Letters to the Editor

Nonrandom Selection and the Attributable Cost of Surgical-Site Infections

To the Editor:

In the April 2002 issue of *Infection Control and Hospital Epidemiology*, Hollenbeak et al.[1] claimed to control for selection bias in a model of the additional cost of surgical-site infection. The authors developed a two-stage model to additionally control for a variable that was derived from the risk factors for wound infection. They argued that these risk factors would also independently increase cost, thus acting as true confounders. Presumably, the purpose of including this risk index on the right-hand side of their regression model was to minimize confounding, which Haley describes as severity of illness bias.[2]

We understand selection bias to occur only when common factors determine participation in the research and the likelihood of acquiring the disease or outcome. For example, selection bias might occur in case–control studies to explore the additional cost of hospital infection when cases are excluded because no match can be found for them, for all variables, from the controls. Because all patients were included in the data set used by Hollenbeak et al., selection bias should not be a problem. We suggest that they controlled for severity of illness (to be welcomed), not selection bias.

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The authors reply.

Drs. Birrell and Graves raise an important point about the language that is used to describe selection bias. As they mention, one form of selection bias can occur when common factors determine participation in a study. More generally, selection bias may arise whenever there is a systematic mechanism that determines both outcomes and study participation or the distribution of treatment. For example, dropouts from randomized trials may lead to selection bias if there is a variable that is related to both the decision to drop out and the treatment effect. In this case, the selection mechanism is self-selection, and the selection bias would be observed in the estimated treatment effect.

In the case of nosocomial infections, we hypothesized that estimates of the attributable cost of surgical-site infections may suffer from selection bias, and proposed a model that would allow us to test for its presence. We framed our discussion in terms of selection bias because there is an underlying mechanism that selects some patients to develop surgical-site infections, and the variables that drive the selection process are correlated with costs. For example, obesity and diabetes have both been shown to increase a patient's risk for surgical-site infections, and have been associated with increased costs independent of infection. The selection mechanism we hypothesized was not standard; it goes without saying that infections are not assigned based on self-selection. However, there is an underlying natural process that results in a systematic distribution of infections; therefore, it is appropriately modeled as a selection mechanism, and its impact on the treatment effect is appropriately called a selection bias.

Note that the selection bias we hypothesize is not due to the systematic deletion of observations, as Drs. Birrell and Graves suppose, but rather because risk factors for infection have two effects on costs: a direct effect, which can be controlled by including the risk factor as a covariate, and an indirect effect, which inflates the treatment effect or the coefficient on the binary infection indicator. Simply controlling for the risk factor as a confounder in, for example, a regression context would address the direct effect but would not mitigate the indirect effect. The purpose of including the inverse Mills ratio was not to minimize confounding, but rather to absorb the selection effects.

It is important to contrast this notion of selection bias with severity bias, which arises when patients who develop infections are "sicker" than patients without infections, even before they developed an infection. It is hoped that it is clear why we described the effect for which we attempted to control as a selection bias and not a severity bias. The variables that contribute to the selection mechanism for infections are not necessarily related to disease severity, although they could be. If they were, disease severity could be included in the first stage probit regression as well as a covariate in the second stage regression.

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Surveillance of Nosocomial Surgical Wound Infections: A Few Suggestions

To the Editor:

The July 2002 issue of *Infection Control and Hospital Epidemiology*

contained several articles and an editorial on surveillance for nosocomial surgical-site infection (SSI). I found these fascinating, as I spent many years directing the nosocomial SSI surveillance program at the Vancouver General Hospital in British Columbia, Canada. The Vancouver General Hospital is a large tertiary-care facility with many specialized surgical units. Detailed studies were performed in spinal surgery and published.[1] The following comments on the problems encountered in conducting surveillance are based on experience gained in attempting to monitor many types of surgery.

Standard surveillance methods in North America are based on the publications of the Hospital Infection Program (currently the Division of Healthcare Quality Promotion) of the National Center for Infectious Diseases and the National Nosocomial Infections Surveillance (NNIS) System, Centers for Disease Control and Prevention.[2-5] The NNIS System regularly publishes[4] infection rates based on information provided by a selected group of hospitals. Surgical procedures are classified according to the *International Classification of Diseases, 9th Revision, Clinical Modifications* (ICD-9-CM).[6] We reported[1] the problems encountered in attempting to use their values in a tertiary-care hospital where many complicated spinal instrumentation procedures are performed. The problems were related to the use of the ICD-9-CM classification with only the two categories laminectomy and fusion. This groups all fusion procedures together, regardless of whether instrumentation is involved. Adjustments present in the NNIS System account for long procedures and patients with elevated American Society of Anesthesiologists scores. We found these insufficient in our setting. Similar difficulties can be noted in other types of surgery and will affect specialized units performing mostly complicated procedures. We have always refused to calculate surgeon-specific SSI rates.

The NNIS System, although it defines incisional (superficial and deep) and organ–space infections,[3] does not report separate values. The definitions of superficial and deep infections are based on abdominal surgery where there is a well-defined fascial layer between the skin and the peritoneal cavity. The interpretation of the definitions becomes more difficult in other areas of the body such as the back or sternum, where bone may be near the body surface. Superficial wound infections can be difficult to distinguish from noninfected wound dehiscence. Rates of superficial infections not only vary with the observer, but are often based solely on office notes, as these infections are usually seen in the surgeon's or family practitioner's office. Because of these wide variations, we did not find the superficial wound infection rates to be of value in monitoring surgical practices. Deep wound infections are sometimes difficult to distinguish from superficial wound infections, but are usually easily identified and show less fluctuation in values. It would be useful if the NNIS System would publish separate rates for superficial and deep surgical wound infections. Organ–space infections are defined as those involving any organ or space other than the incision opened or manipulated during the operative procedure and, in our experience, are rare.

The difficulties of performing postdischarge surveillance as described in the study by Avato and Lai[7] and in the editorial[8] by Platt were similar to our experience. Others have also reported that the use of postdischarge surveillance increases rates, especially those of superficial infections.[9,10] A follow-up period of 30 days is prescribed for patients who have had non-prosthetic surgery, and a 1-year follow-up for those who have had foreign body material implanted.[3] When instrumentation and non-instrumentation procedures are combined, it is uncertain what the follow-up period should be. The definition of a prosthesis can be difficult. Are screws, plates, and spinal instrumentation devices considered to be prostheses? Our experience is that postdischarge follow-up detects mostly superficial incisional infections, whereas deep infections are detected by inpatient surveillance or when the patient is readmitted to a hospital.

These problems resulted in a review of the objectives of our surveillance program. Although there may be many scientific and academic reasons for performing surveillance, the most basic purpose is to obtain information that can be used to reduce the incidence of these infections to the lowest possible level and to reduce morbidity and mortality. The significance of an infection is directly related to the procedures required to manage it. Infections managed in the surgeon's office or outpatient department are usually superficial and may result in increased pain and suffering and cause the patient to remain absent from work for an extended time. They rarely, if ever, influence the final surgical outcome or endanger the life of the patient. One must consider whether the commitment of large amounts of time and money to their detection is economically justified. Patients readmitted to the hospital because of infection, especially infection resulting in incision and drainage or debridement procedures performed in an operating room, are more likely to have significant sequelae. Perhaps it is time to consider classifying infections as those treated out of the hospital and those requiring readmission, instead of superficial and deep. This would be a useful adjunct to current practices and would not require a great deal of resources. An unpublished pilot study of spinal surgery that compared the current NNIS System with a simple classification based on treatment revealed a high correlation between deep infection and readmission to a hospital.

There may be limitations to this suggestion, but it is doubtful that they are any greater than those of the current classification system. The different criteria used by surgeons to readmit patients and to perform incision and drainage procedures will influence the infection rate; however, the variation is no greater than that noted when comparing the classification of a wound infection by different individuals using the current system. Ideally, one individual should examine and classify all wound infections, but few facilities can afford such a program. One advantage to this suggestion is that hospital readmission and incision and drainage procedures performed in an operating room are easily defined and detected indicators. Patients readmitted to a hospital different from the one where the surgery was performed can be detected by a simple mail-back infection survey sent to the surgeon's office. We have used a computer-generated list from the operating room for this purpose. Compliance has been good if the surgeon has to indicate only

whether the patient required hospital readmission and to what facility.

Surgical wound surveillance is becoming more important in the current situation of increasing antibiotic resistance by organisms. Whatever system is used needs to be able to accurately and quickly detect significant changes in infection rates. A common experience is that an outbreak of SSI may be detected by those caring for the patient before it is evident in the surveillance data. This underscores the importance of having infection control practitioners in regular contact with the surgical wards and the surgeons' offices. It also means that the system used must include simple indicators that are easily evaluated. Hospital readmission and surgical procedures for infection are two easily monitored indicators.

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We agree with Dr. Roberts that the current classification system based on the *International Classification of Diseases, 9th Revision, Clinical Modifications* (ICD-9-CM), adjustments by the National Nosocomial Infections Surveillance (NNIS) System for length of procedures, and American Society of Anesthesiologists scores have limitations for the surveillance of surgical-site infections (SSIs) for most surgical procedures.

Our study[1] involved a detailed and extensive surveillance of SSIs associated with coronary artery bypass grafts. We compared our rates with those reported by the NNIS System and found that we had a higher rate of SSIs because of our comprehensive program for postdischarge surveillance for SSIs. However, most of the deep infections were diagnosed before discharge and other serious infections related to the chest or harvest sites required readmission.

Dr. Roberts suggests classifying SSIs into those treated out of the hospital and those requiring readmission. He reasons that because superficial wound infections rarely cause significant sequelae, we should focus our limited resources on identification of infections that may result in morbidity and mortality (ie, infections that are identified during hospitalization and those that result in readmission). Our study confirmed that a great deal of time was expended by dedicated infection control practitioners in the collection and analysis of data including infections postdischarge. We agree with Dr. Roberts that such expenditure of time and money might not be justified for one surgical procedure, and that a system should be developed to quickly detect significant changes in the rates of infection. He suggests a system that includes simple indicators that can be easily monitored, such as hospital readmission and surgical procedures performed because of infection. Even with his suggested system, each institution will need to make prudent decisions to allocate its limited resources to a few surgical procedures at one time, especially procedures that are associated with a higher risk of SSIs.

Improving communications between infection control practitioners and surgeons in all disciplines in conjunction with the simple indicators would certainly help to identify clusters of infections earlier so that interventions could be instituted to reduce morbidity and mortality.

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A Risk Index for Sternal Wound Infection After Cardiovascular Surgery

To the Editor:

I really enjoyed reading the article by Kohli et al. in the January 2003 issue of *Infection Control and Hospital Epidemiology*.[1] The authors have provided a neat study of risk factors for sternal wound infection at the largest center for cardiac surgery in the province of Ontario. A huge data set was tackled, hard work ensued, and the findings are going to stimulate useful discussions among surgeons and non-surgeons alike. I am nonetheless disappointed that the authors made no mention of our study, which was published in 1993 in peer-reviewed cardiac surgery literature.[2]

The epidemiologic study in Minneapolis spanned 15 years of cardiac surgery practice in a system that enjoys the benefits of an aggressive and rigid global surgical infection surveillance program that has been operational since 1977. Detailed microbiology data have always been garnered in that effort as well. In setting the predicate for their study design, Kohli et al. cited four prior studies of risk factors for sternal wound infections, three of which were from 6 to 10 years older than ours. It is no doubt linguistically accurate to state, as they did, that "numerous studies of the risk factors of sternal surgical wound infection exist," but there certainly have not been numerous regression analyses performed with a mainstream