

Original Article

Murottal versus white noise as nursing auditory interventions for immediate blood pressure changes in patients with acute myocardial infarction: A quasi-experimental study

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Abstract

Background: In patients with acute myocardial infarction (AMI), pain, clinical uncertainty, and environmental stress may influence short-term hemodynamic measurements. Qur'anic murottal and white noise represent spiritually meaningful and content-neutral auditory approaches, respectively, but their immediate comparative responses remain uncertain.

Objective: To compare pretest-to-posttest changes in systolic and diastolic blood pressure following Qur'anic murottal and white-noise interventions among hospitalized patients with AMI.

Methods: This non-equivalent, two-group pretest-posttest quasi-experimental study included 52 consecutively recruited patients (26 per group) in a cardiovascular care unit in South Sumatra, Indonesia, in June 2026. Blood pressure was measured before and after the assigned intervention using a calibrated digital sphygmomanometer. Within-group mean reductions were assessed using paired t tests; between-group differences in mean reduction were assessed using independent t tests (two-sided $\alpha = 0.05$). Allocation procedures and several intervention and measurement details were not recoverable from archived records.

Results: Mean systolic reduction was 2.692 mmHg (95% confidence interval [CI] 1.878 to 3.506) with murottal and 1.846 mmHg (95% CI 1.213 to 2.479) with white noise. Mean diastolic reduction was 2.000 mmHg (95% CI 1.476 to 2.524) and 1.692 mmHg (95% CI 1.284 to 2.101), respectively. The murottal-minus-white-noise differences in reduction were 0.846 mmHg (95% CI -0.290 to 1.982; $p = 0.141$) for systolic and 0.308 mmHg (95% CI -0.340 to 0.956; $p = 0.344$) for diastolic pressure.

Conclusion: Both groups exhibited small immediate reductions, without a statistically detected difference between interventions. Nonrandomized allocation, baseline diastolic imbalance, and unavailable treatment and measurement details preclude conclusions about independent efficacy, equivalence, or sustained blood-pressure control.

Background

Acute myocardial infarction (AMI) is characterized by acute myocardial injury in the setting of ischemia and constitutes a time-sensitive form of acute coronary syndrome (Thygesen et al., 2018; Sandoval et al., 2020). Cardiovascular disease continues to impose a substantial worldwide burden: the World Health Organization (2021) estimated 17.9 million cardiovascular deaths in 2019. Contemporary guidelines emphasize timely clinical assessment, reperfusion when indicated, antithrombotic therapy, and evidence-based risk management rather than complementary modalities as substitutes for acute treatment (Byrne et al., 2023; Rao et al., 2025). Indonesian clinical and nursing literature similarly addresses the complexity of AMI recognition and management (Sangadji, 2024; Teguh Triyono,

2024; Nuraini et al., 2026; Maliandary et al., 2026; Sangadji et al., 2025).

Hospitalization following AMI may combine pain, apprehension, impaired sleep, unfamiliar sounds, and changing clinical conditions. Such stressors can coexist with short-term variation in cardiovascular measurements; however, a transient fall in blood pressure is not itself evidence of improved myocardial perfusion or long-term prognosis (Rothwell et al., 2010; Basner et al., 2014; Münzel et al., 2018). Nurse-delivered comfort interventions have been investigated in coronary care and other high-acuity contexts, including relaxation and auditory stimulation (Fiska et al., 2021; Irman, 2023; Ulfa, 2024). Their appropriate role is supportive and preference-sensitive alongside standard AMI management (Faisal Sangadji et al., 2025; Wisuda et al., 2026).

Auditory interventions differ in more than the presence or absence of sound. Music may engage attention, emotion, familiarity, and stress-related neurobiological processes, although proposed physiological pathways are not equivalent to demonstrated therapeutic effects in AMI (Chanda & Levitin, 2013; Koelsch & Jäncke, 2015). Meta-analyses have reported improvements in anxiety, pain, or selected physiological measures in cardiac and surgical populations, with substantial variation in intervention content, controls, and dose (Bradt et al., 2013; Hole et al., 2015; de Witte et al., 2020; Chandrababu et al., 2021; Kakar et al., 2021; Lee et al., 2023). Recent cardiovascular syntheses also suggest that responses differ according to the clinical setting and outcome assessed (Meda et al., 2024; Gregorio-Sanz et al., 2025; Tharwani et al., 2025). Findings from stable hypertension or postoperative care cannot be directly extrapolated to patients experiencing AMI (Cao & Zhang, 2023; Mert et al., 2026).

Qur'anic murottal is recited scripture with linguistic content, rhythm, and spiritual significance. For patients who value Islamic practice, familiarity, emotional reassurance, and religious coping may contribute to an experience distinct from listening to neutral sound; this remains a conceptual account rather than a mechanism tested here. Reviews suggest possible psychological benefits of Qur'anic listening or recitation but repeatedly identify heterogeneity and limitations of available studies (Ghiasi & Keramat, 2018; Saged et al., 2022; Moulaei et al., 2023; Owens et al., 2023). Preliminary cardiac and procedural reports include anxiety or physiological endpoints, although direct head-to-head comparisons with nonreligious masking sound are uncommon (Hudiyawati et al., 2022; AbuRuz et al., 2023; Safitri et al., 2024; Luthfiyyah et al., 2025).

White noise offers a complementary comparator because it supplies ongoing acoustic stimulation without words or specific spiritual content. In principle, masking abrupt ward sounds could diminish their perceptual salience, whereas murottal could add meaning and emotional engagement to rhythmic auditory input. An active auditory comparator can therefore address a different question from usual care or silence: whether two distinct forms of listening are associated with different immediate responses. It cannot, without an additional nonauditory group, identify the absolute effect of either intervention relative to rest or standard care. Moreover, the effectiveness and acceptability of continuous noise depend on sound level, exposure, and setting, and

evidence supporting white noise as a sleep aid is uncertain (Warjri et al., 2022; Riedy et al., 2021; Barzegar et al., 2024; Susanti et al., 2025). This is a theoretical justification for the comparison, not a retrospective claim about an undocumented original allocation or comparator-selection process.

The proposed conceptual contrast is therefore between (a) meaningful religious recitation, potentially engaging attention, affect, familiarity, and spiritual coping, and (b) content-neutral broadband sound, potentially reducing the noticeability of intermittent environmental noise. Both may coincide with quiet rest or nonspecific expectancy effects. Neither pathway can be confirmed from blood-pressure recordings alone; anxiety, autonomic function, medication timing, and auditory dose would be needed to examine mediation. Existing literature does not establish a defensible expected magnitude or direction of the difference specifically in hospitalized AMI. The study consequently examined a nondirectional hypothesis: the mean immediate pretest-to-posttest changes in systolic and diastolic blood pressure would differ between groups. Its principal comparative estimands were the between-group differences in mean pretest-to-posttest blood-pressure reduction (murottal minus white noise), rather than the statistical significance of change within each group.

Methods

Study Design

This was a non-equivalent, two-group, pretest-posttest quasi-experimental intervention study. The comparison concerned immediate within-person changes after one of two assigned auditory modalities, with nonrandomized groups and no silence, usual-care-only, or untreated comparator. Reporting was guided by the Transparent Reporting of Evaluations with Nonrandomized Designs (TREND) statement for nonrandomized intervention evaluations (Des Jarlais et al., 2004). The Template for Intervention Description and Replication (TIDieR) framework informed identification of missing details relevant to replicability, without implying that such information was originally documented (Hoffmann et al., 2014).

Setting and Participants

Data collection took place in June 2026 in the cardiovascular care unit of Siti Fatimah Regional General Hospital, South Sumatra, Indonesia. The

archived study report describes a source population of 125 hospitalized patients with an AMI diagnosis. Consecutive sampling was used to assess eligible patients in the order encountered, a nonprobability approach that does not produce population-representative sampling (Willie, 2024).

Eligibility Criteria

Recorded inclusion criteria were a diagnosis of AMI, full consciousness (*compos mentis*), blood pressure described as $\geq 140/90$ mmHg, no hearing impairment, and willingness to participate. Recorded exclusion criteria were hemodynamic instability, communication disorders, or high-dose sedative therapy. These criteria reflect the archived clinical report (Faisal Sangadji et al., 2025; Sweis, n.d.). The qualifying threshold was not operationally defined as systolic ≥ 140 mmHg, diastolic ≥ 90 mmHg, or both, and its source reading and timing were not archived; no interpretation of that threshold is assumed retrospectively.

Sample Size and Group Assignment

The original report records a G*Power 3.1 minimum sample size of 52, divided equally between the *murottal* ($n = 26$) and white-noise ($n = 26$) groups. The underlying test specification, assumed effect size, type I error, statistical power, and attrition assumptions were unavailable, so the calculation cannot be independently reproduced. Patients were allocated to one of two groups until both reached 26 participants. The allocation rule, whether and by whom assignments were made, the time of assignment, any restrictions or balancing procedures, and concealment were not documented. Therefore, group assignment is described as nonrandomized and potentially susceptible to selection by measured or unmeasured prognostic characteristics; a specific sequential, temporal, ward-based, or investigator-directed procedure is not asserted.

Auditory Interventions and Co-interventions

The intervention group listened to an audio recording of Qur'anic *murottal*; the original participant-flow diagram identifies Surah Ar-Rahman. The comparison group listened to recorded white noise. Both interventions were adjuncts to usual medical and nursing care, not replacements for established AMI treatment. No verified record specified session duration, number

or timing of sessions, volume or decibel level, device type, headphones or speakers, provider, environmental noise conditions, or checks of intervention fidelity and adherence. It was also not possible to establish when sessions occurred relative to prescribed cardiovascular medications, analgesia, procedures, or routine rest. Because these unrecorded parameters might affect immediate pressure readings, no standardization is presumed and there is no basis to claim the two stimuli were matched for intensity or exposure.

Outcomes and Blood-Pressure Measurement

Systolic and diastolic blood pressure (mmHg) were recorded with a calibrated digital sphygmomanometer before and after the auditory intervention. The analytic change variable was defined as pretest minus posttest pressure, so a positive value denotes a reduction. The clinically interpretable comparison was the difference between the two groups' mean reductions. Available records do not identify cuff size, arm used, patient positioning, rest interval, number of repeat readings, time of day, or the interval from intervention completion to posttest. These conditions influence blood-pressure measurement and reproducibility (Muntner et al., 2019; Stergiou et al., 2021). Anxiety, pain intensity, medication administration, autonomic indices, and longer-term cardiovascular outcomes were not reported as measured study outcomes.

Statistical Analysis

Reported descriptive analyses included group frequencies and percentages for age, sex, education, and occupation. Paired-samples *t* tests summarized within-group blood-pressure reductions and 95% confidence intervals. The primary comparisons used two-sided independent-samples *t* tests of change scores, with $\alpha = 0.05$. The between-group mean differences, 95% confidence intervals, and standardized mean differences (Cohen's *d*) in the submitted analysis are preserved as reported; the difference is oriented as *murottal* minus white noise. The results are not treated as tests of equivalence or noninferiority. The archived files do not include an individual-level dataset, a correlation/covariance structure, baseline standard deviations, or normality and variance-assumption diagnostics. Although regression or analysis of covariance can address observed baseline differences when appropriate, its

application and interpretation in nonrandomized, imbalanced groups require full data and assumptions; a new adjusted estimate was therefore not generated (van Breukelen, 2006; Twisk et al., 2018).

Ethical Considerations

Approval was granted by the Research Ethics Committee of Universitas Muhammadiyah Ahmad Dahlan Palembang, No. 000298/KEP/Universitas Muhammadiyah Ahmad Dahlan Palembang/2026, on June 5, 2026, valid through June 5, 2027. The archived report states that all participants provided written informed consent before

enrollment, participation was voluntary, and auditory activities complemented routine treatment. The supplied material contains no patient-identifying images or case details.

Results

A total of 125 patients were assessed for eligibility, of whom 73 were excluded (58 did not meet inclusion criteria; 15 declined participation). Fifty-two patients entered the study (26 per group). All 52 reportedly received their assigned modality, completed posttest assessment, and were included in the analysis; no discontinuations or losses to follow-up were recorded (Figure 1).

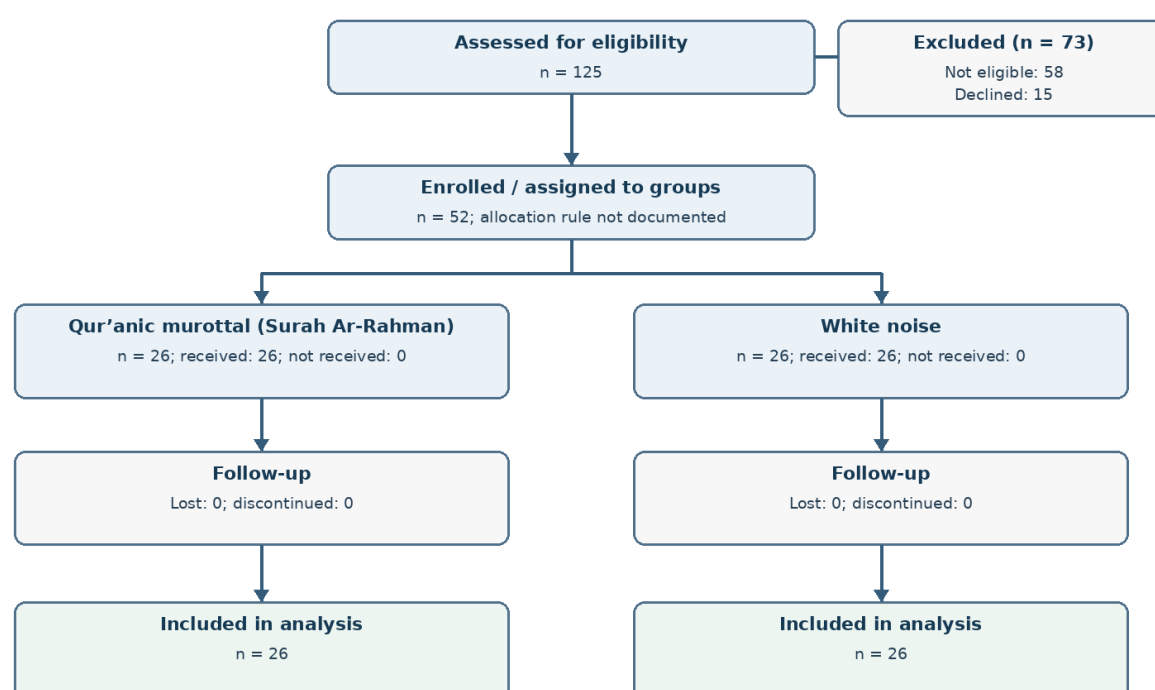


Figure 1. Screening, nonrandomized group assignment, intervention completion, and analysis. The recorded flow does not describe the assignment mechanism.

Both groups comprised 17 men (65.4%) and nine women (34.6%). The 60–74-year age category was most frequent in the murottal group (42.3%), whereas the 45–59-year and 60–74-year categories were equally represented in the white-

noise group (38.5% each). Educational and occupational distributions were not identical (Table 1). These descriptive categories alone are insufficient to establish clinical comparability or absence of selection bias.

Table 1. Recorded baseline demographic characteristics of participants

Characteristic	Murottal (n = 26)	White noise (n = 26)	Total (n = 52)
Age, n (%)			
<45 years	5 (19.2)	6 (23.1)	11 (21.2)
45-59 years	9 (34.6)	10 (38.5)	19 (36.5)
60-74 years	11 (42.3)	10 (38.5)	21 (40.4)
75-90 years	1 (3.8)	0 (0.0)	1 (1.9)
Sex, n (%)			

Characteristic	Murottal (n = 26)	White noise (n = 26)	Total (n = 52)
Male	17 (65.4)	17 (65.4)	34 (65.4)
Female	9 (34.6)	9 (34.6)	18 (34.6)
Education, n (%)			
No formal education	2 (7.7)	3 (11.5)	5 (9.6)
Primary school	4 (15.4)	2 (7.7)	6 (11.5)
Junior high school	3 (11.5)	7 (26.9)	10 (19.2)
Senior high school	13 (50.0)	11 (42.3)	24 (46.2)
Higher education	4 (15.4)	3 (11.5)	7 (13.5)
Occupation, n (%)			
Homemaker	3 (11.5)	1 (3.8)	4 (7.7)
Civil servant/teacher/retired	3 (11.5)	4 (15.4)	7 (13.5)
Self-employed/trader	5 (19.2)	7 (26.9)	12 (23.1)
Farmer/plantation worker/livestock breeder/fisherman	6 (23.1)	10 (38.5)	16 (30.8)
Laborer	3 (11.5)	3 (11.5)	6 (11.5)
Driver	3 (11.5)	0 (0.0)	3 (5.8)
Unemployed	3 (11.5)	1 (3.8)	4 (7.7)

Clinically pertinent baseline characteristics—AMI subtype, time since symptom onset, reperfusion status, hypertension history, cardiovascular drug regimen and administration times, and pain intensity—were not tabulated in the archived source. Likewise, numerical group means and standard deviations for pretest systolic and diastolic blood pressure were not preserved in a self-consistent form. An original comparison reports absolute between-group pretest differences of 3.346 mmHg for systolic pressure ($p = 0.244$) and 5.269 mmHg for diastolic pressure ($p = 0.004$). The diastolic difference indicates a material baseline imbalance, not an intervention response. These archived comparison figures cannot be fully reconciled with all other displayed

summary values, so missing pretest means and dispersions are not back-calculated. Differences in baseline characteristics are therefore described as unadjusted and potentially clinically consequential, not simply categorized by statistical significance.

Both groups had positive pretest-minus-posttest mean changes in systolic and diastolic pressure. The four within-group paired tests were statistically significant (all $p < 0.001$), but these tests do not identify whether the interventions themselves caused the changes because time, routine care, or shared nonspecific conditions may also influence blood pressure (Table 2).

Table 2. Reported immediate within-group pretest-to-posttest blood-pressure reductions

Group	Outcome	Mean reduction (mmHg)	95% CI	t	p
Qur'anic murottal	Systolic	2.692	1.878 to 3.506	6.812	<0.001
Qur'anic murottal	Diastolic	2.000	1.476 to 2.524	7.868	<0.001
White noise	Systolic	1.846	1.213 to 2.479	6.008	<0.001
White noise	Diastolic	1.692	1.284 to 2.101	8.538	<0.001

The direct change-score comparisons did not detect a statistically significant difference between modalities. The systolic difference in mean reduction (murottal minus white noise) was 0.846 mmHg (95% CI -0.290 to 1.982; $p = 0.141$; Cohen's $d = 0.415$), and the diastolic difference was 0.308

mmHg (95% CI -0.340 to 0.956; $p = 0.344$; Cohen's $d = 0.265$) (Table 3). Both confidence intervals cross zero; the results neither demonstrate superiority nor establish that the interventions are equivalent.

Table 3. Primary unadjusted between-group comparison of mean immediate blood-pressure reduction

Outcome	Murottal reduction	White-noise reduction	Difference in reduction	95% CI	Cohen d	p
Systolic	2.692	1.846	0.846	-0.290 to 1.982	0.415	0.141
Diastolic	2.000	1.692	0.308	-0.340 to 0.956	0.265	0.344

The archived posttest means were 135.73 and 138.23 mmHg for systolic pressure and 78.58 and 83.54 mmHg for diastolic pressure in the murottal and white-noise groups, respectively. These posttest values are not substitutes for comparable baseline-adjusted intervention effects. Given the pretest diastolic imbalance and incomplete original allocation and medication information, the primary unadjusted comparisons remain susceptible to confounding.

Discussion

The central finding is that both groups experienced small immediate pretest-to-posttest reductions in blood pressure, but the between-group change-score comparisons did not provide statistically significant evidence of a difference. The comparative estimates—0.846 mmHg for systolic and 0.308 mmHg for diastolic reduction—should be considered together with their confidence intervals, rather than interpreted through the within-group p values alone. A lack of detected difference must not be described as evidence of clinical equivalence, particularly in a modest-sized, nonrandomized study without a prespecified equivalence margin (Greenland et al., 2016).

The tiny absolute between-group differences are unlikely, by themselves, to support changes in acute cardiovascular management. Importantly, they are also small relative to the uncertainty introduced by ordinary blood-pressure measurement conditions. Systematic evidence identifies patient posture, cuff positioning, rest, observation, and measurement procedures as sources of variability, even when an oscillometric device is used (Kallioinen et al., 2017; Muntner et al., 2019; Stergiou et al., 2021). Accordingly, statistical significance in a within-group comparison should not be conflated with a clinically important treatment effect. The study did not assess sustained pressure trajectories, infarct size, recurrent ischemia, arrhythmias, or major cardiovascular events.

Several processes other than the auditory stimulus itself could explain pretest-to-posttest declines in both groups. A quiet observation period may reduce situational arousal; analgesics, antihypertensives, nitrates, other cardiovascular therapies, or procedures may change blood pressure during hospitalization; underlying hemodynamics can fluctuate; and selection based on elevated baseline readings creates a possibility of regression to the mean. Because the present study included two active listening conditions but no usual-care-only or silence group, it cannot separate intervention-specific effects from these shared influences. The nonrandomized allocation and the original baseline diastolic imbalance further weaken causal attribution, particularly when medication and clinical severity variables are unknown (Sterne et al., 2016; Austin, 2011; Hernán & Robins, 2016).

The findings can nonetheless be located within wider evidence on auditory care. Reviews of music and sound-based interventions in cardiac, surgical, and critically ill populations describe improvements in anxiety and related symptoms under some conditions, but have heterogeneous exposure protocols, patient characteristics, and outcomes (Bradt et al., 2013; Umbrello et al., 2019; Kakar et al., 2021; Chandrababu et al., 2021; Lee et al., 2023). Meta-analyses in hypertension or cardiac procedures may report larger blood-pressure changes than observed here, yet differences in underlying disease, treatment duration, comparator, and outcome measurement prevent treating such findings as directly applicable to hospitalized AMI (Cao & Zhang, 2023; Tharwani et al., 2025; Meda et al., 2024). A trial of lemon inhalation aromatherapy in AMI likewise illustrates the diversity of adjunctive sensory approaches, but it does not establish auditory-specific effects (Rambod et al., 2020). A conference abstract addressing music in cardiac intensive care also illustrates continuing interest but should be interpreted with the caution appropriate to preliminary reporting (Gao et al., 2026).

Murottal and white noise differ in proposed experiential mechanisms, not in a mechanism directly demonstrated by this study. For a consenting patient who finds the Qur'an meaningful, murottal might offer familiar rhythm, positive expectations, focused attention, or spiritual coping. Reviews of Qur'anic interventions document promising anxiety-related outcomes but underscore the importance of setting, personal religious preferences, and methodological quality (Ghiasi & Keramat, 2018; Saged et al., 2022; Moulaei et al., 2023; Owens et al., 2023). Cardiac or procedural reports have investigated Qur'anic listening in contexts such as coronary artery bypass surgery and percutaneous coronary intervention, without establishing that its religious content independently determines an acute hemodynamic change (AbuRuz et al., 2023; Hudiawati et al., 2022; Luthfiyyah et al., 2025; Safitri et al., 2024). The broader psychophysiology of music offers plausible hypotheses concerning mood and arousal but does not verify autonomic mediation in this dataset (Chanda & Levitin, 2013; Koelsch & Jäncke, 2015; Irman, 2023).

For white noise, the proposed pathway is acoustic masking: continuous sound may make irregular environmental sounds less conspicuous. That idea is distinct from the expectation that spiritually familiar recitation reduces emotional distress. However, noise itself can influence health through complex auditory and nonauditory pathways, and a systematic review found very low-certainty evidence regarding continuous noise for sleep (Basner et al., 2014; Münzel et al., 2018; Riedy et al., 2021). ICU and AMI studies of white noise have focused primarily on sleep or anxiety, not a controlled, dose-matched evaluation of short-term blood-pressure reductions (Warjri et al., 2022; Barzegar et al., 2024; Susanti et al., 2025). This underscores why the absence of verified sound levels, device characteristics, and dose information matters for both reproducibility and safety.

From a methodological perspective, the study provides a preliminary head-to-head comparison rather than a credible estimate of either intervention's isolated causal effect. The group-allocation mechanism and potentially important prognostic covariates were not archived. Baseline-adjusted analysis may be informative in future work, but adjusted estimates cannot be manufactured from group-level summaries, and

model selection in nonrandomized settings requires careful attention to confounding and the allocation process (van Breukelen, 2006; Twisk et al., 2018; Austin, 2011). Transparent acknowledgment of these limitations is preferable to claiming a level of methodological certainty absent from the available records (Ahmed, 2024; Pasaribu et al., 2025).

Implications for Nursing Practice

The findings support consideration of auditory options as optional comfort-oriented adjuncts—not as independently proven blood-pressure-lowering treatments. For clinically stable patients who are able to consent, nurses may discuss preferences for meaningful recitation, neutral sound, or no added audio, while protecting uninterrupted monitoring, medication delivery, and timely guideline-directed AMI care (Byrne et al., 2023; Rao et al., 2025). Religious audio should not be assumed acceptable to all patients, and neutral audio should not be assumed free of annoyance or adverse effects. Any local implementation would benefit from clear eligibility and stopping criteria, device hygiene, safe and tolerable sound intensity, documentation of dose and co-interventions, and standardized blood-pressure measurement; the current dataset did not evaluate a complete implementation protocol (Hoffmann et al., 2014; Wisuda et al., 2026).

Strengths and Limitations

The study directly compares two relatively accessible auditory modalities in hospitalized AMI patients and reports complete immediate outcome ascertainment for its 52 enrolled participants. Its primary limitations are a single-center, non-equivalent design; limited sample size; unknown allocation procedures and clinical co-interventions; clinically relevant baseline diastolic imbalance; missing original G*Power assumptions and pretest pressure means/standard deviations; and incomplete stimulus- and measurement-protocol information. The originally reported baseline comparisons are not fully reconcilable with all archived summaries, preventing a defensible reconstructed baseline table or adjusted analysis. No participant-level data, hemodynamic time series, intervention fidelity data, pain or anxiety scores, autonomic measures, or extended follow-up were available. As a result, selection bias, residual confounding,

measurement variability, regression to the mean, and limited generalizability cannot be excluded. The study is not an equivalence trial, and the absence of a silent or usual-care-only group prevents attributing observed declines to auditory exposure alone.

Directions for Future Research

Future work should use an adequately justified, prospectively registered randomized design, with concealed allocation where feasible and clearly documented recruitment and assignment processes (Akbar et al 2025; Sujati et al 2023; Rekawati et al 2025). A usual-care or quiet-rest arm in addition to murottal and white noise would distinguish shared time-related changes from auditory-specific responses; preference and acceptability should be considered without compromising allocation integrity. Researchers should standardize sound level, equipment, duration, timing, clinical co-interventions, and pre/post pressure procedures, as recommended for reproducible intervention research (Hoffmann et al., 2014). Prespecified outcomes should include baseline-adjusted repeated blood-pressure measures, clinical severity, infarction subtype, reperfusion and medication exposure, pain, anxiety, patient acceptability, and appropriate follow-up. Clinically meaningful differences and precision—not significance testing alone—should guide interpretation (Greenland et al., 2016).

Conclusion and Recommendation

Among hospitalized patients with AMI, modest immediate systolic and diastolic blood-pressure reductions were observed after either Qur'anic murottal or white noise. The direct comparison did not detect a statistically significant difference in the amount of reduction. The available evidence does not establish equivalence, causality, sustained control, or cardiovascular benefit for either modality. Auditory interventions may be considered according to patient preference and clinical suitability as supplementary comfort measures; stronger, rigorously controlled research is needed before any blood-pressure-lowering recommendation.

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Declaration of conflict of interest

The authors declare that they have no competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Declaration on the Use of AI

The authors declare that no artificial intelligence (AI) or AI-assisted technologies were used in the writing, data analysis, or manuscript preparation process for this study. All work presented is original and authored solely by the researchers.

Data Availability Statement

Data sharing is not applicable to this article.

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