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When Clinical Risk Scores Compete for Limited Review Slots

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ABSTRACT

Clinical risk scores can guide follow-up when care teams have fewer review slots than eligible patients. However, patients with noisier or systematically delayed inputs may be overrepresented or underrepresented at the top of a common ranking. We evaluated three policies in a paired Monte Carlo simulation: raw score ranking, ranking that accounts for group-specific measurement variance, and variance-aware ranking with a minimum allocation to the noisier group. Each of 160 synthetic cohorts included 4,000 candidates. Policies were evaluated under four measurement conditions at review capacities of 10%, 15%, and 25%. At 15% capacity, variance-aware ranking captured 1.42 additional events per 1,000 candidates under unequal but unbiased noise and 5.00 under upward bias. Under downward bias it captured 0.54 fewer events per 1,000 than raw ranking. Adding a 30% minimum allocation in the downward-bias scenario increased the noisier group's mean event recall from 20.85% to 24.84% and recovered 0.32 events per 1,000 relative to variance-aware ranking alone. The results show how measurement variance and directional bias can change the consequences of allocating a fixed number of review slots. The experiment used synthetic outcomes and estimates policy behavior under specified assumptions.

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1. INTRODUCTION

A care team with 600 follow-up slots for 4,000 patients has an allocation problem even if its risk model is accurate on average. A risk score determines which patients reach the review queue and which remain outside it. If one group's inputs are noisier, more of its scores can appear in the extreme upper tail simply because of the extra spread. A systematic shift in the other direction can produce the opposite allocation. Neither mechanism requires the underlying clinical event process to change.

Chronic disease screening provides a concrete motivation. A chronic kidney disease study combined ResNet50 feature extraction with support vector classification and reported 99.21% recall after an 80:20 data split. That estimate describes the reported classifier evaluation, while the present study asks how many events a fixed-size queue would capture after inputs degrade [1].

A diabetes progression study explicitly considered feature reliance and ranking stability alongside one-year prediction. This motivates evaluating who enters the queue when an input becomes unreliable, because a small change in

aggregate predictive performance can coexist with a consequential change near the cutoff [2].

Survey-weighted cardiovascular analysis highlights a related distinction between describing population burden and prioritizing a patient for action. We preserve separate outcome and allocation denominators so the fraction of a group served can be interpreted alongside the fraction of its events captured [3].

The study asks three questions. How does unequal noise affect event capture and representation at fixed capacity? How does the answer change when the noisy measurement is also biased? What happens to capture and subgroup recall when a minimum allocation is imposed? The experiment isolates these questions by applying three policies to the same cohorts, outcomes, and capacity limits.

2. RELATED EVIDENCE AND DESIGN RATIONALE

2.1 *Separating the predictive target from its operational use*

Research on ischemic stroke biomarker discovery illustrates why an apparently informative feature may require multiple data streams to be available together. We compress that dependency into a single latent predictor so the measurement mechanism can be manipulated without also changing the outcome process [4].

Cancer care analytics links early detection to resource use. This informs our choice to report the yield of a fixed review budget, although the experiment does not estimate treatment effects or financial savings [5].

Plant-derived anticancer discovery offers an upstream example of prioritizing candidates for further investigation. Its evidence concerns biological discovery and serves here as a workflow analogy for ranking candidates before a costly second stage [6].

AI-assisted drug discovery provides another example in which a computational ranking can guide subsequent testing. The analogous endpoint in this paper is an event among reviewed candidates, which leaves the downstream value of any follow-up intervention as a separate empirical question [7].

Epidemiological surveillance infrastructure brings attention to the timeliness of data integration across feeds. This informs the interpretation of unequal measurement reliability as a scenario a distributed information system might need to audit [8].

Machine learning for epidemic prediction further illustrates why a forecast's time window must be defined. Our candidates share a common prediction time and outcome horizon, which keeps timing-related measurement differences separate from changes in the definition of the target [9].

2.2 *Capacity and allocation*

A screened cloud-workload simulation found that raising a tenant concurrency limit from four to eight reduced earliest-deadline-first misses from 23.97% to 1.41% in the reported high-arrival condition. We borrow its comparative simulation and capacity sensitivity design. Clinical review differs in that priority is based on event risk and the outcome is event capture [10].

Demand forecasting and supply chain resilience link noisy information to downstream capacity choices. This motivates treating the review budget as a parameter and comparing policies at several budget levels [11].

Supermarket sales forecasting provides another setting in which model choice and the operational decision are distinct. Here, the same candidate pool and review budget are held fixed across policies so allocation differences can be attributed to the ranking rule [12].

Studies of analytics in smart manufacturing motivate tracing the complete decision chain from recorded

inputs to allocation. In this experiment the raw score, quality adjustment, and allocation rule are separate, inspectable steps [13].

Customer experience analytics draws attention to the people who receive a service after a prediction is made. We therefore report group B's realized share of review slots in addition to yield [14].

Workforce retention analytics motivates distinguishing a predicted event from a preventable event. Our labels represent events in the simulated horizon and contain no counterfactual information about the effect of review [15].

Research on microcredit and socioeconomic outcomes motivates reporting who receives access to a scarce service. The simulated floor describes access to review and carries no claim about downstream benefits or demographic equity [16].

3. METHODS

3.1 *Study design and estimands*

We simulated 160 independent cohorts, each with 2,800 group A and 1,200 group B candidates. Group labels represent operational strata with potentially different measurement quality, such as two sources of patient records. They do not encode race, sex, or a clinical diagnosis. Each synthetic candidate has a latent signal Z drawn from a standard normal distribution. Conditional event probability is logistic ($\alpha g + Z$), with $\alpha A = -2.10$ and $\alpha B = -1.65$. The intercepts and group sizes are design choices, rather than parameters fitted to health records. Bernoulli events were generated by comparing each probability with an independently drawn uniform variate. Group membership is fixed within each cohort.

The observed measurement is $X = Z + \sigma g E + b g$, with E standard normal and independent of Z and the event draw. Group A has $\sigma A = 0.35$ and $bA = 0$ throughout. Table 1 defines group B's four conditions. Across scenarios within each cohort we reuse the same latent signals, error draws, and outcomes. The noise manipulation changes information available at ranking, while the event model remains constant. A late feed is one potential interpretation of such measurement error. The simulator does not model timestamp processes, intermittent outages, or a causal relationship between latency and risk.

Table 1. *Group B measurement scenarios*

Scenario	Error SD	Additive bias	Design purpose
Equal noise	0.35	0	Common reliability
Unequal noise	1.60	0	Isolate excess variance
Upward bias	1.60	+1.00	Stress high scores
Downward bias	1.60	−1.00	Stress low scores

Note. Group A remains at SD 0.35 and zero bias. Bias is expressed in latent-signal units.

3.2 *Ranking policies*

The raw policy ranks $\alpha g + X$ and allocates the K highest scores. The quality-aware policy assumes the observed measurement error is centered and that its group-specific variance is known. Under that working model, Z conditional on X has mean $X/(1 + \sigma g^2)$ and variance $\sigma g^2/(1 + \sigma g^2)$. We integrate logistic ($\alpha g + Z$) over this conditional distribution using 16-point Gauss–Hermite quadrature, then select the K largest values. The quality-aware policy uses the generating group intercepts and noise variances, an optimistic benchmark for those parameters. It receives no information about the true direction or size of measurement bias.

The floor policy first selects the highest-scoring group B candidates up to 30% of the K slots, then fills all remaining slots using quality-aware scores across

both groups, excluding candidates already selected. The floor is a prespecified policy choice tied to group B's cohort share. It is a lower bound and does not restrict group B from receiving additional slots. At each capacity the quota is an integer. All methods use the same K, and the simulation checks that each selected candidate is unique.

Explainable decision-support architecture motivates keeping score adjustment and allocation policy inspectable as distinct decisions. An operational audit could show the original score, the quality adjustment, and whether the floor changed a candidate's review status [17].

Data governance research supports making group membership, source reliability, and quota changes explicit inputs with assigned ownership. These elements would require versioned definitions in any implementation [18].

3.3 Evaluation and statistical summaries

The primary analysis uses 15% review capacity, or 600 slots per cohort. Sensitivity analysis uses 10% and 25%, corresponding to 400 and 1,000 slots. The primary measure is the number of selected event cases per 1,000 eligible candidates. We also calculate yield (events divided by selected candidates), group B allocation (selected group B candidates divided by all selected candidates), and event recall within each group (selected events in that group divided by all events in the group). The measures answer different questions even when based on the same queue. For example, a high group allocation can coexist with low yield if the extra selected cases have limited event risk.

We calculate differences within each seed and summarize the mean paired difference with a two-sided 95% t interval across 160 independent replications. These intervals quantify Monte Carlo precision conditional on the simulator. They do not incorporate uncertainty about a real population, transportability, or the clinical value of a review. The four scenarios and three capacities are treated as a structured sensitivity analysis. The intervals are descriptive and unadjusted for multiplicity. A review slot has a common cost in the experiment, so the ranking objective is expected event capture at fixed K.

4. RESULTS

4.1 Overall capture and representation at 15% capacity

The largest gain from variance adjustment occurred under upward bias. Raw ranking assigned 82.11% of slots to group B and captured 46.63 events per 1,000 candidates. Quality-aware ranking assigned 51.62% of slots to group B and captured 51.63 events per 1,000. The paired difference was +5.00 events per 1,000 (95% Monte Carlo interval [4.65, 5.35]). Because the cohort has 4,000 candidates, this corresponds to roughly 20 more events per simulated queue.

Under unequal but centered noise, raw ranking assigned 59.92% of slots to group B. The quality adjustment reduced that share to 37.75% and improved capture from 50.83 to 52.25 events per 1,000. The paired difference was +1.42 [1.14, 1.70]. Under equal noise the capture difference was below 0.01 events per 1,000, with an interval of [−0.09, 0.08], even though the mean allocation changed by about two percentage points.

Table 2. *Event capture and allocation at 15% review capacity*

Scenario	Ranking rule	Events per 1,000	B slots (%)	B recall (%)	A recall (%)	Yield (%)
Equal noise	Raw ranking	58.70	44.91	45.60	30.90	39.13
	Quality-aware	58.70	46.89	46.94	30.09	39.13

Scenario	Ranking rule	Events per 1,000	B slots (%)	B recall (%)	A recall (%)	Yield (%)
	Quality-aware + floor	58.70	46.89	46.94	30.09	39.13
Unequal noise	Raw ranking	50.83	59.92	43.42	24.42	33.89
	Quality-aware	52.25	37.75	30.01	33.81	34.84
	Quality-aware + floor	52.25	37.75	30.01	33.81	34.84
Upward bias	Raw ranking	46.63	82.11	55.73	12.92	31.09
	Quality-aware	51.63	51.62	38.45	28.17	34.42
	Quality-aware + floor	51.63	51.62	38.45	28.17	34.42
Downward bias	Raw ranking	52.25	36.66	29.29	34.24	34.84
	Quality-aware	51.71	24.37	20.85	38.73	34.48
	Quality-aware + floor	52.04	30.00	24.84	36.69	34.69

Note. Values are arithmetic means across 160 cohorts. Group recall uses all events in that group as its denominator.

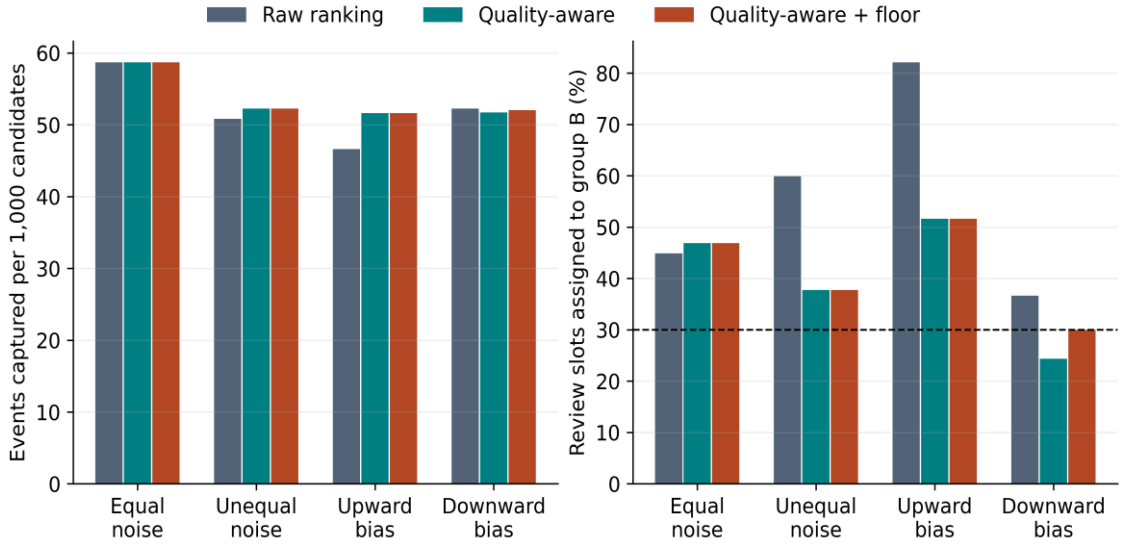


Figure 1. Event capture and group B allocation across measurement scenarios

Note. Bars show means at 15% capacity. The dashed line marks the 30% minimum group B allocation.

4.2 *The floor under downward bias*

When group B's measurements were shifted downward, the centered-error adjustment reduced its mean slot share from 36.66% under raw ranking to 24.37%. Overall capture fell by 0.54 events per 1,000 [−0.75, −0.34]. Applying the floor raised group B's share to 30.00% and recovered 0.32 events per 1,000 [0.18, 0.47] relative to quality-aware ranking. Group B recall rose by 3.99 percentage points, from 20.85% to 24.84%. Group A recall fell from 38.73% to 36.69%, reflecting the reallocation of a fixed review budget.

In the other three conditions the floor left the mean capture unchanged at the displayed precision. The floor's effect in the downward-bias condition was specific to a misspecified score. By moving some reviews toward group B, the constraint counteracted part of the ranking error. The raw policy still had the highest average capture and group B recall in that scenario at 15% capacity.

4.3 Sensitivity to capacity

Figure 2 shows how average capture changed as the review budget expanded. The relative separation between policies was most visible under upward bias. Each policy used the same number of slots at a given budget, making the gaps interpretable as changes in the composition of the queue. At larger capacities the same numerical floor can affect a different portion of the ranking because the marginal candidate has a different risk distribution.

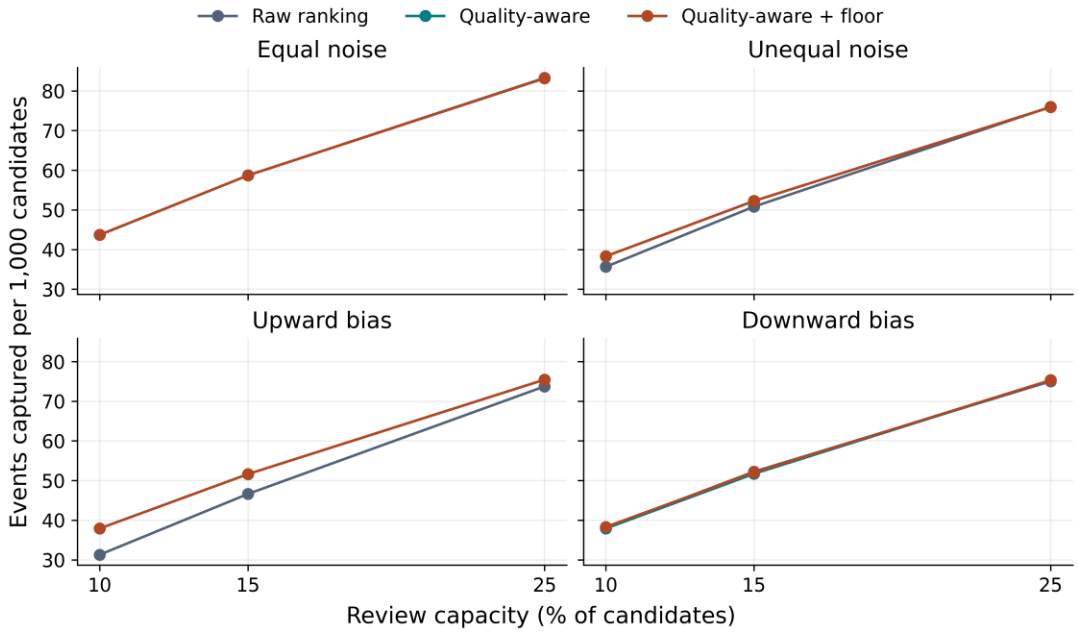


Figure 2. Event capture at three review capacities

Note. Points show cohort means. Overlapping lines indicate that policies produced equal or nearly equal capture.

5. DISCUSSION

5.1 What the experiment establishes

With a fixed review budget, the tail of the score distribution matters. Under unequal centered noise, the raw policy put many group B candidates into the queue because their scores were more dispersed. Incorporating measurement variance reduced that effect, improved aggregate capture, and lowered recall for group B. The difference between group allocation and group recall is operationally important. A policy can find more events overall while finding fewer in a specific group, even when that group receives a share of slots above its cohort share.

Downward bias changed the direction of the aggregate result. The adjustment discounted noisy observations while leaving the source's systematic bias unmodeled. Under that condition, the floor improved total capture relative to the adjusted policy, although it displaced some group A events. In a deployment, evidence of directional error would justify estimating the bias directly, then reassessing whether a floor advances the agreed objective. The simulation compares a centered-error policy with a floor. It cannot identify the best policy among bias correction, recalibration, manual review, or other possible responses.

5.2 What an information system needs to expose

An implementation would need to record when a feed was refreshed, which model version produced the score, and what rule governed admission to the queue. Resilience-oriented AI and MIS research motivates treating these records as operational inputs rather than as retrospective documentation [19].

Cloud project management offers a practical reason to keep data availability separate from policy execution. Upstream jobs can finish at different times, even if the review queue runs once on a fixed schedule [20].

Work on agile MIS suggests a practical evaluation cycle in which changes to scoring and allocation are introduced separately and checked against a frozen baseline [21].

Research on AI-enabled project management informs how exceptions could be assigned to a named reviewer when a source changes or a queue unexpectedly loses coverage [22].

Business energy analytics illustrates why throughput and operating resources are separate dimensions of system performance. Our equal-cost slots abstract from staffing and computing expense [23].

Geospatial analytics using multiple sources motivates testing the reliability of inputs at the level at which they are produced. A shared indicator of feed quality can obscure source-specific measurement differences [24].

5.3 Defensive controls and governance implications

The studies in this section inform operational safeguards. Their methods and findings do not provide numerical inputs for the clinical simulation.

Cloud data protection research motivates checking whether a stale or incomplete feed is authorized for scoring before the review service consumes it [25].

Research on cloud-integrated MIS security similarly informs the separation between the authority to supply data and the authority to change a ranking policy [26].

Policy-bound encryption and access provenance motivate associating a decision record with the identity of the service that accessed the underlying input [27].

Work on decentralized data integrity provides a rationale for keeping verifiable histories of policy versions and overrides. A version history could record who changed a quota and when it took effect [28].

Federated threat intelligence provides an example of institutions sharing updates across organizational boundaries. A multisite study could evaluate site-specific error distributions without treating a pooled estimate as valid for every site [29].

Homomorphic federated models motivate evaluating whether cross-site summaries can be produced under stronger privacy constraints. The statistical behavior of a review rule and the privacy properties of its implementation are separate validation questions [30].

Energy-aware data center research highlights the cost of monitoring and logging at scale. The cadence of quality checks should be evaluated with the timeliness and computing cost of updating the queue [31].

Studies that place automated threat detection inside an MIS motivate a distinction between an alert and the evidence a reviewer can inspect. A clinical worklist likewise benefits from retaining the reasons a candidate was flagged for review [32].

Research on data system security informs the designation of human responsibility for verifying suspect inputs before automated scores influence service allocation [33].

Cloud-based cognitive cyber defense emphasizes the integration of telemetry and decision workflows. The same design principle supports monitoring an input-quality signal in the worklist system that uses it [34].

Work on organizational threat intelligence connects detection to governance. The corresponding requirement in a care worklist is a named owner for evaluating abrupt changes in who is selected [35].

Big-data threat detection provides an operational analogue for distinguishing a stream of observations from the reviewable cases it produces. Queue evaluation therefore needs both feed-quality indicators and case-level outcomes [36].

Research on real-time threat analytics highlights why aggregate alerts should be monitored when inputs arrive at different rates. In this study, monitoring the distribution of selected cases is the analogous defensive check [37].

Comparisons of sequence architectures for threat detection underscore that changing a predictive model can change the alert distribution. Allocation policies should therefore be re-evaluated after a scoring model is updated [38].

Continual learning for defensive insider-threat detection motivates evaluating changes over time. A useful future stress test would permit reliability to drift gradually or change after an upstream software release [39].

Research on sensor security in smart buildings illustrates the dependence of a monitoring system on data generated at the edge. For patient worklists, quality annotations should be traceable to the originating system [40].

Work on biometric verification robustness motivates testing a decision rule under degraded measurements before deployment. Our stress test uses ordinary synthetic measurement noise and directional shifts [41].

Research on emerging defensive strategies supports periodic reassessment when the operating environment changes. A worklist can be subjected to the same discipline using retrospective slices with different source reliability [42].

Research on cybersecurity training motivates recording reviewer actions and evaluating whether staff understand when to override a score or report a suspect input [43].

Studies of AI in IT security project management motivate incorporating rule changes and review outcomes into the same accountability process used for other technology risks [44].

Defensive self-healing research provides a reason to evaluate automated recovery separately from decision quality. Restoring a data service does not establish that the refreshed measurements restore the original ranking distribution [45].

Research on infrastructure protection frames service continuity as a requirement that extends beyond model detection accuracy. The corresponding concern here is the ability to keep a review queue operational while a data source is being checked [46].

Work on supply chain integrity foregrounds the downstream costs of interrupted information. In a clinical worklist those costs would include delayed reviews and additional staff effort, neither of which is measured by the event count alone [47].

Cross-sector MIS research connects infrastructure reliability with the institutions that use its outputs. A health system evaluating a shared feed would therefore need measures that reflect both local operating conditions and common service expectations [48].

National threat intelligence frameworks motivate escalation across organizations that share infrastructure. The transferable operational question is who communicates a feed-quality change when multiple sites use the same upstream source [49].

5.4 Limitations and clinical evaluation

The simulation uses one latent predictor, two fixed groups, Gaussian error, one event horizon, and equal review cost. Real measurements can be missing, misclassified, correlated, or selectively observed after care. Reliability estimates may themselves be uncertain or delayed. The method's access to the generating intercepts and

variances should be interpreted as a favorable condition for estimating the effects of the ranking rule. Directional bias remains unknown to the policy, and the simulated outcomes cannot estimate clinical effectiveness, equity, or the benefit of intervention.

A prospective evaluation would timestamp predictor availability, define one follow-up action and horizon, and hold out future records for testing. It should separately assess calibration, capture at fixed review budgets, subgroup recall, and actual review completion. Site-level uncertainty should propagate into the reliability adjustment. The evaluation should also account for missing outcome ascertainment, especially where receipt of review changes the probability that an event is documented. A silent prospective phase could establish whether feed quality and queue composition change together before the policy affects care. Decision curve analysis provides a framework for introducing the relative consequences of true and false positives when a review can lead to downstream action [50].

The bibliography was supplied by the study requester. It spans several domains and is used for study rationale and design analogies. Selected publisher abstracts were checked for the clinical screening and cloud scheduling findings discussed above. Other entries are cited for the topics evident in the supplied records, and their bibliographic metadata and empirical claims were not independently verified. No published numerical performance estimates were used to parameterize the experiment.

6. CONCLUSION

At a 15% review capacity, modeling unequal measurement variance improved average event capture when the noisy measurement was unbiased or biased upward but reduced it when the measurement was biased downward. A minimum allocation partly recovered capture under downward bias and increased recall in the affected group. These effects depend on the score error, the capacity limit, and the metric used to judge the queue. A clinical evaluation should measure all three before using a quality adjustment or allocation floor in patient follow-up.

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