




Recognizing humanity: dehumanization predicts neural mirroring and empathic accuracy in face-to-face interactions

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Abstract

Dehumanization is the failure to recognize the cognitive and emotional complexities of the people around us. While its presence has been well documented in horrific acts of violence, it is also theorized to play a role in everyday life. We measured its presence and effects in face-to-face dyadic interactions between strangers and found that not only was there variance in the extent to which they perceived one another as human, but this variance predicted neural processing and behavior. Specifically, participants showed stronger neural mirroring, indexed by electroencephalography (EEG) mu-suppression, in response to partners they evaluated as more human, suggesting their brains neurally simulated those targets' actions more. Participants were also marginally more empathically accurate about the emotions of partners deemed more human and performed better with them on a cooperative task. These results suggest that there are indeed differences in our recognition of the humanity of people we meet—demonstrated for the first time in a real, face-to-face interaction—and that this mundane variation affects our ability to neurally simulate, cooperate and empathize.

Key words: dehumanization; motor resonance; empathic accuracy; dyadic interaction; cooperation; mirroring

While it is widely acknowledged that viewing one another as human beings is contextually contingent (e.g. Harris, 2017), very little research has measured this phenomenon in the interpersonal interactions that populate our lives. We therefore investigated whether dehumanization naturally occurs in normal interactions and whether variation in our recognition of others' humanity has any of the deleterious consequences associated with dehumanization, including detriments to empathy, cooperation and neural processing. The present work is the first to test dehumanization in mundane, face-to-face interactions and only the third to measure dehumanization toward a physically present target (Bastian et al., 2012; Gwinn et al., 2013). While

it is believed that dehumanization occurs via passive processes in mundane situations (e.g. Waytz and Schroeder, 2014; Waytz et al., 2014), to our knowledge, measurements of dehumanization in interactions have only come after it has been manipulated.

Despite its association with extraordinary intergroup violence (e.g. Over, 2019), dehumanization is theorized to pervade everyday life. Whether expressed in passing biases of social attention (Waytz et al., 2014), sparse emotional attribution (Paladino et al., 2002) or piecemeal visual encoding (Fincher and Tetlock, 2016), dehumanization's harmful effects abound. However, major intergroup theories treat dehumanization as a rare

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case, overlooking the possibility that humanity is often differentially assigned on the basis of minor social signals (Harris, 2017). While research has focused on the dehumanization of entire social groups, dehumanization and its consequences occur interpersonally (Bastian et al., 2014). Because we can never know other people's minds the way we know our own, it is all too easy to assume that others do not experience things as vividly as we do, control their actions as we control our own or objectively experience reality to the same extent that we do (Waytz et al., 2014). The present research tests whether natural variation in our perceptions of others' humanity changes the way we empathize, cooperate, and neurally process them.

Indeed, research enumerates many negative effects of dehumanization. We struggle to take the perspective of (Vaes et al., 2004), empathize with (Cehajic et al., 2009) or help (Cuddy et al., 2007; Andrighetto et al., 2014) people we do not recognize as fully human. Moreover, research on neural processing of dehumanized targets shows distinct patterns of activity in areas related to inferring others' mental states (the medial prefrontal cortex and the default mode network more broadly; Harris and Fiske, 2006, 2009, 2011; Cikara et al., 2011; Jack et al., 2013; Harris et al., 2014) and areas that may inhibit such inferences (the task positive network; Jack et al., 2013; Bruneau et al., 2018a). These results not only indicate that dehumanization is distinct from prejudice (Harris, 2017; Bruneau et al., 2018a) but accord with our understanding of dehumanization's effect on behavior. If empathetic behaviors respond as a function of perceiving others' minds (Andrighetto et al., 2014; Bruneau et al., 2015, 2018b), it is consistent for regions associated with understanding others' mental states and behaviors to be similarly affected. We seek to add to the small literature showing neural indexing of denials of cognitive or emotional capacity (Harris and Fiske, 2009; Jack et al., 2013; Bruneau et al., 2018a; Vaes et al., 2019) by adding a measure of mirroring, further demonstrating dehumanization's effects.

While mentalizing involves inferring intentions behind others' actions, mirroring—neurally representing others' movements, somatosensory experiences and emotion expressions in shared neural networks—is the complementary process of understanding those actions, experiences and expressions (Iacoboni et al., 2005; Van Overwalle and Baetens, 2009; Becchio et al., 2012). Mirroring therefore informs mentalizing (Aragón et al., 2014). Whether we perceive someone as human—which is, in part, the ability to have intentions (Bastian et al., 2011)—should affect the extent to which we mirror them because identifying a target's actions is closely linked to perceiving them as having a mind (Kozak et al., 2006). The mentalizing and mirroring regions of the brain comprise distinct neural networks which can be isolated in highly controlled experiments (Decety and Jackson, 2004; Shamay-Tsoory, 2011), but which work together for more complex empathic functions (Keysers and Gazzola, 2007; Lombardo et al., 2010; Zaki and Ochsner, 2013; Kanske et al., 2015). As outlined above, dehumanization is associated with less mentalizing but, to our knowledge, its effects on mirroring have yet to be tested.

To index mirroring, we measure EEG mu-suppression, the reciprocal suppression of EEG mu-power picked up at central electrodes during the observation of actions, somatosensory experiences and facial expressions (Fox et al., 2016). Cortical source estimates show that mu-suppression occurs primarily around the central sulcus (Hari and Kujala, 2009), confirming its somatosensory origin, and it correlates with activation of the mirroring network (Perry and Bentin, 2009; Arnstein et al.,

2011; Mizuhara, 2012; Braadbaart et al., 2013). Mu-suppression also correlates with empathic responses (DiGirolamo et al., 2019), action prediction (Woodruff et al., 2011) and emotion identification (Pineda and Hecht, 2009); all of which might be affected by dehumanization. It also responds to manipulations consistent with moderators of dehumanization, like power imbalances (Gwinn et al., 2013; Hogeveen et al., 2014), group differences (Leyens et al., 2000; Gutsell and Inzlicht, 2010), and social relevance (Perry et al., 2010; Aragón et al., 2014; Tropp and Barlow, 2018). While mu-suppression's reaction to factors similar to those that might determine dehumanization does not necessarily mean that it will distinguish humanity, it does suggest that both could reflect a similar social calculation.

The present work tests our preregistered hypotheses (https://osf.io/awd4g/?view_only=aa5eed96021e424eaff0c649d694841a) that un-manipulated dehumanization of a physically present target will correlate with diminished empathy, cooperation and neural mirroring (see Figure 1). We measured these relationships in interpersonal interactions between people wearing EEG headsets. By examining important questions about social cognition in actual, face-to-face meetings between strangers, we improve our understanding of these processes outside of the lab. This ecological validity extends not only to the behavioral measures, but also to a rare in-person test of mirroring indexed by mu-suppression.

Methods

Participants

In total, 72 participants in 36 dyads completed the study for monetary compensation ($M_{Age} = 38.74$, $s.d. = 17.80$). These were split into 12 cross-race and 24 same-race dyads using self-identified race, where cross-race dyads were always Black-White and same-race dyads were always White-White. Participants were recruited through Craigslist and flyers around the university campus and surrounding community, and dyads were age-matched within 10 years when possible. The Institutional Review Board approved the study.

Procedure

After screening, participants were sent a Qualtrics survey that included demographic and scheduling questions. In the lab, participants were kept separate from their partners, fitted for EEG and completed several questionnaires and tasks before the interaction, including a joystick balancing activity akin to the one they then performed during the interaction. Once together, participants took turns watching each other squeeze a stress-ball to provide a measure of mu-suppression (e.g. Hogeveen and Obhi, 2011), before going through a structured ice-breaking task in which they asked each other questions provided on index cards. Next came either the action coordination task, which indexed cooperation, or the emotional story-sharing component of the empathic accuracy task (in random order), before another set of stress-ball squeezing which ended the interaction (see Figure 1). Participants then completed the rest of the measures separately, including rating the videos of the emotional stories as a measure of empathic accuracy and completing the dehumanization measure. They were then compensated, debriefed and dismissed. A number of the questionnaires and tasks are irrelevant to the present work and will be reported elsewhere; see Supplementary Materials for the full list.

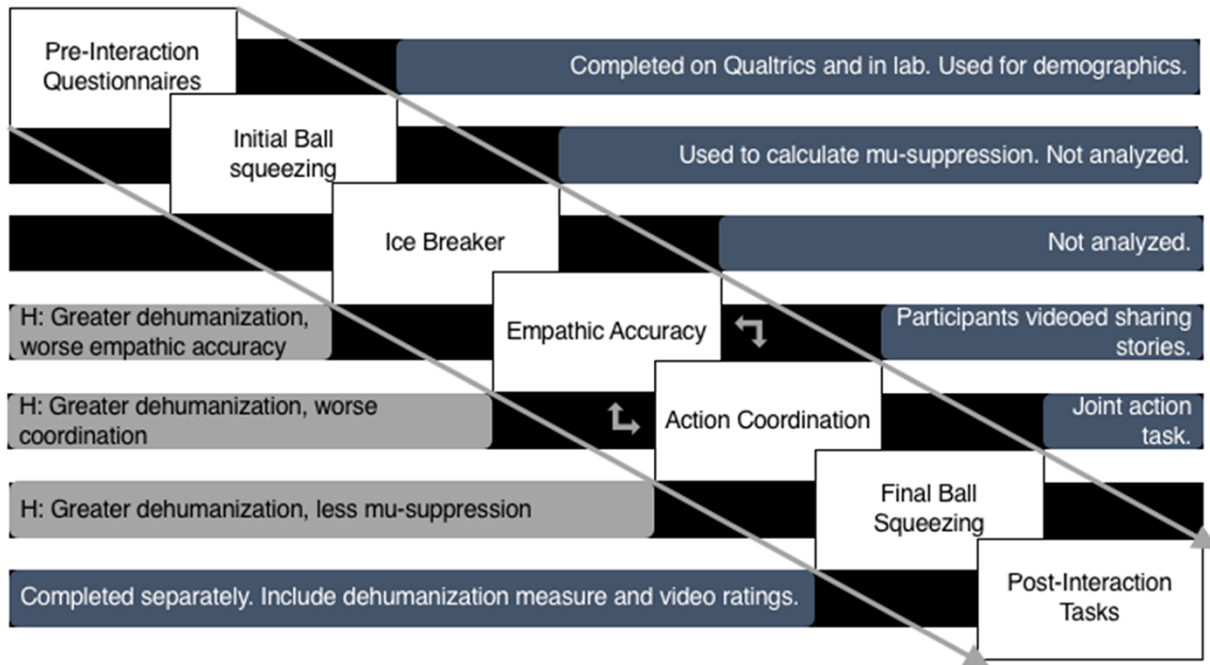


Fig. 1. Study procedure and hypotheses. Explanations of procedure in blue and of hypotheses (H) in gray. Participants were in the same room for all five steps of the interaction. Double-headed arrows indicate random order between dyads.

EEG recording and processing. Participants wore EEG recording equipment throughout the study. EEG was recorded from 31 active electrodes embedded in a stretch-lycra cap (ActiCap, BrainProducts GmbH, Munich, Germany) arranged according to the 10–20 system with impedances kept below 10 k Ω . The EEG was digitized at 250 Hz using BrainAmp amplifiers and the BrainVision recorder software (BrainProducts GmbH, Munich, Germany) with an initial reference at FCz, later re-referenced offline to an average reference with EEGLAB (Delorme and Makeig, 2004). During the interaction, each subject was connected through an individual electrode input box to their own amplifier, allowing for individual reference and ground electrodes. The amplifiers were connected to the same USB 2 Adapter (BUA; BrainProducts, GmbH, Munich Germany) for synchronization between amplifiers. EEG recordings were sectioned into non-overlapping activity-locked segments before further processing, which proceeded with custom 2018b MATLAB (MathWorks) scripts implementing EEGLAB functions. Unusable channels were identified by visual inspection of raw data, excluded and replaced with interpolated channels using the ‘spherical’ method in the EEGLAB function `pop_interp()` (Delorme and Makeig, 2004). Data were filtered with a bandpass between 1 and 30 Hz and had line noise removed using `pop_cleanline()`. Independent components were identified with the second-order blind identification algorithm—which has been shown to be an effective way to extract the mu component (Ng and Raveendran, 2009)—then categorized using `ICLabel()`, an algorithm trained to classify components in EEG data (Pion-Tonachini et al., 2019). Those given a greater than 50% chance of being ocular, muscular or ‘other’ artifacts were removed. Similar segments under 120 s (for example, all four times participants squeezed or watched a stress-ball being squeezed during the interaction) were temporarily combined in order to run this independent component analysis, in keeping with Onton et al., (2006) recommendation of 20 timepoints per number of channels squared. Further artifacts

were removed with the `pop_eegthresh()` function, which took out 1 s epochs containing a change in amplitude greater than 100 μ V.

Measures

Human nature and human uniqueness. After the interaction, participants rated their interlocutors on 40 traits associated with human nature (HN) or human uniqueness (UH; Haslam and Bain, 2007). HN indexes mechanistic dehumanization—denial of emotional capacities—and UH indexes animalistic dehumanization—denial of cognitive capacities. Subjects were prompted, ‘For the following questions, select the response best fitting how you think of your partner.’ from 1, ‘not at all,’ to 7, ‘to a great extent’ (Gwinn et al., 2013). Each of the 40 items in the scale is orthogonally rated as either high or low in both HN and UH, plus a third dimension, desirability, which can be thought of as valence. Our preregistered analysis—based on Haslam and Bain’s (2007)—was to compute difference scores between items high in HN ($\alpha = 0.63$) and items low in HN ($\alpha = 0.44$) and do the same for UH ($\alpha_{\text{High}} = 0.63$, $\alpha_{\text{Low}} = 0.21$), and then, combine them into a single index of dehumanization if they were correlated above $r = 0.60$ (Bastian et al., 2012). Because of the poor reliabilities, we had to modify this approach (see Supplementary Materials for results with the preregistered difference scores).

Instead, we fit factor scores to improve our measurement. Our sample size was not large enough to fully model the triply orthogonal structure of the scale, so we instead fit six separate factor scores: humanity type (high HN/low UH; low HN/high UH; or low HN/low UH) \times desirability (desirable or undesirable).¹

¹ The analysis therefore omitted traits that were high in both HN and UH because we did not have the sample size to deal with that sort of crossed factor structure.

We were then able to subtract the low humanity factor scores from both HN and UH (subtracting desirable from desirable and undesirable from undesirable), mimicking the preregistered difference scores. The logic of this approach is that it allows us to measure attribution of traits connoting cognitive or emotional complexity relative to attribution of traits that connote neither. See the Supplementary Materials for the full list of traits, fit indices and figures representing all six models.

Dehumanization factor difference scores (hereafter referred to as ‘factor scores’) for both desirable, $r(68)=0.47$, 95% CI[0.26, 0.64], $P<0.001$, and undesirable traits, $r(68)=0.83$, 95% CI[0.74, 0.89], $P<0.001$, were significantly correlated across UH and HN. However, only the undesirable correlation exceeded our preregistered threshold of $r=0.60$, so we averaged HN and UH undesirable factor scores to create a single factor of undesirable dehumanization. Desirable traits were kept as separate factors representing animalistic (UH) and mechanistic (HN) dehumanization.

The downside of this approach was that it created three metrics of dehumanization: desirable HN (positively valenced traits that distinguish humans from machines), desirable UH (positively valenced traits that distinguish humans from animals) and undesirable dehumanization (negatively valenced traits that distinguish humans from non-human agents). As a result, desirability is confounded with dehumanization so that effects of dehumanization might be related to valence. Using two types of dehumanization allows us to control this issue somewhat, since differing results for the two desirable forms of humanity would suggest that valence is not the cause. The upside of this approach is far more reliable measurement of the constructs that interest us, even if they are valenced.

Mu-suppression. To measure mu-suppression, we had participants take turns squeezing a stress ball for 30 s with their right hands. This was the final task during their interaction and should therefore give a sense of how much they mirrored after getting to know each other. As a comparative baseline of mirroring to nonsocial movement, participants watched videos of stress balls rolling across a table (Perry et al., 2010) before the interaction.

As preregistered, we applied a surface Laplacian spatial filter (Perrin et al., 1989) to isolate cortical signals and averaged power in the 8–13 Hz frequency range. Power was extracted with a complex Gaussian Morlet wavelet decomposition with a width determined by Morlet parameters ranging between five and ten cycles per wavelet in steps of 1 Hz. Both ball-squeezing and baseline mu-suppression were log transformed, and a ratio was taken by subtracting baseline from ball squeezing such that more negative values indicate more suppression relative to baseline, and therefore more mirroring.

We then tested whether this activity was localized to central electrodes, as it is important to differentiate mu-suppression from alpha-suppression (Bowman et al., 2017; Hobson and Bishop, 2017; DiGirolamo et al., 2019). To do so, we ran a preregistered 2 (posteriority: central or occipital) \times 2 (laterality: left or right) repeated measures ANOVA comparing suppression at central (C3 (left) and C4 (right)) and occipital electrodes (O1 (left) and O2 (right)).

While we did not see a significant effect of posteriority, $F(1, 59)=1.02$, $P=0.32$, and $\eta^2_{general}=0.0054$, there was a significant effect of lateralization, $F(1, 59)=5.00$, $P=0.029$, and $\eta^2_{general}=0.011$, qualified by an interaction between posteriority and lateralization, $F(1, 59)=4.32$, $P=0.042$, and $\eta^2_{general}=0.0053$. Preregistered planned comparisons, performed with the emmeans package version 1.4.5 (Lenth, 2020) for

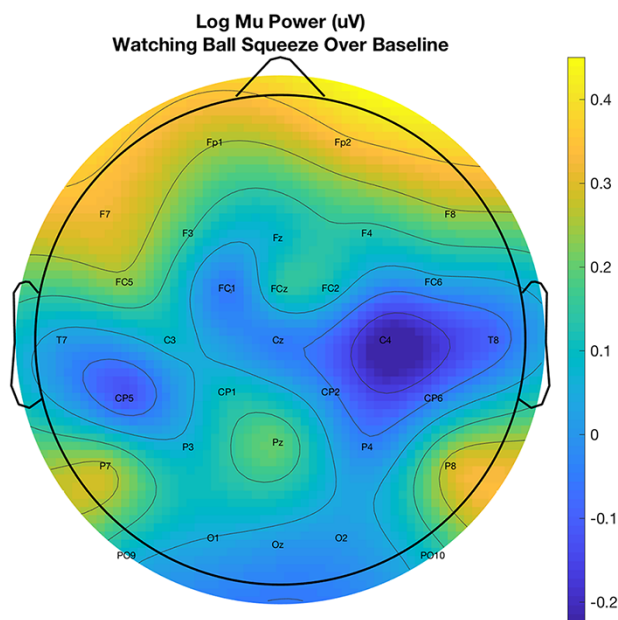


Fig. 2. Topographic distribution of Mu-Suppression. Mu-suppression was calculated as the log ratio of power while watching partner squeeze a stress ball over baseline (watching a ball roll with no biological movement). Lower scores indicate more suppression (more mirroring). Note localization over C4.

R version 3.5.1 (R Core Team, 2018), found that greatest suppression could be found at electrode C4, on the right central part of the scalp (see Figure 2). Simple contrasts using the Benjamini–Hochberg false discovery rate corrections for multiple comparisons (Benjamini and Hochberg, 1995) showed significantly more central suppression on the right versus left side, $t(109.88)=-3.04$, $P=0.003$, $d=0.29$, and marginally more suppression centrally than occipitally for right electrodes, $t(84.94)=-1.81$, $P=0.074$, $d=0.20$. As a result, we used C4 mu-suppression as our measure of neural mirroring.

Empathic accuracy. Because dehumanization is thought to diminish empathy (e.g. Cehajic et al., 2009), we measured participants’ ability to decipher each other’s emotions in a dynamic, realistic context (Mackes et al., 2018). During the interaction, we instructed participants to think of a ‘most positive’ and ‘most negative’ experience that they were comfortable sharing with their interlocutor and—after they first told a neutral story to acclimate to the 2 min time limit—we videotaped them telling each other these positive and negative stories. After the interaction, we showed all four of the videos to both participants so that they could continuously rate how the person telling the story—themselves or their interlocutor—was feeling while telling the story, on a scale from 1, ‘very negative’, to 9, ‘very positive’ (Zaki et al., 2008). We calculated time-course correlations of participants’ ratings as a measure of empathic accuracy; the more similar a participant’s rating of their interlocutor’s feelings to the interlocutor’s ratings of their own feelings, the more empathically accurate the participant was and, arguably, the more successfully they had empathized (Mayukha et al., 2020). Correlations were Fischer’s Z transformed for analysis (Zaki et al., 2008). Accuracies for positive and negative videos were not correlated, $r(47)=0.24$, 95% CI[-0.040, 0.49], $P=0.091$, $BF_{10}=1.00$, so they are analyzed separately.

Action coordination. To test the effect of dehumanization on cooperation, participants worked together on an action coordination task in which they had to balance a digital pendulum using joysticks (see Supplementary Figure S8). Participants sat next to each other and were instructed to neither look at one another nor speak. The pendulum was calibrated so that it would never balance, and participants had to continually work to keep it from falling. The pendulum fell according to the equation $\phi = k_p \sin \phi$, where ϕ is the pendulum's angular deviation from zero degrees and k is the pendulum constant or gravitational level, set to 6.5 m/s. Each of the joysticks had an equal ability to push the pendulum (0.05 rad/s), and if the participants did not coordinate their actions it would fall. When the pendulum fell, it immediately reset near the zero-degree mark (randomly between -9 and 9° with a random starting velocity between zero and 0.5 rad/s). Theoretically, the better that participants represented one another's movements during this task, the better they would perform. To allow us to control for individual ability levels on this task, participants completed a solitary version of the activity during the initial set of questionnaires. We had two metrics of success for the task: the number of times the pendulum 'crashed' (hit zero degrees and reset) and the variance of its position. As preregistered, we standardized both measures and averaged them to provide a single index. Both the individual and joint tasks were 4 min long. Participants averaged 143.86 (s.d. = 46.03) crashes on their own and 123.79 (s.d. = 76.44) crashes as a pair.

Deviations from the preregistration. Our preregistered sample size was 60 dyads in order to additionally examine cross-race versus same-race effects between dyads, but we were forced to stop data collection due to a moratorium on in-person research during the global pandemic in 2020. Simulated sensitivity analysis (Lane and Hennes, 2018) shows that our current sample provides 80% power for an effect of $\beta = 0.38$ for the hypothesized relationships between individual-level variables within dyads, meaning we are adequately powered for our preregistered analyses.² We have less power for relationships between dyad-level variables—80% power for a correlation of $r = 0.45$, according to sensitivity analysis in G*Power 3 (Faul et al., 2007)—so our preregistered neural synchrony results are reported in the Supplementary Materials. Further sensitivity analysis shows that we would have 80% power for an effect of $d = 1.02$ to test our predictions about the effect of participant race, so we hope to address these hypotheses in future work with adequate sample size.

In addition, as outlined above, we computed factor scores in place of our preregistered dehumanization indices due to the unforeseeably low reliabilities of the subscales. By subtracting low humanity factor scores from high HN and UH factor scores, we remained faithful to the preregistered difference scores, ensuring that we measured attribution of human traits relative to non-human traits. Unfortunately, we were limited by our sample size to using separate desirable and undesirable factors, raising the potential for valence to play a role in our results and requiring additional statistical tests. Therefore, to manage the Type I error rate, we correct for multiple comparisons within each set of dehumanization analyses (Benjamini and Hochberg, 1995). We also report Bayes factors, computed with the R package BayesFactor version 0.9.12–4.2 (Morey and Rouder, 2018), as

further quantification of evidence for and against our hypotheses. Except where otherwise indicated, the analyses tested in this paper are preregistered but used a more reliable index of dehumanization.

Results

To deal with the nested nature of the data, many hypotheses were addressed within multilevel models fit with the R package lme4 version 1.1–21 (Bates et al., 2015). For 'micro-macro' models—where the outcome variable is at the level of the dyad—we used adjusted group means (best linear unbiased predictors; BLUPs) for the individual predictors (Croon and Van Veldhoven, 2007). This avoids the biased estimates produced by simply averaging individual values for each dyad. When there was only one predictor in the model, we computed preregistered correlations using these adjusted means. Table 1 contains the zero-order correlations between individual-level variables. There were no gender ($ps > 0.16$) or race ($ps > 0.080^3$) differences on any of these variables.

To test whether dehumanization would predict diminished mu-suppression, we fit preregistered hierarchical linear models with each of the three dehumanization scores predicting mu-suppression at C4. Correcting for multiple comparisons, desirable UH ratings predicted mu-suppression, $\beta = -0.42$, $P = 0.013$, $f^2 = 0.16^4$ and $BF_{10} = 9.03$,⁵ with the negative coefficient indicating that the more uniquely human (less animal-like) participants perceived their partners to be, the more they mirrored them, as we predicted (see Figure 3). Desirable HN ratings neither did predict mu-suppression, $\beta = -0.064$, $P = 0.69$, $f^2 = 0.0028$ and $BF_{10} = 0.30$, nor did predict undesirable dehumanization ratings, $\beta = 0.14$, $P = 0.28$, $f^2 = 0.032$ and $BF_{10} = 0.55$.

To test whether dehumanization predicted reduced empathic accuracy, we fit six preregistered hierarchical linear models with either positive or negative empathic accuracy as the outcome and either desirable HN scores, desirable UH scores or undesirable dehumanization scores as the predictor. Correcting for multiple comparisons, while desirable UH did not predict positive empathic accuracy, $\beta = -0.025$, $P = 1.00$, $f^2 = 0.00059$ and $BF_{10} = 0.32$, it marginally predicted negative empathic accuracy, $\beta = 0.27$, $P = 0.069$, $f^2 = 0.097$ and $BF_{10} = 4.25$, with the Bayes factor suggesting moderate evidence against the null. The more the partner was perceived to have desirable uniquely human traits (to be less animalistic in a positively valenced way), the more empathically accurate the perceiver was for their negative story (see Figure 4). Desirable HN ratings did not predict positive, $\beta = -0.00079$, $P = 1.00$, $f^2 = < 0.001$ and $BF_{10} = 0.30$, or negative empathic accuracy, $\beta = 0.091$, $P = 1.00$, $f^2 = 0.0051$ and $BF_{10} = 0.37$, and undesirable dehumanization ratings also did not predict positive, $\beta = -0.031$, $P = 1.00$, $f^2 = 0.0015$ and $BF_{10} = 0.28$, or negative empathic accuracy, $\beta = -0.072$, $P = 1.00$, $f^2 = 0.0072$ and $BF_{10} = 0.036$.

To test whether dehumanization predicted impaired action coordination, we correlated dyad-level means for our dehumanization factors with dyad-level averages of standardized crashes and joystick variance. The factor scores for desirable UH and

³ $BF_{10} = 1.27$ for the marginal difference on desirable human uniqueness.

⁴ f^2 values reported for coefficients in hierarchical linear models are calculated in accordance with Lorah (2018), using marginal R^2 's (Nakagawa et al., 2017) calculated with MuMin package version 1.43.15 (Bartoń, 2019) in R. All β s are standardized.

⁵ We consider values under 0.33 to be moderate evidence for the null and values over 3 to be moderate evidence against (Keysers et al., 2020).

² Note, too, that we exceed Fox et al.'s (2016) meta-analytic recommendation of 66 participants for mu-suppression studies.

Table 1. Correlations between individual-level variables

	Human uniqueness	Human nature	Negative dehumanization	Mu-suppression	Empathic accuracy positive	Empathic accuracy negative
Human uniqueness						
Human nature	0.47***					
Negative dehumanization	-0.05	-0.02				
Mu-suppression	0.037**	-0.05	0.18			
Empathic accuracy positive	0.01	0.00	-0.06	-0.01		
Empathic accuracy negative	0.29*	0.00	-0.06	-0.13	0.21	
Action coordination	-0.20	-0.13	0.00	0.12	0.03	-0.35**

*** $P < 0.001$; ** $P < 0.01$; * $P < 0.05$.

Notes: UH and HN are positive factor scores with greater values indicating more humanity, negative dehumanization is a factor score with greater values indicating more humanity, Mu-suppression is scored such that lower values indicate greater mirroring, empathic accuracies (for positive and negative videos) are scored such that higher values indicate a better accuracy and action coordination is scored such that higher scores indicate more errors.

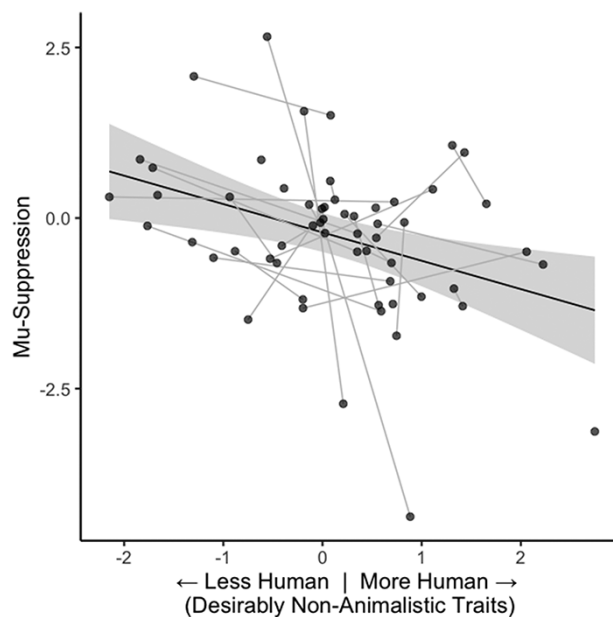


Fig. 3. Mu-suppression by desirable human uniqueness. Mu-suppression was measured as the log ratio of power (μV) at observation vs baseline so that more negative numbers indicate more mirroring. Gray lines represent slopes within dyads, black line is overall slope from the model fit with the standard error of the mean shaded around it. Note that the model allows intercepts but not slopes to vary by dyad and that data are missing for some participants (58 participants are included in the analysis).

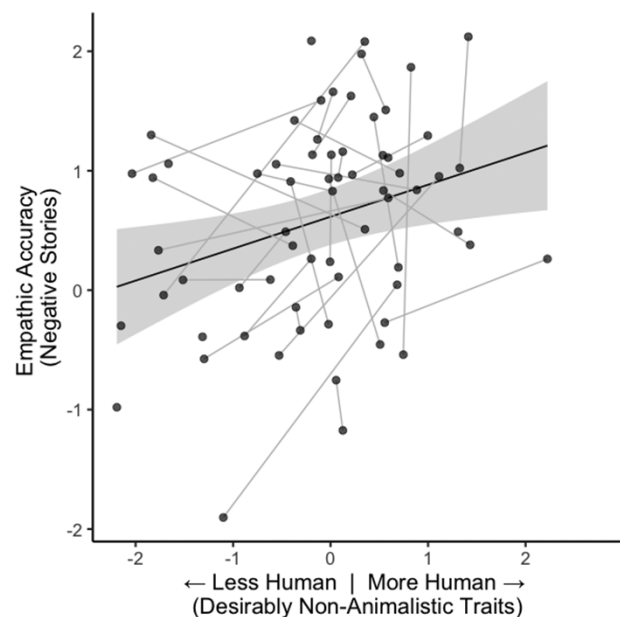


Fig. 4. Negative empathic accuracy by desirable human uniqueness. Gray lines represent slopes within dyads, black line is overall slope from the model fit with standard error of the mean shaded around it. Note that the model allows intercepts but not slopes to vary by dyad and that data are missing for some participants (68 participants are included in the analysis).

undesirable dehumanization produced singular fits when creating BLUPs, so we simply calculated averages for each dyad. Success on the action coordination task did not correlate with desirable HN scores, $r(31) = -0.17$, 95% CI[-0.48, 0.19], $P = 0.36$ and $BF_{10} = 0.55$, desirable UH scores, $r(29) = -0.30$, 95% CI[-0.59, 0.061], $P = 0.10$ and $BF_{10} = 1.23$, or undesirable dehumanization scores, $r(29) = 0.0026$, 95% CI[-0.35, 0.36], $P = 0.99$ and $BF_{10} = 0.39$.

However, when controlling for participants' performance on the individual version of the task (aggregated to the dyadic level as BLUPs) and correcting for multiple comparisons, the relationship between desirable HN and action coordination was significant, $\beta = -0.28$, $P = 0.012$, $f^2 = 0.29$ and $BF_{10} = 7.73$, and

the relationship between desirable UH and action coordination was marginal, $\beta = -0.21$, $P = 0.066$ and $f^2 = 0.20$, $BF_{10} = 1.06$, though the Bayes factors suggest that only HN is predictive. Dyads who perceived each other as more human coordinated better such that the pendulum crashed fewer times and had less positional variance. While this was an exploratory analysis, it shows that once individual differences in task-specific skills were accounted for, the cooperative aspect of the task was influenced by how human partners perceived each other to be. This was somewhat true of both positively valenced animalistic and mechanistic traits, suggesting that valence could be responsible for the effect. Negatively valenced dehumanization did not predict action coordination even when controlling for individual performance, $\beta = -0.14$, $P = 0.18$, $f^2 = 0.11$ and $BF_{10} = 0.37$.

In addition, we exploratorily tested whether mu-suppression predicted our behavioral outcome measures. It did not predict either positive, $\beta = 0.012$, $P = 0.92$, $f^2 = 0.00014$ and $BF_{10} = 0.30$, or negative empathic accuracy, $\beta = -0.12$, $P = 0.37$, $f^2 = 0.014$ and $BF_{10} = 0.36$. It also did not predict action coordination, $\beta = 0.21$, $P = 0.28$, $f^2 = 0.053$ and $BF_{10} = 0.58$, but the relationship was marginal when controlling for individual performance, $\beta = 0.24$, $P = 0.077$, $f^2 = 0.24$ and $BF_{10} = 1.02$, such that, directionally, dyads who mirrored each other more coordinated better, but the Bayes factor suggests that there is insufficient evidence for this conclusion.

Discussion

Naturalistic variance in recognition of one another's humanity predicted neural mirroring, empathic accuracy and action coordination between strangers in a real, face-to-face interaction. Participants who perceived their interlocutors as having less cognitive capacity resonated with them less and were marginally less accurate about their emotions when they recounted a negative story. Participants who perceived their interlocutors as having less cognitive capacity or less emotional capacity also performed worse on a cooperative task when controlling for individual skill level. These measures were chosen because of their connections to empathy and cooperation, important interpersonal processes thought to be reduced by dehumanization (Cehajic et al., 2009; Kteily and Bruneau, 2017). Here, we show those reductions, even though there was no induction of dehumanization. Everyday variance in the extent to which we perceive others as human not only exists but predicts neural and behavioral outcomes of our interactions.

This work adds to increasing evidence that many contexts produce dehumanization, including workplaces (Belmi and Schroeder, 2020), incarceration (Deska et al., 2020) and clinical psychiatric settings (Fontesse et al., 2019). Moreover, physical appearance has been linked to dehumanization through facial width (Deska et al., 2018), obesity (Kersbergen and Robinson, 2019) and even use of cosmetics (Bernard et al., 2020). It therefore appears that the cognitive processing involved in recognizing others' humanity is flexibly and unevenly applied (Harris, 2017). We add to this literature by showing that even in a fairly mundane context (meeting and interacting with a stranger) there is not only variance in how human we perceive one another to be, but this variance has downstream effects on empathy and its neural correlates. Importantly, partner ratings, behavioral indices and neural measures occurred at different points in the study, suggesting these effects came from holistic judgments rather than momentary impressions.

Our neural measure was mu-suppression, an index of mirroring. Specifically, perceiving one's partner as more dissimilar from animals (having more cognitive capacity) was associated with more mirroring. Mu-suppression is known to index group biases (Avenanti et al., 2010; Gutsell and Inzlicht, 2010) and responds differentially to perceptions of a target's traits (Simon et al., 2020). This is the first evidence that perceptions of humanity predict mirroring, which is intriguing given the relationship both have to empathy and pain perception (Cuddy et al., 2007; Perry et al., 2010; Hoenen et al., 2015; Nagar and Maoz, 2017). This suggests that neural simulation of people we interact with may depend on the extent to which we perceive them as human, perhaps supporting the functional account of mirroring that we mirror motivationally relevant targets more (Gutsell and Inzlicht, 2013). It remains to be seen whether mirroring confers humanity or humanity confers mirroring.

Dehumanization also marginally predicted empathic accuracy, with the Bayes factor indicating moderate evidence against the null. Specifically, dissimilarity to animals was again the predictive form of humanization, with partners perceived as having more cognitive ability eliciting more accurate empathic ratings for their negative (but not positive) stories. This suggests that our ability to empathize may be contingent on recognizing others as fully human. Of course, as this evidence is correlational, it is equally possible that accurate empathy increases our sense of another's humanity. Interestingly, this effect was unique both to negative stories—perhaps because expression of negative emotion is considered more diagnostic (Gross et al., 2000; Zaki et al., 2009)—and to human uniqueness. The literature has specifically linked this animalistic dehumanization to empathy gaps (e.g. Cuddy et al., 2007; Cehajic et al., 2009; Bruneau et al., 2018b)⁶ and has even shown that for groups that are animalistically dehumanized stereotypically (like African Americans), that dimension in particular predicts empathy (Andrighetto et al., 2014). Thus, while our findings do not reach conventional statistical significance after accounting for multiple comparisons, they are in line with previous research, suggesting that future work should replicate and explore the effect of mundane denials of mental capacity on empathy.

Lastly, dehumanization predicted how well people were able to coordinate their actions in a cooperative task. The current data suggest that perception of dissimilarity from machines (having greater emotional capacity) predicts more successful action coordination, controlling for individual skill level. If this is true, then recognizing another's human capacities may help us to cooperate, complementing work suggesting a role of theory of mind in cooperation (Liu et al., 2016; Tsoi et al., 2016) and work showing better synchronization of movement with human than robot partners (Keller et al., 2014). Given the importance of cooperation to intergroup (e.g. Bettencourt et al., 1992) and interpersonal (e.g. Deutsch, 1962) interactions, it is important to note that recognition of others' humanity in the form of emotional complexity may be a factor, even in a context like this where partners acted synchronously, without communicating or even looking at one another.

The biggest hurdle for this work was the lack of reliability of our dehumanization measure, which forced us to deviate from our preregistration (see the Supplementary Material for that analysis). Notably, the relationship between animalistic dehumanization and neural mirroring was robust to whether we analyzed the data with factor scores or difference scores, but the empathic accuracy and action coordination results were not. The byproduct of the factor scores was a separation of the measure into desirable and undesirable traits, which complicates interpretation and necessitates multiple comparisons. Dehumanization is perhaps most interesting when divorced from valence, when there is no dislike in the denial of humanity (e.g. Waytz and Schroeder, 2014). Fortunately, the differentiation of UH and HN suggests that we are not simply picking up on positive valence since the two desirable factors diverge in their relationships to the other variables. Moreover, results hold when controlling for how positively participants evaluated their partners to be (see Supplemental Materials). The factor analysis complicated what we were measuring but made that measurement more reliable.

⁶Note that some of this work uses inhumanization which was conceived of as an animalistic measure but has since been shown to relate to mechanistic as well (Martínez et al., 2017).

Conclusion

Subjects underwent in-lab interactions that showed that even face to face, people do not always recognize one another's humanity. Despite talking, sharing stories, and trying to cooperate, participants still rated each as other as human to varying degrees. This is the first time that dehumanization has been demonstrated in a real interaction without manipulation. Contrary to the previous work (Simon and Gutsell, 2020), we found a relationship between this dehumanization and neural mirroring, suggesting that we preferentially mirror not only biological motion (Ulloa and Pineda, 2007), but specifically motion from targets perceived to be less animal-like. We may not mirror those we do not perceive as fully human, which could add to the deleterious outcomes of dehumanization, given mirroring's association with empathy and understanding (Fox et al., 2016). Dehumanization also predicted cooperation. There is variance in perceptions of humanity in face-to-face interactions, and this variance results in differential neural processing, worse emotion recognition and poorer action coordination. Future work should continue to move beyond single-participant studies to better understand the fluctuations of social cognition.

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Conflict of interest

None declared.

Supplementary data

Supplementary data are available at SCAN online.

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