Commentary

Gut Check: Can Cost-Effectiveness Analysis Help Eliminate Gastric Cancer in Asia?

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Gastric cancer represents a unique public health opportunity and a challenge, particularly for developing countries. The opportunity stems from the remarkable advances in knowledge that we have about this disease, almost all of which have been acquired within the last generation. The causal agent of nearly half of all gastric cancers in developing countries, *Helicobacter pylori*, is known, along with the general mechanism for transmission (poor sanitation; ref. 1). Even better, the infectious agent is easily detected by a variety of means and is highly treatable. Moreover, in cases, in which the *H. pylori* causes irreversible gastric-mucosal changes in the host (i.e., those that are part of the carcinogenesis pathway), a relatively effective means for secondary prevention is available, allowing clinicians a chance to detect and treat cancers at an early, highly curable stage. In theory, with the knowledge and tools we have in hand, we could drastically reduce or even eliminate gastric cancer as a cause of death in the world and, crucially, in the developing world.

Why not get on with it then and eliminate this deadly disease? Unfortunately, as is the case with many other public health issues, two important issues stand between theory and practice: economics and the practice of medicine. Although the tests are relatively inexpensive on an individual basis, population-wide screening could be enormously costly. Health planners for developing countries have far more needs than resources to address them, and even the most effective interventions must be economically feasible to implement. The other issue relates to a way most preventive medicine is practiced worldwide—at the level of the patient and his or her health care provider. The fact that there are several ways to attack the problem of gastric cancer means that letting the ''system'' decide—that is, letting clinicians attack the problem using their own devices—will almost certainly lead to a hodgepodge approach that is neither as efficient nor as effective as a coordinated campaign. This is where cost-effectiveness analysis comes in.

Using a decision model populated with data from their own research and other studies of gastric cancer and screening, Lee and colleagues find that a strategy of primary prevention—identifying *H. pylori* carriers with ^{13}C breath testing followed by triple therapy with esomeprazole, amoxicillin, and clarithromycin—was less expensive and slightly more effective than secondary prevention using a screening strategy using serum pepsinogen to identify persons with atrophic gastritis followed by surveillance endoscopy (2). The results seem to hold under a wide range of assumptions regarding key variables for the model. For example, an important issue for primary prevention is the reinfection rate after triple therapy. The authors assume a reinfection rate of 1% per year for their base-case analysis. Primary prevention compares favorably to secondary prevention, even if the reinfection rate after therapy exceeds 2% per year; although at higher levels of reinfection, primary prevention is not necessarily both less costly and more effective than secondary prevention. Both primary prevention and secondary prevention are cost effective compared with no program, using a widely cited but debated US threshold of $50,000 per life year gained, but that ratio is almost certainly too high for countries with more limited resources.

There are a few limitations of Lee and colleagues' analysis that are worth noting. First, the authors' analysis does not include benefits due to reduction of bleeding and nonbleeding duodenal ulcers and relief of a proportion of nonulcer dyspepsia when *H. pylori* is eradicated. Including these benefits would tip the scales further in favor of primary prevention. Second, the analysis does not include costs for development and implementation of either strategy. Implementation costs include such things as educating health care providers about the screening program (for example with clinical practice guidelines), ensuring adequate availability of ^{13}C breath testing and/or upper endoscopy screening, and introducing programs to maximize patient compliance. Such programs are essential for success but add to the overall cost of any program and thus affect its cost effectiveness. Finally, some potential options were not considered, most notably chemoprophylaxis after *H. pylori* antibody testing and one-time endoscopy screening in *H. pylori* carriers at an age in which the prevalence of gastric cancer increases. There is also no real reason to incur the cost of branded esomeprazole for triple therapy when generic omeprazole is available.

The question policy makers face when confronted with this information is what to do. They are constrained by a limited health budget and political issues that may limit the choices they can make, such as patient-advocacy groups or physicians with a professional interest in particular technologies. In accounting for uncertainties that are inherent in all decision models, policy makers face a choice: do they implement a program after the recommendations of the model, choose a gastric cancer reduction program based on some other method, or decide that their health priorities lie elsewhere? For the first two choices, cost effectiveness has the great advantages of maximizing on efficiency (least costly way to attain a health goal) and of transparency: if you don't believe the outcome, the path that took you to that outcome can be retraced and challenged at any point. Political realities and feasibility issues (e.g., availability of trained endoscopists) may force a compromise that isn't as ''pure'' as the optimizing strategy identified by the model. The much more difficult issue is whether decision-makers are willing to make a coordinated, visible, and very costly investment based on the results of the model and related evidence. Here, the relative value of gastric cancer prevention will be scrutinized relative to other health needs. Is $17,000 per life year gained for primary prevention a good investment compared with other programs competing for these dollars? Is it ''worth'' cutting other programs or

Cancer Epidemiol Biomarkers Prev 2007;16(5):873–4

Received 3/28/07; accepted 3/28/07.

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doi:10.1158/1055-9965.EPI-07-0286

raising revenues (the only two choices available under global budgeting) to pay for a new public health program for gastric cancer prevention? Here is the place where researchers who conduct cost-effectiveness studies often become uncomfortable, because it implies taking an advocacy role, possibly in a political arena. I believe it is a necessary role, however, for two reasons: first, most policymakers have limited familiarity with cost-effectiveness methods; second, no one knows better than the designer of the strengths and limitations of a decision model. Taking the time to advocate for cost-effectiveness analysis in general while at the same time having an open discussion of the strengths and limitations of the particular application would go far toward addressing what is still widespread skepticism of economic evaluation of health care technology. Efforts to reduce gastric cancer–related mortality are laudable; addressing the problem efficiently is crucial.

Unlike many discoveries in science, the results of cost-effectiveness analyses are not fixed in time. As circumstances in the environment change, so might the most efficient strategy. This is particularly true for *H. pylori* and gastric cancer in Asia. Most notably, as countries move from developing to developed nations, the population prevalence of *H. pylori* will decline, as perhaps will other risk factors for gastric cancer. As the authors note, gastric cancer is becoming less common in Asia. Will a time come when the disease is rare enough that economic analyses do not justify population-wide screening? Perhaps the authors should not put their model too high up on their shelf.

References

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