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A Novel Electroencephalography-Based Paradigm to Measure Intergroup Prosociality: An Intergenerational Study in the Aftermath of the Genocide Against Tutsis in Rwanda

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Studying how intergroup prosociality evolves in war-torn societies is critical for gaining a better understanding of conflict perpetuation. Rwanda provides a unique example of how two groups must reconcile and manage their intergroup biases following a genocidal process. In this study, we employed a novel intended behavior task to measure intergroup prosociality among former genocide perpetrators, genocide survivors, and their children in Rwanda. Participants were required to choose between various individuals representing their own in-group or their out-group as recipients of their prosocial intentions. We measured how frequently they selected in-group or out-group individuals and to what extent choosing each individual induced cognitive conflict, as measured by reaction times (RTs) and midfrontal theta (FMθ) activity. The results indicated that survivors and their children selected former perpetrators and their offspring less frequently. Furthermore, selecting them involved a higher cognitive conflict, as evidenced by longer RT and a higher FMθ, compared to choosing their own in-group. For the group composed of former perpetrators and their children, we observed a dissociation. They selected out-group individuals more frequently, perhaps as a compensatory behavior for their past wrongdoings. Nonetheless, selecting the out-group individuals involved a higher cognitive conflict than selecting their own in-group. Importantly, we observed a similar intergroup prosociality bias in the children of both survivors and former perpetrators, mirroring that of their parents. These results are important for understanding how past conflicts influence intergroup prosociality bias and the extent to which this bias is transmitted to the next generation.

Public Significance Statement

Conflicts enhance intergroup biases, a phenomenon that can hinder complete reconciliation between two previously conflicting groups. In this study conducted in Rwanda, we showed that former genocide perpetrators and survivors continue to exhibit intergroup biases toward the opposing group, and this effect is also observed in their offspring. These findings contribute to a better understanding of the perpetuation of conflicts.

Keywords: intergroup biases, war-torn societies, transmission, prosociality, EEG

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Humans can undoubtedly act in highly prosocial ways toward others, even when the situation involves costs for oneself and benefits for others. Prosociality encompasses a wide range of behaviors, including helping others, donating time or money, being an organ donor, or providing comfort to others (Batson & Powell, 2003; Pavey et al., 2011; Van Tongeren et al., 2016) and is an important indicator of social and economic development (Andriani & Sabatini, 2015; Meier, 2006). However, these behaviors can significantly depend

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on the recipient's identity (De Dreu et al., 2022; Louis et al., 2019). Numerous studies have consistently shown that perceiving an individual as an out-group member tends to reduce prosociality toward them compared to in-group members (Borinca et al., 2021; Hein et al., 2010; Stürmer et al., 2005). We refer to this phenomenon as the intergroup prosociality bias.

A critical factor influencing intergroup biases is the strained relations that persist following a conflict between different human groups. Indeed, it has been argued in the literature that conflicts amplify intergroup biases (Choi & Bowles, 2007; Gerstenfeld, 2002), including intergroup prosociality (Mironova & Whitt, 2016). However, prosociality also plays a crucial role in understanding how societies affected by war can achieve reconciliation (Mironova & Whitt, 2016; Nadler & Shnabel, 2015) and prevent future conflicts (Fehr & Fischbacher, 2004; Ostrom, 2000). Therefore, comprehending how intergroup prosociality develops in the aftermath of a conflict is essential for gaining a better understanding of how reconciliation can be achieved between groups that were previously in conflict.

In Rwanda, citizens are exposed to a unique case of intergroup reconciliation and must grapple with their intergroup biases, which have been caused by decades of ethnic conflicts and the genocide against Tutsis in 1994 (Newbury, 1998). The genocide had devastating consequences for almost the entire Rwandan population. Over a period of approximately 100 days, nearly 1 million Rwandan citizens, predominantly Tutsis but also moderate Hutus, was massacred in a genocidal process. What is particularly chilling is that they were not killed by foreign invaders; rather, they were brutally murdered by their fellow citizens and neighbors due to their ethnicity or for opposing the ongoing massacres. Survivors and former perpetrators are now expected to reconcile in order to preserve peace in the country and live side by side. Understanding the intergroup prosociality bias is highly relevant in Rwanda. However, this bias has been largely understudied in societies that have overcome a dramatic event, wherein one group decided to massacre the other. Does the intergroup prosociality bias disappear after years of peace between two formerly conflicting groups, despite the brutality of the events? Is this bias also observable in the next generation of individuals? In the present study, we recruited former genocide perpetrators and survivors to examine the intergroup prosociality bias in the aftermath of the genocide against Tutsis. We also systematically recruited one of their children to investigate the extent to which the intergroup prosociality bias is passed down to the next generation of individuals.

In the literature, two main methods have been used for measuring prosocial behaviors, either relying on behavioral measurements in experimental setups (Darley & Latane, 1968; Murphy & Ackermann, 2014; Van Lange et al., 2013) or relying on the use of fake scenarios and self-reported questionnaires (Agerström & Björklund, 2009; Baumsteiger & Siegel, 2019; Greene et al., 2001; Pavey et al., 2011). While measuring prosocial behaviors in experimental setups has the advantage to capture behaviors in a more ecological way than by using fake scenarios, the protocols can be time-consuming and involve complicated setups (Crano et al., 2014). As a consequence, researchers can usually only measure a single type of behaviors per study. Furthermore, some prosocial behaviors are hard to investigate in an experimental setting, such as helping someone to move or spending time with someone. Using fake scenarios and self-report questionnaires is less time-consuming and allows for measuring a wider range of prosocial attitudes or intentions (Baumsteiger & Siegel, 2019). However, it is widely acknowledged that such measurements are highly sensitive to social biases and thus have limited ecological validity and reliability in reflecting real-life behaviors. Participants may not be fully sincere in imagined scenarios and, for instance, exhibit high levels of prosociality toward out-group individuals to appear more fair to the experimenter (Amodio et al., 2008).

In the neuroscientific literature, a promising approach consists of evaluating the extent to which selected behaviors involve cognitive conflict. For instance, previous research has demonstrated that decisions that are difficult to make are associated with longer reaction times (RTs; Cohen & Donner, 2013; Greene et al., 2004) and higher midfrontal theta (FMθ) activity (4–8 Hz; Cavanagh & Frank, 2014; Cohen & Cavanagh, 2011; Cohen, 2014; Cohen & Ridderinkhof, 2013; Nigbur et al., 2012) compared to easier decisions. This FMθ activity can be interpreted as reflecting a cognitive demand prior to decision making. Specifically, choices that elicit more conflict require greater cognitive resources, leading to increased FMθ activity (Cavanagh & Frank, 2014).

In the present study, we developed a novel paradigm called the intended behavior task to assess prosocial intentions using a combination of behavioral, implicit, and neural measurements. This paradigm allows for the inclusion of highly diverse prosocial scenarios that can be easily tailored to the target population. We created four distinct prosocial scenarios that are commonly encountered in the daily lives of Rwandan citizens, such as offering comfort to someone or providing a bed for the night. Participants were initially presented with pictures and stories of three individuals. These individuals included: one in-group individual (e.g., for the child of a survivor, we presented the offspring of a survivor), one out-group-adult individual (e.g., for the child of a survivor, we presented an actual former perpetrator), and one out-group-offspring individual (e.g., for the child of a survivor, we presented the offspring of a former perpetrator).

During the task, participants were required to select the recipient of their prosocial intention from pairs of individuals. On each trial, two individuals were presented, and they could either be the same (i.e., forced-choice) or different (i.e., free-choice). At the behavioral level, we calculated the percentage of selections for each individual to determine participants' preferences regarding their prosocial intentions. Additionally, we measured RT and FMθ activity before the selection to assess the extent to which choosing to act prosocially toward each individual involved cognitive conflict. In the literature on prejudice using electroencephalography (EEG), several studies have reported higher cognitive conflict when participants had to inhibit prejudice toward an out-group compared to not inhibiting prejudice (Amodio & Swencionis, 2018; Amodio et al., 2006, 2008; Bartholow et al., 2006; Correll et al., 2006). However, these studies focused on measuring cognitive conflict elicited by the presentation of stimuli within time-domain event-related potentials, such as the visualization of faces measured with the P300 component. They did not specifically investigate the cognitive conflict elicited by the decision leading to a specific behavior. Since decisions do not have a precise onset time and are not time-locked, relying on FMθ was the most suitable analysis approach. To the best of our knowledge, no previous studies have used FMθ as an index of cognitive conflict to measure prejudice. By combining behavioral measures, RT, and FMθ, we can observe potential dissociations between what individuals explicitly choose to show and how their brain processes the decision. This approach also provides insights into the mechanisms underlying in-group preferences.

A previous study using self-reported questionnaires showed that in-group preferences for prosociality persist even decades after a conflict (Mironova & Whitt, 2016). Therefore, we expected to observe a higher frequency of selecting the in-group individual compared to the out-group individuals. However, the effect could be reversed for the group consisting of former perpetrators. In a previous study, researchers assigned Israeli Jewish participants to either a victim or perpetrator role (Nadler & Shnabel, 2015). Those assigned to the perpetrator role reported an increased need to restore moral identity and displayed more prosocial behavioral tendencies toward Palestinians, while those assigned to the victim role exhibited more antisocial tendencies toward Palestinians. In Rwanda, perpetrators were heavily blamed for the events of 1994, triggering feelings of guilt (Kanyangara et al., 2014). Guilt is a powerful social emotion that drives individuals to seek repair (Haidt, 2003). Consequently, perpetrators may choose to favor the out-group individuals, composed of survivors and their offspring, as a compensatory behavior. However, at the implicit and neural levels, we may still observe an intergroup prosociality bias. This would align with a previous study (Caspar, Pech, et al., 2022) that found the presence of an intergroup empathy bias, characterized by reduced neural processing of the pain of out-group compared to in-group members (Amodio & Cikara, 2021), among former perpetrators, survivors, and their children in the aftermath of the genocide against Tutsis in Rwanda. Therefore, we anticipated that selecting the in-group individual would be easier, resulting in lower cognitive conflict (i.e., shorter RT, lower FMθ) compared to selecting the two out-group individuals, regardless of the participants' groups.

To determine whether the intergroup prosociality bias is transmitted to the next generation, we examined whether the children of survivors and former perpetrators exhibited a similar pattern of results as their parents. As observed in a previous study, the intergroup empathy bias was similar between parents and their children (Caspar, Pech, et al., 2022). Therefore, we anticipated similar findings in the current study, with the children displaying intergroup biases akin to those of their parents.

Method

Participants

We recruited 108 families ($N = 216$) for our study, systematically including an individual who was either recognized as a genocide survivor or a genocide perpetrator, along with one of their children (ages 16–35). The determination of the sample size was conducted with a power of 95%. Since no previous studies had examined the present research question, especially on non-Western individuals, we used a standard low-to-medium effect size ($f = 0.175$). The sample size calculation was performed using GPower, based on our interaction of interest, which involved an interaction between generation (two levels) and individual (three levels) for both the families of survivors and the families of former perpetrators. The estimated total sample size was $N = 86$ (i.e., $N = 44$ per generation) for each family type. However, to account for potential data loss due to testing conditions and to ensure flexibility with our inclusion criteria (see below), we recruited a larger sample: $N = 110$ in the families of former perpetrators (i.e., 55 former perpetrators and one of their children) and $N = 106$ in the families of survivors (i.e., 53 survivors and one of their children). To identify survivors and former perpetrators, we recruited participants through Prison Fellowship Rwanda, a local community-based organization that works with individuals officially recognized as former perpetrators or survivors, aiming to promote reconciliation. As compensation for their participation, recruited participants received a fee of RWF 20,000.

In our analysis, we included an additional between-subject factor that was not initially considered in the reported power calculations. In Rwanda, there are eight reconciliation villages that have been established as part of the reconciliation strategy. These villages allow former perpetrators, survivors, and their families to live alongside each other and engage in socio-therapy activities. The purpose of these villages is to enhance intergroup relations and promote forgiveness between survivors and former perpetrators, as well as their children. When we arrived in Rwanda, we were uncertain whether we would have access to these villages, as travel between districts was restricted due to the COVID-19 pandemic. However, we ultimately succeeded in recruiting half of our sample from these villages. We incorporated this between-subject factor into our analysis; however, it was not initially planned in the power calculation and is therefore exploratory.

Half of our sample was recruited and tested in the district of Kayonza, while the other half was recruited and tested in the district of Bugesera. These districts had their COVID-19 restrictions lifted in August 2021 when the testing took place. Similar to a previous study (Caspar, Pech, et al., 2022), we had to be more flexible with our inclusion criteria compared to classical neuroscience projects conducted on the WEIRD (Western, Educated, Industrialized, Rich, Democratic) population. Approximately half of our sample could not read Kinyarwanda as they had never attended school, and roughly half of them had never seen or used a computer in their entire lives. Additionally, the former genocide perpetrators had spent years in jail and were relatively old at the time of testing. Achieving a gender balance in the group of former genocide perpetrators was impossible for two reasons. Firstly, there were significantly fewer female perpetrators compared to male perpetrators (Verwimp, 2005). Secondly, women were not considered potential perpetrators after the genocide and were therefore not imprisoned like men. During the Gacaca trials, several female perpetrators were identified and sentenced to jail. Consequently, a considerable number of female perpetrators were still incarcerated during our testing. Our inclusion criteria were simply based on being officially recognized as a genocide survivor or perpetrator and having one child aged between 16 and 35. Exclusion criteria established a priori included not understanding the task ($N = 3$) or refusing to participate despite their relative's involvement ($N = 2$). Since this population had never been approached by neuroscience projects before, we could not anticipate all the exclusion criteria in advance. A posteriori-exclusion criteria included being intoxicated and unable to perform the task at the time of testing ($N = 1$), discontinuing the testing due to a storm ($N = 1$), or suspected low intelligence quotient (IQ; $N = 1$). In the case of trauma revival during the experiment, we followed a procedure established in accordance with the Rwanda National Ethics Committee, which involved follow-up by a trained clinical psychologist. One survivor was unable to complete the task due to trauma revival in a preceding task and was consequently excluded from the sample.

After exclusion criteria, our final sample was composed of 207 individuals. In the group of genocide survivors, 20/51 were males and the mean age was 50.43 ($SD = 9.53$) and 25/51 were living in a reconciliation village. In the group of former genocide perpetrators, 51/51 were males and the mean age was 58.98 ($SD = 7.94$) and 30/51 were living in a reconciliation village. In the group of children of genocide survivors, 30/51 were males and the mean age was 20.38 ($SD = 4.26$) and 22/52 were living in a reconciliation village. In the group of children of genocide perpetrators, 28/53 were males and the mean age was 20.26 ($SD = 4.65$) and 31/53 were living in a reconciliation village. The study has been approved by the Rwanda National Ethics Committee (Ref: 167/RNEC/2021).

Material and Method

We conducted the experiment directly in the villages of our volunteers, utilizing buildings that had a minimum of two electrical plugs and provided relative quietness, such as churches or available spaces for public events. Participants were invited to sit in front of a computer screen. Since several buildings did not have isolated rooms, we constructed isolation walls using wooden planks to minimize surrounding distractions (e.g., curious children, goats, etc.). For further details on the planning and execution of the present study, please visit https://emiliecaspar.home.blog/ and refer to the section titled "Behind the paper—Rwanda." To record brain activity, we utilized a 64-channel EEG system. The study was part of a broader research project that also investigated the intergroup empathy bias (Caspar, Pech, et al., 2022) and resting-state connectivity.

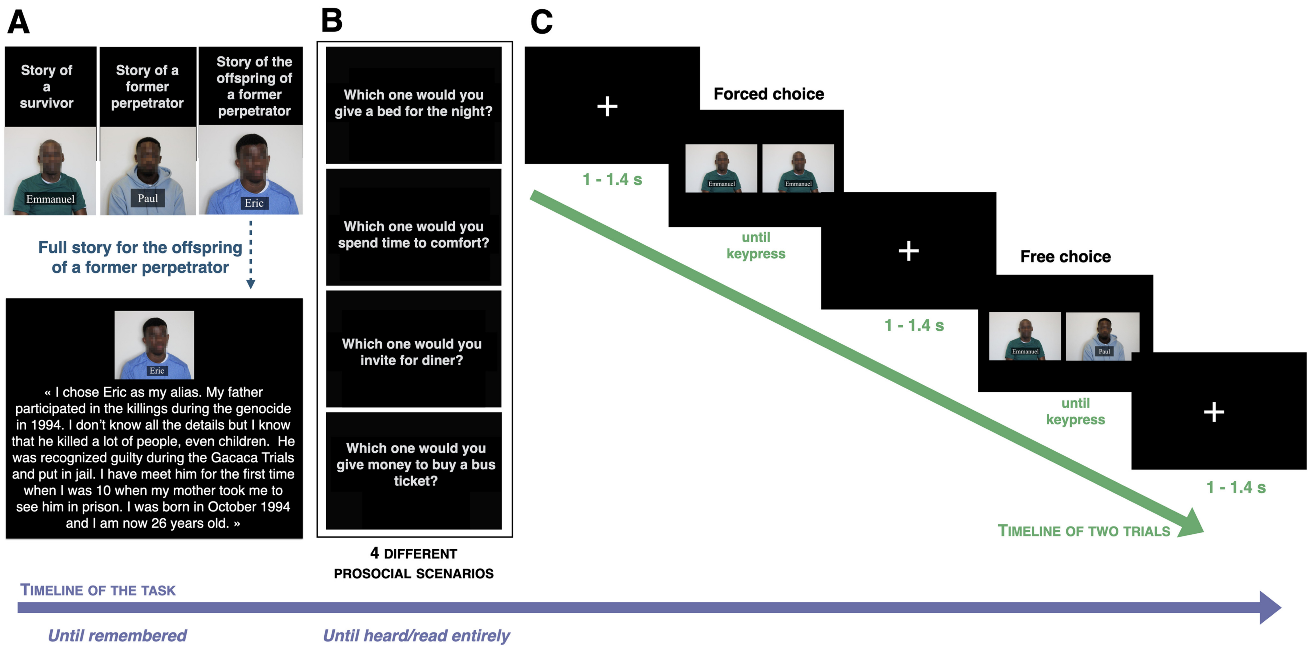
Participants were presented with three individuals on the screen, but the composition of these individuals varied for each group of participants (see Figure 1A). Specifically, for each group, we selected an individual representing the in-group (e.g., a survivor for the group composed of survivors), an individual from the out-group of the same generation (e.g., a former perpetrator for the group composed of survivors), and an individual from the out-group of the following (or previous, in the case of children) generation (e.g., the child of a former perpetrator for the group composed of survivors). To control for gender, only male individuals were displayed in the pictures, as there was a higher percentage of male perpetrators during the genocide compared to female perpetrators (Verwimp, 2005). Importantly, participants also heard these individuals' stories in another task involving the same individuals presented (see Caspar, Gishoma, & Magalhaes de Saldanha da Gama, 2022; Caspar, Pech, et al., 2022), allowing them sufficient time to fully comprehend the stories. The study was conducted by the authors with the assistance of two research assistants who spoke Kinyarwanda and were able to answer participants' questions.

We informed our participants that the individuals presented in the study had agreed to share their stories or the stories of their parents during the genocide, in order to improve the ecological validity of the task. We explained that their anonymity needed to be preserved, and therefore their faces were blurred. Given that the genocide was committed in the name of different ethnicities, which were described as physically distinct, we took measures to ensure that the six individuals depicted had relatively similar skin color and body shape. We personally took the pictures of several individuals who agreed to be photographed in Belgium, and they wore colored shirts to facilitate identification despite the blurred faces. The stories associated with the individuals were randomly assigned between participants to prevent any effects related to a specific individual. Prior to the experimental task, participants were presented with pictures of the three individuals, and their respective stories were presented both orally and in written form in Kinyarwanda. This approach catered to the needs of illiterate individuals and aligned with Rwanda's tradition of oral transmission. The stories were based on actual events that some people experienced during the genocide, such as hiding in swamps to survive or being involved in killings and subsequently sentenced to prison during the Gacaca Trials. We emphasized the importance of maintaining the anonymity of the individuals who shared their stories. To ensure that physical distinctions among the different categories of individuals were not created, which would be ethically unacceptable given that the genocide occurred between two physically distinct ethnic groups, we carefully selected individuals with relatively similar skin color and body shape for the pictures. The association of pictures and stories varied randomly among participants

When participants finished reading the story of each individual, they were asked to press a key on the keyboard to begin the task. They were then presented with several prosocial scenarios that could reflect real acts of prosociality in Rwandan society (see Figure 1B). For example, participants had to decide whom they would give money to buy a bus ticket or whom they would provide a bed for the night. To aid nonreaders in understanding the scenarios, the scenarios were also presented orally in Kinyarwanda, and a relevant picture linked to the scenario was systematically displayed (e.g., a picture of a bed for the scenario involving the decision of whom to provide a bed for the night). Participants were instructed to press a key on the keyboard once they finished reading or listening to the scenario. After the scenario presentation, the trial began with a jittered fixation cross lasting between 1 and 1.4 s. Six combinations of pairs of individuals were then presented (see Figure 1C). The pairs consisted of either the same individual presented twice (forced-choice condition) or a pair with two different individuals (free-choice condition). Since participants had complete freedom in the free-choice condition, there was a possibility of a lack of data toward one of the individuals if they were never selected. The forced-choice condition was introduced to ensure an adequate number of trials and measurements for all individuals. Additionally, Naefgen et al. (2018) found that RT in free-choice decisions involves additional processes compared to forced-choice decisions. This increased complexity may introduce more biases in the interpretation of cognitive conflict for free-choice decisions compared to forced-choice decisions.

Participants were instructed to press either the left or right arrow key on the keyboard as quickly as possible to indicate their preference for the individual mentioned in the prosocial scenario. After each key press, a jittered fixation cross appeared on the screen for 1–1.4 s, ensuring a noise-free baseline for EEG recordings. Once the six combinations of pairs of individuals were randomly presented, another scenario appeared on the screen, and the same procedure was repeated. Each scenario was presented eight times in a randomized order, followed by six combinations of pairs of individuals. The six combinations consisted of three for forced-choice trials and three for free-choice trials, as follows: (a) in-group–in-group, (b) out-group$_{\text{ADULT}}$–out-group$_{\text{ADULT}}$, (c) out-group$_{\text{OFFSPRING}}$–out-group$_{\text{OFFSPRING}}$, (d) in-group–out-group$_{\text{ADULT}}$, (e) in-group–out-group$_{\text{OFFSPRING}}$, and (f) out-group$_{\text{ADULT}}$–out-group$_{\text{OFFSPRING}}$. Each combination was presented 32 times, resulting in a total of 192 trials. We chose this number to avoid participant fatigue during the task, as some of our participants were older and had never used a keyboard in their entire lives. The task was cognitively demanding

Figure 1
Schematic Representation of the Task



Note. (A) The three individuals with their respective stories were presented orally and in written form in Kinyarwanda. (B) One scenario was then presented orally and in written form in Kinyarwanda again. (C) Participants were then presented with six different combinations of individuals, with a jittered fixation cross lasting between 1 and 1.4 s between each presentation. Participants were instructed to press either the left or right arrow key on the keyboard as quickly as possible to indicate the recipient of their prosocial decision. See the online article for the color version of this figure.

for some participants, so we aimed for a shorter duration. Consequently, the task duration ranged from 10 to 20 min, depending on the reactivity of the participant.

Data Analysis EEG

EEG Recordings

EEG recordings were conducted using Biosemi equipment (see http://www.biosemi.com for hardware details), with data acquired at a sampling rate of 2,048 Hz from 64 channels placed according to the international 10–20 system. Additionally, four additional electrodes were used to capture horizontal eye movement and mastoid signals. The data acquisition was performed using the ActiView software.

EEG Processing

Data were processed using MNE-Python (Gramfort et al., 2013). We applied a bandpass filter between 0.1 and 40 Hz. The data were then downsampled to 512 Hz and a notch filter with a bandwidth of 5 Hz was applied using a zero-double phase. The increased bandwidth was necessary due to nonstationary power noise observed in rural villages in Rwanda, where the electric systems have rudimentary infrastructure with frequencies oscillating around 50 Hz. As the mastoids did not provide good signal quality for several participants, likely due to sweat behind the ears caused by the temperatures, we rereferenced the signal by averaging the data from the 64 electrodes. A copy of the data was created for applying automatic independent component analysis with 60 components, using a high-pass filter of 1 Hz. To detect eye movements such as blinks and saccades, the find_bad_eog function in MNE-Python was utilized, which correlates independent components with electrodes labeled as electrooculography. The two external electrodes recording horizontal eye movement and the "Fp1" and "Fp2" electrodes, closest to the eyes, were used for detecting vertical eye movements. Independent components exceeding the threshold (based on adaptive z scoring) were removed ($M = 5.6$, $SD = 1.5$) from the original data, and the copy was no longer used. The data were then epoched from −2 to 2 s. For interpolating bad channels, the Ransac method from the Autoreject library was employed ($M = 19.2$, $SD = 11.1$; Bigdely-Shamlo et al., 2015). To correct direct current offsets and head movements, the robust detrend method (de Cheveigné & Arzounian, 2018) was used with the detrend() function from the MEEGkit library (https://github.com/nbara/python-meegkit). Artifact rejection was done using the autoreject() function (Jas et al., 2017). This function first interpolates noisy epochs and then removes them if the interpolation is deemed insufficient ($M = 13.9$, $SD = 18.9$).

Time–Frequency Representation (TFR) Analysis

The TFR was analyzed on Matlabr2018a software. Based on previous studies, we measured frontal FMθ on FCz (Cohen & Cavanagh, 2011; Cohen & Donner, 2013). We extracted the time–frequency power of each trial after calculating a fast Fourier transform (FFT) on the data and an FFT of a complex Morlet wave with the following parameters: frequency from 2 to 40 Hz with 80 bins spaced logarithmically and 4–14 cycles logarithmically spaced. Then, we used the inverse fast Fourier transform method on the multiplication of the computed FFT on our data and the FFT of the complex Morlet Wave. We used epochs of −2 to 2 s around the keypress (response-locked). All power values in the TFR were normalized to the average poststimulus 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 to 1.2 s after the keypress. Based on visual inspection of the grand average data, we extracted the FMθ power within specific time and frequency windows. The time window was from −1.3 s to −460 ms before the keypress, and the frequency window was from 3.3 to 7.3 Hz (refer to Figure 4 for visualization). For each participant, we calculated the mean value within this defined time-frequency window.

Transparency and Openness

All data, analysis codes, and research materials are available on Open Science Framework (https://osf.io/weq9g/). Data were analyzed using R and JASP.

Statistical Analyses

Our main research question was to investigate whether the intergroup prosociality bias still existed between survivors and former perpetrators of the genocide and how this bias was transmitted to their children. Since the respective "in-group" and "out-group" differed between the groups (e.g., for survivors and their children, the out-group was the former genocide perpetrator and his children), they could not be included in the same repeated-measures analysis of variance (ANOVA). Therefore, we divided the participants into two main groups: the "survivors and their children" group, which included actual survivors and one of their children, and the "former perpetrators and their children" group, which included former perpetrators and one of their children. To ensure that the two groups had a relatively similar age, we conducted an independent samples *t* test. The *t*-test results revealed no significant difference between the two groups ($p > .1$).

As RT can vary significantly among participants of different ages (Dykiert et al., 2012), and considering the high variability in age within each group in our sample, we *z*-scored the RT for each participant (Hedge et al., 2018). The distribution of the raw RT for each group can be found in Figure S1 in Supplementary 2 in the online supplemental materials. RT values below 350 ms were excluded from the analysis (Cohen & Donner, 2013; Semmelmann & Weigelt, 2017). Additionally, we removed trials that deviated by more than 2.5 median absolute deviation from the mean for each participant (Leys et al., 2013).

We identified extreme outlier participants using boxplots in SPSS. In SPSS, extreme outliers are defined as values that are either greater than the third quartile plus three times the interquartile range or less than the first quartile minus three times the interquartile range (link). For RT, we excluded one outlier in the group of survivors and their children and three outliers in the group of former perpetrators and their children. For FMθ activity, we excluded one outlier in the group of former perpetrators and their children. Additionally, we did not analyze the RT and FMθ for participants who selected one of the three individuals presented less than 10 times. We considered that there were not enough trials to perform reliable statistical analysis in such cases. We could not solely rely on the trials in the forced-choice condition as we made a conceptual distinction with the free-choice conditions. Furthermore, a participant could have fewer than 10 selections even in the forced-choice condition if most of their RT values in those trials were either too long or too short. This was observed for five participants (four in the group of survivors and their children, and one in the group of former perpetrators and their children).

The intended prosocial behavior toward each of the individuals (i.e., in-group, out-group$_{\text{ADULT}}$, and out-group$_{\text{OFFSPRING}}$) was measured by counting the number of times an individual was selected in the free-choice condition. Since a few participants had a different number of trials (e.g., due to power cuts or time constraints), we calculated a percentage of selections for each individual. This was done by dividing the number of times an individual was selected by the total number of selections and multiplying it by 100. In an ideal scenario where each individual is selected equally, the percentage of choice would be 33.33 per individual in the free-choice condition (i.e., each individual is selected in 1/3 of the trials). For instance, if a participant consistently chose the in-group individual in every possible combination during the free-choice condition, the in-group individual would have a percentage of selection of 66.66 (i.e., 2/3 of the total). Therefore, a percentage of selection above 33.33 for a particular individual indicates that the individual was chosen more frequently compared to one or both of the other individuals. Conversely, a percentage of selection below 33.33 suggests that the individual was chosen less often than one or both of the other individuals.

To compare the intended behavior, we conducted a repeated-measures ANOVA in the free-choice condition, with individual (in-group, out-group$_{\text{ADULT}}$, and out-group$_{\text{OFFSPRING}}$) as the within-subject factor, and generation (parent, child) and living in a Reconciliation Village (yes, no) as the between-subject factors. The inclusion of the between-subject factor "generation" allowed us to assess whether the observed effects were similar between parents and their children. We used RT and FMθ activity as measures of cognitive conflict, where longer RT and higher FMθ indicated a higher level of cognitive conflict associated with the decision-making process. For both RT and FMθ, we conducted repeated-measures ANOVAs with condition (free-choice, forced-choice) and individual (in-group, out-group$_{\text{ADULT}}$, and out-group$_{\text{OFFSPRING}}$) as the within-subject factors, and generation (parent, child) and living in a Reconciliation Village (yes, no) as the between-subject factors.

In order to explore whether the intergroup effect observed in children could be influenced by the intergroup effect observed in their parents, we conducted exploratory Pearson correlations between the values of parents and their respective children. The intergroup effect was calculated by subtracting the results of the combined out-groups from the in-group—i.e., in-group − [(out-group adult + out-group offspring)/2]—for the percentage of selections, RT, and FMθ. No corrections for multiple comparisons were performed since each column was only correlated once between parents and children. All descriptive statistics can be found in Supplementary 1 in the online supplemental materials.

Results

Intended Prosocial Behavior

In the "survivors and their children" group, we observed a main effect of individual, $F(2, 200) = 52.701$, $p < .001$, $\eta^2_{p=} .345$ (see

Figure 2). Paired sample t tests revealed more frequent selection of the in-group individual ($M = 40.0\%$, $SD = 7.8$) compared to both the out-group$_{ADULT}$ individual, $M = 27.32\%$, $SD = 8.03$; $t(103) = 8.751$, $p < .001$, Cohen's $d = 0.858$, and the out-group$_{OFFSPRING}$ individual, $M = 32.63\%$, $SD = 5.52$; $t(103) = 6.967$, $p < .001$, Cohen's $d = 0.683$. We also observed a higher selection of the out-group$_{OFFSPRING}$ individual compared to the out-group$_{ADULT}$ individual, $t(103) = -4.759$, $p \leq .001$, Cohen's $d = -0.467$. None of the other main effects or interactions were significant (all $ps > .1$).

In the "former perpetrators and their children" group, we observed a main effect of individual, $F(2, 204) = 23.942$, $p < .001$, $\eta_p^2 = .190$, with a lower selection of the in-group individual, $M = 28.44\%$, $SD = 7.68$, compared to both the out-group$_{ADULT}$ individual, $M = 35.6\%$, $SD = 7.8$; $t(105) = -5.317$, $p \leq .001$, Cohen's $d = -0.516$, and the out-group$_{OFFSPRING}$ individual, $M = 35.9\%$, $SD = 6.9$; $t(105) = -6.249$, $p \leq .001$, Cohen's $d = -0.607$. The difference in the number of selections between the out-group$_{ADULT}$ individual and the out-group$_{OFFSPRING}$ individual was not significant ($p > .7$). None of the other main effects or interactions were significant (all $ps > .1$).

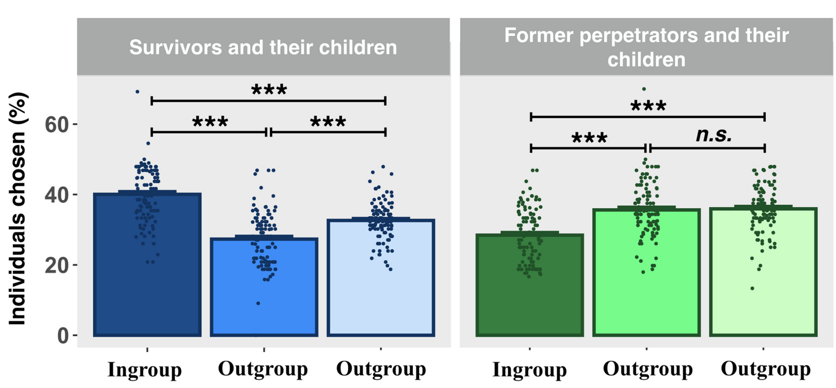
RTs

In the "survivors and their children" group, we observed a main effect of condition, $F(1, 92) = 23.831$, $p < .001$, $\eta_p^2 = .207$, with lower RT in the forced-choice condition (z-scored $M = -0.077$, $SD = .158$) than in the free-choice condition (z-scored $M = 0.084$, $SD = .167$). The main effect of individual was also significant, $F(2, 184) = 5.114$, $p = .007$, $\eta_p^2 = .053$, with lower RT when they selected the in-group individual as the recipient of the intended prosocial action (z-scored $M = -0.041$, $SD = .146$) compared to when they selected the out-group$_{OFFSPRING}$ individual, z-scored $M = 0.033$, $SD = .127$; $t(96) = -3.138$, $p = .002$, Cohen's $d = -0.319$ (see Figure 3A). We observed also a difference with lower RT when they selected the in-group individual compared to the out-group$_{ADULT}$ individual, z-scored $M = 0.023$, $SD = 0.137$; $t(95) = -2.323$, $p = .022$, Cohen's $d = -0.237$. The comparison between the out-group$_{ADULT}$ individual and the out-group$_{OFFSPRING}$ individual was not significant ($p > .7$). We observed an interaction effect Condition × Individual, $F(2, 184) = 14.689$, $p < .001$, $\eta_p^2 = .138$. In the free-choice condition, paired comparisons indicated shorter RT when they selected the in-group individual (z-scored $M = 0.073$, $SD = 0.233$) compared to the out-group$_{OFFSPRING}$ individual, z-scored $M = 0.153$, $SD = 0.238$; $t(97) = -2.685$, $p = .009$, Cohen's $d = -0.271$. We also observed shorter RT when they selected the out-group$_{ADULT}$ individual (z-scored $M = 0.038$, $SD = .279$) compared to the out-group$_{OFFSPRING}$ individual, $t(97) = -3.405$, $p < .001$, Cohen's $d = -0.344$. The comparison between the in-group and the out-group$_{ADULT}$ individuals was not significant ($p > .2$). In the forced-choice condition, paired comparisons revealed shorter RT when they selected the in-group individual (z-scored $M = -0.150$, $SD = 0.222$) compared to the out-group$_{ADULT}$ individual, z-scored $M = 0.006$, $SD = 0.302$, $t(97) = -4.210$, $p < .001$, Cohen's $d = -0.425$. RT was also shorter when they selected the out-group$_{OFFSPRING}$ individual (z-scored $M = -0.088$, $SD = 0.226$) compared to the out-group$_{ADULT}$ individual, $t(98) = 2.651$, $p = .009$, Cohen's $d = 0.266$. We observed also a difference with shorter RT when they selected the in-group individual compared to the out-group$_{OFFSPRING}$ individual, $t(97) = -2.024$, $p = .046$, Cohen's $d = -0.204$. None of the other main effects or interactions were significant (all $ps > .075$).

In the "former perpetrators and their children" group, we also observed a main effect of condition, $F(1, 95) = 7.411$, $p = .008$, $\eta_p^2 = .072$, with lower RT in the forced-choice condition (z-scored $M = -0.036$, $SD = .142$) than in the free-choice condition (z-scored $M = 0.040$, $SD = .146$). We also observed an interaction effect Condition × Individual, $F(2, 190) = 10.811$, $p < .001$, $\eta_p^2 = .102$ (see Figure 3B). In the free-choice condition, paired comparisons revealed shorter RT when they selected the in-group individual (z-scored $M = -0.022$, $SD = 0.280$) compared to the out-group$_{ADULT}$ individual, z-scored $M = 0.081$, $SD = .215$, $t(98) = -3.229$, $p = .002$, Cohen's $d = -0.325$, and the out-group$_{OFFSPRING}$ individual, z-scored

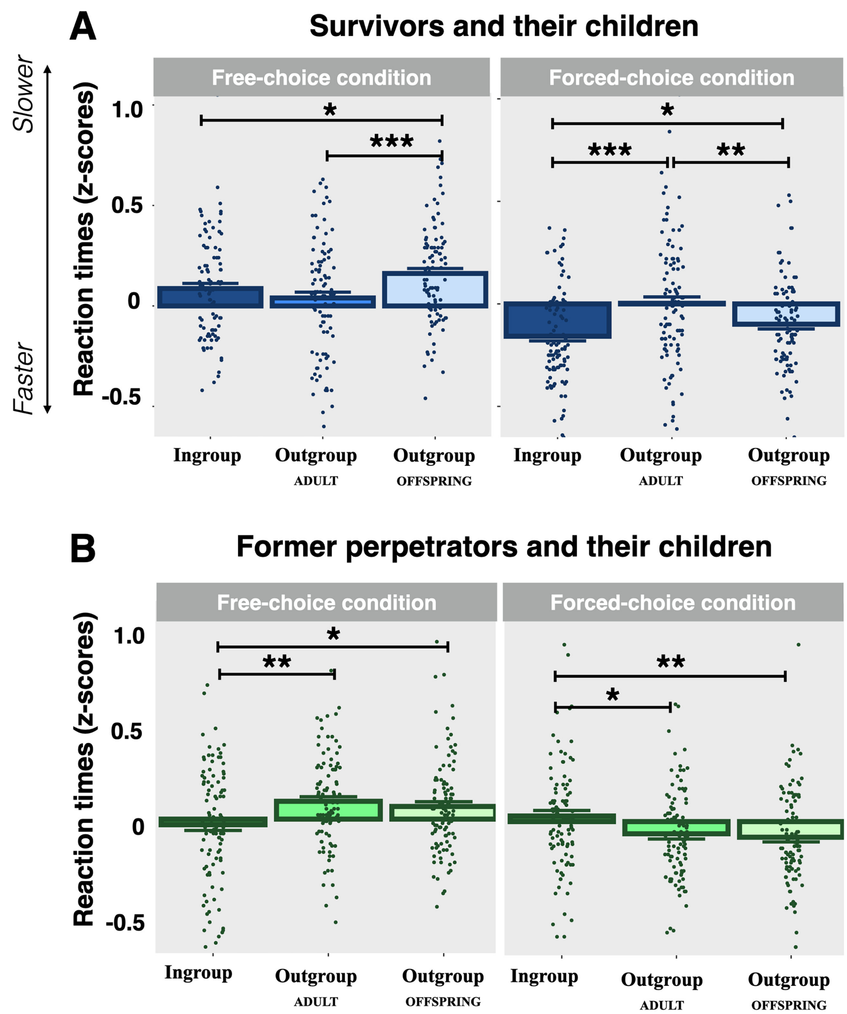
Figure 2

Graphical Representation of the Percentage of Selection of Each Individual in the "Survivors and Their Children" Group (Bluish) and in the "Former Perpetrators and Their Children" Group (Greenish)



Note. All tests were two-tailed. Error bars represent the standard error. *n.s.* = nonsignificant. See the online article for the color version of this figure.
*** $p \leq .001$.

Figure 3
Graphical Representation of the Results on RTs



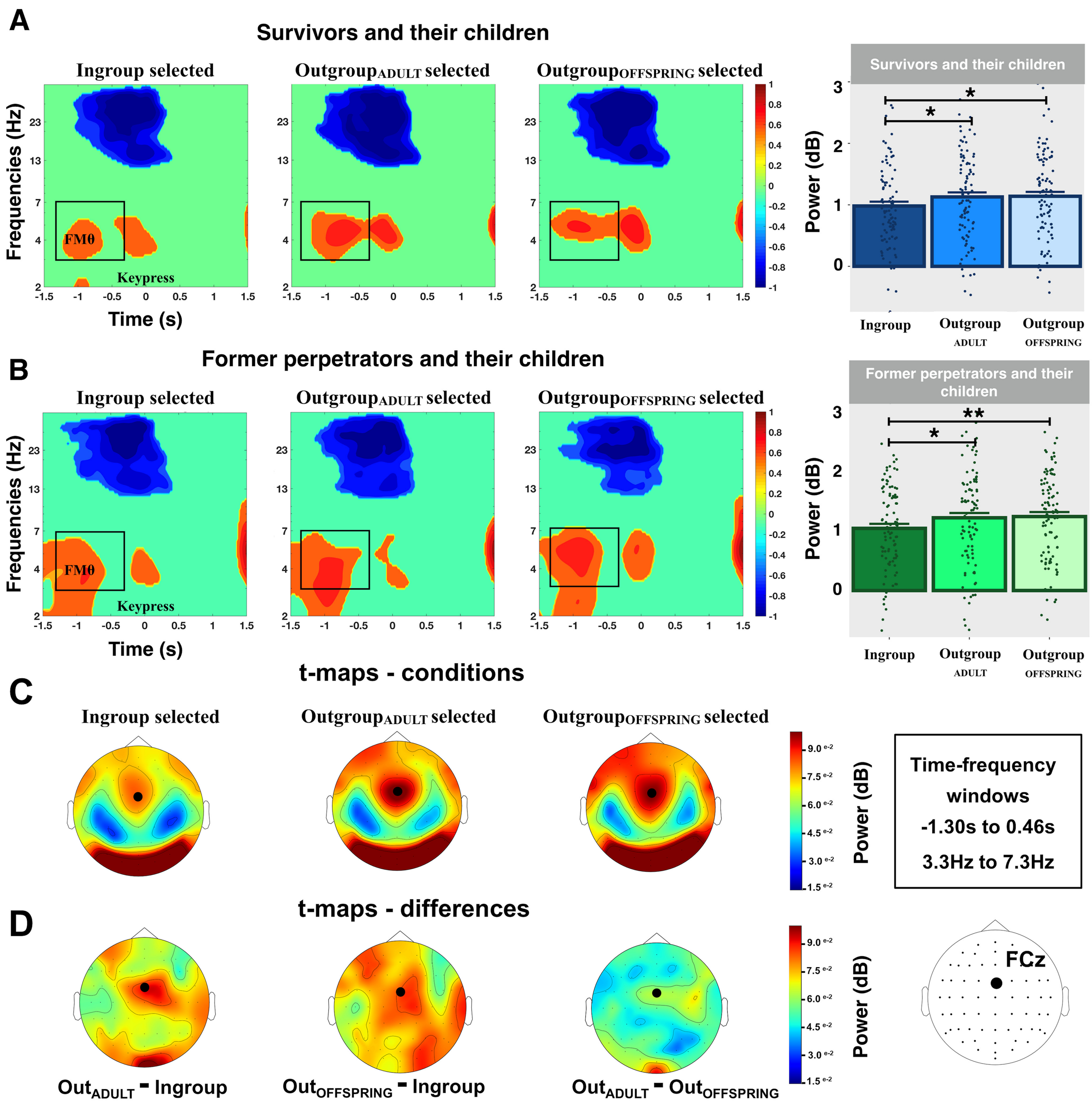
Note. Graphical representation of the RT (z scores) of each individual in the free-choice condition and the forced-choice condition (A) for the "survivors and their children" group and (B) for the "former perpetrators and their children" group. All tests were two-tailed. RT = reaction time. Error bars represent the standard error. See the online article for the color version of this figure.
$^{*}p \geq .01$ and $\leq .05$. $^{**}p \geq .001$ and $\leq .01$. $^{***}p \leq .001$.

$M = 0.060$, $SD = 0.226$, $t(99) = -2.382$, $p = .019$, Cohen's $d = -0.238$. We did not observe difference between the out-group$_{\text{ADULT}}$ individual and the out-group$_{\text{OFFSPRING}}$ individual ($p > .4$). In the forced-choice condition, paired comparisons revealed longer RT when they selected the in-group individual, z-scored $M = 0.019$, $SD = 0.247$, compared to both the out-group$_{\text{ADULT}}$ individual, z-scored $M = -0.063$, $SD = 0.248$; $t(100) = 2.472$, $p = .015$, Cohen's $d = 0.246$, and the out-group$_{\text{OFFSPRING}}$ individual, z-scored $M = -0.070$, $SD = 0.206$; $t(100) = 2.740$, $p = .008$, Cohen's $d = 0.273$. We did not observe difference between the out-group$_{\text{ADULT}}$ individual and the out-group$_{\text{OFFSPRING}}$ individual ($p > .8$). We also observed a significant interaction Individual × Living in a reconciliation village, $F(2, 190) = 4.666$, $p = .011$, $\eta_p^2 = .047$. Independent sample t tests indicated that individuals not living in a reconciliation village were faster to select their own in-group (z-scored $M = -0.031$, $SD = 0.122$) compared to individuals living in a reconciliation village, z-scored $M = 0.037$, $SD = 0.150$; $t(98) = 2.372$, $p = .020$, Cohen's $d = 0.481$. Those not living in a reconciliation village were also faster to select the out-group$_{\text{ADULT}}$ individual (z-scored $M = -0.033$, $SD = 0.115$) compared to those living in a reconciliation village, z-scored $M = 0.041$, $SD = 0.122$; $t(99) = -3.053$, $p = .003$, Cohen's $d = -0.614$. The difference was not significant for the out-group$_{\text{OFFSPRING}}$ individual ($p > .9$). None of the other main effects or interactions were significant (all $ps > .1$).

Midfrontal Theta Activity

In the "survivors and their children" group, we observed a main effect of individual, $F(2, 186) = 3.203$, $p = .043$, $\eta_p^2 = .033$ (see

Figure 4
Graphical Representation of Mid-Frontal Theta Activity



Note. Graphical representation of TFR map with values significantly different from the mean ($p < .05$) for each participant in the "survivors and their children" group (A) and in the "former perpetrators and their children" group (B) are displayed. The x-axis for TFR map was cropped between -1.5 and 1.5 s in order to hide edge artifacts. The black squares represent the time and frequency windows selected to extract the data. On the right, bar plots indicate the power (dB) of the extracted FMθ. Errors bars represent the standard error. All tests were two-tailed. Below the bar plots, the time–frequency windows and the electrode extracted for the FMθ are displayed. (C) Topographic maps averaged on all participants for each condition of the selected time–frequency windows (-1.3 to 0.46 s, 3.3–7.3 Hz). (D) Topographic maps differences averaged on all participants, with out-group$_{\text{ADULT}}$ selected minus in-group selected, out-group$_{\text{OFFSPRING}}$ selected minus in-group selected, and out-group$_{\text{ADULT}}$ minus out-group$_{\text{OFFSPRING}}$ from left to right of the selected time–frequency windows. TFR = time–frequency representation; Hz = Hertz; dB = decibel; FMθ = midfrontal theta; RT = reaction time. All tests were two-tailed. See the online article for the color version of this figure.
$^{*}p \geq .01$ and .001 and $\leq .01$. $^{***}p \leq .001$.

Figure 4A). Paired comparisons indicated that FMθ was lower, indicating a reduced cognitive conflict, when they selected the in-group individual as the recipient of the prosocial action ($M = 0.98$, $SD = 0.69$) compared to when they selected the out-group$_{\mathrm{ADULT}}$ individual, $M = 1.129$, $SD = 0.72$; $t(96) = -2.146$, $p = .034$, Cohen's $d = -0.218$, and when they selected the out-group$_{\mathrm{OFFSPRING}}$ individual, $M = 1.140$, $SD = 0.70$; $t(96) = -2.509$, $p = .014$, Cohen's $d = -0.255$. The comparison between out-group$_{\mathrm{ADULT}}$ and out-group$_{\mathrm{OFFSPRING}}$ was not significant ($p > .8$). Other main effects or interactions were not significant (all $ps > .06$).

In the "former perpetrators and their children" group, we also observed a main effect of individual, $F(2, 192) = 5.014$, $p = .008$, $\eta_p^2 = .050$ (see Figure 4B). Paired comparisons indicated that FMθ was lower, indicating a reduced cognitive conflict, when they selected the in-group individual as the recipient of the prosocial action ($M = 0.99$, $SD = .72$) compared to when they selected the out-group$_{\mathrm{ADULT}}$ individual, $M = 1.15$, $SD = 0.75$; $t(99) = -2.596$, $p = .011$, Cohen's $d = -0.260$, and the out-group$_{\mathrm{OFFSPRING}}$ individual, $M = 1.172$, $SD = 0.71$; $t(99) = -3.026$, $p = .003$, Cohen's $d = -0.303$. The comparison between out-group$_{\mathrm{ADULT}}$ and out-group$_{\mathrm{OFFSPRING}}$ was not significant ($p > .7$). Other main effects or interactions were not significant (all $ps > .079$).

Interestingly for our hypothesis, the interaction Individual × Generation was never significant in any of our variables (i.e., intended prosocial behavior, RT, FMθ, all $ps > .1$). This result showed that children display the same intergroup effects as their parents for both the "survivors and their children" group and the "former perpetrators and their children" group.

Intergenerational Correlations

For the "survivors and their children" group and for the "former perpetrators and their children" group, we observed that the correlation for both the percentage of selections (i.e., intended prosocial behavior; $p > .4$) and for the RT ($p > .1$) was not significant. For the FMθ, we observed small evidence for a positive correlation between the out-group effect of the parents and one of their children ($r = .269$, $p = .089$) for the "survivors and their children" group, but not for the "former perpetrators and their children" group ($p > .1$).

Discussion

In the present study conducted in Rwanda, our objective was to examine the intergroup prosociality bias among former perpetrators and survivors of the genocide, as well as its transmission to the next generation. To accomplish this, we developed a novel task called the intended behavior task, which involved various prosocial scenarios tailored to our specific population. Our approach incorporated behavioral, implicit, and neural measurements to assess the intergroup prosociality bias.

We observed an intergroup prosociality bias among participants from the group consisting of survivors and their children. They exhibited a higher intention for prosocial behavior toward members of their own in-group compared to members of the out-group. This finding aligns with previous research demonstrating in-group preference for prosociality (Hein et al., 2010; Rahal et al., 2020; Reich et al., 2022). It also suggests that intergroup prosociality biases can persist for decades after a genocide, even in a country that emphasizes reconciliation and peace among its citizens (Buckley-zistel, 2006), as shown in a previous study (Mironova & Whitt, 2016). However, it remains unclear whether these results reflect out-group discrimination or in-group favoritism, and a baseline group would have been necessary to address this question (Schiller et al., 2014). Interestingly, the reduced prosociality was more pronounced toward the actual former perpetrator than toward their offspring. These findings may suggest that intergroup prosociality biases are attenuated toward the next-generation individuals. A study has shown that individuals vary in their attribution of guilt to children of former perpetrators (Mukashema & Mullet, 2015), with some considering that children are free from their parents' actions, while others believe that children inherit their parents' wrongdoing. This variation could also explain why the offspring of former perpetrators was selected less frequently than the in-group individuals but more often than the actual former perpetrator.

Critically for our research question, both the actual survivors and their children displayed a similar intergroup prosociality bias, as indicated by the absence of an interaction with the generation factor. This suggests that children may acquire similar biases to their parents.

As anticipated, we observed a reversed intergroup prosociality bias in the group consisting of former perpetrators and their children, with a higher frequency of selecting out-group individuals compared to the in-group. One possible explanation is that former perpetrators may have chosen to favor the group they previously victimized, which aligns with statements made by some participants during post-experimental interviews (Caspar, 2024). This interpretation is supported by a previous study showing that adopting a perpetrator role increases the need to restore moral identity and promotes prosocial behavior toward the conflicting group (Nadler & Shnabel, 2015). Another explanation is that former perpetrators may have decided to penalize their own in-group due to feelings of guilt. Following the genocide, perpetrators in Rwanda faced significant blame for their actions, leading to the experience of guilt (Vollhardt & Bilewicz, 2013). Previous research has shown that guilt can result in self-punishment (Bastian et al., 2011; Nelissen & Zeelenberg, 2009). Although we did not directly investigate feelings of guilt in the present study, a previous study involving prisoners participating in the Gacaca trials, the popular tribunals reintroduced by the Rwandan government in 2002, reported an increased sense of personal guilt for their actions (Kanyangara et al., 2014). Given that our sample consisted entirely of former perpetrators judged during the Gacaca trials, this could support the presence of feelings of guilt among them.

Children of perpetrators in Rwanda also found themselves being blamed for the actions of their parents, albeit to a lesser extent (Mukashema & Mullet, 2015). Consequently, some of them may also experience feelings of guilt (Turner et al., 1988), which could lead to a tendency to penalize their own in-group. Alternatively, these children may feel anger toward their parents' actions and a sense of responsibility to make reparations toward the victims and their children (Brown et al., 2008; Iyer et al., 2003, 2007). Conducting open debriefings in future studies could provide valuable insights into the motivations underlying the observed behaviors.

Overall, we found that RTs were faster in the forced-choice condition compared to the free-choice condition. This aligns with previous studies (Janczyk et al., 2015; Naefgen et al., 2018) and suggests that participants took more time to make decisions when presented with two different individuals. It is well established in the literature that longer RTs indicate higher cognitive conflict (Cohen & Donner, 2013; Greene et al., 2004), and a previous study indicated that participants tend to select the option eliciting lower cognitive conflict (Schouppe et al., 2014). In the group composed of survivors and their children, we observed that when they had the freedom to choose, they selected their own in-group and the former perpetrator faster than the offspring of a former perpetrator. RT and selections were consistent for the in-group and out-group offspring, with shorter RTs associated with a higher selection rate. However, this pattern was not observed for the former perpetrator individual. Although the former perpetrator was selected less frequently than the other individuals, the RTs for the former perpetrator were as short as those for the in-group individual. In the forced-choice condition, both selections and RTs were consistent for all individuals, with shorter RTs associated with more frequent selections in the free-choice condition.

The inconsistence between RT and behaviors in the free-choice condition in both groups could be explained by the fact that interpreting RT only as a marker of cognitive conflict in free-choice conditions may not be reliable (Naefgen et al., 2018; Pierrieau et al., 2022; Wong et al., 2017). It is important to note that different decision strategies can be employed in free-choice scenarios, which can influence RT (Vogel et al., 2018). Thus, caution should be exercised when interpreting RT in free-choice conditions as the sole indicator of decisional preferences. However, since the interaction between condition and individual was not initially predicted in our hypotheses, these post hoc interpretations warrant replication in future studies.

In both groups, we observed that participants exhibited higher FMθ activity when selecting out-group individuals, regardless of their generation, compared to selecting their own in-group. This finding suggests that choosing an out-group individual elicits greater cognitive conflict compared to selecting an in-group individual (Cavanagh & Frank, 2014; Cohen & Cavanagh, 2011; Cohen, 2014; Cohen & Ridderinkhof, 2013; Nigbur et al., 2012). In the group composed of survivors and their children, FMθ activity aligned with their behaviors: they selected out-group individuals less frequently than their own in-group, and selecting out-group individuals induced greater cognitive conflict than selecting their own in-group. In the group of former perpetrators and their children, they more frequently favored out-group individuals over their own in-group, potentially reflecting heightened feelings of guilt. However, the analysis of FMθ activity indicated that choosing out-group individuals still elicited greater conflict compared to choosing their own in-group.

The dissociation observed in the group of former perpetrators and their children, where there was a reversed intergroup prosociality bias for intended prosocial decisions but a classic intergroup bias at the implicit and neural levels, raises intriguing questions about what factors could predict real behavioral change. Is it possible that individuals want to favor their out-group despite experiencing neural conflict? Or is it necessary to have reduced intergroup biases at the implicit and neural levels in order to predict real change? In the literature, it has been suggested that reducing cognitive conflict prior to taking action is a crucial step in changing actual behaviors. Amodio et al. (2008) indeed showed that individuals with lower conflicts when inhibiting to stereotype out-group members have more facility to further change their behaviors toward those out-group members. Another study found that higher cognitive conflict experienced before choosing to engage in immoral behavior toward another individual was associated with a reduction in such behavior (Caspar, Gishoma, & Magalhaes de Saldanha da Gama, 2022). Furthermore, previous studies also showed that a higher cognitive conflict seems to elicit more negative affective reaction (Dignath et al., 2020; Schouppe et al., 2015). It could thus be argued that real changes in behaviors could only be achieved if implicit and neural processes display a similar change. However, future studies are required to confirm this hypothesis.

A wide range of studies has consistently demonstrated that prejudice can be reduced through contact with individuals considered part of an out-group (Beelmann & Heinemann, 2014; Cameron et al., 2011; Crisp & Turner, 2009; Kang et al., 2022). In reconciliation villages, contact not only is promoted, but also the emphasis is on fostering "positive quality" interactions. Specifically, survivors and former perpetrators coexist and participate in joint socio-therapy sessions aimed at achieving reconciliation. However, intriguingly, we observed that the in-group–out-group effects, as reflected in RTs when selecting preferred individuals, appeared to be stronger in the group composed of former genocide perpetrators and their children who lived in a reconciliation village compared to those who did not. In a previous study, we had already observed that living in a reconciliation village had negative effects on prejudice reduction (Caspar, Pech, et al., 2022). A possible interpretation is that former genocide perpetrators and their children living in reconciliation villages are more frequently confronted with the events and consequences of the genocide compared to those not living in such villages. This continuous exposure to the respective stories of the two groups may contribute to the heightened intergroup biases observed at implicit levels on RT, even though they would favor survivors at the behavioral level. However, further research is needed to fully explore and understand this possible interpretation.

In the present study, we observed that children exhibited the same biases as their parents in terms of behavioral, implicit, and neural measurements. In another study conducted with the same sample (Caspar, Pech, et al., 2022), we also found that both children of survivors and children of former perpetrators displayed an intergroup empathy bias similar to that of their parents, as observed in neural measurements. These findings indicate that intergroup biases are transmitted to the next generation, not only in explicit behaviors but also in the implicit and neural mechanisms underlying intergroup biases. This has important implications for understanding the perpetuation of conflicts (Bruneau et al., 2017). However, we did not find significant correlations between the intergroup biases of parents and their children. This lack of direct correlations aligns with findings from two previous studies conducted in Rwanda (Caspar, Pech, et al., 2022; Kang et al., 2022), suggesting that the transmission of intergroup biases is not a direct process and that other factors may influence it.

The main research question of the present study was to understand how the intergroup prosociality bias evolves in the aftermath of a disastrous conflict and how it extends to the following generations. To address this question, we targeted samples that had experienced a massive conflict but with enough time elapsed to include new generations. This study goes beyond convenience samples by involving populations who experienced a genocide, including survivors, former genocide perpetrators, and their respective children. It is important to note that the majority of our participants had limited formal education and had never used a computer or keyboard in their entire lives. Additionally, a significant portion of our sample consisted of older individuals, and some had spent years in jail, which is known to impact cognitive functioning (Meijers et al., 2015, 2018). Therefore, we took measures to minimize the cognitive load and reduce the duration of the study. As a result, participants were presented with three categories of individuals: their own in-group, an out-group adult, and the offspring of an out-group. This limited scope means that we do not have information on other potential effects, such as whether former perpetrators have different prosocial intentions toward the offspring of former perpetrators compared to other groups. Future studies should consider expanding on these findings by exploring additional categories of individuals.

In conclusion, the present study showed that intergroup prosocial biases can still be present even decades after a genocide and that children exhibit similar biases to their parents. However, in a possible seek for repair, some intergroup prosocial biases may be reversed, even though not at the implicit and at the neural levels. Encouragingly, among the group of survivors and their children, we found a reduced intergroup prosocial bias toward the children of perpetrators compared to former perpetrators. This study opens new paths to understand better the transmission of intergroup biases in war- or genocide-torn societies and may be important for understanding better the mechanisms behind those biases.

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