Chlorhexidine and newborn omphalitis and mortality

Authors’ reply

Stephen Hodgins says that cord application of chlorhexidine protects infants against omphalitis equally after birth at home or in hospital, and that we were incorrect to say that it was not effective for infants born in hospital.

The first point is well taken. The 2015 Cochrane review\(^1\) suggests that risk ratios (RR) for omphalitis were similar in hospital (RR 0·48, 95% CI 0·28–0·84) and community trials (0·48, 0·40–0·57). The 2013 Cochrane review\(^2\) included nine relevant hospital studies of chlorhexidine application. For omphalitis, one study\(^3\) suggested a benefit versus dry cord care (0·28, 0·06–1·35); another study\(^4\) found no benefit versus alcohol (2·77, 0·12–66·49); and another\(^5\) found no benefit over hydrophobic gauze (1·36, 0·55–3·36). Imdad and colleagues\(^2\) acknowledged the complete statement of classification of bias risk because of imprecision. Together, these trials included 809 infants.

Hodgins’ second point interprets a statement we made, quoting the WHO recommendation,\(^6\) and saying that the Cochrane reviews,\(^1, 2\) a meta-analysis,\(^7\) and the two trials in The Lancet Global Health\(^8, 9\) had not supported an effect after hospital births. Our Comment\(^1\) begins with, and is permeated by, an emphasis on newborn mortality.

As Hodgins says, community-based trials\(^10\) suggest that the effect on neonatal mortality was similar—even when non-significant—when infants were born in hospital. We do not take issue with this. Indeed, we acknowledge the complete statement by Szazawal and colleagues:\(^10\) “The findings in our study suggest that use of chlorhexidine for the reduction of omphalitis is justified, but in an African setting there is insufficient evidence to promote this intervention to reduce neonatal mortality.”

The emphasis of our Comment was on whether cord application of chlorhexidine might achieve appreciable reductions in newborn mortality in low-income and middle-income countries. We said that two recent trials\(^11, 12\) did not provide evidence to alter current WHO guidelines,\(^7\) which recommend it for infants born at home in environments with neonatal mortality rates of more than 30 per 1000 livebirths. We stand by this.

DD does not work with but has been a co-author of six publications in the past 5 years with Robert Black, a named author of the paper from Tanzania. The publications were produced by large working groups and DD and TC both either contributed data from their research programmes or were members of a distributed expert group.

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