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Sir,

In these guidelines,¹ the authors recommend that ‘A minimum inhibitory concentration (MIC) . . . should be established by a standardized laboratory method to ensure susceptibility.’

What evidence is available that MIC determination improves patient care and outcome or influences the treatment? I know of one presentation at the European Congress for Clinical Microbiology and Infectious Diseases by Walton et al.² from University College London Hospitals who analysed 129 cases of endocarditis and concluded that ‘Antibiotic treatment in endocarditis can be safely chosen on the result of disc sensitivity testing and adjusted on clinical grounds without MIC.’ The same group³ recently published analysis of 125 patients admitted between 1981 and 1999 in whom the MIC had been measured. Their conclusion is inconclusive: ‘The measurement of MIC appears prognostically important in deciding the surgical management of endocarditis.’

Can the experts who formulated the guidelines provide some evidence for their recommendation, please?

References


Endocarditis guidelines: authors’ response

John D. Perry* on behalf of the Working Party of the British Society for Antimicrobial Chemotherapy

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Sir,

Professor Shah¹ has questioned the recommendation² for an MIC determination to be performed on strains isolated from cases of infective endocarditis (IE). When considering the need for MIC determination, one has to consider the nature of the infecting microorganism, the antimicrobial under test and the reliability of any alternative methodology.

It has been stated that disc susceptibility testing is often adequate to determine susceptibility for bacteria causing IE,³,⁴ however the choice of disc susceptibility method is often not stated or discussed. For example, many laboratories in the UK now use the BSAC Standardized Disc Susceptibility Testing Method.⁵ However, guidelines for testing α-haemolytic streptococci, which remain the leading cause of native valve IE, were not available at the end of 2004, which leads to questions as to how such tests might be interpreted. Moreover, the BSAC Working Party² and other authorities,⁶,⁷ recommend therapeutic regimens for α-haemolytic streptococci that vary according to the MIC for the causative strain.

Other examples exist where disc susceptibility methods may not be appropriate, for example, in assessing the penicillin susceptibility of enterococci.⁸ The determination of glycopeptide susceptibility in enterococci remains problematic as shown in recent surveys⁹,¹⁰ and the emergence of staphylococci with intermediate resistance to glycopeptides in cases of IE is likely to provide a further challenge to the diagnostic laboratory in future years.¹¹

Professor Shah cites the report of Walton et al.⁵ who performed a case study to examine the contribution of the MIC to the decision to treat endocarditis surgically. The authors concluded that a moderately elevated MIC of flucloxacinil may be associated with failure of medical treatment, when flucloxacinil is used, even in combination. They also agreed that the MIC of penicillin for α-haemolytic streptococci was useful in determining length of treatment. Such findings support MIC determination for strains causing IE and further studies that examine the relationship between MIC and treatment outcome are warranted.

In conclusion, we accept that for some organism/antimicrobial combinations, disc susceptibility testing may be adequate for guiding therapy, however, our consensus opinion is that our general recommendation to perform MIC testing is justified. IE remains an uncommon disease with high mortality in which the choice and duration of therapy are critical to a successful outcome. Also, if MIC testing is considered by some laboratories to be technically
demanding, the availability of the Etest methodology provides an alternative option for MIC testing.

References


Keywords: antimicrobial resistance surveillance, macrolides, Streptococcus pneumoniae, antibiotic usage

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Sir,

We would like to comment on the article entitled ‘Meta-analysis of bacterial resistance to macrolides’ by Halpern et al.1 The idea of a meta-analysis of published studies on resistance is interesting. However, it does not add much to our knowledge on the prevalence of macrolide resistance. Pooling the results of randomized studies is a method to assess treatment efficacy. However, it does not make sense to use it for resistance, since the reported prevalences in different studies depend almost solely on local factors and the results could be misleading for readers and prescribers in many countries with low prevalence of resistance.

The first problem with this meta-analysis is publication bias. We disagree with the authors on their view that: ‘Given the nature of this topic, it is likely that there would be equal interest in reports of low or high levels of resistance’. Countries with low resistance rarely feel the need—or are even able—to publish their results in peer-reviewed journals, but rather quickly report at national level. In Denmark and Sweden, this is done within 6 months of the end of a calendar year, e.g. data for 2003 were published in June 2004.2,3 However, most peer-reviewed publications take somewhat longer, thus delaying report to prescribers. As mentioned by Halpern et al., the meta-analysis only confirms results that were already available from multinational surveillance studies such as PROTEKT. Moreover, the view of resistance it offers is already out of date since more recent data are available from multinational surveillance projects, e.g. the European Commission-funded European Antimicrobial Resistance Surveillance System (EARSS; http://www.earss.rivm.nl) or PROTEKT, national reports, e.g. DANMAP2 and SWEDRES,3 or abstracts at international conferences. The meta-analysis included very few studies from some regions of the world, raising the possibility that the exclusion criteria used were too stringent. For example, there are many data from Canada, a country with relatively low, but increasing, macrolide resistance. It is unclear why countrywide data on isolates tested centrally by a country with relatively low, but increasing, macrolide resistance. Because of the relatively small number of studies included in the meta-analysis, results could not be presented for each year and each country. Consequently, the meta-analysis only reported on pooled results for 1997–2003, whereas EARSS, as well as national sources,2,3 show that macrolide resistance varied during this period.

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