## Inhibiting orofacial mimicry affects authenticity perception in vocal emotions

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**Inhibiting Orofacial Mimicry Affects Authenticity Perception in Vocal Emotions**

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**Abstract**

Although emotional mimicry is ubiquitous in social interactions, its mechanisms and roles remain disputed. A prevalent view is that imitating others' expressions facilitates emotional understanding, but the evidence is mixed and almost entirely based on facial emotions. In a preregistered study, we asked whether inhibiting orofacial mimicry affects authenticity perception in vocal emotions. Participants listened to authentic and posed laughs and cries, while holding a pen between the teeth and lips to inhibit orofacial responses ($n$ = 75), or while responding freely without a pen ($n$ = 75). They made authenticity judgments and rated how much they felt the conveyed emotions (emotional contagion). Mimicry inhibition decreased the accuracy of authenticity perception in laughter and crying, and in posed and authentic vocalizations. It did not affect contagion ratings, however, nor performance in a cognitive control task, ruling out the effort of holding the pen as an explanation for the decrements in authenticity perception. Laughter was more contagious than crying, and authentic vocalizations were more contagious than posed ones, regardless of whether mimicry was inhibited or not. These findings confirm the role of mimicry in emotional understanding and extend it to auditory emotions. They also imply that perceived emotional contagion can be unrelated to mimicry.

*Keywords:* mimicry; laughter; crying; emotional authenticity; contagion

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When we see a facial expression, say someone smiling, we tend to spontaneously activate the mechanisms involved in producing that same expression. Motor and somatosensory brain systems are engaged (Moore & Franz, 2017), often resulting in measurable facial movements, so-called emotional mimicry (Rymarczyk et al., 2018). Why we imitate others' behavior in social interactions remains debated. Some theories view mimicry as a tool for affiliation and social regulation (Hess, 2021; Hess & Fischer, 2022), whereas others view it as a sensorimotor simulation mechanism for emotional understanding (Niedenthal, 2007; Wood et al., 2016). Simulation accounts, as well as research on the facial feedback hypothesis, posit that recreating others' emotional expressions generates bodily and affective information that facilitates inferences about the meaning of those expressions (e.g., Coles et al., 2019; Goldman & Sripada, 2005; Marmolejo-Ramos et al., 2020; Niedenthal, 2007; Ross & Atkinson, 2020; Wood et al., 2016).

Research on whether sensorimotor mechanisms support emotional understanding focuses primarily on the perception of facial expressions. Electromyography (EMG) studies indicate that mimicry correlates with emotion recognition performance (Oberman et al., 2007) and with evaluations of valence (Davis et al., 2015), emotional intensity (Schneider et al., 2013), and authenticity (Korb et al., 2014). Causal evidence comes from studies showing that transcranial magnetic stimulation (TMS) applied to sensorimotor networks reduces facial mimicry (Korb et al., 2015) and impairs facial emotion recognition (Pitcher et al., 2008) and smile authenticity perception (Paracampo et al., 2017). Furthermore, injecting botulinum toxin to paralyze facial muscles involved in frowning impairs anger detection in morphed expressions (Bulnes et al., 2019). Mimicry manipulations requiring holding a pen in the mouth also delay the detection of emotional changes in morphed facial expressions (Niedenthal et al., 2001), slow down emotion recognition (Lydon & Nixon, 2014), impair happiness recognition (Borgomaneri et al., 2020; Oberman et al., 2007), and decrease the detection of smile authenticity (Maringer et al., 2011).

Although these data support simulation accounts, the evidence is mixed. For example, in congenital facial palsy (Moebius syndrome), the absence of mimicry does not affect facial emotion recognition (Bogart & Matsumoto, 2010; but see Lomoriello et al., 2023). Furthermore, associations between mimicry and emotion recognition are not always replicated (e.g., Hess & Blairy, 2001). A meta-analysis of EMG evidence (Holland et al., 2020) indicates that mimicry correlates with empathy, but not with facial emotion recognition. Only nine studies were included, however, and the findings were heterogeneous. It remains unresolved, then, whether mimicry provides a reliable route for emotional understanding. Perhaps the effect is small and variable, as observed in a comprehensive meta-analysis of the facial feedback literature (Coles et al., 2019). Relatively large samples might be required, and specifying the contexts in which the effect does (and does not) emerge seems key to explain heterogeneity. Publication bias also limits the interpretability of current evidence (Coles et al., 2019), and therefore adopting research practices that counteract it, such as preregistration (Nosek et al., 2022), is important.

In the present preregistered study, we asked whether reducing orofacial mimicry affects evaluations of emotional vocalizations. Compared to the large body of work on face perception, studies on mimicry in the auditory modality are rare. They are important to clarify previous findings, because modality could be a source of heterogeneity, as well as to develop theorizing on sensorimotor contributions to emotion processing. Is sensorimotor simulation a face-specific or a domain-general mechanism? Is mimicry confined to seen actions or does it extend to unseen ones? The notion of mirroring facial movements is intuitive, but although we do not ‘see’ voices, they reflect actions that can be mirrored. Vocal sounds are inherently linked to orofacial movements, such that auditory-motor mapping is central for audition and speech interactions (Lima et al., 2016; Scott et al., 2009). Laughter involves the mouth and the face, for example, and listening to laughs activates motor and somatosensory systems that produce the corresponding orofacial gestures (Lima et al., 2015; McGettigan et al., 2015; O'Nions et al., 2017; Warren et al., 2006). Audio-visual integration of motor and affective systems is therefore to be expected (Arias et al., 2018, 2020, 2021; Hawk et al., 2012).

Sensorimotor activity correlates with vocal emotion perception. Activity in the corrugator supercilii and orbicularis oculi muscles during listening to authentic and posed laughs predicts subsequent authenticity ratings (Lima et al., 2021). Individuals showing stronger sensorimotor brain responses to laughter (McGettigan et al., 2015) and higher trait resonance with others' emotions (trait contagion; Neves et al., 2018) are better at perceiving laughter authenticity. Additionally, an fMRI study showed that a stronger engagement of sensorimotor systems correlates with improved recognition of emotional speech (Correia et al., 2019). Crucially, however, these correlations preclude causal inferences. It cannot be excluded that increased sensorimotor activity is a consequence, not a cause, of improved vocal emotional processing. Particularly because causal evidence is scant and mixed. In one TMS study, suppressing activity in somatosensory and premotor cortices disrupted emotion recognition in vocalizations (Banissy et al., 2010). One pen-holding study showed a similar effect (Hawk et al., 2012), but other did not: reducing mimicry made the vocalizations sound more positive, but left emotion recognition accuracy intact (Wołoszyn et al., 2022).

Here, we reduced orofacial responses using a pen-holding manipulation (e.g., Maringer et al., 2011; Niedenthal et al., 2001, 2009), and we focused on participants' ability to tell authentic from posed emotional vocalizations. Authentic laughter and crying differ from their posed counterparts acoustically, perceptually, and in their neural correlates (Anikin & Lima, 2018; Bryant & Aktipis, 2014; Conde et al., 2022; Lima et al., 2021; Pinheiro et al., 2021). If reducing sensorimotor responses causes decrements in authenticity detection in vocalizations, that would imply that mimicry contributes to emotional understanding across modalities, and inform current debates on why we imitate others in interactions.

Reported associations between sensorimotor mechanisms and authenticity perception in vocalizations (Lima et al., 2021; McGettigan et al., 2015; Neves et al., 2018) motivated our main prediction: compared to participants who can mimic freely, those holding a pen between the teeth and lips will be worse at inferring authenticity in laughter and crying. Because sensorimotor responses are stronger for positive than negative vocalizations (Lima et al., 2021; Warren et al., 2006), mimicry inhibition could affect laughter more than crying perception. Importantly, we included a non-emotional control task (abstract reasoning). If potential pen-holding effects reflect mimicry inhibition as intended, and not unspecific effort related to holding a pen (Rychlowska et al., 2014), they should not be observed for this task. Two more exploratory aspects were also considered. First, participants also rated emotional contagion for each vocalization. Because mimicry is sometimes conflated with contagion (Hess & Fischer, 2022), and facial expressions can influence subjective emotional experiences (Coles et al., 2022), a tentative prediction is that inhibiting mimicry reduces contagion responses. Second, we explored whether scores on questionnaires of trait contagion and empathy correlate with authenticity and contagion evaluations (Neves et al., 2018), and whether such correlations vary across mimicry conditions. The study preregistration is available at https://osf.io/gv76z.

# Methods

## Participants

We tested 150 adults, 101 women and 49 men. They were 26.97 years of age on average (*SD* = 8.68, range = 18-56) and had 14.60 years of education (*SD* = 2.55, range = 12–26). All participants reported normal hearing, normal or corrected-to-normal vision, no history of neurological or psychiatric disorders, and were Portuguese native speakers. They were recruited on campus or from local communities and received partial course credits or a voucher to take part. Written informed consent was obtained from all participants.

Because there is no standard procedure to estimate sample size for mixed-effects models, we decided to recruit and test as many participants as possible within a reasonable time frame (ca. 6 months). We used previous related work as a general reference (e.g., $N$ = 119 in Neves et al., 2018; $N$ = 100 in Lima et al., 2021), and decided to aim for a somewhat higher $N$ because of the online testing format, the exploratory components of the study, and the fact that we have a between-subjects manipulation (unlike Neves et al. and Lima et al.).

**Materials**

**Voices.** The same vocal stimuli were used for authenticity and contagion evaluations, and they were auditory-only stimuli. They consisted of 120 nonverbal vocalizations, 30 unique ones per each of the four conditions: posed laughter, authentic laughter, posed crying, and authentic crying (half of the stimuli in each condition were produced by women). Vocalizations were selected from a larger stimulus set that has been used in behavioral and neuroimaging studies (Lima et al., 2021; O'Nions et al., 2017; Pinheiro et al., 2021). They were generated within a sound-proof anechoic chamber by six speakers (three women), aged 24 to 48 years, who had experience recording vocal materials but were not professional actors. To elicit authentic laughter, an amusement induction procedure was used in a social-interactive setting: the speakers watched short clips that they had identified as amusing beforehand, and the experimenters interacted with them during the recording session to ensure the naturalness of the laughs. For authentic crying, speakers recalled upsetting/sad past life events to self-induce a genuine experience of sadness. All of them confirmed that they were able to cry spontaneously and reported having felt genuine sadness throughout the recording. The same speakers were asked to produce posed laughter and posed crying, and to try to make them sound credible and natural, as per the typical procedure used to record acted vocal stimuli (Lima et al., 2013). The duration of the stimuli was 2450 ms on average (*SD* = 410), and the best possible match was ensured between the duration of authentic (*M* = 2560 ms) and posed ones (*M* = 2330 ms).

**Abstract Reasoning.** The Matrix Reasoning Item Bank (MaRs-IB; (Chierchia et al., 2019) is an 8-min nonverbal reasoning test modeled after Raven's matrices tests. On each trial, participants are shown a 3X3 matrix, corresponding to eight cells containing abstract shapes, and a ninth empty cell. The task consists of selecting one of four options for the empty cell, which requires identifying the rules that govern the pattern of the other eight cells. Participants are told that they have up to 30 s to complete each trial, after which the next one is presented. The test includes 80 matrices, and if participants complete all of them in less than eight min, they are presented again until time is up. Following the test's developers, performance corresponds to the proportion of correct responses (i.e., number of correct responses/number of completed trials), computed for each participant after excluding responses provided in less than 250 ms. Proportions were logit-transformed for statistical analyses.

**Questionnaires.** The Emotional Contagion scale, ECS (Doherty, 1997; Rueff-lopes & Caetano, 2012), includes 15 items and assesses the tendency to resonate with others' emotions as a trait. Items are rated on a five-point scale, from 1 ('never') to 5 ('always'), and cover five emotions: happiness (e.g., 'When someone smiles warmly at me, I smile back and feel warm inside'); love (e.g., 'I melt when the one I love holds me close'); fear (e.g., 'I notice myself getting tense when I'm around people who are stressed out'); anger (e.g., 'It irritates me to be around angry people') and sadness (e.g., 'I cry at sad movies'). Item scores are averaged to produce a total individual score.

The Questionnaire of Cognitive and Affective Empathy, QCAE (Queirós et al., 2018; Reniers et al., 2011), includes 31 items: 19 focused on cognitive and 11 on affective empathy.

Examples of items include ‘I can tell if someone is masking their true emotion, or ‘Before criticizing somebody, I try to imagine how I would feel if I was in their place’. Items are rated on a four-point scale from 1 (‘strongly disagree’) to 4 (‘strongly agree’), and scores are summed to produce cognitive and affective trait empathy scores. Only the cognitive subscale was used because the affective one measures resonance with others’ emotions, which the ECS already covers.

**Procedure**

The tasks and questionnaires were implemented in Gorilla Experiment Builder (Anwyl-Irvine et al., 2020), an online platform for psychological research. Participants took part in one online session lasting around 50 minutes, supervised by an experimenter via Zoom (www.zoom.us). They were in an audiovisual virtual meeting throughout the session, and the experimenter ensured that they followed the instructions, could ask questions, were in a quiet place, and wore headphones during the voice task.

Participants first provided informed consent and completed a background questionnaire that asked for demographic information. They were then assigned randomly to the *inhibited* or *free* face conditions, such that there were 75 participants in each condition. Those in the inhibited condition were asked to keep their face as still as possible and to hold a pen sideways in the mouth while evaluating the vocalizations, lightly using their teeth and lips (Borgomaneri et al., 2020; Niedenthal et al., 2001, 2009; Oberman et al., 2007). EMG evidence confirms that this technique affects orofacial muscle activity (Davis et al., 2015, 2017; Han et al., 2016; Oberman et al., 2007) and results in reduced mimicry (Davis et al., 2017). Those in the free condition did not receive any instructions regarding orofacial movements during the voice tasks. They were instead asked to hold the pen in the mouth during the abstract reasoning test. Thus, all participants underwent the same pen-holding manipulation during the experiment, but on different moments, and none during the voice tasks *and* reasoning test. All were told that the pen-holding manipulation was meant to test how people perform more than one task simultaneously.

Authenticity and contagion evaluations were provided in different blocks, as separate tasks, completed at the start and end of the experimental session in a counterbalanced order. The abstract reasoning test and the questionnaires were completed between the two voice tasks (see Figure 1C). In the authenticity task, participants made two-alternative forced choice judgments, classifying each vocalization as posed or authentic. In the contagion task, they rated their emotional response to each vocalization on a seven-point scale from 1 ('it does not make me feel a similar emotion') to 7 ('it makes me feel a similar emotion'). The number of trials (120) and their structure was similar across the voice tasks: fixation cross for 2 s, stimulus presentation for ≈3 s, and prompt for participants' response (see Figure 1). The next trial started right after the response, and no feedback was given. The tasks were preceded by four practice trials, and participants could take a break half-way through (i.e., after 60 trials). Each task lasted around 14 minutes.

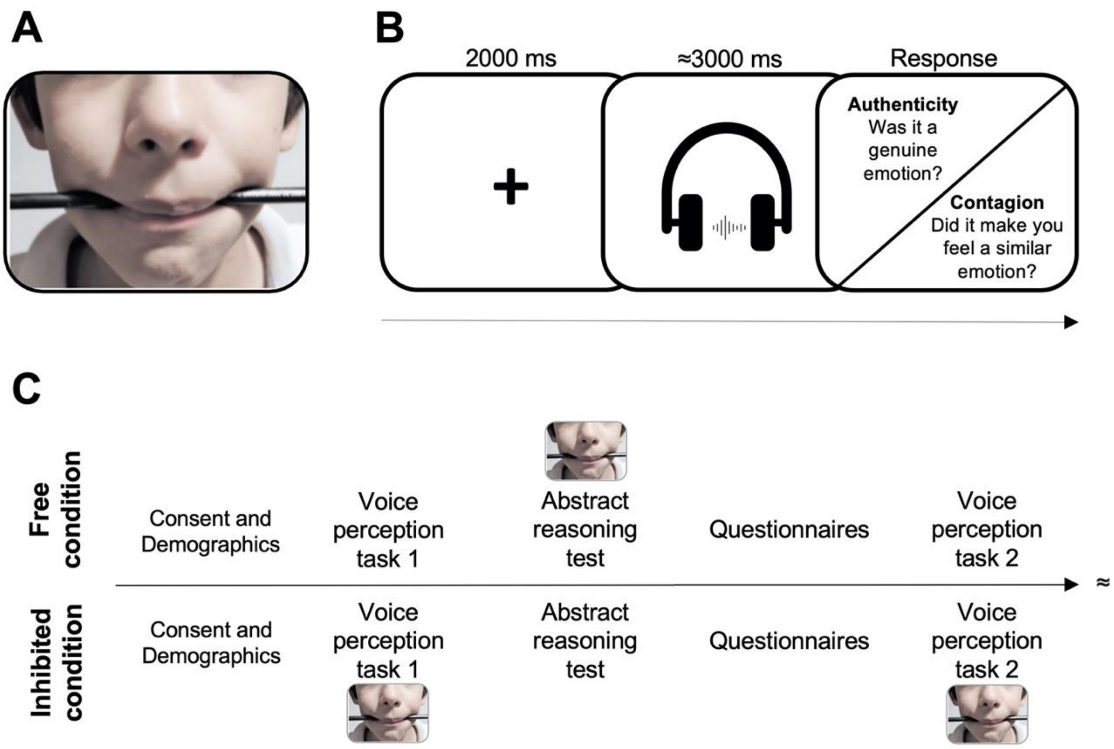


*Figure 1.* Illustration of the pen-holding manipulation (A), structure of the experimental trials in the voice perception tasks (B), and structure of the experimental session in the free and inhibited conditions (C). Authenticity and contagion evaluations were provided separately, in two voice perception tasks, whose order was counterbalanced across participants.

## Data Analysis

Statistical analyses were performed using mixed-effects modelling as implemented in the R package *lme4* (Bates et al., 2015). The fixed effects predictors were sum-coded (-1, 1). Separate models were used for the authenticity and contagion tasks, and significance was tested using the Satterthwaite degrees of freedom approach, as implemented in the R package *lmerTest* (Kuznetsova et al., 2017). If a significant interaction was obtained, we performed follow-up analyses using the *emmeans* package (Lenth et al., 2021), and selected the holm method for controlling for family wise error rate.

For the authenticity task, we modelled accuracy data (correct/incorrect responses) with mixed-effects logistic regression. For the contagion task, we modelled 1-7 ratings with mixed-effects ordinal logistic regression. The full models included face condition (inhibited, free), vocalization type (laughter, crying), authenticity (posed, authentic), and their interaction as fixed factors, as well as by-participant and by-item random intercepts. The models also included by-participant random slopes for vocalization type, authenticity, and the interaction term. The model of authenticity data was of the form:

*AuthenticityEvaluation ~ FaceCondition*VocalizationType*Authenticity + (1+VocalizationType*Authenticity |Participant) +(1|Stimuli)*

For the contagion data, the model was similar, but with 1-7 ratings instead of binary responses.

Demographic data (sex and age) and questionnaire scores (ECS and QCAE scores) were added to these models as covariates to check if they influenced the results. Age and questionnaire scores were mean-centered.

An independent samples t-test examined whether inhibiting facial responses affected the proportion of correct responses in the cognitive test. We did not use the mixed-effects models planned at the preregistration phase, considering evidence that estimating random effects (e.g., random intercepts and slopes) is problematic when there is only a single observation per participant (Barr et al., 2013).

When null results were obtained, we complemented frequentist statistics with Bayesian inference for a substantiated interpretation. Specifically, Bayes Factors (BFs) were used to determine the likelihood of the null compared to the alternative hypothesis, where a $BF_{10} < .333$ provides substantial or stronger evidence for the null hypothesis, according to Jeffreys' guidelines (Jarosz & Wiley, 2014; van Doorn et al., 2021).

**Transparency and Openness**

The data, materials, and code used for analyses are available at https://osf.io/rhv5x/?view_only=c13c3e9c32f144a490063d74c12e244e. The study preregistration is available at https://osf.io/gv76z. We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study.

## Results

Figure 2 shows descriptive statistics for authenticity and contagion evaluations (see also Table S1). Authenticity perception accuracy was 68.11% on average, and performance was above chance across conditions. Contagion ratings were 3.30 on average (scale 1-7), and authentic laughs (4.46) were particularly contagious (all other conditions, ≤ 3.21).

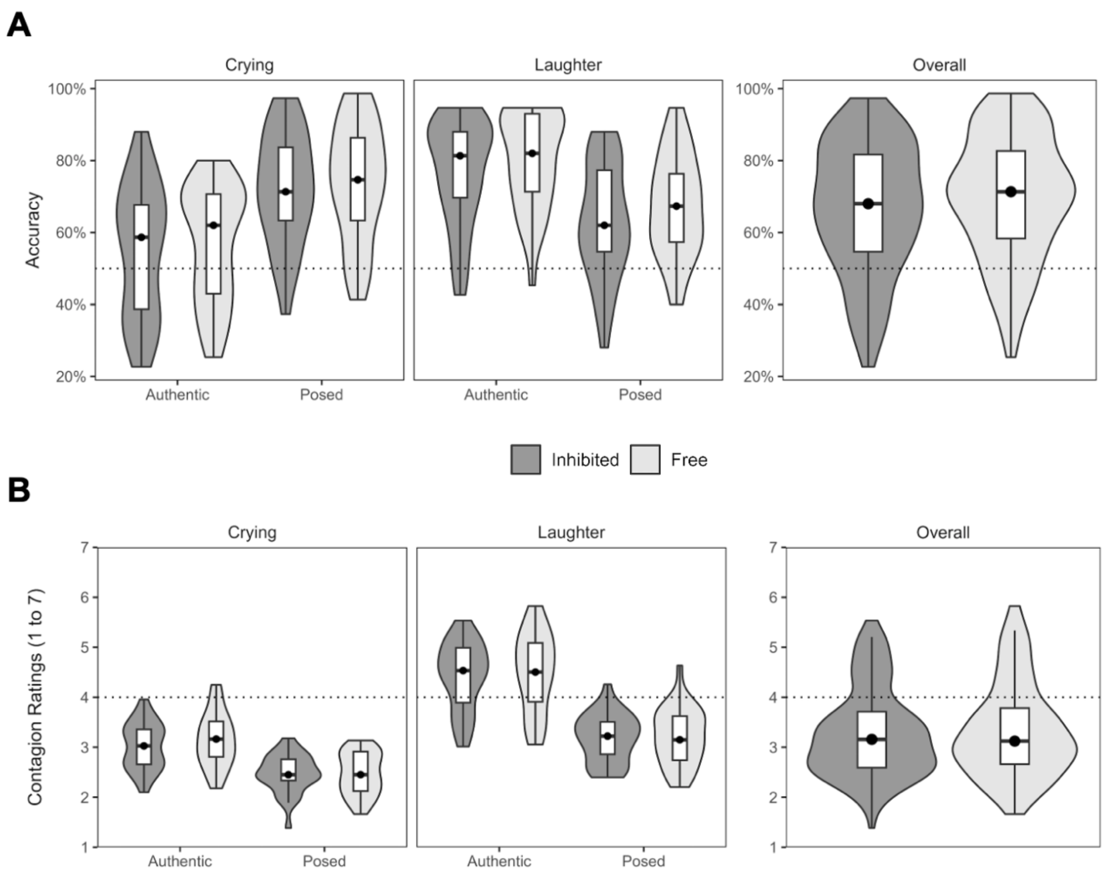


*Figure 2.* Accuracy of authenticity perception (A) and contagion ratings (B) for each condition (Overall corresponds to the average of all conditions of vocalizations). The middle horizontal lines show the medians, the boxes indicate the 25th and 75th percentiles,

and the violin plots show the distribution of mean observed values. The dashed lines indicate chance level in A (50%) and the mid-point of the scale in B (4)

**Authenticity.** Our main analysis confirmed that inhibiting orofacial responses decreased the accuracy of authenticity perception (66.66% vs. 69.57% in the inhibited and free conditions, respectively), as shown by a main effect of face condition, *estimate* = -0.09 (odds ratio, OR = 0.91), *SE* = 0.04, $z = -2.43$, $p = .015$, 95% CI [-0.16, -0.02]. This effect did not interact with vocalization type, *estimate* = 0.04 (OR = 1.04), *SE* = 0.02, $z = 1.59$, $p = .111$, 95% CI [-0.01, 0.09], or authenticity, *estimate* = 0.00 (OR = 1.00), *SE* = 0.05, $z = 0.03$, $p = .980$, 95% CI [-0.10, 0.11]. There was also no three-way interaction between face condition, vocalization type, and authenticity, *estimate* = 0.00 (OR = 1.00), *SE* = 0.04, $z = -0.10$, $p = .922$, 95% CI [-0.08, 0.08].

There was a main effect of vocalization type, *estimate* = -0.21 (OR = 0.81), *SE* = 0.08, $z = -2.49$, $p = .013$, 95% CI [-0.37, -0.04], and an interaction between vocalization type and authenticity, *estimate* = -0.45 (OR = 0.63), *SE* = 0.09, $z = -5.05$, $p < .001$, 95% CI [-0.63, -0.28]. Replicating previous findings (Pinheiro et al., 2021), authenticity perception was more accurate for laughter (78.44%) than for crying (55.89%) when vocalizations were authentic, *estimate* = -1.33 (OR = 0.27), *SE* = 0.24, $z = -5.52$, $p < .001$, 95% CI [-1.80, -0.86], and more accurate for crying (72.96%) than for laughter (65.16%) when they were posed, *estimate* = 0.49 (OR = 1.64), *SE* = 0.25, $z = 1.96$, $p = .050$ 95% CI [0.00, 0.99].

Effects of listener's age and sex did not interact with any of our variables of interest (*p*s > .070). We found a main effect of age, though, with older adults being more accurate than younger ones, *estimate* = 0.01 (OR = 1.01), *SE* = 0.00, $z = 2.44$, $p = .015$, 95% CI [1.00, 1.02]. Sex effects were not significant, *estimate* = 0.05 (OR = 1.06), *SE* = 0.04, $z = 1.44$, $p = .150$, 95% CI [-0.02, 0.13].

**Contagion.** Inhibiting orofacial responses did not affect contagion ratings, as shown by a non-significant main effect of face condition, *estimate* = -0.07 (OR = 0.93), *SE* = 0.14, $z$ = -0.50, $p$ = .616, 95% CI [-0.34, 0.20]. Face condition also did not interact with vocalization type, *estimate* = -0.02 (OR = 0.99), *SE* = 0.05, $z$ = -0.27, $p$ = .781, 95% CI [-0.12, 0.09], or authenticity, *estimate* = -0.02 (OR = 0.98), *SE* = 0.04, $z$ = -0.59, $p$ = .552, 95% CI [-0.10, 0.05], and the three-way interaction between face condition, vocalization type, and authenticity was also not significant, *estimate* = -0.02 (OR = 0.98), *SE* = 0.02, $z$ = -1.01, $p$ = .313, 95% CI [-0.07, 0.02]. Bayesian analyses provided decisive evidence for the null hypothesis, $BF_{10} < 0.01$ (i.e., for a model without vs. with a face condition effect).

As expected (Lima et al., 2021; Neves et al., 2018), laughter was more contagious (3.83) than crying (2.79), *estimate* = -0.81 (OR = 0.44), *SE* = 0.09, $z$ = -8.92, $p < .001$, 95% CI [-0.99, -0.63], and authentic vocalizations (3.78) more contagious than posed ones (2.84), *estimate* = 0.71 (OR = 2.03), *SE* = 0.08, $z$ = 8.55, $p < .001$, 95% CI [0.54, 0.87]. Effects of authenticity on contagion were larger for laughter (authentic: 4.46, posed: 3.21) than for crying (authentic: 3.11, posed: 2.48), as indicted by the interaction between vocalization type and authenticity, *estimate* = - 0.23 (OR = 0.79), *SE* = 0.08, $z$ = -3.04, $p$ = .002, 95% CI [-0.38, -0.08].

Effects of listener's age and sex did not interact with any of our variables of interest (*p*s > .107), and the main effect of age was not significant, *estimate* = -0.00 (OR = 0.99), *SE* = 0.01, $z$ = -0.33, $p$ = .740, 95% CI [-0.04, 0.03]. The only finding was that women reported higher contagion for authentic vocalizations than men, *estimate* = 0.13 (OR = 1.13), *SE* = 0.03, $z$ = 3.16, $p$ = .002 95% CI [0.05, 0.20].

**Abstract Reasoning.** Accuracy in the reasoning test was similar in the inhibited (59.64%) and free (61.00%) conditions, $t(145.85) = 0.61$, $p$ = .541, equal variances assumed, Levene's test, $p = .520$. Bayesian analyses provided substantial evidence for the null hypothesis, $BF_{10} = 0.20$.

Face condition also did not affect the absolute number of correct responses, i.e., regardless of how many trials were completed. Averages were 23.77 and 24.96 in the inhibited and free conditions, respectively, $t(148) = 1.13$, $p = .258$, equal variances assumed, Levene's test, $p = .277$, $BF_{10} = 0.32$.

**Questionnaires.** Trait contagion (ECS) scores were similar to previous findings (Table S1; Doherty, 1997). The effect of trait contagion on authenticity perception was positive as expected, but not significant, *estimate* = 0.10 (OR = 1.10), $SE = 0.08$, $z = 1.22$, $p = .221$, 95% CI [-0.06, 0.26]. Similarly, trait contagion did not interact with any of the variables of interest, $ps > .162$. In contrast, ECS scores predicted higher contagion responses to the vocalizations, *estimate* = 1.09 (OR = 2.98), $SE = 0.28$ $z = 3.88$, $p < .001$, 95% CI [0.54, 1.64]. This effect did not interact with any other variable, $ps > .063$, except for sex, *estimate* = -0.77 (OR = 0.46), $SE = 0.31$, $z = -2{,}49$, $p = .012$, 95% CI [-1.37, -0.16]. ECS predicted contagion responses for men, *estimate* = 2.25 (OR = 9.47), $SE = .048$, $z = 4.63$, $p < .001$, 95% CI [1.28, 3.16], but only marginally for women, *estimate* = 0.71 (OR = 2.04), $SE = 0.39$, $z = 1.87$, $p = .061$, 95% CI [0.00, 1.49].

Average cognitive empathy scores (QCAE) were also similar to previous findings (e.g., Reniers et al., 2011; Vilaverde et al., 2020). Associations with authenticity perception depended on face condition and stimulus authenticity, as indicated by a three-way interaction between empathy, face condition, and authenticity, *estimate* = 0.02 (OR = 1.02), $SE = 0.00$, $z = 2.86$, $p = .004$, 95% CI [0.01, 0.04] (the main effect and other interactions were not significant, $ps > .158$). In the inhibited condition, higher cognitive empathy predicted improved authenticity detection in authentic vocalizations, *estimate* = 0.03 (OR = 1.03), $SE = 0.01$, $z = 2.00$, $p = .045$, 95% CI [0.00, 0.05] (posed vocalizations, $p = .758$). In the free condition, higher empathy predicted improved authenticity detection in posed vocalizations, *estimate* = 0.03 (OR = 1.03), *SE* = 0.01, *z* = 2.47, *p* = .014, 95% CI [0.00, 0.05] (authentic vocalizations, *p* = .059). Higher empathy scores also predicted higher contagion responses to the vocalizations, but only in the inhibited condition, *estimate* = 0.07 (OR = 1.08), *SE* = 0.03, *z* = 2.55, *p* = .010, 95% CI [0.02, 0.13] (free condition, *p* = .600; interaction between empathy and face condition, *estimate* = 0.04 (OR = 1.04), *SE* = 0.01, *z* = 2.26, *p* = .024, 95% CI [0.01, 0.08]; the main effect and other interactions were not significant, *p*s > .104).

**Unplanned (non-preregistered) analyses.** Because the authenticity task was longer than the abstract reasoning task (14 vs. 8 minutes), increased fatigue could explain why face condition affected authenticity perception but not abstract reasoning. Nevertheless, when stimulus order (one to 120) was included in the model for the authenticity task, the main effect of order was non-significant, *estimate* = -0.00 (OR = 0.99), *SE* = 5.33, *z* = -0.35, *p* = .723, 95% CI [-0.001, 0.001], as was the interaction between order and face condition, *estimate* = -0.00 (OR = 1.00), *SE* = 5.32, *z* = -0.15, *p* = .881, 95% CI [-0.001, 0.001]. In other words, performance remained stable throughout the task, suggesting that fatigue did not play a role.

Because our models are based on unaggregated data from individual trials, they do not account directly for possible response biases (e.g., a disproportionate tendency to classify stimuli as authentic). An additional analysis was conducted using unbiased hit rates, or Hu (Wagner, 1993), a measure that considers both correct identifications and the total number of times that each response category is used (i.e., correct and incorrect uses). Hu scores were calculated for authentic and posed stimuli for each participant, arcsine square-root transformed, and submitted to a mixed-design analysis of variance (ANOVA), with authenticity as a repeated measure, and face condition as a between-subjects factor. The main effect of face condition was replicated, $F(1,148) = 4.96$, $p = .027$, $\eta_p^2 = .032$, and it did not interact with authenticity, $p$ = .978. Hu scores were 0.45 and 0.49 for the inhibited and free conditions, respectively.

## Discussion

We asked whether inhibiting orofacial mimicry affected evaluations of emotional vocalizations. Compared to participants who could mimic, those holding a pen between their teeth and lips were worse at perceiving laughter and crying authenticity, despite experiencing intact contagion. The pen-holding manipulation did not affect abstract reasoning, suggesting that it inhibited mimicry without having cognitive costs. Trait emotional contagion predicted contagion responses to the vocalizations, but not authenticity perception. Cognitive empathy predicted contagion responses and authenticity perception, but only in some conditions.

The main finding was that inhibiting mimicry decreased participants' ability to tell authentic from posed vocalizations. The effect was observed both when the analysis was based on data from individual trials, as well as when accuracy rates were corrected for possible response biases. Previous studies documented that listening to vocalizations and emotional speech evokes sensorimotor responses (Arias et al., 2018; Lima et al., 2015, 2021; McGettigan et al., 2015; Warren et al., 2006), even in congenitally blind individuals (Arias et al., 2021). It was also known that such responses predict emotional evaluations (Lima et al., 2021; McGettigan et al., 2015). But there was no evidence for a causal implication of mimicry in authenticity perception, which is crucial for arguments that mimicry supports emotional understanding, and that emotion recognition involves simulation (Niedenthal, 2007; Ross & Atkinson, 2020; Wood et al., 2016). The effect was small, in line with previous related research (e.g., Borgomaneri et al., 2020; Neves et al., 2018) and facial feedback effects in general (Coles et al., 2019). It was also similar across vocalization types, and not stronger for laughter as predicted. Thus, although positive vocalizations evoke larger sensorimotor responses (Lima et al., 2021; Warren et al., 2006), contributions of mimicry to emotional understanding are independent of valence. Indeed, Banissy et al. (2010) and Hawk et al. (2012) found that inhibiting sensorimotor activity affected emotion recognition in positive and negative vocalizations similarly.

A stronger effect for laughter could additionally be expected because our pen-hold manipulation inhibits smiling, but not muscles associated with negative emotions (e.g., corrugator, Lundqvist & Dimberg, 1995). Nevertheless, while in facial expressions distinct muscles relate to distinct valences, in vocalizations both laughter and crying require movements of the mouth and articulators, which could explain the lack of emotion-specific effects.

How can our findings be interpreted? Effort involved in holding a pen is an unlikely explanation, because contagion and cognitive performance remained intact. Authenticity perception accuracy also remained stable throughout the experiment, regardless of whether participants were holding a pen or not, further excluding fatigue as an alternative explanation. Instead, activating motor representations related to sounds arguably enables sensory and affective predictions that optimize perception. When sensorimotor activity is prevented, perceptual inferences are suboptimal. This explanation is aligned with models of auditory perception (Correia et al., 2019; Lima et al., 2016) and simulation accounts of facial emotion recognition (Wood et al., 2016). Our findings suggest that simulation is a supra-modal mechanism, and extend emerging evidence that mimicry is not restricted to observed movements (e.g., Arias et al., 2021; Lima et al., 2021). We do not 'see' voices, but they are paired with faces in naturalistic interactions, and vocalizations require orofacial movements. Additionally, we mimic partially occluded faces (Davis et al., 2022), and patients with cortical blindness show facial responses to non-consciously perceived facial and body expressions (Tamietto et al., 2009; Tamietto & de Gelder, 2010), which is further evidence of imitation of unseen actions.

Inhibiting orofacial activity could decrease emotional contagion, based on views that conflate mimicry with contagion, and that mimicry supports affiliation (Drimalla et al., 2019; Hess, 2021). We did not observe such decrements, though. Perhaps affiliative behaviors emerge primarily in social interactions, not in response to isolated sounds in somewhat artificial experimental contexts. Indeed, contagion ratings were generally low, although sounds like laughter are established to be highly contagious in everyday interactions (Scott et al., 2014). Our task can index individual differences in emotional resonance (O'Nions et al., 2017), and is similar to tests of emotional empathy that correlate with mimicry (Drimalla et al., 2019), but is has limited ecological validity and it captures primarily conscious aspects of contagion. Measures of contagion other than self-reports, such as electrodermal responses (e.g., Lima et al., 2021), could provide additional sensitivity. Nevertheless, dissociations between mimicry and emotional contagion have been observed before (Hess & Blairy, 2001; Lundqvist & Dimberg, 1995), highlighting the need to distinguish them. Mimicry is a motor behavior and contagion a feeling state (Hess & Fischer, 2022). Moreover, the meta-analysis by Coles et al. (2019) indicates that facial feedback effects might be larger for affective judgments compared to emotional experience, even though publication bias cannot be excluded as an explanation for this difference.

Higher trait contagion predicted stronger contagion responses to the vocalizations, which seems intuitive, but not improved authenticity detection, unlike reported by (Neves et al., 2018). The association between trait contagion and authenticity detection might be context-dependent. The effect was also weak in a study of laughter and crying like the current one (Lima et al., 2021), whereas Neves and colleagues examined only laughter. Moreover, our task required binary judgments of authenticity, whereas previous studies required fine-grained ratings in seven-point scales. Cognitive empathy, on the other hand, had selective associations with contagion and authenticity responses. It predicted stronger contagion responses in the inhibited condition, possibly because mentalizing provides a route for contagion when sensorimotor mechanisms are restricted (Lamm et al., 2011). Associations with authenticity detection were observed for authentic vocalizations in the inhibited condition, and for posed vocalizations in the free condition. That cognitive empathy predicts authenticity detection in posed vocalizations could be expected, because posed laughter engages mentalizing systems more strongly than authentic laughter (McGettigan et al., 2015). Not finding the same association in the inhibited condition, where it instead emerged for authentic vocalizations, was unexpected and should be confirmed.

A limitation of our study is that we used a single strategy to inhibit mimicry, such that future work will need to extend our findings across different methods (e.g., Wood et al., 2015). Although the pen-holding manipulation is known to reduce mimicry (Davis et al., 2017) and is optimal for online testing, we cannot be certain that mimicry was fully inhibited because we could not measure orofacial activity. Perhaps some level of mimicry still occurred, and whether a full inhibition would produce larger decrements in performance remains to be determined. Moreover, our task did not emphasize speed, just accuracy. Future studies could emphasize both, considering evidence that speed might be particularly sensitive to sensorimotor manipulations (Banissy et al., 2010). An additional limitation is that our control task (abstract reasoning) differed from the experimental one in aspects such as type of stimulus and duration. We did not use an auditory control task because sounds can access the motor system even in non-emotional tasks (e.g., Liebenthal & Möttönen, 2018; Lima et al., 2016), and potential pen-holding effects could then reflect interference with sensorimotor activity, and not effort as intended. We also excluded the possibility that the longer duration of the authenticity task was associated with increased fatigue. Nevertheless, a closer match between the experimental and the control tasks, namely in response format and number of trials, will be useful to optimize interpretability in future studies.

To conclude, our study shows that inhibiting orofacial mimicry disrupts the ability to tell authentic from posed emotional vocalizations. This finding suggests a novel causal link between emotional mimicry and authenticity detection in the auditory modality, with implications for debates on the roles of mimicry and on the mechanisms underlying emotion recognition.

## Author Note

**Openness.** The data, materials, and code used for analyses are available at https://osf.io/rhv5x/?view_only=c13c3e9c32f144a490063d74c12e244e. The study preregistration is available at https://osf.io/gv76z.

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**Conflicts of interest.** The authors have no competing interests to declare that are relevant to the content of this article.

**Ethics approval.** This study was performed in line with the principles of the Declaration of Helsinki. The questionnaires and methodology for the study were approved by the local ethics committee at Iscte, University Institute of Lisbon.

**Consent to participate.** Participation was voluntary, and informed consent were secured from all participants.

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# Inhibiting Orofacial Mimicry Affects Authenticity Perception in Vocal Emotions

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**Abstract**

Although emotional mimicry is ubiquitous in social interactions, its mechanisms and roles remain disputed. A prevalent view is that imitating others' expressions facilitates emotional understanding, but the evidence is mixed and almost entirely based on facial emotions. In a preregistered study, we asked whether inhibiting orofacial mimicry affects authenticity perception in vocal emotions. Participants listened to authentic and posed laughs and cries, while holding a pen between the teeth and lips to inhibit orofacial responses (*n* = 75), or while responding freely without a pen (*n* = 75). They made authenticity judgments and rated how much they felt the conveyed emotions (emotional contagion). Mimicry inhibition decreased the accuracy of authenticity perception in laughter and crying, and in posed and authentic vocalizations. It did not affect contagion ratings, however, nor performance in a cognitive control task, ruling out the effort of holding the pen as an explanation for the decrements in authenticity perception. Laughter was more contagious than crying, and authentic vocalizations were more contagious than posed ones, regardless of whether mimicry was inhibited or not. These findings confirm the role of mimicry in emotional understanding and extend it to auditory emotions. They also imply that perceived emotional contagion can be unrelated to mimicry.

*Keywords:* mimicry; laughter; crying; emotional authenticity; contagion

**Inhibiting Orofacial Mimicry Affects Authenticity Perception in Vocal Emotions**

When we see a facial expression, say someone smiling, we tend to spontaneously activate the mechanisms involved in producing that same expression. Motor and somatosensory brain systems are engaged (Moore & Franz, 2017), often resulting in measurable facial movements, so-called emotional mimicry (Rymarczyk et al., 2018). Why we imitate others' behavior in social interactions remains debated. Some theories view mimicry as a tool for affiliation and social regulation (Hess, 2021; Hess & Fischer, 2022), whereas others view it as a sensorimotor simulation mechanism for emotional understanding (Niedenthal, 2007; Wood et al., 2016). Simulation accounts, as well as research on the facial feedback hypothesis, posit that recreating others' emotional expressions generates bodily and affective information that facilitates inferences about the meaning of those expressions (e.g., Coles et al., 2019; Goldman & Sripada, 2005; Marmolejo-Ramos et al., 2020; Niedenthal, 2007; Ross & Atkinson, 2020; Wood et al., 2016).

Research on whether sensorimotor mechanisms support emotional understanding focuses primarily on the perception of facial expressions. Electromyography (EMG) studies indicate that mimicry correlates with emotion recognition performance (Oberman et al., 2007) and with evaluations of valence (Davis et al., 2015), emotional intensity (Schneider et al., 2013), and authenticity (Korb et al., 2014). Causal evidence comes from studies showing that transcranial magnetic stimulation (TMS) applied to sensorimotor networks reduces facial mimicry (Korb et al., 2015) and impairs facial emotion recognition (Pitcher et al., 2008) and smile authenticity perception (Paracampo et al., 2017). Furthermore, injecting botulinum toxin to paralyze facial muscles involved in frowning impairs anger detection in morphed expressions (Bulnes et al., 2019). Mimicry manipulations requiring holding a pen in the mouth also delay the detection of emotional changes in morphed facial expressions (Niedenthal et al., 2001), slow down emotion recognition (Lydon & Nixon, 2014), impair happiness recognition (Borgomaneri et al., 2020; Oberman et al., 2007), and decrease the detection of smile authenticity (Maringer et al., 2011).

Although these data support simulation accounts, the evidence is mixed. For example, in congenital facial palsy (Moebius syndrome), the absence of mimicry does not affect facial emotion recognition (Bogart & Matsumoto, 2010; but see Lomoriello et al., 2023). Furthermore, associations between mimicry and emotion recognition are not always replicated (e.g., Hess & Blairy, 2001). A meta-analysis of EMG evidence (Holland et al., 2020) indicates that mimicry correlates with empathy, but not with facial emotion recognition. Only nine studies were included, however, and the findings were heterogeneous. It remains unresolved, then, whether mimicry provides a reliable route for emotional understanding. Perhaps the effect is small and variable, as observed in a comprehensive meta-analysis of the facial feedback literature (Coles et al., 2019). Relatively large samples might be required, and specifying the contexts in which the effect does (and does not) emerge seems key to explain heterogeneity. Publication bias also limits the interpretability of current evidence (Coles et al., 2019), and therefore adopting research practices that counteract it, such as preregistration (Nosek et al., 2022), is important.

In the present preregistered study, we asked whether reducing orofacial mimicry affects evaluations of emotional vocalizations. Compared to the large body of work on face perception, studies on mimicry in the auditory modality are rare. They are important to clarify previous findings, because modality could be a source of heterogeneity, as well as to develop theorizing on sensorimotor contributions to emotion processing. Is sensorimotor simulation a face-specific or a domain-general mechanism? Is mimicry confined to seen actions or does it extend to unseen ones? The notion of mirroring facial movements is intuitive, but although we do not 'see' voices, they reflect actions that can be mirrored. Vocal sounds are inherently linked to orofacial movements, such that auditory-motor mapping is central for audition and speech interactions (Lima et al., 2016; Scott et al., 2009). Laughter involves the mouth and the face, for example, and listening to laughs activates motor and somatosensory systems that produce the corresponding orofacial gestures (Lima et al., 2015; McGettigan et al., 2015; O'Nions et al., 2017; Warren et al., 2006). Audio-visual integration of motor and affective systems is therefore to be expected (Arias et al., 2018, 2020, 2021; Hawk et al., 2012).

Sensorimotor activity correlates with vocal emotion perception. Activity in the corrugator supercilii and orbicularis oculi muscles during listening to authentic and posed laughs predicts subsequent authenticity ratings (Lima et al., 2021). Individuals showing stronger sensorimotor brain responses to laughter (McGettigan et al., 2015) and higher trait resonance with others' emotions (trait contagion; Neves et al., 2018) are better at perceiving laughter authenticity. Additionally, an fMRI study showed that a stronger engagement of sensorimotor systems correlates with improved recognition of emotional speech (Correia et al., 2019). Crucially, however, these correlations preclude causal inferences. It cannot be excluded that increased sensorimotor activity is a consequence, not a cause, of improved vocal emotional processing. Particularly because causal evidence is scant and mixed. In one TMS study, suppressing activity in somatosensory and premotor cortices disrupted emotion recognition in vocalizations (Banissy et al., 2010). One pen-holding study showed a similar effect (Hawk et al., 2012), but other did not: reducing mimicry made the vocalizations sound more positive, but left emotion recognition accuracy intact (Wołoszyn et al., 2022).

Here, we reduced orofacial responses using a pen-holding manipulation (e.g., Maringer et al., 2011; Niedenthal et al., 2001, 2009), and we focused on participants' ability to tell authentic from posed emotional vocalizations. Authentic laughter and crying differ from their posed counterparts acoustically, perceptually, and in their neural correlates (Anikin & Lima, 2018; Bryant & Aktipis, 2014; Conde et al., 2022; Lima et al., 2021; Pinheiro et al., 2021). If reducing sensorimotor responses causes decrements in authenticity detection in vocalizations, that would imply that mimicry contributes to emotional understanding across modalities, and inform current debates on why we imitate others in interactions.

Reported associations between sensorimotor mechanisms and authenticity perception in vocalizations (Lima et al., 2021; McGettigan et al., 2015; Neves et al., 2018) motivated our main prediction: compared to participants who can mimic freely, those holding a pen between the teeth and lips will be worse at inferring authenticity in laughter and crying. Because sensorimotor responses are stronger for positive than negative vocalizations (Lima et al., 2021; Warren et al., 2006), mimicry inhibition could affect laughter more than crying perception. Importantly, we included a non-emotional control task (abstract reasoning). If potential pen-holding effects reflect mimicry inhibition as intended, and not unspecific effort related to holding a pen (Rychlowska et al., 2014), they should not be observed for this task. Two more exploratory aspects were also considered. First, participants also rated emotional contagion for each vocalization. Because mimicry is sometimes conflated with contagion (Hess & Fischer, 2022), and facial expressions can influence subjective emotional experiences (Coles et al., 2022), a tentative prediction is that inhibiting mimicry reduces contagion responses. Second, we explored whether scores on questionnaires of trait contagion and empathy correlate with authenticity and contagion evaluations (Neves et al., 2018), and whether such correlations vary across mimicry conditions. The study preregistration is available at https://osf.io/gv76z.

## Methods

### Participants

We tested 150 adults, 101 women and 49 men. They were 26.97 years of age on average (*SD* = 8.68, range = 18-56) and had 14.60 years of education (*SD* = 2.55, range = 12–26). All participants reported normal hearing, normal or corrected-to-normal vision, no history of neurological or psychiatric disorders, and were Portuguese native speakers. They were recruited on campus or from local communities and received partial course credits or a voucher to take part. Written informed consent was obtained from all participants.

Because there is no standard procedure to estimate sample size for mixed-effects models, we decided to recruit and test as many participants as possible within a reasonable time frame (ca. 6 months). We used previous related work as a general reference (e.g., *N* = 119 in Neves et al., 2018; *N* = 100 in Lima et al., 2021), and decided to aim for a somewhat higher *N* because of the online testing format, the exploratory components of the study, and the fact that we have a between-subjects manipulation (unlike Neves et al. and Lima et al.).

## Materials

**Voices.** The same vocal stimuli were used for authenticity and contagion evaluations, and they were auditory-only stimuli. They consisted of 120 nonverbal vocalizations, 30 unique ones per each of the four conditions: posed laughter, authentic laughter, posed crying, and authentic crying (half of the stimuli in each condition were produced by women). Vocalizations were selected from a larger stimulus set that has been used in behavioral and neuroimaging studies (Lima et al., 2021; O'Nions et al., 2017; Pinheiro et al., 2021). They were generated within a sound-proof anechoic chamber by six speakers (three women), aged 24 to 48 years, who had experience recording vocal materials but were not professional actors. To elicit authentic laughter, an amusement induction procedure was used in a social-interactive setting: the speakers watched short clips that they had identified as amusing beforehand, and the experimenters interacted with them during the recording session to ensure the naturalness of the laughs. For authentic crying, speakers recalled upsetting/sad past life events to self-induce a genuine experience of sadness. All of them confirmed that they were able to cry spontaneously and reported having felt genuine sadness throughout the recording. The same speakers were asked to produce posed laughter and posed crying, and to try to make them sound credible and natural, as per the typical procedure used to record acted vocal stimuli (Lima et al., 2013). The duration of the stimuli was 2450 ms on average (*SD* = 410), and the best possible match was ensured between the duration of authentic (*M* = 2560 ms) and posed ones (*M* = 2330 ms).

**Abstract Reasoning.** The Matrix Reasoning Item Bank (MaRs-IB; (Chierchia et al., 2019) is an 8-min nonverbal reasoning test modeled after Raven's matrices tests. On each trial, participants are shown a 3X3 matrix, corresponding to eight cells containing abstract shapes, and a ninth empty cell. The task consists of selecting one of four options for the empty cell, which requires identifying the rules that govern the pattern of the other eight cells. Participants are told that they have up to 30 s to complete each trial, after which the next one is presented. The test includes 80 matrices, and if participants complete all of them in less than eight min, they are presented again until time is up. Following the test's developers, performance corresponds to the proportion of correct responses (i.e., number of correct responses/number of completed trials), computed for each participant after excluding responses provided in less than 250 ms. Proportions were logit-transformed for statistical analyses.

**Questionnaires.** The Emotional Contagion scale, ECS (Doherty, 1997; Rueff-lopes & Caetano, 2012), includes 15 items and assesses the tendency to resonate with others' emotions as a trait. Items are rated on a five-point scale, from 1 ('never') to 5 ('always'), and cover five emotions: happiness (e.g., 'When someone smiles warmly at me, I smile back and feel warm inside'); love (e.g., 'I melt when the one I love holds me close'); fear (e.g., 'I notice myself getting tense when I'm around people who are stressed out'); anger (e.g., 'It irritates me to be around angry people') and sadness (e.g., 'I cry at sad movies'). Item scores are averaged to produce a total individual score.

The Questionnaire of Cognitive and Affective Empathy, QCAE (Queirós et al., 2018; Reniers et al., 2011), includes 31 items: 19 focused on cognitive and 11 on affective empathy.

Examples of items include ‘I can tell if someone is masking their true emotion, or ‘Before criticizing somebody, I try to imagine how I would feel if I was in their place’. Items are rated on a four-point scale from 1 (‘strongly disagree’) to 4 (‘strongly agree’), and scores are summed to produce cognitive and affective trait empathy scores. Only the cognitive subscale was used because the affective one measures resonance with others’ emotions, which the ECS already covers.

**Procedure**

The tasks and questionnaires were implemented in Gorilla Experiment Builder (Anwyl-Irvine et al., 2020), an online platform for psychological research. Participants took part in one online session lasting around 50 minutes, supervised by an experimenter via Zoom (www.zoom.us). They were in an audiovisual virtual meeting throughout the session, and the experimenter ensured that they followed the instructions, could ask questions, were in a quiet place, and wore headphones during the voice task.

Participants first provided informed consent and completed a background questionnaire that asked for demographic information. They were then assigned randomly to the *inhibited* or *free* face conditions, such that there were 75 participants in each condition. Those in the inhibited condition were asked to keep their face as still as possible and to hold a pen sideways in the mouth while evaluating the vocalizations, lightly using their teeth and lips (Borgomaneri et al., 2020; Niedenthal et al., 2001, 2009; Oberman et al., 2007). EMG evidence confirms that this technique affects orofacial muscle activity (Davis et al., 2015, 2017; Han et al., 2016; Oberman et al., 2007) and results in reduced mimicry (Davis et al., 2017). Those in the free condition did not receive any instructions regarding orofacial movements during the voice tasks. They were instead asked to hold the pen in the mouth during the abstract reasoning test. Thus, all participants underwent the same pen-holding manipulation during the experiment, but on different moments, and none during the voice tasks *and* reasoning test. All were told that the pen-holding manipulation was meant to test how people perform more than one task simultaneously.

Authenticity and contagion evaluations were provided in different blocks, as separate tasks, completed at the start and end of the experimental session in a counterbalanced order. The abstract reasoning test and the questionnaires were completed between the two voice tasks (see Figure 1C). In the authenticity task, participants made two-alternative forced choice judgments, classifying each vocalization as posed or authentic. In the contagion task, they rated their emotional response to each vocalization on a seven-point scale from 1 (‘it does not make me feel a similar emotion’) to 7 (‘it makes me feel a similar emotion’). The number of trials (120) and their structure was similar across the voice tasks: fixation cross for 2 s, stimulus presentation for ≈3 s, and prompt for participants’ response (see Figure 1). The next trial started right after the response, and no feedback was given. The tasks were preceded by four practice trials, and participants could take a break half-way through (i.e., after 60 trials). Each task lasted around 14 minutes.

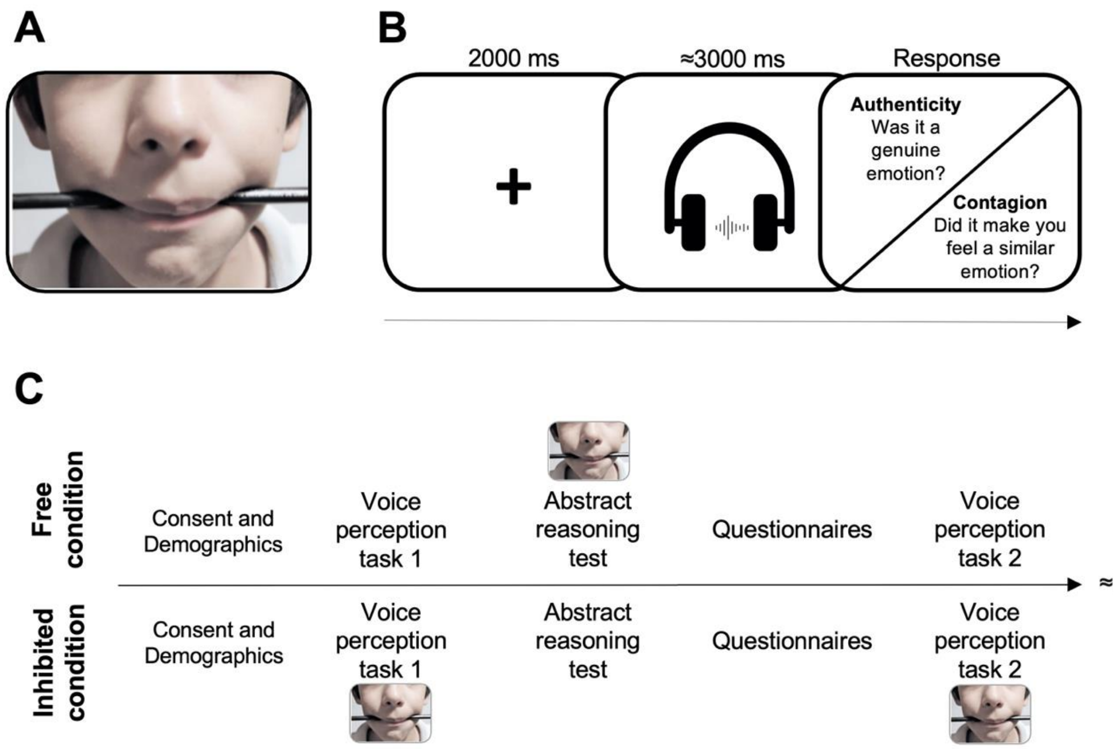


*Figure 1.* Illustration of the pen-holding manipulation (A), structure of the experimental trials in the voice perception tasks (B), and structure of the experimental session in the free and inhibited conditions (C). Authenticity and contagion evaluations were provided separately, in two voice perception tasks, whose order was counterbalanced across participants.

**Data Analysis**

Statistical analyses were performed using mixed-effects modelling as implemented in the R package *lme4* (Bates et al., 2015). The fixed effects predictors were sum-coded (-1, 1). Separate models were used for the authenticity and contagion tasks, and significance was tested using the Satterthwaite degrees of freedom approach, as implemented in the R package *lmerTest* (Kuznetsova et al., 2017). If a significant interaction was obtained, we performed follow-up analyses using the *emmeans* package (Lenth et al., 2021), and selected the holm method for controlling for family wise error rate.

For the authenticity task, we modelled accuracy data (correct/incorrect responses) with mixed-effects logistic regression. For the contagion task, we modelled 1-7 ratings with mixed-effects ordinal logistic regression. The full models included face condition (inhibited, free), vocalization type (laughter, crying), authenticity (posed, authentic), and their interaction as fixed factors, as well as by-participant and by-item random intercepts. The models also included by-participant random slopes for vocalization type, authenticity, and the interaction term. The model of authenticity data was of the form:

*AuthenticityEvaluation ~ FaceCondition*VocalizationType*Authenticity + (1+VocalizationType*Authenticity |Participant) +(1|Stimuli)*

For the contagion data, the model was similar, but with 1-7 ratings instead of binary responses.

Demographic data (sex and age) and questionnaire scores (ECS and QCAE scores) were added to these models as covariates to check if they influenced the results. Age and questionnaire scores were mean-centered.

An independent samples t-test examined whether inhibiting facial responses affected the proportion of correct responses in the cognitive test. We did not use the mixed-effects models planned at the preregistration phase, considering evidence that estimating random effects (e.g., random intercepts and slopes) is problematic when there is only a single observation per participant (Barr et al., 2013).

When null results were obtained, we complemented frequentist statistics with Bayesian inference for a substantiated interpretation. Specifically, Bayes Factors (BFs) were used to determine the likelihood of the null compared to the alternative hypothesis, where a $BF_{10} < .333$ provides substantial or stronger evidence for the null hypothesis, according to Jeffreys' guidelines (Jarosz & Wiley, 2014; van Doorn et al., 2021).

**Transparency and Openness**

The data, materials, and code used for analyses are available at https://osf.io/rhv5x/?view_only=c13c3e9c32f144a490063d74c12e244e. The study preregistration is available at https://osf.io/gv76z. We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study.

## Results

Figure 2 shows descriptive statistics for authenticity and contagion evaluations (see also Table S1). Authenticity perception accuracy was 68.11% on average, and performance was above chance across conditions. Contagion ratings were 3.30 on average (scale 1-7), and authentic laughs (4.46) were particularly contagious (all other conditions, ≤ 3.21).

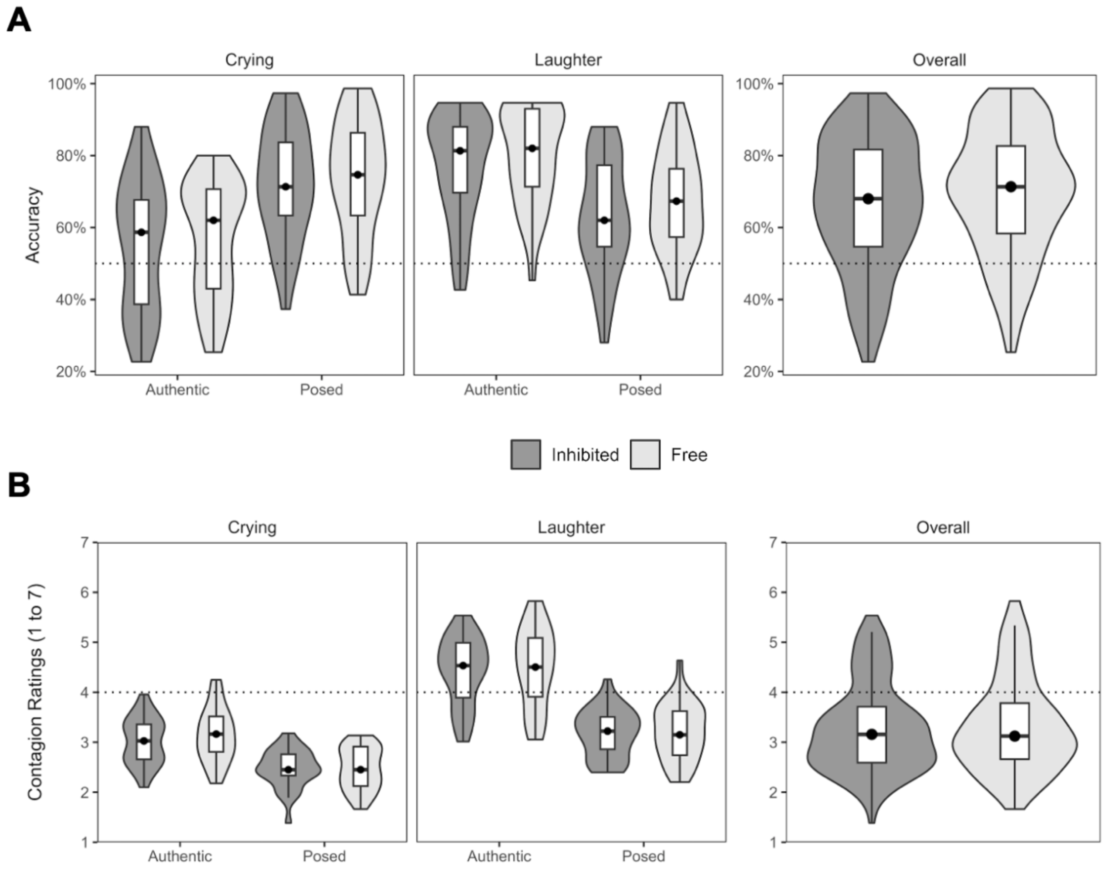


*Figure 2.* Accuracy of authenticity perception (A) and contagion ratings (B) for each condition (Overall corresponds to the average of all conditions of vocalizations). The middle horizontal lines show the medians, the boxes indicate the 25$^{th}$ and 75$^{th}$ percentiles,

and the violin plots show the distribution of mean observed values. The dashed lines indicate chance level in A (50%) and the mid-point of the scale in B (4)

**Authenticity.** Our main analysis confirmed that inhibiting orofacial responses decreased the accuracy of authenticity perception (66.66% vs. 69.57% in the inhibited and free conditions, respectively), as shown by a main effect of face condition, *estimate* = -0.09 (odds ratio, OR = 0.91), *SE* = 0.04, *z* = -2.43, *p* = .015, 95% CI [-0.16, -0.02]. This effect did not interact with vocalization type, *estimate* = 0.04 (OR = 1.04), *SE* = 0.02, *z* = 1.59, *p* = .111, 95% CI [-0.01, 0.09], or authenticity, *estimate* = 0.00 (OR = 1.00), *SE* = 0.05, *z* = 0.03, *p* = .980, 95% CI [-0.10, 0.11]. There was also no three-way interaction between face condition, vocalization type, and authenticity, *estimate* = 0.00 (OR = 1.00), *SE* = 0.04, *z* = -0.10, *p* = .922, 95% CI [-0.08, 0.08].

There was a main effect of vocalization type, *estimate* = -0.21 (OR = 0.81), *SE* = 0.08, *z* = -2.49, *p* = .013, 95% CI [-0.37, -0.04], and an interaction between vocalization type and authenticity, *estimate* = -0.45 (OR = 0.63), *SE* = 0.09, *z* = -5.05, *p* < .001, 95% CI [-0.63, -0.28]. Replicating previous findings (Pinheiro et al., 2021), authenticity perception was more accurate for laughter (78.44%) than for crying (55.89%) when vocalizations were authentic, *estimate* = -1.33 (OR = 0.27), *SE* = 0.24, *z* = -5.52, *p* < .001, 95% CI [-1.80, -0.86], and more accurate for crying (72.96%) than for laughter (65.16%) when they were posed, *estimate* = 0.49 (OR = 1.64), *SE* = 0.25, *z* = 1.96, *p* = .050 95% CI [0.00, 0.99].

Effects of listener's age and sex did not interact with any of our variables of interest (*p*s > .070). We found a main effect of age, though, with older adults being more accurate than younger ones, *estimate* = 0.01 (OR = 1.01), *SE* = 0.00, *z* = 2.44, *p* = .015, 95% CI [1.00, 1.02]. Sex effects were not significant, *estimate* = 0.05 (OR = 1.06), *SE* = 0.04, *z* = 1.44, *p* = .150, 95% CI [-0.02, 0.13].

**Contagion.** Inhibiting orofacial responses did not affect contagion ratings, as shown by a non-significant main effect of face condition, *estimate* = -0.07 (OR = 0.93), *SE* = 0.14, *z* = -0.50, *p* = .616, 95% CI [-0.34, 0.20]. Face condition also did not interact with vocalization type, *estimate* = -0.02 (OR = 0.99), *SE* = 0.05, *z* = -0.27, *p* = .781, 95% CI [-0.12, 0.09], or authenticity, *estimate* = -0.02 (OR = 0.98), *SE* = 0.04, *z* = -0.59, *p* = .552, 95% CI [-0.10, 0.05], and the three-way interaction between face condition, vocalization type, and authenticity was also not significant, *estimate* = -0.02 (OR = 0.98), *SE* = 0.02, *z* = -1.01, *p* = .313, 95% CI [-0.07, 0.02]. Bayesian analyses provided decisive evidence for the null hypothesis, $BF_{10} < 0.01$ (i.e., for a model without vs. with a face condition effect).

As expected (Lima et al., 2021; Neves et al., 2018), laughter was more contagious (3.83) than crying (2.79), *estimate* = -0.81 (OR = 0.44), *SE* = 0.09, *z* = -8.92, *p* < .001, 95% CI [-0.99, -0.63], and authentic vocalizations (3.78) more contagious than posed ones (2.84), *estimate* = 0.71 (OR = 2.03), *SE* = 0.08, *z* = 8.55, *p* < .001, 95% CI [0.54, 0.87]. Effects of authenticity on contagion were larger for laughter (authentic: 4.46, posed: 3.21) than for crying (authentic: 3.11, posed: 2.48), as indicted by the interaction between vocalization type and authenticity, *estimate* = - 0.23 (OR = 0.79), *SE* = 0.08, *z* = -3.04, *p* = .002, 95% CI [-0.38, -0.08].

Effects of listener's age and sex did not interact with any of our variables of interest (*p*s > .107), and the main effect of age was not significant, *estimate* = -0.00 (OR = 0.99), *SE* = 0.01, *z* = -0.33, *p* = .740, 95% CI [-0.04, 0.03]. The only finding was that women reported higher contagion for authentic vocalizations than men, *estimate* = 0.13 (OR = 1.13), *SE* = 0.03, *z* = 3.16, *p* = .002 95% CI [0.05, 0.20].

**Abstract Reasoning.** Accuracy in the reasoning test was similar in the inhibited (59.64%) and free (61.00%) conditions, *t*(145.85) = 0.61, *p* = .541, equal variances assumed, Levene's test, $p$ = .520. Bayesian analyses provided substantial evidence for the null hypothesis, $BF_{10}$ = 0.20.

Face condition also did not affect the absolute number of correct responses, i.e., regardless of how many trials were completed. Averages were 23.77 and 24.96 in the inhibited and free conditions, respectively, $t(148) = 1.13$, $p = .258$, equal variances assumed, Levene's test, $p = .277$, $BF_{10} = 0.32$.

**Questionnaires.** Trait contagion (ECS) scores were similar to previous findings (Table S1; Doherty, 1997). The effect of trait contagion on authenticity perception was positive as expected, but not significant, *estimate* = 0.10 (OR = 1.10), $SE = 0.08$, $z = 1.22$, $p = .221$, 95% CI [-0.06, 0.26]. Similarly, trait contagion did not interact with any of the variables of interest, $p$s > .162. In contrast, ECS scores predicted higher contagion responses to the vocalizations, *estimate* = 1.09 (OR = 2.98), $SE = 0.28$ $z = 3.88$, $p < .001$, 95% CI [0.54, 1.64]. This effect did not interact with any other variable, $p$s > .063, except for sex, *estimate* = -0.77 (OR = 0.46), $SE = 0.31$, $z = -2,49$, $p = .012$, 95% CI [-1.37, -0.16]. ECS predicted contagion responses for men, *estimate* = 2.25 (OR = 9.47), $SE = .048$, $z = 4.63$, $p < .001$, 95% CI [1.28, 3.16], but only marginally for women, *estimate* = 0.71 (OR = 2.04), $SE = 0.39$, $z = 1.87$, $p = .061$, 95% CI [0.00, 1.49].

Average cognitive empathy scores (QCAE) were also similar to previous findings (e.g., Reniers et al., 2011; Vilaverde et al., 2020). Associations with authenticity perception depended on face condition and stimulus authenticity, as indicated by a three-way interaction between empathy, face condition, and authenticity, *estimate* = 0.02 (OR = 1.02), $SE = 0.00$, $z = 2. 86$, $p = .004$, 95% CI [0.01, 0.04] (the main effect and other interactions were not significant, $p$s > .158). In the inhibited condition, higher cognitive empathy predicted improved authenticity detection in authentic vocalizations, *estimate* = 0.03 (OR = 1.03), $SE = 0.01$, $z = 2.00$, $p = .045$, 95% CI [0.00, 0.05] (posed vocalizations, $p = .758$). In the free condition, higher empathy predicted improved authenticity detection in posed vocalizations, *estimate* = 0.03 (OR = 1.03), $SE$ = 0.01, $z$ = 2.47, $p$ = .014, 95% CI [0.00, 0.05] (authentic vocalizations, $p$ = .059). Higher empathy scores also predicted higher contagion responses to the vocalizations, but only in the inhibited condition, *estimate* = 0.07 (OR = 1.08), $SE$ = 0.03, $z$ = 2.55, $p$ = .010, 95% CI [0.02, 0.13] (free condition, $p$ = .600; interaction between empathy and face condition, *estimate* = 0.04 (OR = 1.04), $SE$ = 0.01, $z$ = 2.26, $p$ = .024, 95% CI [0.01, 0.08]; the main effect and other interactions were not significant, $p$s > .104).

**Unplanned (non-preregistered) analyses.** Because the authenticity task was longer than the abstract reasoning task (14 vs. 8 minutes), increased fatigue could explain why face condition affected authenticity perception but not abstract reasoning. Nevertheless, when stimulus order (one to 120) was included in the model for the authenticity task, the main effect of order was non-significant, *estimate* = -0.00 (OR = 0.99), $SE$ = 5.33, $z$ = -0.35, $p$ = .723, 95% CI [-0.001, 0.001], as was the interaction between order and face condition, *estimate* = -0.00 (OR = 1.00), $SE$ = 5.32, $z$ = -0.15, $p$ = .881, 95% CI [-0.001, 0.001]. In other words, performance remained stable throughout the task, suggesting that fatigue did not play a role.

Because our models are based on unaggregated data from individual trials, they do not account directly for possible response biases (e.g., a disproportionate tendency to classify stimuli as authentic). An additional analysis was conducted using unbiased hit rates, or Hu (Wagner, 1993), a measure that considers both correct identifications and the total number of times that each response category is used (i.e., correct and incorrect uses). Hu scores were calculated for authentic and posed stimuli for each participant, arcsine square-root transformed, and submitted to a mixed-design analysis of variance (ANOVA), with authenticity as a repeated measure, and face condition as a between-subjects factor. The main effect of face condition was replicated, $F(1,148) = 4.96$, $p$ = .027, $\eta_p^2 = .032$, and it did not interact with authenticity, $p = .978$. Hu scores were 0.45 and 0.49 for the inhibited and free conditions, respectively.

## Discussion

We asked whether inhibiting orofacial mimicry affected evaluations of emotional vocalizations. Compared to participants who could mimic, those holding a pen between their teeth and lips were worse at perceiving laughter and crying authenticity, despite experiencing intact contagion. The pen-holding manipulation did not affect abstract reasoning, suggesting that it inhibited mimicry without having cognitive costs. Trait emotional contagion predicted contagion responses to the vocalizations, but not authenticity perception. Cognitive empathy predicted contagion responses and authenticity perception, but only in some conditions.

The main finding was that inhibiting mimicry decreased participants' ability to tell authentic from posed vocalizations. The effect was observed both when the analysis was based on data from individual trials, as well as when accuracy rates were corrected for possible response biases. Previous studies documented that listening to vocalizations and emotional speech evokes sensorimotor responses (Arias et al., 2018; Lima et al., 2015, 2021; McGettigan et al., 2015; Warren et al., 2006), even in congenitally blind individuals (Arias et al., 2021). It was also known that such responses predict emotional evaluations (Lima et al., 2021; McGettigan et al., 2015). But there was no evidence for a causal implication of mimicry in authenticity perception, which is crucial for arguments that mimicry supports emotional understanding, and that emotion recognition involves simulation (Niedenthal, 2007; Ross & Atkinson, 2020; Wood et al., 2016). The effect was small, in line with previous related research (e.g., Borgomaneri et al., 2020; Neves et al., 2018) and facial feedback effects in general (Coles et al., 2019). It was also similar across vocalization types, and not stronger for laughter as predicted. Thus, although positive vocalizations evoke larger sensorimotor responses (Lima et al., 2021; Warren et al., 2006), contributions of mimicry to emotional understanding are independent of valence. Indeed, Banissy et al. (2010) and Hawk et al. (2012) found that inhibiting sensorimotor activity affected emotion recognition in positive and negative vocalizations similarly.

A stronger effect for laughter could additionally be expected because our pen-hold manipulation inhibits smiling, but not muscles associated with negative emotions (e.g., corrugator, Lundqvist & Dimberg, 1995). Nevertheless, while in facial expressions distinct muscles relate to distinct valences, in vocalizations both laughter and crying require movements of the mouth and articulators, which could explain the lack of emotion-specific effects.

How can our findings be interpreted? Effort involved in holding a pen is an unlikely explanation, because contagion and cognitive performance remained intact. Authenticity perception accuracy also remained stable throughout the experiment, regardless of whether participants were holding a pen or not, further excluding fatigue as an alternative explanation. Instead, activating motor representations related to sounds arguably enables sensory and affective predictions that optimize perception. When sensorimotor activity is prevented, perceptual inferences are suboptimal. This explanation is aligned with models of auditory perception (Correia et al., 2019; Lima et al., 2016) and simulation accounts of facial emotion recognition (Wood et al., 2016). Our findings suggest that simulation is a supra-modal mechanism, and extend emerging evidence that mimicry is not restricted to observed movements (e.g., Arias et al., 2021; Lima et al., 2021). We do not 'see' voices, but they are paired with faces in naturalistic interactions, and vocalizations require orofacial movements. Additionally, we mimic partially occluded faces (Davis et al., 2022), and patients with cortical blindness show facial responses to non-consciously perceived facial and body expressions (Tamietto et al., 2009; Tamietto & de Gelder, 2010), which is further evidence of imitation of unseen actions.

Inhibiting orofacial activity could decrease emotional contagion, based on views that conflate mimicry with contagion, and that mimicry supports affiliation (Drimalla et al., 2019; Hess, 2021). We did not observe such decrements, though. Perhaps affiliative behaviors emerge primarily in social interactions, not in response to isolated sounds in somewhat artificial experimental contexts. Indeed, contagion ratings were generally low, although sounds like laughter are established to be highly contagious in everyday interactions (Scott et al., 2014). Our task can index individual differences in emotional resonance (O'Nions et al., 2017), and is similar to tests of emotional empathy that correlate with mimicry (Drimalla et al., 2019), but is has limited ecological validity and it captures primarily conscious aspects of contagion. Measures of contagion other than self-reports, such as electrodermal responses (e.g., Lima et al., 2021), could provide additional sensitivity. Nevertheless, dissociations between mimicry and emotional contagion have been observed before (Hess & Blairy, 2001; Lundqvist & Dimberg, 1995), highlighting the need to distinguish them. Mimicry is a motor behavior and contagion a feeling state (Hess & Fischer, 2022). Moreover, the meta-analysis by Coles et al. (2019) indicates that facial feedback effects might be larger for affective judgments compared to emotional experience, even though publication bias cannot be excluded as an explanation for this difference.

Higher trait contagion predicted stronger contagion responses to the vocalizations, which seems intuitive, but not improved authenticity detection, unlike reported by (Neves et al., 2018). The association between trait contagion and authenticity detection might be context-dependent. The effect was also weak in a study of laughter and crying like the current one (Lima et al., 2021), whereas Neves and colleagues examined only laughter. Moreover, our task required binary judgments of authenticity, whereas previous studies required fine-grained ratings in seven-point scales. Cognitive empathy, on the other hand, had selective associations with contagion and authenticity responses. It predicted stronger contagion responses in the inhibited condition, possibly because mentalizing provides a route for contagion when sensorimotor mechanisms are restricted (Lamm et al., 2011). Associations with authenticity detection were observed for authentic vocalizations in the inhibited condition, and for posed vocalizations in the free condition. That cognitive empathy predicts authenticity detection in posed vocalizations could be expected, because posed laughter engages mentalizing systems more strongly than authentic laughter (McGettigan et al., 2015). Not finding the same association in the inhibited condition, where it instead emerged for authentic vocalizations, was unexpected and should be confirmed.

A limitation of our study is that we used a single strategy to inhibit mimicry, such that future work will need to extend our findings across different methods (e.g., Wood et al., 2015). Although the pen-holding manipulation is known to reduce mimicry (Davis et al., 2017) and is optimal for online testing, we cannot be certain that mimicry was fully inhibited because we could not measure orofacial activity. Perhaps some level of mimicry still occurred, and whether a full inhibition would produce larger decrements in performance remains to be determined. Moreover, our task did not emphasize speed, just accuracy. Future studies could emphasize both, considering evidence that speed might be particularly sensitive to sensorimotor manipulations (Banissy et al., 2010). An additional limitation is that our control task (abstract reasoning) differed from the experimental one in aspects such as type of stimulus and duration. We did not use an auditory control task because sounds can access the motor system even in non-emotional tasks (e.g., Liebenthal & Möttönen, 2018; Lima et al., 2016), and potential pen-holding effects could then reflect interference with sensorimotor activity, and not effort as intended. We also excluded the possibility that the longer duration of the authenticity task was associated with increased fatigue. Nevertheless, a closer match between the experimental and the control tasks, namely in response format and number of trials, will be useful to optimize interpretability in future studies.

To conclude, our study shows that inhibiting orofacial mimicry disrupts the ability to tell authentic from posed emotional vocalizations. This finding suggests a novel causal link between emotional mimicry and authenticity detection in the auditory modality, with implications for debates on the roles of mimicry and on the mechanisms underlying emotion recognition.

## Author Note

**Openness.** The data, materials, and code used for analyses are available at https://osf.io/rhv5x/?view_only=c13c3e9c32f144a490063d74c12e244e. The study preregistration is available at https://osf.io/gv76z.

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**Conflicts of interest.** The authors have no competing interests to declare that are relevant to the content of this article.

**Ethics approval.** This study was performed in line with the principles of the Declaration of Helsinki. The questionnaires and methodology for the study were approved by the local ethics committee at Iscte, University Institute of Lisbon.

**Consent to participate.** Participation was voluntary, and informed consent were secured from all participants.

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