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Single-Channel EEG Denoising and Artifact Removal: A Systematic Scoping Review of Methods, Benchmarking, and Reproducibility

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Abstract

Objective. Single-channel and wearable electroencephalography (EEG) reduce acquisition burden but limit the spatial information available to conventional multichannel artifact-removal methods. We conducted a systematic scoping review of single-channel-compatible EEG artifact removal, linking method operation with artifact coverage, evaluation design, generalisation, deployment, comparison, and reproducibility. **Approach.** Database and citation searches through 15 August 2026 identified 178 articles describing 199 method variants. For learned methods, 303 reported model–dataset evaluations were also examined for data independence, generalisation, and deployment. **Main results.** Most variants were reference-free at inference (179/199), while 13 used a non-neural auxiliary reference. Methods spanned seven operating paradigms, including decomposition, transformed source separation, adaptive correction, and learned reconstruction. Ocular and muscle artifacts were evaluated most often, and 38/178 articles tested simultaneous or overlapping mixtures. Across 380 dataset uses, evaluation centred on controlled signal reconstruction. Known-target fidelity was reported for 170/199 variants, compared with 21 for clean-input or non-artifact signal comparison, 32 for contaminated downstream utility, and three for clean-input downstream non-degradation. Among 81 learned variants, 18 reported source-trained application to an external dataset, device, or site; only 31/303 evaluations explicitly separated test windows or epochs, clean sources, and artifact sources from training. No measured deployment evidence was reported for 122/199 variants. Public method resources were available for 21 variants, while trained weights and explicit licences were uncommon. **Significance.** The review provides a framework for selecting datasets, metrics, comparators, and implementations, and for distinguishing reconstruction, neural preservation, transfer, and wearable deployment evidence.

Keywords: single-channel EEG; artifact removal; EEG denoising; wearable EEG; benchmarking; scoping review; reproducibility; neural preservation

1 Introduction

Electroencephalography (EEG) is increasingly used outside conventional laboratory and clinical environments. Wearable, mobile, low-density, and single-channel systems reduce hardware complexity, setup time, cost, and participant burden, making EEG more practical for brain-computer interfaces (BCIs), neurofeedback, sleep and fatigue monitoring, workload assessment, ambulatory health monitoring, and home-based use. These advantages come with a signal-quality challenge: reduced scalp coverage, dry or semi-dry electrodes, motion, device movement, changing electrode-skin contact, and natural behaviour can all increase artifact burden [1–3].

Artifacts arise from multiple physiological and non-physiological sources. Ocular activity can dominate frontal channels; muscle activity can overlap with beta and gamma frequency ranges; motion can create transient or nonstationary disturbances; and cardiac, contact, drift, electrode-pop, line-noise, and environmental artifacts can distort amplitude, phase, spectral power, and downstream features [4, 5]. Artifact handling is therefore necessary before many EEG analyses, but it is not neutral. A method that suppresses visible contamination may also attenuate or reshape neural information, alter event-related potentials (ERPs), distort rhythmic power, or change BCI-relevant discriminative features. Denoising can therefore be evaluated in terms of both artifact suppression and neural-signal preservation [6–8].

Single-channel artifact removal is not simply a smaller version of multichannel preprocessing. Approaches such as independent component analysis (ICA), artifact subspace reconstruction (ASR), spatial filtering, and multichannel regression benefit from spatial redundancy, stable sensor geometry, calibration data, or reference channels. These conditions are weakened or absent when only one EEG channel is available. Single-channel methods instead rely mainly on the temporal, spectral, statistical, or morphological structure of the observed signal, sometimes with a non-neural reference such as electrooculography (EOG), electrocardiography (ECG), electromyography (EMG), inertial sensing, or contact impedance [6, 9, 10].

Some methods first generate components from one channel through empirical mode decomposition (EMD), variational mode decomposition (VMD), wavelets, singular spectrum analysis (SSA), or a related transform, and then apply ICA, canonical correlation analysis (CCA), or another separation method. These transformed single-channel pipelines differ from conventional multichannel blind source separation (BSS), which operates on spatially distinct EEG channels [6, 9, 10].

The resulting landscape spans adaptive and template-based correction, signal decomposition, transformed source separation, optimisation, and machine-learning (ML) or deep-learning (DL) reconstruction. The approaches also differ in reference requirements, artifact specificity, windowing, parameter selection, online feasibility, and their treatment of clean or minimally contaminated data [4, 9, 11, 12].

Recent reviews answer several complementary questions around EEG artifact handling. Arpaia et al. synthesized artifact detection and category identification for wearable EEG, whereas Seok et al. connected wearable motion artifacts to electrodes, analog front ends, mechanical coupling, and signal-processing mitigation [1, 2]. Ocular correction has been examined through a large systematic review and a practical tutorial spanning regression, ICA, artifact subspace reconstruction (ASR), and deep learning [5, 7]. Method-focused reviews provide greater detail for wavelet denoising and learned reconstruction, including architecture choices, public benchmarks, metrics, generalisation, and real-time considerations [11–13]. Broader reviews organize conventional, decomposition, hybrid, and learned methods around artifact classes, channel and reference assumptions, datasets, metrics, preservation concerns, and implementation constraints [4, 6, 9, 10]. BCI-oriented surveys

add application context and routes to relevant primary studies [3,14,15].

Together, these reviews establish strong foundations in wearable acquisition and detection, artifact-specific correction, method families, datasets, metrics, and BCI applications. However, evidence about method requirements, artifact coverage, evaluation design, transfer, deployment, and implementation remains distributed across method papers, dataset reports, and applications. It is therefore difficult to determine what a published method requires, what its evaluation supports, and whether it can be reproduced or used as a benchmark.

The distinctive contribution of this systematic scoping review is an integrated framework that relates each method’s operation and inference-time requirements to artifact coverage, datasets, metric purpose, validation, transfer, deployment, comparator sources, and available implementation resources. This framework supports method design, benchmark and metric selection, assessment of neural preservation and transfer, and practical reuse. It addresses six questions:

- RQ1.** How have single-channel-compatible methods evolved, and what inference-time inputs and computational paradigms do they use?
- RQ2.** Which artifacts are addressed, including multiple artifact types and simultaneous mixed contamination?
- RQ3.** Which datasets, targets, metrics, and validation layers are used, and what conclusions can these designs support about reconstruction, downstream utility, and neural preservation?
- RQ4.** How are learned-method independence, generalisation, computational feasibility, and practical deployment demonstrated?
- RQ5.** Which comparator implementations recur, and what public method resources, licences, and trained weights are available?
- RQ6.** Which evidence gaps limit method selection, benchmarking, transfer, and practical use, and what priorities follow from the included literature?

2 Methods

2.1 Review design and scope

This systematic scoping review mapped the operation and evaluation of single-channel-compatible electroencephalography (EEG) artifact-removal methods. Reporting followed the Preferred Reporting Items for Systematic reviews and Meta-Analyses extension for Scoping Reviews (PRISMA-ScR) where applicable [16]. A protocol specified the scope, eligibility criteria, and extraction framework before the final synthesis; it was not externally registered. The six review questions are stated in the Introduction.

Eligible primary studies proposed or evaluated an EEG artifact-correction, suppression, denoising, reconstruction, or selective-handling method compatible with one acquired EEG channel. Channel-wise methods applied independently to several electrodes were eligible when they did not use spatial information from other EEG channels. Methods requiring a non-neural auxiliary signal, such as electrooculography (EOG), electrocardiography (ECG), electromyography (EMG), an inertial measurement unit (IMU), or another artifact proxy, were analysed as a separate auxiliary-reference group when the denoised target remained a single EEG channel.

Methods that generated modes, delayed copies, pseudo-observations, or latent components from one acquired channel were treated as transformed single-channel methods. Blind source separation (BSS) or subspace operations applied to these representations were distinguished from conventional multichannel BSS applied directly to several acquired neural channels. Few-channel joint and

conventional multichannel methods were used only as supporting evidence when they provided a methodological boundary, validation reference, dataset contribution, or relevant contrast.

Studies were excluded from the main analysis when they were not EEG studies, lacked a distinct artifact-handling contribution, used denoising only as routine preprocessing for another task, or required multichannel neural spatial information without a channel-wise or single-channel-compatible variant. Reviews, dataset descriptions, hardware studies, detection-only methods, preservation studies, and relevant multichannel contrasts were used as supporting literature and excluded from the numerical summaries.

2.2 Information sources and search strategy

The review combined database searching, backward and forward citation searching, and a targeted update. The initial search was conducted on 15 June 2026 in OpenAlex, PubMed, and Crossref for records published from 2000 to 2026. Four search concepts covered EEG configuration; artifact removal, denoising, correction, or suppression; classical and learned method families; and evaluation design. The configuration terms included single-channel, low-density, wearable, and mobile EEG. Method and evaluation terms covered adaptive filtering, signal decomposition, transformed source separation, optimisation, learned models, datasets, benchmarks, neural endpoints, preservation, generalisation, and real-time deployment. Terms and syntax were adapted to each source.

The same sources were searched again on 15 August 2026 for records published from 2020 to 2026. The update added terms for generative, self-supervised, transformer, and state-space methods; compact and edge implementations; cross-participant, cross-dataset, and cross-device evaluation; unseen or mixed artifacts; downstream neural tasks; and clinical or mobile transfer. Peer-reviewed reports were assessed for inclusion, while preprints were used only as emerging supporting evidence. Backward and forward citation searches used included method studies, dataset and benchmark descriptions, and relevant reviews as starting points. Publications found through citation searching underwent the same screening and eligibility assessment as database records.

2.3 Study selection and eligibility assessment

Records were deduplicated first by digital object identifier (DOI) and then by normalized title and bibliographic details. Titles and abstracts were screened against the eligibility criteria. A missing abstract was not grounds for exclusion, and uncertain channel or reference requirements were assessed from the full text. Full-text eligibility required single-channel compatibility, a distinct method contribution and artifact target, EEG evaluation, and sufficient methodological detail. Reports unavailable in full text were listed separately and were not assessed for eligibility.

Duplicate conference and journal reports and metadata inconsistencies were resolved before extraction so that each publication was counted once. Study-selection counts are reported in the Results and Figure 1.

2.4 Units of analysis and data extraction

Results were reported at three levels. An *article* represented one included publication. A *proposed-method variant* represented a distinct central method or operating configuration within an article. A *learned evaluation* represented one reported evaluation of a model on a dataset under a defined condition. Dataset and metric results were summarized for each reported article–dataset–metric combination, and implemented external comparators were linked to their original or framework publications.

For each article, extraction covered publication details, proposed methods and variants, acquired

inputs, auxiliary or internally derived references, calibration, automation, and artifact targets. We also recorded datasets, clean-target construction, metrics and their purposes, downstream endpoints, comparators, computation and deployment, code, trained weights, data access, and parameters.

The publication timeline used the first online year when it preceded assignment to a journal issue; references use the final issue citation where available.

Numerical results specify whether totals refer to articles, proposed-method variants, or learned evaluations. Supplementary Data S1 provides the included-study table, variant counts, input requirements, artifact targets, and validation-layer classifications.

2.5 Classification framework

Inference-time input requirements were analysed separately from the method's internal computation. Single-channel-compatible variants included direct one-channel processing, internally transformed one-channel processing, and channel-wise independent processing. Additional groups covered methods requiring a non-neural auxiliary reference, target- or oracle-assisted output selection, or an unclear inference input.

A variant was classified as reference-free at inference when one acquired EEG channel was sufficient, no concurrent non-neural reference was required, and no paired clean target from the evaluated sample was used to select components, tune a sample-specific output, or choose the reported reconstruction. Internally generated modes or pseudo-references remained compatible with this definition. Training data, pretrained weights, calibration, manual component decisions, and evaluation-only references were considered separately.

For channel-wise studies, input classification followed the signal used to produce each denoised channel, not the number of electrodes recorded for evaluation. A downstream multichannel analysis did not change the denoiser's input class; joint use of interelectrode information did.

Each variant was assigned one primary computational paradigm according to the operation that estimated the artifact, selected what was removed, or controlled the final reconstruction. The seven paradigms were decomposition with direct coefficient or component editing; transformed source or subspace separation; adaptive-reference, template, event-driven, or physiological-model correction; learned estimation, reconstruction, segmentation, or control; sparse, optimisation, state-space, or parametric modelling; conventional filtering or rule-based correction; and integrated or other methods with no single dominant operation. Secondary transforms and modules were described separately, allowing hybrid architectures to be represented without assigning more than one primary paradigm. For example, decomposition followed by direct thresholding was classified as direct decomposition editing, whereas decomposition followed by independent component analysis (ICA), canonical correlation analysis (CCA), or another decisive separation operation was classified as transformed source separation. Comparators and ablations did not affect this classification.

Artifact targets were determined from the methods and evaluation sections. Ocular, muscle, motion, cardiogenic, slow-drift, electrode-contact or shift, electrical or environmental, and other artifacts were analysed separately. Evaluation of several artifacts in separate experiments was distinguished from simultaneous or overlapping contamination. Unified multi-artifact models were also distinguished from artifact-specific models or checkpoints.

2.6 Evaluation and validation framework

Each evaluated dataset was considered separately and linked to its cited source where identifiable. We distinguished dataset construction, availability, the origin of the clean target, real contaminated EEG, wearable or mobile context, neural or task content, and use for reconstruction, downstream, or preservation analysis. Metrics were associated with the relevant dataset and endpoint, then

grouped by purpose: error, similarity, signal-to-noise, spectral, temporal or phase, detection, task, statistical-neural, and computational measures.

Four independent validation layers were defined. V1, *known-target signal fidelity*, required quantitative comparison with an explicit synthetic, semi-simulated, paired, or proxy clean target. V2, *clean-input or non-artifact signal comparison*, required a separate distortion test after processing clean EEG or non-artifact regions. V3, *contaminated downstream utility*, required evaluation after cleaning through a neural, clinical, or task endpoint, such as brain-computer interface decoding, event-related potential analysis, steady-state visual evoked potential analysis, seizure detection, or sleep staging. V4, *clean-input downstream non-degradation*, required comparison of a downstream endpoint before and after applying the denoiser to clean or minimally contaminated EEG. Each layer indicates whether the corresponding test was performed, not whether its result was favourable. The layers support different claims: reconstruction agreement does not establish real-world utility, and signal-level clean-input similarity does not establish preservation of a neural or task endpoint.

For learned methods, each reported model-dataset evaluation was examined separately. We assessed clean and artifact sources, mixture generation, the timing and unit of data partitioning, participant and session independence, overlapping windows, source reuse, normalisation, model-selection data, external datasets, retraining or adaptation, unseen participants or devices, artifact conditions, and mixed-artifact evaluation. Joint source separation required separation of evaluation windows or source epochs, clean sources, and artifact sources. Participant and session independence were reported separately. Unreported grouping was treated as unclear, not as evidence of leakage.

Generalisation evidence ranged from no assessable independent test, through held-out samples or source epochs and participant- or session-independent evaluation, to source-trained application on an external dataset, device, or site and prospective online or field evaluation. We distinguished source-trained external application from evaluation after retraining, tuning, or target-set adaptation. Deployment assessment considered causal operation, windowed or continuous processing, overlap and stitching, workstation timing, target-device latency or throughput, model size and hardware, online or closed-loop evaluation, and live field or wearable testing. Application to stored wearable recordings was considered offline evaluation.

2.7 Comparator, reproducibility, and synthesis

Comparator analysis included external methods implemented on the evaluated data. Background citations, copied literature values, internal ablations, and modules of the proposed method were excluded. A comparison was classified as protocol-matched when the dataset construction, metric definition, and reported split were aligned.

Cross-paradigm examples were selected from within-study comparisons whose methods, data construction, evaluation reference, endpoints, and outcomes could be identified from the full text.

For reproducibility, we assessed whether each study reported an accessible repository for the proposed method, source code, trained weights, a licence, data access, parameter details, the computing environment, a data-split description, or independent execution. Public dataset links did not count as method code, and code available only on request was considered non-public.

The synthesis was descriptive, with denominators matched to the question: articles, method variants, learned evaluations, dataset uses, or comparator applications. Counts that allow more than one category state the relevant total. No pooled performance estimate or universal method ranking was calculated because dataset construction, target definition, contamination severity, partitions, preprocessing, comparators, and metric definitions varied substantially. Numerical comparisons were limited to sufficiently aligned protocols; other findings were synthesized according to operating assumptions, coverage, and validation purpose.

No aggregate risk-of-bias or methodological-quality score was applied. Instead, the review examined

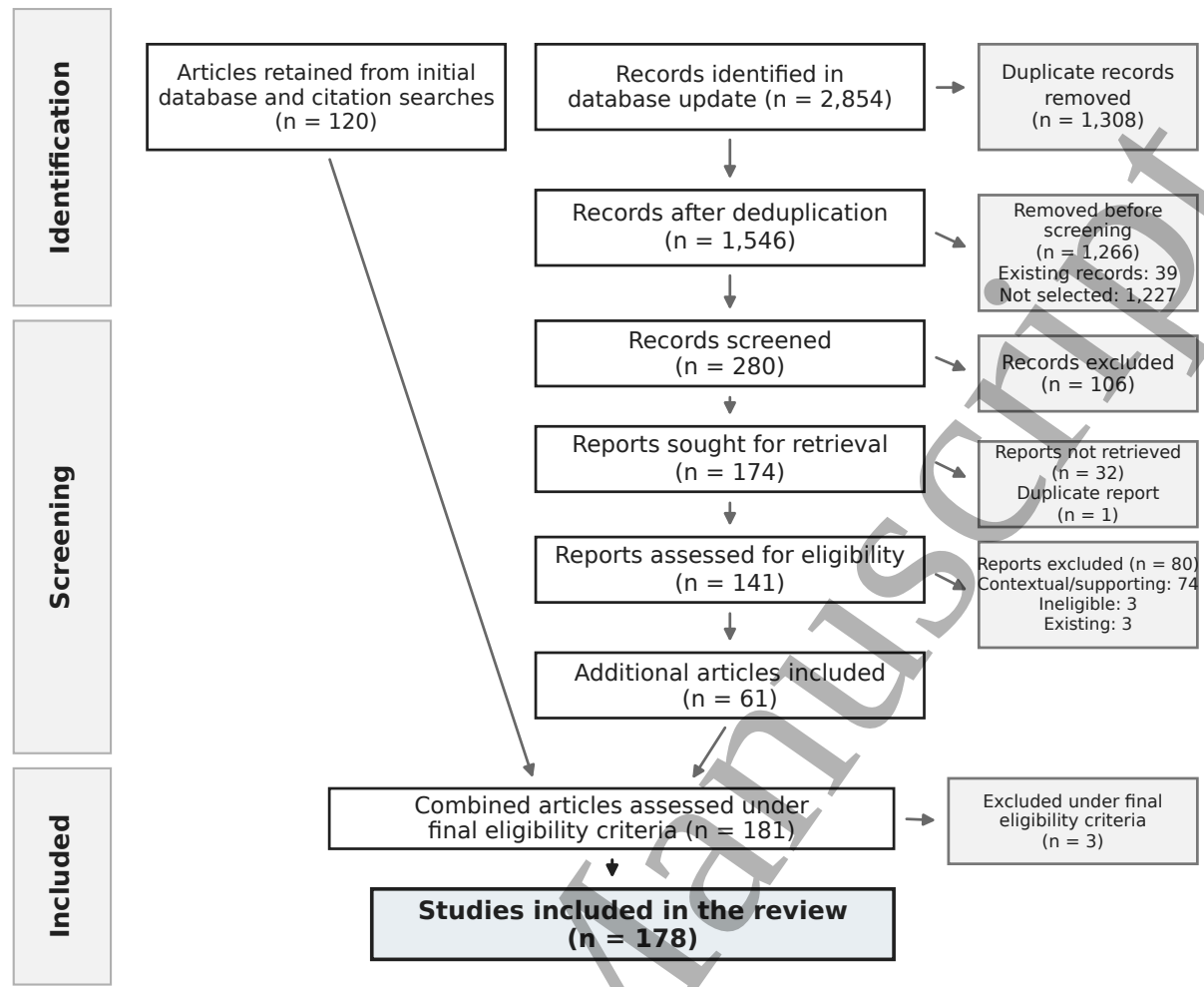


Figure 1: **Study selection.** The initial searches retained 120 articles, and the August 2026 update added 61. Applying the same final eligibility criteria to the combined 181 articles yielded 178 included studies.

design and reporting features that affect interpretation, including target origin, split independence, external evaluation, comparator matching, validation endpoints, deployment, and reproducibility.

3 Study selection and included studies

Figure 1 summarizes study identification, screening, and inclusion. The initial database and citation searches retained 120 articles, and the August 2026 update added 61 non-overlapping articles. The combined 181 articles were assessed against the same final eligibility criteria. Three did not meet these criteria, leaving 178 articles in the review.

The 178 articles described 199 distinct proposed-method variants. For learned methods, 303 reported model–dataset evaluations were examined for data independence, generalisation, and deployment. Article, variant, and evaluation counts describe different aspects of the included literature.

4 Operating landscape

4.1 Inference-time inputs and operating paradigms

The 178 included articles described 199 proposed-method variants. Each variant was assigned one primary operating paradigm from its signal path and inference-time requirements, while artifact classes and validation layers were analysed separately. Most variants were reference-free at inference (179/199). These variants form the single-channel-input, reference-free denoising subset: each can produce an output from one acquired EEG channel. Some were evaluated by independently processing channels from multielectrode recordings; such tests do not establish one-electrode acquisition or a single-channel downstream pipeline. Thirteen used a non-neural auxiliary reference, such as electrooculography, electrocardiography, or inertial sensing, and five used target or oracle information for method selection. Two variants had unclear inference-input requirements. Methods using one EEG channel with a non-neural reference remained single-channel-compatible; methods requiring spatial information from multiple EEG channels were excluded.

Learned reconstruction or control was the largest of seven operating paradigms (80 variants), followed by decomposition and direct editing (42) and transformed source or subspace separation (32). The remaining variants used adaptive, template, or event correction (24); sparse, optimisation, or state-space modelling (13); conventional or rule-based correction (4); or integrated methods (4). Input requirements did not determine the operating paradigm: adaptive methods included both reference-free artifact estimates and explicit non-neural references, while transformed separation generally operated on components generated from one acquired EEG channel. Table 1 summarizes the defining operations and inference requirements.

4.2 Operating principles

The seven paradigms describe the operation that estimates the artifact, selects what is modified, or controls the final reconstruction. Within each paradigm, the included studies used different signal representations, selection rules, reference assumptions, and model architectures.

Decomposition and direct component editing. This paradigm represents the observed EEG at multiple scales, modes, or lagged components, modifies selected elements, and reconstructs the signal.

Wavelet transforms. Wavelet denoising decomposes an EEG trace into time-scale localized coefficients, modifies selected coefficients, subbands, or windows, and reconstructs the signal by inverse transform [17–19]. This makes wavelets naturally compatible with single-channel EEG, especially when transient blinks, bursts, or movement events are localized in time. The main preservation issue is scale overlap: blink activity, slow cortical potentials, ERP deflections, alpha rhythms, and muscle bursts can occupy overlapping coefficient ranges.

Applications included direct discrete or stationary wavelet transform (DWT/SWT) thresholding, maximal overlap discrete wavelet transform (MODWT) or surrogate-based coefficient editing, wavelet-packet decomposition, wavelet-derived pseudo-reference construction for adaptive filtering, blink-zone gated denoising, and wavelet features embedded in broader SSA, decomposition, or classifier pipelines [20–26]. Selective processing was common: several papers cleaned detected windows, artifact-relevant subbands, or candidate components instead of applying uniform filtering to the complete signal [27–31].

Singular spectrum analysis. Singular spectrum analysis embeds a single time series into a trajectory, or Hankel, matrix using delayed copies, decomposes that matrix with singular value

decomposition, and reconstructs the signal by grouping or editing components followed by diagonal averaging [32, 33]. SSA is therefore naturally single-channel-compatible, but its behavior depends on embedding length, component grouping, and the criterion used to identify artifact-related components.

Applications included local SSA, online SSA, local-mobility grouping, sequential SSA, SSA-derived pseudo-reference ANC, overlap/adaptive SSA, entropy-guided SSA, SSA-ASR, and SSA plus total-variation regularization [34–40]. Selection rules included minimum description length, eigenspectrum or frequency separation, local mobility, clustering, fractal dimension, entropy, kurtosis, outlier grouping, variance thresholds, and event timing [41–48].

Empirical mode decompositions. EMD-family methods, including EMD, EEMD, complete ensemble empirical mode decomposition (CEEMD), complete ensemble empirical mode decomposition with adaptive noise (CEEMDAN), and local mean decomposition (LMD), decompose a signal adaptively into intrinsic mode functions (IMFs) or related local amplitude-frequency components, allowing nonlinear and nonstationary artifacts to be represented without a fixed basis or spatial montage [49–52]. Denoising then removes, edits, thresholds, or reuses candidate artifact modes. The recurring practical issues are mode mixing, endpoint effects, ensemble settings, stopping criteria, and artifact-mode selection.

Applications included direct IMF thresholding, adaptive dictionary construction for sparse orthogonal matching pursuit blink estimation, frequency-aware muscle-artifact decomposition, residual blink-segment editing, motion-component separation before Laplacian-of-Gaussian filtering, same-channel pseudo-reference generation for adaptive filtering, pseudo-component generation for artifact subspace reconstruction, and masking-EMD segment editing [45, 53–60].

VMD, VME, and adaptive modes. Variational mode decomposition formulates mode extraction as an optimization problem, estimating band-limited modes centered around adaptive frequencies whose sum reconstructs the observed signal [61]. Variational mode extraction (VME) targets one or more modes of interest; VME-DWT is a prominent single-channel blink example [62]. These methods provide explicit parameters such as mode count, bandwidth penalty, center-frequency initialization, and compactness constraints, making them useful for controlled single-channel artifact separation.

Applications included modified VMD for ECG correction, two-stage VMD with turning-point-count ocular detection, VMD with zero-crossing muscle detection, VMD-based motion separation, and adaptive chirp mode decomposition for ocular artifact removal [63–67]. Their explicit optimisation parameters aid reporting, but the number of modes and the artifact-mode selection rule remain consequential design choices.

Empirical and other adaptive transforms. Empirical wavelet transform (EWT) methods build a filter bank from the signal spectrum, or from predefined frequency boundaries, and reconstruct the signal from selected subbands [68]. In EEG artifact handling, EWT-style methods place artifact energy into a rhythm, empirical band, multiresolution component, or event-bearing subband, clean that part selectively, and recombine it with less-contaminated components. Their reliability depends on boundary selection and artifact-subband identification. Other direct-editing studies used Stockwell, cepstral, circulant-SSA, Fourier, swarm-decomposition, and hybrid representations to localize the signal elements to be modified.

Together, 42 articles contributed 42 decomposition and direct-editing variants. They ranged from threshold-based wavelet and empirical-mode methods to event-gated, entropy-guided, and optimisation-assisted component editing [20–23, 25–31, 34–38, 41, 46, 48, 54–58, 60, 62–67, 69–79]. Across these families, the transform alone did not define the denoiser: component detection, se-

lection, editing, and reconstruction determined what was removed and what neural information could be altered.

Transformed source or subspace separation. Conventional blind source separation uses multiple simultaneous sensors to estimate latent sources. Transformed single-channel BSS hybrids instead first generate multiple observations from one EEG channel through EMD, EEMD, CEEMDAN, VMD, wavelets, SSA, or related decompositions, and then apply ICA, CCA, principal component analysis (PCA), second-order blind identification (SOBI), multiset canonical correlation analysis (MCCA), or related component analysis to those generated observations [80–83]. The components are algorithmic constructs, not independent EEG sensor channels, so the front-end transform and artifact-component selection rule are central to performance.

Examples included EEMD-MCCA with low-autocorrelation muscle-source removal, CEEMD-ICA for blink-component rejection, CEEMDAN-FastICA with sample-entropy ocular-component selection, VMD-SOBI with fuzzy-entropy selection of EOG or EMG components, and SSA-ICA or grouped-SSA-ICA variants for ocular or mixed artifacts [84–89]. CCA-, PCA-, and ASR-based variants extended the same transformed-observation principle to muscle, motion, cardiac, and dry-electrode contamination.

The 32 transformed-separation variants appeared in 26 articles [45, 84–108]. Several studies edited selected components before reconstruction instead of deleting them outright, reflecting the possibility that separated components contain both artifact and neural activity.

Adaptive reference, template, or event correction. These methods estimate an artifact reference, waveform template, recurring event, or local clean-signal model and subtract, regress, or adapt that estimate against the EEG, commonly using adaptive-filter principles [109]. Their operation depends on reference quality and, for event-synchronous methods, reliable event localization. Reference-assisted adaptive noise cancellation remained in scope when the reference was non-neural, such as ECG, EOG, EMG, inertial sensing, or an internally generated artifact proxy.

Examples include smoothed nonlinear energy operator and energy-interval-histogram ECG detection followed by modified ensemble-average subtraction, modified S-transform QRS localization with ensemble-average correction, physiology-based blink modeling, automatic blink detection and correction in frontal single-channel recordings, decomposition-derived pseudo-reference adaptive filtering, event-synchronous adaptive noise cancellation, and adaptive prediction from locally clean EEG [24, 39, 110–115].

The 24 variants in this paradigm appeared in 23 articles and included externally referenced, internally referenced, template-based, event-driven, and physiological-model designs [24, 39, 40, 44, 59, 110–127]. The reference assumption is therefore a defining part of each method.

Learned estimation, reconstruction, or control. Machine-learning and deep-learning methods use learned mappings, classifiers, or control policies to estimate clean EEG, artifact residuals, contamination labels, artifact severity, or denoising parameters [128, 129]. Denoising-autoencoder work provides an established precedent for the noisy-to-clean learning formulation [130]. In single-channel EEG, these models usually operate on temporal windows, time-frequency maps, or engineered features from one observed channel. Their evidence therefore depends on training mixtures, artifact models, and external validation.

Most used supervised reconstruction from constructed noisy and clean pairs, including convolutional neural network (CNN), long short-term memory (LSTM), generative adversarial network (GAN), transformer, autoencoder, U-Net, and temporal convolutional network architectures [131–140]. A smaller subset used learning as part of selective artifact handling, for example by estimating artifact load before classical denoising, segmenting contaminated samples, or gating a correction pipeline

[141–143].

Recent work broadened both architecture and learning objective. Self-supervised pretraining and noisy-target consistency reduced reliance on paired targets, while conditional diffusion, adversarial reconstruction, state-space networks, spiking attention, and deep unfolding introduced different assumptions about target formation and temporal context [144–149]. Other models learned an artifact estimate from a non-neural reference, embedded learned quantities within Kalman filtering or smoothing, or routed correction according to predicted artifact type and severity [150–154].

In total, 71 articles contributed 80 learned variants [131–201]. Architecture names alone did not determine the inference requirement or supported claim; those depended on input references, target construction, data separation, and evaluation design.

Sparse, optimisation, state-space, or parametric modelling. These methods estimate a clean signal or artifact component through explicit priors, constrained objectives, compact dictionaries, low-rank structure, or dynamical models. Implementations included EMD-derived dictionaries with orthogonal matching pursuit, wavelet- or SSA-assisted total-variation correction, morphological component analysis, low-rank template factorisation, and nonconvex sparse regularisation [42,53,202–205]. Twelve articles contributed 13 variants in this paradigm [42,43,47,53,202–209]. Their behaviour was governed by the selected representation and by regularisation, rank, dictionary, or state parameters.

Conventional, rule-based, and integrated methods. Four conventional or rule-based variants used fixed or parameterised correction, including non-local means, EWT-domain Laplacian-of-Gaussian filtering, fractional-order Butterworth filtering, and selective Savitzky–Golay subtraction [121, 210–212]. Four integrated variants combined multi-stage adaptive and non-local filtering, alternative direct and transformed-separation branches, or artifact-type-routed correction [25, 66, 121, 213].

Table 1: **Operating paradigms and inference-time requirements for 199 proposed-method variants.**

Paradigm	Defining operation and representative techniques	Inference-time inputs
Decomposition and direct editing (42)	Represent the signal at multiple scales, modes or lagged components; modify selected coefficients or components; then reconstruct. Includes wavelets, singular spectrum analysis, empirical and variational mode decompositions, and related adaptive decompositions.	reference-free 42
Transformed source or subspace separation (32)	Generate multiple components from one acquired EEG channel and apply ICA, CCA, PCA, SOBI or another separation operation before reconstruction. This group includes 23 transformed-BSS variants.	reference-free 27; auxiliary 2; target-assisted 3
Adaptive, template or event correction (24)	Estimate an artifact reference, waveform template or recurring event and apply adaptive subtraction, regression or localized correction.	reference-free 18; auxiliary 5; unclear 1
Learned reconstruction or control (80)	Learn a clean waveform, artifact residual, mask, latent representation or correction parameter from paired, proxy-labelled or self-supervised data.	reference-free 74; auxiliary 5; unclear 1
Sparse, optimization or state-space modelling (13)	Infer a clean signal using explicit priors, constrained objectives, compact dictionaries, parametric models or state-space estimation.	reference-free 13
Conventional or rule-based correction (4)	Apply fixed temporal, spectral or parametric correction without learned estimation or multicomponent source separation.	reference-free 4
Integrated or other (4)	Combine several operating principles in a configuration that is not adequately represented by one of the preceding paradigms.	reference-free 1; auxiliary 1; target-assisted 2

Reference-free methods need no additional signal at inference; auxiliary methods use a non-neural physiological or motion reference; target-assisted methods use clean or oracle information for method selection. BSS, blind source separation; CCA, canonical correlation analysis; EEG, electroencephalography; ICA, independent component analysis; PCA, principal component analysis; SOBI, second-order blind identification.

4.3 Temporal development

Figure 2 shows each article according to publication year and primary paradigm. Learned methods formed the largest group (71 articles) and were concentrated from 2021 onward. Decomposition, transformed separation, and adaptive correction extended across longer periods and remained active in recent years. Three articles contained multiple primary paradigms at article level.

5 Artifact and evaluation landscape

5.1 Artifact coverage

Artifact targets were analysed separately for each proposed-method variant. Ocular or electrooculographic (EOG) artifacts were evaluated by 127/199 variants and muscle or electromyographic (EMG) artifacts by 90/199. Motion contamination was evaluated by 41 variants and cardiogenic contamination by 32. Technical noise, slow drift or contact disturbance, generic noise, and stimulation artifacts were each represented by 15 or fewer variants. Every operating paradigm included ocular, muscle, and motion evaluation, although their relative emphasis differed (Figure 3A).

Article-level breadth was assessed separately. Forty-six of the 178 articles evaluated more than one artifact type in separate conditions, while 38 evaluated explicit simultaneous or overlapping mixtures. At variant level, 35/199 variants addressed a simultaneous or mixed target. These categories distinguish a method tested against several isolated artifacts from one tested when artifacts coexist in the same signal.

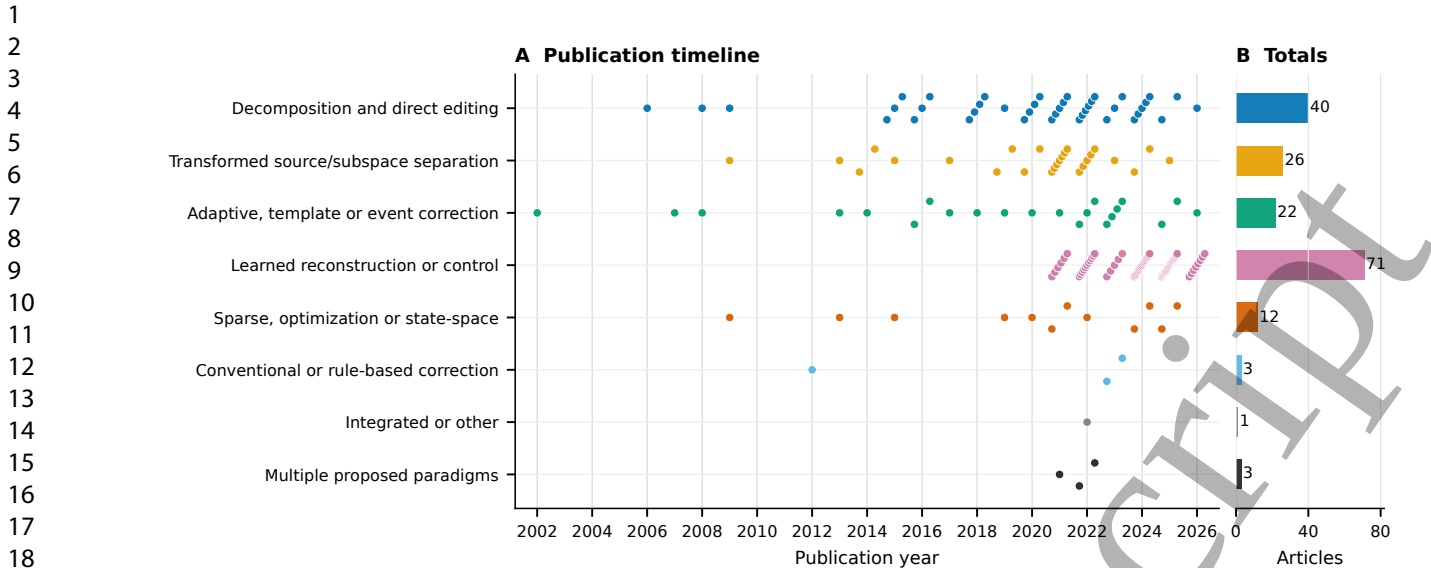


Figure 2: **Temporal development of the literature.** (A) Each point represents one article, grouped by year and article-level paradigm; jitter separates articles from the same year. Three articles with variants in more than one paradigm appear in the “Multiple proposed paradigms” row. (B) Article totals.

5.2 Dataset construction, target status and metric purpose

The 178 articles reported 380 dataset uses. Known-target constructed evaluations were the largest category (176 uses in 130 articles), followed by real recorded signal-level evaluations (137 uses in 111 articles). Real task or neural-endpoint evaluations contributed 29 uses in 27 articles. In a further 30 uses across 14 articles, datasets supplied signals used to construct a separate evaluation dataset. Eight uses across six articles combined contexts or did not report construction clearly.

Constructed evaluations ranged from fully generated signals to mixtures assembled from recorded physiological sources. The latter commonly retain clean EEG before controlled artifact addition, as in the Klados–Bamidis EEG/EOG resource and EEGdenoiseNet [214, 215]. Real motion, BCI, clinical, and physiological collections provide recorded conditions or task endpoints but often lack the latent clean waveform [216, 217]. Table 2 compares the three principal evaluation contexts and links their target status to suitable metrics and supported interpretations. Supplementary Table S1 lists 36 publicly accessible dataset/source families; Supplementary Data S1 links these sources to the included articles and their reported evaluation details.

Reuse was concentrated around a small set of publicly accessible sources. EEGdenoiseNet was used in 55 articles, the Klados–Bamidis semi-simulated EEG/EOG dataset in 37, the BCI Competition IV dataset family in 25, and the Sweeney/PhysioNet motion-artifact dataset in 23. Their different target states explain why they support different combinations of reconstruction, real-data exposure, motion robustness, and downstream evaluation. Supplementary Table S1 provides the complete dataset directory.

Ninety-seven articles used at least two distinct dataset source families, 49 used at least three, and 11 combined constructed, real recorded, and downstream evaluation contexts.

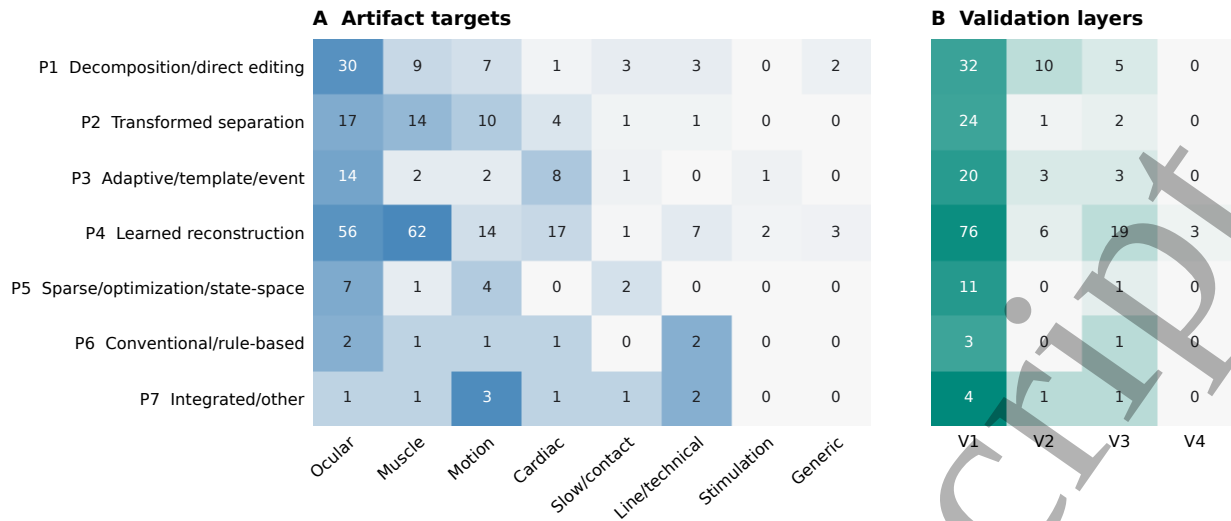


Figure 3: **Artifact targets and validation layers by operating paradigm.** (A) Multi-label artifact counts, with shading showing the percentage within each paradigm. (B) V1 known-target fidelity, V2 clean-signal comparison, V3 contaminated downstream utility, and V4 clean-input downstream testing. Counts indicate test presence, not outcome. BCG, ballistocardiographic; ECG, electrocardiographic; EMG, electromyographic; EOG, electrooculographic.

Table 2: **Principal dataset evaluation contexts, target status, and metric interpretation.**

Evaluation context	Uses / articles	Reference and suitable measures	Supported interpretation	Interpretive limitation
Known-target constructed evaluation	176 / 130	Generated or retained clean waveform. Reconstruction error; signal similarity; signal-quality change; spectral, phase, or temporal fidelity	Agreement with the specified or retained target under controlled contamination	Interpretation depends on physiological source realism, generator assumptions, mixing rules, source reuse, and split construction
Real recorded signal-level evaluation	137 / 111	Often no clean waveform target. Artifact detection/suppression; signal quality; spectral or temporal plausibility; qualitative inspection	Artifact attenuation and signal plausibility in recorded conditions	Without a clean target, waveform recovery error cannot be established directly
Real task or neural-endpoint evaluation	29 / 27	Task labels or neural endpoints rather than a clean waveform. Downstream accuracy; ERP/SSVEP or other neural endpoints; task-level SNR; effect preservation	Utility of the processed EEG for the evaluated task or neural endpoint	Results are task-, cohort-, and pipeline-specific and do not alone establish waveform fidelity

The three contexts account for 342 of 380 dataset uses. A further 30 uses supplied signals for constructing a separate evaluation dataset; eight could not be assigned to one context because construction was mixed or incompletely reported. BCI, brain-computer interface; ERP, event-related potential; SNR, signal-to-noise ratio; SSVEP, steady-state visual evoked potential.

Across all article-dataset evaluations, the studies reported 1,284 metric applications, grouped here by purpose. At article level, signal-similarity metrics appeared in 105/178 articles, reconstruction error or distortion in 101, and spectral, phase, or temporal fidelity in 100. Computation or deployment measures and signal-quality or artifact-suppression measures each appeared in 55 articles; downstream or neural-endpoint measures appeared in 49, artifact-detection measures in 37, and qualitative assessment in 34. Counts overlap because an article could use several metric groups. A metric contributed to V3 only when it evaluated the proposed method on contaminated task or neural data.

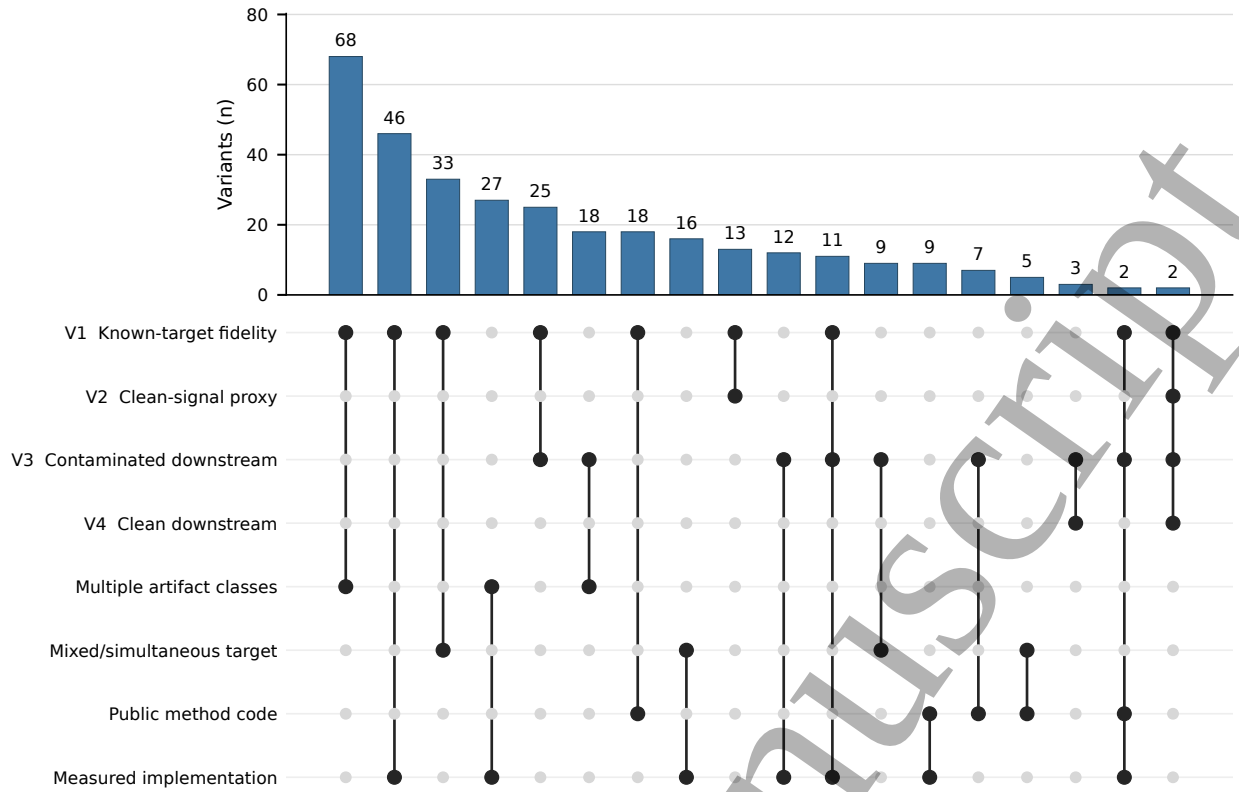


Figure 4: **Selected evidence intersections.** Each bar includes variants meeting the connected black-point criteria, regardless of the grey-point criteria. Public method code indicates an accessible repository for the proposed method. Measured implementation includes reported runtime, throughput, target-device, online, or field testing.

5.3 Validation layers and portfolios

Known-target signal fidelity (V1) was reported for 170/199 variants. Clean-input or non-artifact signal comparison (V2) was reported for 21 variants, contaminated downstream utility (V3) for 32, and clean-input downstream non-degradation testing (V4) for three (Figure 3B). V2 includes signal-level or regional comparisons and is therefore distinct from V4, which tests a downstream neural or task endpoint on clean or minimally contaminated input.

The 32 V3 variants covered ten downstream task families. Only one variant evaluated more than one contaminated downstream task family [71].

Validation was dominated by V1 alone (135/199 variants). The most common two-layer combinations were V1 with V3 (22) and V1 with V2 (10); all other combinations occurred in three or fewer variants. Fourteen variants did not use any V1–V4 test and instead reported outcomes such as artifact detection, qualitative inspection, or implementation measures.

Figure 4 relates validation, artifact breadth, public code, and implementation evidence. The largest intersections paired V1 with multiple artifacts (68 variants), measured implementation (46), or downstream utility (25). Intersections with public code or clean-input downstream testing were uncommon.

6 Learned-method generalisation and deployment

The learned-method analysis comprised 81 variants and 303 reported model–dataset evaluations. Eighty variants belonged primarily to the learned operating paradigm; one transformed-separation

variant also contained a learned stage. Independence and transfer were assessed for each evaluation because a proposed variant could be tested under several conditions.

6.1 Source independence and generalisation

For each evaluation, we examined whether training and test data were separated at three levels: evaluation windows or source epochs, clean-EEG sources, and artifact sources. All three were explicitly separated in 31/303 evaluations. One additional evaluation used distinct datasets, but the complete splitting procedure was not reported. At least one level of separation could not be determined in 166 evaluations; 105 reported source reuse or a split that did not preclude it. Participant independence was established in 23 evaluations and session independence in 27; both were unclear in most reports. Separating only samples or epochs therefore often left participant, recording, or mixture-source independence uncertain.

For each variant, we recorded the strongest generalisation evidence reported. Seven had no identifiable independent test, 49 used held-out samples or source epochs, seven established participant- or session-independent evaluation within a dataset, and 18 applied a source-trained model to an external dataset, device, or site. No variant reported prospective online or field evaluation of a learned model. External evaluation was absent for 47/81 variants. Of the 34 variants applied to an external source, 18 reported source-trained application only, seven reported retraining or tuning only, one reported both source-trained and adapted conditions, and eight did not clearly report the adaptation status.

Among the 81 learned variants, 30 addressed simultaneous or mixed artifacts, 20 reported a downstream or task evaluation, 34 used an external dataset, device, or site, and three were designed specifically for lightweight, edge, or spiking implementation.

6.2 Computational and deployment evidence

Across all 199 proposed-method variants, 122 had no measured deployment evidence. Twenty-four were evaluated offline in a real or wearable application context, and 45 reported measured offline runtime or throughput. Four executed on a target device using stored or test signals, one reported live or online processing without a field demonstration, and three demonstrated live field or wearable operation. Offline runtime or wearable motivation alone did not count as device or live deployment.

7 Comparators and reproducibility

7.1 Implemented comparator practice

External methods were counted as comparators only when they were implemented or applied to the reviewed data. Background mentions, copied values from another publication, and internal ablations were excluded. EEGdenoiseNet NovelCNN and EEMD-ICA were the most frequently implemented comparators, each appearing in 25 articles, followed by 1D-ResCNN in 23 and EEGdenoiseNet SimpleCNN and EEMD-CCA in 20 each (Table 3).

In total, 146/178 articles reported 609 uses of external comparators. Among these, 165 comparisons in 28 articles aligned the data, metric definitions, and reported splits.

The recurrent comparators arose from two main benchmark traditions. Learned baselines were anchored by EEGdenoiseNet and later denoisers such as NovelCNN, 1D-ResCNN, EEGDNet, DuoCL, GCTNet, and DeepSeparator [168, 198, 215, 218–221]. Transformed single-channel separation comparators included EEMD-ICA, EEMD-CCA, EMD-ICA, EMD-CCA, and SSA-ICA [106, 222–224].

Protocol matching was less common than comparator use. Matching rates were higher for several learned baselines than for the recurrent transformed-separation comparators, although they varied across methods (Table 3).

Table 3: **Recurring implemented comparators and protocol matching.**

Implemented comparator	Articles, <i>n</i>	Matched / total	Original or framework source
EEGdenoiseNet NovelCNN	25	10 / 25	NovelCNN method [218]
EEMD-ICA	25	2 / 25	EEMD-ICA method [222]
1D-ResCNN	23	10 / 23	1D-ResCNN method [219]
EEGdenoiseNet SimpleCNN	20	9 / 20	EEGdenoiseNet benchmark implementation [215]
EEMD-CCA	20	2 / 20	EEMD-CCA method [106]
EEGdenoiseNet FCNN	19	8 / 19	EEGdenoiseNet benchmark implementation [215]
EEGDNet	17	10 / 17	EEGDNet method [220]
DuoCL	13	8 / 13	DuoCL method [168]
EMD-ICA	11	4 / 11	EMD-ICA method [223]
EEGdenoiseNet ComplexCNN	9	3 / 9	EEGdenoiseNet benchmark implementation [215]
GCTNet	9	6 / 9	GCTNet method [221]
SSA-ICA	9	0 / 9	SSA-ICA method [224]
DeepSeparator	8	3 / 8	DeepSeparator method [198]
EMD-CCA	8	5 / 8	EMD-CCA method [106]
RNN	7	1 / 7	Generic recurrent-network baseline; implementation sources varied.

Matched/total gives comparisons using the same data, metric definition, and reported split. CCA, canonical correlation analysis; CNN, convolutional neural network; EEMD, ensemble empirical mode decomposition; EMD, empirical mode decomposition; FCNN, fully connected neural network; ICA, independent component analysis; ResCNN, residual convolutional neural network; RNN, recurrent neural network; SSA, singular spectrum analysis.

7.2 Public implementation resources

Public method resources were available for 21 variants across decomposition, transformed separation, adaptive correction, and learned reconstruction. Three repositories stated a CC0 or MIT licence, and trained weights were reported for one resource. On 27 August 2026, all links resolved to resources for the proposed methods; end-to-end execution was not tested.

Supplementary Data S1 lists the studies with publicly accessible method resources, together with the proposed method, URL, licence, and trained-weight status.

8 Discussion

This systematic scoping review shows that single-channel-compatible EEG artifact removal is methodologically diverse, while its evaluation remains centred on controlled signal reconstruction. Evidence for real-data transfer, downstream utility, clean-input preservation, wearable deployment, and reusable implementation is less common and often comes from different groups of studies. Interpretation therefore depends on the reference state, data independence, endpoint, parameter selection, and implementation conditions as well as the denoising method itself.

The temporal and operating analysis indicates expansion rather than wholesale replacement: decomposition, transformed separation, and adaptive approaches remained active as learned reconstruction became the largest and most recently concentrated paradigm. Most variants operated from one EEG channel without an inference-time reference, while smaller groups required a non-neural auxiliary signal or target-assisted selection. This distinction is practical as well as taxonomic: auxiliary-reference methods depend on additional sensing and synchronisation, while target-assisted selection does not by itself define an operational selection rule.

The operating paradigms also present different trade-offs. Decomposition and transformed-separation methods are often train-free and expose intermediate components, but their output

depends on decomposition settings and component-selection rules, and individual components may contain both artifact and neural activity [20,106]. Reference-assisted adaptive or learned estimators can track an observed artifact source, but their performance depends on reference quality and they may attenuate neural activity that covaries with that reference [106,150]. Learned reconstruction can represent nonlinear and multi-artifact mappings, while introducing dependence on training targets, source independence, and transfer conditions [146,153]. Sparse and state-space approaches make model assumptions explicit, but remain sensitive to the chosen prior and parameter settings. Method suitability therefore depends on artifact observability, available inputs, adaptation, endpoint, and latency as well as the operating paradigm.

8.1 Interpreting denoising without biological ground truth

The artifact-free EEG that would have been recorded at the same electrode and moment is generally unobservable during real contamination. Consequently, the references used in denoising studies are approximations with different strengths and limitations. Constructed evaluations may use fully generated signals, recorded clean EEG mixed with isolated physiological artifacts, or recorded EEG with an implanted artifact. Real-data studies may instead use a nearby electrode, an auxiliary physiological channel, a minimally contaminated interval, or a neural or task endpoint. These designs answer different questions, and their interpretation depends on the reference each provides.

Constructed known-target evaluations were the most common, accounting for 176 of 380 dataset uses. They permit direct calculation of error, similarity, and signal-to-noise measures, but their interpretation depends on the retained target, mixing model, artifact source, severity distribution, and independence of the constituent signals. Real recorded evaluations were also common, although they frequently lacked a sample-aligned clean waveform. The 29 real task or neural-endpoint uses addressed functional consequences instead of exact waveform recovery.

Metric choice follows the same distinction. Agreement with a specified reference can establish fidelity to that reference, but it does not establish preservation of an event-related potential (ERP), oscillatory phase, steady-state response, class-discriminative feature, or clinical interpretation. Metrics can also change the apparent ordering of configurations. In the wavelet comparison by Khatun et al., blink-free similarity and time-frequency inspection favoured different configurations [20]. A metric is therefore most informative when its target, interval, preprocessing, artifact severity, and aggregation rule are reported.

8.2 Generalisation across artifacts, sources and tasks

The use of multiple dataset sources was common, but source count alone does not demonstrate generalisation. Sources may share a parent dataset, and a method may be recalibrated, tuned, or retrained for each one. What was held independent and what adaptation occurred are therefore equally important.

Among learned methods, training and test data were explicitly separated at all three levels—evaluation windows or source epochs, clean-EEG sources, and artifact sources—in 31/303 evaluations. One further evaluation used distinct datasets but did not report the complete splitting procedure; in 166 evaluations, at least one level of separation could not be determined. Participant independence was established in 23 evaluations and session independence in 27. Clear reporting of source selection, segmentation, splitting, and mixture construction is needed to determine what a test set is independent from.

Only 18/81 learned variants reported source-trained application to an external dataset, device, or site. EEGDfus reported both source-trained application to the Klados-Bamidis dataset and a condition fine-tuned on part of that dataset, while BCGNet improved performance on unseen participants after participant-specific retraining [146,150]. These conditions answer different ques-

tions: the source-trained test assesses transfer of a fixed mapping, whereas retraining measures performance after adaptation.

Artifact and task breadth provide further dimensions of generalisation. Forty-six articles tested several artifact classes separately, while 38 tested simultaneous or overlapping artifacts. The latter examines interactions when sources coexist in time and frequency. Ocular and muscle artifacts dominated both forms of evaluation, while motion, contact, drift, stimulation, and changing severity were less represented despite their relevance to mobile recordings. Downstream evidence covered several task families, but almost every V3 variant evaluated only one. A prespecified task may be sufficient for an application-specific method; broader claims need evidence across independent sources, conditions, or tasks.

8.3 Hyperparameters and fair comparison

Important settings differ by paradigm: wavelet basis, level, threshold, and shrinkage rule; SSA embedding, rank, and component selection; EMD-family sifting and intrinsic-mode-function selection; VMD or VME mode number, penalty, and initialization; adaptive-filter order, step size, and reference quality; and learned-model windowing, architecture, loss, training data, and checkpoint selection. Khatun et al. demonstrated endpoint-dependent wavelet preferences, while Kaongoen and Jo varied both the decomposition front end and artifact-subspace-reconstruction threshold in their single-channel adaptations [20,45]. Method labels therefore do not fully specify an implementation.

Parameter evaluation should identify whether settings were prespecified, selected on validation data, guided by a target, or optimised on final test data. Several transformed-decomposition studies used paired clean or nearby-electrode references to choose removed components, producing best-case or target-assisted reconstructions that were distinct from an independently deployable selection rule [25,66,106]. VME-DWT selected parameters on excluded development segments and applied fixed settings to held-out semi-simulated and real datasets [62]. Sensitivity or ablation analyses are most informative when they identify a stable operating range, exclude final evaluation data from selection, and state whether settings remain fixed during external tests.

Parameter choices also alter computational cost. Reducing EEGDfus from 500 to 50 diffusion steps lowered reported inference time from 85.365 to 3.892 ms while temporal RRMSE increased from 0.121 to 0.143 [146]. A supporting oracle study compared recoverability and cost across DWT, SSA, VMD, and CEEMDAN using clean-target component weighting [225]. Because the clean signal determined the component weights, this analysis estimated decomposition capacity under oracle selection, not deployable denoising performance. Fidelity–cost results should therefore identify whether a comparison represents an upper bound, an ablation, or an operational method.

Of 609 implemented external comparator uses, 165 aligned the data, metric definitions, and reported splits. Table 3 identifies recurring implementations and their sources. Fair comparison also requires aligned preprocessing, artifact severity, tuning, and endpoints; Table 4 gives selected within-study examples.

Table 4: **Selected within-study comparisons across contrasting evaluation designs.** Values apply to the stated protocols.

Reviewed study and setting	Comparison	Evaluation reference and endpoints	Observed result and interpretation
Huang et al. [146]; ocular mixtures; Klados–Bamidis semi-simulated dataset, 2 s at 200 Hz	Same EEGDfus model, linear schedule: 500 versus 50 steps	Retained clean EEG: temporal RRMSE; inference time	Inference time decreased from 85.365 to 3.892 ms while RRMSE increased from 0.121 to 0.143. This was a within-model platform result, not live wearable latency.
Mourad et al. [53]; blinks; one custom 20 s EEG plus recorded EOG mixture	EMD-OMP versus EEMD-FastICA and EEMD-JADE	Implanted artifact: artifact-estimate CC; no clean-EEG target	CC was 0.93, 0.79 and 0.59, respectively. Comparator outputs were single components selected as closest to the implanted artifact.
Sweeney et al. [106]; induced electrode motion; 24 EEG recordings from six participants	Reference-guided EEMD-CCA versus wavelets	Nearby-electrode proxy: SNR improvement and correlation-derived artifact reduction	SNR improvement was 8.20 versus 8.08 dB; artifact reduction was 52.2 versus 55.3%. Outcomes were metric-dependent and selection used reference information.
Khatun et al. [20]; blinks; seven AF3 recordings from four participants using Emotiv EPOC	DWT/SWT, four wavelets and two threshold rules	Raw/processed blink-free intervals: CC, mutual information and spectral inspection; no latent clean target	Similarity and spectral criteria favoured different configurations. Non-blink similarity assessed signal-level distortion, not neural-task preservation.
McIntosh et al. [150]; cardiac artifacts; auditory oddball EEG-fMRI from 19 participants	ECG-driven BCGNet versus optimal basis set correction	No latent clean target: PSD, evoked responses and stimulus classification	Neural-task evaluation complemented artifact-suppression measures. ECG-correlated neural activity remained a preservation consideration.

BCG = ballistocardiographic; CC = correlation coefficient; CCA = canonical correlation analysis; DWT = discrete wavelet transform; ECG = electrocardiographic; EEG = electroencephalography; EEMD = ensemble empirical mode decomposition; EOG = electrooculography; FastICA = fast independent component analysis; fMRI = functional magnetic resonance imaging; ICA = independent component analysis; JADE = joint approximate diagonalization of eigenmatrices; OMP = orthogonal matching pursuit; PSD = power spectral density; RRMSE = relative root-mean-square error; SNR = signal-to-noise ratio; SWT = stationary wavelet transform.

8.4 Downstream utility and neural preservation

The validation layers address different claims, and their use was uneven. Known-target reconstruction was reported for 170/199 variants, compared with 32 for contaminated downstream utility and three for clean-input downstream testing. Neural utility and clean-input preservation were therefore tested much less often than reconstruction.

Downstream evaluation asks whether denoising changes an endpoint relevant to the intended use. A2DM, for example, evaluated motor-imagery classification after training an artifact-aware reconstruction model, while BCGNet evaluated auditory responses and stimulus classification after electrocardiogram-driven artifact estimation [150, 153]. These studies complement waveform or artifact-suppression evidence, but a downstream improvement does not establish recovery of the unobserved clean waveform. Mechanism-specific risks also remain relevant: a reference-driven estimator may attenuate neural activity that covaries with the reference signal [150].

Clean-input testing addresses a separate concern. Processing EEG that contains little correctable contamination can alter signal structure and statistical conclusions [8]. Signal-level checks in clean or non-artifact intervals (V2) can reveal gross distortion, but they are not equivalent to testing a neural or task endpoint on clean input (V4). Suitable endpoints may include ERP amplitude, latency, morphology, or significant-channel retention; oscillatory power and phase; steady-state visually evoked potential characteristics; BCI performance; or a clinically relevant measure. Where preservation is the question, confidence intervals and prespecified equivalence or non-inferiority margins provide stronger evidence than interpreting a non-significant difference as proof of no effect.

Naturally artifact-free real EEG is difficult to establish. A curated recording with low artifact burden, or a surrogate produced by an independently specified and established multichannel pre-processing pipeline, can provide a practical preservation reference. Such data remain a proxy, and explicit labelling helps interpretation. Methods with broad denoising and preservation claims may therefore combine controlled known-target reconstruction, contaminated downstream utility, and clean-input downstream non-degradation. The appropriate evidence portfolio depends on the intended claim and application.

8.5 Real-time feasibility and wearable deployment

Measured deployment evidence was absent for 122/199 variants. Although 53 variants reported runtime, target-device, or live evidence, only eight reached target-device or live evaluation and four reached live or field testing. Offline processing of wearable data provides application context, but it does not measure operation within a live acquisition pipeline.

Execution time for an isolated segment is only one component of operational latency. Window duration, future context, overlap, stitching, buffering, acquisition and transfer, preprocessing, memory, energy, and sustained operation all affect feasibility. A method may process a segment faster than its duration while remaining acausal or dependent on future samples. LTDNet-EEG reported Raspberry Pi execution on prepared data but used a backward smoothing pass, while EKFNet also reported target-device processing on stored signals [151, 152]. STAND-Net estimated energy from operation counts and activation sparsity instead of measuring a neuromorphic device [148]. End-to-end live testing would extend each of these forms of implementation evidence.

Evidence for real-time wearable use is strongest when a causal or explicitly bounded-look-ahead implementation is measured on the intended hardware, with end-to-end latency, resource use, and continuous operating conditions reported. Offline timing remains informative when its scope is clear.

8.6 Reproducibility and method reuse

Public method resources were available for 21/199 variants, while licences and trained weights were uncommon. Availability improves inspectability and potential reuse, but does not by itself establish reproducibility.

For practical reuse, relevant components include method identity, versioned code, software environment, preprocessing, parameter settings, trained weights where applicable, licence, example data, and an expected-output test. These properties also affect comparator quality: public code reduces reimplement ambiguity, but it does not resolve differences in datasets, splits, tuning, or metrics. Supplementary Data S1 links included articles to publicly accessible method resources.

8.7 Priorities for future studies

The evidence needed for a method depends on the claim being made. Waveform recovery is assessed most directly against a known target or defined artifact reference. Generalisation requires independence across the claimed dimension, such as participants, sessions, sources, datasets, devices, or artifact conditions, with calibration and adaptation reported. Neural utility requires a relevant downstream endpoint; preservation requires clean-input or appropriately qualified proxy evidence; and real-time use requires causal end-to-end measurements. Reproducibility requires sufficient material for independent implementation or execution.

A broadly applicable denoiser may therefore be evaluated with a controlled known-target benchmark, independent real contaminated data, a relevant downstream endpoint, and a clean-input preservation test. Wearable evaluation adds timing and resource measurements on the intended platform, while reuse depends on versioned code, parameters, licences, and weights. A method designed for a narrow artifact or application may use a smaller portfolio when its conclusions remain within that scope.

Selective processing provides one design response to the preservation problem. Artifact detection, type or severity estimation, or an uncertainty score can determine whether a segment is processed and which correction is applied, allowing low-contamination intervals to pass without routine modification [27, 28, 143, 153]. Its value depends on the complete pipeline: missed artifacts, false-positive cleaning, transitions at window boundaries, and routing errors can offset gains from selective operation. Evaluation can therefore report detection performance, the proportion of data processed, clean-input effects, downstream outcomes, window overlap and stitching, and end-to-end latency alongside denoising metrics.

Interpretation and reuse improve when studies report the dataset derivative and parent source, target and mixture construction, artifact severity, participant and source splits, segmentation and splitting order, parameter-selection data, metric aggregation, adaptation status, windowing and causality, and implementation environment. These details support comparison across paradigms without imposing one metric or protocol on methods designed for different purposes.

8.8 Scope and review limitations

The review searched OpenAlex, PubMed, and Crossref through 15 August 2026 and included backward and forward citation searching. This multidisciplinary strategy supported broad coverage but did not include every subscription index. Thirty-two reports from the update could not be retrieved, so their eligibility could not be assessed.

The scope included transformed separation applied to components generated from one acquired EEG channel, channel-wise methods that did not require multichannel neural spatial information, and methods assisted by non-neural references. Methods dependent on multichannel neural spatial

structure were used only as supporting context.

This systematic scoping review mapped methods, operating assumptions, evaluation designs, and implementation resources. It did not apply a study-level risk-of-bias instrument or estimate a pooled effect. Heterogeneity in datasets, targets, preprocessing, splits, metrics, and aggregation prevented an across-study performance estimate. Publications after the final search date were outside the review period.

9 Conclusion

This systematic scoping review mapped 178 articles describing 199 proposed-method variants across seven computational paradigms. Most variants were reference-free at inference, while separate groups used non-neural auxiliary references or target-assisted selection. Ocular and muscle artifacts dominated the literature; motion, contact, drift, technical contamination, and simultaneous mixtures received less attention. Evaluation concentrated on known-target reconstruction, with fewer studies examining downstream utility, clean-input neural preservation, independent transfer, wearable deployment, and reusable implementation.

By relating method operation and inference requirements to artifact coverage, validation purpose, transfer, deployment, comparator sources, and available implementation resources, the review provides a structured basis for selecting, evaluating, and implementing single-channel EEG denoisers. This integrated view clarifies what current evidence supports and where further validation is needed.

Acknowledgements and Declarations

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Data and code availability

No new experimental EEG data were generated for this review. The findings are supported by the included studies and the accompanying supplementary material. Public dataset and method-code repositories discussed in the review are cited in the relevant tables and reference list.

Ethics statement

This review used published literature and public dataset/source records only and did not involve new human or animal data collection; ethics approval was not required.

Conflicts of interest

The authors declare no conflict of interest.

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