



Syncope Pathways and the Unnecessary Admission: Risk Scores, Observation Units and What Actually Reduces Bed Days

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Abstract

Roughly one-third of syncope patients evaluated in United States emergency departments have historically been admitted, and reported admission rates after emergency syncope evaluation range from under 20% in Canada to over 80% in the United States, a spread far wider than any difference in underlying illness could explain. Validated risk scores exist to narrow that gap: the Canadian Syncope Risk Score (CSRS) discriminates well (area under the curve 0.91 in its derivation and validation cohorts, 0.85 internationally across 8 countries), and its very-low- and low-risk categories carry 30-day serious adverse event rates of 0.3% and 0.7%. The older San Francisco Syncope Rule performs far less consistently outside its own derivation set: sensitivity fell from 96-98% at derivation to 74-89% in independent validations, with one external cohort finding the rule would have cut admissions by 41% only by also missing more serious outcomes than its authors intended. A retrospective review of 200 hospitalized syncope patients across 11 emergency departments found that 39% had a low-risk CSRS despite being admitted, with an estimated 30-day serious event rate under 1% in that group -- a concrete estimate of how much current admission practice may not track actual risk. Yet the evidence that simply handing clinicians a risk score changes what they do is weaker than the scores' own statistics suggest: a retrospective cohort study found that applying syncope guidelines in the emergency department did not reduce admission rates, and a pilot stepped-wedge implementation trial of the Canadian syncope pathway saw reach into eligible patients fall from 67.5% to 55.0% over its rollout even as physician adoption and recommendation fidelity rose. Structural changes look more durable: a randomized trial of a dedicated emergency department observation protocol for intermediate-risk syncope cut admission from 92% to 15% and hospital length of stay from 47 to 29 hours, with similar safety outcomes at 30 days and 6 months and a modest cost saving, changes a risk score alone cannot deliver without somewhere structured to send the patients it identifies as not needing an inpatient bed. This review sets the risk-score evidence and the structural-pathway evidence for syncope side by side and argues that the honest, if less satisfying, conclusion is that a validated score is necessary but not sufficient: the observation unit or structured pathway built to receive its low-risk output is doing most of the work of actually reducing bed days.

Keywords: Syncope; Risk assessment; Clinical decision rules; Emergency service, hospital; Observation units

Introduction

Syncope -- transient loss of consciousness with spontaneous, complete recovery -- sends a large volume of patients to

emergency departments worldwide, and what happens to them next varies enormously depending on where, rather than how sick, they happen to be. Reported hospital admission rates after

an emergency department syncope evaluation range from under 20% in Canada to over 80% in the United States, and roughly one-third of syncope patients evaluated in United States emergency departments have historically been admitted [14]. Most of those admissions end without a serious cardiac cause being found, which is precisely why syncope has become a proving ground for both validated risk-stratification tools and, more recently, structured emergency-department pathways built specifically to avoid unnecessary inpatient bed days.

Two families of intervention are usually offered as the fix. The first is the clinical decision rule: a small number of history, examination and ECG variables combined into a score that separates patients unlikely to have a serious 30-day outcome from those who need further workup or monitoring. The second is the structural pathway: a dedicated emergency-department observation protocol, syncope unit, or care pathway that gives clinicians somewhere specific to send a risk-stratified patient rather than the binary choice of discharge or an inpatient bed. This review reads the evidence behind both families side by side -- the San Francisco Syncope Rule and the Canadian Syncope Risk Score as the two most-studied decision rules, and the observation-unit and implementation-trial literature as the structural alternative -- to ask which one actually moves the admission-rate and bed-day numbers when tested in practice, not only in a derivation cohort.

Neither family is a new idea by 2026; risk-stratification rules for syncope have been published and re-tested for over two decades, and dedicated emergency-department observation protocols have existed almost as long. What has accumulated in that time is not agreement on a single best approach, but a body of evidence unusually well suited to answering the specific question this review asks, because several of the same research groups that derived and validated these risk scores went on to test, separately, whether deploying them changed real admission decisions -- a second step that many decision-rule literatures never take.

The honest complication this review foregrounds is that these two families do not perform equally well once they leave the controlled conditions of a validation study. A risk score can discriminate excellently in the population it was built and tested on and still fail to change clinician behaviour once deployed, while a structural pathway change that gives a physician somewhere to send a low-risk patient has repeatedly shown a large, measurable effect on admission and length of stay in randomized comparison. That asymmetry, not a simple story of risk-score adoption solving the unnecessary-admission problem, is what the literature actually supports.

Material and Methods

This is a narrative review drawing on the derivation and validation publications of the San Francisco Syncope Rule and the Canadian Syncope Risk Score, an international multicenter validation cohort, a retrospective analysis of the potential impact of risk-score-guided disposition on an existing hospitalized syncope cohort, a retrospective cohort study of guideline application and admission rates, a pilot stepped-wedge implementation trial of a structured syncope pathway, a barriers-and-facilitators qualitative study of the same implementation programme, a randomized controlled trial of an emergency-department observation protocol versus routine admission, and recent narrative reviews summarizing cross-country admission-rate variation, identified through the sources cited here and citation tracking between them, without a pre-registered protocol or systematic search string. All percentages, sensitivity/specificity values, area-under-the-curve estimates, hours, dollar figures and p-values reported here are taken directly from the cited sources; none has been invented, estimated, or extrapolated.

Results

The scale of the problem: wide, largely unexplained variation in who gets admitted

Reported syncope admission rates vary from under 20% in Canadian emergency departments to over 80% in United States emergency departments, and roughly one-third of United States syncope patients have historically been admitted overall [14]. This spread is far wider than any plausible difference in the underlying rate of serious cardiac disease between health systems, and it is the central fact motivating both risk-score development and structural pathway design: if admission decisions tracked risk consistently, national admission rates would not differ by a factor of four.

The San Francisco Syncope Rule: a rule whose performance did not travel well

The San Francisco Syncope Rule combines a history of congestive heart failure, an abnormal ECG, shortness of breath, a systolic blood pressure below 90 mmHg, and an haematocrit below 30% into a binary decision aid. In its original derivation, it identified serious 7-day outcomes with 96% sensitivity and 62% specificity [1], and an internal prospective validation by the same group reported 98% sensitivity and 56% specificity, projecting a 7% reduction in admissions if the rule were applied [2]. Figure 1 sets these numbers against what happened when independent groups tested the rule elsewhere.

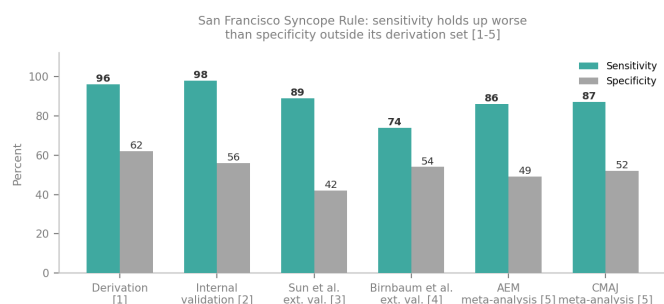


Figure 1: Sensitivity and specificity of the San Francisco Syncope Rule across its derivation, internal validation, two external validations, and two pooled meta-analyses [1-5].

An external validation by Sun and colleagues in 477 patients found lower performance, 89% sensitivity and 42% specificity [3], and a second independent validation by Birnbaum and colleagues found sensitivity fell further still, to 74%, missing more than a quarter of patients who went on to have a serious outcome without meeting any of the rule's criteria [4]. That same study estimated the rule would have reduced the overall admission rate by 41% -- a large apparent gain that came only because the rule was, at that sensitivity, also willing to discharge some patients who were not actually low risk [4]. Pooled meta-analyses have since converged on intermediate figures of roughly 86-87% sensitivity and 49-52% specificity across the available validation cohorts [5]. Cardiac dysrhythmia was the most frequently missed serious outcome across these external validations [4]. The pattern across Figure 1 is consistent: specificity was modest even at derivation, but it is sensitivity -- the property that determines how many at-risk patients a fully-implemented rule would send home -- that degraded most from derivation to independent testing.

The practical consequence for a department considering the San Francisco Syncope Rule is that its two headline numbers cannot simply be read off the derivation paper and expected to hold locally. A rule reported at 96-98% sensitive and 56-62% specific looks like it discharges roughly half of truly low-risk patients while catching nearly all serious outcomes; a rule that behaves, once deployed, closer to the 74-89% sensitivity actually observed on independent testing [3,4] will miss a meaningfully larger share of the dysrhythmias, strokes, intracranial haemorrhages, and structural cardiac diagnoses the rule was built to catch [4]. A newer score does not automatically avoid the same fate, which is why the Canadian Syncope Risk Score's own measured very-low- and low-risk category event rates of 0.3% and 0.7% [6,7], and its international validation's low-risk event rate of 0.6% [8], matter as much as its headline area-under-the-curve figures: it is the low-risk categories' event rate, replicated outside the derivation cohort, that determines whether a discharge decision built on the score is actually safe once the tool leaves the hands that built it.

The Canadian Syncope Risk Score: a more consistent discriminator

The Canadian Syncope Risk Score (CSRS) was developed from nine predictors, including vasovagal predisposition, a history of heart disease, an abnormal blood pressure at triage, an elevated troponin, and an abnormal ECG, weighted into a score from -3 to +11 [6]. In its development and multicenter validation cohorts, the 30-day serious adverse event rate was 0.3% in the very-low-risk category, 0.7% in the low-risk category, roughly 8% in the medium-risk category, roughly 20% in the high-risk category, and roughly 50% in the very-high-risk category, with an overall area under the curve of 0.91 (95% CI 0.88-0.93) and, at a threshold of -1, a sensitivity of 97.8% (95% CI 93.8-99.6%) and specificity of 44.3% [6,7]. Table 1 sets these risk categories and their event rates out in full.

CSRS risk category (points)	30-day serious adverse event rate	Suggested disposition
Very low risk (≤ -2)	0.3%	Discharge
Low risk (-1 to 0)	0.7%	Discharge, outpatient follow-up
Medium risk (1 to 3)	~8%	Further workup / monitoring
High risk (4 to 5)	~19-20%	Monitored observation or admission
Very high risk (≥ 6)	~50%	Admission, urgent workup

Table 1: Canadian Syncope Risk Score categories and their measured 30-day serious adverse event rates [6,7].

An international validation extended this beyond Canada: 2,283 patients from 8 countries across 3 continents, mean age 68, were assessed, with a 30-day serious adverse event rate of 7.2% for the full composite outcome and 3.1% once procedural interventions were excluded [8]. The CSRS again discriminated well in this international cohort, with an area under the curve of 0.85 (95% CI 0.83-0.88) for the primary composite outcome and 0.80 (95% CI 0.75-0.84) for the secondary outcome, outperforming the older OESIL score, whose corresponding areas under the curve were 0.74 and 0.69 [8]. Among patients classified as low risk, the serious adverse event rate was 0.6% (19 of 1,388) for the CSRS compared with 1.5% (17 of 1,104) for OESIL [8]. Figure 2 sets these discrimination statistics out together, and a notable secondary finding from the CSRS's own validation work is that a simplified model built from only the treating clinician's overall syncope classification achieved similar discrimination (area under the curve 0.83) to the full multi-variable score, suggesting a meaningful share of the score's performance is carried by a single clinical-gestalt variable rather than by its more granular components [7].

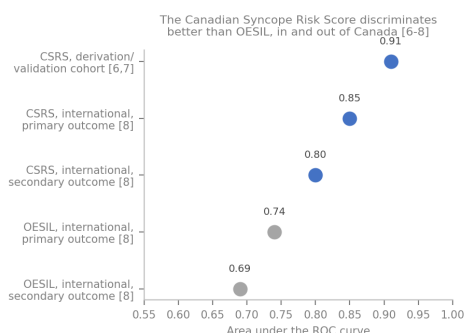


Figure 2: Discrimination (area under the curve) of the Canadian Syncope Risk Score versus the OESIL score, in derivation/validation and international cohorts [6-8].

How much of current admission practice is actually unnecessary by these scores' own criteria

A retrospective analysis applied the CSRS to a cohort of patients who had already been hospitalized for syncope, screening 5,718 hospitalized syncope patients across 11 emergency departments and analysing the 200 who met full inclusion criteria after excluding those with comorbidities independently requiring admission or those actually discharged from the emergency department [9]. Figure 3 sets this selection process out in full. Thirty-nine percent of the hospitalized patients analysed had a low-risk CSRS despite having been admitted, with an estimated 30-day serious adverse event rate below 1% in that low-risk admitted group [9]. Those low-risk admitted patients were meaningfully different from their higher-risk, also-admitted counterparts: younger on average (mean 61.2 versus 70.6 years), far less likely to have known cardiac disease (1.3% versus 61.8%), and more likely to have a documented substance use disorder (14.3% versus 4.9%) [9]. This is the closest this literature comes to a direct estimate of how much current inpatient syncope care may not track a validated measure of risk -- and it describes a retrospective, single-time-point gap between actual practice and risk-score-concordant practice, not a demonstrated causal reduction achievable simply by introducing the score.

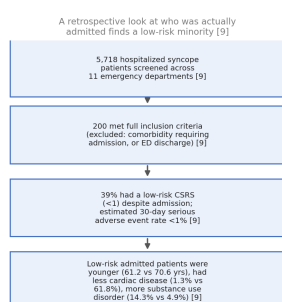


Figure 3: Retrospective analysis of 5,718 hospitalized syncope patients: selection to the analysed cohort and the proportion found to be low risk by the Canadian Syncope Risk Score despite admission [9].

The gap between validating a score and changing what clinicians actually do

The discrimination statistics in Figures 1 and 2 describe how well these tools separate risk categories in a research cohort; they say nothing directly about whether introducing a score into routine practice changes admission decisions. A retrospective cohort study designed specifically to test this found that application of syncope guidelines in the emergency department did not reduce admission rates [13] -- a title that is itself the study's central, sobering finding, and one that sits uncomfortably next to the strong discrimination reported for the CSRS in Figure 2. A qualitative study of 41 emergency physicians identified why: roughly 98% of physicians agreed with CSRS-based recommendations to discharge low-risk patients, but for medium- and high-risk patients, a lack of access to monitors and to timely outpatient follow-up for the recommended 15-day cardiac monitoring meant physicians often chose brief hospitalization instead, citing patient-safety concerns rather than disagreement with the score itself [11]. In other words, physicians accepted the score's classification but lacked the infrastructure the score's own recommendations assumed would be available.

A pilot non-randomized stepped-wedge trial of a structured Canadian syncope pathway -- combining the CSRS with explicit practice recommendations -- at two hospitals over 7 months (349 patients in the control period, 653 in the intervention period, mean age 56.6, 51% male) illustrates the same tension in prospective, quantitative form [10]. Figure 4 tracks its three pre-specified process measures from the first month of intervention to its end: physician adoption of the pathway rose from 70.7% to 84.4%, and fidelity to the pathway's recommendation once applied stayed roughly stable at 83.8% to 83.3%, but reach -- the proportion of eligible patients actually covered by the pathway -- fell from 67.5% to 55.0% over the same period [10]. The trial met every pre-specified benchmark required to proceed to a larger multicenter implementation trial except this one on reach [10], a result that reads less as failure than as an honest description of the gap between a pathway working well when used and a pathway reliably reaching the patients it was designed for.

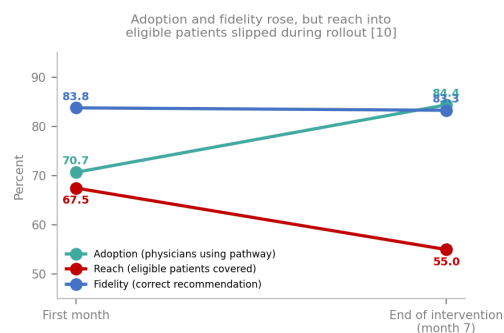


Figure 4: Process measures from a pilot stepped-wedge trial of the Canadian syncope pathway, first month versus end of a 7-month rollout [10].

What a structural pathway change looks like when it works: the observation-unit trial

Figure 5 sets out the structural alternative to a risk score used in isolation: a pathway that pairs risk stratification with a specific, protocol-driven place to send patients who are not clearly low risk but do not obviously need an inpatient bed either. A randomized controlled trial tested exactly this idea directly against routine practice: 124 patients aged 50 years or older with syncope or near-syncope, assessed as intermediate risk across 5 emergency departments, were randomized to a structured emergency-department observation protocol or to routine inpatient admission [12]. The results were large and precise. Hospital admission occurred in 15% of the observation group versus 92% of the admission group (95% CI for the difference, -88% to -66%), and hospital length of stay averaged 29 hours in the observation group versus 47 hours in the admission group (95% CI for the difference, -28 to -8 hours) [12]. Figure 6 sets both outcomes out directly. Safety after discharge was similar between groups: serious outcomes occurred in 3% versus 0% of patients at 30 days (95% CI for the difference, -1% to 8%) and in 8% versus 10% at 6 months (95% CI for the difference, -13% to 9%) [12]. Index hospital costs were approximately \$629 lower in the observation group (95% CI, -\$1,376 to -\$56), and no meaningful difference was detected in 30-day quality-of-life scores or patient satisfaction [12].

A risk score sorts patients; a dedicated observation pathway is what changes their bed [11-13]

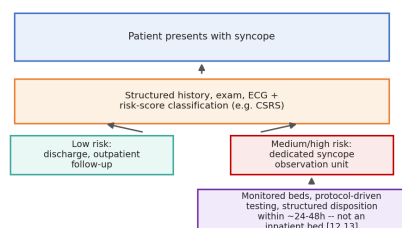


Figure 5: A structural syncope pathway: risk stratification paired with a dedicated observation-unit disposition, rather than a binary discharge-or-admit decision [11,12].

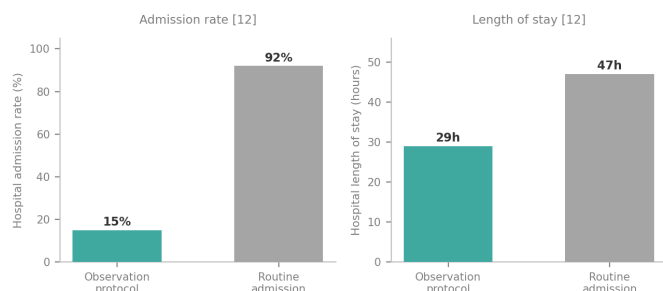


Figure 6: Randomized comparison of an emergency-department observation protocol versus routine inpatient admission for intermediate-risk syncope: admission rate and hospital length of stay [12].

Table 2 places the risk-score evidence and the structural-pathway evidence next to each other in summary form. The contrast is the central finding of this review: a validated score can discriminate risk categories with an area

under the curve above 0.85 and still not, by itself, move the admission-rate needle in routine practice, while a randomized structural pathway change moved admission from 92% to 15% in a single trial. Both facts are real and well-supported; they simply answer different questions, and only one of them has been shown to change bed-day outcomes when actually tested against usual care.

Intervention	What it changes	Measured effect on admission / bed days
Risk score alone (CSRS, adopted into guidelines) [11,13]	Classifies patients by measured risk (AUC up to 0.91)	Retrospective cohort: guideline application did not reduce admission rates [13]
Risk-score implementation pathway, pilot [10]	Adds explicit practice recommendations + training	Adoption/fidelity rose, but reach into eligible patients fell 67.5%→55.0% [10]
Dedicated ED observation protocol, RCT [12]	Gives clinicians a structured place to send intermediate-risk patients	Admission 92%→15%; length of stay 47h→29h; similar safety at 30d/6mo [12]

Table 2: Three intervention types aimed at reducing unnecessary syncope admissions, and what each has actually been shown to change when tested [10,12,13].

Discussion

Read together, this evidence supports a specific and somewhat uncomfortable conclusion: a validated risk score is necessary but not sufficient to reduce unnecessary syncope admissions. The Canadian Syncope Risk Score discriminates risk better than its predecessors, internationally as well as in Canada [6-8], and a retrospective look at real hospitalized patients suggests a genuinely low-risk minority -- 39% in one 11-site analysis -- is being admitted despite a sub-1% estimated serious event rate [9]. But when the same investigators who built and validated the score tried to see whether using it changed what emergency physicians actually did, the results were mixed at best: a retrospective cohort study found guideline application did not reduce admission rates [13], and a prospective implementation pilot saw the pathway's reach into eligible patients fall rather than rise over its rollout even as physician agreement with its recommendations stayed high [10,11]. The barrier was not disagreement with the score -- roughly 98% of interviewed physicians endorsed its low-risk discharge recommendation -- but the absence of the infrastructure its medium- and high-risk recommendations assumed, chiefly rapid access to ambulatory cardiac monitoring [11].

The observation-unit trial is the clearest demonstration in this literature of an intervention that did change bed-day outcomes when tested head-to-head against usual care, cutting admission from 92% to 15% and length of stay nearly in half, with safety outcomes that were statistically indistinguishable at

30 days and 6 months [12]. The mechanism is not mysterious: an observation protocol gives a clinician a concrete, protocol-driven alternative to the binary choice between sending an intermediate-risk patient home and admitting them to an inpatient bed, whereas a risk score alone only tells the clinician which category the patient is in without changing what options exist once that classification is made. This reading is consistent with why the San Francisco Syncope Rule's real-world admission-rate reductions have also proven hard to bank on: its own internal validation projected only a 7% reduction if applied, and the much larger 41% reduction seen in an external cohort came bundled with a drop in sensitivity that would, if the rule were actually followed, have sent home a meaningfully higher share of patients with a genuine serious outcome [2,4].

The economics of the structural approach are also more favourable than a risk score alone can offer, and not only in the syncope-specific trial reviewed here. A national analysis of dedicated hospital observation units across all short-stay diagnoses, not only syncope, estimated that if every hospital lacking one added a well-run observation unit, the resulting national savings would reach \$3.1 billion a year, driven by the same mechanism seen in the syncope-specific trial: patients who would otherwise occupy an inpatient bed for a full admission are instead managed, tested, and discharged from a shorter, protocol-driven observation stay [15]. That figure is not specific to syncope, and should not be read as an additional syncope-specific saving on top of the \$629 per-patient difference reported by Sun and colleagues [12], but it does show that the observation-unit mechanism this review highlights for syncope is part of a broader, independently replicated pattern in short-stay hospital economics, rather than a one-off finding limited to this single condition [12,15].

None of this argues against using a validated risk score; the Canadian Syncope Risk Score's discrimination is real, reproducible across continents, and considerably better than the tools it is displacing [6-8]. It argues instead that the score should be understood as one component of a system change rather than as the change itself. A health system that adopts the CSRS without also building the observation capacity, monitoring access, and rapid outpatient follow-up its own risk categories imply should not expect its admission rate to move much, exactly as the retrospective guideline-application study found [13]. A health system that builds a dedicated observation pathway, by contrast, has direct randomized evidence that admission and bed-days will fall substantially, whether or not a formal risk score is the instrument used to select who goes into that pathway [12].

Study limitations

This is a narrative review, not a systematic review with a pre-registered protocol, and its conclusions rest on a curated set of decision-rule validation studies, one retrospective potential-impact analysis, one implementation pilot, one barriers-and-facilitators qualitative study, and one randomized observation-unit trial, rather than an exhaustive search of the syncope disposition literature. The retrospective potential-impact analysis (39% of hospitalized patients low-risk) describes association at a single time point across 11 emergency departments, not a trial of what would happen if those patients were prospectively discharged, and its authors explicitly frame it as a retrospective estimate rather than validated prospective safety data [9]. The pilot stepped-wedge implementation trial was non-randomized, ran at only two hospitals over 7 months, and reported process measures (adoption, reach, fidelity) rather than a hospitalization or bed-day endpoint, so its finding that reach fell during rollout is informative about implementation friction but does not by itself quantify an admission-rate effect [10]. The observation-unit randomized trial, though methodologically the strongest single study reviewed here, enrolled only 124 patients across 5 emergency departments and restricted eligibility to patients 50 years or older at intermediate risk, so its very large admission-rate and length-of-stay effects may not generalize without modification to lower- or higher-risk populations, different health systems, or emergency departments without pre-existing observation-unit infrastructure [12].

A further point worth stating plainly for practice: this review's central contrast -- risk score versus structural pathway -- is not a strictly fair comparison, because no single trial identified here randomized a risk-score-alone strategy against a structural-pathway strategy head-to-head with admission or bed-days as the primary outcome. The retrospective guideline-application study and the observation-unit randomized trial answer related but distinct questions using different designs, patient populations, and time periods, so the conclusion that structural pathway change outperforms risk-score adoption alone is an inference drawn across separate studies rather than a finding from one head-to-head comparison [12,13]. The San Francisco Syncope Rule's declining sensitivity across independent validations is itself a caution about generalizing any single validation cohort's numbers, a caution that should also be applied, with appropriate humility, to the Canadian Syncope Risk Score's own strong international validation statistics as further, larger cohorts are studied over time [1-4,8].

Conclusion

Validated syncope risk scores, particularly the Canadian Syncope Risk Score, discriminate genuine risk well across derivation, multicenter, and international validation cohorts, and a retrospective analysis suggests a real, sizeable minority of current hospital admissions for syncope occur in patients such a score would classify as low risk. But the literature testing whether simply introducing a score changes admission rates in practice is disappointing: guideline application alone has been shown not to reduce admissions, and a prospective implementation pilot saw the pathway's reach into eligible patients fall during rollout even as clinicians agreed with its recommendations. What has been shown, in a randomized trial with precise and clinically important effect sizes, to reduce admission and length of stay without a detectable safety cost is a dedicated emergency-department observation pathway. For a health system trying to reduce unnecessary syncope bed-days, the honest reading of this evidence is that adopting a risk score is a reasonable and evidence-based first step, but the observation unit or structured pathway built to act on that score's output is doing most of the actual work.

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