



# Evaluation of a quality improvement intervention to reduce anastomotic leak following right colectomy (EAGLE): pragmatic, batched stepped-wedge, cluster-randomized trial in 64 countries

ESCP EAGLE Safe Anastomosis Collaborative and NIHR Global Health Research Unit in Surgery

NIHR Global Health Research Unit on Global Surgery, Institute of Applied Health Research, University of Birmingham, Birmingham, UK

Correspondence to: Elizabeth Li, NIHR Global Health Research Unit on Global Surgery, Institute of Applied Health Research, University of Birmingham, Birmingham B15 2TT, UK (e-mail: [elizabeth.li@nhs.net](mailto:elizabeth.li@nhs.net))

Details of collaborating authors can be found under the heading Collaborators.

## Abstract

**Background:** Anastomotic leak affects 8 per cent of patients after right colectomy with a 10-fold increased risk of postoperative death. The EAGLE study aimed to develop and test whether an international, standardized quality improvement intervention could reduce anastomotic leaks.

**Methods:** The internationally intended protocol, iteratively co-developed by a multistage Delphi process, comprised an online educational module introducing risk stratification, an intraoperative checklist, and harmonized surgical techniques. Clusters (hospital teams) were randomized to one of three arms with varied sequences of intervention/data collection by a derived stepped-wedge batch design (at least 18 hospital teams per batch). Patients were blinded to the study allocation. Low- and middle-income country enrolment was encouraged. The primary outcome (assessed by intention to treat) was anastomotic leak rate, and subgroup analyses by module completion (at least 80 per cent of surgeons, high engagement; less than 50 per cent, low engagement) were preplanned.

**Results:** A total 355 hospital teams registered, with 332 from 64 countries (39.2 per cent low and middle income) included in the final analysis. The online modules were completed by half of the surgeons (2143 of 4411). The primary analysis included 3039 of the 3268 patients recruited (206 patients had no anastomosis and 23 were lost to follow-up), with anastomotic leaks arising before and after the intervention in 10.1 and 9.6 per cent respectively (adjusted OR 0.87, 95 per cent c.i. 0.59 to 1.30;  $P=0.498$ ). The proportion of surgeons completing the educational modules was an influence: the leak rate decreased from 12.2 per cent (61 of 500) before intervention to 5.1 per cent (24 of 473) after intervention in high-engagement centres (adjusted OR 0.36, 0.20 to 0.64;  $P<0.001$ ), but this was not observed in low-engagement hospitals (8.3 per cent (59 of 714) and 13.8 per cent (61 of 443) respectively; adjusted OR 2.09, 1.31 to 3.31).

**Conclusion:** Completion of globally available digital training by engaged teams can alter anastomotic leak rates. Registration number: NCT04270721 (<http://www.clinicaltrials.gov>).

## Research in context

This study focused on reducing a serious complication called anastomotic leak (occurring in 8 per cent of patients) after right colectomy, a common bowel operation. The authors developed a global programme called EAGLE, involving 64 countries and 3268 patients. The programme included online training for surgical techniques, a digital risk calculator, and an in-theatre checklist. Although the overall leak rates did not decrease, hospitals with over 80 per cent team engagement saw reduced leak rates. This study highlights the importance of team involvement in implementing interventions successfully. The EAGLE programme is a cost-effective and scalable solution for preventing anastomotic leaks. Surgeons, including trainees, can access the training module at <https://eagle-escp.eu.com/>.

## Introduction

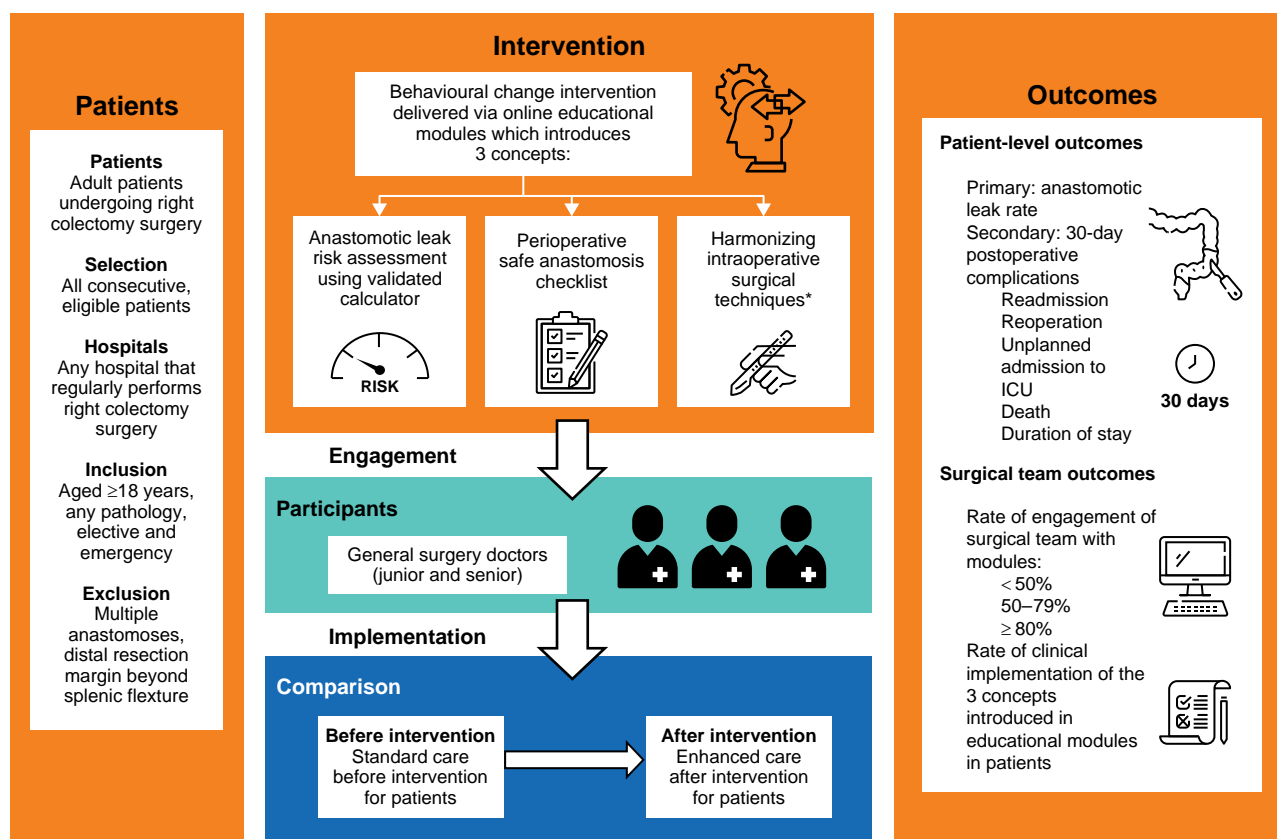
Right colectomy is performed most frequently for cancer (78 per cent)<sup>1</sup> or Crohn's disease (12 per cent)<sup>2</sup>, by both general and specialist surgeons in high- and lower-middle-income countries. Anastomotic leak arises in over 8 per cent of patients with a 10-fold increased risk of postoperative death<sup>3</sup>. When it occurs, there is frequently the need for a stoma, a two-fold increased risk of cancer recurrence<sup>4</sup>, and reduced cancer-specific survival after cancer surgery. A James Lind Alliance Priority Setting Partnership<sup>5</sup> identified anastomotic leak as a research priority for patients as there has been little improvement recently<sup>4,6</sup>.

A multinational audit<sup>2,3</sup> demonstrated variation in surgical techniques and anastomotic leak rates. It also identified patient and disease factors to be additional major risk indicators for

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**Fig. 1** Modified PICO graph for EAGLE study

\*Harmonizing of intraoperative techniques is a suite of learning materials, discussions, and operative videos that explore the challenges that may face surgeons during operation and supports surgeons' decision-making by presenting the best evidence available on how to tackle these challenges. PICO, patients, intervention, comparator, outcome.

anastomotic leaks<sup>1</sup>. These multiple factors point to numerous stages of success or failure, and indicate that the application of a complex intervention based on the Medical Research Council/National Institute for Health Research (NIHR) framework appear to provide the most appropriate approach. The aim of EAGLE was to develop a quality improvement intervention that included behavioural change tested by prospective, randomized methodology<sup>7</sup>.

## Methods

### Intervention development

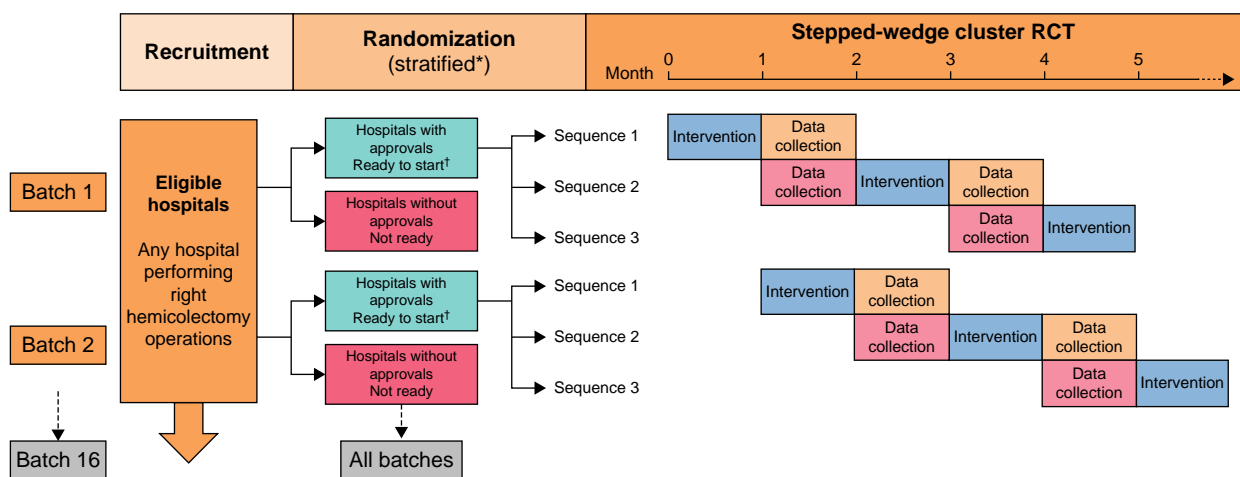
The quality improvement intervention was developed based on the Medical Research Council framework for complex interventions<sup>8</sup> and the COM-B (Capacity, Opportunity, Motivation, Behaviour) model<sup>9,10</sup> for behavioural change (Fig. S1). The authors acknowledged that quality improvement and behavioural change can be time-consuming, so EAGLE was designed to be pragmatic, prioritizing engagement. EAGLE was investigator-initiated and investigator-led.

The intervention was co-developed through iterative cycles with key stakeholders, including a diverse global team of senior surgeons, anaesthetists and theatre staff, surgical trainees, patient representatives, methodologists, and statisticians. In 2018, a Delphi consultation was conducted with 200 senior surgeons from 30 countries to underpin the content domains of the programme. Online learning was selected as the main strategy for delivering training (Appendix S1) because it offered the opportunity for learners from different parts of the world to

engage with educational packages despite separation in geography, time zones, and chronological time (across the batched study). A scoping review was undertaken to identify a validated, high-quality, easy-to-use anastomotic leak risk calculator. The quality improvement programme was further refined by a core team of multinational consultant and trainee surgeons, and patient representatives for online learning and in-theatre checklists (Appendix S2). Pilot modules were developed and presented in May 2019 to 200 surgeons, and a consultation exercise was conducted for feedback, suggestions, and identifying and troubleshooting problems. Online modules were further refined and beta-tested by 50 surgeons, leading to a final round of changes in July 2019.

### Intervention description

EAGLE was a complex intervention, providing a hospital-level educational programme targeted at surgical teams. Patients who underwent surgery before the intervention received standard care, and those who had surgery after the intervention received treatment by surgical teams that were exposed to the EAGLE Safe Anastomosis programme. The intervention was an online educational platform, delivered at the hospital level and implemented at patient level. It comprised three components (Fig. 1). The first was an anastomotic leak calculator (anastomoticleak.com<sup>11,12</sup>), which is an online validated tool for calculating the risk of anastomotic leak (with additional intraoperative recalculation, if necessary). A risk calculation tool was chosen provided an objective, evidence-based estimation of leak risk. Use of the tool before operation allowed the hospital



**Fig. 2** Trial design schematic

\*Randomization stratified by number of beds, referral or non-referral hospital, country income. †Minimum of 18 hospitals per batch to be ready for randomization.

team time to assess risks and plan the optimal procedure with each patient. Importantly, it facilitated shared decision-making with the patient by opening dialogue and quantifying their individual leak risk. The specific risk tool was chosen as it was the only one available that had been externally validated<sup>11,12</sup>. The second component, the EAGLE Safe Anastomosis Checklist, is an intraoperative checklist involving the whole theatre team, used immediately before forming an anastomosis (Appendix S3). This checklist instituted a pause in theatre activities immediately before the decision was made to form an anastomosis, and/or regarding the configuration and technique of stoma formation. This offered an opportunity for the whole team to reassess the operation and the stability of the patient, and voice any concerns to facilitate team decision-making. The final component was a harmonization of surgical techniques package, aimed at unifying the wide variety of operative approaches previously observed in the European Society of Coloproctology (ESCP) audits. The online educational package curriculum is summarized in Appendix S2 and can be accessed at <https://eagle-escp.eu.com/>.

## Engagement and completion

The authors set out to maximize completion, defined as the number of surgeons who finished all five modules of the online learning materials, and team engagement, defined as the proportion of surgeons who completed all five modules in each hospital team. Access to the modules was provided at the start of a 4-week intervention interval, within hospital teams in each sequence (Fig. 2). The principal investigators (PIs) (surgeon, anaesthetist, and lead theatre staff member) from each hospital were invited to a webinar, in which they were introduced to the module structure and content. They were also provided with a slide set that was editable to create bespoke presentations tailored to their teams. Three separate slide sets were produced for surgeons, anaesthetists, and theatre staff.

The surgical PI provided and registered the contact details for all members of their team, who they felt could participate in the study. This was to maximize engagement, but it was recognized that individual surgeons within a cluster could, and would, choose not to undertake the online modules. This design enabled the impact of team engagement to be assessed within

the trial structure. An individual login was provided to each registered surgeon for the online modules.

A webinar was hosted for each batch of clusters before they opened to recruitment. This included hospital teams that had and had not received the intervention, and so made no reference to the online modules. A separate webinar for each batch of clusters was hosted before the intervention was undertaken. Update webinars were held for all investigators every 3 months to share progress in trial development and delivery; a link to an example of a webinar can be found in Appendix S3.

## Trial design, ethics, and approvals

EAGLE was an international, multicentre, batched, cluster-randomized study, in which clusters were individual hospital teams (Fig. 2). As the study involved implementation of team training, a cluster-randomized design, with each hospital team as a cluster, was necessary. A stepped-wedge design was chosen instead of a conventional parallel-arm cluster trial to maximize statistical efficiency, so that all clusters would eventually be exposed to the intervention and could act as their own control<sup>13,14</sup>. This also facilitated timely, equitable access to the evidence-based educational materials by all teams, irrespective of which arm they were randomized to. A batched design was adopted so that randomization could occur in batches, depending on the readiness of individual hospital teams and obtaining local approvals<sup>15,16</sup>. This made the trial pragmatic as hospital teams did not all need to start the intervention at the same time. It also minimized the time from randomization to intervention, and minimized any potential bias or contamination. Recognizing that the intervention could not be withdrawn from a hospital once it had been implemented, an incomplete stepped-wedge (specifically, a dog-leg) design was finally chosen<sup>17,18</sup>. The hospital teams were randomized in batches once a suitable number were ready (Fig. 2). A minimum of 18 hospital teams were randomized in each batch and stratified to minimize any potential bias in differing hospital characteristics in each randomization arm (Fig. 2). Hospital teams in each batch were randomized in a dog-leg design to three possible sequences with up to two intervals of data collection. In the first sequence, the

intervention was implemented before any data collection took place, and data were collected in the first interval only. In the second sequence, data were collected in both intervals, with the intervention implemented in between. In the third sequence, the intervention was not implemented until after data collection, and data were collected in the second interval only. This incomplete stepped-wedge design was chosen to maximize statistical efficiency and minimize the burden of data collection. More detail on the design and lessons learned from its implementation have been reported previously<sup>15</sup>.

The study was registered at ClinicalTrials.gov (NCT04270721) and the protocol has been published<sup>17</sup>. As the intervention comprised training delivered at a hospital-level, Health Research Authority (HRA) approval was obtained for research not requiring patient-level consent in the UK (REC reference:19/HRA/5656). This was also the case for the large majority of hospitals, internationally. Where ethical approval was required, the national PI was responsible for obtaining this before the hospital was ready for entry into randomization. Consent was obtained for surgery as part of routine care and consent to research was not required in most countries, as the intervention was implemented at hospital level and the primary outcome derived from routinely collected patient outcomes.

### Hospital eligibility, team structure

EAGLE was designed to be globally inclusive, enrolling any surgical unit that performed right colectomy regardless of hospital size or annual case volume (Table S1). Registration required identification of surgical, anaesthetic, and nursing PIs.

### Patients and data collection

Consecutive patients, aged 18 years and over, undergoing right colectomy (including ileocaecal resection) for any indication or urgency were included in the study, whether or not an anastomosis and/or stoma was formed. Excluded were patients undergoing more than one anastomosis, those having additional synchronous procedures, and those undergoing a second eligible operation in the data collection window. The full inclusion and exclusion criteria are listed in Appendix S3. Routine patient data were collected from hospital notes and reported electronically via the Research Electronic Data Capture web application (REDCap; Vanderbilt University, Nashville, TN, USA)<sup>19,20</sup>, hosted in the University of Birmingham, UK.

### Outcomes

The primary outcome was anastomotic leak in patients who had a primary anastomosis, within 30 days of surgery (operation day was day 0). The study used the ESCP consensus definition of 'leak or intraperitoneal (abdominal or pelvic) fluid collection identified radiologically or clinically'<sup>6</sup>. Follow-up was for 30 days after surgery. Routinely collected patient data were retrieved from medical notes, electronic health records or routine in-person assessments by the local team.

Secondary clinical outcomes were assessed at 30 days after operation, and included clinical leaks, reoperation for anastomotic leak, readmission to hospital, reoperation for any reason, unplanned admission to critical care, duration of hospital stay (10 or fewer versus more than 10 days), stoma formation (stoma without primary anastomosis and defunctioning ileostomy), and death.

### Sample size and adjustments

The original sample size was calculated based on published evidence from ESCP audit data (2015)<sup>6</sup>, with mean recruitment of 10 patients per 8-week recruitment phase, and an intraclass correlation coefficient of approximately 0.05; full details are available in the protocol<sup>17</sup>. Detection of a 30 per cent reduction in anastomotic leak from 8.1 to 5.6 per cent, was calculated to require 333 hospital teams and 4440 patients. *A priori* trial planning allowed sample size adjustments after the first look. Interim, blinded results from the first eight completed batches were presented to the Data Monitoring Committee, and subsequently to the Trial Steering Committee, showing that the leak rate aggregated across intervention and control arms was 9.0 per cent. For this aggregated leak rate of 9.0 per cent, the predicted difference in leak rate would need to fall from 10.6 per cent in the control to 7.4 per cent in the intervention arm to represent a 30 per cent reduction in leak. The number of procedures observed in each data collection phase in each hospital averaged around 8, which was smaller than the figure of 10 assumed in the original sample size calculation. Following the *a priori* plan, sample size reassessment based on these figures produced a new minimum target sample size of 312 clusters and 3328 participants. Owing to the lower than anticipated patient recruitment per site, the Trial Steering Committee recommended that the original cluster target of 333 hospital teams should stand.

### Randomization

When a batch of clusters was ready for randomization, the central team matched hospital teams into triplets that were matched according to: World Bank country income classification (low, lower middle, upper middle or high income; see Appendix S3 for definitions)<sup>21</sup>; tertiary referral status (whether their hospital accepts referrals from other hospitals); and hospital size (total number of hospital beds, dichotomized to below 500 and 500 or more). The clusters in each triplet were then randomized to each of the three sequences. Cluster allocation was undertaken by the trial statistician using a REDCap randomization module (Fig. 2).

### Blinding

Hospital teams did not receive the EAGLE intervention material until the start of their intervention interval, to prevent contamination. As a hospital-level intervention, neither surgeons nor hospital teams were blinded; however, patients were unaware of their hospital participation status. Study teams were required to collect outcome data objectively. They were unblinded to hospital intervention status.

### Case ascertainment

To ensure consecutive case ascertainment, each hospital team nominated an independent assessor (usually theatre staff not part of the EAGLE hospital team) to identify retrospectively all eligible patients from theatre logbooks. These data were cross-checked by the Birmingham trials team. Inconsistencies resulted in rechecking the whole data collection period for that centre (Fig. S2).

### Data validation

The EAGLE operations team completed missing data by direct contact with the local investigators. Data accuracy was also validated at two hospital teams per randomization sequence



(6 per batch; approximately 20 per cent of hospital teams), via a random number generator. A local nominated assessor completed the data validation on 10 key data points for up to the first 10 consecutive patients in a single data collection interval. Any hospital team with more than 10 per cent overall data inaccuracy was required to recheck the whole data collection.

## Statistical analysis

Analysis followed the plan specified in the published protocol and the statistical analysis plan<sup>17</sup>.

### Primary outcome

In each study batch, the effect of the intervention on anastomotic leak rate was estimated using mixed-effects logistic regression with random cluster effects (normally distributed on the logistic scale) to estimate the effect of intervention adjusting for time interval, that is data collection interval 1 *versus* interval 2. Adjustment was also made for cluster matching characteristics (country income status, tertiary referral status, and hospital size) in so far as these varied within the batch, and for patient sex and operative urgency<sup>22</sup>. These co-variables were all categorical (for more details, see protocol in [supplementary material](#)). The log OR and standard error for the intervention effect were extracted from the mixed logistic regression analysis for each batch, and pooled in a random-effects meta-analysis using the inverse-variance approach of DerSimonian and Laird<sup>23</sup>. A forest plot was prepared showing the overall intervention effect, along with 95 per cent c.i. and P value.

### Secondary outcomes

The plan was to analyse secondary outcomes in the same way as the primary outcome, but, owing to the low prevalence of secondary outcomes, frequent problems were encountered with convergence of the mixed regression model within individual batches. Therefore, in a change to the planned analysis, a single multilevel model was fitted to data from all batches, with batch as a random effect, and cluster within batch as a second level of clustering. This simpler analytical model fits a constant treatment effect across clusters rather than the random effect fitted by the meta-analysis of the primary outcome, but the authors wanted to fit as few random effects as possible to minimize the risk of non-convergence. The model adjusted for a fixed effect of the time period within the entire span of the study that data collection was conducted. This was assumed to be the same for all batches. The following prespecified secondary outcomes were assessed: occurrence of clinical leak; reoperation for anastomotic leak; reoperation for any reason; unplanned admission to critical care; readmission to hospital; postoperative death; stoma without primary anastomosis; and defunctioning ileostomy with primary anastomosis. For the purpose of analysis, hospital stay was dichotomized into 10 or fewer *versus* more than 10 days.

### Subgroup analyses

To assess possible modification of the intervention effect, planned subgroup analyses were conducted for prespecified hospital- and patient-level characteristics. Hospital-level characteristics were: number of hospital beds (less than 500, or 500 or more); right colectomy volume (below 10, or 10 or more per 2-month interval); early *versus* late involvement in the trial (batch 1–8 or batch 9–16); health service expenditure; World Bank income group; proportion of operating surgeons who completed the

online educational modules (less than 50, 50–79, or at least 80 per cent). Patient-level characteristics were: indication; urgency; ASA grade<sup>24</sup>; operative approach (laparoscopic, robotic or open); anastomosis formation technique (stapled or handsewn); primary operating surgeon grade; and primary operating surgeon specialism. The plan for each characteristic was to fit a mixed logistic regression model in each batch (as for the primary analysis), but with the addition of a statistical interaction between the potential effect modifier and the intervention effect. The log of the ratio of ORs representing the interaction (with its standard error) would be extracted for each batch and pooled in a random-effects meta-analysis, as for the primary analysis. In practice, model convergence issues arose, similar to those encountered with secondary outcomes, and, in a change to the planned analysis, a single multilevel model was fitted to all batches, as for the analyses of secondary outcomes. Owing to the number of factors considered in these subgroup analyses, the results were considered exploratory and to be interpreted with caution.

### Sensitivity analysis

Planned sensitivity analysis was limited to per-protocol analysis of the primary outcome. Because of the unforeseen impact of the COVID-19 pandemic on the trial, an additional sensitivity analysis was undertaken that excluded the first two batches, which were interrupted.

### Missing data

The mixed logistic regression analyses included only patients with complete outcome data. This approach is valid and unbiased under the assumption that missingness in the outcome is systematically related only to the co-variables and other variables that are included in the analysis model (a missing-at-random assumption). For inclusion in the primary intention-to-treat analysis, patients must have had a primary anastomosis formed and had surgery within the data collection time interval specified by the design. Recorded data collected outside of this interval (in error) were excluded.

## Results

Between 3 February 2020 and 1 December 2022 (including a 5-month pause owing to the global SARS-CoV-2 pandemic lockdown), the EAGLE study randomized 355 hospital clusters, in 16 batches, from 64 countries ([Fig. 3](#)). For each randomization batch, the sequence allocation and exclusions are detailed in [Fig. S4a,b](#). A total of 15 clusters (4.2 per cent) (hospital teams) withdrew from the study before patients were recruited and a further 8 (2.3 per cent) who did not enter any patients during the data collection interval, leaving 332 hospital clusters in the study. Some 141 hospital teams (39.2 per cent) were from low- and middle-income countries ([Table 1](#)). Both hospital stratification factors ([Table 1](#)) and patient variables ([Table S3](#)) were distributed evenly across the three randomization sequences. The need for emergency intervention varied between regions, as did patient characteristics: hypoproteinaemia was more common in Asia and Africa, and obesity and anticoagulation more common in Europe ([Table S1](#)).

From 332 hospital teams, a total of 3268 patients were included in the trial. Patient flow is shown in [Fig. 4](#), and detailed by randomization batch in [Fig. S4a,b](#). Some 217 patients (6.3 per cent) underwent stoma formation without anastomosis. Primary outcome data were missing for 23 of 3062 patients (0.7 per cent).

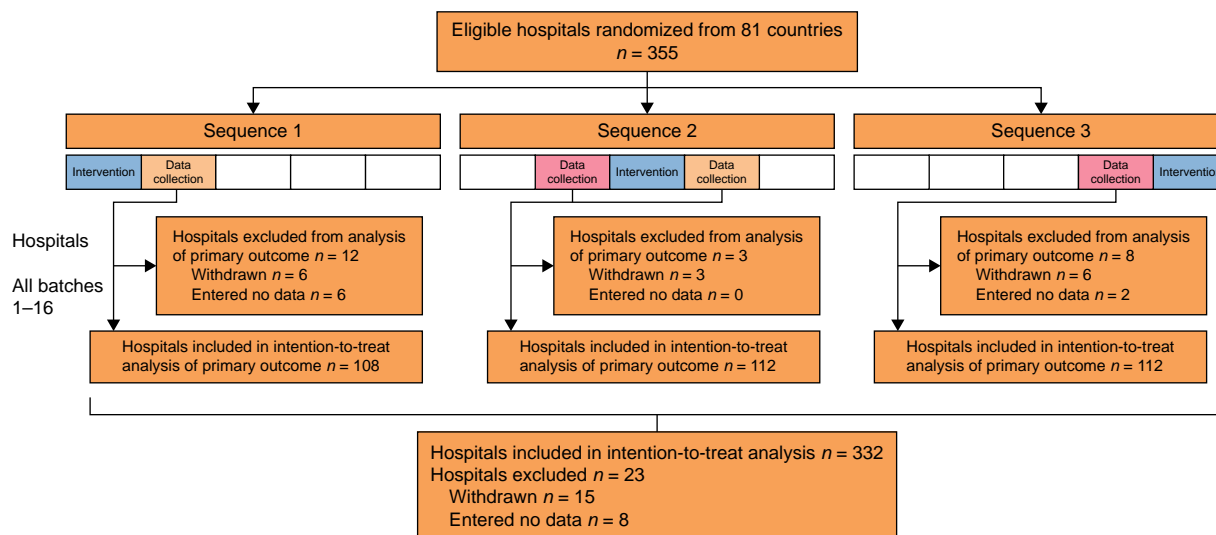


Fig. 3 Hospital-level CONSORT diagram

Table 1 Hospital characteristics by randomization sequence

	Sequence 1 n = 120	Sequence 2 n = 115	Sequence 3 n = 120
<b>No. of beds</b>			
< 500	53 (44.2)	49 (42.6)	56 (46.7)
≥ 500	67 (55.8)	66 (57.4)	64 (53.3)
<b>Type of hospital</b>			
Referral	96 (80.0)	91 (79.1)	90 (75.0)
Non-referral	24 (20.0)	24 (20.9)	30 (25.0)
<b>Region</b>			
Africa	8 (6.7)	12 (10.4)	7 (5.8)
Asia	23 (19.2)	20 (17.4)	18 (15.0)
Europe	76 (63.3)	73 (63.5)	82 (68.3)
South America	11 (9.2)	10 (8.7)	11 (9.2)
Oceania	2 (1.7)	0 (0)	2 (1.7)
<b>Country income</b>			
Low	5 (4.2)	7 (6.1)	5 (4.2)
Middle	49 (40.8)	37 (32.2)	42 (35.0)
High	66 (55.0)	71 (61.7)	73 (60.8)

Values are n (%).

The remaining 3039 patients were included in the primary analysis. Patient characteristics and operative variables according to timing of data collection (before or after intervention) are summarized in Tables 2 and 3, and detailed by sequence and batch in Tables S3–S6. These represent patient and operative risk factors for anastomotic leak, identifiable before or during surgery; they were evenly distributed before and after intervention. Demographics and operative factors for patients excluded from the primary analysis appeared representative of the whole population (Table S7). This study represents a high-risk population; 1354 of 3268 patients (41.4 per cent) had an ASA grade of III–V, 663 (20.3 per cent) had diabetes, 694 (21.2 per cent) had emergency operations, and there were 2540 operations for cancer (78.0 per cent).

Some 4411 surgeons were invited to undertake the intervention; 2774 (62.9 per cent) surgeons started and 2143 (48.6 per cent) completed the online training and registered for a certificate. A further 393 anaesthetists and 393 theatre staff also took part in the study. The median number of surgical team

members participating in each hospital was 7 (i.q.r. 5–21, range 1–54).

### Primary outcome

The absolute anastomotic leak rate was 10.1 per cent (170 of 1691) during data collection before the intervention and 9.6 per cent (129 of 1348) in the data collection interval after the intervention (Fig. 5). The pooled OR obtained from a mixed logistic regression analysis of each batch of clusters, adjusting for time interval, was 0.87 (95 per cent c.i. 0.59 to 1.30;  $P=0.498$ ) (Fig. 5). This confidence interval was wide and included 1.0, although it did rule out a reduction of more than 40 per cent in the odds of anastomotic leak as a result of the intervention.

### Secondary outcomes

As with the primary outcome, confidence intervals for intervention effects on secondary outcomes were wide (Tables 4, S8, and S9) but ruled out reductions of more than 40 per cent in the odds of death, readmission, reoperation, unplanned ICU admission, stoma without primary anastomosis, and longer hospital stay.

### Subgroup analyses

Results of subgroup analyses are shown Fig. 6, with ORs for hospital- and patient-level factors. Further details can be found in Tables S10–S22 and Fig. S5. The intervention effect was modified by the proportion of surgeons in a hospital team completing online educational modules (team engagement). Anastomotic leak rates before and after intervention were 8.3 and 13.8 per cent respectively in hospitals with low team engagement (less than 50 per cent) (adjusted OR 2.09, 95 per cent c.i. 1.31 to 3.31); 10.5 and 10.2 per cent in hospitals with intermediate team engagement (50–79 per cent) (adjusted OR 0.94, 0.55 to 1.62); and 12.2 and 5.1 per cent in those with high team engagement (at least 80 per cent) (adjusted OR 0.36, c.i. 0.20 to 0.64) ( $P$  for interaction < 0.001). This interaction suggests that there was a reduction in anastomotic leak rate as a result of the intervention at hospitals with the highest level of engagement, although it also suggests an increase in

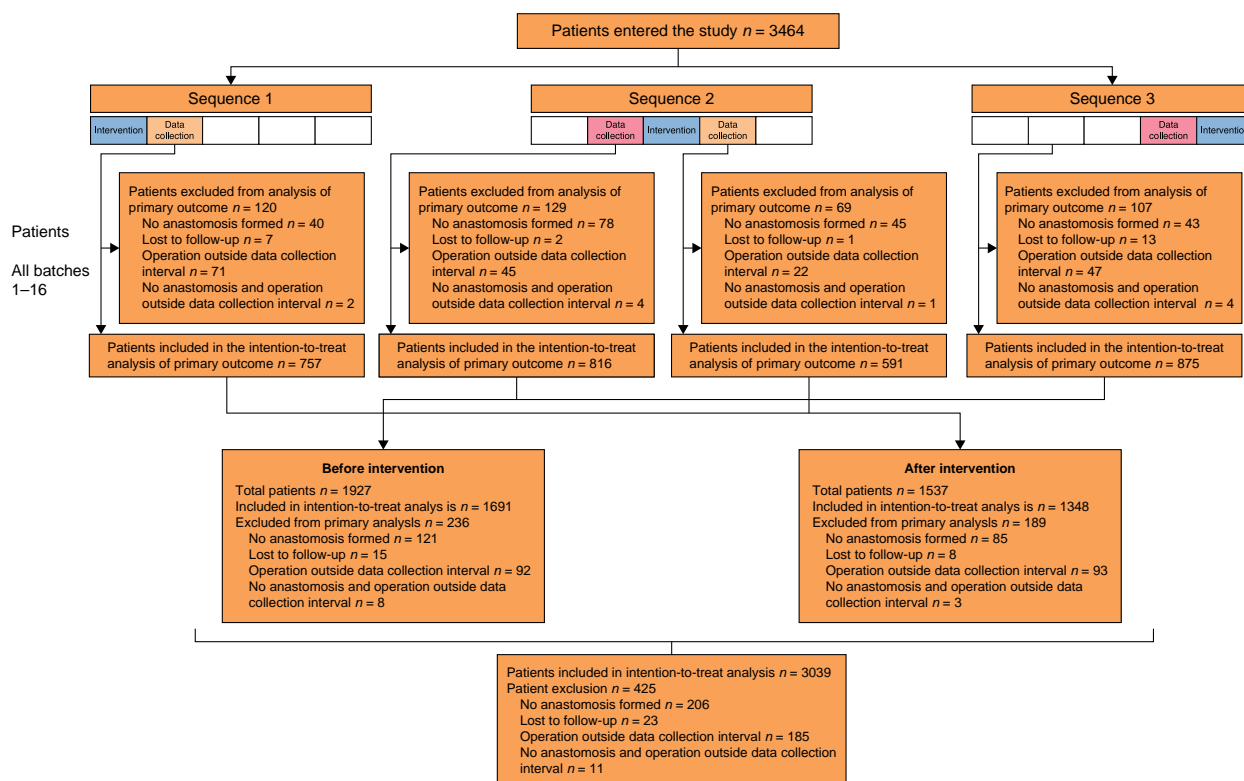


Fig. 4 Patient-level CONSORT diagram

Table 2 Patient characteristics by timing of intervention relative to surgery

	Surgery before intervention n = 1827	Surgery after intervention n = 1441	Total n = 3268
Age (years), median (i.q.r., range)	68 (56–77, 1–100)	68 (56–77, 2–95)	68 (56–77, 1–100)
<b>Sex</b>			
Male	951	738	1689
Female	874	701	1575
<b>BMI &gt; 30 kg/m<sup>2</sup></b>			
No	1455 (80.3)	1146 (79.6)	2601 (80.0)
Yes	357 (19.7)	293 (20.4)	650 (20.0)
<b>Known diabetes</b>			
No	1444 (79.1)	1159 (80.5)	2603 (79.7)
Yes	382 (20.9)	281 (19.5)	663 (20.3)
<b>History of IHD or stroke</b>			
No	1439 (78.9)	1170 (81.2)	2609 (79.9)
Yes	385 (21.1)	271 (18.8)	656 (20.1)
<b>Smoking status</b>			
Never smoked	1199 (65.9)	978 (68.3)	2177 (67.0)
Ex-smoker (stopped > 6 weeks ago)	372 (20.5)	285 (19.9)	657 (20.2)
Current smoker or stopped < 6 weeks ago	248 (13.6)	168 (11.7)	416 (12.8)
<b>Oral anticoagulants</b>			
No	1540 (84.7)	1229 (85.5)	2769 (85.0)
Yes	279 (15.3)	208 (14.5)	487 (15.0)
<b>Preoperative total protein level (g/dl)</b>			
≤ 4.5	128 (7.8)	81 (6.1)	209 (7.1)
4.5–5.5	360 (22.0)	273 (20.7)	633 (21.4)
6.0–7.5	1068 (65.1)	905 (68.6)	1973 (66.7)
≥ 8.0	84 (5.1)	61 (4.6)	145 (4.9)
<b>Preoperative haemoglobin (g/dl)</b>			
< 60	22 (1.2)	23 (1.6)	45 (1.4)
60–89	175 (9.6)	160 (11.1)	335 (10.3)
90–119	821 (45.3)	607 (42.3)	1428 (44.0)
120–139	534 (29.4)	415 (28.9)	949 (29.2)
≥ 140	262 (14.4)	230 (16.0)	492 (15.1)

Values are n (%) unless otherwise indicated. IHD, ischaemic heart disease.

**Table 3 Operative factors by timing of intervention**

	Surgery before intervention n = 1827	Surgery after intervention n = 1441	Total n = 3268
<b>Timing of surgery</b>			
Elective (planned)	1221 (66.8)	1004 (69.7)	2225 (68.1)
Expedited (within 2 weeks of decision)	205 (11.2)	144 (10.0)	349 (10.7)
Emergency (unplanned)	401 (21.9)	293 (20.3)	694 (21.2)
<b>Indication for surgery</b>			
Malignancy	1399 (76.7)	1141 (79.6)	2540 (78.0)
Inflammatory bowel disease	171 (9.4)	103 (7.2)	274 (8.4)
Other	253 (13.9)	190 (13.2)	443 (13.6)
<b>Bowel preparation</b>			
None	414 (22.7)	274 (19.1)	688 (21.1)
Mechanical bowel preparation only	322 (17.7)	272 (18.9)	594 (18.2)
Mechanical bowel preparation with oral antibiotics	1086 (59.6)	892 (62.0)	1978 (60.7)
<b>ASA grade<sup>25</sup></b>			
I	217 (12.0)	160 (11.2)	377 (11.6)
II	838 (46.4)	669 (46.8)	1507 (46.5)
III	653 (36.1)	534 (37.3)	1187 (36.7)
IV	88 (4.9)	64 (4.5)	152 (4.7)
V	11 (0.6)	4 (0.3)	15 (0.5)
<b>Primary operating surgeon</b>			
Consultant colorectal surgeon	934 (51.2)	764 (53.1)	1698 (52.0)
Trainee colorectal surgeon	210 (11.5)	188 (13.1)	398 (12.2)
Consultant general surgeon	465 (25.5)	338 (23.5)	803 (24.6)
Trainee general surgeon	215 (11.8)	150 (10.4)	365 (11.2)
<b>Most senior surgeon in theatre</b>			
Consultant colorectal surgeon	1188 (65.0)	950 (65.9)	2138 (65.4)
Trainee colorectal surgeon	38 (2.1)	44 (3.1)	82 (2.5)
Consultant general surgeon	494 (27.0)	378 (26.2)	872 (26.7)
Trainee general surgeon	107 (5.9)	69 (4.8)	176 (5.4)
<b>Operative approach</b>			
Open	883 (48.3)	643 (44.6)	1526 (46.7)
Laparoscopic (completed)	762 (41.7)	650 (45.1)	1412 (43.2)
Laparoscopic (converted to open)	133 (7.3)	121 (8.4)	254 (7.8)
Robotic (completed)	47 (2.6)	26 (1.8)	73 (2.2)
Robotic (converted to open)	2 (0.1)	1 (0.1)	3 (0.1)
<b>Operative field contamination*</b>			
Clean-contaminated	1586 (87.0)	1258 (87.4)	2844 (87.2)
Contaminated	158 (8.7)	125 (8.7)	283 (8.7)
Dirty	79 (4.3)	56 (3.9)	135 (4.1)

Values are n (%). \*Centers for Disease Control and Prevention<sup>26</sup>; definition in Appendix S1.

anastomotic leak rate at hospitals with the lowest engagement—something that was not hypothesized. Team engagement was lower in hospitals involved later in the trial (Fig. S5).

There was no evidence of an interaction of the intervention effect with number of hospital beds, right colectomy volume, early versus late involvement in the trial, health service

expenditure, or World Bank income group, at participant level, indication, urgency, ASA grade, operative approach, anastomosis formation technique, primary operating surgeon grade, or primary operating surgeon specialism.

## Sensitivity analyses

Per-protocol sensitivity analysis was also conducted for the primary outcome in patients in whom all three intervention components (training, risk stratification, and checklist) were completed (OR 0.84, 95 per cent c.i. 0.53 to 1.31;  $P = 0.439$ ) (Fig. S6 and Tables S23–S28).

Clusters from randomization batches 1 and 2 were paused owing to the COVID-19 pandemic and restarted when ready. Details have been published<sup>15</sup>. Because of this disruption, a sensitivity analysis was performed, after exclusion of batches 1 and 2 (OR 0.85, 0.56 to 1.30;  $P = 0.461$ ) (Fig. S7).

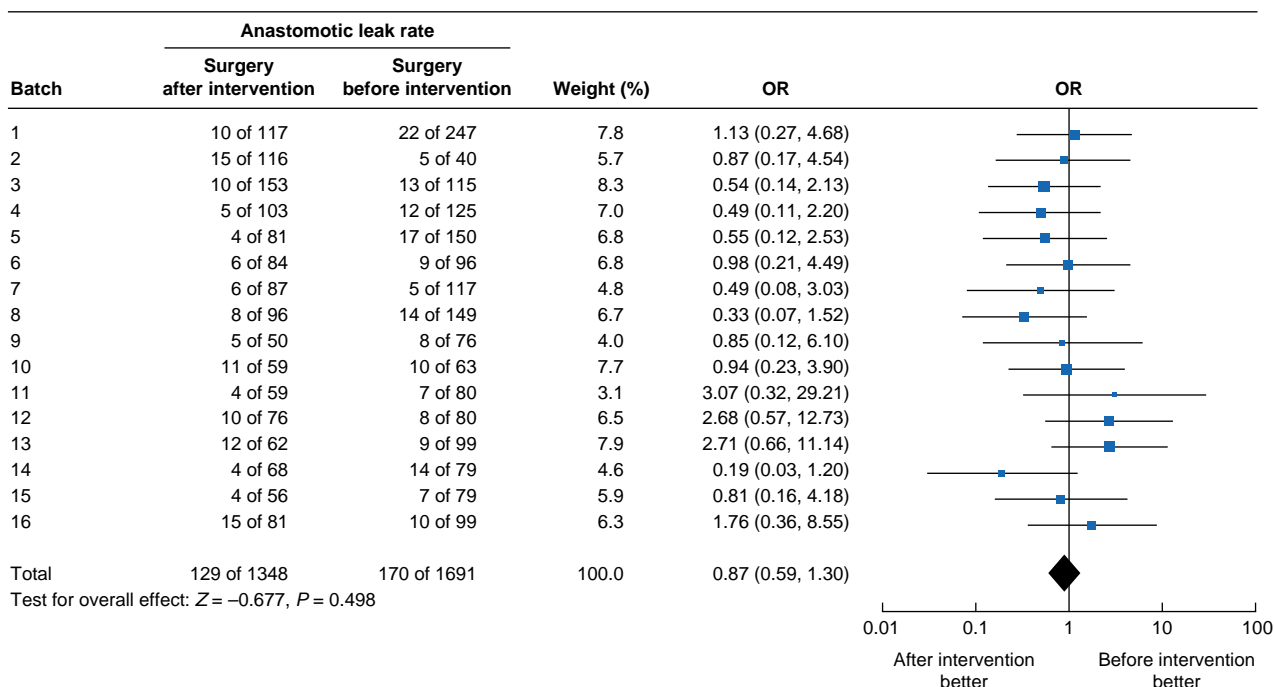
## Discussion

The EAGLE trial did not find conclusive evidence for a reduction in anastomotic leak rates following the intervention, although a subgroup analysis did identify a reduction in leak rates in hospital teams with high team engagement in the intervention. This may reflect better knowledge, better decision-making, and more engaged team performance through enhancement of non-technical skills.

This complex intervention required the participation of a multidisciplinary team of surgeons, nurses, and anaesthetists to implement practice change within a hospital. It cannot be ignored that the EAGLE trial was carried out when team function was being severely challenged by the COVID-19 pandemic. There was evidence of reduced team engagement in the later randomization batches (Fig. S5), reflecting surgical services struggling during the recovery phase of the pandemic. This clinical challenge was reported widely<sup>27–29</sup>, being explained by the overwhelming demand on restarting elective surgical services. This also coincided with a rising baseline of anastomotic leaks during this phase of the EAGLE trial (Table S12), perhaps reflecting the significant challenges faced by surgeons at this time and more advanced disease resulting from delayed patient presentation.

Quality improvement can be an important tool for hospital teams. Examples such as the WHO Surgical Safety Checklist<sup>30</sup> and the INTERACT3 trial<sup>31</sup> have improved patient outcomes. It is being increasingly recognized that low-standard quality improvement needs to be avoided<sup>7</sup>, but that high-standard quality improvement will need lighter-touch, digital processes (INTERACT3 and EPOCH<sup>31,32</sup>). Dissemination and uptake of new tools, such as the WHO Trauma Checklist<sup>33</sup>, could benefit from such evaluation. The EAGLE online quality improvement intervention was accessed by 2774 surgeons across 5 continents in 332 hospital teams, changing clinical practice across 3 continents. The methodology was highly efficient, able to be delivered at scale and speed. EAGLE incurred no direct costs for participants and was exceptionally affordable. This global multicentre RCT was delivered at a total cost of Euros 460 000. The digital format of the intervention and global generalizability represent a substantial shift towards democratizing education and quality. The team structures used to deliver the study, and the inclusion of anaesthetists, nurses, as well as surgeons, supported its distribution and proved essential to its implementation. Challenges with cultural and language barriers were overcome, leading to the intervention being broadly





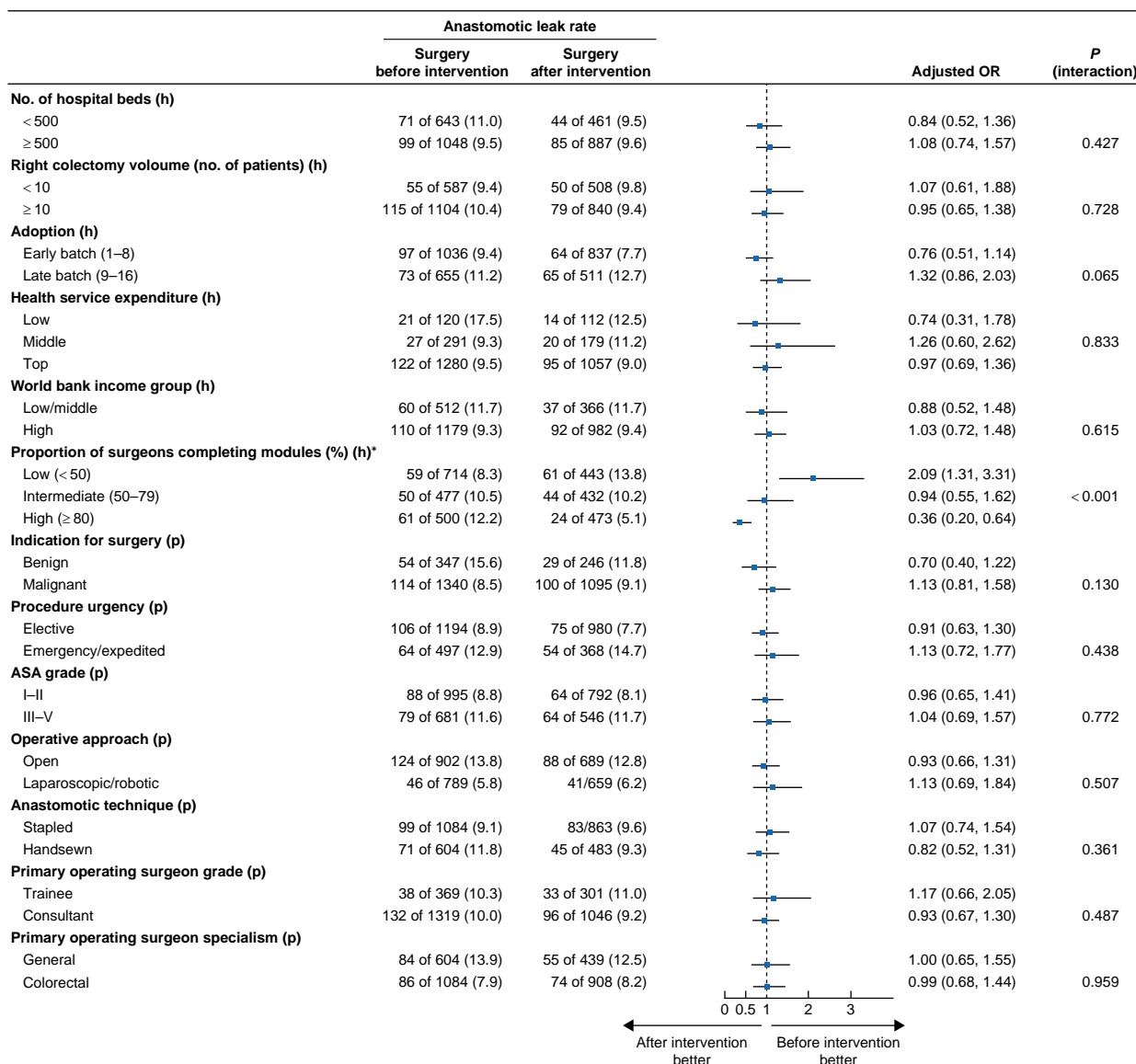
**Fig. 5** Forest plot showing effect of intervention on anastomotic leak rate

Meta-analysis was conducted across all 16 batches of the study. Each batch was analysed using a mixed-effects logistic regression model adjusting for hospital number of beds, country income, type of hospital (referral versus non-referral), sex, urgency, and data collection interval. Hospital was included as a random effect. Overall effect pooled in a random-effects meta-analysis using the inverse-variance approach of DerSimonian and Laird<sup>23</sup>. ORs are shown with 95% confidence intervals.

**Table 4** Effect of intervention on secondary outcomes, analysed across all batches

	Surgery before intervention	Surgery after intervention	OR*	P
<b>Clinical leak</b>				
No	1594 of 1689 (94.4)	1287 of 1348 (95.5)		
Yes	95 of 1689 (5.6)	61 of 1348 (4.5)	0.77 (0.53, 1.12)	0.165
Missing	17	8		
<b>Death</b>				
No	1727 of 1802 (95.8)	1375 of 1432 (96.0)		
Yes	75 of 1802 (4.2)	57 of 1432 (4.0)	1.02 (0.70, 1.49)	0.906
Missing	25	9		
<b>Reoperation</b>				
No	1668 of 1804 (92.5)	1332 of 1432 (93.0)		
Yes	136 of 1804 (7.5)	100 of 1432 (7.0)	0.94 (0.70, 1.26)	0.667
Missing	23	9		
<b>Readmission</b>				
No	1680 of 1804 (93.1)	1351 of 1432 (94.3)		
Yes	124 of 1804 (6.9)	81 of 1432 (5.7)	0.90 (0.64, 1.26)	0.523
Missing	23	9		
<b>Duration of hospital stay (days)</b>				
≤ 10	1327 of 1800 (73.7)	1044 of 1424 (73.3)		
> 10	473 of 1800 (26.3)	380 of 1424 (26.7)	1.02 (0.82, 1.27)	0.881
Missing	27	17		
<b>Unplanned admission to ICU</b>				
No	1735 of 1802 (96.3)	1381 of 1427 (96.8)		
Yes	67 of 1802 (3.7)	46 of 1427 (3.2)	0.95 (0.61, 1.46)	0.803
Missing	25	14		
<b>Stoma without primary anastomosis</b>				
No	1703 of 1824 (93.4)	1354 of 1439 (94.1)		
Yes	121 of 1824 (6.6)	85 of 1439 (5.9)	0.96 (0.67, 1.39)	0.829
Missing	3	2		
<b>Defunctioning ileostomy with primary anastomosis</b>				
No	1772 of 1821 (97.3)	1413 of 1436 (98.4)		
Yes	49 of 1821 (2.7)	23 of 1436 (1.6)	0.71 (0.34, 1.49)	0.364
Missing	6	5		
<b>Any stoma formation</b>				
No	1654 of 1827 (90.5)	1331 of 1441 (92.4)		
Yes	170 of 1827 (9.3)	108 of 1441 (7.5)	0.89 (0.62, 1.27)	0.532
Missing	3	2		

Values are n (%) unless otherwise indicated; \*values in parentheses are 95% confidence intervals. ORs and P values were estimated from a three-level mixed-effects logistic regression model adjusting for hospital number of beds, country income, type of hospital (referral versus non-referral), sex, urgency, and data collection interval. Hospital and batch were included as random effects, with hospital nested within batch. Results split by batch are available in [Tables S8 and S9](#).



**Fig. 6** Subgroup analyses of primary outcome, analysed across all batches

ORs and P values were estimated from a three-level mixed-effects logistic regression model adjusting for hospital number of beds, country income, type of hospital (referral versus non-referral), sex, urgency, and data collection interval. Hospital and batch were included as random effects, with hospital nested within batch. \*Proportion of surgeons in each hospital-team completing online modules during the implementation of the intervention. Values in parentheses are percentages unless indicated otherwise; ORs are shown with 95% confidence intervals. h, Hospital-level factors; p, patient-level factors. Results split by batch are available in Tables S10–S22.

accepted across four continents, but there was a universal need to optimize engagement across hospital teams. Implementation was greatly enabled by the batched randomization process, allowing the management team to shift focus from one batch of clusters to the next and preventing collapse of the trial during the pandemic. These methods could be used by researchers targeting improvement in other common health-related topics and should be widely available<sup>31</sup>.

EAGLE was co-developed iteratively by a multinational, multidisciplinary network that used expertise from both clinical and industry backgrounds. These factors may have helped in ensuring relevance, promoting engagement, and imparting ownership for all members of the team with a focus on optimizing patient care. Online modules can be viewed at the learner's speed to allow for differences in language fluency, time available, and surgeon experience. Upon trial completion, all

EAGLE interventions are being made globally accessible and open access (<https://eagle-escp.eu.com/>).

There were limitations to this study. The COVID-19 pandemic required it to be suspended for 5 months during the initial lockdown. The pandemic had a devastating effect on elective surgery worldwide and often halted non-COVID research activity<sup>29</sup>. The global changes in surgical patient flow created imbalances whereby some hospitals ceased elective surgery altogether and had to withdraw from the study. The flexibility of the methodology was a key element in its successful completion. However, the pandemic did have a temporal impact on the study. Reduced engagement with the intervention within hospital teams was detected in the later batches (9–16) of the study (Fig. S5). There was inconsistency in the uptake of educational modules in different hospital teams. Not all registered surgeons in each department started the modules,

and not all who started the modules completed them. There was also evidence of variation in impact across different cultures and languages. Qualitative studies will explore how this might be improved.

The EAGLE training modules can help underpin best clinical practice across the world and might help deliver additional non-technical benefits, notably in hospitals with high levels of team engagement. The intervention is generalizable and cross-cultural, and the study methodology and delivery are applicable to diverse clinical settings. The authors recommend that new surgeons and established surgical teams around the world consider undertaking the EAGLE training modules.

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## Collaborators

### Writing team (alphabetical)

Nicholas Avellaneda, Mahmoud Al Masri, Abylai Baimkhanov, Dinesh Balasubramaniam, Aneel Bhangu, Konstantinos Bouchagier, Osman Bozbiyik, Anu Carpelan, Carina Chwat, Miguel Cunha, Brett E Dawson, Maria Victoria Duque Mallen, Mark Bremholm Ellebæk, Tomás Elosua Gonzalez, Mohamedraed Elshami, Ildar Fakhradiyev, Omer Faruk Ozkan, Francesco Fleres, Kaori Futaba, Gaetano Gallo, Dhruv Ghosh, James C Glasbey, Ewen Harrison, Richard L Hooper, Ritu Jain, Sivesh Kamarajah, Sreejith Kannummal Veetil, Erdinc Kamer, James Keatley, Charles H Knowles, Mukhtar Kulimbet, Pawanindra Lal, Elizabeth Li\*, Bala Mahesh Polamreddy, Ana Minaya-Bravo, Rohin Mittal, Dion Gregory Morton\*\*, Luis Roberto Manzione Nadal, Ionut Negoï, Dmitri Nepogodiev, Omar Omar\*, Francesco Pata, Thomas Pinkney, Mark A Potter, Matteo Rottoli, Guilherme Santos, Iain Smith, Antonino Spinelli, Mary Venn\*.

\*Joint first authors, \*\*Senior author, and overall guarantor

### Data Analysis

Omar Omar, Elizabeth Li, Richard Hooper, Dion Morton.

### Ops team and meta-coordinators (alphabetical)

Edward Bywater, Brett E Dawson, Mohamedraed Elshami, Jonathan Evans, James C Glasbey, Richard L Hooper, Sivesh Kamarajah, James Keatley, Elizabeth Li, Laura Magill, Siobhan McKay, Dion Gregory Morton, Dmitri Nepogodiev, Omar Omar, Daniel Osei Bordon, Joana Simões, Mary L Venn.

### Co-ordinators (alphabetical)

Ademola Adeyeye, Nicolas Avellaneda, Nicolas Buchs, Liam Cato, Miguel Cunha, David Finch, Gaetano Gallo, Garzali Ibrahim Umar, Ana Minaya Bravo, Helio Moreira Junior, Peter Neary, Francesco Pata, Tomas Poskus, April Roslani, Elgun Samadov, Mohammed Sbaih, Mostafa Shalaby, Baljit Singh, Aad Sinha, Armen Vardanyan.

### Educational team (alphabetical)

Sanjay Chaudhry, Matteo Frasson, James C Glasbey, Elizabeth Li, Ana Minaya-Bravo, Ionut Negoï, Mark A Potter (Chair), Jim Millward, Luis Sánchez-Guillén, Niels-Derrek Schmitz, Jasper Stijns, David D E Zimmerman.

### Data Monitoring Committee (alphabetical)

David Beard, Marion Campbell, Susan Moug.

### ESCP Cohort Studies and Audits Committee (alphabetical)

Erman Aytac, Sue Blackwell, Pamela Buchwald, Sharfuddin M. Chowdhury, Dragomir Dardanov, Audrius Dulskas, Muhammed Elhadi, Alaa El-Hussuna (Chair 2021-current), Zoe Garoufalia, Muhammad Imran Aslam, Michael Kelly, Charles Knowles, Beatriz Mendes, Dion Morton, Simon Ng, Gianluca Pellino, Thomas Pinkney (Chair 2014-2021), Shaji Sebastian, Emre Sivrikoz, Antonino Spinelli, Patricia Tejedor, Carolynne Vaizey.

### ESCP Global Research Committee

Nicolas Avellaneda (Argentina), Romina Bianchi (Argentina), Pamela Buchwald (Sweden), Peter Christensen (Denmark), James Keatley (ESCP Research Manager, United Kingdom), Suk-Hwan Lee (Korea), Varut Lohsiriwat (Thailand), Surendra Kumar Mantoo (Singapore), Luqman Mazlan (Malaysia), Dion Morton (Co-Chair, United Kingdom), Simon Ng (Co-Chair, Hong Kong), Jun Won Um (Korea), Jaw Yuan Wang (Taiwan), Jun Watanabe (Japan), Hongwei Yao (China).

## Collaborators (country in alphabetical order, and hospital)

**Algeria:** A. Bengueddach, A. Tidjane, B. Tabeti, C. Behilil, N. Boudjenan-Serradj, S. Bensafir, S. E. I. Meharzi (Etablissement Hospitalier et Universitaire), A. Aissat, A. K. Ghouali, K. Larabi, O. Riffi, S. E. O. Kacimi, S. N. Mesli, W. Rezoug (University Hospital Center, Dr Tidjani Damerdji); **Argentina:** A. Mitidieri, A. José, C. Pablo, C. A. Rodriguez, M. F. Panettiere, N. Barbalace, T. Juan (Churruca-Visca), J. Lucena, A. Houdin, E. Fernandez, J. Lococo, L. Pedro, M. Loban, P. Vecchio (Grupo Galeno), A. Grinblat, A. Carrie, F. Veraciero, M. Santillan, M. Napolitano, N. Avellaneda, S. La Rosa (Hospital Universitario Cemic), C. M. Gonzalez, E. Ferro, J. P. Muñoz, T. Venterutti (Nueva Proctologia), C. Cabrera, C. Carrizo, C. Mospane, F. Leiro, J. Espino, M. Trama, R. Bianchi (Penna Hospital), A. Potolicchio, F. Dindri, L. Buey, N. Lucas, P. Catalano, R. Astilleta, Y. Quiroga (Hospital Español de Buenos Aires), C. Chwat, D. Valli, F. Alexandre, G. Martres, G. Rosato, G. Lemme, V. Romero (Hospital Universitario Austral), A. M. Doniquian, D. A. Pantoja Pachajoa, F. Llahi, J. M. Fiorenza, M. Parodi, N. Ocaña (Clinica Universitaria Reina Fabiola), A. Gallardo, A. Valenzuela, J. Perriello, R. Nador (Hospital Privado de Comunidad), C. Fermani, L. Garay, P. Blanco, S. Villalobos (Hospital Luis Lagomaggiore), F. Posner, N. Vieyra, P. Fiorito, P. Ciabattari (Sanatorio Laprida); **Aruba:** C. de Kort, D. Daryanani, J. Smit, M. P. Gosselink, N. Janssen (Dr. Horacio E Oduber Hospital); **Australia:** A. Feiss, C. H. A. Lee, D. Taylor, J. Edington, N. Chen, W. M. Ong (Bendigo Health); **Austria:** F. Aigner, G. Moitzi, G. Gemes, M. Braumille, M. Mitteregger, S. Gerald, S. Uranitsch (Barmherzige Brüder Graz), A. Belarmino, J. Waha, J. Kahn, M. Treiber, P. Schemmer, S. Mikalauska (Medical University of Graz); **Azerbaijan:** A. Ibrahimli, E. Samadov, E. Orujova, I. Namazov, J. Alikhanli, M. Asgarov (Leyla Medical Center); **Bahrain:** A. Kutkut, E. Almahmeed, H. Aljawder, I. Juma, K. Johnston, M. F. Saeed, S. Khairi (King Hamad University Hospital); **Bosnia And Herzegovina:** E. Matovic, M. Omerasevic, S. Delibegovic, S. Hodzic (University Clinical Center Tuzla);

**Brazil:** A. G. Rudell, J. Fuzari, J. F. Farah, L. R. M. Nadal, M. B. A. Dos Santos, R. A. Lupinacci, T. S. Pereira (Hospital do Servidor Público Estadual-Francisco Morato de Oliveira), A. da Silveira Sete, A. Lacerda Filho, B. L. de Souza Pires, F. Lopes de Queiroz, H. A. T. Amaral, M. A. M. Dos Santos, S. C. de Miranda Silvestre (Felício Rocho), B. Hanan, C. Reis, G. Cassia, M. da Luz, M. Delgado, R. Campanati, R. Silva (Hospital das Clínicas da Universidade Federal de Minas Gerais/Ebserh), A. Bomfa-Barbosa, B. C. Alves, B. X. M. Costa, B. G. Werneck Cortes, F. H. T. Lemos, K. Arreguy-Borges, T. Barbosa Silva (Orizonti), A. Assis, A. H. Freitas, D. Rezende, H. Silva, I. Alvarenga, R. Cascais, T. Silva (Polícia Militar do Estado de Minas Gerais, Oncomed), G. Santos, H. Pinto, I. Silva, L. Leite, M. Massahud, M. Meyer, R. Tibúrcio (Santa Casa de Misericórdia), C. A. R. Martinez, D. R. Espinha, F. A. L. Marson, I. A. de Oliveira Góes, L. M. de Souza Artioli, M. N. Boschiero (Hospital Universitário São Francisco de Assis na Providência de Deus), C. Y. Takahashi, F. G. de Oliveira, H. F. Lyra Junior, J. C. C. de Oliveira, J. M. Dos Santos, M. T. Gerber, T. R. Erdmann (Hospital Universitário Professor Polydoro Ernani de São Thiago-Hu/Ufsc/Ebserh), A. Barbosa, A. Fernandes, A. Nasser Júnior, H. Moreira Júnior, J. Moreira, M. Ribeiro, P. Moreira (Hospital das Clínicas), A. Carvalho, B. Santos, F. Fidelis, I. Cruz, L. Codes, R. Aibe, S. Boudoux (Hospital Universitário Professor Edgard Santos), B. C. Favacho, F. Pinto, G. Gava, I. R. Pivati, L. F. Vilain, N. Kim, R. Leite Nunes (Notre Dame Intermédica-Hospital Salvalus); **Bulgaria:** D. Dimitrov, K. Peneva, M. Karamanliev, S. Dimitrova, T. Ivanov (University Hospital Dr Georgi Stranski), B. Atanasov, G. Dzharov, M. Shtereva, M. Slavchev, N. Belev, P. Krastev (University Hospital Eurohospital), A. Arabadzhiyev, D. Tonova, D. Tzoneva, M. Sokolov, P. Gribnev, S. Maslyanov, V. Pavlov (University Hospital Alexandrovska); **Croatia:** B. Bakmaz, E. Dijan, I. Čoza, J. Mihanovic, J. Kučić, N. Sulen, Z. Katušić (Zadar General Hospital); **Czech Republic:** K. Hudáček, M. Farkašová, M. Marková, T. Grolich, Z. Kala (University Hospital), F. Pazdírek, J. Hoch, L. Filipová, P. Kocián, P. Příkryl (Department of Surgery, Second Faculty of Medicine, Charles University And Motol University Hospital); **Denmark:** A. Høgn, B. Møller, D. K. Slot, J. K. Bælum, K. La Cour, M. B. Ellebæk, N. D. Eriksen (Odense And Svendborg University Hospital); **Egypt:** A. Mahmoud, A. Abdellatif, A. Nafea, M. Ewedah, M. Soliman, N. Yakout, O. Argawy (Alexandria Main University Hospital), I. Sallam, I. Kamel, M. Sherif, S. Ashmawy (Misr Cancer Center), A. Ali, M. Saad, M. Ahmed, M. Mohamed, N. Mohamed, S. Mahmoud, S. Mahmoud (Assiut University Hospital), A. M. Rashed, M. A. Abd-Errazik, M. A. Ammar, R. Mohammed (Ain Shams University Specialized Hospital, Cairo), A. Ebrahim, A. A. El Aal, K. Abdou, M. Wagdy, M. Qassem, M. Taha, Y. A. El-Wafa (Ain Shams University Hospital (El-Demerdash Hospital)), A. Shehta, A. El Metwally, H. Hamed, M. A. Ali, M. Elrefai, M. El Sorogy, M. Abdelkhalek (Gastrointestinal Surgery Center), A. Ragab, B. Refky, I. H. Metwally, M. Abdelkhalek, M. Kamal, M. Zuhdy, M. Shetiwy (Oncology Center Mansoura University), A. Sakr, A. Elfallal, H. Elfeki, I. Eldakrouy, K. Elbahrawy, M. Mostafa, S. Emile (Mansoura University Hospital); **Ethiopia:** A. Tessema, A. Tasew, H. Gezahegn, K. Bekele, M. Belay, Z. Gudisa, Z. Teferu (Maddawalabu University Goba Referral Hospital); **Finland:** A. Väyrynen, A. Kechagias, A. Turunen, J. Katunin, M. Parhiala, P. Pengerma, P. Lahti (Kanta-Hame Central Hospital), A. Carpelan, H. Vihervaara, H. Huhtinen, J. Pasonen, R. Heino, S. Pakarinen, V. Falenius (Turku University Hospital); **France:** B. Pinard, C. Nobile, E. Duchalais, F. Drissi, G. Meurette, J. Podevin, M. Lepenndu (Chu Nantes); **Germany:** A. Hoetzel, A. Jud, D. Jauch, F-F. Stefan, H. Neeff, P. Holzner, P. Nguyen (University Hospital Freiburg), A. Ryl, F. Kehl, F. Pullig, J. Baral, S. Münch (Klinikum Karlsruhe), C. Fütterer, C. Reißfelder, F. Sandra-Petrescu, F. Herrle, J. Hardt, L. Lamm, S. Seyfried (Mannheim University Medical Center (Universitätsmedizin Mannheim)), A. Gharbi, F. Aydin, G. Stavrou, J. Sperber, J. de Deken, K. Schwarzkopf, R. Widyaningsih (Klinikum Saarbruecken); **Greece:** A. Polidorou, A. Mpakas, E. Tokidis, L. Loun, T. Petropoulou (Athens Euroclinic), D. Balalis, D. Korkolis, D. Manatakis, E. Assimakopoulou, M. Bourazani (Agios Savvas Anticancer Hospital), A. Gklavas, E. Kalamara, I. Papaconstantinou, K. Theodoraki, L. Chardalias, M. Konstadoulakis, T. Theodosopoulos (Aretaieion Hospital), E. Soulioti, F. Kavezou, J. Filippou, M. Papadoliopoulou, N. Michalopoulos, P. Vassiliu, T. Sidiropoulos (Attikon University General Hospital), A. Charakopoulou, A. Panagiotou, K. Bouchagier, K. Kokkinakis (Evangelismos General Hospital), E. Lambridi, M. Psarologos, M. Sotiropoulou, S. Giannaraki, S. Kapisiris (Evangelismos General Hospital), A. Triantafyllou, C. Theodoropoulos, G. Matthaïou, N. Westzaan, S. Etelka, T. Triantafyllou, G. Theodoropoulos (Hippocratio General Hospital), D. Schizas, E. Mpaili, L. Karydakis, M. Mpoura (Laiko University Hospital), D. Danassi, D. Schizas, I. Karavokyros, L. Karydakis, M. Mpoura, N. Dimitriou, X. Livanou (Laiko University Hospital), A. Kikira, G. Tsourouflis, N-K. Tomara, N. Nikiteas, P. Dorovinis, Z. Garoufalia (Laiko University Hospital), G. Tsiotos, K. Stamou, N. Kocka, N. Ballian, S. Kalakonas (Mitera Hospital), E. Athanasakis, E. Chrysos, E. Xynos, J. Tsiaoussis, K. Papadaki, S. Xenaki, V. Nyktari (University Hospital of Heraklion Crete And Interclinic Hospital of Crete), C. Sarakatsianou, E. Bompou, E. Arnaoutoglou, G. Tzovaras, I. Baloyiannis, I. Mamaloudis, K. Perivoliotis (General University Hospital of Larissa), E. Apostolidi, F. Mulita, G. Karpetas, I. Maroulis, M. Vailas (General University Hospital of Patras), A. Petra, E. Kontis, E. Kaouras, I. Katsaros, L. Katsiaras, P. Manikis, T. Papadopoulou (Metaxa Cancer Hospital), A. Papadopoulos, E. Manioti, G. Zeringa, I. Katsinelis, O. Mouzakis, P. Kouki, V. Nikolaou (General Hospital of Nikaia), A. Marinis, K. Alexopoulou, M. Papadaki, S-A. Vederaki (Tzaneio General Hospital), A. Samara, D. Giakoustidis, G. Christodoulidis, M. Bareka, P. Chatzikomnitsa, S. Tsigara, V. Papadopoulos (Papageorgiou General Hospital), E. Zarzava, E. Anestiadou, L. Lydia, M. Apostolakidou, O. Ioannidis, S. Simeonidis, S. Mpitsianis (George Papanikolaou General Hospital of Thessaloniki); **Hong Kong Sar, China:** K. Futaba, P. T. H. Tam, S. S. M. Ng, V. N. M. Lau, W. K. Tse (Prince of Wales Hospital); **Hungary:** B. Bánky, E. Dolhai, É. Horváth, J. Golub, J. Marton, L. Lakatos, N. Suszták (Szent Borbála Kórház); **India:** A. P. Maurya, H. Kumawat, P. Saxena, R. Lather, V. Waindeskar (All India Institute of Medical Science Bhopal), A. Khanduri, L. Goyal, R. Gupta, S. Singh (Synergy Institute of Medical Sciences), A. R. Saksena, B. K. Rayani, J. Kasula, K. Raju, M. M. Shah, S. R. Thammineedi, S. C. Patnaik (Basavatarakam Indo American Cancer Hospital & Research Institute), D. Rathod, D. R. Poonia, J. R. Vishnoi, N. Sharma, R. Byshtetty, R. K. Yadav, S. Misra (All India Institute of Medical Sciences (Aims), Jodhpur), A. Dhali, G. K. Dhali, J. Biswas, S. Ray, T. Ghose (Institute of Postgraduate Medical Education And Research), A. Pipara, H. M. Singh, M. K. Roy, N. Desai, R. Thambudorai, V. Ishwarappagol (Tata Medical Center), A. Chaturvedi, D. Verma, N. Akhtar, S. Gupta, S. Rajan, T. Tiwari, V. Kumar (King George's Medical University), A. Mahajan, D. Jain, D. Liddle, J. Jyoti, P. D. Haque, R. Jain, S. K. Veetil, W. Prem (Christian Medical College & Hospital), B. Lakshminarayana, B. Veerabhadrapa S, P. H. Poojary, R. R. Krishna Bhat, S. Mathew (Kasturba Medical College Hospital, Manipal), L. Gupta, L.h Bains, L. Bains, M. Kumar, N. Singh, P. Lal (Maulana Azad Medical College), A. Sharma, A. Kumar, L. Garg, M. Aggarwal, R. Jakhar (Government Medical College



Patiala), A. Kumar, B. Bose, F. Huda, N. Thakur, P. Dhar, R. Kottayasamy Seenivasagam, S. Prakash (All India Institute of Medical Sciences), A. Chaudhari, A. Shukla, C. Mahakalkar, G. Saxena, K. Mahuli, M. Kaple, S. Kshirsagar (Acharya Vinoba Bhave Rural Hospital), A. Mehraj, F. Parray, G. Aziz, N. Chowdri, R.f Wani, S. Gurcoo, Z. Shah (Sher-I-Kashmir Institute of Medical Sciences), B. M. Polamreddy, S. Madhavan, S. Subbarayan, S. Vino, U. Aravindan (Thanjavur Medical College), G. George, C. K. Harikumar, I. P. Yadev, M. Chisthi, P. S. Revathy, R. C. Sreekumar, V. Vijayan (Government Medical College Thiruvananthapuram), B. Theodore, C. Victor, M. R. Jesudason, N. Arulappan, R. Philip, R. Mittal, S. Durai (Christian Medical College & Hospital); **Ireland:** B. Creavin, C. Cullinane, C. Davis, C. Flynn, D. Liam, F. Cooke, H. Earley, P. McCullough, P. Neary, V. Hamilton (University Hospital Waterford/University College Cork); **Italy:** A. Di Trani, I. Conversano, P. Cianci, R. Petta (Lorenzo Bonomo), A. Picciariello, D. F. Altomare, G. Martines, S. Grasso, S. Moffa, V. Papagni (University of Bari 'Aldo Moro'), A. Zanoni, A. Belvedere, A. Gori, A. Romano, D. Cuicchi, G. Di Croce, M. Tanzanu, M. Rottoli, S. Cardelli (Ircs Azienda Ospedaliero-Universitaria Di Bologna), A. Zanoni, A. Lauro, G. Di Croce, L. Sartarelli, M. Binetti, M. Cervellera, V. Tonini (Ircs Azienda Ospedaliero-Universitaria Di Bologna), B. Sanna, E. Pusceddu, M. Runfola, R. Manunza (Azienda Ospedaliera G. Brotzu), A. Pisanu, A. Cois, F. Frongia, G. Esposito, M. Podda, P. Mura (Cagliari University Hospital), A. Marzella, B. Scotto, D. Cuneo, F. Ascari, G. Giulino, M. Varoli, N. Laquatra (Bernardino Ramazzini), C. Fortunato, F. Marino, F. Perrone, M. Pace, O. Convertini (Ircs 'Saverio de Bellis'), C. Cutellè, G. Ammerata, G. Sena, M. Pignataro, M. Scozzafava, M. Mazza (Azienda Ospedaliera Pugliese-Ciacchio Di Catanzaro), A. Bruni, G. Ammerata, G. Curro, J. Hila, M. Ammendola, S. Curcio, V. Signorelli (University 'Magna Graecia' of Catanzaro), A. Marano, D. Sasia, F. Borghi, M. Carmela Giuffrida, M. Bonardello, S. Palmisano, V. Testa (Santa Croce E Carle Hospital, Cuneo), A. Porta, F. Scaltrini, G. Jacob, I. Gaspa, L. Guatteri, M. Coladonato (Ospedale Sacra Famiglia), A. Zapparoli, A. Pesce, C. V. Feo, F. Pindozi, I. Bigoni, M. Torchiano, N. Fabbri (Ospedale Del Delta, Azienda Unità Sanitaria Locale Di Ferrara, University of Ferrara), F. Cianchi, F. Staderini, F. Elena, F. Coratti, G. Barbato, L. Fortuna, S. Romagnoli (Azienda Ospedaliera Universitaria Careggi), C. Bergamini, G. Villa, I. Cecchini, J. Martellucci, M. Izzo, M. Trafeli, M. Scheiterle (Azienda Ospedaliera Universitaria Careggi), F. Manoocheehri, F. Cagnazzo, M. G. Spampinato, N. Adam, N. Depalma, S. D'Ugo, S. Garritano (P.O.'Vito Fazzi'), C. Mazzeo, E. Cucinotta, F. Melita, F. Viscosi, F. Fleres, S. A. Biondo, V. F. Tripodi (Policlinico Universitario G. Martino of Messina), A. Spinelli, C. Foppa, M. Greco, M. Sacchi, M. Carvello, V. Deac, V. Laurenti (Humanitas Research Hospital), A. Frontali, A. Zappone, F. Cammarata, F. Colombo, L. Ferrario, M. C. Currà, P. Danelli (Ospedale Luigi Sacco Milano), A. Vignali, C. Umberto, F. Mastriale, R. Riccardo, S. Turi, U. Elmore (San Raffaele Scientific Institute, Milan), A. Galimberti, A. Pisani Ceretti, D. Vespo, E. Opocher, M. Longhi, N. M. Mariani (Santi Paolo E Carlo), C. Flandoli, C. Ciulli, G. Vaira, L. Ripamonti, L. Cigagna, M. Oldani, N. Tamini (Ospedale San Gerardo), A. Palumbo, A. Castaldi, C. Antropoli, N. Rupealta, N. Palmiero, V. Donatiello (Azienda Ospedaliera Di Rilievo Nazionale Antonio Cardarelli), A. Novi, A. Cappiello, F. Bianco, M. de Luca, P. Incollingo, R. Esposito, S. Gili (Ospedale S. Leonardo-Asl Napoli 3 Sud, Castellammare Di Stabia), G. Aprea, G. de Simone, G. Palomba, M. Capuano, R. Basile, S. D'Angelo (Federico II University of Naples), F. P. Tropeano, G. Luglio, G. Pagano, G. D. de Palma, G. de Simone, M. Cricri, S. D'Angelo (Federico II University of Naples), G. D. de Palma, G. de Simone, M. Milone, M.e Manigrasso, P. Anoldo, S. D'Angelo (Federico II University of Naples), G. Boccia, G. Raiano, M. M. Di Nuzzo, M. Calabria, R. Peltrini, S. Castiglioni, U. Bracale (Federico II University of Naples), C. Cervone, D. Rega, D. Caliendo, E. Benetti, P. Delrio, R. Guarino, S. de Franciscis (Istituto Nazionale Tumori Fondazione, Pascale-I.R.C.C.S.), F. Coppolino, F. M. Romano, F. Selvaggi, G. Pellino, G. Sciaudone, L. Selvaggi, M. de Stefano (Università Della Campania 'Luigi Vanvitelli', Naples), A. Resendiz, F. Carosso, M. Degiuli, P. Salusso, R. Reddavid, S. Benetti (San Luigi Gonzaga Hospital), C. Callari, D. Di Miceli, D. Pardo, G. Sabatino, L. Licari, M. C. Liroso, V. Sorce (Fbf Buccheri La Ferla Palermo), A. Peri, A. Mori, L. Pugliese, M. Filardo, P. Nuccio, T. Dominioni (Policlinico San Matteo), Ciciliot M., F. Giuseppina, F. Angelo, G. Raffaele, M. Michele, P. Roberto (Ospedale Santa Corona, Pietra Ligure (Sv)), A. Mondini, A. Muratore, C. Valentino, M. Calabrò, R. Danna (Edoardo Agnelli), C. Cremonini, C. Sbarbaro, D. Tartaglia, F. Coccolini, M. Chiarugi, S. Musetti, S. Pagani (Azienda Ospedaliero Universitaria Pisana), C. M. Neri, C. Leoni, E. Rossi, E. Randisi, M. Puccini, P. Bucciante, R. Balestri (Azienda Ospedaliero Universitaria Pisana), A.o Vasale, G. Crescentini, G. Ranieri, G.i Sinibaldi (Fatebenefratelli Isola Tiberina), A. Biondi, A. P. Sanesi, C. Vacca, L. Lorenzon, L. Sollazzi, R. Persiani, R. Pezzuto (Fondazione Policlinico Universitario Agostino Gemelli), A. F. Ferroni, F. Stipa, L. Volturo, M. Moretti, S. M. Tierno, S. Marina (Madre Giuseppina Vannini), A. Mingoli, E. Fiori, F. Pugliese, G. Brachini, G. Carta, P. Sapienza, P. Lapolla (Policlinico Umberto I), E. Mazzotta, F. Carannante, G. T. Capolupo, G. Masciana, I. Sica, M. Caricato, S. Antonelli (Policlinico Universitario Campus Bio Medico of Rome), D. Fiume, D. Spoletini, G. Lisi, M. Carlini, V. Menduni (Sant'Eugenio Hospital), A. Arturi, B. Proietto, C. de Lucia, C. Baldi, D. G. Passantino, G. Romeo, M. Di Paola (San Pietro Fatebenefratelli), B. Sensi, G. Sica, L. Siragusa, M. Dauri, M. Franceschill, M. Campanelli, V. Bellato (Policlinico Tor Vergata Hospital, Rome), A. Porcu, A. G. Giuseppina Garau, M. L. Masala, S. Oggianu, T. Tedde, T. Perra, V. Pazzola (Cliniche San Pietro, A.O.U. Sassari), D. F. Venezia, F. Roviello, G. Giacomini, L. Carbone, L. Marano, L. Verre, M. Marano (Azienda Ospedaliero Universitaria Senese), A. A. Ciccarese, F. Bagnardi, J. Shahu, V. Tonini, W. Siciliano (Santissima Annunziata), A. Iacomino, C. Armellin, F. Marson, G. Zanus, G. A. Santoro, M. de Paoli, U. Grossi (Ca' Foncello); **Japan:** M. Sekimoto, T. Kobayashi (Kansai Medical University), A. Horiguchi, D. Koike, H. Yonekura, H. Kato, K. Yoshino, S. Arakawa, Y. Asano (Fujita Health University Bantane Hospital); **Jordan:** F. Alawneh, I. Rajab, M. Ramadan, M. Al Masri, M. Al Qaisi, M. Alsamneh, O. Mubaidin (King Hussein Cancer Center), A. Al-Jarrah, H. Ababneh, K. Khadair, T. Tawarh, W. Sabri, Z. A. Arida (Prince Hamza Hospital); **Kazakhstan:** B. Dzhumabekov, D. Klyshbayeva, I. Fakhradiyev, I. Fakhradiyev, K. Ispbayeva, M. Nadyrov, S. Tanabayeva, T. Saliev (Karasay Central District Hospital), A. Jumanov, A. Aitbaeva, A. Arynov, M. Nadyrov, M. Kulimbet, N. Maulenov, Z. Dushimova (Kazakh Institute of Oncology And Radiology); **Korea, Rep:** E. Jung Park, H. Kim, J. Kang, S. H. Baik, Y. Song (Gangnam Severance Hospital, Yonsei University College of Medicine), H. D. Kwak, J. Kim, J. H. Son, J. S. Chung (Chonnam National University Hospital), H. Lee, J.-H. Baek, K. H. Nam, K. C. L., W.-S. Lee, Y. Jeon (Gil Medical Center), C. W. Kim, S.-E. Park, S.-H. Lee (Kyung Hee University Hospital At Gangdong), G. M. Son, H. M. Ahn, I. Y. Lee, T. K. Kim (Pusan National University Yangsan Hospital); **Latvia:** A. Pcolkins, A. Sivins, D. Lobovs, G. Ancans, I. Kreice, I. Budnikova, I. Jelovskis (Riga East Cuh Latvia Oncology Center); **Libya:** A. Emhareb, A. Abaidalla, A. Hammed, M. Khairallah, S. S. Jibreel, W. A. S. Mohmmad, W. Hasan (Albayda Medical Center); **Lithuania:** D. Venskutonis, E. Daugėla, E. Dainius, J. Juočas, J. Kutkevičius, K. Vaitkutė, S. Bradulskis (Lithuanian University of



Health Sciences Kaunas Clinical Hospital), A. Macas, H. Pauzas, M. Jokubauskas, P. Lizdenis, R. Budrikenė, S. Svagzdys, Z. Saladziuskas (Lithuanian University of Health Sciences Kaunas Clinics), A. Dulskas, G. Volkoviene, J. Makauskienė, J. Kuliavas, V. Bernotaite (National Cancer Institute), D. Danys, E. Kontrimaviciute, E. Poskus, M. Kryzauskas, M. Jakubauskas, T. Poskus, V. Jotautas (Vilnius University Hospital); **Malaysia:** A. D. Zakaria, M. W. P. Kai, R. H. M. Zainy, S. S. Tan, W. Z. W. Zain, Z. Zakaria, Z. M. Yusoff (Hospital Universiti Sains Malaysia), F. Raduan, I. Sagap, N. Y. Fazlina Razali, N. Abdul Rahman, S. B. Chandra Kanthan, Y. Chih Nie, Z. A. M. Azman (University Kebangsaan Malaysia Medical Centre), C. W. Ang, C. Y. Tang, K. S. Poh, K. Ng, N. M. Hashim, R. Xavier, T. L. Khong (University Malaya Medical Centre), A. S. Amir, C. S. Che Ali, C. K. Yew, F. Henry, J. Muniandy, S. Baharam, S. C. Ren Eng (Selangor Hospital), F. Su-Min Hu, H. Amin-Tai, M. F. Jabar, M. H. S. Abdul Satar, N. Q. Fathi, P. S. Rao (Hospital Pengajar Universiti Putra Malaysia (Hpupm)); **Malta:** A. M. Camilleri Podesta, C. Cini, J. Psaila, M. Debono, M. Sammut, N. Spiteri, P. Andrejevic (Mater Dei Hospital); **Mexico:** A. Galaviz Muro, B. Perez Leon, J. A. Valenzuela Perez, J. R. Acosta Quiñonez, M. T. Ramirez Vera, V. M. Maciel Gutierrez (Hospital Civil Dr. Juan I Menchaca), B. Jimenez Bobadilla, C. M. Martinez, J. L. de León Rendón, J. A. Villanueva Hererro, M. V. Y. Cuichan, N. Fernández Baez, P. G. Prevost (Hospital General de México 'Dr Eduardo Liceaga'), A. Hoyos-Torres, E. A. Ruiz-Muñoz, F. E. Alvarez-Bautista, G. Ortíz-Méndez, J. L. Zamudio-Bautista, N. Salgado-Nesme, O. Vergara-Fernández (Instituto Nacional de Ciencias Médicas Y Nutrición 'Salvador Zubirán'), E. Sanchez-Valdivieso, G. Martinez, J. Navarrete-Aleman, J. Navarrete-Aleman, J. Perez-Arellano, R. Maldonado-Barron, R. Hernandez-Krauss (Hospital de Alta Especialidad de Veracruz), A. Ousadden, E. B. Benjelloun, I. Bzikha, K. Ibnmajdoub, K. Mazaz, O. Marghich, S. Touzani (University Hospital Hassan II), A. Benkabbou, A. Souadka, B. E. Ahmadi, F. Zaari, I. Laamri, M. A. Majbar, R. Mohsine (Institut National D'Oncologie); **Netherlands:** C. Berlo Van, D. Seuren, F. Gillissen, F. Aarts, J. Konsten, M. Heinsbergen Van, S. Schlooz (Viecuri Medisch Centrum); **New Zealand:** A. Naiqiso, A. Merrie, C. Varghese, F. Pugh, I. Bissett, K. Wilson, S. Mitchell (Auckland City Hospital), C. Dawson, C. Hill, D. Wright, S. Hubley (Dunedin Public Hospital), A. Lin, D. Jones, L. Siggins, P. Fagan, S. Wu (Wellington Regional Hospital); **Nigeria:** A. Ekwesiana, C. Nwoye, D. Agara, G. Ejiofor, N. Sundaynweke, N. Kwentoh, R. Ewah (Alex-Ekwueme Federal University Teaching Hospital Abakaliki Nigeria), A. Aremu, A. Ballah, A. Ningi, E. Ohia, I. Mienda, M. Aliyu, N. Oloko (Abubakar Tafawa Balewa University Teaching Hospital Bauchi), A. Okunlola, H. Abiyere, O. E. Bolanle, O. Babatunde, S. Fatudimu, T. Orewole (Federal Teaching Hospital, Ido Ekiti), E. Louis Mba, M. S. Felix, M. Abdulfatah, N. Ibrahim, U. Masoro (Federal Teaching Hospital, Gombe), A. Fakoya, D. Irabor, F. Anyadike, I. Ulasi, M. Orji, O. Afuwape, O. Ayandipo (University College Hospital), A. Adeyeye, I. Aremu, K. Adegboye, P. Gbadegesin (University of Ilorin Teaching Hospital), B. Aminu, I. K. Aghadi, J. G. Makama, S. Joshua, S. A. Kache (Barau Dikko Teaching Hospital), A. A. Sheshe, A. M. Bala, I. U. Garzali, M. M. M. Abdullahi, N. Muhammad, N. A. Umar, S. Muhammad (Aminu Kano Teaching Hospital), A. Okoye, C. Emegoakor, C. Nwosu, O. A. Egwuonwu, O. Ekwunife, R. Amadigwe, S. Ojiakor (Nnamdi Azikiwe University Teaching Hospital), A. Akere, C. Okereke, O. Olaleye, O. Ige, O. Olubayo, T. Olatiilu (Federal Medical Centre), M. Idris, A. Shehu, D. Oluyori, E. Nwabuoku, L. Ukwubile, M. Bashir, M. Daniyan (Ahmadu Bello University Teaching Hospital); **Pakistan:** H. Rauf, M. Ali, M. Zakaria, M. Hameed, N. Amanullah, T. Chawla, U. Waqar (Aga Khan University), A. Jamal, A. Butt, A. Kerawala, S. Samson (Cancer Foundation Hospital), A. A. Dodhy, A. J. Gill, A. Malik, D. Ali, H. Mohsin, M. F. Afzal, Sana Batool (Lahore General Hospital, Pgrmi, Amc), A. Suleman, J. Khalid, M. Zafar, M. Dilawar, M. H. Janjua, M. Z. Sarwar, S. A. Naqi (King Edward Medical University, Mayo Hospital, Lahore), A. Akbar, A. Afzal, J. Anwar, K. M. Gondal, M. Shaukat, M. Waheed, M. Sohail (King Edward Medical University, Mayo Hospital, Lahore), N. Mukhtiar, A. S. Ammar, A. Hussain, A. N. Khan, A. Sarwar, I. Khan, S. Khattak (Bahria International Hospital, Bahria Orchard); **Palestine:** B. Oqaili, B. Saada, D. Zadari, F. Jubran, F. Hamdan, M. Emar, R. Alawi (Al-Ahli Hospital); **Panama:** G. Jacome, I. Insturain, J. Arauz, M. Hurtado, R. I. Beron, R. Castaño, V. H. Bruno Cao (Hospital Regional Rafael Hernandez Csa); **Poland:** A. Kwiatkowski, G. Dobkowski, K. Bartosiak, M. Wałędziak, M. Mozański, M. Zadrozna, P. Kowalewski (Military Institute of Medicine), A. Fiedziuk, A. Wątroba, A. Los, M. Sitarska, M. Rząca, M. Zawadzki, R. Czarnecki (Wroclaw Regional Hospital); **Portugal:** C. Tavares, F. Santos, F. Borges, J. Corte Real, M. J. Lima, S. Carlos, V. Pereira (Hospital Garcia de Orta), A. Pacheco, G. Gomes, J. Pimenta, L. Elisario, M. Jervis, V. Gonçalves, V. Pedro (Unidade Local de Saude do Baixo Alentejo), D. Jordão, F. Reis Neves, L. Conceição, M. D. Ângelo, P. Marques, R. Prata Saraiva, T. Vieira Carçoço (Instituto Português de Oncologia de Coimbra Francisco Gentil), A. Windels, A. Machado, C. Ribeiro, C. Velez, J. Oliveira, M. R. Melo, R. Lima (Hospital do Espirito Santo), A. Pires, C. Lareiro, R. Martins, T. Revez (Hospital Faro, Centro Hospitalar Universitario do Algarve), A. Martins, D. Cardoso, I. Alegre, P. Estevão (Hospital Sao Francisco Xavier), D. Andrade, D. C. Gomes, M. J. Duarte, P. Custódio, R. A. Nemesio, S. Rodrigues (Centro Hospitalar Universitário Lisboa Central), A. Correia, H. Domingos, I. Herrando, J. Azevedo, L. Fernandez, P. Azevedo, P. Vieira (Champalimaud Foundation), A. Torre, A. Amado, M. Paiva, R. Saraiva, S. Costa, T. Mendes, T. Queirós (Centro Hospitalar Vila Nova de Gaia/Espinho), A. Silva, A. Faustino, A. Freitas, J. Mendes, L. Amaral, R. Quintanilha, R. Silva (Hospital do Divino Espírito Santo), A. M. Coelho, A. Ribeiro, C. Pinto, N. Ribeiro, R. Reis, S. Costa, V. Fernandes (Centro Hospitalar do Tamega E Sousa), A. Sanches, E. Amorim, I. Miguel, J. Rachadell, Madalena Sanches, S. Oliveira, V. Baptista (Centro Hospitalar Universitario do Algarve-Unidade de Portimão), A. Roxo, H. Devesa, O. Teslyak, R. Barradas, S. Marques, S. Martins, S. Pepino (Hospital de Santarem), A. Silva, A. C. Deus, A. Ferreira, D. Marinho, D. Sousa, M. J. Martins, N. Matias (Unidade Local de Saúde do Litoral Alentejano), A. Pinto, D. Correia, F. Amado, L. Cordeiro, M. Morales, M. Lamas, S. Marçal (Centro Hospitalar de Setúbal), A. I. Rodrigues, A. Santos, A. Marçal, A. Oliveira, C. Gomes, C. Ferreira, R. Marques (Centro Hospitalar de Trás-Os-Montes E Alto Douro, E.P.E.); **Romania:** A. Chitul, C. Alexandrescu, C. Bezedo, D. Cristian, D. Mandi, F. Grama, R. Ungureanu (Coltea Clinical Hospital), B. Stoica, C. Diaconescu, C. Ciubotaru, I. Tanase, I. Grintescu, I. Negoii, V. M. Negoita (Emergency Clinical Hospital Bucharest), C. Calin, C. Simeanu, D. C. Ciotarla, M. Caltea, R. M. Mirica (Saint John Emergency Hospital), A. Luca, A. Pasca, C. Vlad, E-A. Bonci, I. Stefanescu, P. A. Achimas-Cadariu, V. A. Gata (Prof. Dr. Ion Chiricuta' Institute of Oncology), A. Capusan, C. Petrisor, G. Dindelegan, R. Seicean, R. Scurtu, V. Bintintan, V. Fagarasan (Clinica Chirurgie I, Spitalul Clinic Judetean de Urgenta), C. Ionescu, D. Crisan, L. Zanc, M-V. Ene-Cocis, M-S. Muresan, S. M. Mihalcea, V. Dudric (Cluj-Napoca Municipal Hospital), A-M. Musina, A-I. Ristescu, C-E. Roata, M. Moglan, M-G. Dimofte, S. Lunca, S. Iacob (Regional Institute of Oncology Iasi); **Russian Federation:** A. Dychko, A. Litvin, A. Kapustina, A. Provozina, E. Anokhin, M. Zabiya (Immanuel Kant Baltic

Federal University, Regional Clinical Hospital), A. Shin, K. Djumabayev, M. Kuznetsova, S. Gordeyev, S. Kochkina, Z. Mamedli (N.N.Blokhin Russian Cancer Research Center), D. Markaryan, E. Galliamov, E. Semina, M. Agapov, P. Malahov, T. Garmanova, V. Kakotkin (Moscow Research And Educational Center, Lomonosov Moscow State University), A. Zaycev, A. Sumbaev, A. Bedzhanyan, E. Orman, K. Petrenko, M. Bredikhin, Y. Frolova (Petrovsky National Research Centre of Surgery), I. Tulina, O. Bashilkina, P. Tsarkov, S. Rodimov, V. Stamov, V. Balaban (Clinic of Coloproctology And Minimally Invasive Surgery, Sechenov Medical State University), A. Alexnder, A. Yanishev, D. Rogozhev, N. Yakunina, N. Chubukova, R. Nugmanov (Privolzhsky Research Medical University, Nizhny Novgorod Regional Clinical Hospital), A. Karachun, A. Petrov, A. Domanskiy, L. Panaiotti, M. Smolina, T. Saponova, Y. Pelipas (N.N. Petrov National Medical Research Center of Oncology), E. Zagaynov, G. Khrykov, L. Davidovskaja, N. Burlov, N. Mankevich, T. Tverdohlebova (Leningrad Regional Cliniiical Oncology Dispencary), A. Bogatikov, A. V. Lodygin, C. Krasnoselsky, E-S. Vasiukova, N. R. Kopteyev, T. Ovchinnikov, V. A. Kashchenko (Sokolov's North-Western District Scientific And Clinical Center of The Federal Medical And Biological Agency), A. Novikova, E. Terentyeva, O. Kuleshov, R. Pavlov (Saint Petersburg State University Hospital), A. Koshel, D. Kostromitsky, E. Drozdov, S. Klokov (Siberian State Medical University); **Saudi Arabia:** A. Camacho, F. N. Khan, M. Al Bandar, R. Shamim, S. Chowdhury (King Saud Medical City); **Serbia:** B. Kovacevic, I. Krdzic, M. Zdravkovic, M. Kenic, M. Milentijevic, N. Petkovic, R. Radulovic (Zvezdara University Medical Center); **Singapore:** J. Ngu, N. Z. Teo, P. A. Singh, S. Y. Ong, S. Li (Changi General Hospital), B. en Siew, C. Chee, J. J. M. Koh, K. Y. Lee, K. K. Tan, S. C. Wong, W. Loh (National University Hospital); **Spain:** A. F. Pujol, J. C. Rubio, L. P. Farrés, L. L. Vendrell, M. I. Ureña Del Olmo (Hospital Germans Trias I Pujol), A. B. Pedregosa, C. Galmes, D. Luckute, D. Casanova, M. Artigot, X. Guedes, Y. Olivella (Hospital de Igualada), M. S. Sarda, M. J. Toscano, M. P. Damieta, M. Pera, S. A. Gonçalves, S. T. Galvez, S. S. Ruiz (Hospital Del Mar), E. Espin-Basany, F. Marinello, L. Villarino-Villa, M. Vega-Las Heras, R. Martin-Sanchez, R. Mata Mata, R. Blanco-Colino (Vall D'Hebron University Hospital), A. Otero, A. Maria de Lacy, J. M. Sanahuja, R. Bravo, T. Ferraz, V. Gonzabay (Hospital Clinic Barcelona), F. Gonzalez, P. Menendez, V. C. Garcia Del Castillo, V. M. Lopez-Pelaez (Hospital General de Valdepeñas), Á. S. Silva, C. Lillo-García, E. M. Tauler, L. Sánchez-Guillén, M. C. Estañ Manresa, S. L. Pérez, S. Q. Llopis (Hospital General Universitario de Elche), A. V. Rubio, E. R. Castillo, F. J. Jiménez Miramón, J. L. Ramos Rodriguez, L. A. Rizo-Lamberti, P. G. Garrido, V. J. Carneros (Getafe University Hospital), B. A. Alfonso, B. G. Sierra, C. G. Amador, M. D. Picardo Gomendio, M. V. Romo Palomino, R. de La Plaza Llamas (Hospital Universitario de Guadalajara), E. G. Cafranga, J. L. Esteban Ramos, M. C. Estudillo, R. E. Pérez, R. M. Pernas, S. de Lebrusant Fernández, W. M. Sánchez Bautista (Hospital Universitario Jerez de La Frontera), A. L. Llamazares, A. L. Valbuena, L. A. Moran, L. J. Alvarez, L. G. Raposo, S. F. Ceron, T. E. Gonzalez (Complejo Asistencial Universitario de Leon), A. C. Calvo, C. R. Valcárcel, J. Perez Peña, L. M. Jimenez Gómez, M. C. Díez, P. D. Lindenbaum, S. K. Mata (Hospital General Universitario Gregorio Marañón), A. Ruiz-De-La-Hermosa, A. Abad-Motos, B. Toribio-Combarro, J. Ripollés-Melchor, M. L. Fuenmayor-Valera, P. Ortega-Domene (Infanta Leonor University Hospital), A. Loscos, C. S. Del Pueblo, J. Dziakova, J. M. Mugüerza, P. S. Carlin, R. Anula, Y. Mouvet (Hospital Clinico San Carlos), A. Forero-Torres, B. D. San Andrés, C. M. Marcos, I. Rubio, I. Pascual, J. Yague, N. G. Alcolea (Hospital Universitario La Paz), A. Alonso, B. Diéguez, I. Ibañez, J. L. Pérez, M. Losada, M. García-Conde, M. Hernández (Hospital Universitario Del Sureste), A. Blazquez-Martin, C. Vera-Mansilla, F. Mendoza-Moreno, J. Hernandez-Salvan, M. Diez-Alonso, P. Hernandez-Juara, S. Barrera-Blazquez (Hospital Universitario Principe de Asturias), A. M. Minaya-Bravo, A. Galván-Pérez, C. S. Miguel-Méndez, E. Gonzalez-Gonzalez, M. Alvarez-Díez, M. Á. García-Ureña, M. Llorente-Moreno (Hospital Del Henares), C. Ruiz-Lozano, E. Colás-Ruiz, J. Pérez-Calvo, J. A. Gomila-Sanso, L. Álvarez-Llano, S. C. Serrano-Fuentes (Hospital de Manacor), C. Soto-Montesinos, I. Dedeu-Bastardas, I. Perez-Reche, M. Labró-Ciurans, S. Pardo-López (Fundació Althaia-Xarxa Assistencial Universitària de Manresa), E. G. Pérez, I. O. Fernández, L. O. Canals, P. C. Espino, P. G. Ruano, V. Ricardo (Hospital Universitario de Móstoles), E. P. Ros, E. M. Manuel, J. A. Benavides Buleje, M. M. Carrasco Prats, P. A. Parra Baños, P. M. González, R. G. Celdrán (Hospital General Reina Sofía), E. M. Pellicer-Franco, G. Valero-Navarro, J. P. Vicente-Villena, M. M. Martinez-Mercader, M. Baeza-Murcia, M. Mengual-Ballester, V. Soria-Aledo (Morales Meseguer University Hospital), D. Fernández-Martínez, L. Varela-Rodríguez, L. J. Garcia-Flórez, M. Fernández-Hevia, M. J. Gonzalez-Diaz, S. Fernández-Arias (Hospital Universitario Central de Asturias (Huca)), C. P. Puertas, E. de San Pío Carvajal, E. S. Cebolla, E. Brainsa, J. M. Muros Bayo, M. C. Castro, R. R. Blanco (Hospital Universitario Infanta Cristina), E. Gutierrez, F. L. Pinto, J. M. Alegre, N. Flores, S. N. O'Sullivan (Infanta Sofía University Hospital), B. F. Fernández, J. E. Alonso, J. G. Ais Conde, N. M. Roper, R. Á. Bayón, S. H. Dominguez, S. Ramirez (Hospital General de Segovia), A. M. de Pablos, A. Perez-Sanchez, A. Cano-Matias, F-J. Del-Rio-Lafuente, J. Caballero-Delgado, J. Valdes-Hernandez, J-C. Gomez-Rosado (Hospital Universitario Virgen Macarena), C. Martinez, H. Cholewa, J. Sancho-Muriel, M. J. Alberola, M. Navasquillo, V. Primo, V. Moreno (Hospital Universitario Y Politécnico La Fe), A. Espí-Macías, D. Moro-Valdezate, I. Carrascosa-Morales, J. Martín-Arévalo, M. Soro-Domingo, S. García-Botello, V. Pla-Marti (Hospital Clínico Universitario de Valencia), A. M. Abellán, C. M. Pérez, C. M. Pérez, G. F. Valderas Cortés, L. F. Blasco, M. R. Chornet, R. S. Martín (Consortio Hospital General Universitario), A. Romero-De Diego, A. Vázquez-Fernández, A. Pascual, B. de Andrés-Asenjo, J. B. de Heredia, M. Ruiz-Soriano, R. Rodríguez-Jiménez (Hospital Clínico Universitario de Valladolid), E. M. Iribarren, E. V. Flores Rodríguez, M. D. C. Casas García, M. P. García-Señoráns, Ó. Cano Valderrama, P. F. Rodríguez, R. S. Santos, R. P. Currás, V. Vigorita (Álvaro Cunqueiro Hospital), C. G. Roche, E. Delgado, F. Lafuente, I. Gascon, M. V. Duque Mallen, S. Saudi, V. Fraj (Hospital Universitario Miguel Servet) M. E. Gómez, E. G. Santos, E. A. Mena, J. V. Mantecón, S. S. García, A. Fernández-Camuñas (Hospital General Universitario de Ciudad Real); **Sri Lanka:** D. Wickramasinghe, I. de Zoysa, N. Samarasekera, R. Wickramarathne, V. Dassanayake, Y. Balathayalan (National Hospital of Sri Lanka), D. de Silva, M. Perera, S. Pulleperuma, S. Jayasekara, W. Wijenayake (University Hospital Kdu), B. Gunetilleke, N. Abeysinghe, P. Chandrasinghe, S. Kumarage (North Colombo Teaching Hospital); **Sudan:** A. G. Abdalradiy, A. B. H. Widatalla, A. Y. Ahmed, H. A. Mohamed, H. K. S. Hamid, M. H. Ali, S. J. Eldin (Ibrahim Malik Teaching Hospital); **Sweden:** E. Agger, H. Jutesten, J. Lindgren, M. Lepsenyi, N. Azhar, P. Buchwald, P. Hansdotter (Skane University Hospital Malmö), A. Ekepil, Å. Lindén, G. Brandström, J. Smedberg (Visby Hospital); **Switzerland:** E. Schiffer, F. Ris, G. Longchamp, J. Meyer, L. Dupret, N. Buchs (Geneva University Hospitals), K. Galetti, L. Regusci, M. Grischott, M. Malugani (Ente Ospedaliero Cantonale); **Syrian Arab Republic:** A. El-Fattah Mouhandes, A.

K. Danial, M. Khayat, M. H. Eddin Sbahi, M. K. Marawy, M. A. Abdullah, Z. Douba (Aleppo Private Hospital), A. Mansour, A. Niazi, A. Hamza, A. H. Mohamad, M. Awead, S. Mohammad, S. Salloum (The Arabic Medicine), A. Al Jabar, A. Zazo, B. Shebli, K. Ayoub, L. Younes, M. H. Bannoud, R. Zazo (Aleppo University Hospital), A. Saad, A. Hamdan, H. Wakkaf, L. Adra, M. Souliman, M. Anton, S. Hannouf (Tishreen University Hospital); **Taiwan:** J. Y. Wang, K. L. Li, K. I. Cheng, S. J. Ji, Y. C. Hsieh (Kaohsiung Medical University Hospital); **Turkey:** E. A. Parlak, M. Demir, U. Kara, Y. S. Peker (Gulhane Training And Research Hospital), D. Yiğit, N. Unal, N. Iflazoğlu, Ö. Yalkin, S. Topal (Bursa City Hospital), B. Gulcu, E. Ozturk, G. Gümbelek, S. Terkanhoğlu (Medicana Bursa Hospital), A. Koklucan, G. Ince, M. Sen, O. Isik, S. Kural, S. Akesen, T. Yilmazlar (Bursa Uludag University School of Medicine), H. Sungurtekin, U. Sungurtekin, U. Vural, U. Ozgen (Pamukkale University School of Medicine), A. Isik, D. Onk, E. Kurnaz, T. S. Ozker (Erzincan University Hospital), A. Ipek, A. Ferlencez, C. Erturk, C. Tatar, H. Sevik, O. Akay, O. Sensoy (Istanbul Education And Research Hospital), M. B. Hayirlioglu, S. Aktas, V. Ozben, Z. Aliyeva (Acibadem Atakent Hospital), A. U. Mutlu, B. V. Gökay, C. Saraçoğlu, E. Aytac, M. Gülmez, M. Ü. Işık (Acibadem Atakent Hospital), A. Hacim, A. Akbas, F. Soyhan, M. A. Turgut, S. Demirkan, S. Meric, Y. Altinel (Bagcilar Research And Training Hospital), B. Baris, E. Akova, E. Kahraman, H. F. Kucuk, K. T. Saracoglu, S. Kaya, S. Lel (Kartal Dr. Lutfi Kırdar Training And Research Hospital), E. K. Gurbulak, E. Caz, M. Kostek, M. Mihmanli, P. Yazici, S. Oba (Sisli Hamidiye Etfal Training And Research Hospital), E. F. Kirkan, H. S. Ulgur, M. Kalın, M. D. Dinkci, O. F. Ozkan, O. Duzgun, S. Ozturk (University of Health Sciences Istanbul Umraniye Training And Research Hospital), A. K. Zengin, A. Aşkar, A. N. Şanlı, E. Erginöz, M. F. Özçelik, S. Ergün, S. S. Uludağ (Istanbul University-Cerrahpaşa Medical Faculty), D. Kara, G. Yılmaz, I. Ş. Sarıcı, Y. Kara (Kanuni Sultan Suleyman Training And Research Hospital), A. Incesu, C. Arican, S. D. Atici, T. Kaya, T. Gezer, Y. Kirmizi (University of Health Sciences Tepecik Training And Research Hospital), E. Kamer, G. Aydin, O. Namdaroglu, S. Adakaya (University of Health Sciences Tepecik Training And Research Hospital), A. E. Canda, D. Ozzebek, N. Coskun, S. Sokmen, S. Ozkardesler, T. Bisgin (Dokuz Eylul Univ. Hospital), A. Miftari, C. Caliskan, E. Akgun, G. Avseren, N. Deniz, O. Bozbiyik, T. Yoldas (Ege University Hospital), A. Güreşin, G. Zayakov, G. Pöstecki, N. Z. Utkan, O. C. Tatar, Ö. Akçay, S. A. Güler (Kocaeli University Teaching Hospital), B. Mantoglu, E. Demirel, E. Akın, E. Gonullu, F. Altintoprak, O. Palabyık, Z. Bayhan (Sakarya Faculty of Medicine), A. B. Ciftci, E. Colak, E. Aybar, H. K. Celik, H. Eraslan, K. Yemez, S. S. Ozbilgin, S. Senol (Samsun Training And Research Hospital), F. A. Gultekin, O. Piskin, O. Guler, Y. Karadere (Zonguldak Bulent Ecevit University School of Medicine Research And Training Hospital); **Uganda:** A. Kakeeto, B. Oguttu, F. K. Sikakulya, H. Lule (Kiryandongo Hospital); **Ukraine:** A. Rybachuk, A. Shudrak, A. Beznosenko, I. Lisnyy, V. Rozhkova, V. Zvirych (National Cancer Institute); **United Arab Emirates:** D. Alawlaqi, F. E. Jamali, I. Al Balooshi, M. Ahmed, M. Albers, N. Al Ali, R. Church (Sheikh Shakhboub Medical City); **United Kingdom:** G. Dudas, J. Wells, M. Pavlova, S. Sebastiani (Bronglais General Hospital), C. Paterson, M. Kaushal, P. Patel, S. Panchal, S. Handa, S. Tezas, S-N. Zaidi (Furness General Hospital), G. Raj, J. Wright, S. Hallam, S. Karandikar, Z. Gates (Heartlands Hospital), A. Marshall, A. Thompson, A. Tennakoon, M. Rao, R. Callan, R. Callan, S. Tufail (Pilgrim Hospital), G. Rajendran, K. Polisetty, N. Husain, N. Clarke, S. Naranayanasamy (University Hospitals of Derby And

Burton), A. Hallett, E. Lorejo, N. Ward, R. Antakia (West Suffolk Hospital), A. Xanthis, C. Simillis, E. Tweedle, I. Panagiotopoulou, L. Grimes, L. Mounstephen, R. Bocancia (Addenbrooke's Hospital), C. Carden, J. Lynch, M. S. Noveros, R. Shaalan, T. Khalil, W. Marshall (Ninewells Hospital), K. Hodge, J. Balfour, K. Mcintosh, L. Buijs, M. A. Potter, M. Yule, P. G. Vaughan-Shaw, S. Smith, T. Anderson (Western General Hospital), F. D. Mcdermott, I. R. Daniels, J. Tapp, N. Smart, N. Rajaretnam, R. Bethune, T. Clark (Royal Devon And Exeter Hospital), C. Delimpalta, C. Liao, G. Banham, L. Induruwage, V. Velchuru (James Paget Univeristy Nhs Foundation Trust Hospital), A. Lawrence, A. Rahman, J. Bennett, M. Badawi, R. Harshen (East Sussex Healthcare (Conquest Hospital And Eastbourne District General Hospital)), A. Bhargava, K. Gorrela, M. Jumah, M. Venn, M. Hanson, S. Arya, T. Atendido (King George Hospital), A. Shrestha, E. Cook, I. Rakhimov, J. Collins, N. Alamin, N. Vigneswaran, P. Basnyat (East Kent Hospitals Nhs Foundation Trust), A. Shamardal, A. Chacko, D. Wanshantha, G. Bisheet, H. Ebdewi, M. Abdellatif, P. Adu-Poku (Kettering General Hospital), A. Tore, F. Adams, K. Allen, K. Ahmed, N. Kulkarni (Lincoln County Hospital), A. Chitnis, H. Patel, J. Magsino, V. Sarodaya (Newham University Hospital), A. Minicozzi, C. Dempsey, H. Ahmed, H. Patel, J. D. Jayasinghe, M. H. Okail, M. Thaha, S. Hallworth (Royal London Hospital), C. Parmar, L. Chua, M. Pizantias, R. Samin, T. Young (The Whittington Hospital), J. Sagar, L. Yorkmui, N. Cirocchi, S. Ahmed, S. C. Barreda, S. Kudchadkar (Luton And Dunstable University Hospital), A. Baker, B. Jayasankar, D. Balasubramaniam, J. Jackson, K. Abdelsaid, M. Hassan, S. Shetty (Tunbridge Wells Hospital), C. Coldwell, E. Davies, H. Nader, M. Raistrick, O. Ryska, P. Hawkin, T. Raymond (Royal Lancaster Infirmary), C. Witjes, K. Van de Steen, N. Crabtree, S. Boyce, W. Somera (Churchill Hospital), A. Woodward, K. Ryan, M. Kassai, M. Aleem, S. Keogh-Bootland (Jersey General Hospital), A. Ghosh, D. Rixson, E. Lewis, N. Lynch (Morrison Hospital Swansea), C. Shovelton, E. Zywicka, F. Guest, J. Barton, R. Purnell, R. Bamford, T. Teare (Musgrove Park Hospital), B. Adams, G. Chmielewski, I. Smith, L. Smith, L. Connolly, R. Niblett (Royal Cornwall Hospital), A. Singh, G. Halliwell, M. Paraoan, N. Doree, P. Asaad (Wrightington, Wigan & Leigh Nhs Foundation Trust), C. Kilbride, H. Carpenter, J. Wilson, J. Fletcher, K. A. Vijayagopal, M. Abbakar, T. Zaimis (Wirral University Teaching Hospital), A. Walsh, A. Kubisz-Pudelko, J. Nono, L. Pippard, M. Chowdhary, R. Dalton, T. Moussa (Yeovil District Hospital); **Uruguay:** F. Dominguez, G. Solla, J. Curbelo, M. Laurini, M. Viola, N. Brito (Medica Uruguay); **Yemen, Rep.:** A. Al-Alnsi, H. Al-Naggat, L. Saryah, M. Al-Shehari, R. Alsayadi, R. Al-Hutheifi, S. Shream (Al-Thawra Modern General Hospital).

#### Data handling

Samerah Saeed, Eleanor Margaret Spurring

#### Author contributions

Writing, interpretation and methodology and investigation (writing team): NA, MAM, AB, DB, AB, KB, OB, AC, CC, MC, BED, MVDM, MBE, TEG, ME, IF, OFO, FF, KF, GG, DG, JCG, EH, RLH, RJ, SK, SKV, EK, JK, CHK, MK, PL, EL, BMP, AMB, RM, DGM, LRMN, IN, DN, OO, FP, TP, MAP, MR, GS, IS, AS, MLV. Conceptualisation, investigation and project administration (ops team, coordinators and meta-coordinators): EB, BED, ME, JE, JCG, RLH, SK, JK, EL, LM, SK, DGM, DN, OO, DOB, JS, MLV, AA, NA, NB, LC, MC, DF, GG, GIU, AMB, HMJ, PN, FP, TP, AR, ES, MS, MS, BS, AS, AV. Investigation, supervision, dissemination and validation (ESCP Research Committee, ESCP Global Research Committee, Data Monitoring



Committee): EA, SB, PB, SMC, DD, AD, ME, AEH, ZG, MIA, MK, CHK, BM, DGM, SN, GP, TP, SS, ES, AS, PT, CV, NA, RB, PC, SHL, VL, SKM, LM, JWU, JYW, JW, HY, DB, MC, SM. Formal analysis: OO, EL, RLH, DGM. Resources (educational team): SC, MF, JCG, EL, AMB, IN, MAP, JM, LSG, JS, DDEZ. Data Curation: SS, EMS.

## Disclosure

The authors declare no conflict of interest.

## Supplementary material

Supplementary material is available at *BJS* online. This includes translations of the Abstract into Chinese, Spanish, Korean, Portuguese, and Italian.

## Data availability

Data sharing requests will be considered by the management group upon written request to the corresponding author.

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