cinema” had a higher prevalence than that in the general US population, and not to study whether “movies influence smoking habits of their viewers,” as other authors have done. In order to answer our question, we had to exclude films that took place “outside the United States, in earlier times… and smoking by United States citizens” because they obviously do not portray the lives of US citizens in the United States.

Equally as important, we introduced objective methodology to a difficult subject. In their zeal to demonstrate a high prevalence of smoking in the movies, the authors from the same institution have repeatedly used arbitrary definitions such as “no smoking signs,” implied or actual tobacco consumption, talking about tobacco, and tobacco paraphernalia. As such, they have demonstrated an astonishingly high prevalence of smoking in all movies, and an even higher prevalence in R-rated movies. These data must unfortunately be dismissed since they are neither scientific nor objective.

Furthermore, the letter of Glantz and Polansky has several major errors. First, we did not study “a select group of films from the 1990s.” We studied every movie that met our inclusion criteria. Science fiction and animation (not “fantasy”) movies have an even lower smoking prevalence than live action and non-science fiction movies. If included, the prevalence of smoking in all movies in our study would have been even lower and, therefore, not statistically significant, exactly the same as it is now. Similarly, since none of these movies are rated R, they would not have changed the findings for R-rated movies in our study. Obviously, the net effect would still be exactly the same. These movies would not have changed either our findings or our conclusions.

Second, “To obtain [our] target sample size, [we] collected data in R-rated films (from January 1) 1996–until December 31) 1998” by watching these movies in the theaters, but also continued to watch R-rated movies on video dating back to 1990, even though we had reached the necessary sample size. We collected data from virtually every PG and PG13 movie made in the 1990s that met the inclusion criteria, because there are so few of these movies released each year. Therefore, “this sampling strategy [could] underestimate smoking in PG13 films” does not make any sense since our study includes virtually all PG and PG13 movies of the 1990s, and R-rated ones from every year of that decade, especially from the later 1990s.

Finally, excluding the movie Titanic does not make our study “impossible to extrapolate from.” Our study simply states that the prevalence of smoking in contemporary American movies about American life in the 1990s is the same as that in the general US population in the 1990s. Titanic sank in 1912 when everybody smoked. It is improbable that James Cameron accepted payment from the tobacco industry while letting go of his own salary, to portray his two top characters smoke an unidentifiable cigarette.

The movie studios, run by only the single dictum of “profits,” pick up many movies in film festivals such as Cannes, Sundance, or Telluride for distribution without having any control whatever over their production or contents. Many of these movies were credited in the study by Polansky and Glantz as “studios,” although these studios were only the distributing companies. For example, their data lists The Quiet American (2002) and Tadpole as Disney movies. Although distributed by Buena Vista, there is no way that these movies should be considered studio products. The Quiet American was made by independent German, Australian, and US companies, and Tadpole was made by the Independent Film Channel! It is as if one called a washer a “Home Depot” washer simply because that is where it was purchased. They also failed to distinguish the English-speaking foreign movies in that study. Furthermore, in their studies, SKG is counted as “independent.” Although debatable, Steven Spielberg cannot possibly be placed in the same category as Todd Solondz. It is because of errors like these that our study is important since it deals with the subject matter scientifically and without bias.

Glantz et al concluded that “smoking in movies has returned to levels observed in 1950.” If the prevalence of smoking has in fact been on the rise in recent years, it is simply due to the rise of the independent movies. Other authors have simply not paid attention to the fact that, as most experts agree, starting with Pulp Fiction (1994) and the parallel rise to power of Miramax Pictures, independent movies have become as important, if not more so, than the studio movies. As any researcher knows, the danger lies when one’s personal beliefs and biases influence the way that one collects and interprets data.

As pulmonologists, we are also greatly disturbed by smoking in American movies especially since so much of it is completely unnecessary. However, there is enough evidence that, if collected and interpreted correctly, obviates the need for arbitrary and forced conclusions that are cluttering the so-called scientific journals with their authors’ prejudices.

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REFERENCES

Lung Resection in the Elderly

To the Editor:

The article by Mery et al in a recent issue of CHEST (July 2005) is an important contribution indicating that minimal resection in patients with stage I and II lung cancer may be as
It's in the Definition

To the Editor:

We applaud Carden and colleagues for their excellent review of tracheomalacia (March 2005).1 We would, however, like to clarify the definition.

Tracheomalacia, according to Taber’s Cyclopedic Medical Dictionary is, “softening of the cartilages of the trachea.”2 This is distinct from dynamic airway collapse (DAC), which is due to excessive laxity of the posterior membranous wall with an intact integrity of the cartilaginous support.3 This results in tracheal collapse during expiration but preserves airway caliber during inspiration. Although the review by Carden et al4 suggested the abnormality in tracheomalacia was in the pars membranacea with structurally normal cartilage, they referred to a study wherein cartilage from one patient with a presumed case of tracheomalacia was compared with that from the airway of a healthy person.4 Distinguishing tracheomalacia from DAC is essential as DAC appears to be a benign condition, whereas tracheomalacia tends to progress.3,5

Therefore, drastic interventions such as surgical splinting, placation, or even tracheal stenting must be reconsidered in individuals with DAC. In such situations, conservative measures such as therapy with bronchodilators, treatment of underlying infection, or perhaps treatment with positive airway pressure devices should be considered the primary therapy. This paradigm should minimize the number of unnecessary procedures performed.

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REFERENCES


To the Editor:

We thank Drs. Park and Edell for their kind words and suggestions as they relate to our article on tracheomalacia.1 It is correct that some dictionaries may define tracheomalacia as a weakness of the supporting cartilage, but most clinicians and the scientific literature in general are not in agreement with that proposal: when searching the term tracheomalacia in a medical literature search rather than a dictionary, this distinction is not followed.

Contrary to the assumption outlined in the comments, there is actually no need to do so either. It is the opinion of the authors that the article quoted by Park and Edell2 is insufficient to offer...