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Recorded ASA physical status and major postoperative complications after colorectal cancer surgery in Jeddah: a secondary cohort analysis with a source-data audit

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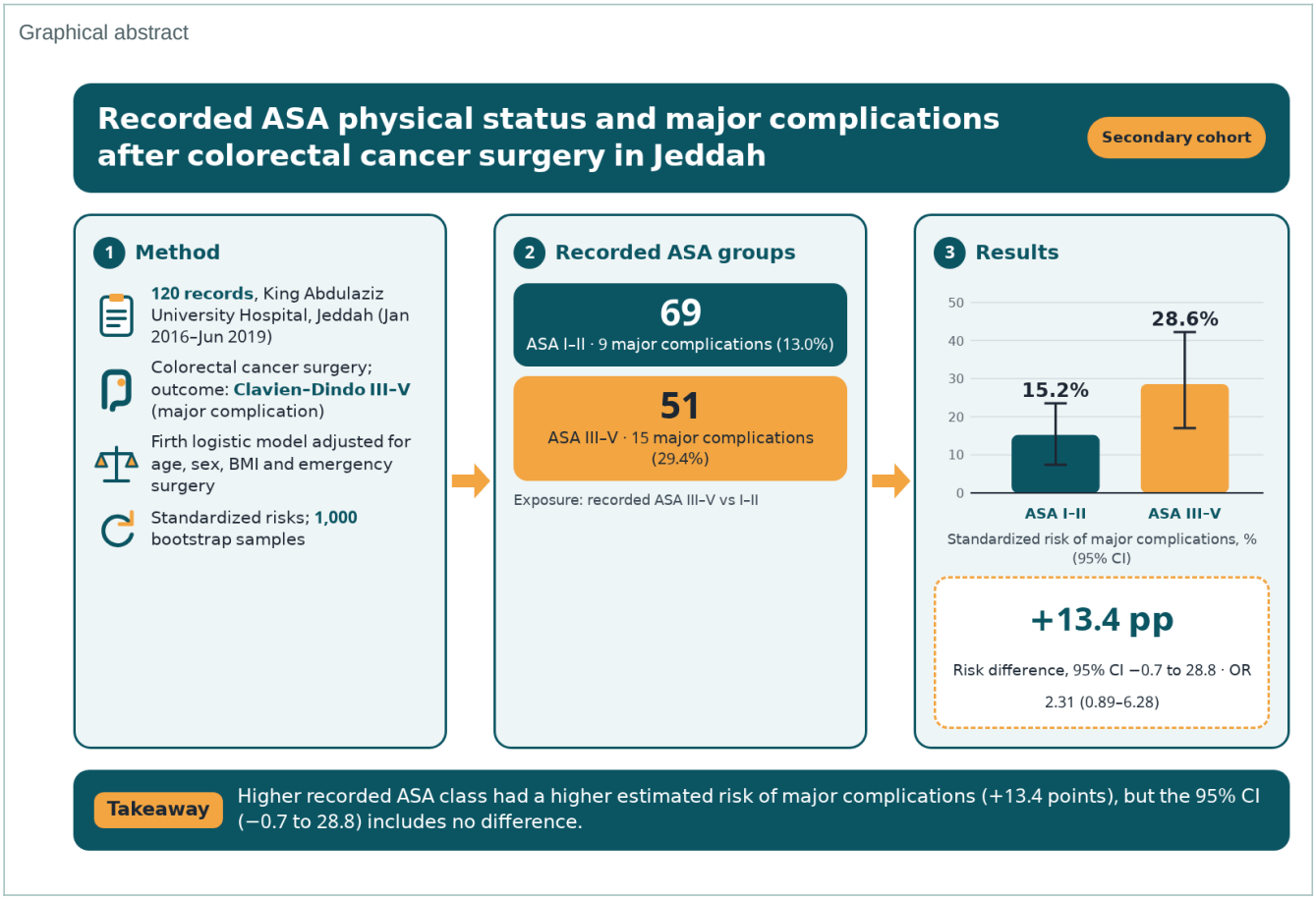
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Abstract

Graphical abstract



Background: ASA physical status is widely recorded around surgery, but its association with the severity of postoperative morbidity depends on case mix and outcome ascertainment. We estimated adjusted absolute risks in an open Saudi colorectal surgery dataset while documenting source limitations and potentially overlapping prior evidence.

Methods: We reanalyzed 120 records from King Abdulaziz University Hospital, Jeddah, covering January 2016–June 2019. A frozen plan compared ASA III–V with I–II for recorded Clavien–Dindo III–V complications. Firth logistic regression adjusted for age, sex, body mass index and emergency surgery. The primary measure was a marginal standardized risk difference with a 1,000-sample bootstrap interval. Prespecified analyses added operative approach, excluded ASA IV–V, examined any complication and used propensity overlap weighting.

Results: There were 69 ASA-I–II and 51 ASA-III–V records, with 9 and 15 major complications, respectively. Standardized risks were 15.2% and 28.6%; the difference was 13.4 percentage points (95% CI, -0.7 to 28.8). The conditional odds ratio was 2.31 (95% profile CI, 0.89–6.28). Excluding the 3 ASA-IV–V records gave a difference of 9.7 percentage points (95% CI, -4.0 to 22.8). Overlap weighting gave 13.4 percentage points (95% CI, -1.4 to 28.0). Four source records had grade 0 despite a named complication indicator. The assessor, precise ASA assessment time and uniform active 30-day grade ascertainment were undocumented.

Conclusions: The point estimate suggested greater major morbidity in higher recorded ASA classes, but uncertainty included no difference. Source coding, selection and possible patient overlap with a later same-center publication restrict interpretation. These results are an auditable secondary severity analysis, not an independently validated risk tool or a causal effect of ASA class.

Keywords: ASA physical status; Clavien–Dindo; colorectal surgery; postoperative complications; Saudi Arabia; perioperative assessment; secondary data analysis

Introduction

The American Society of Anesthesiologists physical-status classification is a familiar component of perioperative documentation. Its evolution reflects the need for a concise description of a patient's physical condition, but the class is not itself a complete procedure-specific risk model [1]. Studies have examined its relationship with postoperative outcomes, while investigations of classification by different clinicians illustrate that assigning a class also involves judgment [2,3]. Historical reviews emphasize the distinction between physical status and the broader collection of patient, procedural and institutional factors that influence surgical outcomes [4]. For a secondary analysis, the meaning of a recorded class therefore depends on how it was assigned, when it was recorded and which outcomes are available.

Complication severity introduces a second measurement problem. An endpoint that counts every postoperative deviation equally can combine events with very different consequences. The Clavien–Dindo framework classifies complications according to their management and severity, with death represented in its highest category [5,6]. Its use makes it possible to distinguish major morbidity from an undifferentiated count of complications, provided the grades have been assigned consistently. Colorectal studies have applied graded complication assessment to characterize outcomes and examine associated factors [7,8]. Such work also shows why a study should specify whether it evaluates any complication, major complications, particular clinical events or mortality. These endpoints should not be interchanged after observing which association appears strongest.

Colorectal operations provide a clinically relevant setting for evaluating these distinctions because patient characteristics, urgency and the operative procedure can all contribute to postoperative outcomes [9,10]. Saudi evidence already includes work at King Abdulaziz University Hospital in Jeddah linking ASA class with postoperative complications. Alzahrani and colleagues studied 195 patients operated on between January 2017 and August 2022, presenting unadjusted comparisons for both intraoperative and any postoperative complications [11]. A separate, earlier publication from the same hospital compared laparoscopic and open approaches in 120 colorectal cancer operations from January 2016 through June 2019 [12]. Its individual-level data and coding document were deposited openly [13]. The recruitment windows overlap, so these reports cannot be assumed to represent independent populations.

We used the earlier open dataset to examine a narrower severity question: the adjusted association between recorded ASA III–V, compared with I–II, and major postoperative complications. The contribution is an explicit estimand, reproducible coding, standardized absolute risks and an audit of limitations in the source fields. It is not the first evaluation of ASA and postoperative complications in Saudi Arabia or at this hospital. We also compared whole-cohort standardization with a propensity overlap estimand and examined whether the small number of ASA-IV–V records materially affected the result. Because the release has limited clinical detail and no uniform complication-assessment timestamp, we framed the analysis as an exploratory association study rather than development of a clinical prediction tool.

Methods

Design, source and prior-publication context

This retrospective secondary cohort analysis used the unmodified workbook deposited at Zenodo under DOI 10.5281/zenodo.4730346 and the accompanying variable-coding document [13]. The source describes adult patients undergoing colorectal cancer surgery at King Abdulaziz University Hospital, a tertiary center in Jeddah, Saudi Arabia, from January 2016 through June 2019 [12]. Of 134 operations considered in the source study, 14 conversions from laparoscopy to open surgery were excluded before the released cohort of 120 was formed. The source also describes exclusions for disseminated cancer, concurrent procedures and peritonitis/sepsis. We could not recover or characterize the unavailable excluded records. The analysis therefore concerns the selected release rather than all colorectal operations at the institution.

The original article's displayed status was version 1, with one review approved with reservations and one not approved [12]. Its open reviews raised issues including heterogeneous procedures, emergency operations, interpretation of outcome definitions and inclusion of an ASA-V patient. We treated those reports as relevant source context rather than assuming that public availability certified the clinical data. The present analysis preserves the coded records, reports source discrepancies and includes a planned restriction excluding ASA IV–V. It does not claim to resolve the source investigation's broader clinical questions or to have independently adjudicated its measurements.

We reviewed the later same-center publication before finalizing the manuscript. It included January 2017–August 2022 cases, explicitly compared ASA class with any postoperative complications and did not report a Clavien–Dindo severity endpoint or adjusted regression for that relationship [11]. Our source window overlaps it from January 2017 through June 2019. Patient-level linkage between the publications was not available, and neither complete identity nor complete independence can be established. The present major-grade analysis is therefore described as a secondary severity reanalysis with possible overlapping patients. Its secondary any-complication result is contextual and is not offered as a new discovery or an independent replication of the later report.

Analysis plan, eligibility and data reconstruction

The statistical analysis plan was frozen after review of the source publication, codebook, marginal category counts and missingness, but before ASA–outcome cross-tabulations or fitted associations. The local timestamp and file hash are retained. No prospective trial or study registration is claimed. STROBE and applicable RECORD items guided reporting [14,15]. The executable code, original source files, exact analysis matrix, bootstrap draws, model diagnostics and locked results are retained in the accompanying package. All primary and sensitivity models were specified before analysis, and no model was selected for the headline result according to its confidence interval.

Eligibility required an adult record with ASA code 1–5, complication-grade code 0–5, sex code 1 or 2, elective or emergency classification, and valid age and body mass index. All 120 released records met these criteria. The original worksheet row number was retained as a reproducibility identifier. It is not a clinical patient identifier, and it cannot establish whether the same person appears in a different publication. The first worksheet column contains data-entry timestamps; these were not used as dates of surgery, complication onset or discharge. Blank metadata columns were excluded from clinical analysis without modifying the original workbook.

The analysis relies on the source's description of one patient-level surgical record per row. We found no identical full clinical rows after excluding the entry timestamp and blank columns, but that check cannot validate longitudinal uniqueness. No deduplication based on approximate age, sex or body size was attempted. Such a procedure could incorrectly merge different people and would not resolve the absence of a linkage identifier. Clinical variables were parsed according to the supplied dictionary, with explicit source-to-analysis mappings saved for review. No patient characteristic, treatment or event time was inferred from a blank cell.

Exposure and outcome definitions

The primary exposure was recorded ASA III–V versus I–II, a prespecified grouping of the source codes. Individual classes were retained for descriptive reporting. We use the term recorded ASA physical status because the source groups ASA among perioperative or intraoperative variables and does not identify the assessor or exact assessment time [12]. The analysis does not assume that an independent anesthesiologist prospectively assigned every value at a standardized preoperative visit. The variable is understood as the physical-status assessment associated with the surgical episode, but the precision of its timing cannot be reconstructed from the release.

The primary outcome was a recorded Clavien–Dindo grade of III, IV or V. Codes 0, I and II formed the comparison outcome category. The codebook explicitly maps the terminal code 5 to grade V, whereas the source article's corresponding table labels its last category VI [12,13]. We retained the codebook mapping and disclosed the discrepancy. The two grade-V records were counted as major events, not censored or removed. The analysis did not independently regrade a complication from its treatment description because the details needed for adjudication were unavailable. Grading according to management is an important feature of the classification, but the presence of a numeric code alone does not validate its application [5,6].

The prespecified secondary outcome was any recorded complication, defined by grade I–V versus grade 0. It was analyzed within the same study and did not replace the primary endpoint. We audited whether the grade agreed with the available named-complication indicators. Four records with grade 0 had at least one named postoperative complication. Their grades were retained rather than silently corrected. Positive grades without one of the listed complication types were also counted, but these were not automatically considered errors because the named fields may be incomplete. The study estimates associations with the supplied grade, whose clinical accuracy remains an unresolved source limitation.

The outcome is recorded postoperative morbidity associated with the surgical episode. The release does not establish that the grade was assessed through uniform active follow-up to postoperative day 30. A separate field records 30-day readmission, which is not evidence that every other complication was sought over the same period. We therefore did not label the endpoint a validated 30-day complication rate. Exact complication and death dates were absent, precluding a time-to-event analysis. Postoperative duration fields containing code 999 were treated as unavailable information, not literal 999-day intervals. They were not used to fabricate follow-up times or to assign event order.

Primary adjustment and estimation

The primary Firth logistic model included six coefficients: an intercept, higher-ASA indicator, age, sex, body mass index and emergency surgery. Age was centered at 60 years and expressed per 10-year increase; body mass index was centered at 25 kg/m² and expressed per 5-unit increase. Source sex code 2 defined the female indicator, and surgery-timing code 2 defined emergency surgery. These covariates were chosen

as available patient and urgency descriptors. The model was deliberately limited because only 24 major events were present in the marginal source count. No stepwise selection, outcome-driven cut point or interaction search was performed.

Individual comorbidities were not added indiscriminately to the primary model because they contribute to the clinical construct summarized by ASA and would change the interpretation of the contrast. Postoperative analgesia, mobilization, blood loss, transfusion, length of stay and subsequent complications were not primary adjustment variables. Actual operative approach was reserved for a sensitivity analysis because it reflects clinical and procedural selection and was the focus of the original publication. The resulting adjustment does not account for every relevant aspect of disease severity, cancer stage, functional status, procedure complexity or clinician judgment. The term adjusted association refers only to the covariates specified here.

Firth estimation maximized the binomial log likelihood plus one half of the log determinant of its information matrix. The penalty addresses coefficient bias and instability in small samples and can yield finite estimates when ordinary logistic estimates are affected by separation [16]. It does not supply information about missing confounders or validate the underlying coding. We reported the conditional odds ratio for higher ASA with a 95% profile penalized-likelihood interval. We did not interpret the odds ratio as a risk ratio, particularly because major complications were not extremely rare in this cohort.

The primary estimand was the marginal standardized risk difference, higher minus lower ASA. For each record, we calculated two fitted probabilities after setting its ASA group first to I–II and then to III–V, while retaining its other recorded covariates. Averaging each set of probabilities gave the two standardized risks. Their difference was expressed in percentage points. This method uses the cohort's empirical covariate distribution rather than predicting for a hypothetical person with mean covariates [17]. The calculations provide a model-based comparison of recorded risk groups. They do not imply that changing an ASA label would change an individual's outcome or that all determinants of physical status are exchangeable between groups.

A patient bootstrap repeated fitting and standardization in 1,000 samples for the primary model. The 2.5th and 97.5th percentiles defined the reported interval. Standardization was repeated over each bootstrap sample, allowing variation in both fitted coefficients and the sampled covariate distribution. We recorded every successful and failed fit and retained a deterministic seed schedule. Numerical checks included design rank, the maximum absolute adjusted score, information-matrix eigenvalues, fitted probability ranges and leverage. These checks address the calculation, not clinical model validation. We did not develop a points score, select a risk threshold or estimate deployment performance.

Prespecified sensitivity and overlap analyses

Four additional Firth analyses were specified: an unadjusted major-complication model, addition of open versus laparoscopic approach to the primary model, restriction to ASA I–III, and the primary covariate model applied to any complication. Each used 400 bootstrap samples. The ASA-I–III restriction excluded the three ASA-IV/V records and compared class III with I–II. It addresses the contribution of sparse extreme classes without asserting that those records were miscoded. The operative-approach analysis estimates a further conditional association; a change in its coefficient does not identify mediation or prove which covariate set is causally correct.

We also estimated a descriptive overlap-weighted major-complication contrast. An ordinary logistic model estimated the probability of higher ASA from age, sex, body mass index and emergency surgery. Higher-ASA records received weights of one minus this probability, while lower-ASA records received the probability itself. These weights emphasize patients whose measured characteristics provide greater representation in both groups [18]. We calculated weighted outcome risks and their difference, effective sample sizes, propensity ranges and absolute standardized mean differences before and after weighting. One thousand bootstrap samples repeated propensity estimation and weighted risk calculation. Nonconvergence or separation was prespecified as a reportable failure rather than a reason to silently substitute another estimator.

The overlap population differs from the whole-cohort population used for primary standardization. Agreement between their estimates is informative about this particular change in weighting, but not proof of general robustness. Propensity-model misspecification and limited overlap can affect interpretation even when estimated weights are numerically stable [19]. Balance was assessed only for the measured covariates in the model, using the unweighted pooled standard deviation as the reference scale. We did not equate near-zero mean differences with balance in every unmeasured clinical characteristic. Because ASA is a composite risk marker rather than a randomized intervention, the overlap result remained descriptive.

All primary fields were complete, so no imputation was required for the main model. Unknown smoking and unavailable postoperative durations were not converted into complete measurements. Missing-data methods require a defensible account of what is missing and why [20]; the availability of an imputation algorithm would not justify inventing complication dates or a follow-up interval. The analysis also did not treat complete fields as proof of complete clinical ascertainment. A field can contain a valid numeric code while still lacking sufficient documentation to establish its clinical meaning, as illustrated by the source grading discrepancies.

Results

Cohort characteristics and source audit

All 120 released records entered the primary analysis. There were 69 ASA-I–II records and 51 ASA-III–V records. Individual classes comprised 6 class I, 63 class II, 48 class III, 2 class IV and 1 class V records. The higher-ASA group was older, with mean age 66.5 versus 57.6 years, and had more emergency operations: 16/51 (31.4%) versus 11/69 (15.9%). Mean body mass index was 27.5 versus 26.9 kg/m². Both nationality categories and both recorded operative approaches were represented in each ASA group (Table 1). These baseline differences motivate adjustment but do not exhaust the clinical differences between groups.

The recorded Clavien–Dindo distribution was 72 grade 0, 8 grade I, 16 grade II, 9 grade III, 13 grade IV and 2 grade V. Major complications therefore occurred in 24 records and any complication in 48 (Table 2 and Figure 1). The four grade-0 records with a named complication indicator remained classified according to the original grade. Twelve positive-grade records did not have one of the listed named-complication indicators, which could reflect events outside that list. These audit findings were recorded without retrospectively rewriting the endpoint. One regular-diet duration, 14 mobilization durations and one length-of-stay value used the unavailable-information code 999; none was used as a modeled event time.

Primary adjusted association

Major complications were recorded in 9/69 lower-ASA records (13.0%) and 15/51 higher-ASA records (29.4%). The prespecified model estimated standardized risks of 15.2% (95% CI, 7.4–23.5) and 28.6% (95% CI, 17.0–42.2), respectively. The primary risk difference was 13.4 percentage points (95% CI, -0.7 to 28.8). The conditional odds ratio was 2.31 (95% profile penalized-likelihood CI, 0.89–6.28). The positive point estimate is compatible with greater major morbidity in the higher recorded classes, but the interval also includes no difference and is too wide to support a precise risk increment.

The primary matrix had full rank for all 6 coefficients. The maximum absolute adjusted score was 3.5e-09, the minimum information eigenvalue was 2.674, and maximum leverage was 0.138. All 1000 planned primary bootstrap samples produced retained fits. These diagnostics support successful numerical estimation. They do not resolve the source's clinical measurement limitations or establish that the selected covariates fully account for differences between ASA groups. The dataset and sample size support an exploratory estimate with uncertainty, rather than a validated prediction claim.

Sensitivity analyses and overlap comparison

The unadjusted major-complication difference was 16.2 percentage points (95% CI, 1.6 to 32.1). Adding operative approach to the primary model gave 12.7 percentage points (95% CI, -3.7 to 27.5). Restriction to ASA I–III left 117 records and 21 major events, with a difference of 9.7 percentage points (95% CI, -4.0 to 22.8). Thus the three ASA-IV/V records all contributed major events, and excluding them reduced the point estimate without producing a precise association. The prespecified any-complication analysis gave 7.2 percentage points (95% CI, -11.3 to 23.3). All four analyses retained their 400 requested bootstrap samples (Table 3).

Propensity overlap weighting estimated risks of 13.2% in the lower-ASA group and 26.5% in the higher-ASA group, for a difference of 13.4 percentage points (95% CI, -1.4 to 28.0). Effective sample sizes were 58.5 and 45.4, respectively. The overall fitted propensity range was 0.060–0.816. Before weighting, absolute standardized mean differences were greatest for age and emergency surgery; after weighting, all four modeled covariate mean differences were below 0.001 (Table 4 and Figure 3). All 1000 overlap bootstrap samples succeeded. The close point estimates under overlap weighting and whole-cohort standardization do not change the wide uncertainty or establish balance in unrecorded clinical factors.

The primary and sensitivity estimates for major morbidity are displayed together in Figure 2. The unadjusted interval excluded zero, whereas the primary adjusted interval did not. This difference was not used to replace the prespecified primary analysis. The estimates share the same underlying records and should not be counted as separate positive or negative studies. Independent read-back of all six bootstrap files reproduced their recorded risk-difference identities and interval percentiles. Independent reconstruction from the raw workbook and a derivative-free Firth implementation reproduced the primary and four additional Firth models, with a maximum coefficient difference below 5×10^{-8}. The primary standardized difference and profile interval also agreed to numerical precision. These checks verify the specified computation and do not adjudicate ASA or complication grades.

Discussion

This secondary analysis found a positive but imprecise adjusted association between recorded ASA III–V and major postoperative complications in a selected Jeddah colorectal cancer surgery cohort. The

standardized point estimate was an increase of approximately 13.4 percentage points, but the interval included no difference and extended to a substantially larger increase. Removing the three ASA-IV/V records reduced the estimate, while adding operative approach and using an overlap population produced broadly similar point estimates with wide intervals. The principal inference is therefore uncertainty around a potentially important association, not confirmation of a specific risk multiplier or proof that ASA alone can guide a clinical decision.

The result concerns complication severity and adds a different level of description to earlier work from the same hospital. The original open-data publication compared operative approaches and several postoperative outcomes [12]. The later 195-patient publication explicitly examined ASA and any postoperative complications [11]. Our major-grade analysis does not erase that prior evidence. Its potentially overlapping recruitment period means it cannot be treated as an independent Saudi replication. A future synthesis would need to investigate patient overlap before combining estimates; counting the publications as independent samples could exaggerate precision. The contribution here is a transparent secondary estimate and a reproducible audit, with the overlap acknowledged rather than inferred away.

The distinction between any and major complications was consequential. The higher-ASA group had 24 recorded complications of any grade, the same absolute number as the lower-ASA group, despite fewer patients. The composition of those events differed, and the estimated standardized difference for any complication was smaller than for major complications. This does not establish a biological mechanism or prove that physical status specifically determines severity. It shows that the endpoint definition changes the numerical question. Grading systems can make these distinctions visible, but their usefulness depends on consistent application and adequate recording [5,6]. Retaining the prespecified primary outcome prevents the analysis from choosing whichever endpoint appears most favorable afterward.

ASA is also a summary of clinical state rather than a manipulable exposure. Age, comorbidity, functional limitations and the clinician's overall assessment may influence the recorded class, while some of the same features influence postoperative outcomes. Adjustment for age, sex, body mass index and urgency narrows the comparison but does not produce a contrast between otherwise identical patients. Research on ASA classification and morbidity supports examining its prognostic information, yet does not turn a retrospective class label into a treatment assignment [1–4]. The present standardized risks are best understood as adjusted descriptions of the released cohort. They cannot be used to claim that lowering the label itself would prevent a complication.

The sparse extreme classes illustrate why preserving category detail matters. Only two records were ASA IV and one was ASA V, and all three had major events. A combined III–V group conceals that small but influential component unless individual grades and a planned restriction are shown. The source's open review had already questioned the ASA-V inclusion [12]. We did not answer that clinical question by deleting the record after seeing its outcome, nor did we assume it was correct simply because it was present in a public file. The prespecified I–III sensitivity addresses its influence on the numerical estimate. Determining whether the class was assigned appropriately would require source documentation beyond this release.

The complication-grade audit raises a separate concern. A grade of 0 in a record with a named postoperative complication is inconsistent at face value, but the available fields do not identify whether the grade, event indicator, timing or abstraction process is responsible. Replacing such codes with an analyst-assigned grade would introduce unsupported clinical judgments. We therefore retained the supplied

endpoint and reported the contradictions. Positive grades without a named event may have a less problematic explanation because the list of event types is not necessarily exhaustive. These distinctions are important: an audit should expose a specific inconsistency without claiming that every unusual combination proves an error. The major-grade association remains contingent on clinical verification of the source classification.

Follow-up also restricts the meaning of the reported risks. A postoperative grade recorded during a surgical episode does not automatically represent complete ascertainment through a uniform date. Patients discharged earlier may have complications managed elsewhere or at a later visit, and the release does not document an active surveillance protocol for the grade. Conversely, a readmission field labeled within 30 days cannot supply the onset time of every other complication. Our use of a recorded episode endpoint avoids assigning a precision to the follow-up that the data do not support. Future prospective work should specify both the assessment horizon and the mechanism for identifying events after discharge, including complications treated at other institutions.

Selection into the released cohort is another major limitation. The exclusion of laparoscopic conversions removes cases whose operative course may differ from completed laparoscopic or planned open procedures. Their absence cannot be repaired by adjusting for the approach remaining in the dataset. The source also excludes several clinical presentations and combines a variety of colorectal operations. Findings from these selected records cannot be generalized directly to all colorectal cancer resections, all emergency surgery or a national Saudi surgical population. The observed nationality distribution further emphasizes that a Saudi treatment setting includes both Saudi and non-Saudi patients. It is a geographic institutional cohort, not a representative national sample.

The overlap analysis helps explain one aspect of case-mix adjustment. Age and urgency were more imbalanced than sex or body mass index before weighting. The selected propensity model achieved numerical balance in those four covariate means and retained substantial effective sample sizes. That is a property of this fitted comparison, not evidence that all causes of morbidity were balanced. Differences in cancer stage, operative extent, physiological reserve, functional status, nutrition, institutional decisions and unrecorded comorbidity could remain. Methodological work on overlap weights emphasizes the changed target population and the implications of limited overlap or model specification [18,19]. We therefore present this estimator as a companion description rather than an independent validation of the primary model.

The relatively small number of major events also limits what can be learned from adjustment. Firth estimation permitted a stable six-coefficient model and a profile interval without relying on large-sample normality of the exposure coefficient alone [16]. Nonetheless, it did not make the information content equivalent to a larger cohort. The primary interval remains wide, and the apparent precision of a single point estimate should not overshadow it. The secondary any-complication analysis has more events but asks a different question. Expanding the model until every available field is included would introduce additional instability and could adjust for variables that follow surgery. A larger study with clinically selected baseline variables would be preferable to a more elaborate regression on the same small release.

Several clinical variables that would help interpret the association were unavailable or unsuitable for the present model. The release does not establish a uniform ASA assessor, assessment time, functional-status measure or comprehensive cancer staging variable for this analysis. Differences among operative procedures may matter beyond the broad open-versus-laparoscopic distinction. Postoperative analgesia

and mobilization are recorded, but their timing and relationship to evolving complications make them unsuitable as baseline confounders. The source also includes unavailable-duration sentinels that should not be treated as follow-up data. These are restrictions on the question the dataset can answer, not reasons to invent measurements or assume that a complete spreadsheet is a complete clinical record.

The reproducibility package provides a useful but limited form of assurance. It preserves original bytes and coding, a frozen plan, the exact primary matrix, model diagnostics and all bootstrap draws. A reader can inspect how each reported number follows from the chosen endpoint and model. Such transparency can reveal transcription errors, hidden exclusions or a mistaken interpretation of a sentinel value. It cannot establish the clinical truth of a grade, resolve overlapping patients across publications or replace institutional review. The same distinction applies to the open reviews of the source: acknowledging the source's limitations is necessary, but a new statistical method does not retrospectively certify its measurements.

A stronger Saudi follow-up study would retain consecutive eligible operations, including conversions, and define the role of each assessment before data collection. It would document who assigned ASA and when, record cancer and procedure characteristics, and adjudicate complications using a specified severity framework through a common follow-up horizon. Recording discharge and event dates would permit a clearer treatment of follow-up and mortality. Multiple centers would broaden the case mix and allow evaluation of transportability. Such work could assess whether ASA adds clinically useful information beyond other baseline factors; the current small reanalysis does not establish that incremental predictive value. Its practical research contribution is to identify what should be measured and verified before a stronger claim is made.

Conclusion

In this selected historical colorectal surgery dataset from Jeddah, higher recorded ASA class was associated with a higher point estimate of major postoperative morbidity, with substantial uncertainty that included no difference. Sparse extreme classes, source grading contradictions, incomplete follow-up documentation and possible overlap with later same-center evidence limit interpretation. The analysis offers reproducible adjusted estimates and a source-data audit, while supporting no causal claim, national estimate or validated bedside prediction rule.

Tables

Table 1. Released cohort characteristics by recorded ASA group.

Characteristic	ASA I–II (n=69)	ASA III–V (n=51)
Records	69	51
Age, years, mean (SD)	57.6 (13.7)	66.5 (12.5)
Body mass index, kg/m², mean (SD)	26.9 (5.0)	27.5 (6.0)
Female sex	26 (37.7%)	22 (43.1%)
Emergency operation	11 (15.9%)	16 (31.4%)
Open operation	33 (47.8%)	32 (62.7%)

Characteristic	ASA I–II (n=69)	ASA III–V (n=51)
Saudi nationality	35 (50.7%)	25 (49.0%)
Colon cancer	49 (71.0%)	32 (62.7%)
Rectal cancer	13 (18.8%)	8 (15.7%)
Colorectal cancer, combined source category	7 (10.1%)	11 (21.6%)
Recorded ASA I	6 (8.7%)	0 (0.0%)
Recorded ASA II	63 (91.3%)	0 (0.0%)
Recorded ASA III	0 (0.0%)	48 (94.1%)
Recorded ASA IV	0 (0.0%)	2 (3.9%)
Recorded ASA V	0 (0.0%)	1 (2.0%)

Values are n (%) unless indicated. Diagnosis categories retain the source dictionary; the combined category is not reclassified from clinical notes. Sex and nationality are recorded source categories. No baseline significance tests were used to select adjustment variables.

Table 2. Recorded Clavien–Dindo grades and derived endpoints.

Recorded grade or endpoint	ASA I–II (n=69)	ASA III–V (n=51)
0: no recorded grade	45 (65.2%)	27 (52.9%)
I	5 (7.2%)	3 (5.9%)
II	10 (14.5%)	6 (11.8%)
III	4 (5.8%)	5 (9.8%)
IV	5 (7.2%)	8 (15.7%)
V: death	0 (0.0%)	2 (3.9%)
Major, III–V	9 (13.0%)	15 (29.4%)
Any, I–V	24 (34.8%)	24 (47.1%)

Code 5 is grade V in the codebook; the article table labels the terminal category VI. Four grade-0 records have a named postoperative complication and remain uncorrected because clinical adjudication is unavailable. Follow-up is the recorded postoperative episode; uniform active day-30 grade ascertainment is not established.

Table 3. Prespecified standardized and overlap-weighted associations.

Analysis	Records/events	Lower ASA risk %, 95% CI	Higher ASA risk %, 95% CI	Difference, percentage points, 95% CI
Primary: major complications	120/24	15.2 (7.4 to 23.5)	28.6 (17.0 to 42.2)	13.4 (-0.7 to 28.8)
Unadjusted: major complications	120/24	13.6 (6.8 to 21.0)	29.8 (17.8 to 42.7)	16.2 (1.6 to 32.1)
Add operative approach	120/24	15.7 (8.1 to 24.9)	28.4 (16.2 to 40.2)	12.7 (-3.7 to 27.5)

Analysis	Records/events	Lower ASA risk %, 95% CI	Higher ASA risk %, 95% CI	Difference, percentage points, 95% CI
Restrict to ASA I–III	117/21	15.2 (8.2 to 23.5)	24.9 (14.3 to 35.8)	9.7 (-4.0 to 22.8)
Secondary: any complication	120/48	37.2 (27.0 to 49.8)	44.5 (31.6 to 57.5)	7.2 (-11.3 to 23.3)
Overlap-weighted major complications	120/24	13.2 (5.0 to 22.3)	26.5 (14.1 to 39.5)	13.4 (-1.4 to 28.0)

Primary adjustment: age, sex, body mass index and emergency surgery. The approach sensitivity additionally includes open surgery. Restriction to I–III compares III with I–II in 117 records. The secondary row uses any complication, a different endpoint. Overlap weighting uses the primary covariates in the ASA-group model and targets an overlap population; the other rows standardize over their analysis cohorts. Intervals use 1,000 bootstrap samples for primary/overlap and 400 for each remaining row. All requested samples were retained. The primary conditional odds ratio is 2.31 (95% profile interval 0.89 to 6.28).

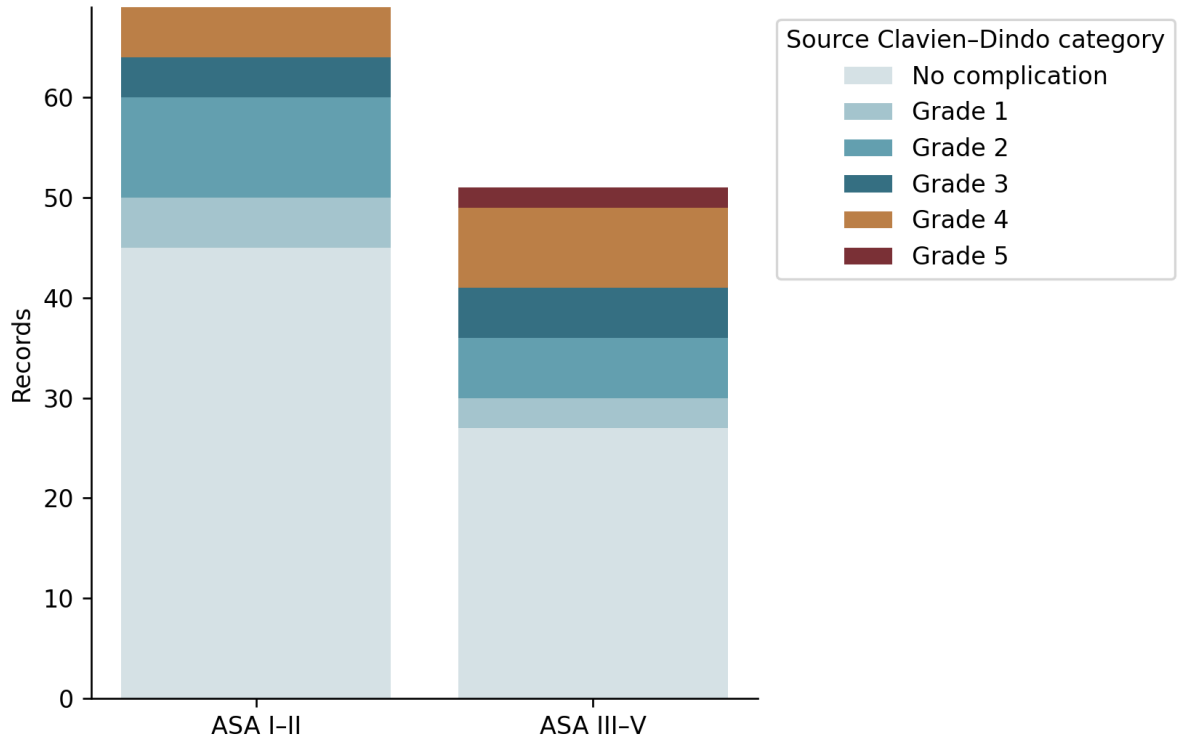
Table 4. Overlap-weighting diagnostics.

Covariate	Absolute SMD before	Absolute SMD after
Age	0.679	<0.001
Female sex	0.110	<0.001
Body mass index	0.109	<0.001
Emergency operation	0.366	<0.001

SMD: standardized mean difference, with the unweighted pooled standard deviation held fixed as denominator. Effective sample sizes were 58.5 (I–II) and 45.4 (III–V), from 69 and 51 records. Fitted propensity ranges were 0.060–0.716 and 0.204–0.816, respectively. Mean balance is a property of the fitted model and does not establish unmeasured balance, causal exchangeability or national representativeness.

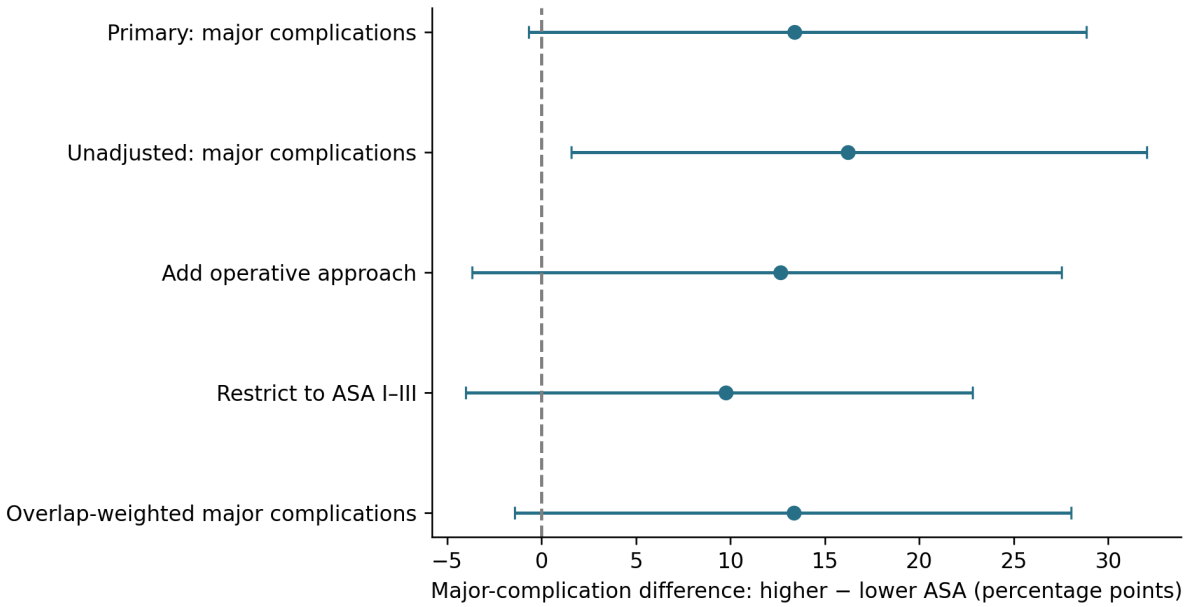
Figures

Figure 1. Recorded complication grades by ASA group.



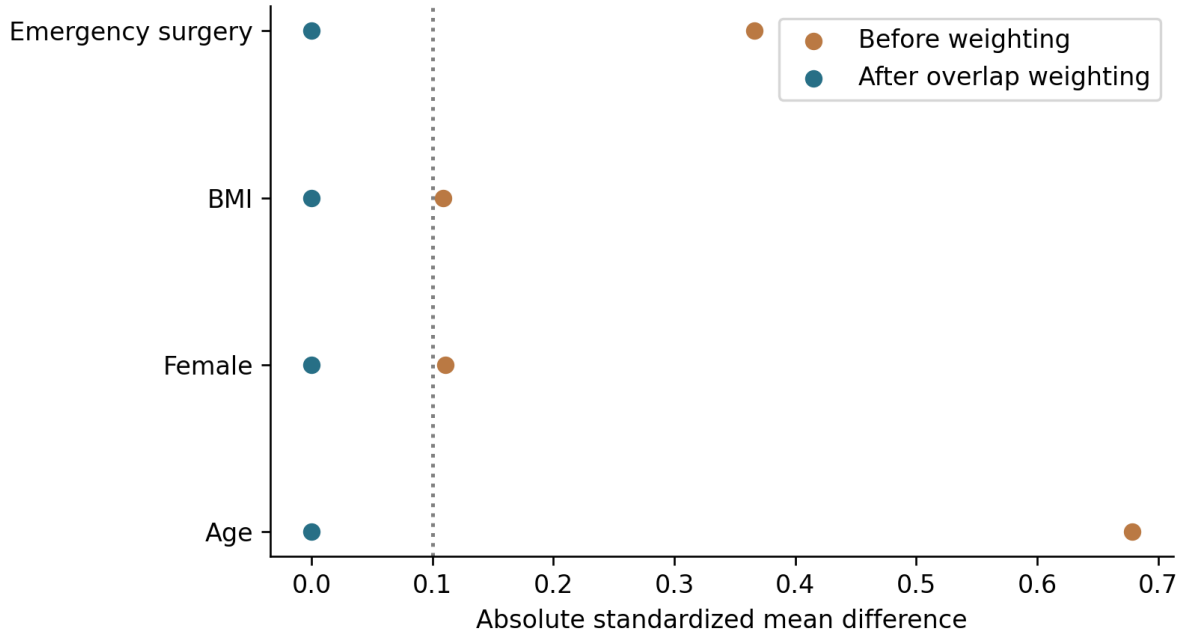
Stacked bars show record counts in each supplied Clavien–Dindo category. The unequal group sizes are retained. Code 5 is interpreted as grade V according to the source codebook despite the article table's grade-VI label. The two grade-V records remain major events. The display does not imply independent clinical adjudication.

Figure 2. Major-complication risk differences across planned analyses.



Points represent higher minus lower ASA; bars are percentile 95% bootstrap intervals. The primary and overlap analyses used 1,000 resamples, and the other displayed sensitivities used 400. The ASA-I–III restriction compares III with I–II. Overlap weighting targets a different covariate population from whole-cohort standardization. Secondary any-complication results appear in Table 3 because they concern a different endpoint.

Figure 3. Measured covariate balance before and after overlap weighting.



Absolute mean differences are standardized by the unweighted pooled standard deviation. The reference line at 0.1 is descriptive, not a certification of exchangeability. Balance pertains to the four modeled covariates and does not demonstrate balance in unmeasured disease severity or procedure characteristics.

Declarations and data availability

The original workbook and codebook are available from Zenodo under Creative Commons Attribution 4.0 and are attributed to their creator and source publication [12,13]. The analysis package contains unmodified source files, derived records, scripts, the frozen plan and locked results. No new recruitment, patient contact or chart access was undertaken. The source reports its own institutional approval and consent waiver; these belong to the original investigation. The present secondary analysis was exempt from ethical review because the data are publicly available and de-identified.

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This research received no external funding. The authors declare no competing interests. Artificial intelligence was not used in the preparation of this manuscript.

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