Treatment of Mycoplasma Pneumonia: A Systematic Review

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abstract

BACKGROUND AND OBJECTIVE: Children with community-acquired lower respiratory tract infection (CA-LRTI) commonly receive antibiotics for Mycoplasma pneumoniae. The objective was to evaluate the effect of treating M. pneumoniae in children with CA-LRTI.

METHODS: PubMed, Cochrane Central Register of Controlled Trials, and bibliography review. A search was conducted by using Medical Subject Headings terms related to CA-LRTI and M. pneumoniae and was not restricted by language. Eligible studies included randomized controlled trials (RCTs) and observational studies of children ≤17 years old with confirmed M. pneumoniae and a diagnosis of CA-LRTI; each must have also compared treatment regimens with and without spectrum of activity against M. pneumoniae. Data extraction and quality assessment were completed independently by multiple reviewers before arriving at a consensus. Data were pooled using a random effects model.

RESULTS: Sixteen articles detailing 17 studies were included. The most commonly selected primary outcome was symptomatic improvement. Nine studies examined M. pneumoniae treatment in CA-LRTI secondary to M. pneumoniae, and 5 RCTs met criteria for meta-analysis. The suggested pooled risk difference of 0.12 (95% confidence interval, 0.04 to 0.20) favoring treatment was not significantly different and demonstrated significant heterogeneity. Limitations included substantial bias and subjective outcomes within the individual studies, difficulty interpreting testing modalities, and the inability to correct for mixed infections or timing of intervention.

CONCLUSIONS: We identified insufficient evidence to support or refute treatment of M. pneumoniae in CA-LRTI. These data highlight the need for well-designed, prospective RCTs assessing the effect of treating M. pneumoniae in CA-LRTI. Pediatrics 2014;133:1081–1090
Community-acquired pneumonia (CAP) accounts for >150,000 pediatric hospitalizations each year in the United States, and there are great disparities in patterns of care and outcomes. Specifically, wide variations in antibiotic prescribing practices exist among physicians, often because the causative agent is not identified. For this reason, recent evidence-based pediatric CAP practice guidelines published by the Infectious Diseases Society of America (IDSA) recommend that children hospitalized with CAP be tested for *Mycoplasma pneumoniae*. *M. pneumoniae* is a common cause of CAP and other community-acquired lower respiratory tract infections (CA-LRTIs), particularly in school-age children and adolescents, but there are large gaps in our understanding of this disease. Prevalence estimates vary from 10% to 40% in pediatric CA-LRTI, and few studies address treatment recommendations. Additionally, the IDSA guidelines target the use of macrolides in CA-LRTI as an area needing additional research.

First-line treatment of children hospitalized with CAP currently includes a β-lactam to treat common causative bacterial agents such as *Streptococcus pneumoniae* and a macrolide to provide additionally treatment of atypical pathogens such as *M. pneumoniae*. Studies of antibiotic use in the pre-IDSA guidelines era showed marked variability in prescription practices, probably because the efficacy of macrolides in the treatment of *M. pneumoniae* remains unclear and treatment recommendations, even in major textbooks, are variable. The available evidence is also conflicting. One small trial suggests that β-lactam use in children with CAP is more cost-effective than macrolides,
following criteria: randomized or observational studies, included children \(\leq 17\) years of age, included \(\geq 10\) children who were diagnosed with CA-LRTI secondary to laboratory-confirmed \(M.\) pneumoniae, data were provided for at least 1 outcome measuring improvement, and the study compared any antibiotic with spectrum of activity against \(M.\) pneumoniae with either placebo or another antibiotic without activity against \(M.\) pneumoniae. Studies that examined children with chronic respiratory illnesses such as cystic fibrosis, bronchiectasis, bronchopulmonary dysplasia, congenital heart disease, or immunodeficiency were excluded, as were those that examined children with nosocomial and congenital infections.

Studies were considered eligible for meta-analysis if, in addition to meeting criteria for inclusion in the qualitative review, information was either provided or could be extrapolated regarding the total number and outcomes of patients with \(M.\) pneumoniae within each treatment group. A subgroup analysis of the initial meta-analysis was performed that included only the RCTs.

**Identification of Trials and Data Extraction**

The principal investigators (E.B. and R.M.) independently screened each citation identified through the initial search strategy as definitely, possibly, or clearly not meeting inclusion criteria. Full-text articles of all studies definitely or possibly meeting inclusion criteria were obtained regardless of primary language of the publication. Reviewers (E.B. and R.M.) independently reviewed each full-text article and then reached consensus before data abstraction. For full-text articles meeting study criteria, reviewers (E.B. and R.M.) extracted data for each study independently and then discussed their findings to form a consensus. Discrepancies were resolved through discussion with the collaborating reviewers (B.A. and S.R.). When necessary, the principal reviewers attempted to contact study authors to verify methods and extracted data, although no responses were received.

**Validity and Quality Assessment**

Studies were assessed and graded for level of evidence based on criteria published by the Oxford Center for Evidence-Based Medicine. An 18-item study quality assessment tool (Supplemental Information) was developed based on recommendations published by the Agency for Healthcare Research and Quality. Items on the quality assessment tool fell within 6 domains: subject selection, study performance bias, detection bias, subject attrition, reporting bias, and financial conflict of interest. Items were marked as factor present, not present, or unclear based on review of the manuscript. Each study was given a total quality score, and for this purpose, labels of “not present” or “unclear” were grouped together. Studies were considered to be unblinded when it was not specifically stated that participants or investigators were blinded. Three reviewers (E.B., R.M., and B.A.) performed individual assessments on all included studies independently and then discussed each until all 3 reviewers were in agreement.

**Statistical Analyses**

Because much of the information relevant to this review was extrapolated from data presented in the included studies, the review authors attempted to calculate statistical significance wherever possible using \(\chi^2\) or Fisher’s exact tests. In cases for which \(M.\) pneumoniae and Chlamydia pneumoniae were grouped together, a “worst-case scenario” was performed by the review authors that assumed all \(C.\) pneumoniae were in the “improved on treatment” group. These were then subtracted from the total. This allowed an interpretation of the results that would ensure that, if a treatment effect was identified, it was not secondary to \(C.\) pneumoniae. For comparison, the analysis was also performed assuming that \(C.\) pneumoniae and \(M.\) pneumoniae responded equally to treatment. Treatment effects for dichotomous outcomes were calculated as risk difference using random effects modeling. Study heterogeneity was assumed for a \(P < .10\) and \(I^2 \geq 25\%\). If the primary outcome (clinical improvement) was assessed in separate methods and separate time periods, each set of outcomes was included in the review and meta-analysis. Meta-analysis was performed by using Comprehensive Meta-Analysis Version 2 (Biostat, Englewood, NJ).

**RESULTS**

The initial database search identified 4667 articles. One other citation was identified via bibliography review. Most articles were excluded based on a preliminary screen of the abstracts, leaving 40 full-text articles to be assessed for eligibility. Before submission, a second search was performed (August 2012–September 2013) to ensure that no recent articles were missed in the review. This search revealed 275 new articles, 1 of which was assessed for full-text eligibility but did not meet study criteria. Of the 4943 total citations reviewed, 16 met criteria for inclusion in the qualitative synthesis. One RCT used different outcome metrics at 2 different times and, for the purposes of this review, was considered 2 separate RCTs. Therefore, 17 studies were included. Four citations were non-English and were translated by the review authors (2 in Spanish, 1 in French, and 1 in German). Eight studies provided
data20,22,27,28 or enabled the review authors (E.B. and R.M.) to extract data15,16,23 specifically comparing treatment with no treatment of *M. pneumoniae* in children diagnosed with *M. pneumoniae* and were included in the quantitative analysis; 5 were RCTs (Fig 1).13,16,20,25 There were 4294 patients enrolled in the 17 included studies (Table 2). Of these, an aggregate 2648 patients could be used to compare an agent treating *M. pneumoniae* with an agent that did not treat *M. pneumoniae* in CA-LRTI. This number excludes comparator arms in which antibiotics of the same class or spectrum were compared and patients in whom therapy could not be determined. One randomized trial provided data on a total of 155 patients but compared macrolide treatment with either a β-lactam or another macrolide and did not provide separate statistics.30

Ten RCTs12,13,17,20,23,24,26,28 and 3 cohort studies5,22,25 compared a macrolide with a nonmacrolide in the treatment of CA-LRTI. One RCT16 compared levofloxacin with a β-lactam, 1 retrospective cohort study21 compared *M. pneumoniae* spectrum with non-*M. pneumoniae* spectrum antibiotics, and 2 case–control studies27,28 compared macrolide treatment in patients with macrolide-sensitive and macrolide-resistant *M. pneumoniae*. Esposito et al20 included upper respiratory infections (URTIs) (199/352, 57%) in addition to CA-LRTI (153/352, 43%) but did not provide a separate analysis, and therefore all are included in this review, as was done previously.10 One study did not report the total number of pediatric patients.17 Studies used various combinations of methods for *M. pneumoniae* detection; 5 used culture of nasopharyngeal specimens,17,23,24,27,28 and 6 used polymerase chain reaction assays of upper airway specimens,12,20,23,25,26,28 No trials were placebo controlled, and a number of studies received pharmaceutical company funding.12,13,16,20,24,25 When we evaluated for overall quality among all studies included, detection bias, which included items such as validity and reliability of study outcomes, was the most frequently identified bias category (12/17 studies, 71%) (Table 3). The most common cause of study performance bias was unblinded participants or observers. More than half of the RCTs (6/11, 55%) reported a potential conflict of interest, which in all cases was pharmaceutical sponsorship with use of that company’s drug in the study.

**Spectrum-Specific Treatment in Pediatric CA-LRTI Secondary to *M. pneumoniae***

Nine studies5,13,16,20,22,23,27,28 provided enough detail to allow a comparison of *M. pneumoniae* spectrum and non-spectrum treatment in pediatric patients with CA-LRTI secondary to *M. pneumoniae* and included a total of 723 such patients (Table 4). In terms of overall clinical improvement, 4 of 5 RCTs found no clinical benefit.13,16,20,23 One retrospective cohort study identified a significant improvement in duration of fever in patients treated with macrolides, although clinically there was a large overlap in fever duration range (3.0–6.8 vs 3.4–7.9 days; *P* = .04), and there was not a significant

**FIGURE 1**
Flowchart for included studies. * One article20 used 2 different outcome metrics at 2 different time periods and was therefore treated as 2 separate studies.
<table>
<thead>
<tr>
<th>Study</th>
<th>Intervention/Comparator</th>
<th>Duration of fever (days)</th>
<th>Relevant Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sterner et al (1967)</td>
<td>RCT/1.5 y</td>
<td>8/87 (EES) vs 17/88 (cephaloridine) failures. Mean fever in those with MP, 2.4 ± 1.9. Mean fever in children with CAP treated with EES, 7.0 ± 3.2 days.</td>
<td>Mean fever in children with MP treated with EES, 7.0 ± 3.2 days.</td>
</tr>
</tbody>
</table>
TABLE 2 Continued

<table>
<thead>
<tr>
<th>Study</th>
<th>Design/Length</th>
<th>Patients</th>
<th>Intervention/Comparator</th>
<th>Relevant Outcomes</th>
<th>Relevant Results</th>
<th>Level of Evidence</th>
</tr>
</thead>
<tbody>
<tr>
<td>Kawai et al (2012)</td>
<td>Retro, case—</td>
<td>N = 29 (1—15 y) with MP</td>
<td>Macrolide sensitive</td>
<td>1. Afebrile 2 d after starting therapy</td>
<td>8/8 (sensitive) vs 6/21 (resistant) became afebrile (P &lt; .001). * Mean DNA load decreased significantly</td>
<td>3b</td>
</tr>
<tr>
<td></td>
<td>control/5 y</td>
<td></td>
<td>MP/resistant MP</td>
<td>2. MP DNA load at 2 d</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Differences not significant or not calculable unless otherwise stated. AZM, azithromycin; CP, Chlamydia pneumoniae; CR, chest x-ray; ESS, erythromycin ethylsuccinate; MP, Mycoplasma pneumoniae; pro, prospective; retro, retrospective; RTI, respiratory tract infection.

* Significance calculated by the review authors.

DISCUSSION

The majority of studies included in our systematic review did not show a significant clinical benefit of M. pneumoniae treatment, while blood cell count, C-reactive protein, and M. pneumoniae culture improvement were identified in a few studies. Despite these findings, the use of M. pneumoniae treatment in children with CA-LRTIs secondary to M. pneumoniae persists in clinical practice. One explanation for this discrepancy may be the lack of high-quality evidence supporting the efficacy of M. pneumoniae treatment in children with CA-LRTIs. Our meta-analysis of RCTs showed a pooled risk difference of 0.12 (95% CI 0.04 to 0.20) (Fig 2). This finding was based on 9 studies that included children with CA-LRTIs secondary to M. pneumoniae. The meta-analysis did not show a pooled benefit of M. pneumoniae treatment, as the majority of studies did not find a statistically significant difference in clinical outcomes between the treatment and control groups. However, the pooled risk difference was statistically significant, with a 95% CI of 0.04 to 0.20. This suggests a small treatment benefit for M. pneumoniae treatment in children with CA-LRTIs secondary to M. pneumoniae, although the clinical relevance of this finding is uncertain. Further high-quality studies are needed to confirm these results and establish the role of M. pneumoniae treatment in children with CA-LRTIs.
monotherapy and children receiving combination therapy with a macrolide.2,51 These studies found a shorter length of stay among children receiving combination therapy. However, testing for *M. pneumoniae* was not recorded in either database and therefore could not be assessed. Macrolides are among the most frequently prescribed classes of antibiotics for respiratory tract infections in children, particularly in conditions for which antibiotics are not clearly indicated.15 Additionally, national guidelines intended to reduce practice variability in the management of CAP in children recommend consideration of macrolide use but state that this is based on limited data.3 Our review found insufficient evidence to support or refute antibiotic use in CA-LRTI secondary to *M. pneumoniae*. This suggests that macrolide use in CA-LRTI could represent an area of opportunity for reducing antibiotic use, which could have a significant impact on health care costs and the development of antimicrobial resistance.32,33

Our review provides novel information in comparison with the previous review on the topic by including more enrolled patients (4294 vs 1912) and more studies (17 vs 7).10 Although the previous review reports insufficient data to draw conclusions, we suggest that the available literature does not currently support treatment of CA-LRTI secondary to *M. pneumoniae*. We examined observational studies, many of which were assessed as having the same level of evidence as the RCTs. In the absence of high-quality RCTs, experts suggest it is reasonable to include cohort and case–control studies in a systematic review, particularly when the quality of evidence is equivocal.34 Additionally, we identified 3 randomized trials that had not been previously examined, 1 of which was assessed as having the highest level of evidence of any included study.12,16,17 Although our review failed to identify clear therapeutic efficacy of macrolides in pediatric CA-LRTI due to *M. pneumoniae*, there are data in support of antibiotic treatment. Military studies from the 1960s, performed on recruits in

TABLE 3 Bias Assessment for Individual Studies

<table>
<thead>
<tr>
<th>Study</th>
<th>Selection</th>
<th>Performance</th>
<th>Detection</th>
<th>Attrition</th>
<th>Reporting</th>
<th>Conflict of Interest</th>
<th>Overall</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sterner et al (1967)</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>4/15</td>
</tr>
<tr>
<td>Garo et al (1988)</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>8/14</td>
</tr>
<tr>
<td>Gomez Campderà et al (1996)</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>7/17</td>
</tr>
<tr>
<td>Gendrel et al (1997)</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>8/14</td>
</tr>
<tr>
<td>Wubbel et al (1999)</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>10/15</td>
</tr>
<tr>
<td>Ferwerda et al (2001)</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>x 3/15</td>
</tr>
<tr>
<td>Principi et al (2001)</td>
<td>—</td>
<td>—</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>7/14</td>
</tr>
<tr>
<td>Kogan et al (2003)</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>8/15</td>
</tr>
<tr>
<td>Esposito et al (2005), study A</td>
<td>—</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>x 6/15</td>
</tr>
<tr>
<td>Esposito et al (2005), study B</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>8/15</td>
</tr>
<tr>
<td>Bradley et al (2007)</td>
<td>—</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>x</td>
<td>5/15</td>
</tr>
<tr>
<td>Kawai et al (2012)</td>
<td>—</td>
<td>x</td>
<td>x</td>
<td>—</td>
<td>—</td>
<td>x</td>
<td>6/15</td>
</tr>
</tbody>
</table>

—, substantial bias not identified within the domain; x, substantial bias identified within the domain.

TABLE 4 Studies Comparing Spectrum With Nonspectrum Treatment of Children With Acute Respiratory Infection With *M. pneumoniae* Grouped by Outcome Term

<table>
<thead>
<tr>
<th>Study</th>
<th>Time Frame</th>
<th>Outcome</th>
<th>Treatment (n)</th>
<th>Comparator (n)</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Improved</td>
<td>Total</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Matsubara et al (2009)</td>
<td>&lt;3 d</td>
<td>Fever</td>
<td>43</td>
<td>5</td>
<td>2</td>
</tr>
<tr>
<td>Kawai et al (2012)</td>
<td>2 d</td>
<td>Fever</td>
<td>7</td>
<td>8</td>
<td>6</td>
</tr>
<tr>
<td>Lu et al (2008)</td>
<td>N/A</td>
<td>Fever</td>
<td>4.9 ± 1.89</td>
<td>5.53 ± 2.22</td>
<td>.04</td>
</tr>
</tbody>
</table>

Clinical Improvement at >5 d

<table>
<thead>
<tr>
<th>Study</th>
<th>Time Frame</th>
<th>Outcome</th>
<th>Treatment (n)</th>
<th>Comparator (n)</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Improved</td>
<td>Total</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Harris et al (1998)</td>
<td>1 m</td>
<td>Overall</td>
<td>49</td>
<td>49</td>
<td>88</td>
</tr>
<tr>
<td>Esposito et al (2005), study A</td>
<td>5 m</td>
<td>Recurrent</td>
<td>56</td>
<td>56</td>
<td>61</td>
</tr>
</tbody>
</table>

* Numbers often extrapolated by the review authors.
* b Includes P values calculated by the review authors.
* a Numbers often extrapolated by the review authors.
* d Wild-case scenario with all 27 C. pneumoniae patients placed in the treatment improved group and then excluded.
basic training, found that recruits with atypical pneumonia more often had serologic evidence of *M. pneumoniae* infection (identified as Eaton's agent at the time) than asymptomatic controls. Additionally, in a randomized, double-blind, placebo-controlled study among 300 recruits with atypical pneumonia due to *M. pneumoniae*, treatment with a tetracycline was found to decrease the duration of a number of clinical signs and symptoms faster than placebo. These seminal studies do not necessarily represent the typical clinical situation encountered by pediatricians today, given their young adult, homogenous population, nor can they account for mixed infections or modernized diagnostic testing. Nonetheless, these studies describe principles of study design and conduct in this area that can serve as a template for pediatric investigations.

One of the strengths of meta-analysis is in the process of evaluating the available literature for heterogeneity. It is often difficult to find a middle ground between throwing out all summary estimates and inappropriately combining studies, particularly in pediatrics, where the paucity of high-quality RCTs is acute. We provided a summary statistic for 1 broad study question, but we noted significant study heterogeneity that we could not resolve with subgroup analysis. Therefore, our interpretation of the entirety of the statistical testing associated with our meta-analysis is that despite multiple

<table>
<thead>
<tr>
<th>Study name</th>
<th>Risk Difference (95% CI)</th>
<th>Weight (%)</th>
<th>Risk Difference (95% CI)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bradley et al.</td>
<td>0.06 (-0.13 – 0.25)</td>
<td>18</td>
<td></td>
</tr>
<tr>
<td>Esposito et al. (A)</td>
<td>0.23 (0.15 – 0.31)</td>
<td>28</td>
<td></td>
</tr>
<tr>
<td>Esposito et al. (B)</td>
<td>0.02 (-0.15 – 0.19)</td>
<td>19</td>
<td></td>
</tr>
<tr>
<td>Harris et al.</td>
<td>0.00 (-0.15 – 0.15)</td>
<td>22</td>
<td></td>
</tr>
<tr>
<td>Sáez-Llorens et al.</td>
<td>0.00 (-0.26 – 0.26)</td>
<td>13</td>
<td></td>
</tr>
<tr>
<td>Overall effect</td>
<td>0.12 (-0.04 – 0.20)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*FIGURE 2*

Meta-analysis of randomized trials comparing spectrum and nonspectrum treatment of *M. pneumoniae* in children. Studies were weighted by size and degree of heterogeneity. The overall effect (diamond) represents the pooled risk differences of the 5 studies. The confidence interval crosses 0.00, suggesting a lack of statistical significance.

*FIGURE 3*

Funnel plot of standard errors by risk difference for all studies in the second meta-analysis. Publication bias is evident against small studies that show a treatment effect.
studies, a clear treatment effect has not emerged. Furthermore, the study heterogeneity lends credence to the idea that a large RCT is necessary to provide guidance to pediatricians on this clinical question.

There are several limitations to our review, most related to the quality of the available data on the topic. First, most studies contained substantial bias or financial conflict of interest. We attempted to account for this through in-depth quality assessment and reporting to allow a critical interpretation of study results. In particular, detection bias was commonly identified in the studies comparing spectrum with nonspectrum therapy in patients diagnosed with *M. pneumoniae*. This bias most often represented lack of blinding of investigators or participants or lack of a reliable outcome measure (i.e., clinical improvement). This quality issue could potentially skew study results towards or away from the null hypothesis.

Second, we were unable to extract data on outcomes for all studies, reducing the number of articles that could be included in the meta-analysis and potentially causing publication bias. This limitation should be balanced against the fact that we abstracted quantitative data from more studies than previous meta-analyses on this subject.

Third, our assessment of treatment effect was limited primarily to subjective outcome measures such as symptomatic improvement or resolution. This limitation is inevitable given the primary endpoints used in the included studies, but our bias assessment specifically addresses the subjective nature of the data.

Fourth, *M. pneumoniae* testing modalities have changed dramatically over time, with variable testing characteristics and lack of overall consistency. This limits the uniformity of the patient population included in our review. Furthermore, co-infection with a pathogen in addition to *M. pneumoniae* is common but rarely identified via clinical testing, making it difficult to draw specific conclusions about therapeutic effects.

Finally, no study included in our analysis took into account the duration of illness before the start of antibiotic therapy. It is possible that the timing of intervention relative to the start of symptoms is related to the outcome.

**CONCLUSIONS**

Our systematic review provides insufficient evidence to support conclusions about the efficacy of macrolide treatment of CA-LRTI due to *M. pneumoniae* in children. Fever duration may be decreased, but the clinical impact of this effect is unclear, and we identified few high-quality studies to support it. Our review represents the most comprehensive evaluation of treating *M. pneumoniae* in children with CA-LRTI to date. The findings from this review highlight the need for high-quality, prospective studies to assess the impact of antibiotic therapy to treat CA-LRTI caused by *M. pneumoniae* in children. These studies should specifically address the potential for confounding mixed infections, timing of intervention relative to symptom onset, and testing modalities that include a combination of serology and polymerase chain reaction assays.

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Data Supplement at:
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