INTERNATIONAL JOURNAL OF ENVIRONMENT, ENGINEERING & EDUCATION

ISSN 2656-8039 (ONLINE)
VOLUME 8 • ISSUE 3 • SEPTEMBER 2026

Received: 1 June 2026 | Accepted: 16 August 2026 | Published: 2 September 2026

Research Article

Electrode-Level EEG Assessment of Sequential Taste and Packaging Exposure in Processed Foods: An Exploratory Study

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ABSTRACT

This study examined whether sustained electrode-level neural signals differentiate successive stages of a commercial food encounter and whether response patterns vary by gender. Fifty adults (25 male, 25 female; 20–40 years) completed a fixed protocol comprising a composite baseline, post-ingestive viewing of a branded product while residual flavor remained, later viewing of the same product under a recall instruction, and viewing of an unrelated snack product. Signals were acquired from eight scalp channels using a portable system, filtered at 0.5–45 Hz, notch-filtered at 50 Hz, common-average referenced, and aggregated into participant-level mean device-native amplitudes. Electrode-specific 4 × 2 mixed-design repeated-measures ANOVAs tested Condition, Gender, and their interaction; Greenhouse – Geisser correction addressed sphericity violations, and Benjamini – Hochberg adjustment controlled the false discovery rate. Condition effects were significant at all eight electrodes (all $q \leq 0.028$), with partial ηp^2 ranging from 0.073 at O1 to 0.523 at C3 and the largest effects at C3, C4, and F4. Baseline-to-post-ingestive viewing yielded the broadest adjusted paired differences. Neither the Gender main effect nor the Condition × Gender interaction remained significant after correction, while individual trajectories showed marked heterogeneity. Portable low-density recordings therefore detected sequence-associated sensor-level differentiation across the protocol. Because condition was confounded with serial position, the baseline combined eye states, package stimuli were unmatched, amplitudes were uncalibrated, and behavioral validation was absent, the findings do not establish causal effects of flavor, product identity, recall, preference, cortical source, or consumer choice. Randomized, counterbalanced, behaviorally validated, and higher-density replication is required.

KEYWORDS consumer neuroscience • cross-modal integration • device-native amplitude • commercial product cues • multisensory perception • portable electroencephalography • sensor-space analysis

ARTICLE CITATION
H. Velingkar, R. R. Kulkarni, P. P. Patavardhan, "Electrode-Level EEG Assessment of Sequential Taste and Packaging Exposure in Processed Foods: An Exploratory Study," International Journal of Environment, Engineering and Education, Vol. 8, No. 3, pp. 403-420, 2026. https://doi.org/10.55151/ijeedu.v8i3.557

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1. INTRODUCTION

Flavor is a temporally evolving percept formed through taste, retronasal olfaction, oral somatosensation, temperature, audition, and vision [1]–[4]. Modality weighting varies with timing, experience, attention, and physiological state, while contextual cues generate expectations against which sensory evidence is evaluated [5], [6]. This principle is especially relevant to processed foods, which are encountered as coordinated commercial objects. A potato wafer combines seasoning, aroma, crispness, and oral texture with package color, imagery, brand identity, and consumption history. The commercial encounter consequently extends from pre-ingestive visual exposure to consumption, residual oral stimulation, and package re-exposure.

Packaging is therefore an active perceptual context. Product-extrinsic cues influence expected flavor, liking, quality, and value, subject to congruency, product category, cultural learning, and interaction stage [5], [7]. Color and graphic form acquire taste-related meaning, while product visibility and imagery modify pre-ingestive expectations [8], [9]. Package shape and color affect dessert expectations [10], tactile design properties transfer to taste evaluations [11], and imagery alters flavor judgements for an unchanged beverage [12]. Visual influence is strongest during selection, whereas taste dominates consumption [13]. Color, texture, scent, and opening dynamics can also affect taste intensity, emotion, and willingness to pay [14]. These findings establish contextual influence, but physiological responses cannot automatically be classified as attention, preference, memory, or purchase intention.

Brand information adds learned semantic and affective content. Familiar cues and anticipated identity can alter beverage preference and neural responses to otherwise identical products [15], [16], while stated price can affect experienced pleasantness [17]. Semantic labels modify olfactory processing, and cognitive descriptors alter affective responses to taste and flavor [18], [19]. Learned information therefore interacts with sensory and reward-related processing [20]. However, brand identity, familiarity, expectancy, recognition, liking, novelty, and reward history are distinct constructs. Without independent measurement, package responses cannot be attributed uniquely to brand preference or taste-memory reactivation.

Consumer neuroscience is most informative when physiological evidence complements operationalized behavioral constructs [21], [22]. Its central hazard is reverse inference: activity at a sensor, region, frequency, or component does not independently identify the underlying cognitive process [23]. Valid interpretation requires matched controls, explicit constructs, transparent analyses, convergent behavioral evidence, and restrained translation to consumer behavior [21], [22], [24]. These requirements are critical because commercial packages simultaneously vary in visual properties, familiarity, cultural meaning, expectancy, and affective value.

Electroencephalography (EEG) records scalp potentials non-invasively, provides millisecond-scale resolution, and permits repeated observations with limited physical restriction. Reviews identify these advantages but also heterogeneous paradigms, preprocessing, outcomes, and inferential practices [25], [26]. Portable systems improve usability, but usability does not establish signal quality, spatial coverage, or construct validity [27]. Scalp potentials mix neural and non-neural activity and are affected by volume conduction, reference choice, sensor geometry, ocular and muscular artefacts, movement, and noise [28], [29]. An electrode is therefore not a direct measure of a cortical region. Reproducibility requires transparent acquisition, filtering, epoching, artefact handling, exclusions, analytical units, and source-claim boundaries [30], [31]. Standardized preprocessing and data organization reduce variability but cannot repair an under-controlled contrast [32], [33].

Food-related consumer neuroscience remains methodologically heterogeneous [26], [34]. Visual food cues engage distributed perceptual and valuation systems [35]. EEG studies differentiate image energy value [36], hunger-related food-cue processing [37], and responses to processed versus unprocessed foods [38]. Standardized images are necessary because visual properties, palatability, and caloric content may covary [39]. Multivariate evidence indicates rapid differentiation of food properties [40]; event-related potentials distinguish package-related semantic congruency [41]; and cross-modal EEG measures relate to perceived taste-visual similarity [42]. Most evidence derives from event-locked images, priming, categorization, or explicit judgement and cannot be transferred uncritically to sustained mean amplitude during consumption with a sparse portable montage.

A specific empirical gap consequently remains. Existing research does not establish whether low-density portable EEG can detect systematic participant-level amplitude differences across an ecologically continuous sequence involving ingestion, immediate package viewing, later viewing of the same package under a recall instruction, and presentation of another commercial package. This question differs from estimating an event-related component, decoding a food category, or localizing a cortical generator. It concerns the feasibility and inferential limits of condition-level sensor measurements across prolonged sequential stages. Any detected difference must therefore be described as sequence-associated unless stimulus identity, timing, residual sensory stimulation, familiarity, and relevant behavioral constructs are independently controlled.

The present exploratory feasibility study involved 50 healthy adults familiar with a branded potato-wafer product. EEG was recorded from eight scalp electrodes during a fixed sequence comprising an operational

Baseline, post-tasting viewing of the branded package, later viewing of that package under a taste-recall instruction, and viewing of an unrelated snack package. Tasting denotes recording after chewing and swallowing, while residual taste remained and the branded package was visible; it is not a taste-only condition. The design tested whether participant-level mean device-native amplitude differentiated these stages while preserving continuity between ingestion and package exposure.

Baseline combined an eyes-open epoch in a white enclosure with an eyes-closed epoch. These states differ systematically in EEG power and topography, including activity within visual, sensorimotor, and distributed resting-state systems [43]–[46]. The composite is thus an operational reference, not a physiologically homogeneous resting state. Contrasts involving Baseline may reflect visual input, eye state, vigilance, and averaging across distinct physiological conditions as well as differences related to food and packaging.

The fixed order creates a decisive causal limitation. All participants received the same sequence, and the unrelated and branded packages were not counterbalanced. Counterbalancing is required to distinguish treatment effects from plausible position and residual effects in repeated sequences [47]. Food-image judgments can also exhibit serial dependence, whereby a preceding stimulus influences a subsequent evaluation [48]. Condition is therefore inseparable from serial position and elapsed time. Differences may reflect residual taste, cumulative exposure, habituation, expectation, attentional change, fatigue, movement, or recording drift in addition to stimulus-related variation.

The packages were not matched for luminance, color distribution, graphic complexity, product visibility, recognition, familiarity, liking, or flavor expectancy, although these features can affect product evaluation [5], [7]–[9]. Measures of recall vividness, familiarity, novelty, liking, perceived similarity, arousal, and purchase intention were also absent. The study can therefore characterize electrode-level changes associated with the sequence but cannot isolate causal effects of taste, package identity, memory, brand preference, or consumer choice.

Participant-level mean amplitude was compared across Baseline, Tasting, Package, and Unrelated conditions at each electrode using a mixed repeated-measures model. Condition was the within-participant factor, and the recorded male/female variable, labelled Gender in the dataset, was the between-participant factor. Predefined paired contrasts decomposed omnibus Condition effects. Effect sizes distinguished estimated magnitude from threshold-based significance, and multiplicity adjustment addressed false-positive accumulation across electrodes [49]–[51]. Interpolated scalp maps were treated only as summaries of sampled sensors because source localization depends materially on electrode density and spatial coverage [28], [52], [53]—the eight-electrode montage comprised six frontal-central, one parietal, and one occipital site.

Accordingly, the study had three linked objectives. The primary objective was to determine whether participant-level mean EEG amplitude differed across Baseline, Tasting, Package, and Unrelated conditions at the eight sampled electrodes. The secondary objective was to identify which of four predefined within-participant transitions - Baseline to Tasting, Tasting to Package, Tasting to Unrelated, and Package to Baseline - contributed to any omnibus Condition effect, while retaining descriptive Package-to-Unrelated percentage changes outside the inferential contrast family. The third objective was to assess whether the condition-response profile differed between male and female groups through the Condition-by-Gender interaction, rather than by comparing the number of statistically significant tests obtained separately within each group. Because the binary dataset variable did not constitute a multidimensional assessment of sex and gender, interpretation was confined to the recorded categories and did not infer biological or sociocultural mechanisms [54].

The primary non-directional hypothesis was that mean electrode-level amplitude would vary across at least some stages of the four-condition sequence. No directional hypothesis was specified for a particular electrode, transition, or gender group because the fixed order, sparse montage, composite baseline, unmatched package comparison, and absence of behavioral validation did not support a defensible process-specific prediction. The gender analysis and the spatial distribution of sensor-level effects were therefore explicitly exploratory. Under this framework, evidence of a Condition effect would demonstrate electrophysiological differentiation across the experimental sequence. Without additional controls and convergent behavioral evidence, it would not establish a unique neural signature of taste, package identity, memory, familiarity, novelty, gender, preference, or purchase-related behavior.

2. MATERIALS AND METHODS

2.1. Participants

Fifty healthy adults aged 20-40 years were included in the analysis (25 male and 25 female). We recruited participants through convenience sampling via notices circulated among students and staff at a single academic institution. Of 61 individuals who expressed interest, seven were ineligible, three declined after receiving study information, and one enrolled participant did not complete the scheduled recording; the final analyzed sample was therefore 50. Equal group sizes were used to support balanced descriptive comparisons, not to test a prespecified gender-specific hypothesis. Participant

recruitment, eligibility assessment, protocol completion, and inclusion in the final analysis are summarized in Figure 1.

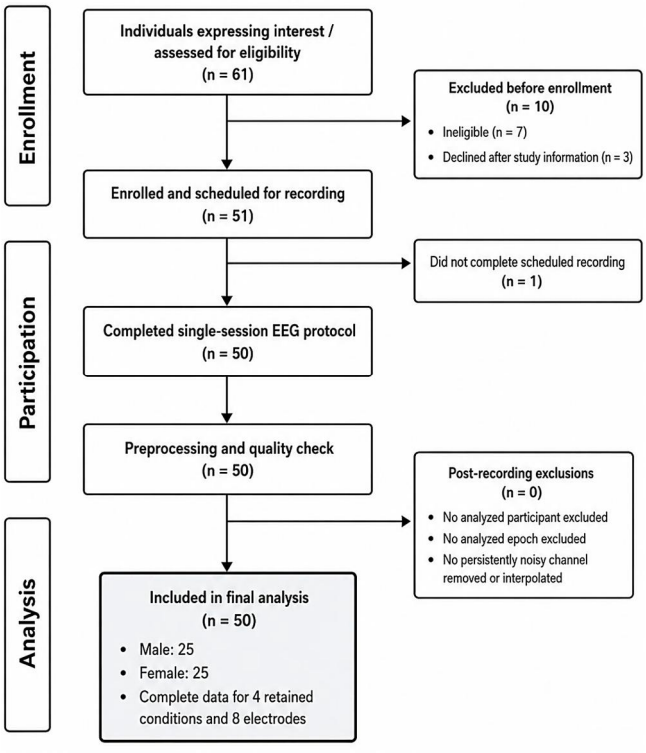

Figure 1. Participant recruitment and inclusion flow for the exploratory EEG study (CONSORT-Style).

The sample size was determined by participant availability and practical recruitment rather than an a priori power calculation. A post hoc sensitivity analysis indicated that paired comparisons conducted separately within each group (n = 25) had 80% power to detect moderate-to-large effects (Cohen's dz ≥ 0.58, alpha = 0.05), whereas between-group comparisons were adequately powered only for large effects (Cohen's d ≥ 0.81). Gender-related findings, including the Condition and Gender interaction, were consequently treated as exploratory.

Eligibility required normal or corrected-to-normal vision, self-reported right-handedness, no history of neurological or psychiatric disorders, no taste or smell disorder, no allergy to the test product, willingness to consume the stimulus, and familiarity with the branded potato-wafer product. Exclusion criteria comprised neurological illness, psychoactive medication use, allergy to potato or spice ingredients, excessive alcohol intake, strong dislike of potato wafers, or failure to complete the protocol. Written informed consent was obtained before participation.

Before testing, participants completed a standardized screening questionnaire covering demographics, medication use, olfactory and gustatory health, sleep quality, time since the last meal, hunger, recent caffeine and alcohol intake, product familiarity, and food preferences. Participants were instructed to consume their last meal 2-3 hours before testing, avoid caffeine for at least 4 hours and alcohol for 24 hours, and maintain their usual night's sleep. Responses were reviewed before EEG acquisition to confirm eligibility. The overall mean age was 28.4 ± 5.6 years.

Table 1. Participant characteristics (n = 50)

Characteristic	Male (n = 25)	Female (n = 25)	Overall (n = 50)
Age range (years)	20 – 40	20 – 40	20 – 40
Height (cm)	175.2 ± 6.7	166.3 ± 5.9	170.8 ± 9.4
Weight (kg)	78.4 ± 10.2	65.8 ± 8.3	72.1 ± 12.5
BMI (kg/m²)	25.6 ± 2.8	23.8 ± 2.6	24.7 ± 3.1
Vision	Normal/corrected	Normal/corrected	All participants
Neurological disorders	None	None	None
Handedness	Right-handed	Right-handed	All participants

2.2. Laboratory Setting and EEG Acquisition

Recordings were obtained in a controlled laboratory. Participants remained seated approximately 50 cm from the stimulus and directed their gaze into a white cardboard enclosure to reduce visual distraction. Visual stimuli were presented against a plain white background under uniform indoor LED illumination of approximately 400-500 lux. The branded package was presented upright and measured approximately 22 x 16 cm.

EEG was acquired with a Neuphony 8+2-channel Flex Cap (Pankhtech India Pvt. Ltd.) using dry Ag/AgCl-coated electrodes positioned according to the international 10-20 system [55]. Eight scalp channels (Fp1, Fp2, F3, F4, C3, C4, Pz, and O1) were sampled at 250 Hz with a 24-bit analog-to-digital converter. Reference and ground electrodes were placed on the left and right earlobes, respectively. Electrode impedance was maintained below 10 kΩ, contact quality was verified before recording, and signals were monitored continuously; impedance control is relevant because electrode impedance can affect electrophysiological data quality under some recording configurations [56]. Acquisition used the Neuphony Desktop Application. A recent direct comparison with a BioSemi 64-channel

system indicates that the Neuphony platform can capture continuous EEG and ERP features, while also documenting device-dependent differences in spectral strength and ERP amplitude and latency; device-native amplitudes therefore warrant cautious interpretation [57].

2.3. Experimental Protocol and Analytical Conditions

The protocol comprised approximately 8 minutes of EEG recording distributed across eight sequential protocol stages, with an additional approximately 30-second pause for food consumption. Four stages were subsequently retained as analytical conditions: Baseline, Tasting, Package, and Unrelated. In Minute 1, participants viewed the white enclosure (Baseline 1); in Minute 2, they closed their eyes without a visual stimulus (Baseline 2). They then consumed three Lays Indian Magic Masala potato wafers (PepsiCo Ltd.), chewing and swallowing over approximately 30 seconds. EEG acquisition was paused during chewing to limit jaw-muscle and mastication artifacts. Recording resumed immediately after swallowing while the taste remained perceptible and the branded package was viewed for 2 minutes. Thus, the analytical Tasting condition represents post-tasting activity during concurrent package viewing, not EEG recorded during chewing or taste exposure in isolation.

During Minute 5, participants rinsed their mouths with 250 mL of water while the branded package remained visible. During Minute 6, they viewed the same package and were instructed to recall the taste experience. During Minute 7, an unrelated commercial snack package of similar size and color intensity, but a different brand and flavor, was presented under the same viewing conditions. The control package was not formally pretested or matched for luminance, visual complexity, perceived similarity, brand recognition, familiarity, liking, or flavor expectancy. Minute 8 repeated the white-enclosure condition (Final Baseline). Post-session subjective responses were collected but were not included in the analyses reported in these sections.

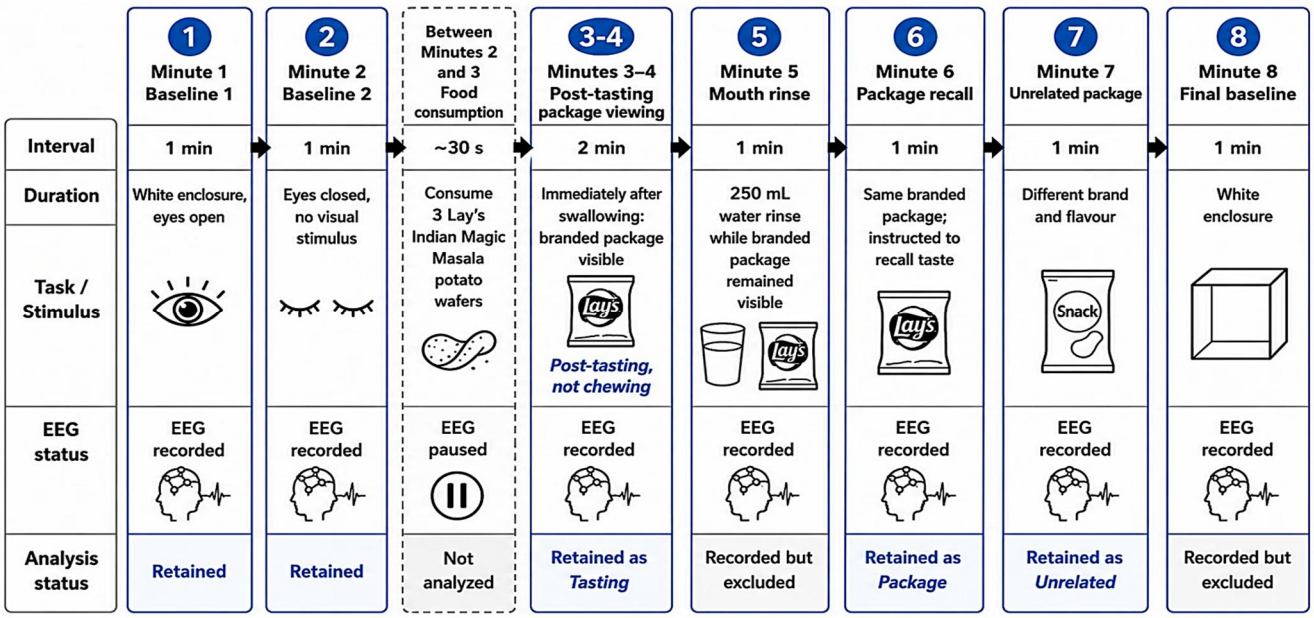

Figure 2. Experimental sequence and mapping of protocol stages to retained EEG analytical conditions.

Table 2. Operational mapping from protocol stages to statistical conditions.

Statistical condition	Protocol stage	Duration	Operational meaning
Baseline	Mean of Baseline 1 and Baseline 2	2 min total	Composite of eyes-open white-enclosure and eyes-closed epochs
Tasting	Post-tasting package viewing (Minutes 3-4)	2 min	Lingering taste with concurrent viewing of the branded package
Package	Package recall (Minute 6)	1 min	Same package after mouth rinse; taste recall instruction
Unrelated	Unrelated package (Minute 7)	1 min	Different brand and flavor

2.4. EEG Preprocessing and Quality Control

Preprocessing was conducted in Python using MNE-Python [58] and SciPy [59]. Preprocessing decisions, including filtering, referencing, bad-channel handling, and artifact treatment, can materially influence downstream EEG features; the workflow and parameter choices are therefore reported explicitly [60]. For each channel, a single DC-offset correction was performed by subtracting that channel's mean across the full

continuous recording (approximately 120,000 samples). This operation was applied once before filtering and was not repeated within individual conditions. The recording-wide sample-weighted channel mean is therefore zero by construction, whereas condition-level means may remain non-zero and reflect epoch differences subject to the selected reference and preprocessing operations.

Signals were filtered with a fourth-order Butterworth band-pass filter from 0.5 to 45 Hz, followed by a 50-Hz notch filter. Filter parameters are reported explicitly because filter design can alter the temporal and amplitude characteristics of electrophysiological signals [61]. The data were then re-referenced using common average referencing [62], and segmented at manually annotated stage markers. Electrode impedance and contact quality were verified before acquisition, signals were monitored continuously, and electrodes were adjusted when required. Post-preprocessing visual inspection confirmed successful filtering and CAR. No persistently noisy channel required removal or interpolation, and no analyzed participant or epoch was excluded. Transparent reporting of reference choice, channel quality, and artifact handling is consistent with EEG/MEG reporting recommendations [30], [31].

Table 3. Preprocessing and quality-control workflow.

Step	Operation	Implementation
1	Recording-wide centering	Subtract each channel's mean across the full continuous recording; apply once only
2	Band-pass filtering	Fourth-order Butterworth filter, 0.5-45 Hz
3	Line-noise suppression	50-Hz notch filter
4	Re-referencing	Common average reference across the eight scalp channels
5	Segmentation	Split the recording at manually annotated experimental-stage markers
6	Quality review	Inspect filtered, re-referenced signals; no noisy channels, epochs, or analyzed participants excluded

Independent component analysis and other component-based artifact-removal procedures were not applied because of the limited eight-channel montage. The study examined sustained activity during prolonged conditions rather than event-locked responses; event-related-potential analysis was therefore not performed. These decisions are stated explicitly because artifact-handling procedures and outcome definitions are central reproducibility parameters in EEG research [30], [31].

2.5. Construction and Statistical Analysis

2.5.1. Participant-Level Condition Means

The participant, rather than the individual EEG sample, was treated as the analytical unit. For each participant, electrode, and retained condition, the mean of the preprocessed device-native ADC amplitude samples was calculated as:

$$\overline{X} = \frac{1}{N}\sum_{i=1}^{N} x_i \quad (1)$$

where $(\overline{X})$ denotes the participant-level mean EEG amplitude and N the number of preprocessed samples in the corresponding epoch. Because the device output was not calibrated in microvolts, amplitudes were retained in device-native ADC units. The Baseline condition was then defined as the arithmetic mean of the Baseline 1 and Baseline 2 epoch means:

$$M_{\text{Baseline}} = \frac{M_{\text{Baseline 1}} + M_{\text{Baseline 2}}}{2} \quad (2)$$

Recording-wide centering was used only for DC-offset correction and did not replace the participant-level condition means used in subsequent analyses.

2.5.2 Descriptive Transformations

Descriptive percentage change between two condition means was calculated as:

$$\text{Percentage Change } (\%) = \frac{|M_2 - M_1|}{|M_1|} \times 100 \quad (3)$$

Here, M_1 denotes the reference-condition mean and M_2 the comparison-condition mean. Percentage change was used only for descriptive visualization because the ratio may become unstable when the reference mean approaches zero after recording-wide centering; accordingly, the 40% reference line was treated solely as a visual threshold. F3, F4, and C3 were selected for focused descriptive plots because they showed comparatively large changes across multiple condition pairs. This data-dependent selection was not used for inference, which was conducted across all eight electrodes to reduce selection-driven inference [30], [63].

Participant-level trajectory plots were standardized separately within each electrode as:

$$Z = \frac{X - \mu_e}{\sigma_e} \quad (4)$$

where X is the participant-level condition mean and μ_e and σ_e are, respectively, the mean and standard deviation across all participant-condition observations contributing to electrode e. The resulting z-scores facilitated visual comparison across electrodes with different device-native

amplitude ranges but did not replace raw participant-level means in inferential analyses.

2.5.3. Primary Mixed-Design ANOVA

The primary inferential analysis was a 4 × 2 mixed-design repeated-measures ANOVA conducted separately for each of the eight electrodes. Condition was the four-level within-participant factor (Baseline, Tasting, Package, and Unrelated), and Gender was the two-level between-participant factor (male and female). For each electrode, the classical split-plot model was:

$$Y_{ijk} = \mu + G_i + C_j + (G \times C)_{ij} + S(G)_{ik} + \varepsilon_{ijk} \quad (5)$$

where (Y_{ijk}) is the participant-level mean EEG amplitude, (μ) is the overall mean, (G_i) and (C_j) are the main effects of Gender and Condition, $(G \times C)_{ij}$ is their interaction, $S(G)_{ik}$ is the participant effect nested within Gender, and (ε_{ijk}) is the residual error. Type III sums of squares were used. Dependence among repeated observations was handled through the repeated-measures error structure of the classical split-plot ANOVA rather than a linear mixed-effects model.

Sphericity was evaluated separately at each electrode using Mauchly's test. When violated, Greenhouse–Geisser-adjusted degrees of freedom were used for within-participant f-tests [64]:

$$df_{GG} = \widehat{\varepsilon_{GG}}, df \quad (6)$$

where (df) is the uncorrected degrees of freedom and $\widehat{\varepsilon_{GG}}$ the estimated Greenhouse–Geisser epsilon.

Multiplicity across the eight electrodes was controlled with the Benjamini–Hochberg false-discovery-rate (FDR) procedure, applied separately to the prespecified Gender, Condition, and Gender × Condition effect families. For an ordered set of m p-values, the decision rule was:

$$p_{(i)} \leq \frac{i}{m} \quad (7)$$

where $(p_{(i)})$ is the ith ordered p-value, (i) its rank, $(m = 8)$ the number of electrode-level tests within each effect family, and α the nominal significance level [51]. Statistical significance was based on FDR-adjusted q-values with q < 0.05.

Effect magnitude for each ANOVA effect was quantified using partial (η) squared:

$$\eta_p^2 = \frac{SS_{effect}}{SS_{effect} + SS_{error}} \quad (8)$$

where (SS_{effect}) is the sum of squares for the effect of interest and (SS_{error}) its corresponding error sum of squares. Partial eta squared was reported alongside the inferential statistics to characterize effect magnitude independently of statistical significance [50].

2.5.4. Secondary Paired Contrasts and Visualization

Secondary electrode-level paired-samples t-tests were conducted separately for male and female participants. Four prespecified within-participant contrasts were evaluated at each electrode: Baseline–Tasting, Tasting–Package, Tasting–Unrelated, and Package–Baseline. The descriptive percentage-change analysis additionally included Package–Unrelated for selected electrodes; this comparison was not part of the inferential contrast set. Each gender-specific inferential family therefore comprised 32 tests.

For each contrast, participant-level paired differences were summarized by their mean $(\overline{d})$ and standard deviation (sd), and the paired-samples statistic was calculated as:

$$t = \frac{\bar{d}}{s_d/\sqrt{n}} \quad (9)$$

Effect magnitude was quantified using paired Cohen's (d_z):

$$d_z = \frac{\bar{d}}{s_d} \quad (10)$$

Cohen's (d_z) standardizes the mean paired difference by the standard deviation of paired differences and was reported with 95% confidence intervals to characterize effect magnitude and precision [50]. Multiplicity was controlled using the same Benjamini–Hochberg procedure defined above, applied separately within the male and female 32-test families; inference used FDR-adjusted q-values with q < 0.05 [51].

FDR-adjusted q-values were summarized using electrode-by-contrast heatmaps. For qualitative topographic visualization, adjusted values were transformed as:

$$-\log_{10}(q) \quad (11)$$

so that smaller q-values received greater visual prominence. The transformed values were interpolated across the eight recorded scalp locations. These scalp maps were treated exclusively as qualitative sensor-space visualizations and not as evidence of cortical source localization or inferential information beyond the sampled electrodes, because sensor-space interpolation does not establish the anatomical location of the underlying neural generators [30], [53].

Percentage-change plots, z-score displays, heatmaps, forest plots, and topographic maps were treated as complementary descriptive visualizations rather than substitutes for the prespecified inferential analyses. Percentage-change values remained descriptive

because relative ratios may become unstable when the reference-condition mean approaches zero after recording-wide centering.

Formal normality testing using the Shapiro–Wilk test and homogeneity-of-variance testing using Levene's test were not performed. Statistical analyses were conducted in MATLAB and JASP (Version 0.97.0). The analytical hierarchy comprised the primary mixed-design ANOVA, gender-specific FDR-adjusted paired contrasts with effect-size estimation, and subsequent descriptive percentage-change, z-score, heatmap, forest-plot, and topographic visualizations.

3. RESULTS

All 50 participants in the final sample contributed complete data for the four retained analytical conditions at all eight recorded electrodes. Approximately 120,000 temporal samples per channel per participant were available in the continuous recording before condition-level aggregation. No analyzed participant or retained condition epoch was excluded, and no persistently noisy channel was removed or interpolated. Accordingly, the primary and secondary analyses were performed on the complete participant-level dataset without missing condition cells.

3.1. Descriptive Electrode-level Changes

Figures 3-5 present the focused descriptive percentage-change plots for F3, F4, and C3, as specified in Section 2.5.2. The 40% dashed line is a visual reference only and does not represent an inferential threshold. Across these selected electrodes, the largest displayed changes occurred for Package-Unrelated, ranging from approximately 63% at F3 in females to 80% at F4 in males. Baseline-Tasting changes were also prominent, spanning approximately 39%-59% across F3, F4, and C3. These ratios describe relative change from the specified reference condition and are not inferential effect estimates.

Other displayed transitions were less uniform. Baseline-Package was approximately 27%-44% at F3 and C3; Tasting-Package was approximately 40%-42% at F4 and approximately 14%-42% at C3. These sex-stratified percentages are descriptive and do not constitute evidence of gender differences.

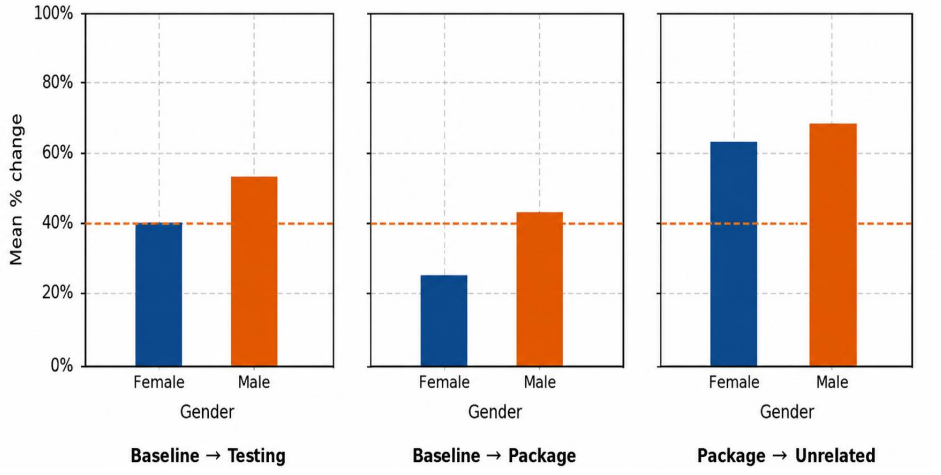

Figure 3. Descriptive percentage changes in participant-level mean EEG amplitude across experimental transitions (electrode F3).

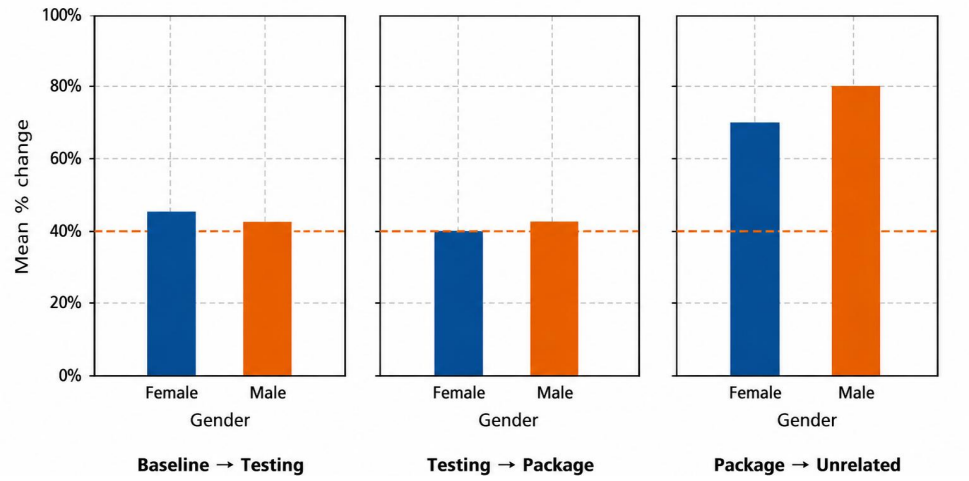

Figure 4. Descriptive percentage changes in participant-level mean EEG amplitude across experimental transitions (Electrode F4).

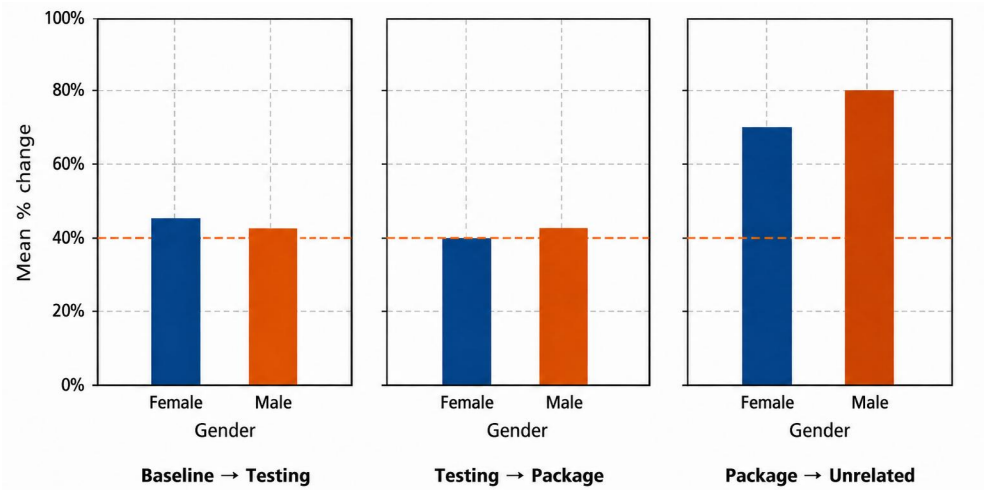

Figure 5. Descriptive percentage changes in participant-level mean EEG amplitude across experimental transitions (Electrode C3).

The participant-level z-scored trajectories in Figure 6 show substantial heterogeneity around each electrode's group mean. Several participants followed markedly different trajectories from the central concentration of observations, indicating that the group pattern was not uniformly expressed by every participant. The standardized display permits comparison of trajectory shape and dispersion across electrodes; it does not indicate calibrated voltage, and it does not alter the participant-level device-native means used for inference.

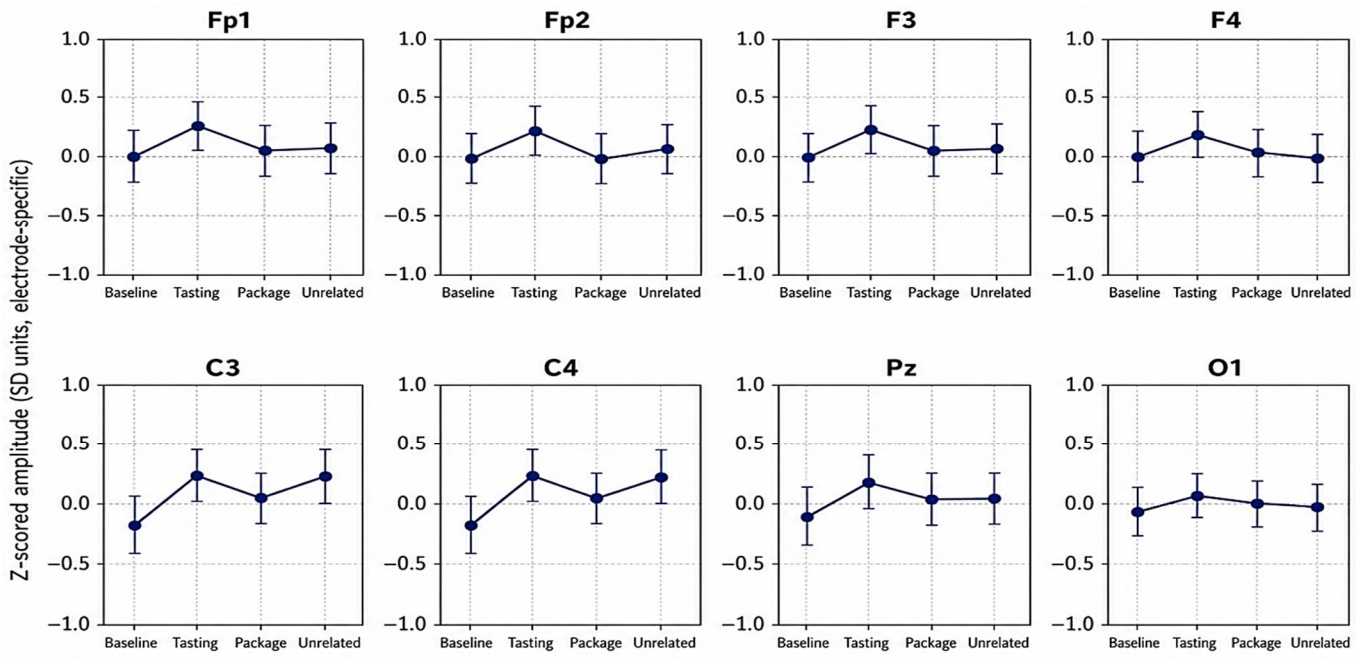

Figure 6. Standardized participant-level EEG amplitude trajectories (z-scores) across retained experimental conditions (n = 50).

3.2. Primary Mixed-design ANOVA

Mauchly's test indicated a violation of sphericity for Condition at all eight electrodes; Greenhouse-Geisser-corrected degrees of freedom are therefore reported for the Condition and Condition × Gender terms. The primary model showed an FDR-significant Condition main effect at every electrode (all $q \leq 0.028$). Partial eta-squared values ranged from (0.073) at O1 to (0.523) at C3, with the next largest values at C4 (0.502) and F4 (0.329). Thus, condition-related variation was detected across the entire sampled montage, with the largest Condition effect-size estimates at C3, C4, and F4 in this dataset.

No Gender main effect remained significant after FDR correction (all $q \geq .082$). Although the unadjusted p values were below (0.05) at Pz, C3, and C4, the adjusted q value was (0.082) at each site. The Condition × Gender interaction was non-significant at every electrode (all unadjusted ($p \geq 0.116$); all ($q \geq 0.477$), and associated effect sizes were small (partial eta-squared (≤ 0.047). The

primary evidence therefore supports amplitude differences among conditions but does not support a statistically reliable difference in the condition pattern between male and female participants.

Table 4. Gender main effect from the electrode-specific mixed-design ANOVA

Electrode	F	df	p	q (FDR)	ηp²
Fp1	0.88	(1, 48)	0.352	0.451	0.018
Fp2	2.89	(1, 48)	0.096	0.153	0.057
F3	0.74	(1, 48)	0.395	0.451	0.015
F4	2.97	(1, 48)	0.091	0.153	0.058
Pz	5.38	(1, 48)	0.025	0.082	0.101
C3	4.97	(1, 48)	0.031	0.082	0.094
C4	6.00	(1, 48)	0.018	0.082	0.111
O1	0.01	(1, 48)	0.919	0.919	0.000

Table 5. Condition main effect from the electrode-specific mixed-design ANOVA (Greenhouse-Geisser corrected)

Electrode	F	df	p	q (FDR)	ηp²
Fp1	7.55	(2.18, 104.60)	<0.001	<0.001	0.136
Fp2	8.80	(1.39, 66.54)	0.002	0.002	0.155
F3	12.56	(2.33, 111.80)	<0.001	<0.001	0.207
F4	23.55	(1.92, 92.33)	<0.001	<0.001	0.329
Pz	12.23	(1.37, 65.58)	<0.001	<0.001	0.203
C3	52.70	(1.49, 71.53)	<0.001	<0.001	0.523
C4	48.40	(1.50, 71.96)	<0.001	<0.001	0.502
O1	3.76	(1.89, 90.72)	0.028	0.028	0.073

Table 6. Condition × Gender interaction from the electrode-specific mixed-design ANOVA (Greenhouse-Geisser corrected)

Electrode	F	df	p	q (FDR)	ηp²
Fp1	1.03	(2.18, 104.60)	0.365	0.539	0.021
Fp2	1.82	(1.39, 66.54)	0.179	0.477	0.037
F3	0.17	(2.33, 111.80)	0.876	0.876	0.003
F4	0.91	(1.92, 92.33)	0.404	0.539	0.019
Pz	1.27	(1.37, 65.58)	0.276	0.539	0.026
C3	2.35	(1.49, 71.53)	0.116	0.477	0.047
C4	1.85	(1.50, 71.96)	0.174	0.477	0.037
O1	0.18	(1.89, 90.72)	0.822	0.876	0.004

3.3. Secondary Paired Contrasts and Effect Sizes

The gender-stratified in Table 7 and 8 summarize the four secondary paired contrasts. In males, F3, F4, Pz, and C4 were FDR-significant in all four contrasts. Fp1 was significant for Baseline-Tasting, Tasting-Package, and Package-Baseline but not Tasting-Unrelated (q = 0.664). Fp2 was significant for Baseline-Tasting and Package-Baseline; C3 was significant in all contrasts except Tasting-Package (q = 0.794); and O1 was significant only for Baseline-Tasting (q = 0.006). Within male participants, F3, F4, Pz, and C4 therefore reached FDR-adjusted significance in all four tested paired contrasts, whereas the remaining electrodes showed significance in a smaller subset of contrasts.

In females, Baseline-Tasting was significant at six electrodes (Fp1, Fp2, F4, Pz, C3, and C4), whereas F3 (q = 0.125) and O1 (q = 0.085) were not significant. Package-Baseline was significant at Fp2, F4, Pz, and C3. Tasting-Package was significant only at Fp1, and Tasting-Unrelated was significant at Fp1, F4, and C4. O1 was not significant in any female contrast; its smallest adjusted value was observed for Package-Baseline (q = 0.060). These results identify the paired condition comparisons that reached FDR-adjusted significance within each gender-stratified analysis. Differences in the number or distribution of significant within-group contrasts do not constitute evidence of a between-gender effect and should be interpreted in conjunction with the non-significant Condition × Gender interaction.

Table 7. FDR-adjusted q values for secondary paired contrasts in male participants (n = 25).

Electrode	Baseline–Tasting	Tasting–Package	Tasting–Unrelated	Package–Baseline
Fp1	<0.001**	0.0431*	0.6636	0.0431*
Fp2	<0.001**	0.3840	0.7501	0.0029**
F3	<0.001**	<0.001**	<0.001**	0.0013**
F4	0.0013**	0.0129*	0.0017**	0.0012**
Pz	<0.001**	<0.001**	0.0017**	<0.001**
C3	<0.001**	0.7935	0.0122*	<0.001**
C4	<0.001**	<0.001**	<0.001**	0.0269*
O1	0.0062**	0.4868	0.6636	0.1463

Note. ** q < 0.01; * 0.01 ≤ q < 0.05 Benjamini – Hochberg

Table 8. FDR-adjusted q values for secondary paired contrasts in female participants (n = 25).

Electrode	Baseline–Tasting	Tasting–Package	Tasting–Unrelated	Package–Baseline
Fp1	0.0104*	0.0114**	0.0297*	0.7144
Fp2	0.0104*	0.9487	0.9607	0.0104*
F3	0.1249	0.4964	0.3032	0.4148
F4	<0.001**	0.1588	0.0151*	<0.001**
Pz	0.0115*	0.7144	0.7816	<0.001**
C3	<0.001**	0.9510	0.4052	<0.001**
C4	0.0013**	0.3032	0.0013**	0.6568
O1	0.0852	0.2405	0.8254	0.0601

Note. ** q < 0.01; * 0.01 ≤ q < 0.05 Benjamini – Hochberg

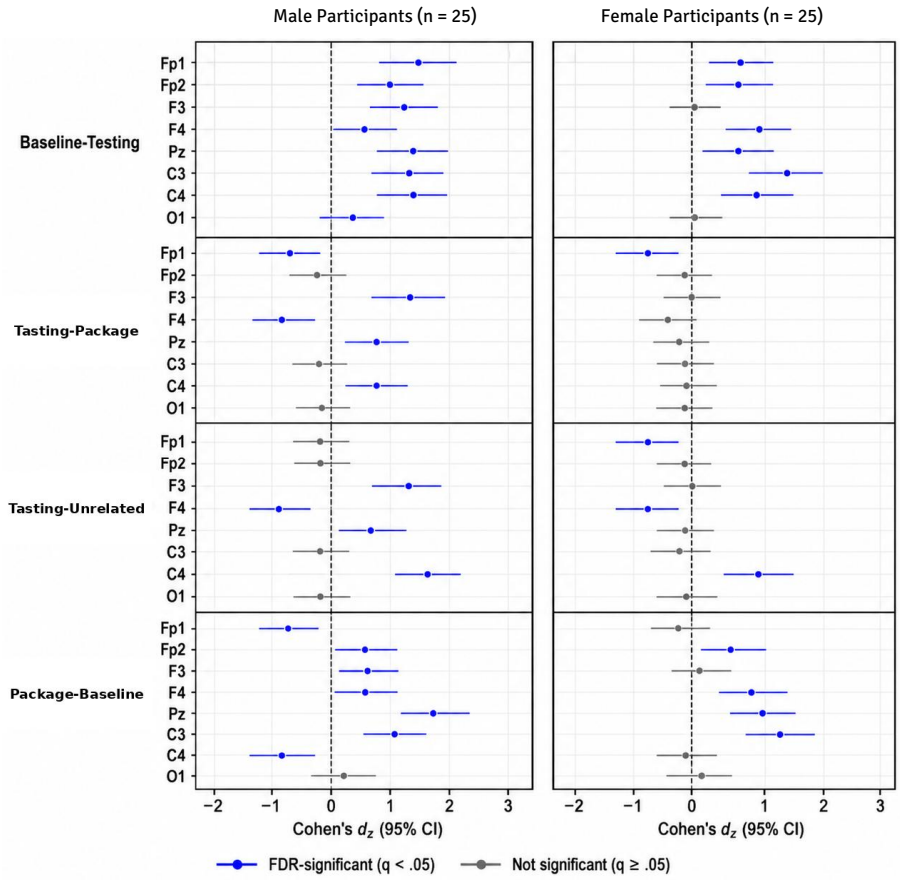

Figure 7. Paired Cohen's d_z effect sizes and 95% confidence intervals by electrode and contrast. Blue markers denote FDR-significant comparisons; grey markers denote comparisons (q ≥ 0.05).

The forest plot in Figure 7 shows paired Cohen's dz estimates with 95% confidence intervals for the same contrasts. The largest reported effect was observed in males at Pz for Package-Baseline (dz = 2.40, 95% CI [1.57, 3.22]). Female estimates were generally more variable at F3 and O1. Comparisons that did not survive FDR correction generally had confidence intervals crossing zero, consistent with the heatmap classification. The effect-size display therefore reinforces the strongest paired changes while also making the uncertainty of weaker estimates visible.

3.4. Spatial Display of Electrode-level Significance

Figure 8 interpolates -log10(q) values between the eight recorded electrodes for qualitative display. In males, Baseline-Tasting was FDR-significant at all eight sites. Tasting-Unrelated was significant at F3, F4, Pz, C3, and C4, while Package-Baseline was significant at seven sites, with only O1 non-significant (q = 0.146). In females, Baseline-Tasting was significant at six sites and Package-Baseline at four sites, matching the heatmap results.

Across both groups, O1 showed the fewest FDR-significant secondary contrasts. This pattern is an electrode-level observation only. Six of the eight channels sampled frontal or central scalp positions, whereas the montage contained one parietal channel and one occipital channel. The interpolation therefore does not establish spatial dominance, regional cortical engagement, or neural-source localization.

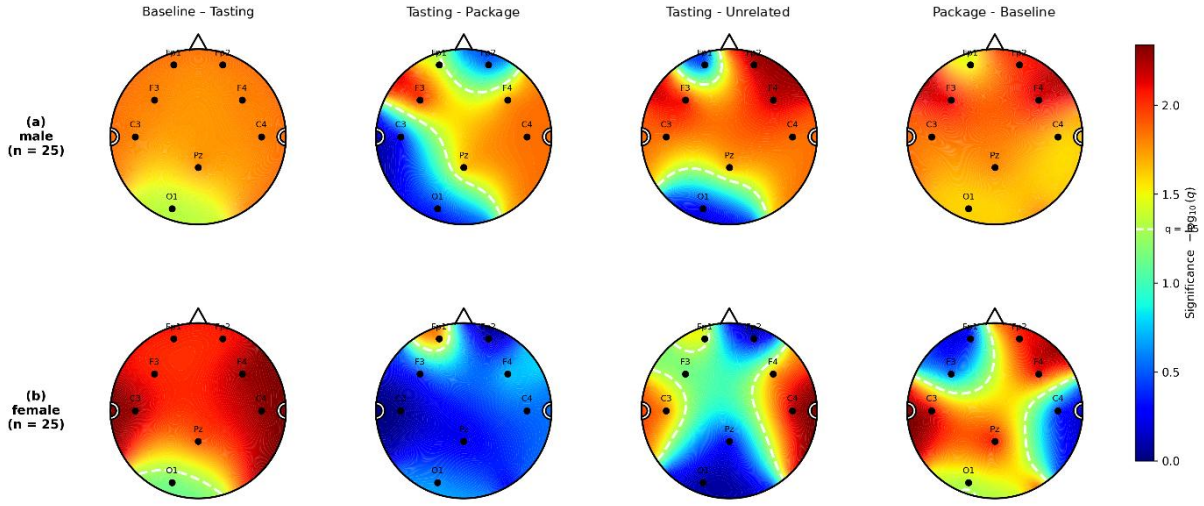

Figure 8. Sensor-space visualization of FDR-adjusted q-values across EEG electrodes and experimental contrasts.

The primary finding was a significant Condition effect at all eight electrodes after Greenhouse–Geisser and FDR corrections, indicating that participant-level mean device-native EEG amplitude varied among the four retained conditions. The omnibus effect does not imply that every condition pair differed, and secondary paired contrasts showed that contributing transitions varied across electrodes and groups. Because conditions followed a fixed sequence, results represent sequence-associated changes in sustained electrode-level amplitude, not causal effects uniquely attributable to taste, package identity, memory, familiarity, or novelty.

Baseline–Tasting produced the broadest FDR-significant distribution, involving all eight electrodes in males and six in females. Yet this contrast does not isolate taste: Tasting was recorded after swallowing while participants viewed the branded package, whereas Baseline combined eyes-open white-enclosure and eyes-closed epochs. Because these resting states differ electro-physiologically [43], the contrast reflects a transition from composite baseline to post-tasting package viewing. Cross-modal taste–visual EEG findings provide contextual plausibility [42], without changing this interpretation.

Significant Package–Baseline and Tasting–Unrelated contrasts, with relatively large descriptive Package–Unrelated changes at selected electrodes, also indicate amplitude differences across package-related transitions. They neither identify mechanisms nor demonstrate brand preference, taste-memory reinstatement, familiarity, expectancy, novelty, liking, purchase intention, or consumer choice. Although fMRI studies indicate that brand information or anticipated identity can modulate preference- or gustatory-related responses [15], [16], these provide only contextual plausibility.

Gender-stratified contrasts differed descriptively in their distributions of FDR-significant comparisons. However, neither the Gender main effect nor the Condition and Gender interaction remained significant after FDR correction. Thus, no reliable evidence indicates different condition-response patterns between males and females; stratified analyses are within-group

decompositions of the Condition effect, not evidence of between-gender differences.

C3, C4, and F4 had the largest partial eta-squared estimates for Condition, while O1 had fewer FDR-significant secondary contrasts than most sites. These are sensor-level observations only. The limited, spatially uneven eight-electrode montage cannot establish cortical dominance, differential regional engagement, or source-localized activity, consistent with reporting guidance and evidence that spatial sampling density affects localization accuracy [30], [31], [53].

4. DISCUSSION

This exploratory study found that sustained participant-level EEG amplitude differed across four successive stages of a familiar commercial-food encounter. Condition was FDR-significant at every sampled electrode (all $q = 0.028$), with partial eta-squared ranging from 0.073 at O1 to 0.523 at C3 and the largest effects at C3, C4, and F4. Neither the Gender main effect nor the Condition x Gender interaction survived FDR correction; interaction estimates were small at all electrodes (partial eta-squared ≤ 0.047; adjusted $q \geq 0.477$). The direct interaction, rather than differences in subgroup-specific significance, is the appropriate test of whether male and female response profiles differed [65]. The primary evidence therefore supports condition-related differentiation across the fixed sequence, not a gender-specific response or a localized cortical signature.

Secondary contrasts showed that no single transition drove the omnibus effect. Baseline-Tasting produced the most spatially consistent adjusted findings, whereas Tasting-Package, Tasting-Unrelated, and Package-Baseline varied by electrode and subgroup. The largest paired estimate occurred in males at Pz for Package-Baseline (Cohen's dz = 2.40, 95% CI [1.57, 3.22]), but this isolated result does not override the non-significant interaction. Significant effects from modest subgroups may also be imprecise or overestimated [66]. The dispersed participant trajectories indicate a detectable but heterogeneous sequence-associated change, not a uniform trajectory or a single established physiological mechanism.

4.1. Multisensory Food and Packaging

Condition effects accord with multisensory integration across gustatory, olfactory, somatosensory, thermal, auditory, and visual inputs [2], [3]. Extrinsic and packaging cues affect expectations, taste, sweetness-sourness judgments, behavior, emotion, and willingness to pay [5], [6], [11], [14], [67]. Yet fixed order conflated residual taste, package information, familiarity, task demands, and time. Controlled studies identified EEG correlates of food similarity and decoded taste quality from high-density patterns [42], [68]; this study measured sustained means across prolonged stages. The exploratory findings show sequence-associated amplitude differences, not N400, gamma, taste-code, similarity, or other neurometric measures. The design cannot distinguish sensory integration from attention, expectation, familiarity, semantics, residual taste, or time; specific attribution is reverse inference [23].

4.2. The Most Consistent Transition

Baseline-Tasting was FDR-significant at all eight electrodes in males and six in females, driving the omnibus effect. It was not a pure tasting contrast: recording followed consumption, flavor persisted, branding remained visible, and the baseline mixed eye states. Residual flavor integrates gustatory and retronasal-olfactory processing [2], whereas eye states differ in EEG power and topography [43]. This heterogeneous-baseline/post-ingestive visual-sensory contrast is compatible with cross-modal differentiation and flavor integration [3], [42], but cannot isolate taste, package viewing, eye state, or engagement. Confirmation requires matched eye state, timed tasting delivery, and hidden branding. Electrode counts do not establish gender effects because they depend on effect size, variance, sample size, and uncertainty; the Condition and Gender interaction was non-significant [65].

4.3. Later Package and Unrelated-Package Transitions

Package-Baseline was significant at seven electrodes in males and four in females. This post-rinse stage showed the same package and required taste recall. Brands and marketing information can alter preference, anticipated gustatory processing, pleasantness, and neural responses [15]–[17], but do not validate taste-memory reinstatement. Recall, familiarity, expectation, liking, and vividness were unanalyzed; prespecified behavioral outcomes strengthen neural construct validity [20], [69].

Tasting-Package was less consistent; only Fp1 survived FDR correction in females. Shared exposure cannot establish memory retention because the stages differed in timing, residual flavor, rinsing, duration, and recall instructions. Tasting-Unrelated was significant at several frontal-central and parietal electrodes in males and at Fp1, F4, and C4 in females. Package-Unrelated changes of approximately 63%–80% were ratios, not standardized effects, potentially inflated by near-zero denominators after centering. The unrelated package was unmatched for luminance, complexity, familiarity, recognition, liking, expectancy, and semantics. Differences may reflect novelty, salience, expectation violation, order, fatigue, or drift alongside learned packaging-taste associations [34], [67], [70].

4.4. Electrode-Level and Measurement Meaning

Condition effects were strongest at C3, C4, and F4 and weakest at O1, but only for recorded sensors. Scalp

potentials depend on volume conduction, reference, conductivity, sensor position, and artifact; amplitude cannot uniquely identify a neural generator [29], [71]. Source localization requires electrode coordinates, forward and inverse models, and adequate coverage [53]. Six frontal-central versus one parietal and one occipital channel made the distribution montage-dependent. Interpolated maps cannot identify cortical or functional networks [30], [31].

The endpoint was a recording-wide-centered mean of filtered, common-average-referenced, device-native ADC amplitudes, not calibrated microvolts or absolute activation. In a sparse montage, one contaminated sensor can affect all channels [33]. No spectral, event-related, connectivity, or time-frequency endpoint was calculated; frontal alpha asymmetry, engagement, reward, or memory interpretations are unsupported. Device dependence precludes cross-device or absolute amplitude comparisons [57].

4.5. Gender Analysis and Participant Heterogeneity

Non-significant Gender and Condition and Gender effects neither demonstrate equivalence nor support different profiles. Only large between-group effects were detectable, and gender was observational rather than mechanistically specified. Claims about sex-linked processing, gendered preference, or differential susceptibility exceed the design. Subgroup significance cannot replace direct interaction testing in modest samples [65], [66].

Participant trajectories were heterogeneous, but stable differences cannot be separated from measurement variability without repeated sessions or reliability estimates. Unanalyzed liking, familiarity, expectancy, recall, similarity, and purchase intention leave this unexplained. Physiological measures should be linked to behavioral outcomes and tested for incremental value beyond self-report [20], [69].

4.6. Methodological Strengths and Contribution

All 50 participants provided complete values for each retained condition and electrode. Participant-level aggregation avoided treating approximately 120,000 samples per participant as independent [72]. The mixed model tested Condition, Gender, and their interaction; Greenhouse–Geisser correction addressed sphericity; multiplicity was controlled; and paired contrasts reported FDR-adjusted values, effect sizes, and confidence intervals. FDR controls the expected proportion of false discoveries among rejections, not each finding's certainty or replicability [51].

The protocol combined consumption with repeated package exposure; portable EEG reduced applied-marketing constraints [25]. Participant-level analysis with a low-density system detected sequence-associated differences. Separating sensor evidence, descriptive displays, and inference follows reproducible EEG guidance and limits translation into consumer constructs [24], [30], [31], [69]. These strengths support auditability and hypothesis generation but do not remove fixed-order confounding.

4.7. Limitations of Study

The fixed condition order confounded each stage with elapsed time and cumulative exposure. Habituation, expectation, declining residual flavor, fatigue, attentional drift, and recording drift therefore remain plausible explanations. Excluding the post-rinse and Final Baseline epochs prevented assessment of recovery and temporal trends, while the absence of randomization or counterbalancing restricted causal interpretation. Composite conditions and unequal durations also preclude direct attribution to taste, memory, preference, familiarity, novelty, reward, or purchase intention without risking reverse inference [23], [24].

Measurement constraints included eight unevenly distributed dry electrodes, sparse common-average referencing, device-native units, manual markers, one sustained mean per condition, and no source modelling or ICA. Residual artefacts therefore cannot be excluded, particularly because ICA requires adequate channels and transparent validation [31], [73]. Food consumption was not recorded, and normality and homogeneity tests were unreported. Generalizability was limited by single-institution convenience sampling, 50 healthy product-familiar adults aged 20–40 years, one brand, and no repeated-session or independent replication. Equal gender counts did not provide precision for subtle group effects [66].

4.8. Implications for Confirmatory Research

Confirmatory studies should separate tasting and packaging effects through matched eyes-open baselines, blinded tasting, package-only viewing, congruent and incongruent pairings, familiar and unfamiliar packages, and pretested unrelated stimuli. Pretesting should address luminance, complexity, recognition, liking, and flavor expectancy because packaging features convey learned taste associations [67], [70]. Randomization, counterbalancing, washout intervals, repeated baselines, and temporal modelling are required.

Behavioral outcomes should be prespecified, including taste intensity, pleasantness, expectancy, familiarity, similarity, recall, liking, and purchase intention. EEG acquisition should use calibrated units, denser sensor coverage, documented artefact procedures, and theory-based endpoints. Preregistered contrasts, adequate power, reliability assessment, replication, and complete preprocessing documentation are necessary [30], [31]. EEG should complement controlled manipulation and validated behavioral measures rather than replace them [20], [34], [69].

5. CONCLUSION

Sustained device-native EEG amplitude differed across the four stages of the fixed taste-and-package sequence at every sampled electrode after FDR correction, with the largest Condition effects at C3, C4, and F4. Baseline-Tasting was the most consistent transition. Neither Gender nor the Condition and Gender interaction was statistically reliable, and participant-level trajectories were heterogeneous. The findings therefore establish sequence-associated sensor-level differentiation in this sample, not a uniform response or a gender-specific pattern.

Interpretation must remain constrained by the fixed order, composite conditions, sparse eight-channel montage, device-native amplitude scale, residual artefact risk, absence of behavioral validation, and limited external validity. The study supports the feasibility of portable low-density EEG for exploratory food-and-packaging research. Still, it does not establish causal sensory or cognitive mechanisms, cortical sources, taste-memory reinstatement, package preference, or purchase-related effects. Confirmatory research requires randomized or counterbalanced conditions, controlled sensory and visual manipulations, calibrated denser acquisition, prespecified behavioral outcomes, reliability assessment, and independent replication.

ACKNOWLEDGMENTS

The authors acknowledge the support provided by the university, colleagues, and laboratory staff during the conduct of this research. Their assistance and institutional support contributed to the successful completion of the study.

CONFLICTS OF INTEREST

The authors declare that no conflicts of interest are associated with this study. All aspects of the research were conducted with the utmost integrity and transparency.

DATA AVAILABILITY

The datasets utilized and analyzed during this research are available from the corresponding author upon reasonable request.

ETHICAL STATEMENTS

The authors confirm that the study complied with all applicable local laws, ethical standards, and institutional guidelines, including obtaining approval from relevant ethics committees.

FUNDING

This research received financial support from the Department of Science & Technology, Government of Goa, India, specifically for procuring electrodes used in the study.

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