



# Does disgust-eliciting propaganda shape children's attitudes toward novel immigrant groups?

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## ABSTRACT

Propaganda frequently leverages themes of dirtiness and disease to foster negative attitudes toward marginalized social groups. Although history suggests that this tactic is highly successful, empirical evidence is required to evaluate propaganda's potential efficacy. Inspired by previous evidence that children rapidly form attitudes about social groups, we conducted an exploratory investigation into whether 5- to 9-year-olds' ( $N = 48$ ) judgments of novel foreign groups could be swayed by visually depicting one of these groups as disgusting in poster-sized illustrations. Across a wide battery of tasks, there was no clear indication that children readily internalize messages from propaganda in evaluating members of novel social groups. This finding held regardless of the type of disgustingness that was depicted in the propaganda, and generalized across the age range we investigated. Overall, our results are encouraging in a practical sense, suggesting that children are not easily swayed by negative misrepresentations of immigrants in propaganda.

## 1. Introduction

When social scientists diagnose the driving forces behind the Holocaust, the Rwandan genocide, and other mass slaughters, they commonly point to the manipulative use of anti-group propaganda as a fundamental precursor to genocide. For instance, the allegedly educational documentary *The Eternal Jew* was deliberately designed as a “demonstration of the parasitical nature of the Jews” (Fritz, 2011, p. 22) and may have been a critical form of instilling outgroup hate among the German population. Appeals to dirtiness and contamination in this agitprop movie are representative of tendencies to use disgust-eliciting themes to create and facilitate negative social attitudes and further derogate stigmatized others (Marshall & Shapiro, 2018; Speltini & Passini, 2014; Suedfeld & Schaller, 2002; Taylor, 2007). Ethnic immigrant groups have been blamed for the spread of disease (Markel, 1999; Markel & Stern, 2002) and the associations between foreigners and contagion often appear in the literature on immigration (Faulkner et al., 2004). Far from being relics, subtle versions of disgust-eliciting propaganda are still present in modern-day media and in the rhetoric of current political leaders. The prevalence of disgust-eliciting themes in prejudicial propaganda suggests that disgust may be particularly powerful in motivating brutality toward misrepresented groups. While

propaganda invoking contagiousness and dirtiness is generally disguised as serving an informative function despite having manipulative aims (Jowett & O'Donnell, 1992), these representations of foreign groups distort reality (Speltini & Passini, 2014); social groups misrepresented by propaganda as being filthy or contagious do not typically possess these qualities. Thus, it is possible that disgust-eliciting propaganda can be effective even when outgroups are not actually dirty or infectious.

Much of the Nazis' disgust-eliciting propaganda was aimed at youth (Gottfried, 2001). Children may be especially susceptible to the sway of propagandistic forms of media influence, especially at the present time; children are highly receptive to information about social groups (Conder & Lane, 2021; Lane et al., 2020; Over et al., 2018) and young people may now spend more time engaging with media than with their peers and families (Ward, 2003). The extent to which propaganda can meaningfully influence children's judgments of others remains an open empirical question, however. This is particularly the case given mixed evidence on children's abilities to transfer information from media to their everyday experiences (see Hopkins & Weisberg, 2017) as well as mixed evidence regarding children's tendencies to defer to authorities or to rely on firsthand perceptions when forming beliefs (see Harris, 2012). Here, we present findings from the first study on whether disgust-eliciting propaganda can socialize children's prejudicial tendencies toward

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immigrants.

### 1.1. Bias formation

By the time they are in preschool, children effortlessly categorize others into social groups based on a range of perceptually salient factors (see Banaji & Gelman, 2013). Children quickly move beyond mere social categorization (Chalik et al., 2022), and begin to show in-group preference (Aboud, 2013) and display intergroup biases (Dunham & Degner, 2013; Patterson & Bigler, 2006). These biases are sometimes directed toward novel or minimal groups that children have not yet encountered (Dunham, 2018; Lane et al., 2020), such as immigrant groups known to children only indirectly (e.g., through media exposure).

As children are exceptional social learners (see Tomasello, 2019), social learning has been identified as a primary mechanism behind intergroup prejudice formation (Chalik & Rhodes, 2015; also see Over & McCall, 2018). Most social learning research has been centered around the knowledge that children acquire from their primary caregivers, usually parents (Degner & Dalege, 2013). Some research, however, suggests that the effect of parental attitudes on their children's attitude formation may be overestimated (Aboud, 2013; Aboud & Doyle, 1996). In one study, young children of Black parents showed pro-White favoritism despite the fact that their parents were actively involved in the Black Pride civil rights movement (Branch & Newcombe, 1986). More recent research on the effects of peer and parent messages on children's interracial inclusion highlights significant role that peer socialization plays in shaping children's interracial inclusion tendencies (Burkholder et al., 2021). These findings together suggest that children's socialization of intergroup biases may occur largely outside of children's immediate families. Instead, children may be picking up on a larger social discourse regarding various social groups. Navigating multiple layers of cultural contexts imbued with rhetoric about unencountered groups, such as foreigners or immigrants, children may form negative attitudes about the groups they have not even personally met, something frequently noticed in areas with largely homogeneous populations (McGlothlin & Killen, 2006). In some cases, children may learn social biases rapidly and via relatively passive processes (Lane et al., 2020).

The socialization of outgroup beliefs is likely to be uniquely tailored to particular groups. While some outgroups are tolerated, others are dehumanized and discriminated against, as evidenced by popular support for opening national borders for some groups of immigrants while shutting them for others. Different social groups elicit unique patterns of emotions; for example, gay men tend to elicit more disgust than fear, while African Americans tend to elicit more fear than disgust (Cottrell & Neuberg, 2005). Groups that are associated with disgust may evoke especially strong prejudice. Indeed, functional neuroimaging research has shown that outgroups which are most likely to be perceived as disgusting are uniquely unlikely to activate others' mentalizing networks (Harris & Fiske, 2007). Additionally, research has indicated that children and adults exhibit negative biases toward physically dirty individuals, when attributing resources, deciding whose testimony to trust, evaluating character traits, and forming implicit associations (Rottman et al., 2020), and physical cleanliness may affect children's social preferences even more than race (Epstein et al., 1976). Judging by the apparent effectiveness of propaganda that utilizes disgust to demonize outgroups, it is possible that figuratively presenting social groups as disgusting may similarly elicit high levels of prejudice.

### 1.2. Contagiousness, dirtiness, and bias

Given that disgust is often targeted by propaganda and may play a particularly insidious role in dehumanizing and marginalizing outgroups (e.g., Buckels & Trapnell, 2013; Marshall & Shapiro, 2018; Skinner & Hudac, 2017), we focused our efforts on measuring the effects of propaganda that presented outgroup members as either clean or disgusting. Disgust is a complex emotion, however, and people can be

depicted as disgusting in myriad ways. The function of disgust is contested and the avoidance it engenders could be targeted primarily toward germs, undesirable social partners, and/or anything that is generally considered to be impure (see Rottman et al., 2019; Rozin et al., 2016). Previous research has suggested that children's and adults' social biases against physically dirty individuals emerge regardless of whether these individuals harbor disease, act in non-normative ways, or became accidentally soiled (Rottman et al., 2020); here, we investigated whether a similar variety of causes would exert different effects on prejudice when they are figuratively represented in propaganda rather than existing in reality.

The avoidance of contagion is the function most commonly ascribed to the emotion of disgust (e.g., Oaten et al., 2011), and therefore disgust-eliciting propaganda may be particularly effective when it presents outgroups as vectors of disease. There is some empirical evidence to suggest that invoking perceptions of vulnerability to disease encourages negative attitudes and behaviors toward foreigners (Faulkner et al., 2004; Prati & Pietrantonio, 2016), as well as in-group favoritism (Navarrete & Fessler, 2006). Children's social preferences may be affected by perceptions of contagiousness as well. A study that examined 6- and 7-year-olds' behavior in a playground setting showed that children avoided contact with sick individuals, and children's ability to make predictions about contagious illnesses was a significant predictor of their avoidance behaviors (Blacker & LoBue, 2016).

However, it is also possible that certain non-normative behaviors can elicit disgust, even in the absence of contagion, as disgust may operate to police social boundaries and exclude others who are perceived to be deviant or who are "othered" in some way (e.g., Giner-Sorolla & Sabo, 2016; Rottman et al., 2018). Recent research has indicated that political outgroup members are considered to be grosser than ingroup members even though they are not considered to be less healthy (Landy et al., 2022). Thus, novel social groups that are depicted as disgustingly indecorous rather than infectious may yield high levels of prejudice.

Whereas previous research has focused on the effect of physical manifestations of dirtiness and contagiousness, people also project dirtiness and contagion onto stigmatized others, such that "dirtiness" can be a product of metaphor or imagination (Nemeroff & Rozin, 1994). The effects of associations between concrete bad smells and negative abstract concepts, common in many world languages, may be bidirectional. In one study, participants' exposure to fishy smells led to a more suspicious and less cooperative attitude toward others, while a social suspicion prime led to a more accurate identification of fishy smells (Lee & Schwarz, 2012). The bidirectionality of widely endorsed metaphors provides a reason to think that general appeals to impurity and contagiousness could create perceptions of disgustingness even in the absence of physical evidence of dirtiness or contagion. Therefore, appeals to concepts of dirtiness could be a powerful tool used against stigmatized groups represented as dirty, disgusting, or smelly, even if the individual members of these groups do not tend to be stinky or filthy in reality (Lizardo, 2012).

### 1.3. Overview of the present research

Given the prevalence of disgust-eliciting themes in prejudicial propaganda and the power of disgust to dehumanize others (Buckels & Trapnell, 2013; Nussbaum, 2004), we investigated whether children's judgments of novel immigrant group members would be swayed by propaganda depicting these groups as dirty or contagious, even if the actual members of these groups do not themselves exhibit any physical signs of dirtiness or disease. Studying the origins of these biases in children can provide an understanding of why and how young children begin to display negative attitudes toward others represented as dirty or sick, with the goal of inspiring future interventions to prevent discriminatory treatment of misrepresented groups. Additionally, if biases formed upon perceived contagiousness or dirtiness of others have different underlying mechanisms based on the cause of dirtiness,

potential interventions need to distinguish and target these distinct pathways toward prejudice. To our knowledge, no previous study has investigated the effects of disgust-eliciting propaganda on outgroup bias. As such, the present research represents an exploratory first step in this direction, using a wide array of measures in order to uncover patterns that future research can rigorously investigate in greater depth (see Rozin, 2001).

In investigating whether propaganda can socialize children's prejudicial tendencies toward immigrants, we focused on the effects of disgust-eliciting propaganda on elementary-school-aged children's explicit and implicit attitudes toward novel ethnic groups. This age group was selected because middle childhood is a crucial period for developing outgroup biases (see Raabe & Beelmann, 2011). Because discrimination varies across different outgroups (Cottrell & Neuberg, 2005; Harris & Fiske, 2007) and because globalization facilitates encountering foreigners who are represented both negatively and positively, we asked participants to make judgments of two equally unfamiliar immigrant groups differing only in their degree of cleanliness as depicted in propaganda posters. We chose to expose children to fabricated novel groups to avoid negatively influencing their attitudes toward real groups and because recent research has demonstrated that children rapidly form attitudes about novel groups when provided with relevant social information (Dunham et al., 2011; Lane et al., 2020). These groups were portrayed as being ethnically dissimilar (i.e., representing distinct social kinds and being from different countries) while not differing in skin color, because superficial differences often lead to biases in the absence of socialization (see Hailey & Olson, 2013), and because children can form intergroup biases even in the absence of perceptual dissimilarities (Heiphetz et al., 2013). In order to examine how readily children would be to generalize negative traits depicted in propaganda to real individuals, participants were only briefly introduced to propaganda posters targeting these two novel groups at the beginning of the study session.

Mirroring previous research that compared three different sources of uncleanliness (Rottman et al., 2020), we created three different negative propaganda posters, each displaying a distinct form of disgustingness. In contrast to an immigrant group that was depicted as clean and appealing, the "disgusting" immigrant group was depicted either as contagious, as acting in socially aberrant ways, or as being immersed in a dirty environment. These three between-subjects conditions allowed us to investigate whether different types of figuratively depicted dirtiness may produce different forms of social bias, as contrasted to the "clean" immigrant group. Given that our study was exploratory and that Rottman et al. (2020) did not find condition effects in most of their tasks, we did not make predictions about condition differences. We similarly did not make any specific predictions about developmental changes between our youngest participants (five-year-olds) and our oldest participants (nine-year-olds), although our broad age range provided us with the ability to measure patterns of change across a broad range of ages within a crucial period of social cognitive development.

We administered a variety of tasks, each chosen to measure a distinct manifestation of ingroup bias. Some of these were versions of measures used in Rottman et al. (2020), including a trait attribution task, a measure of selective trust (Koenig & Harris, 2005), a measure of resource allocation (Buttelmann & Böhm, 2014), and a drawing task measure of implicit distancing (inspired by Diesendruck & Menahem, 2015). Additionally, we included several new measures: a measure of expected cultural norms, a measure of moral parochialism (Rhodes & Chalik, 2013), and two measures of dehumanization: one explicit and blatant (Kteily et al., 2015) and one implicit and subtle (McLoughlin & Over, 2017). The trait attribution task provided insight into a particularly straightforward and transparent form of bias. Selective trust was measured because children are more prone to trust information from groups they prefer (e.g., Elashi & Mills, 2014), and because this tendency has the potential to systematically silence members of disliked groups. Resource allocation was measured because it uncovers a form of

bias that manifests in unequal outcomes and because it indicates behavioral tendencies. The drawing task provided a much more subtle measure of bias, which could demonstrate a tendency to perceive more disgusting groups as being more physically distant. The expected cultural norms task was similar, but instead measured distance less concretely, in terms of cultural practices (which is arguably a more socially relevant form of dissimilarity). The task measuring moral parochialism allowed us to assess whether children perceive moral obligations to be more readily extended to some outgroups over others. Finally, dehumanization has been associated with disgust and yields some of the most severe negative consequences (Haslam & Loughnan, 2014), and it might operate differently when measured blatantly as compared to subtly (Kteily & Landry, 2022).

Finally, we assessed individual differences in a few relevant traits. As interpersonal disgust sensitivity and social dominance orientation play a role in outgroup dehumanization and prejudice against foreigners (Costello & Hodson, 2014; Hodson & Costello, 2007) and as fear of disease has been linked to ethnocentric and xenophobic attitudes (Faulkner et al., 2004; Navarrete & Fessler, 2006), we included three individual difference measures—disgust sensitivity, social dominance orientation, and dispositional fear—to explore potential mechanisms underlying biases against members of foreign outgroups who are figuratively represented as disgusting.

We have reported all measures, conditions, data exclusions, and procedures for sample size determination below. All materials and data can be publicly accessed at <https://osf.io/g7q2n/>.

## 2. Method

### 2.1. Participants

Participants were 48 5- to 9-year-old children tested in a college laboratory in the northeastern United States (18 female;  $M_{\text{age}} = 7.37$ ,  $SD_{\text{age}} = 1.38$ ; 7 five-year-olds, 13 six-year-olds, 11 seven-year-olds, 9 eight-year-olds, 8 nine-year-olds). Although we did not collect data on participants' SES, race, or ethnicity, our participant database is predominantly comprised of families who are middle- to upper-middle-class and White. Two additional participants were tested but excluded and replaced due to uncooperative behavior ( $n = 1$ ) or experimenter error ( $n = 1$ ). Because our procedure was modeled after Study 2 in Rottman et al. (2020), and because this research was exploratory, we did not run any formal power analyses to determine sample size, but rather determined our sample size by matching the number of children tested in this previous study. Although we happened to randomly sample more boys than girls, we had no expectations that gender would influence our results and so did not attempt to correct this imbalance.

### 2.2. Materials and procedure

Participants were greeted and tested by the experimenter (who was the first author, with the exception of four participants who were tested by another undergraduate research assistant). Prior to testing, parents or legal guardians gave informed consent to their children's participation in the study. Participants' verbal assent to participating was obtained upon entering the testing room. Participants were seated at a table facing two large (42" × 54") posters hanging on the wall in front of them, positioned side-by-side (the sides of the posters were counterbalanced). One of the illustrations depicted a group that was portrayed as disgusting while the other depicted a group that was portrayed as orderly and clean. Each participant was randomly assigned to view one of the three versions of the disgusting poster, which differed in the form of grossness that was depicted (see Fig. 1). All participants saw the same version of the clean poster. The Contagiousness poster depicted people who were surrounded by a clean environment but who looked ill in various ways ( $n = 16$ ). The Aberration poster depicted people who acted in abnormal, disgusting, ways ( $n = 16$ ). The Dirty Environment poster depicted



Fig. 1. Propaganda illustrations used as stimuli.

Note. Top row from left to right: Contagiousness poster, Aberration poster. Bottom row from left to right: Dirty Environment poster, Clean poster. The poster backgrounds were chartreuse and black or white and blue to heighten associations with disgustingness or cleanliness, respectively.

people who were immersed in a filthy environment but who themselves were physically clean ( $n = 16$ ). Aside from the version of the disgusting poster used, nothing differed across the three conditions.

All participants were told that the posters were made by someone else to educate locals about two groups of immigrants who were starting to move into town, and who might therefore be increasingly encountered in participants' schools and neighborhoods. One group was referred to as Gozzers, who were from the country of Gozzilia, and another group was labeled as Smopes, who were from the country of Smopeland (names were counterbalanced). Although the posters remained on the wall throughout the study as participants completed the dependent measures, we intentionally never referred back to these posters given that our intention was to measure whether and how social biases could be internalized upon a single brief presentation of figurative propaganda.

After the presentation of the propaganda poster depicting Gozzers, participants were presented with five photographs of "real" Gozzer children who were the targets used in all subsequent tasks. Similarly, after the presentation of the propaganda poster depicting Smopes, participants were presented with five photographs of "real" Smope children. The children in these 10 photographs were White and approximately matched to participants' age, and have successfully been used in previous studies on social cognitive development (e.g., Kinzler et al., 2007). These children appeared happy and healthy and did not show any signs of physical dirtiness, illness, or abnormality. The two sets of photographs were roughly matched on hair color and similarity in facial expressions, and the sets were counterbalanced as portraying either the disgusting group or the clean group.

Once children were familiarized with the two immigrant groups, they were presented with the following tasks, presented in a pre-determined fixed order: Subtle Dehumanization, Selective Trust, Resource Distribution, Explicit Evaluations, Selective Morals, Distancing, Blatant Dehumanization, Social Dominance Orientation Scale, Disgust Scale, Fear Scale, Cultural Norms, and the Realism Check. (This fixed order was chosen to minimize the possibility of carryover effects from earlier tasks to subsequent tasks, as in Rottman et al., 2020.) These tasks are described below and summarized in Table 1.

The entire study lasted approximately 35–45 min for each participant. Upon completing the study, all participants were debriefed with their families and given a small gift.

## 2.2.1. Dependent measures

### 2.2.1.1. Explicit evaluations.

Participants were presented with a white mat, atop which were green and yellow letter-size pieces of paper and two small photographs of a Gozzer and a Smope. Participants were then given a list of characteristics and they were asked to stick the photographs of the Gozzer and the Smope onto a patch of Velcro on the green square if the word matched with the person and onto a patch of Velcro on the yellow square if the word did not match with the person. Thus, participants could attribute each item to only one person, both, or neither.

First, participants were presented with eight traits that varied in valence (positive vs. negative) and distinct humanness (high human vs. low human), inspired by Demoulin et al. (2004) and Haslam et al. (2005). The four negative traits included: mean, lazy, can be irresponsible, can feel ashamed. The four positive traits included: tells the truth, smart, can have good imagination, can feel brave. Out of these eight traits, the four high-human traits were: can be irresponsible, can have a good imagination, can feel ashamed, can feel brave. The remaining four traits were categorized as low-human traits. Participants were also asked with whom they would be friends and why they would be friends with this person. Additionally, participants were given a list of five moral transgressions that each exemplified one of five basic moral foundations (Graham et al., 2009) in a child-friendly manner (e.g., talking back to

the teacher as a violation of loyalty).<sup>1</sup>

The four negative traits and five moral transgressions were reverse-coded such that attributing the trait or transgression to the individual from the "disgusting" group resulted in a score of 1 (i.e., favoritism in the direction of the individual from the clean group) and attributing the trait or transgression to the individual from the clean group resulted in a score of -1 (i.e., favoritism in the direction of the individual from the disgusting group). Identifying the trait or transgression with neither or both of the individuals resulted in a score of 0, indicating no bias.

A factor analysis of the 14 items revealed that all items loaded onto one factor, indicating that children did not reliably differentiate between uniquely human vs. non-uniquely human traits or between positive and negative traits, or between trait attribution items and moral transgression evaluations. As there was excellent internal consistency among these items (Cronbach's  $\alpha = 0.90$ ), we averaged responses across the 14 explicit evaluations.

### 2.2.1.2. Cultural norms.

Participants were asked about Gozzers' and Smopes' food and toy preferences by indicating whether the Gozzers and Smopes would prefer to eat a pizza with corn dogs on top or a cheese pizza and whether these groups would prefer to play Parcheesi or Monopoly. As each of these sets included an unfamiliar stimulus and a familiar stimulus, choices served as a proxy for perceived cultural strangeness. Each participant evaluated food preferences and toy preferences. However, since there was no effect of item type in the linear model, we used a single score of cultural preferences for each participant, averaged across the two pairs of items.

### 2.2.1.3. Resource distribution.

Participants were given four desirable items (fidget spinner, stuffed animal, box of candy, box of new headphones) and four undesirable items (ginger root, broken toy, floppy disk, crushed soda can) in a randomized order. Then participants were asked to distribute these items between a bucket that belonged to a Gozzer, a bucket that belonged to a Smope, and an empty bucket (adapted from Buttelmann & Böhm, 2014). Following the distribution, participants were asked why they gave the items to the Gozzer and to the Smope.

The four undesirable resources were reverse-coded such that giving the resource to the individual from the "disgusting" group resulted in a score of 1 (i.e., a preference in the direction of the individual from the clean group) and giving the resource to the individual from the clean group resulted in a score of -1 (i.e., a preference in the direction of the individual from the disgusting group). Putting the resource in the empty bucket (giving it to neither of the individuals) resulted in a score of 0, indicating no bias. Therefore, positive numbers indicate a positive bias toward individuals from the clean group and a prejudice against individuals from the disgusting group.

A parallel analysis of the eight items revealed that all items loaded onto one factor with acceptable internal consistency, Cronbach's  $\alpha = 0.67$ ; there was no evidence of positive and negative items loading onto separate factors. Therefore, we averaged responses across the eight items.

### 2.2.1.4. Selective trust.

Participants were shown a Gozzer and a Smope who provided them conflicting labels for two novel objects (*koba* vs. *hux* and *fiffin* vs. *blap*, names counterbalanced; pictures acquired from Horst

<sup>1</sup> Participants were also given a brief warm-up at the beginning of this task to ensure that they understood the instructions. As part of this warm-up, participants were presented with a picture of a Smope and a picture of a Gozzer (from the set of photographs that had previously been identified with each group) and asked to identify which was a Gozzer. This served as a mid-study attention check, ensuring that participants still remembered differences between the members of the two novel groups. All children ( $N = 48$ ) successfully identified a picture of a Gozzer as Gozzer, giving us greater confidence in their attention to the tasks.

**Table 1**  
Summary of the primary measures used in the study.

Task	Sample item	Response options	Coding
Explicit Evaluation	Stick anybody who would hit someone really hard on green.	Gozzer Smope Both Neither	1 = Preference for clean group -1 = Preference for disgusting group 0 = Both/neither
Cultural Norms	What kind of pizza do people like eating in Smopeland?	Normal Strange	1 = (Normal for clean group, strange for disgusting group) -1 = (Strange for clean group, normal for disgusting group)
Resource Distribution	Which bucket should this go in?	Gozzer Smope Neither	1 = Preference for clean group -1 = Preference for disgusting group 0 = Neither
Selective Trust	Do you think that this toy is called a koba like the Gozzer said, or do you think that this toy is called a hux like the Smope said?	Gozzer Smope	1 = Trusting clean group -1 = Trusting disgusting group
Selective Morals	What if there was no rule against teasing? Then how bad would it be for the Gozzer to tease a Smope and hurt his/her feelings?	Not at all bad A little bad Pretty bad Very, very bad	Difference scores = (harm wrongness for clean group – harm wrongness for disgusting group)
Blatant Dehumanization	Which one is most like a Gozzer?	Four silhouettes of primates, each increasingly more bipedal and humanlike	Difference score = (clean group rating – disgusting group rating)
Subtle Dehumanization	What do you think the Gozzers were doing?	Open-ended	Difference scores = (clean group's mental states – disgusting group's mental states)
Distancing	Where do you think Smopeland is, where the Smopes are from?	Dots drawing	Difference score = (disgusting group's hometown distance – clean group's hometown distance)

& Hout, 2016). Then participants were asked what each object was named, indicating which informant participants trusted (see Koenig & Harris, 2005). Each participant had a single final score of trust tendencies, which was averaged across the two items.

**2.2.1.5. Selective morals.** Participants were told two short stories featuring a Gozzer and a Smope. Each story was accompanied by a simple picture of a situation and depicted a Gozzer and a Smope either as a perpetrator or as a victim (adapted from Rhodes & Chalikh, 2013), counterbalanced across the two stories. In one scenario, participants were told that a Gozzer or Smope teased a member of the other social group and hurt their feelings. In another scenario, a Gozzer or Smope took all the blocks for themselves and wouldn't share them with a member of the other social group, making them sad. Participants were then asked to rate how bad the actions of the perpetrator were on a 4-point smiley Likert scale (Not at all bad; A little bad; Pretty bad; Very, very bad). After that, participants were told to imagine that there were no rules against teasing or not sharing and they subsequently rated the badness of the perpetrator's action on the same 4-point smiley Likert scale. Two difference scores were calculated by subtracting the ratings of the wrongness of harm caused to the individual from the clean group from the ratings of the individual from the "disgusting" group.

**2.2.1.6. Blatant dehumanization.** Participants were presented with an "Ascent of Man" schematic with four stages of hominid evolution (adapted from Keily et al., 2015) progressing from a chimp-like ape to a human. Then they were asked to point to the hominid who was most like a Gozzer and to the hominid who was most like a Smope.

Participants' scores were coded on a 1–4 scale with 1 identifying the rating of the individual as most ape-like and 4 as most human-like. A difference score was computed by subtracting the rating of the individual from the clean group from the rating of the individual from the "disgusting" group. Thus, higher numbers represented larger relative dehumanization of the individual from the disgusting group compared to the individual from the clean group. To make more direct comparisons with other tasks, we recoded the difference scores such that a difference score of 3 was represented as either 1 (when a member of the "clean" group was judged as more human) or –1 (when a member of the "disgusting" group was judged as more human), and scores of 2 and 1

were recoded as  $\pm 0.66$  and  $\pm 0.33$ , respectively.

**2.2.1.7. Subtle dehumanization.** Participants were presented with two Frith-Happé animations originally developed by Abell et al. (2000) which have been previously used to test mental state attribution (McLoughlin & Over, 2017). The videos were approximately 40 s each and depicted two animated triangles interacting with each other within a square. In the Coaxing video, one of the triangles appeared to coax the other out of the square. In the Surprising video, one of the triangles appeared to surprise the other. The original colors of the triangles were changed to black, following the procedure of McLoughlin and Over (2017). Participants were told that each video would show either Gozzers or Smopes (counterbalanced across participants). After participants watched a video, they were given an opportunity to watch it a second time. Then participants were asked to describe what was happening in the video and what the Gozzers or Smopes were doing. Participants were also asked to tell the researcher about each triangle separately. This provided participants with ample opportunity to utilize mental state terms if they were indeed thinking about the Gozzers or Smopes in mentalistic ways. For example, the following five items from one participant's description were coded as mental states terms: "he is trying to help the other Gozzer," "the big Gozzer was trying to help the little Gozzer cause he did not know," "they were helping each other," "he was trying to help," "he was trying to get in but did not know how to," with a total of two unique mental states terms ("help" and "know"). Two independent coders coded children's descriptions of the events, blind to condition, and demonstrated substantial agreement (Total mental states:  $\kappa = 0.66$ ; Unique mental states:  $\kappa = 0.71$ ). All disagreements were resolved through mutual discussion with a third party, who was also blind to condition.

A difference score was computed by subtracting the total number of mental states ascribed to the members of the clean group from the total number of mental states ascribed to the members of the disgusting group. A separate difference score was computed for the number of unique mental states ascribed to the members of the clean and disgusting groups. Thus, higher numbers on both measures represented larger relative subtle dehumanization of the disgusting group members (see McLoughlin & Over, 2017).

**2.2.1.8. Distancing.** Participants were given a black and white map of the world which had no labels except for their hometown. Then, participants were asked to indicate the probable locations of Gozzilia and Smopeland on the map.

A difference score was computed by subtracting the distance (in centimeters) between the participant's hometown and their indication of the "disgusting" group's country on the map from the distance between the participant's hometown and their indication of clean group's country on the map. Thus, higher numbers represented larger relative distances that were estimated between the participant's hometown and the "disgusting" group's country.

## 2.2.2. Additional measures

**2.2.2.1. Social dominance orientation.** Participants were asked whether all children in the world should be treated in the same way and whether all children in the world should have a chance to do the same kinds of things (adapted from Vezzali et al., 2018). The responses were recorded on a 4-point smiley Likert scale (Definitely not, Probably not, Probably yes, Definitely yes). All scores were reverse-coded and were intended to be averaged into a single composite.

**2.2.2.2. Trait disgust.** Participants were administered the 15-item Child Disgust Scale to evaluate their disgust sensitivity (Viar-Paxton et al., 2015). The Child Disgust Scale used a 3-point Likert scale (0 = always, 1 = sometimes, and 2 = never). The items were presented in a question (rather than statement) form since the experimenter administered the scale (rather than participants completing it on their own). After reverse-coding some items, scores across the fifteen items were intended to be averaged into a single composite.

**2.2.2.3. Trait fear.** Participants were administered a dispositional Fear Scale created for this study, which consisted of 8 items asked in a question form (e.g., "How afraid are you of rats and mice?"). Items were generated based on typical fear elicitors discussed in the literature, and were intended to be averaged into a single composite.

**2.2.2.4. Realism measure.** Participants were asked whether they thought that all Gozzers and all Smopes looked like those on propaganda posters in real life. Participants' responses were recorded on a 4-point smiley Likert scale (Definitely not, Maybe not, Maybe yes, Definitely yes) and the two items were averaged, such that higher scores indicated stronger beliefs in the reality of the outgroups.

## 2.3. Coding

All analyses were conducted in RStudio, Version 4.2.0 (R Core Team, 2022; RStudio Team, 2022). Across all tasks, we were primarily interested in whether participants showed positive biases toward individuals from the clean group and negative biases toward individuals from the "disgusting" group. Our analytic strategy directly followed Rottman et al. (2020). As described above, all responses were coded such that positive biases for individuals from the clean group and negative biases for individuals from the disgusting group were given a score of 1, positive biases for individuals from the disgusting group and negative biases for individuals from the clean group were given a score of -1, and no bias (in cases when a neutral option was possible) was given a score of 0, for all measures for which this coding scheme was possible.

## 3. Results

### 3.1. Condition and age differences

We largely followed an analytic strategy used by Rottman et al. (2020). Given the large number of dependent measures, we aimed to

reduce the number of tests conducted—and also to increase power—by initially examining whether there were condition and age differences for each measure and then collapsing across conditions and ages when there were no significant differences. Thus, for each measure, we first tested for condition differences with linear or logistic models that accounted for random effects of participant and/or item, or with linear or logistic regressions in cases where there was only one response per participant. The Dirty Environment condition was dummy-coded as the baseline condition to which the other two conditions were compared, since this condition did not depict the immigrants as inherently dirty or disgusting; rather, only the context in which the immigrants were embedded was depicted as dirty, and thus a bias is particularly unwarranted in this condition. There were no significant effects of either condition or age (see Supplemental materials for regression coefficients and model specifics).<sup>2</sup> The presence of bias was determined by conducting one-sample *t*-tests against chance (i.e., 0, or 50%), collapsing across conditions and ages (see Table 2 for summary findings). For all tests, we applied a Bonferroni correction to account for the fact that we had 9 dependent measures; to attenuate the risk of false positives, we set our alpha level to .005.

### 3.2. Dependent measures

#### 3.2.1. Explicit evaluations

A single one-sample *t*-test was conducted to evaluate whether participants were more likely to give more favorable evaluations of members of the clean group than members of the "disgusting" group. There was no statistically significant difference from chance levels (with  $\leq .005$ ) in participants' ascriptions of negative and positive characteristics to the individuals from the disgusting group and individuals from the clean group,  $t(47) = 2.26, p = .029, d = 0.33$ .

#### 3.2.2. Cultural norms

To test whether participants were more likely to evaluate cultural practices of the clean group as more typical than those of the "disgusting" group, a one-sample *t*-test compared judgments of typical cultural practices to chance levels. There was no statistically significant difference (with  $\leq .005$ ) in participants' ascriptions of cultural tendencies to the individuals from the clean and disgusting groups,  $t(47) = 2.49, p = .014, d = 0.23$ .

**Table 2**

Tendencies to favor members of the clean group across tasks and conditions.

	<i>n</i>	<i>M</i>	<i>SD</i>	<i>t</i>	<i>p</i>	Cohen's <i>d</i>
1. Explicit Evaluations	48	0.15	0.45	2.27	.028	0.33
2. Cultural Norms	48	0.23	0.90	2.49	.014	0.25
3. Resource Distribution	48	0.12	0.50	1.65	.105	0.24
4. Selective Trust	48	0.08	0.54	1.07	.290	0.15
5. Selective Morals	48	0.18	0.61	2.03	.049	0.29
6. Blatant Dehumanization	48	0.14	0.50	1.93	.060	0.28
7. Subtle Dehumanization (Total)	47	0.15	2.65	0.38	.702	0.06
8. Subtle Dehumanization (Unique)	47	0.21	1.38	1.06	.297	0.15
9. Distancing	47	1.19	6.35	1.28	.207	0.19

Note. Mean, standard deviations, and one-sample *t*-test results for all the primary tasks capturing overall preferences for members of the clean group. Given the number of tests conducted, the alpha level was set to .005.

<sup>2</sup> Additionally, we reran all linear models with the Aberration condition dummy-coded as the baseline condition, as this allowed us to compare the Aberration condition to the Contagiousness condition. There were similarly no differences between these two conditions.

### 3.2.3. Resource distribution

A one-sample *t*-test was conducted to evaluate whether participants were more likely to favor the individual from the clean group when distributing resources compared against chance levels. This *t*-test indicated no significant difference in participants' tendencies to distribute resources to individuals from the clean or disgusting groups,  $t(47) = 1.65$ ,  $p = .105$ ,  $d = 0.24$ .

### 3.2.4. Selective trust

To evaluate whether participants were more likely to trust the individual from the clean group than the individual from the "disgusting" group, a one-sample *t*-test compared overall tendencies to trust each individual to chance. This *t*-test indicated no significant difference in participants' trust preferences across the two novel outgroups,  $t(47) = 1.07$ ,  $p = .290$ ,  $d = 0.15$ .

### 3.2.5. Selective morals

To evaluate whether participants were more likely to judge harmful actions toward the individual from the clean group as more wrong compared to the harmful actions against the individual from the "disgusting" group, a one-sample *t*-test compared overall tendencies to evaluate wrongness to chance. This *t*-test indicated no significant difference in participants' wrongness judgments across the two novel outgroups,  $t(47) = 2.03$ ,  $p = .049$ ,  $d = 0.29$ .

### 3.2.6. Blatant dehumanization

To evaluate whether participants were more likely to dehumanize the individual from the "disgusting" group than the individual from the clean group, one-sample *t*-tests compared overall relative dehumanization tendencies to chance levels. There was no statistically significant difference in participants' likelihood to dehumanize the individual from the disgusting group,  $t(47) = 1.93$ ,  $p = .060$ ,  $d = 0.28$ .

### 3.2.7. Subtle dehumanization

One participant was excluded from this analysis due to an experimenter error. To test whether participants were more likely to dehumanize individuals from the "disgusting" group than individuals from the clean group, two one-sample *t*-tests compared the differences in relative dehumanization to chance. These tests indicated no significant difference in the number of total ascribed mental states,  $t(46) = 0.38$ ,  $p = .702$ ,  $d = 0.06$ , and no significant difference in the number of unique ascribed mental states,  $t(46) = 1.06$ ,  $p = .297$ ,  $d = 0.21$ , across the two novel outgroups.

### 3.2.8. Distancing

One participant was excluded from this analysis due to the non-specificity of their markings of the map. To test whether participants were more likely to place the disgusting group's country farther away from their hometown than the clean group's country, a one-sample *t*-test compared the difference in distances to chance. This test indicated no significant difference in relative distances between the home countries of the two novel outgroups,  $t(46) = 1.28$ ,  $p = .207$ ,  $d = 0.19$ .

## 3.3. Additional measures

None of our individual difference measures were internally consistent (Disgust Scale:  $\alpha = 0.58$ ; Fear Scale:  $\alpha = 0.34$ ; SDO:  $r = 0.27$ ). Therefore, no analyses were conducted with these scales. We ran correlations between all dependent measures (a Bonferroni correction was applied to account for 45 correlation tests; to attenuate the risk of false positives, we set our alpha level to .001). There were moderate negative correlations between participants' belief in the reality of the clean and disgusting groups and their tendency to distribute resources to the member of the clean group ( $r = -0.52$ ,  $p < .001$ ) and their tendency to place the disgusting group's country farther away from their hometown than the clean group's country ( $r = -0.48$ ,  $p < .001$ ). This means that the

more participants believed that the Gozzers and the Smopes were real, the more likely they were to be biased against the members of the "disgusting" group on these two measures. All other correlations were non-significant, and most were small in magnitude ( $r_s < 0.3$ ); see the Supplemental materials.

## 4. Discussion

Our goal in this exploratory study was to examine whether children's biases against members of novel immigrant groups would be influenced by their exposure to illustrations depicting these groups as contagious, disgustingly deviant, or living in squalor. Our results indicated that there was no clear effect of propaganda on leading children to develop negative biases toward an immigrant group that was portrayed as being "disgusting"; across all of the tasks we employed, there was no statistically significant indication that children immediately internalized messages from propaganda when evaluating members of novel social groups. Also, in line with the findings of previous research by Rottman et al. (2020), no condition effects or age effects emerged on any of the tasks. These null effects are striking in light of previous work indicating that social communication (e.g., adults' testimony) can be highly effective in influencing children's attitudes toward others (e.g., Lane et al., 2020; Shinohara et al., 2021). It is possible that the indirect and impersonal nature of propaganda renders it a less efficacious medium for influencing how children evaluate novel groups.

Overall, in stark contrast to the strong effects of physical dirtiness and contagion on both implicit and explicit biases found by Rottman et al. (2020), we uncovered no clear biases against the members of the "disgusting" outgroups in the present study. The differences between biases against physically dirty individuals and biases against individuals from figuratively disgusting outgroups are especially apparent when comparing effect sizes of similar tasks used in Study 2 of Rottman et al. (2020) and the study reported here. In the case of selective trust, Rottman et al. found that participants tended to selectively trust a physically clean individual more than a physically dirty individual when forming beliefs about novel animals and novel foods ( $d = 0.59$ ). Although our results tended in the same direction of bias, the effect was much weaker ( $d = 0.15$ ). Moreover, the tendency to favor clean children when distributing resources was twice as strong in Rottman et al. ( $d = 0.43$ ) as compared to the present research ( $d = 0.24$ ). Even for the Explicit Evaluation Task, which yielded a significant result, the effect we found for children's trait attributions ( $d = 0.33$ ) was much weaker than Rottman et al.'s, which uncovered medium to large effect sizes across conditions on a similar task ( $d$ s ranged from 0.55 to 1.48). Taken together, these comparisons suggest that, to the extent that figuratively portraying an outgroup as disgusting in propaganda can produce any negative social biases, these effects are substantially weaker than the effects of actual physical dirtiness.

Certainly, one limitation of our research was our modest sample size, which restricted our precision in uncovering the presence of biases. Given the large battery of tasks we used, we applied a conservative alpha level correction in order to minimize the risk of false positive results. Of course, this simultaneously increases the (arguably lesser) risk of false negative results. The diminutive effect sizes we uncovered suggest that the present study was quite underpowered, to the extent that true effects existed that we failed to detect. Of course, a plausible alternative is that disgust-eliciting propaganda does not strongly impact children's social beliefs, and thus there was no true effect to be detected for many tasks. Future research will need to account for the feasibility of detecting small effects with child samples, and should focus more specifically on particular tasks to mitigate the need to reduce alpha levels.

Additionally, several differences between our study paradigm and previous research on bias development may explain why we did not find any significant results in the present study. Typically, research on children's social biases assesses relative preferences for children's own ingroup over an outgroup; this is the case both for research on real-world

groups (see Kinzler, 2020) and for research on minimal groups (see Dunham, 2018). In our study, we instead asked participants to make judgments about two equally unfamiliar outgroups, which ensured that the target characters were matched on all possible dimensions (including on salient demographic features such as gender, race, and age), with the sole exception of purported disgustingness. Finally, we included a relatively subtle experimental manipulation, and participants were asked questions only about the individuals depicted in our photographs (who showed no physical manifestation of dirtiness or contagiousness) rather than about the groups depicted in our propaganda posters.

Several other limitations of our study also pose constraints on interpreting these results as conclusive and generalizable. Although we did not uncover robust age effects, participants on the younger side of the age range may have had a more difficult time understanding the study set-up and staying attentive throughout the whole session. Additionally, because we were interested in whether children would internalize biases from a very brief presentation of negative social attitudes, as in some previous research (e.g., Lane et al., 2020), we did not engage children with the propaganda as much as we could have, nor did we administer manipulation checks throughout the study session to ensure children's recall that the photographs of novel groups corresponded to the figurative depictions of these novel groups; future research could be more heavy-handed in these regards. Finally, although we aimed to investigate the specific effects of disgust-eliciting propaganda, it is possible that children instead perceived the members of the disgusting outgroups in a differently negative light (e.g., as impoverished) or were instead guided by arbitrary preferences (e.g., the colors of the posters). Future research could better control for such potential confounds and could also directly compare the effect of disgust-eliciting propaganda to the effects of other negative depictions of immigrants, such as having low social status or as evoking anger. Given that disgust leads to dehumanization while anger does not, it is possible that children would display different types of social bias when presented with different forms of outgroup misrepresentation (Rai et al., 2017). This research could also help to tease out whether perceptions of disease specifically fuel outgroup negativity (e.g., Taylor, 2007) or whether any form of negativity directed toward outgroups leads to negative emotions like disgust that might in turn fuel prejudice (e.g., Landy et al., 2022).

## 5. Conclusion

As anti-immigrant propaganda can negatively influence the lives of misrepresented groups in multiple ways, there is a need for more basic research on the mechanisms underlying the detrimental effects of propaganda, as well as applied research on possible interventions. The present research represents an initial, exploratory step in this direction. Overall, our results are encouraging, suggesting that children are not heavily swayed by negative misrepresentations of novel groups in propaganda. However, this is a context in which any influence is concerning, as even small effects could have outsized social impacts. While we did not uncover children's tendencies to internalize biases toward novel social groups when presented with disgusting imagery, it is critical that we move forward in pursuing a better empirical understanding of whether and when propaganda accelerates the development of intergroup biases, so that we can better equip ourselves for creating decelerating counterinfluences.

## CRedit authorship contribution statement

**Anastasiia D. Grigoreva:** Conceptualization, Methodology, Investigation, Formal analysis, Writing – original draft, Funding acquisition. **Joshua Rottman:** Conceptualization, Methodology, Formal analysis, Writing – review & editing, Supervision, Funding acquisition.

## Declaration of competing interest

None.

## Data availability

All research data and analysis code are publicly available on the Open Science Framework, at <https://osf.io/g7q2n/>.

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## Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.actpsy.2022.103790>.

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