Endnote

Although few readers may have noticed, in the new **ecp** Presentation Guidelines, found in the Guidance for Authors section, we have begun to ask authors to convert odds ratios (ORs) to relative risks (RRs) whenever possible. (This conversion is not possible when the data are from a case–control study because the absolute event rates are not known.). In this Endnote, I'd like to review the background of this decision.

I have a long history of confusion about the topic. As a student, I was taught that ORs that had been developed from case–control studies were a good approximation of the RR. There was something about a "rare disease assumption," however. I can remember playing around with the two measures in a spreadsheet, observing how they diverged and struggling with the question, "Which one is right?" It wasn't until I became a junior faculty member that one of my mentors pointed out that both are right—they are just different (see **Primer**).

But the fact is that most of us think in terms of probabilities. Few of us can quickly interpret odds, much less ratios of odds. The result is that most of us have to default to interpreting an OR as an RR. I first realized that the interpretation could become the principal "sound bite" for an investigation in a *New England Journal of Medicine* editorial about the risk for appendiceal rupture in managed care.[1] Arnold Relman (an editor I greatly admire) interpreted an OR of about 1.2 and 1.5 as "Rupture was 20% more frequent among patients covered by private fee-for-service insurance and approximately 50% more frequent among those without insurance."[2] Because appendiceal rupture is common (30% of all cases), the increased probability is closer to 10% and 30% (i.e., RR approximately 1.1 and 1.3).[3] Not long after, a *JAMA* editorialist interpreted an OR of 0.33 as meaning, "White faculty were 3 times as likely [to be promoted] as black and Hispanic faculty," when the probability was actually twice as likely (i.e., RR approximately 0.5).[4, 5] And more recently, a finding of 40% less referral for cardiac catheterization (OR, 0.6) was widely reported in major print and electronic media when the probability was 7% less (RR, 0.93).[6, 7]

Is this quibbling? Or do these differences matter? In an era when researchers devote so much effort to exact *P* values and 95% confidence intervals (sometimes with absurd precision), it seems reasonable to try get the main effect right. And part of getting it right means making it easy for readers to understand.

ORs are always more extreme (i.e., farther from 1) than the associated RR (see **Primer**). The magnitude of the difference between the measures increases as the baseline probability of the event under consideration increases. One of the most extreme examples is the data from the article by Deshpande and Gazmararian in this issue of **ecp**.[8] In this study, breast-feeding was common among women who did not have access to postpartum breast-feeding assistance (75% of the women breast-fed). However, almost all the women (97%) who had access to postpartum breast-feeding assistance breast-fed. The authors quite appropriately used logistic regression to control for several factors that might confound the relationship (e.g., age, socioeconomic status). The logistic model produces an OR, and in this case it's a big number: 15. The number itself is correct. Had the authors reported it, however, I believe that most readers would either infer that the effect was larger than it was or simply be confused. Instead, the authors performed the conversion and reported an RR of 1.3, which I believe is much more likely to be correctly interpreted by readers.

And that's one of our primary goals at **ecp**.

References

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