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INFECTION PREVENTION: SSI PREVENTION AND SKIN PREPARATION/TECHNICAL FACTORS

Hi, I'm Patchen Dellinger, General Surgeon from the University of Washington, and I'm going to chat with you right now about skin preparation and other technical factors that may or may not influence infection risk in surgery. This is an area with lots of opinions and sometimes good evidence and some not.

One area is, "Should all patients bathe with Chlorhexidine before an operation?" Well interestingly, this is recommended and done in just about every system I've ever heard of. But when you look at a Cochrane Systemic Review from 2012, they looked at seven randomized controlled trials with a total of over 10,000 participants, and these studies compared Chlorhexidine 4% versus either bar soap or no bath or shower. And they compared whole body Chlorhexidine versus localized washing, one wash versus more than one wash; and the final conclusion was there is no evidence that washing with Chlorhexidine reduced the evidence of surgical site infection.

Now, I'm not saying it's bad to wash your patients with Chlorhexidine or have them wash themselves; and in fact, that's what all my patients do as it is the standard in our hospital. And what we do know is that patients who have washed with Chlorhexidine have lower colony counts of bacteria on their skin compared to patients who have not. And clearly, it cannot be bad to have lower colony counts on your skin. But the evidence that it actually reduces surgical site infection is basically missing.

Now, some possible reasons for this is perhaps you need repeated applications. Maybe it would work better if the patient showered with Chlorhexidine for three whole days before the operation. Or maybe skin cleansing methods that avoid rinsing away the Chlorhexidine might have the potential to be more effective. The standard trials have mostly been done with the patients issued Chlorhexidine soap and told to bathe or shower with it; and, of course, it gets rinsed off at the end. And perhaps using the Chlorhexidine cloths and not rinsing after the cloths would be more effective; however, this study has not been done. So the practice lower skin flora, and that can't be bad, but it has not at this stage ever been shown to reduce surgical site infections.

What about skin preparation? Does the antiseptic used make a difference? Well, the first study that looked at this in a serious way was by Swenson and colleagues from the University of Virginia in a study published in 2009. And what they did is they did a time sequence study. For six months, all patients were prepped first with alcohol, then with Povidone-iodine. For the next six months, patients were prepped first with alcohol and then Chlorhexidine actually as the standard prep of ChloraPrep; and then in the last six months with alcohol and iodine-povacrylex, another type of Iodoform, and this is DuraPrep. And what they found was for any surgical site infection, they actually got the best results with one of the preparations with alcohol plus an Iodoform compared to alcohol plus Chlorhexidine -- although if you look at the other infections, the differences in the subspecies of infections were not that different.

So the next study that came out was this study by Darouiche and colleagues in the *New England Journal of Medicine*; and they compared Chlorhexidine and alcohol, that is ChloraPrep with Povidone-iodine alone. And they showed that there was a significantly lower infection rate with Chlorhexidine alcohol



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compared to Povidone-Iodine. And a number of commentators said after this, "Oh gosh, we've all got to go using Chlorhexidine." But what they didn't point out was that two things were different here, both the Chlorhexidine and the alcohol; and there was no alcohol used in the Povidone-Iodine group. And if you put together data from the Swenson paper and data from the Darouiche paper and you just look at clean contaminated cases, because those were the only types of cases that were in the Darouiche study, what you find is that Povidone-Iodine without alcohol gave a pretty high rate; but Povidone-iodine and alcohol or Chlorhexidine and alcohol in either of the two studies gave very similar rates. And there were slightly better rates with the Iodoform plus alcohol.

So my conclusion from this is that probably what makes a difference is having alcohol, which causes very rapid killing of essentially all pathogens, and then adding to it either an Iodoform or Chlorhexidine for better long-term control of bacteria on the skin during the duration of an operation. Now, that's my opinion. You can't say that that's Level I evidence. But putting together the information from these two studies, that's what seems to me to make the most logical sense.

Next question, "Do wound protectors reduce surgical site infection risk?" This is an example of one type of wound protector available currently on the market. It has two plastic rings and flexible plastic between them. And what you do is you put one of these inside the abdomen, and then it springs out. And this one springs out on top of the skin. And what it does is it covers the exposed subcutaneous tissue and fasciae and in theory helps reduce bacterial contamination and perhaps improves the health of the wound by preserving moisture and warmth in the wound.

Gheorghe and colleagues in a paper published this past year in *Annals of Surgery* looked at a number of trials here comparing patients with wound protectors and without wound protectors and concluded that a combination – a meta-analysis – of all of the studies together, indicated about a 20% reduction in infection risk with wound protectors.

So it's possible that wound guards may significantly reduce the incidents of surgical site infections in patients undergoing open abdominal surgery compared to standard; however, the conclusion in this paper also noted that the quality of the available evidence was generally very poor due to methodologic flaws and reporting failures, and that there is a need for more high-quality, multicenter, randomized control trials. So depending on your local experience, this might be something you would like to consider introducing, particularly for high-risk cases such as open colectomies.

Another area of interest in terms of infection risk is the influence of surgical technique on the risk of surgical site infection. And most surgeons will tell you, "Oh for sure, surgical technique is very important and probably influences infection risk." But then when you ask, "How do you measure surgical technique?" there's not all that much agreement on how it should be done. Some surgeons may say, "The way I do it; that's good surgical technique," or, "It's the way my chief made me do it." If you want to look at theoretical issues, I think that we would all agree that minimal tissue trauma, minimal blood loss, and minimal foreign bodies are part of good surgical technique, but that you would want to also have short operating time without sacrificing the above.

Now, there have been few studies – only one that I know of – that actually prospectively compared different surgical techniques. But this graph comes from an interesting paper. It was one of the earliest studies reporting the efficacy of prophylactic antibiotics by Hiram Polk published in 1969. And at the end of the operation, he measured the amount of bacteria present in the wound – quantitative measurement. And



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he looked at the rate of surgical wound infection. And what he showed was that in a dry wound, in a patient that got placebo, there was an increasing infection rate with increasing numbers of bacteria in the wound. But if the wound fluid had a hematocrit of greater than 8%, and we might say this was poor hemostasis which could be an element of surgical technique, the same bacterial count gave a much higher rate of surgical site infection. And then, of course, the best results came from wounds with good hemostasis and with prophylactic antibiotics and good concentrations of antibiotics in the wound fluid. So this shows the interaction of surgical technique and antibiotic prophylaxis.

Another study showing something similar was this study by Hoyer and colleagues. Hoyer studied patients having colectomy and receiving either Doxycycline or placebo. And they were criticized for doing this study with placebo, and it's the last known study of placebo in colectomies. They also kept track during the operation, was there gross contamination? Was there spillage of feces during the operation? If no, there was a 7.5% infection rate with antibiotic, 32% with placebo. If there was gross spill, the infection rate went up to 39% with antibiotics, still better than with placebo. But you can see that antibiotic plus gross spill was about the same as a technically good operation with placebo, and the best results was technically good operation and prophylaxis – another suggestion that surgical technique matters.

The final paper I want to discuss with you briefly is this paper by Melbourne and colleagues. And in this study, it was actually a prospective randomized study in which patients were randomized either to have long stiches, a centimeter back from the fascial edge and a centimeter apart, or short stiches, 5 to 8 millimeters apart and 5 to 8 millimeters back from the edge of the fascia. In each case, that results in a stich length to incision length ratio of more than 4. What they found was a lower rate of dehiscence, a lower rate of surgical site infections, and a lower rate of incisional hernias in the patients with the short stiches placed close together -- a suggestion that that aspect of surgical technique does influence infection.

When they looked at the predictors of infection in this study, as expected, they found that wound contamination increased infection rate 2.8 times greater. Diabetic patients had a higher infection rate than non-diabetics. And patients with the longer stiches had a higher double infection rate compared to patients with the short stiches placed close together.

Another question that has been raised is, "Should we use antimicrobial impregnated sutures?" Now, the basis for thinking this might be useful goes back to experiments done in the '50s by Elick and published in the *British Journal of Experimental Pathology*. Back in those days, you could do this kind of study. He established that in human volunteers, it took a million bacteria or more to cause a subcutaneous infection in a human; but it required only one hundred bacteria on a silk suture to cause a more severe infection than occurred by simply placing one million without a suture under the skin.

So this raised the idea that providing sutures with some antimicrobial properties might discourage the attachment of bacteria to the suture and discourage surgical site infection. A meta-analysis published in the *Annals of Surgery* in this past year looked at six or seven trials comparing impregnated with non-impregnated sutures. And they saw a result when all are combined, it was slightly in favor of impregnated sutures but did not reach statistical significance. It is my understanding that another meta-analysis is in process these days that includes more studies and that may have a slightly different approach. But at this stage, I guess I would say that impregnated sutures make sense; but that the data showing a significant reduction in surgical site infections are missing at this time.



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How about wound dressings and surgical site infection? I'll start right off the bat by saying that we have no Level I evidence here. But I'll review some suggestive data that influenced the way I manage my dressings, which you may want to think about in your own practice.

In this very interesting animal experiment done by DeMortiae (sp?) and colleagues in the 1960s, they took a guinea pig, did a laparotomy, closed the incision, and then they swabbed the skin next to the incision with a broth culture of bacteria with staph aureus. And you can see that when they did it right away or for any time up to six hours after the operation, every single incision got infected. So it's clearly possible to infect an incision early on afterwards before the incision is sealed. And this is, of course, one of the reasons why we put dressings on in the OR. After a day had passed, there was only 65% infections, dropping to 55% at two days; 30% at three days; 10% at four days; and no infections occurred after four days – suggesting that if you could protect an incision from external contamination for the first three to four days, that you would reduce the risk of external infection. Now, of course, none of us pours broth cultures over our fresh incisions.

In this study, Shauerhammer (sp?) and colleagues again looked at staph aureus on guinea pig incisions, this time on the guinea pig back, and again showed that with a suture-closed incision, there was a high rate of infection immediately after the operation that dropped dramatically after a shorter period of time than seen in the original study by DeMortiae. And when the incision was closed with tape rather than percutaneous incisions, there were no infections – suggestion that one of the routes that bacteria were getting into the wound was along the percutaneous sutures. With E. coli they showed similar, although less impressive results for those operations.

So, should we leave our operation room dressings on for three to five days after the operation? These animal data are intriguing. I haven't seen any human data. I can tell you that in my own practice, I leave my dressings on for a minimum of three days after the operation. And if there is drainage or some reason why the dressing needs to be changed, I change it in a sterile manner as if we were still in the operating room; and I re-prep the skin around the incision, using the type of alcohol/Chlorhexidine swab that I would use in the operating room to prep before putting a new sterile occlusive dressing on.

The last topic I want to cover is the area of nutrition evaluation in the management and its risk of surgical site infection. There are a number of data showing that patients with poor nutrition are at increased risk for surgical site infection. In this paper by Hennessey in the past year, they looked at a multivariate analysis of risk for surgical site infection and found an increasing risk with increasing age – which has been known for many years – and an increased risk of infection for patients having operations with an albumin lower than three.

"B" looked at prealbumin levels and infectious complications in patients having gastric surgery and found again that there was an increased risk for combined resections and an increased risk when the pre-albumin was below the lower limit of normal of 18 milligrams per deciliter with a threefold increase in risk for patients with poor preoperative nutritional parameters.

Maramisu (sp?) looked at a series of operations examining the benefit of feeding patients either before the operation, around the time of operation, or postoperatively with special immunonutrition products containing extra quantities of arginine and omega 3 fatty acids. And in this meta-analysis, they showed a dramatic reduction in infection risk through the provision of these immunonutrition supplements before



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and after or following operations with gastrointestinal anastomosis. I think that there's room for more research in this area, but these data are highly suggestive.

I'll just end with this note. This is a poster from our hospital. And many of my colleagues are on similar posters promoting hand hygiene, which we know is good for prevention of infection both in surgical and non-surgical patients.

I thank you for your attention and hope that you will be seeing fewer infections in your surgical patients in the coming years.

Good-bye.