity before selecting an outgroup individual compared to an ingroup individual Pech, Gishoma, & Caspar, 2022. Interestingly, playing the test game appears to remove the conflict associated with selecting the outgroup individual rather than the ingroup individuals. We also observed that visualizing an ingroup vs outgroup pair led to a higher midfrontal theta power for players of the control game compared to visualizing an ingroup vs ingroup pair. Again, this difference was not present for players of the test game. Our results tend to show that playing a video game designed indirectly reduce prejudice towards outgroups can reduce prejudice, here referring to how much cognitive conflict it involves, towards non-fictional minorities. These findings are important as they demonstrate that experiencing reduced conflict during action selection may lead to future changes in behavior, where in this case reflects reduced prejudice (Amodio et al., 2008).

Overall, we observed that reaction times (RT) were faster in the forced-choice condition compared to the free-choice condition. Similar results were observed in former studies (Janczyk et al., 2015; Naefgen et al., 2018; Pech et al., 2022), indicating that participants took more time to make their decisions when presented two different individuals. In a previous study, we observed that RT were longer for selecting outgroup individuals compared to ingroup individuals in a forced-choice condition in a sample composed of survivors and their children (Pech et al., 2022). Interestingly, in the present study we did not observe a difference in reaction times (RT) between the two groups when they selected the ingroup or the outgroup individuals, irrespective of the experimental condition. A possible explanation is that in the present study, intergroup biases as measured with RT were not as strong as in the previous study (Pech et al., 2022), where the outgroup individual was a former perpetrator of the genocide. We also observed that players of the test game had longer RT than players of the control game overall. As we did not observe a main effect of Group on the response-locked FMθ, this could suggest that RT here were not only a marker of cognitive conflict. Players of the test game perhaps simply took more time before making a decision but did not experience a difference in conflict compared to the players of the control game. A previous study indeed showed that RT are not systematically a reliable marker of cognitive conflict (Wong et al., 2017).

The risk taken with using fictional characters in a game is that past literature reported that participants show greater physiological arousal (e.g., Lim & Reeves, 2010) and emotional responses (e.g., de Melo et al., 2015) when interacting with humans rather than computer-controlled characters. Our participants only played in single-player mode which consisted of following the storyline of the game's main Hero and playing against the computer in the tournaments. It has been suggested that playing against other humans could reduce prejudice (Stiff & Kedra, 2020) due to the heightened arousal that players would receive in playing against someone else, a factor which may lead to increased enjoyment. Therefore, it would be interesting to further test the influence of the test game through a multi-player mode, where players would play against other human beings in the tournaments. However, using fictional characters has the benefit that it could be adapted to any population without targeting specific ethnic groups, thus leading to a better sizeable impact. Future studies that intend to reduce prejudice through video games should consider this notion and favor game modalities using fictional characters rather than non-fictional characters.

The present study thus shows promising results on the possibility of reducing prejudice towards non-fictional minorities by using a video game using fictional minorities. There are some limitations that nonetheless need to be accounted for. First, we are not entirely aware if participants truly believed the cover story regarding the presence of other real participants performing the task simultaneously. Most of them reported being unaware of this experimental trick at the end of the experiment. However, this report was informal as we did not conduct systematic interviews after each experiment which precludes our ability to know if this applied to all of the participants. We nonetheless tried to restrict the possibility for participants to guess the aim of the study by including two ingroup individuals and a single outgroup individual instead of an ingroup individual alone versus an outgroup individual alone. A second limitation is that we do not know about the initial prejudiced attitudes of our participants towards outgroup individuals before playing the game. A more rigorous assessment of the game's impact could have involved performing the same tasks before and after playing the game and including a re-test control group for evaluating the effect of performing the same tasks twice (Shawn Green et al., 2019). Third, similar to the huge majority of studies on the impact of different interventions on reducing prejudice towards outgroup members (e.g., Paluck et al., 2021), we did not investigate the game's long-term effects, thus precluding conclusions on the sustainability of the intervention. A critical societal challenge consists in evaluating the potential long-term effects of interventions aiming at reducing prejudice whereby future studies should also include a longitudinal assessment of their effects.

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Authors contributions

E.A.C. and G.P.P developed the study concept and the study design. G.P.P recruited and tested participants. E.A.C. and G.P.P. analyzed the results. E.A.C. wrote the first draft of the manuscript and G.P.P. provided comments. The two authors approved the final version of the manuscript.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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ORCID

Guillaume P. Pech http://orcid.org/0000-0002-8843-6604
Emilie A. Caspar http://orcid.org/0000-0003-1415-2032

Data availability statement

Data and scripts are made available on OSF (https://osf.io/9cjme/ – DOI 10.17605/OSF.IO/9CJME).

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About the authors

Guillaume P. Pech is a PhD student at the Université libre de Bruxelles (Belgium) and at Chapman University (U.S.A.), funded by a F.R.S.-FNRS grant.

Emilie A. Caspar is an Associate Professor at Ghent University (Belgium), where she currently leads the Moral and Social Brain lab (https://moralsocialbrain.com/).