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Effectiveness of Antimicrobial Stewardship Programmes in Reducing Hospital-Acquired Multidrug-Resistant Bacterial Infections: A Multicentre Cohort Study

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ABSTRACT

Background: Antimicrobial stewardship programmes (ASPs) seek to improve antibiotic prescribing and reduce selection pressure for multidrug-resistant organisms (MDROs). Whether patient-level exposure to ASP care is independently associated with fewer hospital-acquired multidrug-resistant infections remains uncertain.

Methods: We analysed an 800-patient observational cohort from eight anonymised hospitals in the United States, conducted from January to December 2022. Patients receiving ASP-managed care (n=433) were compared with usual care (n=367) at a 48-hour hospital landmark. The primary endpoint was first new hospital-acquired multidrug-resistant bacterial infection before discharge, inpatient death or day 30. Cause-specific Cox regression was stratified by hospital and adjusted for demographics, comorbidity, severity, device exposure, recent antibiotic use, known colonisation, immunosuppression and baseline ward hand-hygiene compliance. Discharge and inpatient death were treated as competing events in descriptive cumulative-incidence analyses.

Results: New resistant infections occurred in 69/433 ASP-managed patients (15.9%) and 83/367 usual-care patients (22.6%); crude risk ratio 0.70 (95% confidence interval [CI] 0.53–0.94). The adjusted cause-specific hazard ratio was 0.68 (95% CI 0.40–1.13; P=0.138). Baseline hand-hygiene compliance was substantially higher in ASP wards (86.4% versus 66.0%). ASP management was associated with fewer broad-spectrum antibiotic days (adjusted geometric mean ratio 0.84, 95% CI 0.76–0.92), but no clear difference in 30-day mortality (adjusted odds ratio 1.21, 95% CI 0.40–3.69).

Conclusions: ASP-managed care was associated with a lower unadjusted occurrence of new hospital-acquired resistant infections and reduced broad-spectrum antibiotic exposure. The primary association was uncertain after adjustment, particularly given marked differences in ward infection-prevention context. Randomised or well-controlled longitudinal studies are needed to distinguish stewardship effects from concurrent infection-control improvements.

Keywords: antimicrobial stewardship; multidrug resistance; hospital-acquired infection; cohort study; antibiotic utilisation; competing risks

1. INTRODUCTION

Antimicrobial resistance poses an escalating threat to modern inpatient care by diminishing therapeutic options, increasing the complexity of treatment and imposing a disproportionate burden on patients requiring invasive devices, intensive care or prolonged antibiotic exposure. Large-scale burden estimates and regional surveillance analyses show that resistant bacterial infections contribute substantially to deaths and disability worldwide. Although global burden estimates differ in their denominators and microbiological scope, they consistently indicate the importance of prevention alongside treatment development. Multidrug-resistant organisms (MDROs) are conventionally defined through acquired resistance across antimicrobial categories, but operational hospital surveillance definitions can vary between institutions. [1–4, 24, 25]

Selection pressure from antimicrobial exposure is one of several interacting determinants of resistance. Patient-to-patient transmission, contaminated environments, invasive devices, host susceptibility and infection-prevention practices also contribute to hospital-acquired infection. Accordingly, antimicrobial stewardship should be viewed as one component of an integrated prevention strategy rather than a standalone substitute for hand hygiene, surveillance or source control. Because the same hospital may intensify antimicrobial review and infection-prevention practices together, the effects attributed to either component can be difficult to isolate in non-randomised studies. [4, 5, 24, 35]

Antimicrobial stewardship programmes (ASPs) use interventions such as prospective audit with feedback, preauthorisation and guideline-based reassessment to optimise antibiotic selection, duration, route and spectrum. Professional guidelines and implementation frameworks advocate accountable clinical leadership, pharmacy participation, timely reassessment and measurement of antibiotic use. Prior reviews consistently describe improvements in prescribing and antibiotic consumption; evidence for reductions in individual resistant pathogens is more heterogeneous, reflecting differing case definitions, intervention fidelity, observation periods and local ecology. [5–14]

For example, a published systematic review found that stewardship strategies were associated with reduced rates of several resistant organisms, but the contributing designs were diverse and often combined stewardship with additional institutional interventions. Long-term interrupted time-series analyses have shown encouraging resistance trends, whereas other studies demonstrate that effects vary by pathogen, prescribing target and implementation setting. Such evidence supports investment in stewardship while cautioning against interpreting crude differences between participating and nonparticipating patients as causal effects. [9, 12, 34–36, 38]

The present multicentre cohort study evaluated the association between patient-level ASP-managed care and incident hospital-acquired multidrug-resistant bacterial infection at eight anonymised US hospitals. Additional outcomes were broad-spectrum antibiotic exposure, hospital length of stay, mortality and readmission; associations with different ASP strategies were explored. We hypothesised that ASP exposure would be associated with fewer resistant infections and reduced broad-spectrum antibiotic use, while recognising the potential influence of concurrent infection-prevention practices.

2. METHODS

2.1 Study design and population

We conducted a multicentre observational cohort study of 800 patients cared for at eight hospitals in the United States between January and December 2022. Participating hospitals are identified only as H01–H08 to preserve institutional confidentiality. De-identified patient-level records were used for analysis. The study data dictionary defined time zero as a 48-hour hospital landmark, with all analysed patients free of an active hospital-acquired infection at that time. Each record represented a distinct participant, and no duplicate identifiers were found. Patient-level admission dates and the source-population screening log were not available in the analytic extract; accordingly, calendar-time effects and sampling completeness could not be independently evaluated. The study was reported in accordance with STROBE principles. [39]

2.2 Exposure and stewardship strategies

The exposure was active ASP management at the 48-hour landmark, contrasted with usual care. Assignment was observational rather than randomised. The programme variable identified prospective audit with feedback (strategy 1), antibiotic preauthorisation (strategy 2), or combined audit and preauthorisation (strategy 3). These categories describe the existing management pathway at the landmark and do not establish a uniform programme launch date, intervention intensity or clinical adherence. Defining exposure before subsequent infection avoided attributing later stewardship consultation retrospectively to unexposed patient-time. Guidelines recommend these strategies, but their real-world uptake is context dependent. [6–9, 27, 30]

2.3 Outcomes and follow-up

The primary endpoint was first new hospital-acquired multidrug-resistant bacterial infection occurring after the landmark and before discharge, inpatient death or day 30, whichever came first. The patient-level extract recorded a binary infection indicator and time to first event but did not contain organism identity, infection site, specimen-level results, susceptibility thresholds or adjudication records. Consequently, the composite endpoint was analysed as defined in the source dataset and could not be independently recoded against international MDRO or national surveillance definitions. For patients without an event, observation ended at first discharge, death or day 30. Discharge and inpatient death thereby competed with inpatient infection; remaining in hospital at day 30 constituted right-censoring for the infection analysis. [3, 42]

Secondary outcomes were whole days of broad-spectrum antibiotic treatment after the landmark, length of hospital stay after the landmark, in-hospital death during the index admission, all-cause death by day 30 and readmission by day 30 among patients discharged within 30 days. The data dictionary described complete ascertainment of 30-day vital status. Length of stay could exceed the 30-day window for infection surveillance. Readmission analyses were limited to patients discharged by day 30 because those remaining hospitalised were not yet at risk of readmission.

2.4 Covariates and potential confounding

Candidate baseline covariates were age, sex, Charlson Comorbidity Index (excluding age points), intensive-care location, Sequential Organ Failure Assessment (SOFA) score, systemic antibiotic exposure in the preceding 90 days, known MDRO colonisation, immunosuppression, central venous catheter, invasive ventilation and hand-hygiene compliance. Ward hand-hygiene compliance referred to the month preceding the landmark and was therefore treated as contextual baseline information rather than a downstream ASP outcome. These variables represent overlapping but clinically relevant host, device, severity and institutional determinants. The Charlson and SOFA instruments have established clinical origins, although their performance was not independently validated in this cohort. [4, 35, 40, 41]

2.5 Statistical analysis

Categorical data were summarised as counts and percentages; continuous values as mean with standard deviation or median and interquartile range as appropriate. The unadjusted absolute risk difference, relative risk and their approximate 95% confidence intervals were calculated from observed cumulative event proportions. Fisher’s exact test was used for the crude comparison. The primary time-to-event analysis used cause-specific Cox proportional hazards regression with Efron handling of ties, stratified by hospital and adjusted for the 11 specified patient- and ward-level covariates. A cause-specific hazard ratio is the relative instantaneous rate among patients remaining in hospital and infection-free; it is not equivalent to an adjusted 30-day risk ratio or a cumulative-incidence effect.

The Aalen–Johansen estimator was used to display cumulative incidence of first MDRO infection while classifying discharge and inpatient death without preceding infection as competing events. Patients without an event who remained hospitalised at day 30 were administratively censored. As a sensitivity analysis, adjusted logistic regression examined the binary observed infection endpoint, and a Poisson model with log observed inpatient time as offset estimated the infection event rate ratio; these analyses answer different questions from the cause-specific hazard model and were not used to overturn the primary interpretation. An additional Cox model excluding hand-hygiene compliance assessed sensitivity to this strongly unbalanced contextual variable. [42–44]

For secondary outcomes, we used covariate-adjusted ordinary least-squares regression of log-transformed antibiotic days and hospital days, reporting exponentiated coefficients as ratios of geometric means. Hospital fixed effects were included in these models. Binary clinical outcomes were analysed with adjusted logistic regression and heteroskedasticity-robust confidence intervals; the readmission model was restricted to eligible discharged patients. Strategy-specific Cox comparisons with usual care were exploratory, adjusted with the same covariates and stratified by hospital, without multiplicity adjustment. Two-sided P values below 0.05 were described as statistically significant, with emphasis on uncertainty and effect sizes. Data fields used in the analyses were complete; blank infection-onset times for non-cases were structural rather than missing measurements.

2.6 Ethical considerations

Institutional ethical approval was obtained from the relevant ethics review bodies for the participating hospitals. The analysis used de-identified patient records, and all eight hospitals are identified only by study codes (H01–H08) to preserve institutional confidentiality.

3. RESULTS

3.1 Cohort characteristics

The analysis included 800 unique patients: 433 (54.1%) were managed under an active ASP and 367 (45.9%) received usual care. Eight hospitals contributed between 93 and 112 records each. Mean age was 53.3 years in the ASP group and 53.2 years in the usual-care group, and men represented approximately 48% of each group. The ASP group contained more intensive-care patients (34.9% versus 23.7%) and had a higher mean SOFA score (3.91 versus 3.05). Prior systemic antibiotic exposure, colonisation, immunosuppression and device use were also somewhat more frequent among ASP-managed patients. Baseline ward hand-hygiene compliance differed substantially, averaging 86.4% in the ASP group and 66.0% in usual care, with a standardised mean difference of approximately 2.52. Baseline characteristics are displayed in Table 1.

3.2 First new hospital-acquired resistant infection

Overall, 152 patients (19.0%) experienced a new hospital-acquired multidrug-resistant bacterial infection during observed inpatient follow-up. Events occurred in 69 of 433 patients receiving ASP care (15.9%) and 83 of 367 usual-care patients (22.6%). The crude risk difference was −6.68 percentage points (95% CI −12.18 to −1.18), and the unadjusted relative risk was 0.70 (95% CI 0.53–0.94; Fisher’s exact P=0.019). Infection incidence was 13.44 versus 18.96 events per 1,000 observed inpatient days in the ASP and usual-care groups, respectively. The cumulative-incidence curves (Figure 1) reflect discharge and inpatient death as competing events; their final values equal the observed inpatient event proportions because ascertainment ended at discharge, death or day 30.

The crude cause-specific hazard ratio was 0.72 (95% CI 0.52–0.99; P=0.040). After adjustment for baseline clinical and ward factors and stratification by hospital, the cause-specific hazard ratio was 0.68 (95% CI 0.40–1.13; P=0.138), leaving a clinically relevant range of possible benefit and little effect. Higher baseline SOFA score and known MDRO colonisation were independently associated with higher event hazards in the multivariable model, although all regression coefficients remain associational. Full group-level findings and principal model estimates are shown in Table 2.

3.3 Sensitivity and strategy analyses

An adjusted infection-status logistic model produced an odds ratio of 0.50 (95% CI 0.28–0.90; P=0.020), and a Poisson rate model produced an incidence rate ratio of 0.66 (95% CI 0.41–1.05; P=0.080). The alternate endpoints and differing assumptions account for some discordance with the primary cause-specific analysis and should not be framed as independent confirmation. When hand-hygiene compliance was omitted from the otherwise adjusted hospital-stratified Cox model, the hazard ratio moved to 0.63 (95% CI 0.45–0.89; P=0.008), indicating the importance of the contextual imbalance. The propensity scores for ASP exposure displayed extremely limited overlap and potentially extreme weights; inverse-probability weighting was therefore not considered reliable as a primary analysis.

Among ASP-managed patients, 145 received audit and feedback, 151 preauthorisation and 137 a combined strategy. New MDRO infection occurred in 15 (10.3%), 29 (19.2%) and 25 (18.2%) patients in those respective groups. Compared with usual care, the adjusted hospital-stratified hazard ratios were 0.50 (95% CI 0.23–1.06), 0.88 (0.45–1.73) and 0.79 (0.40–1.58). This exploratory comparison was not randomised and is especially vulnerable to variation in programme targeting, ward adherence and patient selection. Descriptive event proportions also varied across H01–H08, supporting caution when interpreting one pooled estimate.

3.4 Antibiotic exposure and clinical outcomes

The median number of broad-spectrum antibiotic days was six (interquartile range 4–7) in the ASP group versus seven (5–9) with usual care. The adjusted ratio of geometric means was 0.84 (95% CI 0.76–0.92; P<0.001), consistent with approximately 16% lower broad-spectrum exposure conditional on measured factors. Median post-landmark hospital stay was 11.9 days in ASP care and 12.4 days in usual care; the adjusted geometric mean ratio was 0.84 (95% CI 0.73–0.96; P=0.010). Because the crude median difference was small and the observational groups differed in severity and ward context, the adjusted stay association warrants external confirmation.

In-hospital death occurred in 15 ASP-managed patients (3.5%) and 12 usual-care patients (3.3%). Thirty-day all-cause mortality occurred in 20 (4.6%) and 18 (4.9%), respectively; its adjusted odds ratio was 1.21 (95% CI 0.40–3.69; P=0.739). Of 734 patients discharged within 30 days, 39 of 399 ASP patients (9.8%) and 39 of 335 usual-care patients (11.6%) were readmitted by day 30; the adjusted odds ratio was 0.98 (95% CI 0.42–2.26; P=0.954). The event counts and broad confidence intervals do not support a conclusion that ASP exposure reduced mortality or readmissions in this cohort. Table 3 and Figure 2 place these secondary estimates beside the primary effect.

4. DISCUSSION

This eight-hospital cohort demonstrates an informative contrast between crude and adjusted assessments of antimicrobial stewardship. Patients receiving ASP-managed care had a lower observed proportion of new hospital-acquired multidrug-resistant bacterial infections and fewer days of broad-spectrum antibiotic treatment. The lower crude infection risk is compatible with a beneficial programme effect, but the adjusted cause-specific hazard ratio did not exclude the null. The most defensible conclusion is therefore that ASP participation was associated with a favourable descriptive infection pattern and a more consistent antibiotic-utilisation signal, without sufficiently precise independent evidence that ASP exposure prevented new resistant infections in this dataset.

The results should be interpreted in the broader stewardship literature. The foundations of ASPs lie in optimising prescribing decisions while maintaining clinical effectiveness; reduced antibiotic consumption can be observed earlier and more reliably than changes in facility-level resistance ecology. Hospital studies have demonstrated decreases in antimicrobial use, cost and inappropriate prescribing after multidisciplinary review and restriction policies. Yet heterogeneous intervention packages, temporal trends and organism-specific outcomes make direct comparison between programmes difficult. A reduction in measured exposure is therefore an important operational endpoint but should not be assumed to imply an equivalent decline in resistant infections. [10–14, 16–23, 25, 29–33]

Several published studies have described favourable trends in hospital-acquired resistance following sustained stewardship implementation. In a long controlled interrupted time-series analysis, Peragine and colleagues reported an improvement in the institutional burden of hospital-acquired antibiotic-resistant organisms, while estimates for hospital-acquired MDROs retained appreciable uncertainty. Other programmes targeting carbapenems or fluoroquinolones reported reductions in resistant-pathogen indicators, but intervention mixtures and local epidemiology differed. These findings support the biological plausibility of an association between appropriate antibiotic use and resistance while reinforcing the need to disentangle antibiotic prescribing changes from concurrent infection prevention. [27, 28, 33–38]

A central feature of our cohort is the pronounced imbalance in baseline ward hand-hygiene compliance. ASP-managed patients had far higher reported compliance, creating plausible confounding by the institution’s broader safety culture, staffing, surveillance

intensity or quality-improvement investment. Such differences cannot be corrected simply by including one hand-hygiene percentage in a regression model. The possibility of measurement error and lack of common support between otherwise similar exposed and unexposed patients limits causal interpretation. Indeed, omitting this variable materially strengthened the estimated ASP association. Treating the contextual covariate as irrelevant would overstate the degree of stewardship-specific protection demonstrated by the data. [4, 35, 43, 44]

An additional challenge is that hospital discharge and inpatient death change the time available to develop an observed nosocomial infection. Simple proportions can mislead when one group spends less time hospitalised. Here, person-time rates and cause-specific hazards were used to complement the crude risk, while the cumulative-incidence curve explicitly incorporated discharge and death as competing events. These models address different estimands: a cause-specific hazard reflects infection among patients who remain eligible for the event, whereas cumulative incidence incorporates the changing probability of staying in hospital. The adjustment-dependent results and the distinct assumptions of the sensitivity regressions argue against describing one significant P value as the final answer. [42, 44]

The audit-and-feedback subgroup had the lowest descriptive infection proportion, consistent with the practical advantages of timely multidisciplinary review and prescriber dialogue. However, strategy-level findings were not definitive after adjustment, and there was no random allocation to strategy. Differences could reflect selection of services, availability of specialist pharmacists, stewardship intensity or underlying ward infection-control performance. These data therefore do not justify ranking the three approaches by effectiveness. Published guidelines endorse both audit-feedback and preauthorisation as core programme components, with the appropriate configuration determined by institutional capacity and local needs. [6–8, 19, 27, 30, 33]

The association with shorter antibiotic exposure is clinically relevant even if effects on resistant infections remain uncertain. Days of broad-spectrum therapy represent a readily measurable prescribing outcome and may reflect opportunities for de-escalation, avoidance of unnecessary initiation or shortened duration. That said, the analytic dataset lacks agent-level prescriptions, defined daily doses, antibiotic indication, appropriateness of empirical coverage and adverse-effect outcomes. Consequently, we cannot determine which stewardship actions drove the lower exposure or assess whether individual prescriptions met contemporary practice standards. Reduced use alone is insufficient evidence of improved patient-centred antibiotic quality. [10–15, 18, 23]

The absence of a clear survival or readmission signal is not surprising in a cohort with few deaths and potentially heterogeneous baseline risks. Infection prevention can plausibly benefit populations over extended periods without producing a detectable short-term mortality effect in a single observational sample. The mortality model yielded a wide interval spanning possible benefit and harm. The adjusted length-of-stay estimate suggested shorter stays, but that signal was not mirrored by a large crude median difference, and hospital discharge may itself depend on illness severity, service organisation and clinical response. Such secondary associations should be considered hypothesis generating, not guarantees of clinical benefit. [13, 14, 22, 34, 38]

The principal strengths of this work are a clear 48-hour exposure landmark, patient-level infection timing, an explicitly stated primary estimand, consideration of discharge and death as competing events, specified severity and colonisation covariates and inclusion of eight anonymised US hospitals. The study dataset was complete for the variables used in analysis, enabling internally reproducible numerical estimates without imputation. Individual-level modelling also allowed us to account for several measured patient and ward characteristics that could confound the observed association.

Important limitations remain. Observational assignment permits confounding by indication, institutional infrastructure and contemporaneous infection-prevention activity; residual confounding is likely even after adjustment. The marked ward hand-hygiene imbalance produced weak exposure overlap and ruled out a convincing propensity-weighted causal contrast. Although recruitment occurred between January and December 2022, patient-level calendar dates and intervention roll-out histories were not retained in the analytic extract; consequently, seasonal patterns, secular changes and staggered adoption could not be modelled. Microbiological details, susceptibility thresholds and pathogen-level stratification were unavailable, limiting independent verification of the composite MDRO endpoint. The analytic dataset also lacked an eligibility screening log, admission diagnoses, antibiotic agents, stewardship process adherence and time-varying covariates. There were only 152 primary events and 38 deaths by day 30, making some multivariable intervals wide; clustering across eight sites further limits precision in generalisation. [3, 39, 43, 44]

Future evaluations should prospectively capture specific resistant organisms, contemporaneous antibiotic dispensing, stewardship recommendations accepted or rejected, ward-level infection-control performance and complete calendar-time information. Where feasible, stepped-wedge or interrupted time-series designs, or rigorous target-trial emulation with documented common support and competing-risk estimands, could help determine whether reductions in resistant infections are attributable to stewardship itself. Antimicrobial stewardship should nevertheless be implemented as part of an accountable hospital-wide programme that couples rational prescribing with infection prevention and routine outcome surveillance. [6–8, 26, 43, 44]

5. CONCLUSION

Among 800 patients in eight hospitals, ASP-managed care was associated with fewer observed hospital-acquired multidrug-resistant bacterial infections and shorter broad-spectrum antibiotic exposure. The adjusted primary cause-specific hazard estimate suggested potential protection but remained statistically uncertain after accounting for patient risk, hospital and baseline hand-hygiene context. Mortality and readmission benefits were not established. These data support stewardship as a promising component of integrated infection prevention while highlighting the need for designs that better separate programme effects from concurrent ward quality improvements.

Table 1. Baseline characteristics at the 48-hour landmark

Characteristic	Usual care (n=367)	ASP (n=433)
Age, years; mean ± SD	53.2 ± 20.5	53.3 ± 21.1
Male sex, n (%)	175 (47.7)	210 (48.5)
Charlson index; mean ± SD	1.03 ± 1.23	1.23 ± 1.34
ICU at landmark, n (%)	87 (23.7)	151 (34.9)
SOFA score; mean ± SD	3.05 ± 2.43	3.91 ± 2.70
Antibiotics in previous 90 days, n (%)	124 (33.8)	165 (38.1)
Known MDRO colonisation, n (%)	40 (10.9)	51 (11.8)
Immunosuppressed, n (%)	53 (14.4)	78 (18.0)
Central venous line, n (%)	120 (32.7)	163 (37.6)
Invasive ventilation, n (%)	70 (19.1)	102 (23.6)
Ward hand-hygiene compliance, mean %	66.0	86.4
Median MDR observation time, days (IQR)	10.6 (7.2–15.3)	10.7 (6.9–15.4)

Abbreviations: ASP, antimicrobial stewardship programme; ICU, intensive care unit; IQR, interquartile range; MDRO, multidrug-resistant organism; SOFA, Sequential Organ Failure Assessment. Values were calculated from the 800-patient cohort.

Table 2. Primary MDRO infection analysis and sensitivity models

Measure	Usual care	ASP / adjusted estimate
New MDRO infections	83/367 (22.6%)	69/433 (15.9%)
Crude absolute risk difference (95% CI)	—	−6.68 pp (−12.18 to −1.18)
Crude risk ratio (95% CI)	—	0.70 (0.53–0.94); P=0.019
Observed infection rate / 1,000 patient-days	18.96	13.44
Crude cause-specific HR (95% CI)	Reference	0.72 (0.52–0.99); P=0.040
Primary adjusted, hospital-stratified HR	Reference	0.68 (0.40–1.13); P=0.138
Adjusted incidence rate ratio, Poisson	Reference	0.66 (0.41–1.05); P=0.080
Adjusted binary endpoint odds ratio	Reference	0.50 (0.28–0.90); P=0.020
Adjusted HR without hand hygiene	Reference	0.63 (0.45–0.89); P=0.008

Primary Cox model includes age, sex, Charlson index, ICU, SOFA, prior antibiotic exposure, known MDRO colonisation, immunosuppression, central line, ventilation and baseline ward hand-hygiene compliance, stratified by hospital. Poisson and logistic models also adjust for hospital fixed effects. Different estimands should not be interpreted interchangeably.

Table 3. Antibiotic consumption and secondary clinical outcomes

Outcome	Usual care	ASP	Adjusted ASP association (95% CI)
Broad-spectrum days, median (IQR)	7 (5–9)	6 (4–7)	Geometric ratio 0.84 (0.76–0.92)
Post-landmark stay, days, median (IQR)	12.4 (8.8–18.2)	11.9 (7.6–17.4)	Geometric ratio 0.84 (0.73–0.96)
In-hospital death, n (%)	12/367 (3.3%)	15/433 (3.5%)	OR 1.15 (0.34–3.93)
All-cause death by 30 days, n (%)	18/367 (4.9%)	20/433 (4.6%)	OR 1.21 (0.40–3.69)
Readmission by day 30 among discharged	39/335 (11.6%)	39/399 (9.8%)	OR 0.98 (0.42–2.26)

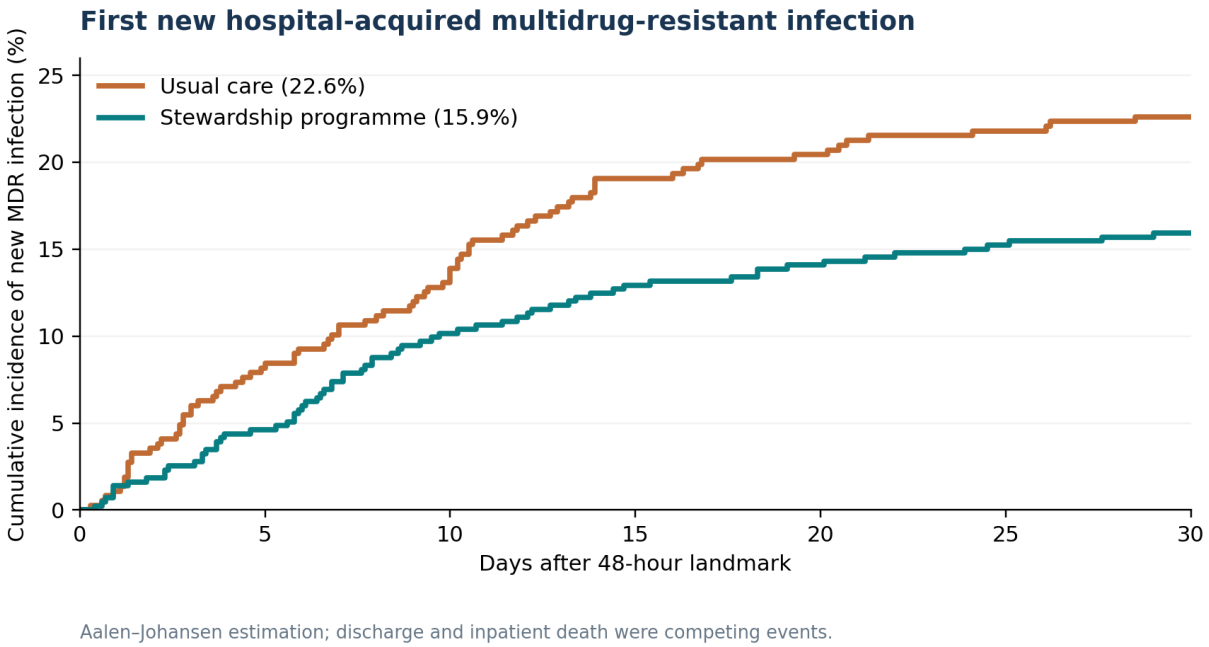
Log-transformed duration models and logistic models are adjusted for measured baseline factors and hospital fixed effects; death models have limited event counts. Readmission denominator excludes those without discharge by day 30.

Table 4. Exploratory ASP strategy comparison with usual care

Care strategy	Participants	New MDRO n (%)	Adjusted cause-specific HR (95% CI)
Usual care	367	83 (22.6%)	Reference
Prospective audit and feedback	145	15 (10.3%)	0.50 (0.23–1.06)
Antibiotic preauthorisation	151	29 (19.2%)	0.88 (0.45–1.73)
Combined audit and preauthorisation	137	25 (18.2%)	0.79 (0.40–1.58)

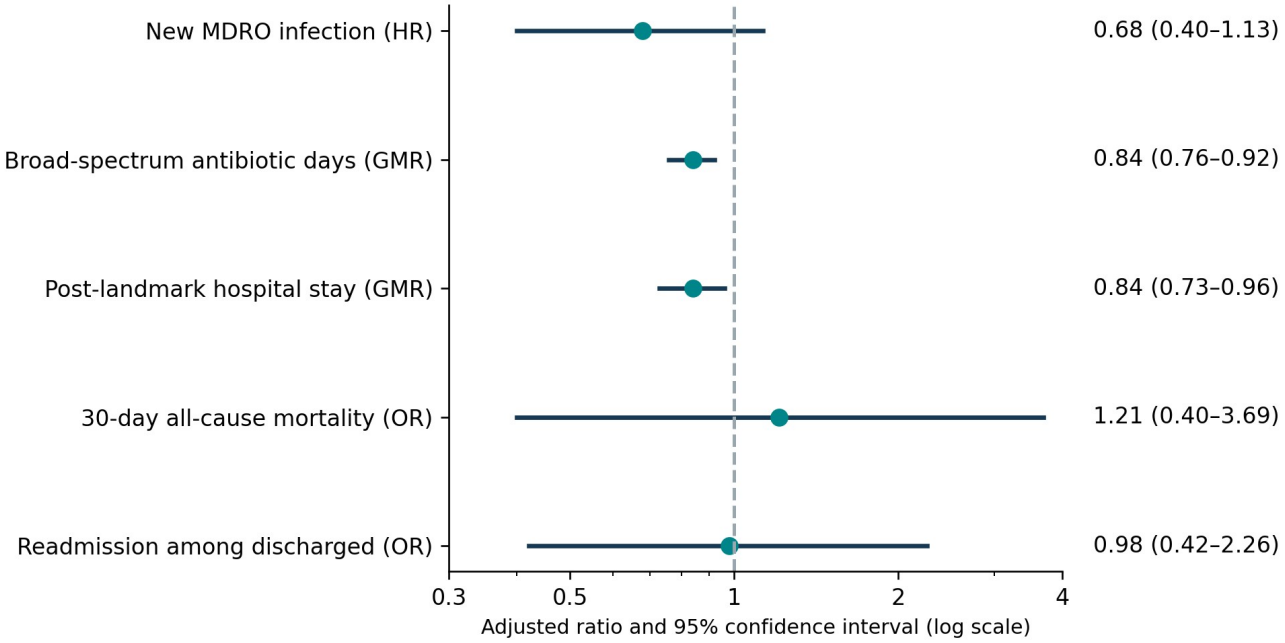
Exploratory, nonrandomised, hospital-stratified models using the primary baseline adjustment set. No adjustment for multiple comparisons; strategy allocation may be confounded.

Figure 1. Time to first new resistant infection.



Aalen–Johansen estimates, accounting for hospital discharge and inpatient death as competing events.

Figure 2. Adjusted patient outcomes.



The outcomes use different estimands—hazard ratio, geometric mean ratio, and odds ratio—and are presented alongside each other for interpretation, not pooled. HR, hazard ratio; GMR, geometric mean ratio; OR, odds ratio.

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