Mortality after surgery in Ireland

The European Surgical Outcomes Study (EuSOS) shows mortality rate in Ireland of 6.4% (95% CI 4.8–8.1%) for all elective and non-elective inpatient surgery, excluding planned day-case surgery, cardiac surgery, neurosurgery, radiological surgery, and obstetric surgery, during a week in April, 2011. This rate was significantly higher than that of 3.6% (95% CI: 3.2–3.9%) for the UK, which was the reference country. If true, these data have serious implications for the Irish health-care system.

There were repeated unsuccessful requests to the EuSOS authors by the Royal College of Surgeons in Ireland (RCSI) and the College of Anaesthetists of Ireland to get access to the EuSOS data. In view of the inability to validate the Irish EuSOS data and the importance of the findings, a direct replication—the Irish Surgical Outcomes Study (ISOS)—was done (appendix). This study involved all 17 Irish hospitals that participated in EuSOS, and we applied the same methods (details were available from the EuSOS website) and covered the same period in April, 2011.

The ISOS findings showed substantial differences from the EuSOS data for the same period. An additional 215 eligible patients were identified, but fewer deaths (table).

These substantial differences raise serious concerns regarding the quality and completeness of EuSOS. Ireland is not the only country to dispute EuSOS findings;2–4 at least three countries (of 28) have publicly challenged the integrity of EuSOS data. These concerns call into question the propriety of retaining the original paper in the literature and plans for the original team to continue to produce a series of further papers from this dataset.

We declare that we have no conflicts of interest.

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Authors’ reply

Sally Doherty and colleagues report the findings of a retrospective study of surgical mortality in Ireland during the same period studied in our prospective European study (EuSOS).1 Using a different method, the authors collected data describing a larger cohort of patients and identified fewer deaths, resulting in a different mortality estimate. While the authors use the original EuSOS data as a reference for their findings, they also suggest these data are inaccurate. The overall mortality for any large population of surgical patients is crucially dependent on the representation of high-risk surgical patients within it. The lower numbers of critical-care admissions and deaths suggest the high-risk group was not so strongly represented in this repeat study population. This difference might also represent a stronger tendency for investigators to include patients undergoing complex surgery in the prospective study. Nonetheless, the hospital mortality of 2.5% is higher than previous estimates, which range from 1.1 to 2.1%,2–4 and remains a cause for concern.

The authors sought our assistance with their study and we encouraged them to make full use of our original protocol and case record form. We also confirmed which Irish hospitals took part in our original study. The authors did request the EuSOS data for Ireland but, despite our repeated requests, were unable to provide a prospective statistical analysis plan. We remain prepared to share the data provided this basic methodological standard is met. Since publication of the report, we have worked with various groups to further analyse the EuSOS data and better understand our findings. Prospectively defined analyses of the relation between mortality and haemoglobin, serum sodium, surgery at nighttime, and use of the WHO checklist have all generated important findings and confirmed the validity of our data. Notably, prospective linkage with Swedish registry data has confirmed the accuracy of the stated hospital mortality and shows a four-fold increase in mortality within 1 year of surgery. Therefore, surgical patients could remain at risk even in nations with low early postoperative mortality rates.

We previously acknowledged the pragmatic nature of the EuSOS study. We have repeatedly indicated that our study does not provide a definitive mortality estimate, particularly in countries that contributed few patients, but that it demonstrates the need for further research and audit of
outcomes for this population. In view of the very large size of the surgical population, such measures might lead to a substantial reduction in the number of deaths.

We declare that we have no conflicts of interest.

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E. anophelis outbreak in an intensive-care unit

We read with interest Jeanette Teo and colleagues’ report (Sept 7, p 855)1 of the first outbreak of Elizabethkingia anophelis identified by 16S rRNA sequencing and whole-genome assembly. The subgroup of isolates had been previously identified as Elizabethkingia meningoseptica on the basis of matrix-assisted laser desorption-ionisation time-of-flight (MALDITOF) mass spectrometry analysis.

The history of this microorganism starts with its description as a cause of infant meningitis by Elizabeth O King at the US Centers for Disease Control and Prevention (CDC). She first isolated an organism referred to as CDC group IIa in 1959 and named it Flavobacterium meningosepticum. It was subsequently renamed Chryseobacterium meningosepticum, and classified in the new genus Elizabethkingia, in 2005.2

We believe that modern techniques (such as MALDITOF and sequencing) might generate more and more pseudo first outbreaks. Outbreaks of F meningosepticum, C meningosepticum, and E meningosepticum have been described in several patient settings, including intensive-care units.3–5 Thus, what is new here, except the name? To be considered as new outbreaks, future reports should describe a new source or pathway of transmission and not merely one that appears new because of the diagnostic methods presently used.

We declare that we have no conflicts of interest.

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Authors’ reply

Andreas Voss and colleagues have alluded to the fact that 16S rRNA sequencing in the early 2000s allowed Elizabethkingia to be placed separately from the genus of Chryseobacterium. Next-generation sequencing has facilitated a higher level of differentiation between two very distinct species of Elizabethkingia, namely Elizabethkingia meningoseptica and Elizabethkingia anophelis.3–5 Our analyses identifying the intensive-care unit outbreak strain as E anophelis is not just a reclassification of an old species as Voss and colleagues suggest. E anophelis is an entirely separate species with infection potential. E anophelis is presently understudied but should not be considered irrelevant in the clinical setting. Our sequencing data suggest the presence of a substantial number of virulence determinants, and studies to assess E anophelis’ virulence potential in animal models are in progress.

Investigation of novel outbreaks when paired with comparative genome sequencing data provides important information to understand transmission of a pathogen, and especially so for rare organisms. Comparative genomics is a crucial approach in the discovery of virulence determinants and genetic markers of uncharacterised bacterial species. Genome-based approaches can be associated with other omics-based approaches (eg, transcriptomics and proteomics)6 to analyse bacterial physiology and pathogenesis mechanisms.

An intriguing and important issue is the transmission pathway of E. anophelis. We speculate that malaria carriage in patients might be at the origin of E anophelis transmission in the hospital setting, which we are investigating.

We declare that we have no conflicts of interest.

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Mortality after surgery in Europe

The Association of Anaesthesiologists and Reanimatologists of Latvia and the Latvian Association of Surgeons would like to state that the mortality data published in the paper by Rupert Pearse and colleagues (Sept 22, p 1059) were completely incorrect regarding Latvia.

During the 7-day cohort study between April 4 and April 11, 2011, there was one death out of 85 patients at the Paul Stradins Clinical University Hospital; one death out of 104 patients at the Riga East Clinical University Hospital “Gailezers” and no deaths out of 113 patients at the Traumatology-Orthopaedic Hospital in Riga. Therefore, during this period, only two of 302 patients who were enrolled in this study actually died, giving a mortality rate of 0.66%, not 21.5% as published by Pearse and colleagues. The heads of these departments undertook an internal audit and they did not find any errors.

The important issue is that the data reported by Pearse and colleagues’ data were not controlled and verified by the Latvian coordinator before publication, despite repeated requests. Such highly unusual and unlikely results were published without further clarification and confirmation from the original source.

We declare that we have no conflicts of interest.

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I was asked by Rupert Pearse to participate in the European Surgical Outcomes Study (EuSOS) as a national coordinator and to propose other Polish participants for the trial.

Unfortunately, Pearse and colleagues did not give me the opportunity to see the results before submitting the paper, so I was not able to review the results of the study before publication.

Seeing the published results with respect to Poland, I have to state that the striking rate of mortality given for Poland (17.9%) is significantly higher than the actual rate. I collected information on the number of deaths at the hospitals that had participated in the study. According to these data, during the period of study, only two deaths were reported of 397 patients included (mortality rate 0.5%). The incredibility and incoherence of the data presented in the paper are further demonstrated by the number of 71 deaths, when there were only eight patients in intensive-care units (ICUs).

The number of ICU patients and the number of postoperative deaths should be proportional, because if a patient’s condition deteriorates, he or she is transferred to the ICU.

The data reported by Pearse and colleagues have the potential to mislead the medical community and should be corrected.

I declare that I have no conflicts of interest.

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Rupert Pearse and colleagues present a study assessing in-hospital deaths after surgical procedures in Europe, which shows rather high average mortality (4%). This rate is partly driven by very high mortality in some countries—eg, Poland at 17.9%. Such a figure does not seem to reflect reality. A death rate of 17.9% would be unacceptable in any hospital, and the extrapolation of the results from a 7-day study in six hospitals in Poland to the whole country, as Pearse and colleagues have done, seems inappropriate.

We assessed data for 2011 from the database of the National Health Foundation (NHF) in Poland. The database, which is not publicly accessible, includes almost all major and most minor surgical procedures (it does not cover obstetric, radiological, or paediatric procedures, nor the usually minor procedures done privately, but does cover planned 1-day, cardiac, and neurological surgery, which were excluded by Pearse and colleagues). We noted the type of discharge from hospital (in this case “death”), predefined in the computer system and reported to the NHF.

As shown in the table, the average in-hospital mortality for all surgical procedures in 2011 in Poland was 0.98%—ie, 18 times lower than that shown by Pearse and colleagues. Additionally, we have extracted from the NHF database the data on mortality in the six hospitals in Poland that took part in Pearse and colleagues’ study. In those six hospitals, average in-hospital mortality after all surgical procedures in 2011 was 1.07%, which is very similar to the whole-country rate.

We suggest that Pearse and colleagues’ methods are misleading.

We declare that we have no conflicts of interest.

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Table: In-hospital mortality after surgical procedures in Poland in 2011, according to surgical specialty

<table>
<thead>
<tr>
<th>Surgical Specialty</th>
<th>Number of procedures</th>
<th>Number of in-hospital deaths</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vascular</td>
<td>100 792</td>
<td>1964 (1.9%)</td>
</tr>
<tr>
<td>Eye</td>
<td>216 208</td>
<td>104 (0.005%)</td>
</tr>
<tr>
<td>Skin and mammary gland</td>
<td>307 896</td>
<td>355 (0.33%)</td>
</tr>
<tr>
<td>Gastrointestinal tract</td>
<td>355 706</td>
<td>7501 (2.2%)</td>
</tr>
<tr>
<td>Head and neck</td>
<td>123 864</td>
<td>98 (0.08%)</td>
</tr>
<tr>
<td>Endocrinological</td>
<td>23 381</td>
<td>26 (0.11%)</td>
</tr>
<tr>
<td>Heart and circulation</td>
<td>235 891</td>
<td>4129 (1.8%)</td>
</tr>
<tr>
<td>Bone and muscle</td>
<td>422 329</td>
<td>2948 (0.70%)</td>
</tr>
<tr>
<td>Genitourinary tract</td>
<td>125 641</td>
<td>308 (0.25%)</td>
</tr>
<tr>
<td>Neurosurgery</td>
<td>42 763</td>
<td>2591 (6.1%)</td>
</tr>
<tr>
<td>Respiratory system</td>
<td>27 537</td>
<td>464 (1.7%)</td>
</tr>
<tr>
<td>Liver, pancreas, and spleen</td>
<td>118 070</td>
<td>1291 (1.2%)</td>
</tr>
<tr>
<td>Female genital tract</td>
<td>361 033</td>
<td>109 (0.03%)</td>
</tr>
<tr>
<td>Polytrauma</td>
<td>146</td>
<td>40 (27.4%)</td>
</tr>
<tr>
<td>Total</td>
<td>2 241 257</td>
<td>22 028 (0.98%)</td>
</tr>
</tbody>
</table>

and insufficient to draw conclusions valid for whole countries. The results might therefore be unreliable not only for Poland but also for other European countries.

We declare that we have no conflicts of interest.

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We have three concerns about Rupert Pearse and colleagues’ groundbreaking EuSOS study.1

First, the use of in-hospital mortality is not robust, since local behaviour introduces substantial bias.2 The use of 30-day mortality would remove this problem. The registration details of the study3 specify “28-day mortality” as a secondary outcome, a measure absent from the study documentation4 and report.1

Second, it is reasonable to adjust mortality with the UK as the reference standard. Less reasonable is to use the UK (22.8% of all data) as a benchmark for statistical comparison. Table 4 in the EuSOS appendix supports this contention, since Pearse and colleagues’ regression model shows a p value of <0.0001 for “country” versus the UK. With an appropriate analysis that uses the whole study population as a reference standard, the UK is likely to be an outlier, which is clearly impossible if it is used as the reference standard.

Third, Pearse and colleagues state that low rates of admission to critical care prevent “any detailed comparison of this resource between nations”, but postulate that availability of intensive-care facilities affects outcomes, citing Germany versus the UK. Examination of their data, however, seems to refute their hypothesis—for example, Sweden has both a low mortality rate (1.8%) and a low rate of admission to critical care (3.2%). More formal analysis of this relation seems appropriate, with adjustment for confounding variables.

Pearse and colleagues might be able to respond to our concerns by re-examining their data, and applying the suggested analyses.

We declare that we have no conflicts of interest.

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In their study on postoperative mortality in Europe,1 Rupert Pearse and colleagues point out that international comparative data might provide important insights into the delivery of health care for patients undergoing surgery. As a consequence, they provide estimates for in-hospital mortality in 28 European countries. Unadjusted mortality rates differed substantially, ranging from 1.2% in Iceland to 21.5% in Latvia. Poland, Latvia, Romania, and Ireland had higher mortality rates than the UK even after adjustment for confounding variables.

However, the representativeness of the samples and comparability of countries seems questionable: in Poland, only six hospitals (including one university hospital) took part in the study, whereas in the UK, the reference country, 100 hospitals (including 52 university hospitals) were studied.

The observation period of 7 days is also probably not representative of the volume of surgery in a hospital. It seems highly unlikely that every fifth or sixth patient dies after an operation, as described for Latvia or Poland. Thus conclusions about any difference between countries remain speculative.

Nonetheless, this dataset clearly describes a large cross-section of health care in Europe and provides relevant information on the drivers of postoperative mortality. However, it would have been worthwhile to get more detailed information on other influencing factors—eg, preoperative assessment, checking of equipment and drugs, syringe labelling, and infection control—as described in the Helsinki Declaration on Patient Safety in Anaesthesiology.5

We declare that we have no conflicts of interest.

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Authors’ reply

We did a large study to provide data at a European level on mortality after surgery.1 We are not aware of any previous work exploring surgical outcomes on an international basis. However, the 7-day cohort design does not provide definitive data. The intention was to inform and stimulate...
the design of further research to improve outcomes for patients undergoing surgery.

We specifically state in the paper that these data cannot be used as an accurate indication of mortality in individual countries. There are obvious risks of overgeneralising small cohorts with likely selection bias. Some correspondents suggest that mortality has been over-reported in some centres. During the data cleaning process we asked local investigators to complete a large number of validation checks and also asked national coordinators to facilitate this process.

We are aware that the mortality rates in the EuSOS study have subsequently been compared to registry data in some countries. Such a comparison could stimulate helpful debate as we attempt to understand surgical outcomes in individual countries. Indeed, we requested the help of national coordinators in identifying any publicly available data against which our data should be compared. We presented all such evidence where it was made available. Nonetheless, we advocate caution because differences in estimates are not unexpected when comparing mortality estimates calculated by use of very different methods. Our selection criteria excluded low-mortality patients who receive care in dedicated care pathways such as obstetrics, day case, and cardiac surgery. It is unclear to us whether comparisons with registry data allow correct application of the same selection criteria used in our study. We also note that most national health-care registries were not designed to allow correct application of the same measures are clearly described in the full protocol, which is available online and has also been published in summary form in a peer-reviewed journal.

Any exploration of the effect of critical-care admission on postoperative mortality is affected by the definition used. We predefined critical care as a facility routinely capable of admitting patients who require invasive ventilation overnight. We suspect that, in some countries, at least a proportion of postanaesthetic recovery units meet these criteria but are not locally regarded as critical care. Meanwhile, in other nations, there is evidence to suggest that some facilities are identified as intensive-care units but do not offer organ support.

To further inform the discussion of our findings, we have done an additional, more conservative, sensitivity analysis in which we excluded hospitals above the 95th centile for mortality and also those that recruited ten patients or fewer during the 7-day study period. This process excludes 72 centres and 944 patients from the cohort, leaving 426 centres and 45,595 patients to be analysed. Since high-mortality centres were excluded, we saw an overall reduction in mortality from 4% to 3%. The findings of this sensitivity analysis remain consistent with our original conclusion that mortality was higher than expected, with significant variations between nations. In this analysis, outcomes in Finland were better than the UK (odds ratio 0·5, 95% CI 0·2–0·9)), whereas outcomes in Romania were worse (2·8, 1·7–4·6).

We agree that the overall patient population could also be used as a reference in making such comparisons. This has very little effect, however, on the relative position of nations and does not alter our conclusions. We reported data as in-hospital mortality censored at 60 days. The great majority of deaths occurred within 14 days of surgery. The primary and secondary outcome measures are clearly described in the full protocol, which is available online and has also been published in summary form in a peer-reviewed journal.

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