Cureus
Part of SPRINGER NATURE

Open Access Review Article

# Percutaneous Coronary Intervention Versus Conservative Management for Spontaneous Coronary Artery Dissection: A Systematic Review of Treatment Selection, Procedural Complications, and Clinical Outcomes

Aoumar G. Chamma [1], Wendy Saliba [1], Linda Chamma [2]

1. Cardiology, University of Balamand, Beirut, LBN 2. Urology, Lebanese University, Beirut, LBN

**Corresponding author:** Aoumar G. Chamma, aoumar.chama@gmail.com

**Review began** 11/04/2026
**Review ended** 11/14/2026
**Published** 11/21/2026

© **Copyright** 2026
Chamma et al. This is an open access article distributed under the terms of the Creative Commons Attribution License CC-BY 4.0., which permits unrestricted use, distribution, and reproduction in any medium, provided the original author and source are credited.

**DOI:** 10.7759/cureus.120029

## Abstract

Spontaneous coronary artery dissection (SCAD) presents a therapeutic challenge because patients selected for percutaneous coronary intervention (PCI) often have more severe clinical or angiographic presentations than those managed conservatively. This systematic review examined treatment selection, PCI procedural outcomes, and clinical outcomes associated with initial PCI versus conservative management. The retained PubMed/MEDLINE export dated October 19, 2026, and an earlier combined Semantic Scholar/OpenAlex export yielded 2,692 records before deduplication. After removal of 152 duplicate records, 2,540 records were screened. A total of 13 included reports representing 10 source families were identified. Eight selected numerical results underwent a structured risk-of-bias assessment, and all were judged to have serious overall risk of bias. A structured synthesis without meta-analysis separated initial-strategy comparisons from eventual PCI, combined PCI/coronary artery bypass grafting (CABG), and administrative-coded evidence. In one included registry, reported two-year major adverse cardiovascular event estimates were 13.9% after initial PCI and 11.7% after conservative management, with an unresolved discrepancy in the published composite event totals. A linked registry report contributed only descriptive cohort-level findings. Certainty for direct clinical treatment comparisons was very low. Given confounding by indication, inconsistent outcome and exposure definitions, and potentially overlapping cohorts, available observational evidence does not establish superiority or equivalence of either initial strategy.

**Categories:** Cardiology, Emergency Medicine, Internal Medicine
**Keywords:** conservative management, coronary revascularization, percutaneous coronary intervention, spontaneous coronary artery dissection, systematic review, treatment selection

## Introduction And Background

Spontaneous coronary artery dissection (SCAD) causes acute coronary syndrome through separation of the coronary arterial wall, often accompanied by intramural hematoma that compresses the true lumen. Its underlying mechanism differs from atherosclerotic plaque rupture, and spontaneous vessel healing has been documented in selected patients. These characteristics complicate the application of revascularization approaches developed for atherosclerotic coronary disease [1,2].

Conservative management has commonly been selected for clinically stable patients with preserved coronary flow, whereas persistent ischemia, severely impaired flow, threatened myocardium, or hemodynamic instability may prompt revascularization. Percutaneous coronary intervention (PCI) in SCAD presents particular technical challenges, including false-lumen wiring, extension of intramural hematoma, propagation of dissection, and the need for extensive stenting [1,3]. Patients selected for PCI also tend to have more severe clinical or angiographic presentations than those managed conservatively. Consequently, observational treatment comparisons are strongly affected by confounding by indication [3-6]. Conservative management generally involves medical treatment and clinical monitoring, with rescue revascularization considered when persistent ischemia, hemodynamic instability, or worsening coronary flow makes continued conservative treatment inappropriate.

Earlier syntheses have compared invasive and conservative management, but differences in patient populations, revascularization definitions, outcomes, and follow-up complicate interpretation [7]. This review examines the following three related questions: which features are associated with the selection of PCI, how PCI performs technically, and how clinical outcomes differ between treatment groups. It distinguishes initial-treatment comparisons from analyses based on PCI received at any time, combined PCI and coronary artery bypass grafting (CABG), and administrative diagnoses. Reporting was structured using the PRISMA 2020, Synthesis Without Meta-analysis (SWiM), and PRISMA-S reporting guidelines [8-10]. Supplementary materials are organized as follows. Appendix A documents source-data discrepancies and handling decisions. Appendix B provides supplementary data extraction. Appendix C presents search

**How to cite this article**
Chamma A G, Saliba W, Chamma L (November 21, 2026) Percutaneous Coronary Intervention Versus Conservative Management for Spontaneous Coronary Artery Dissection: A Systematic Review of Treatment Selection, Procedural Complications, and Clinical Outcomes. Cureus 18(11): e120029. DOI 10.7759/cureus.120029

sources, strategies, and PRISMA-S documentation. Appendix D lists full-text exclusions and reports not retrieved. Appendix E provides detailed Risk of Bias in Non-randomized Studies of Interventions (ROBINS-I) assessments, and Appendix F provides detailed Grading of Recommendations Assessment, Development and Evaluation (GRADE) assessments. Appendix G contains the completed PRISMA 2020 checklist, and Appendix H contains the completed SWiM checklist.

# Review

## Methods

*Protocol and Registration*

This review was not prospectively registered. The available protocol is labeled version 1.0 and dated July 15, 2026, but retained documentation does not establish that it was finalized before screening and extraction began. Amendments dated October 20, 2026, clarified eligibility criteria, treatment definitions, handling of overlapping cohorts, and outcome assessment after preliminary examination of the evidence. These amendments are therefore reported as retrospective.

*Review Question and Eligibility*

The primary question was whether initial PCI, compared with initial conservative management, was associated with different clinical outcomes in adults with confirmed SCAD. A related question concerned the baseline clinical and angiographic characteristics associated with treatment selection.

The primary comparison concerned adults aged 18 years or older with SCAD confirmed by coronary angiography or intracoronary imaging. Eligible designs included original comparative cohorts and prospective or retrospective registries, including original data published as research letters or brief reports. Randomized trials were eligible if identified. Additional eligible evidence included cohorts classified by PCI received during the index admission, studies combining PCI with CABG, and administrative-code studies without independent diagnostic adjudication. These studies contributed indirect evidence and were analyzed separately from the primary initial-treatment comparison.

Outcomes of interest were all-cause death, myocardial infarction, study-defined major adverse cardiovascular events, recurrent SCAD, repeat revascularization, technical success, procedural complications, and rescue treatment. Original non-comparative SCAD series and procedural reports were eligible for separately labeled descriptive evidence when they provided extractable original management, technical, procedural safety, or clinical outcome data. They were not treated as estimates of a causal PCI-versus-conservative treatment effect. Eligibility for the formal included-report inventory required an original comparative result or an original descriptive/procedural result selected under these criteria. Individual case reports, secondary reviews, and publications without a distinct eligible result were excluded from the included-review dataset. Report-specific reasons for exclusion after full-text assessment are provided in appendix D, together with bibliographic details and links between related reports. Reports excluded from the formal evidence synthesis could be cited separately for background context, but were not counted among the 13 included reports. These eligibility clarifications were retrospective amendments recorded on October 20, 2026.

Case reports, reviews, opinion-only editorials, traumatic dissections, and primary iatrogenic coronary dissections were excluded from the primary evidence synthesis. Publications without relevant original comparative patient data were excluded from the primary treatment comparison but could be cited for contextual information.

The primary comparison was defined by treatment intent at diagnostic angiography. Patients undergoing rescue PCI after an initial conservative decision were considered part of the initial conservative strategy for this comparison. Where published results could not be reconstructed according to the initial strategy, they were retained as indirect evidence under the reported treatment classification.

Evidence was organized into four treatment-exposure tiers. Tier 1 comprised cohorts with angiographically confirmed SCAD classified according to initial PCI versus initial conservative management. Tier 2 included cohorts classified according to PCI received during the index admission or after initial management, including rescue PCI. Tier 3 consisted of studies combining PCI and coronary artery bypass grafting (CABG) within a single revascularization group. Tier 4 included administrative-code studies without independent diagnostic adjudication of SCAD. Small descriptive series and exploratory anatomy-stratified analyses were considered separately and were not used to estimate direct comparative treatment effects.

Studies were eligible regardless of publication language when sufficient information could be obtained from the original publication or an available translation. No minimum follow-up duration was required. Outcomes were retained within their reported follow-up periods. Reports that could not be retrieved were recorded separately from reports excluded after full-text assessment.

*Information Sources and Search*

PubMed/MEDLINE searching began on July 15, 2026. The retained export dated October 19, 2026, contained 2,418 records. A corrected query was rerun on October 20, 2026, with the same reported record count, but the execution-history file was not retained. Consequently, the exact correspondence between the executed search and the transcribed strategy could not be retrospectively verified. The original execution dates and complete search histories for the earlier Semantic Scholar/OpenAlex export were also unavailable. The recorded query was as follows: ("spontaneous coronary artery dissection"[Title/Abstract] OR "spontaneous coronary artery dissections"[Title/Abstract] OR "spontaneous coronary dissection"[Title/Abstract] OR "spontaneous coronary dissections"[Title/Abstract]) AND 1800/01/01:2026/10/19[Date - Publication]. Following a syntax correction, the PubMed search was rerun on October 20, 2026, returning the same reported count of 2,418 entries. The rerun was not counted as a separate identification source.

The PubMed export was combined with an earlier Semantic Scholar/OpenAlex export containing 274 entries. The execution date and platform-specific retrieval count for this earlier export were not preserved. Available search terms are reproduced in appendix C, section C3. This export was treated as a supplementary source of records, and its original execution could not be reproduced from retained documentation. Embase, CENTRAL, Scopus, and Web of Science were not searched. Reference lists of eligible reports and relevant reviews were examined, and targeted checks were undertaken. These activities contributed no additional unique records to the screening inventory.

Aoumar G. Chamma and Wendy Saliba consulted the ClinicalTrials.gov entries for Dissezioni Spontanee Coronariche (DISCO) (NCT04415762) and Spanish Registry on Spontaneous Coronary Artery Dissection (SR-SCAD) (NCT03607981) on August 14, 2026. These activities were targeted registry checks rather than a systematic registry search. Consultation of these entries did not establish the availability of outcome-specific prespecified analysis plans.

Appendix C distinguishes retained search documentation from the chronology reported by the review authors. PubMed/MEDLINE searching reportedly began on July 15, 2026; the retained export is dated October 19, 2026. The earlier combined Semantic Scholar/OpenAlex export contains 274 records, but its original execution dates, platform-specific counts, and complete native query histories were not retained. The corrected PubMed rerun on October 20 reportedly returned 2,418 records and was not counted as an additional identification source. Its execution-history record was unavailable, preventing confirmation that the query transcribed in appendix C, section C2, exactly matched the executed query. Recorded retrieval attempts and the historical unretrieved-report cutoff are documented in appendix D.

*Study Selection and Data Extraction*

The identification streams contained 2,692 entries, comprising 2,418 PubMed/MEDLINE entries and 274 entries from the earlier combined Semantic Scholar/OpenAlex export. Available accounting reported removal of 152 duplicate entries, including 150 cross-source duplicates and two additional duplicates, leaving 2,540 records for title and abstract screening. Of these records, 2,460 were excluded, and 80 reports were sought for retrieval. Four reports were not retrieved, leaving 76 reports assessed for eligibility. A total of 63 reports were excluded, and 13 reports representing 10 source families were included.

Aggregate screening and eligibility counts were reconciled against retained Covidence systematic review software (Melbourne, Australia: Veritas Health Innovation)-derived records. The record-level duplicate crosswalk confirmed removal of 152 duplicate entries, comprising 150 cross-source duplicates and two additional duplicates, leaving 2,540 records for screening. Among the 76 reports assessed at full text, 63 were excluded as follows: 56 because of ineligible publication type or study design, six because they provided no independently eligible comparative or descriptive result, and one because it was a linked overlapping report without an additional eligible result selected for synthesis. Appendix D provides the report-level exclusion reasons and separately lists the four reports not retrieved. These records support the reported aggregate counts but do not reconstruct the complete historical reviewer decision process.

Aoumar G. Chamma, Wendy Saliba, and Linda Chamma jointly undertook study screening, full-text eligibility assessment, data extraction, and risk-of-bias assessment. These activities were not performed as independent duplicate assessments. Differences were resolved through discussion. Historical individual reviewer decision logs were not retained, limiting reconstruction of the reviewer-level decision process.

*Extraction Variables*

The extraction framework covered report identifiers, registry or center, recruitment dates, eligibility criteria, diagnostic confirmation, sample size, age, sex, initial treatment, rescue treatment, clinical and anatomical severity, endpoint components, event counts, follow-up, effect estimates, adjustment covariates, and missingness. Separate fields distinguished patients from lesions, individual participants from weighted hospitalizations, and crude proportions from time-to-event estimates. Missing values were not imputed.

Data were extracted from source publications and available supplementary materials. Appendix A documents unresolved numerical discrepancies, source-verification limitations, and the handling of affected results. Appendix B provides study characteristics, clinical outcomes, available source locations, PCI procedural outcomes, and report-family relationships and potential cohort overlap.

*Cohort Families, Overlap, and Report Selection*

Reports were linked to a source family when they drew on the same registry, center, recruitment period, or participant flow. Participants and events were not summed across linked reports or across cohorts with possible overlap. For a given outcome and follow-up period, report selection prioritized correspondence to the target comparison, completeness of denominators, and then sample size and recency. Reports from the same family with different endpoint definitions were retained as separately labeled results and were not treated as independent replication.

Families with possible participant overlap were considered potentially dependent, and agreement between them was not counted as independent consistency. Shared authorship alone was not considered sufficient to establish or exclude overlap. Report-family relationships are summarized in appendix B.

*Handling of Discrepant Data*

Source-reported counts and percentages were retained with their definitions and denominators. Percentages were recalculated from reported counts where possible, and discrepancies between printed and calculated values were identified explicitly. Conflicting source values were not resolved by choosing the value that appeared more plausible. Where a discrepancy remained unresolved, alternative reported values were displayed, or the affected overall result was excluded from comparative synthesis.

Crude risk ratios and log-Wald 95% confidence intervals were calculated only from reported group counts. These estimates were labeled reviewer-calculated, unadjusted, and descriptive. Time-to-event percentages were not converted into crude event counts or risks. Discrepancies are documented in appendix A. Directly reported event counts were extracted separately from survival-analysis results. Count-based calculations were not treated as equivalent to censoring-adjusted estimates.

*Risk-of-Bias Assessment*

The original seven-domain Risk of Bias in Non-randomized Studies of Interventions (ROBINS-I) framework, published in 2016, was applied to eight selected numerical results [11]. Assignment to initial treatment at diagnostic angiography was the target comparison. Analyses based on eventual PCI, mixed revascularization, or administrative diagnoses were assessed with attention to their departures from that target.

Eight results were selected to represent the principal comparative findings across the different treatment-exposure tiers and major contributing cohorts. For outcomes without separate formal ROBINS-I assessments, the available study-level judgments informed the interpretation of shared sources of bias, particularly confounding and intervention classification. Outcome-specific concerns, including missing data, outcome measurement, and selective reporting, were not assumed to be identical across endpoints. This limitation was considered when interpreting the GRADE findings.

Potential confounders included presenting ischemia, shock or cardiac arrest, preintervention Thrombolysis in Myocardial Infarction (TIMI) flow, lesion location and extent, myocardium at risk, ST-segment elevation myocardial infarction (STEMI), baseline ventricular function, age, pregnancy association, center, and recruitment period. Domain judgments and supporting explanations for the eight result-specific assessments are provided in appendix E. All eight assessed results were judged to have serious overall risk of bias. A domain was recorded as no information when the available evidence was insufficient for a judgment. These assessments do not imply independence of potentially overlapping populations and do not cover every numerical result reported in the review.

*Synthesis*

Meta-analysis was not undertaken because the evidence did not provide a sufficiently independent set of compatible treatment-effect estimates. Studies differed in treatment classification, outcome definitions, follow-up, diagnostic confirmation, and participant populations. Several reports also had possible cohort overlap.

A structured synthesis without meta-analysis separated initial-strategy comparisons from indirect comparisons. Results were organized by treatment-exposure tier, clinical outcome, and follow-up period, preserving the original effect measures and endpoint definitions. Findings were not synthesized by counting statistically significant studies [9]. No pooled effect estimate, meta-regression, or quantitative across-study

heterogeneity statistic was calculated. The accompanying Synthesis Without Meta-analysis (SWiM) checklist describes reporting of the synthesis.

*Assessment of Missing Results*

No formal statistical assessment of reporting bias was undertaken because sufficiently comparable, independent effect estimates were unavailable. Accessible protocols, registry entries, and supplementary materials were consulted, but no outcome-specific prespecified analysis plan was available for appraisal for any of the eight assessed results. ROBINS-I domain seven was therefore recorded as no information. This judgment reflects insufficient information about selection of the reported result and does not establish absence of reporting bias. Potentially missing studies and outcomes were considered separately as limitations of the evidence base.

*Certainty of Evidence*

Certainty was evaluated for clinically important outcomes using the Grading of Recommendations Assessment, Development and Evaluation (GRADE) framework. Risk-of-bias judgments were informed by available result-specific assessments, with consideration of inconsistency, indirectness, imprecision, and possible publication bias [12,13]. The primary initial-treatment comparison was distinguished from indirect evidence based on eventual PCI, mixed PCI/CABG exposure, or administrative coding. Potentially overlapping reports were not treated as independent replications.

Imprecision was considered using available event counts and confidence intervals. Where suitable effect estimates or precision measures were unavailable, this limitation was stated explicitly. Reviewer-calculated crude risk ratios were descriptive and were distinguished from published adjusted estimates. Non-randomized design was not automatically counted as an additional downgrade after incorporating the consequences of non-randomization through ROBINS-I.

For the five direct comparative outcomes, risk of bias was downgraded by two certainty levels because the contributing evidence was judged to have very serious limitations for causal inference. Although the assessed numerical results were classified as serious overall risk of bias using ROBINS-I, the magnitude and direction of confounding by indication, substantial baseline differences between treatment groups, and limited adjustment for key clinical and angiographic factors substantially reduced confidence in the estimated treatment effects. These outcome-level downgrades were applied using the ROBINS-I-informed GRADE framework and were not based solely on the observational study design. Outcome-level judgments are provided in appendix F. Not all contributing numerical results received a separate formal ROBINS-I assessment, which limits the completeness of the outcome-level appraisal. Procedural complication frequencies were presented as descriptive evidence rather than causal treatment comparisons.

## Results

*Study Selection and PRISMA 2020 Reporting*

The search streams identified 2,692 entries, including 2,418 from PubMed/MEDLINE and 274 from the earlier Semantic Scholar/OpenAlex export. After removal of 152 duplicate entries verified against the record-level crosswalk, 2,540 records remained for title and abstract screening. Of these, 2,460 were excluded, 80 reports were sought for retrieval, and four were not retrieved, leaving 76 reports assessed at full text. A total of 63 full-text reports were excluded: 56 because of ineligible publication type or study design, six because they provided no independently eligible result, and one because it was a linked overlapping report without an additional eligible result selected for synthesis. A total of 13 reports representing 10 source families were included. Report-specific exclusion reasons and the four reports not retrieved are listed separately in appendix D. Study identification, screening, eligibility assessment, and inclusion are summarized in Table *1* and Figure *1*.

| Stage | Number | Evidence basis or explanation |
|---|---|---|
| PubMed/MEDLINE records identified | 2,418 | Retained export dated October 19, 2026. The corrected October 20 rerun was not counted as an additional identification source. Search documentation is provided in appendix C. |
| Records from the earlier combined Semantic Scholar/OpenAlex export | 274 | Retained combined export. Original execution dates and platform-specific retrieval counts were not preserved; documentation limitations are described in appendix C. |
| Total records identified before deduplication | 2,692 | Combined identification inventory: 2,418+274. |
| Duplicate records removed | 152 | Record-level duplicate crosswalk confirmed 150 cross-source duplicates and two additional duplicate entries. |
| Records screened | 2,540 | Identification inventory after deduplication: 2,692-152. |
| Records excluded at title and abstract screening | 2,460 | Aggregate screening count reconciled against Covidence systematic review software (Melbourne, Australia: Veritas Health Innovation)-derived records. |
| Reports sought for retrieval | 80 | Reports retained after title and abstract screening: 2,540-2,460. |
| Reports not retrieved | 4 | Listed separately in appendix D as NR01-NR04. October 15, 2026 was the historical retrieval-status cutoff. |
| Full-text reports assessed for eligibility | 76 | Reports sought minus reports not retrieved: 80-4. |
| Full-text reports excluded | 63 | Report-specific reasons are documented in appendix D, entries E01-E63. The reason categories below account for all 63 exclusions. |
| Ineligible publication type or study design (R1) | 56 | Includes individual case reports, secondary reviews, and other publications that did not meet the review’s eligible-design criteria. Report-specific explanations are provided in appendix D. |
| No independently eligible comparative or descriptive result (R3) | 6 | Reports without a distinct eligible result selected for synthesis. Report-specific explanations are provided in appendix D. |
| Linked overlapping report without an additional eligible result selected (R5) | 1 | A linked report without an additional eligible result selected for synthesis. Its relationship to the retained evidence is documented in appendix D. |
| Reports included in the review | 13 | Count of included publications, rather than independent cohorts or participants. |
| Source families represented | 10 | Related publications were grouped into source families. Report relationships and potential overlap are described in appendix B. |

**TABLE 1: PRISMA 2020 study identification, screening, eligibility, and inclusion.**

E01-E63 identify reports excluded after full-text assessment; NR01-NR04 identify reports not retrieved. R1, R3, and R5 are exclusion-reason codes defined in appendix D. Reports not retrieved are separate from full-text exclusions. Aggregate count reconciliation does not establish independent duplicate screening or reconstruct unavailable historical reviewer decision logs. Source families should not be assumed to represent mutually independent participant populations.

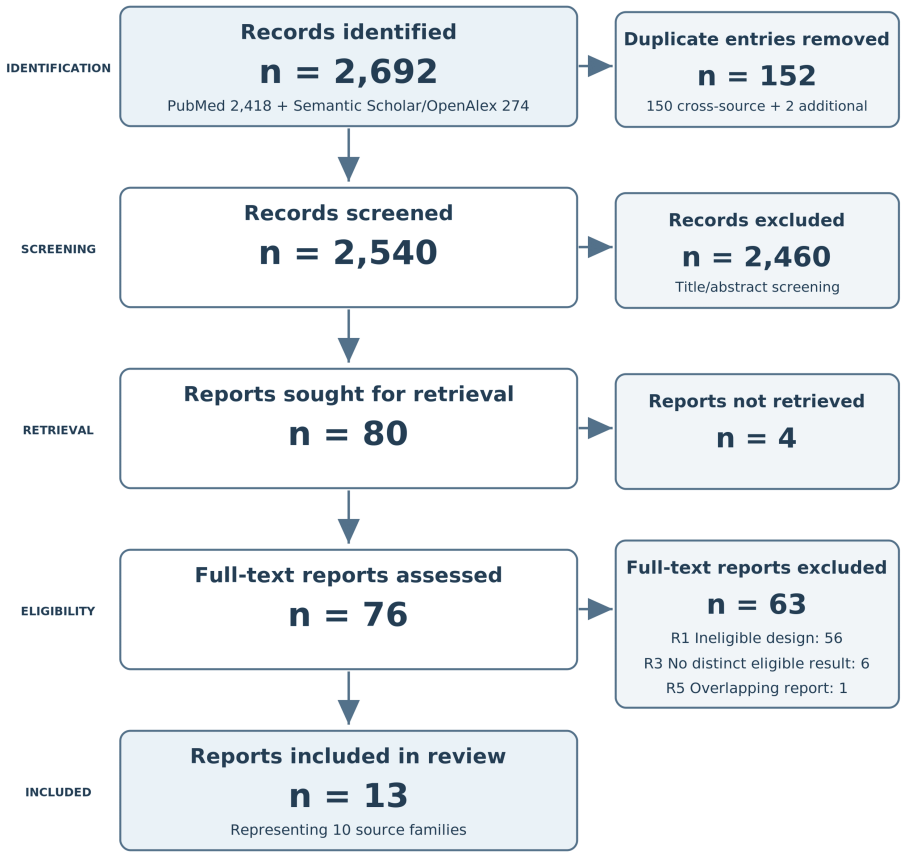



**FIGURE 1: PRISMA 2020 flow diagram of study identification, screening, eligibility assessment, and inclusion.**

This diagram summarizes the identification and selection of 13 included reports representing 10 source families. Four reports not retrieved are presented separately from the 63 reports excluded after full-text assessment. Aggregate selection counts were reconciled to Covidence systematic review software-derived accounting; the diagram does not establish independent duplicate screening.

*Study Characteristics and Cohort Families*

The included registries comprised the Italian-Spanish Dissezioni Spontanee Coronariche (DISCO, Italy/Spain) registry and the Spanish Registry on Spontaneous Coronary Artery Dissection (SR-SCAD). The 13 included reports represented multicenter registries, referral-center cohorts, single-center observational studies, administrative datasets, and a descriptive case series. Studies differed in diagnostic confirmation, baseline severity, treatment classification, outcome definitions, and follow-up.

Initial PCI versus initial conservative management in confirmed SCAD was the most directly relevant comparison. Studies involving PCI received during the index admission, mixed PCI/CABG revascularization, or administratively coded SCAD contributed indirect evidence. Descriptive series were considered separately.

The included evidence also comprised the Spanish SR-SCAD procedural report [14], the anatomy-stratified cohort by Ma et al. [15], the Vancouver registry analysis by Hassan et al. [16], the multicenter PCI study by Kotecha et al. [17], the US National Inpatient Sample analysis by Krittanawong et al. [18], and the Spanish administrative cohort described by Alfonso et al. [19]. Seidl et al. [20], Vanzetto et al. [21], and Meng et al. were considered relevant contextual reports but were excluded from the formal evidence synthesis and were not counted among the 13 included reports [22]. Table *2* summarizes the included source families, including the descriptive series by Manhães et al. [23]. Additional characteristics are presented in appendix B.

| Source family | Included reports | Design/population | Treatment groups | Role in synthesis |
|---|---|---|---|---|
| DISCO Italy/Spain | Benenati et al. (2023) [3] | Multicenter registry, n=369 | 129 initial PCI; 240 conservative | Tier 1, initial-treatment comparison |
| SR-SCAD | García-Guimaraes et al. (2021) [4], (2022) [5], linked PCI letter (2022) [14] | Prospective Spanish registry; 2021 n=318; 2022, n=389 | 2021: 70 initial PCI; 248 conservative; 2022: 84 initial PCI; 305 conservative | 2021 report: tier 1 adjusted association with in-hospital MAE. 2022 report: descriptive cohort-level outcomes. Linked PCI letter: procedural evidence. Linked reports were treated as one source family |
| Mayo Clinic | Tweet et al. (2014) [1], (2012) [2] | Referral center cohort; 2014, n=189 | 95 PCI/CABG; 94 conservative | Tier 3 mixed revascularization, 2012 procedural context |
| Duke | Jensen et al. (2024) [6] | Single-center cohort; n=186 | 43 PCI; 9 CABG; 134 medical | Tier 3 for pooled PCI/CABG long-term outcomes |
| Zhongshan Hospital, Fudan University, Shanghai, China | Ma et al. (2023) [15] | Anatomy-stratified cohort; n=81 | 40 PCI; 36 conservative; 5 CABG | Exploratory treatment-specific outcomes: 1-year MACE 3/40 PCI and 5/36 conservative, source-verified; not a graded tier 1 effect |
| Vancouver | Hassan et al. (2021) [16] | Prospective registries analyzed retrospectively; n=403 | 75 index-admission PCI; 328 no index PCI | Tier 2, includes rescue PCI |
| UK/Dutch/Spanish | Kotecha et al. (2021) [17] | Multicenter PCI cohort and selected controls; n=436 | 215 PCI; 221 non-PCI | Tier 2, eventual/any PCI; possible overlap with SR-SCAD |
| US National Inpatient Sample | Krittanawong et al. (2024) [18] | Administrative dataset; 31,105 weighted hospitalizations | 20,625 PCI; 10,480 conservative | Tier 4; hospitalization-level coded diagnoses |
| Spanish administrative cohort | Alfonso et al. (2022) [19] | Administrative cohort; 804 hospitalization episodes | Coded PCI vs conservative | Tier 4, administrative |
| Instituto do Coração (InCor), Hospital das Clínicas, University of São Paulo, Brazil | Manhães et al. (2014) [23] | Original consecutive SCAD series, n=25 | 10 PCI (9 stents, 1 balloon); 14 medicals; 1 CABG | Descriptive only. In-hospital MACCE 2/25; late MACCE 2/19; no treatment-effect estimate |

**TABLE 2: Characteristics of included reports and their study/source families.**

Composite outcomes retain each publication's definition. Treatment-exposure tiers are defined in Methods.

CABG: coronary artery bypass grafting; MACCE: major adverse cardiovascular and cerebrovascular events; MACE: major adverse cardiovascular events; MAE: major adverse events; PCI: percutaneous coronary intervention; SCAD: spontaneous coronary artery dissection; SR-SCAD: Spanish Registry on Spontaneous Coronary Artery Dissection; DISCO: Dissezioni Spontanee Coronariche

The Portuguese and English versions of Manhães et al. refer to the same original consecutive 25-patient series, counted once [23]. A total of 14 patients were treated medically, 10 underwent PCI (nine stent procedures and one balloon angioplasty), and one underwent CABG. The series contributed descriptive management and cohort-level outcome evidence, not a PCI-versus-conservative comparative treatment effect. Ma et al. provided source-verified one-year major adverse cardiovascular events (MACE) counts of 3/40 in the PCI group and 5/36 in the conservative group; this anatomy-stratified, non-randomized comparison was retained as exploratory evidence outside the formal eight-result ROBINS-I set [15]. The linked García-Guimaraes et al. report remained included for verified overall in-hospital and late outcomes, but no independently confirmed arm-specific effect on late major adverse cardiovascular and cerebrovascular events (MACCE) was available [5]. Narducci et al. was recorded as a report not retrieved (appendix D, entry NR01) [24].

*Overlap Resolution*

Established and suspected relationships were considered when interpreting the evidence. Linked reports were not treated as independent populations, and potential overlap between source families prevented simple addition of participants or events. Table *3* summarizes these relationships, with further details in appendix B.

| Reports | Relationship | Handling in the synthesis |
|---|---|---|
| SR-SCAD: García-Guimaraes et al. (2021, 2022) [4,5] | Reports from the same national registry with extended recruitment | Treated as one family; different endpoints retained separately, not as independent replication |
| DISCO: Benenati et al. (2023) [3] and SR-SCAD: García-Guimaraes et al. (2021, 2022) [4,5] | Possible participant overlaps through shared Spanish recruiting centers; overlap not quantified | Considered potentially dependent; participants and events not summed |
| UK/Dutch/Spanish SCAD: Kotecha et al. (2021) [17] and SR-SCAD: García-Guimaraes et al. (2021, 2022, 2022) [4,5,14] | Kotecha et al. includes a Spanish subset; overlap with SR-SCAD remains possible | Tier 2 evidence retained separately; events not added to registry totals |
| Mayo Clinic SCAD: Tweet et al. (2014) [1] and (2012) [2] | Same institution and overlapping recruitment periods | 2014 used for comparative outcomes; 2012 used for procedural context |
| Vancouver General Hospital SCAD: Hassan et al. (2021) [16] and other Vancouver/Canadian registry reports | Shared registry populations may produce overlapping reports | Hassan et al. retained as the included Vancouver report; external reports not counted as independent included cohorts |
| US National Inpatient Sample (NIS): Krittanawong et al. (2024) [18] and Spanish National Health System Minimum Basic Data Set (MBDS): Alfonso et al. (2022) [19] | Different administrative data sources using coded diagnoses | Kept separate and not merged with confirmed-SCAD cohorts |
| Portuguese and English versions of Manhães et al. (2014) [23] | Two language versions of the same series | Counted once |

**TABLE 3: Known and suspected overlap among SCAD reports.**

SCAD: spontaneous coronary artery dissection; SR-SCAD: Spanish Registry on Spontaneous Coronary Artery Dissection

*Treatment Selection*

PCI recipients generally had more severe clinical or angiographic presentations. In DISCO, STEMI occurred in 68% of PCI recipients versus 35% of medically managed patients, proximal involvement in 32% versus 7%, TIMI 0-1 flow in 54% versus 20%, and resuscitated cardiac arrest in 9% versus 3% [3]. The SR-SCAD 2021 report included 318 patients, of whom 70 (22.0%) underwent initial PCI and 248 (78.0%) received initial conservative management [4]. The separate 2022 report included 389 patients, comprising 84 initially managed with PCI and 305 treated conservatively [5]. In the Spanish procedural report, STEMI, proximal involvement, and impaired flow were more frequent among PCI recipients. Operators identified low flow, ongoing ischemia, and proximal disease as reasons for intervention [14].

In the UK/Dutch/Spanish cohort, STEMI occurred in 55.3% of PCI recipients versus 34.8% of non-PCI patients, and proximal coronary segments were involved in 29.8% versus 7.7%. Only 8.8% of PCI recipients underwent intervention outside the reported contexts of STEMI or cardiac arrest, TIMI 0/1 flow, or proximal dissection [17]. In Vancouver, STEMI occurred in 50.7% of PCI recipients versus 19.8% of patients not receiving PCI [16]. Duke revascularization patients more often had STEMI, left anterior descending involvement, and reduced ventricular function [6]. Ma et al. described treatment selection according to anatomical risk [15].

These findings describe how clinicians selected treatment. They do not establish that the same characteristics identify patients who derive a causal benefit from PCI. Across the included cohorts, revascularization was more frequently selected in patients with ongoing ischemia, impaired TIMI flow, proximal coronary involvement, hemodynamic compromise, or a greater amount of myocardium at risk. These characteristics should be interpreted as determinants of treatment selection rather than evidence of differential treatment benefit.

*Clinical Outcomes by Endpoint and Horizon*

Table 4 presents comparative outcomes by treatment-exposure tier. Time-to-event estimates were retained as reported and were not converted to crude risks. Crude risk ratios were calculated from reported event counts and are unadjusted descriptive estimates. DISCO treatment comparisons were unadjusted. Its multivariable models examined treatment selection rather than an adjusted effect of treatment on outcomes [3]. Clinical outcome extraction and source locators are provided in appendix B.

| Evidence tier | Outcome and follow-up | Studies | PCI or revascularization group | Conservative or non-PCI group | Effect estimates and interpretation |
|---|---|---|---|---|---|
| 1 | In-hospital composite outcome | DISCO: Benenati et al. (2023) [3] | 9/129 (7.0% calculated; 6.9% published) | 20/240 (8.3%) | Reviewer-calculated unadjusted RR 0.84 (95% CI: 0.39-1.78); published p=0.795 (appendix A and B) |
| 1 | In-hospital MAE | SR-SCAD: García-Guimaraes et al. (2021) [4] | Adjusted OR: 5.97 versus conservative management | Reference group | Adjusted OR: 5.97 (95% CI: 1.78-20.00); p=0.004. This is an adjusted association, not a group-specific event rate |
| 1 | Two-year MACE | DISCO: Benenati et al. (2023) [3] | 18 events; published estimate 13.9% | 28 events; published estimate 11.7% | Published overall total of 45 events differs from the treatment-group sum of 46. Log-rank p=0.467. No crude two-year RR was calculated (appendix A) |
| 1 | All-cause mortality at two years | DISCO: Benenati et al. (2023) [3] | One death; published estimate 0.7% | One death; published estimate 0.4% | The published overall total is one death despite one death in each treatment group; p=0.652. No corrected overall total was assumed (appendix A) |
| 1 | Myocardial infarction at two years | DISCO: Benenati et al. (2023) [3] | Published estimate 9.3%; reported count 10/129 (7.75% calculated) | Published estimate 8.3%; reported count 20/240 (8.33% calculated) | The published two-year Kaplan-Meier estimates were 9.3% for PCI and 8.3% for conservative management (p=0.921). The crude observed proportion of 10/129 (7.75%) in the PCI group is not directly interchangeable with the Kaplan-Meier estimate. No crude two-year treatment-effect estimate was calculated (appendix A) |
| 1 | Repeat PCI or revascularization at two years | DISCO: Benenati et al. (2023) [3] | Published estimate 12.4% | Published estimate 8.7% | Time-to-event estimates were retained as published (p=0.229). This endpoint is sensitive to subsequent management decisions |
| 2 | In-hospital MACE | Vancouver: Hassan et al. (2021) [16] | 22/75 (29.3%) | 9/328 (2.7% calculated; 2.8% published) | Reviewer-calculated unadjusted RR: 10.69 (95% CI: 5.13-22.27). The PCI group includes patients undergoing rescue PCI |
| 2 | Postdischarge MACE; median follow-up 3.7 years | Vancouver: Hassan et al. (2021) [16] | 18/75 (24.0%) | 65/328 (19.8%) | Reviewer-calculated unadjusted RR: 1.21 (95% CI: 0.77-1.91) |
| 2 | Postdischarge MACCE; median follow-up 900 days | UK/Dutch/Spanish SCAD: Kotecha et al. (2021) [17] | 31/215 (14.4%) | Alternative published counts of 21/221 (9.5%) and 22/221 (10.0%) | Reviewer-calculated unadjusted RR: 1.52 (95% CI: 0.90-2.56) using 21 control events, or 1.45 (95% CI: 0.87-2.42) using 22. The conflicting control counts remain unresolved (appendix A) |
| 2 | Recurrent myocardial infarction | UK/Dutch/Spanish SCAD: Kotecha et al. (2021) [17] | 20/215 (9.3%) | 17/221 (7.7%) | Descriptive, unadjusted comparison |
| 2 | Repeat revascularization after discharge | Vancouver: Hassan et al. (2021) [16] | 11/75 (14.7%) | 10/328 (3.0%) | Reviewer-calculated unadjusted RR: 4.81 (95% CI: 2.12-10.91) |

| | | | | | |
|---|---|---|---|---|---|
| 2 | Recurrent SCAD | UK/Dutch/Spanish SCAD: Kotecha et al. (2021) [17] | 13/214 (6.1%) | 15/220 (6.8%) | Reviewer-calculated unadjusted RR: 0.89 (95% CI: 0.43-1.83). Endpoint-specific denominators are retained as reported |
| 2 | Recurrent SCAD | Vancouver: Hassan et al. (2021) [16] | 5/75 (6.7%) | 40/328 (12.2%) | Reviewer-calculated unadjusted RR: 0.55 (95% CI: 0.22-1.34) |
| 3 | Five-year TVR | Mayo: Tweet et al. (2014) [1] | Kaplan-Meier estimates 30% | Kaplan-Meier estimates 19% | The revascularization group combines PCI and CABG (p=0.06) |
| 3 | Five-year recurrent SCAD | Mayo: Tweet et al. (2014) [1] | Kaplan-Meier estimates 23% | Kaplan-Meier estimates 31% | The revascularization group combines PCI and CABG (p=0.70) |
| 3 | Freedom from the composite outcome at 10 years | Duke: Jensen et al. (2024) [6] | Kaplan-Meier estimates 54% | Kaplan-Meier estimates 55% | The revascularization group combines PCI and CABG (p=0.36). Values represent freedom from the outcome, rather than event incidence |
| 4 | In-hospital mortality | US NIS: Krittanawong et al. (2024) [18] | 5.67% | 1.62% | Multivariable adjusted OR: 1.89 (95% CI: 1.24-2.90). Propensity-matched OR: 1.58 (95% CI: 1.00-2.50; p=0.051). These analyses used different adjustment approaches and populations. Administrative coding, uncertain treatment timing, and residual confounding limit causal interpretation. |
| 4 | In-hospital mortality | Spanish National Health System MBDS: Alfonso et al. (2022) [19] | 4.9% | 1.4% | Adjusted OR: 1.49 (95% CI: 0.45-4.90); p=0.513. Administrative-code analysis of hospitalization episodes. |

**TABLE 4: Clinical outcomes by treatment strategy, endpoint, and follow-up horizon.**

CABG: coronary artery bypass grafting; MACCE: major adverse cardiovascular and cerebrovascular events; MACE: major adverse cardiovascular events; MAE: major adverse events; NIS: National Inpatient Sample; PCI: percutaneous coronary intervention; RR: risk ratio; SCAD: spontaneous coronary artery dissection; TVR: target-vessel revascularization

Tier 1 comprises confirmed-SCAD cohorts classified by initial treatment strategy. Tier 2 comprises cohorts classified by PCI received during the index admission or after initial management, including rescue PCI. Tier 3 combines PCI and CABG within the revascularization group. Tier 4 comprises administrative-code studies without independent diagnostic adjudication of SCAD.

Composite outcomes retain each publication's definition. Reviewer-calculated risk ratios (RRs) and confidence intervals are unadjusted descriptive estimates derived from reported event counts. The components and observation periods of the principal composite endpoints, including SR-SCAD in-hospital MAE and the Duke long-term composite, are detailed in appendix B. Rescue revascularization and progression of the index dissection were not uniformly distinguished from recurrent events across studies, limiting direct comparison of composite outcomes. Published time-to-event estimates were not converted into crude event counts or risks. Conflicting source values are presented explicitly without selecting a preferred value. Discrepancy identifiers refer to appendix A, and result identifiers and source locations are provided in appendix B. Linked or potentially overlapping populations were not treated as independent, and estimates were not pooled.

The DISCO source prints 6.9% for the in-hospital PCI composite, whereas 9/129 rounds to 7.0%. The Vancouver source prints 2.8% for the conservative in-hospital composite, whereas 9/328 rounds to 2.7%. All crude RRs and their confidence intervals are reviewer-calculated and unadjusted. Alternative Kotecha et al. control counts are retained because the discrepancy was unresolved [17]. Estimates were not pooled.

The SR-SCAD reports were treated as linked publications with different endpoint definitions rather than independent cohorts. Vancouver and Kotecha et al. classified patients according to PCI received during or after initial management [17]. Mayo and Duke combined PCI with CABG within their revascularization comparisons. Administrative results were interpreted as hospitalization-level findings rather than independently adjudicated patient-level SCAD outcomes. These differences, together with confounding by indication and potential overlap, limited direct comparison of treatment effects.

The linked SR-SCAD 2022 Heart report included 389 patients, comprising 84 initially managed with PCI and 305 treated conservatively [5]. Overall, 25/389 patients experienced an in-hospital major adverse event. The follow-up analysis included 355 participants, among whom 46 (approximately 13%) experienced MACCE at a median follow-up of 29 months. These findings describe outcomes in the overall cohort and do not establish a treatment-specific effect. Previously extracted treatment-specific event estimates could not be verified against a traceable primary-source table, figure, or supplementary analysis and were excluded from the comparative numerical synthesis. The report therefore included only descriptive cohort-level outcomes. The adjusted in-hospital association from the 2021 report is presented separately [4]. Verification limitations and handling decisions are documented in appendix A.

Rescue or subsequent early revascularization was also reported following an initially conservative strategy. In the Mayo cohort, nine of 94 initially conservatively managed patients (9.6%) underwent early revascularization [1]. In SR-SCAD 2021, seven of 248 initially conservatively managed patients (2.8%) required PCI during hospitalization because of recurrent ischemia [4]. These results describe subsequent intervention after the initial management decision, and differences in study definitions prevent direct comparison of rescue rates across cohorts.

*PCI Technical Outcomes and Complications*

Procedural success varied according to the definition used. The Spanish registry letter reported conventional success of 54%, success according to final TIMI flow of 84%, and complications in 37% [14]. These procedural results arose from a linked registry report and were distinct from the follow-up MACCE analysis [5]. In Vancouver, PCI was classified as successful in 26/75 patients (34.7%), partly successful in 28/75 (37.3%), and unsuccessful in 21/75 (28.0%). Dissection propagation occurred in 33/75 patients (44.0%), and bailout CABG was required in 4/75 (5.3%) [16].

In the UK/Dutch/Spanish cohort, the reported overall complication count of 83/215 corresponded to 38.6%, and serious complications occurred in 28/215 patients (13.0%). Hematoma extension was reported in 58/215 (27.0%) and iatrogenic dissection in 18/215 (8.4%). TIMI flow improved in 118/140 patients (84.3%) with reduced baseline flow and worsened in 15/215 (7.0%). The complication definition included events occurring during the diagnostic phase, so not all events could be attributed specifically to stent implantation [17]. The discrepancy between reported complication counts of 83 and 87 remained unresolved and is documented in appendix A.

Earlier Mayo reports described technical failure in 15/43 patients (35%) and a conventional technical failure rate of 53% in the later cohort [1,2]. Overall procedural complications, serious complications, and dissection propagation represent different endpoints and were reported separately. Potential overlap between Kotecha et al. and SR-SCAD further precluded pooling [17]. Procedural extraction details are presented in appendix B.

Reported procedural success rates were not based on uniform definitions. Conventional angiographic success, improvement in TIMI flow, achievement of TIMI 3 flow, and clinical success represent distinct outcomes. For example, SCAD-specific success based on improvement to TIMI 2-3 flow should not be interpreted as complete TIMI 3 reperfusion. These differences limit direct comparison of reported technical success rates.

*Resolution of Discrepant Results*

Appendix A documents unresolved source-data discrepancies and their handling in the synthesis. For Hassan et al. (appendix A), the reported overall PCI MACE count of 44/75 was not reconciled with the separately reported in-hospital and postdischarge counts of 22 and 18 [16]. Period-specific counts cannot automatically be summed to establish the number of unique patients with an event. The overall composite was excluded from comparative synthesis, while the period-specific results were retained [16].

For Kotecha et al., alternative control-group MACCE counts of 21 and 22 were retained as reported (appendix A) [17]. Differences among denominators of 433, 436, and 434 were recorded without assuming that they represented identical analysis populations (appendix A). The distinction between reported complication counts of 83 and 87 remained unresolved (appendix A) [17]. No conflicting value was selected solely because it appeared more plausible.

The Spanish follow-up endpoint was attributed to the Heart report [5], whereas the linked American Journal of Cardiology letter supplied procedural information [14]. These publications were not treated as independent registry populations. Incomplete source locators and unavailable supplementary methods remained limitations of the extraction record.

*Risk of Bias*

All eight selected numerical results were judged to have serious overall risk of bias using ROBINS-I (2016). Confounding by indication was the most consistent concern, reflecting baseline differences between patients selected for PCI and those managed conservatively. Additional concerns involved treatment classification, rescue intervention after initial management, mixed revascularization categories, administrative coding, and incomplete information for several domains. Table *5* presents the domain-level judgments. Detailed assessments are provided in appendix E.

| Assessed result | Tier | D1 | D2 | D3 | D4 | D5 | D6 | D7 | Overall |
|---|---|---|---|---|---|---|---|---|---|
| Benenati et al. [3] (DISCO), two-year MACE | 1 | S | NI | L | NI | NI | M | NI | Serious |
| García-Guimaraes et al. [4] (SR-SCAD 2021), in-hospital MAE | 1 | S | NI | L | NI | NI | M | NI | Serious |
| Tweet et al. [1] (Mayo), five-year TVR | 3 | S | NI | L | NI | NI | M | NI | Serious |
| Jensen et al. [6] (Duke), 10-year composite outcome | 3 | S | NI | L | NI | NI | M | NI | Serious |
| Krittanawong et al. [18] (US NIS), in-hospital mortality | 4 | S | NI | S | NI | NI | L | NI | Serious |
| Hassan et al. [16] (Vancouver), in-hospital MACE | 2 | S | NI | S | NI | L | M | NI | Serious |
| Hassan et al. [16] (Vancouver), postdischarge MACE | 2 | S | NI | S | NI | L | M | NI | Serious |
| Kotecha et al. [17] (UK/Dutch/Spanish SCAD), postdischarge MACCE | 2 | S | S | S | NI | NI | M | NI | Serious |

**TABLE 5: Result-specific ROBINS-I (2016) risk-of-bias assessments.**

Tier 1 represents initial-treatment comparisons in confirmed SCAD; tier 2 represents comparisons based on PCI received during or after initial management, including rescue PCI; tier 3 combines PCI and CABG within the revascularization group; tier 4 represents administrative-code evidence. Assessments apply to the eight specified numerical results, rather than automatically to every outcome from each publication. NI must not be interpreted as low risk of bias. Linked or potentially overlapping populations are not independent replications. Detailed judgments and supporting explanations are provided in appendix E.

ROBINS-I: Risk of Bias in Non-randomized Studies of Interventions; D1: confounding; D2: selection of participants; D3: classification of interventions; D4: deviations from intended interventions; D5: missing data; D6: measurement of outcomes; D7: selection of the reported result; L: low risk; M: moderate risk; S: serious risk; NI: no information; MACE: major adverse cardiovascular events; MACCE: major adverse cardiovascular and cerebrovascular events; MAE: major adverse events; NIS: National Inpatient Sample; TVR: target-vessel revascularization; PCI: percutaneous coronary intervention; CABG: coronary artery bypass grafting; SCAD: spontaneous coronary artery dissection; SR-SCAD: Spanish Registry on Spontaneous Coronary Artery Dissection

These assessments concern eight selected numerical results. Other findings, including the Ma et al. comparison, the descriptive SR-SCAD 2022 outcomes, and DISCO in-hospital composite, mortality, myocardial infarction, and repeat revascularization outcomes, did not receive separate formal ROBINS-I assessments [15].

*Certainty of Evidence*

Certainty in comparative causal treatment effects was generally very low. The main concerns were confounding by indication, differences in treatment and outcome definitions, limited precision, and possible overlap between populations. The outcome-level ratings distinguish direct initial-strategy evidence from indirect treatment comparisons. They do not imply that each numerical result received an individual formal risk-of-bias assessment. Table *6* summarizes the judgments, with supporting detail in appendix F.

| Outcome | Contributing evidence | PCI versus conservative | Effect estimate/precision | Certainty and interpretation |
|---|---|---|---|---|
| In-hospital major adverse events | DISCO [3], n=369; SR-SCAD 2021 [4], n=318 | DISCO 9/129 versus 20/240. SR-SCAD 2021 adjusted OR: 5.97 versus reference | DISCO unadjusted RR: 0.84 (95% CI: 0.39-1.78); SR-SCAD adjusted OR: 5.97 (1.78-20.0) | Very low. High start; RoB -2, imprecision -2; other direct-outcome downgrades 0. SR-SCAD 2022 unverified rates excluded |
| Longer-term MACE/MACCE | DISCO [3] direct. SR-SCAD 2022 [5] descriptive follow-up only | DISCO 18 events (13.9%) versus 28 events (11.7%) at two years | DISCO count/percentage and overall-total discrepancy; no reconstructed crude two-year RR. SR-SCAD 2022 46/355 overall only | Very low for direct DISCO comparison. High start; RoB -2, imprecision -1; other direct-outcome downgrades 0 |
| All-cause mortality | DISCO [3] direct; coded administrative cohorts [18,19] indirect and separate | DISCO at two years: 0.7% versus 0.4%; arm death counts one each; conflicting overall total | No reliable direct adjusted effect or compatible interval; unpooled | Very low. High start; RoB -2, imprecision -2; remaining direct-outcome downgrades 0 |
| Myocardial infarction | DISCO [3] direct | At two years: printed 9.3% versus 8.3%; reported counts 10/129 versus 20/240 | Published two-year Kaplan-Meier estimates: 9.3% versus 8.3% (p=0.921). The crude PCI proportion (10/129, 7.75%) is not directly comparable. No crude two-year RR calculated | Very low. High start; RoB -2, imprecision -1; remaining direct-outcome downgrades 0 |
| Repeat PCI/revascularization | DISCO [3] direct; Mayo [1] PCI/CABG combined, indirect | DISCO at two years: 12.4% versus 8.7%; Mayo five-year TVR: 30% versus 19% (indirect) | No common adjusted effect; time-to-event estimates not converted to crude counts | Very low for direct DISCO comparison. High start; RoB -2, imprecision -1; remaining direct-outcome downgrades 0 |
| Recurrent SCAD | No extracted tier 1 initial-strategy effect; Kotecha et al. [17] and Vancouver [16] tier 2 | Kotecha et al. 13/214 versus 15/220; Vancouver 5/75 versus 40/328 | Unadjusted reviewer RRs: 0.89 (0.43-1.83) and RRs: 0.55 (0.22-1.34), respectively | Tier 1 not graded. Tier 2 very low: high start; RoB -2, indirectness -1, imprecision -1 |
| Serious PCI procedural harm | Kotecha et al. [17], 215 PCI recipients | Serious complications 28/215 (13.0%) | No analogous conservative-procedure comparator; descriptive frequency | Not graded as a causal PCI-versus-conservative treatment effect |

**TABLE 6: GRADE summary of findings for clinically important outcomes.**

GRADE: Grading of Recommendations Assessment, Development and Evaluation; MACCE: major adverse cardiovascular and cerebrovascular events; MACE: major adverse cardiovascular events; OR: odds ratio; PCI: percutaneous coronary intervention; RR: risk ratio; SCAD: spontaneous coronary artery dissection; TVR: target-vessel revascularization

Report populations are not additive because of linked publications and possible participant overlap. Crude RRs are reviewer-calculated and unadjusted. Time-to-event estimates were not converted into crude risks. Procedural complication frequencies do not estimate the causal effect of PCI versus conservative management. The Vancouver tier 2 repeat-revascularization result is presented in Table *4* as supplementary indirect evidence and was not included in the GRADE assessment for repeat revascularization.

GRADE was assessed by outcome, distinguishing direct initial-strategy evidence from indirect treatment classifications. Using the ROBINS-I-informed high-starting-certainty approach, each of five direct comparative outcomes was downgraded two levels for risk of bias. In-hospital major adverse events and all-cause mortality were downgraded two levels for imprecision; longer-term MACE/MACCE, myocardial infarction, and repeat revascularization were downgraded one level. Inconsistency, indirectness, and publication bias received no additional numerical downgrade in the five direct comparative profiles. All five final certainty judgments were very low. Recurrent SCAD had no extracted tier 1 initial-treatment effect and was graded very low only for indirect Tier 2 comparisons (risk of bias -2, indirectness -1, imprecision -1).

Serious procedural complications were retained descriptively and were not graded as a between-strategy causal effect.

## Discussion

This review identified substantial baseline differences between patients selected for PCI and those managed conservatively. PCI recipients more often had STEMI, impaired coronary flow, proximal involvement, or other high-risk presentations [3-6,16,17]. The treatment groups were therefore not directly comparable.

The absence of a statistically significant difference in some studies does not establish equivalence. Similarly, higher event rates among PCI recipients cannot be attributed to the intervention without accounting for baseline severity and treatment selection. Differences in rescue treatment, endpoint composition, follow-up, and cohort overlap further constrain interpretation.

The large in-hospital difference in Vancouver, 29.3% versus 2.7% when calculated from reported counts, arose from an analysis that included rescue PCI within the treated group and outcomes that could occur during the procedure [16]. This comparison addresses a different treatment classification from an analysis based solely on the initial strategy. The available aggregate results do not consistently distinguish adverse events that prompted rescue PCI from those occurring during or after the intervention. Consequently, the higher observed event rate in PCI recipients should not be interpreted as a direct measure of PCI-related harm.

The Spanish SR-SCAD 2021 publication reports an adjusted association between initial PCI and in-hospital major adverse events (OR: 5.97, 95% CI: 1.78-20.0) [4]. The linked 2022 Heart publication reports overall hospital and follow-up events from a related registry population; its treatment-stratified comparisons could not be verified against an identifiable primary-source table, figure, or supplementary analysis and were therefore excluded from the numerical synthesis [5]. These reports differ in endpoints, recruitment periods, and analysis populations and cannot be treated as independent replications. DISCO provides a separate direct initial-strategy comparison, but differences in outcome definitions, baseline severity, and possible cohort overlap prevent a pooled causal estimate [3-5].

Administrative analyses add information from large hospitalization datasets but provide indirect evidence. Their diagnoses and treatment classifications are based on codes, and their adjusted mortality estimates differ [18,19]. These results cannot be merged uncritically with angiographically confirmed SCAD cohorts.

An earlier meta-analysis reported similar clinical outcomes and lower target-vessel revascularization with conservative management [7]. The present review emphasizes that the underlying comparisons differ in treatment definition, may share participants, and are affected by substantial confounding. The available evidence does not support a pooled causal estimate from these heterogeneous comparisons.

Procedural studies document important technical hazards. The Spanish letter reports an overall complication estimate of 37%, Kotecha et al. report serious complications in 28/215 patients, and Vancouver reports dissection propagation in 33/75 patients [14,16,17]. These endpoints are not interchangeable. Overall complication counts, serious complications, and propagation should remain separately labeled. Nevertheless, coronary flow improved in many PCI recipients with reduced baseline flow [17]. This finding describes a procedural result within a selected treated population and does not establish comparative clinical benefit over conservative management.

A predominantly conservatively managed Swiss cohort documented spontaneous healing during selected angiographic follow-up, although this selected population does not establish a healing rate for all conservatively managed patients [20]. Other observational reports provide additional clinical context but do not contribute independent treatment-effect estimates to this review [21,22]. These findings should not be interpreted as supporting avoidance of PCI in patients with ongoing ischemia, hemodynamic instability, impaired coronary perfusion, or threatened myocardium. Rather, they emphasize that treatment decisions in SCAD remain dependent on individual clinical and angiographic circumstances, while available observational comparisons cannot reliably determine the relative causal benefit of either strategy.

Future studies should define treatment intent at diagnostic angiography and explicitly distinguish planned intervention from rescue treatment. Initial-strategy analyses should retain rescue procedures within the initial conservative group where appropriate to the estimand. Investigators should report baseline coronary flow, clinical instability, lesion extent, reasons for intervention, and timing of events.

Standardized endpoint definitions, group-specific event counts, confidence intervals, and complete follow-up are needed. Participant-level collaboration across registries could improve precision, but it would first require explicit identification and resolution of overlapping patients. Analytical methods must also address baseline clinical and angiographic differences rather than relying on unadjusted comparisons alone.

## Limitations

The included evidence was observational and substantially affected by confounding by indication. Patients selected for PCI frequently had greater clinical or angiographic severity. Differences in intervention timing, rescue PCI classification, combined PCI/CABG exposure, outcome definitions, and follow-up limited comparability.

Several publications originated from linked or potentially overlapping populations, and participant-level independence could not be established. Sparse events and limited precision affected clinically important outcomes. Numerical discrepancies involving Hassan et al. and Kotecha et al. were documented without unsupported imputation or selection of preferred values [16,17]. Several source locators and supplementary methods remained unavailable or unverified.

Search coverage was limited to PubMed/MEDLINE and an earlier combined Semantic Scholar/OpenAlex export; Embase, CENTRAL, Scopus, and Web of Science were not searched. Although the October 19 PubMed export and record-level duplicate crosswalk were retained, the original July search execution history, complete native search histories for the earlier export, and historical individual reviewer decision logs were unavailable. Retained records supported reconciliation of aggregate counts but did not permit complete reconstruction of the search and screening process. Screening, extraction, and risk-of-bias assessment were undertaken jointly rather than as independent duplicate assessments. The October 20 executed query could not be checked against the transcription in appendix C. Four reports were not retrieved, including Narducci et al., which reported an original observational SCAD cohort with conservative and revascularization treatment groups [24]. Because the full text was unavailable for eligibility assessment, its potential contribution to the review could not be established. This may have affected the completeness of the evidence synthesis. Formal ROBINS-I assessment covered eight selected results rather than every result contributing to the review. Unresolved source-data discrepancies and their consequences for synthesis are documented in appendix A. No formal statistical assessment of reporting bias was performed.

The review was not prospectively registered. The timing of finalization of the original protocol relative to review conduct could not be independently established, and amendments dated October 20, 2026, followed the documented PubMed search and a preliminary evidence review.

### Data availability

Materials supporting this review are provided in the separate supplementary appendix A-H file, available through the Google Drive link in the appendix section as follows: source-data discrepancies and handling decisions (appendix A); supplementary data extraction (appendix B); search strategies and available search documentation (appendix C); full-text exclusions and reports not retrieved (appendix D); detailed ROBINS-I assessments (appendix E); detailed GRADE assessments (appendix F); and completed PRISMA 2020 and SWiM checklists (appendices G and H). The retained October 19, 2026 PubMed export and record-level duplicate crosswalk were used to reconcile identification and deduplication counts but are not included in the published appendices. The original July search execution history, complete native-platform histories for the earlier Semantic Scholar/OpenAlex export, and historical individual reviewer decision logs were unavailable. Appendix A documents unresolved source-data discrepancies and their handling.

## Conclusions

Available observational evidence does not establish whether initial PCI or initial conservative management produces superior clinical outcomes in SCAD. Treatment-selection bias, inconsistent treatment and outcome definitions, and potential cohort overlap limit interpretation of published comparisons.

More reliable evidence requires consistent definition of the initial treatment strategy, explicit handling of rescue procedures, standardized clinically important outcomes, complete follow-up, and analytical methods that address baseline clinical and angiographic differences between treatment groups.

## Appendices

Supplementary Appendices A-H are available at the following Google Drive link:

https://drive.google.com/file/d/1pNoa3Jn4m0zL7PHjdoCIaWYYsNQFPves/view?usp=sharing

Appendix A: source data discrepancies and handling decisions.
Table S1: source discrepancies and handling decisions.

Appendix B: supplementary data extraction.
Table S2: study and source characteristics.
Table S3: clinical outcome extraction.
Table S4: PCI procedural outcomes and contextual clinical course.
Table S5: cohort overlap and analysis rules.

Appendix C: search sources strategies and PRISMA S documentation.

Appendix D: full-text exclusions and reports not retrieved.

Appendix E: detailed ROBINS-I assessments.

Appendix F: detailed GRADE assessments.

Appendix G: PRISMA 2020 checklist.

Appendix H: SWiM checklist.

Numbered citations in the supplementary file refer to the reference list in the main article.

# Additional Information

## Author Contributions

All authors have reviewed the final version to be published and agreed to be accountable for all aspects of the work.

**Concept and design:** Aoumar G. Chamma, Wendy Saliba, Linda Chamma

**Acquisition, analysis, or interpretation of data:** Aoumar G. Chamma, Wendy Saliba, Linda Chamma

**Drafting of the manuscript:** Aoumar G. Chamma, Wendy Saliba, Linda Chamma

**Critical review of the manuscript for important intellectual content:** Aoumar G. Chamma, Wendy Saliba, Linda Chamma

**Supervision:** Aoumar G. Chamma

## Disclosures

**Conflicts of interest:** In compliance with the ICMJE uniform disclosure form, all authors declare the following: **Payment/services info:** All authors have declared that no financial support was received from any organization for the submitted work. **Financial relationships:** All authors have declared that they have no financial relationships at present or within the previous three years with any organizations that might have an interest in the submitted work. **Other relationships:** All authors have declared that there are no other relationships or activities that could appear to have influenced the submitted work.

# References

1. Tweet MS, Eleid MF, Best PJ, et al.: Spontaneous coronary artery dissection: revascularization versus conservative therapy. Circ Cardiovasc Interv. 2014, 7:777-86. 10.1161/CIRCINTERVENTIONS.114.001659
2. Tweet MS, Hayes SN, Pitta SR, et al.: Clinical features, management, and prognosis of spontaneous coronary artery dissection. Circulation. 2012, 126:579-88. 10.1161/CIRCULATIONAHA.112.105718
3. Benenati S, Giacobbe F, Zingarelli A, et al.: Interventional versus conservative strategy in patients with spontaneous coronary artery dissections: insights from DISCO Registry. Circ Cardiovasc Interv. 2023, 16:10.1161/CIRCINTERVENTIONS.122.012780
4. García-Guimaraes M, Bastante T, Macaya F, et al.: Spontaneous coronary artery dissection in Spain: clinical and angiographic characteristics, management, and in-hospital events. Rev Esp Cardiol (Engl Ed). 2021, 74:15-23. 10.1016/j.rec.2020.04.002
5. García-Guimaraes M, Masotti M, Sanz-Ruiz R, et al.: Clinical outcomes in spontaneous coronary artery dissection. Heart. 2022, 108:1530-8. 10.1136/heartjnl-2022-320830
6. Jensen CW, Kang L, Moya-Mendez ME, et al.: Initial management strategy and long-term outcomes in 186 cases of spontaneous coronary artery dissection. Semin Thorac Cardiovasc Surg. 2024, 36:387-97. 10.1053/j.semtcvs.2023.05.001
7. Bocchino PP, Angelini F, Franchin L, et al.: Invasive versus conservative management in spontaneous coronary artery dissection: a meta-analysis and meta-regression study. Hellenic J Cardiol. 2021, 62:297-303. 10.1016/j.hjc.2021.02.013
8. Page MJ, McKenzie JE, Bossuyt PM, et al.: The PRISMA 2020 statement: an updated guideline for reporting systematic reviews. BMJ. 2021, 372:10.1136/bmj.n71
9. Campbell M, McKenzie JE, Sowden A, et al.: Synthesis Without Meta-analysis (SWiM) in systematic reviews: reporting guideline. BMJ. 2020, 368:10.1136/bmj.l6890
10. Rethlefsen ML, Kirtley S, Waffenschmidt S, Ayala AP, Moher D, Page MJ, Koffel JB: PRISMA-S: an extension to the PRISMA statement for reporting literature searches in systematic reviews. Syst Rev. 2021, 10:10.1186/s13643-020-01542-z
11. Sterne JA, Hernán MA, Reeves BC, et al.: ROBINS-I: a tool for assessing risk of bias in non-randomised studies of interventions. BMJ. 2016, 355:10.1136/bmj.i4919
12. Schünemann HJ, Higgins JP, Vist GE, Glasziou P, Akl EA, Skoetz N, Guyatt GH: Completing 'summary of findings' tables and grading the certainty of the evidence. Cochrane Handbook for Systematic Reviews of

Interventions. Second Edition. Higgins JP, Thomas J, Chandler J, Cumpston M, Li T, Page MJ, Welch VA (ed): Cochrane, London, UK; 2024.

13. Schünemann HJ, Cuello C, Akl EA, et al.: GRADE guidelines: 18. How ROBINS-I and other tools to assess risk of bias in nonrandomized studies should be used to rate the certainty of a body of evidence. J Clin Epidemiol. 2019, 111:105-14. 10.1016/j.jclinepi.2018.01.012
14. García-Guimaraes M, Fuertes-Ferre G, Jiménez-Valero S, et al.: Characteristics, acute results, and prognostic impact of percutaneous coronary interventions in spontaneous coronary artery dissection (from the prospective Spanish Registry on SCAD [SR-SCAD]). Am J Cardiol. 2022, 171:177-8. 10.1016/j.amjcard.2022.02.008
15. Ma Y, Zhong X, Yin J, Lu H, Pan C, Huang D, Ge J: Treatment strategy for spontaneous coronary artery dissection based on anatomical characteristics. Eur J Med Res. 2023, 28: 10.1186/s40001-023-00986-y
16. Hassan S, Samuel R, Starovoytov A, Lee C, Aymong E, Saw J: Outcomes of percutaneous coronary intervention in patients with spontaneous coronary artery dissection. J Interv Cardiol. 2021, 2021:10.1155/2021/6686230
17. Kotecha D, García-Guimaraes M, Premawardhana D, et al.: Risks and benefits of percutaneous coronary intervention in spontaneous coronary artery dissection. Heart. 2021, 107:1398-406. 10.1136/heartjnl-2020-318914
18. Krittanawong C, Rodriguez BC, Ang SP, et al.: Conservative approach versus percutaneous coronary intervention in patients with spontaneous coronary artery dissection from a national population-based cohort study. Rev Cardiovasc Med. 2024, 25:10.31083/j.rcm2511404
19. Alfonso F, Fernández-Pérez C, del Prado N, et al.: Characteristics and outcomes of percutaneous coronary interventions in patients with spontaneous coronary artery dissection. A study from the administrative minimum data set of the Spanish National Health System. Front Cardiovasc Med. 2022, 9:10.3389/fcvm.2022.1054413
20. Seidl S, Rickli H, Rogowski S, et al.: Long-term follow-up of medically treated patients with spontaneous coronary artery dissection: a prospective, Swiss single-centre cohort study. Swiss Med Wkly. 2021, 151:10.4414/smw.2021.w30067
21. Vanzetto G, Berger-Coz E, Barone-Rochette G, et al.: Prevalence, therapeutic management and medium-term prognosis of spontaneous coronary artery dissection: results from a database of 11,605 patients. Eur J Cardiothorac Surg. 2009, 35:250-4. 10.1016/j.ejcts.2008.10.023
22. Meng PN, Xu C, You W, et al.: Spontaneous coronary artery dissection as a cause of acute myocardial infarction in young female population: a single-center study. Chin Med J (Engl). 2017, 130:1534-9. 10.4103/0366-6999.208245
23. Manhães EB, Gomes WF, Bezerra CG, et al.: Dissecção espontânea de artéria coronária: abordagem terapêutica e desfechos de uma série consecutiva de casos. Rev Bras Cardiol Invasiva. 2014, 22:32-5. 10.1590/0104-1843000000007
24. Narducci ML, Flore F, Simonini C, et al.: High incidence of adverse events in spontaneous coronary artery dissection patients during mid-term follow-up: a persistent challenge ahead. Minerva Cardiol Angiol. 2025, 73:713-21. 10.23736/S2724-5683.25.06814-0