routinely use filters. There is a lot of evidence to suggest that filters are not necessary.[3] None of this evidence was discussed in the recent article about filter usage. The article, supported by only six references, seemed most designed to voice an opinion rather than generate scientific discussion. One of the best articles supporting the use of IV filters[4] was not even mentioned. I would invite further discussion on this important topic.

Bryan Simmons, MD
Methodist Hospitals of Memphis
Memphis, Tennessee

REFERENCES

1. Weinstein S: Intravenous filters. *Infect Control* 1987; 8:220-221.
2. Chrystal C: Selecting an IV tubing system. *Infect Control* 1985; 6:384-385.
3. Simmons BP: Alternatives to IV filter usage. *Infect Control* 1985; 6:342-343.
4. Quercia RA, Hills SW, Klimek JJ, et al: Bacteriologic contamination of intravenous delivery systems in an intensive care unit. *Am J Med* 1986; 80:364-368.

Ms. Weinstein responds to Ms. Gurevich and Dr. Simmons:

I appreciate the opinions voiced by Ms. Gurevich and Dr. Simmons concerning my Product Commentary on IV Filters. I will address one of Dr. Simmons' comments first: "The article is so biased and of such poor scientific quality that it should be followed by an opposing view." Perhaps, Dr. Simmons is unaware of the fact that a Product Commentary is not, nor is it intended to be, a "scientific paper." It is exactly what it states: "a product update," intended to stimulate discussion and interest on the part of the reader. Also, as familiar as Simmons is with professional publications, he must know that published material does not always reflect the view of the editor or publisher. If an editor were to publish *only* his or her own views, available reading material would be quite biased and limited, would it not? The editor of *Infection Control* should be applauded for recognizing the importance of sharing articles such as mine and Ms. Chrystal's with the readership. (Chrystal C: Selecting an IV tubing system. *Infect Control* 1985; 6:384-385.)

Contrary to Gurevich's interpretation, the article does not "start out with an erroneous statement that is not referenced." It begins with a description of the purpose of IV filtration, which is entirely correct. The CDC guideline states that "using IV in-line filters is not recommended as a routine infection control measure." A Category II classification is applied; this classification has already been explained by Gurevich as "that which has not been adequately studied but has a logical or strong theoretical rationale indicating probable effectiveness." I believe that the question is one of interpretation. While Gurevich is correct in that the Category II rating is not a *weak* recommendation, Category II is also not a *strong* recommendation. By her own admission, a Category II classification is inconsistent with her statement that the "CDC strongly recommends *against* it in a Category II statement from 1981." The CDC categorized the use of IV filters as "Category II: moderately recommended for adoption." The CDC's recommendations are often interpreted as contraindicating the use of IV filters, or indicating that such filters are worthless. It is apparent that the CDC's main area of concern is infectious disease, transmitted under normal circumstances. These comments apply to Simmons's criticism as well.

While no IV filter can replace good sterile technique, nor protect a patient against infection transmitted below it in the line or on the skin, the filter *can* protect against extraordinary, potentially catastrophic contamination of an IV solution or line by an opportunistic pathogen. My comments addressed this issue: "the filter is not a panacea; it should never be considered a substitute for quality care and excellent technique." The filter additionally can protect the patient against the particulate matter seen in all IV infusions; this is the primary use of IV filters today. My article did point out that "while filters can undoubtedly reduce phlebitis due to particulate or chemical substances, studies to prove their value in clinical infection have not yet been done." Gurevich's concerns address the subject of clinical infection; because I stated that these studies had not yet been done, her criticism lacks substance.

As far as her comments relevant to IV fluid contamination, Gurevich again misread my material. I cited two studies, both Rapp (a classic in IV filtration and elimination of which would have been inappropriate and unjustified), and Falchuk, whose study addressed not only phlebitis, but *microparticulate-induced phlebitis* resulting from particulate contamination of IV fluids. Again, Gurevich refers only to infections; infections were not the subject of my manuscript although I briefly addressed the fact that "most studies indicate that infection is associated with the insertion site and the use of the IV cannula, areas that can be enhanced by excellent technique on the part of the IV specialist." I make no attempt here to weigh the merits of steel needles over IV catheters. I state instead that insertion of any IV infusion device should be limited to those who have been properly trained in the use of such products and that the quality of IV care is enhanced when an IV team is responsible for the delivery of such care. As far as the "nonissue for the use of filters is air in the tubing," I challenge Ms. Gurevich to publish "the many ways that nurses have prevented air from causing problems." One need only do a literature search to recognize the plethora of court cases citing infusion of air into the bloodstream as a cause of malpractice litigation and the patient's injury or demise. I have testified as an expert witness in several of these cases, and I can only assume that Ms. Gurevich is uninformed on this matter.

Ms. Gurevich is correct that "IV-related phlebitis is due to many causes." I addressed IV phlebitis by citing studies by Falchuk, Friedland, and Rusho. I stated that "according to Friedland, filters may well have an important role in selected patient groups." My entire article addressed the use of IV filters in specific patient groups! I bring to Gurevich's attention a paper by Quercia (*Am J Med* 1986; 80:364-368) in which a double-blind study was described. The study included patients admitted to a surgical intensive care unit; patients were randomly assigned a final filterset containing either a 0.22 micron bacterial retention filter (IVEX®-2) or an identical in-line cartridge without a filter. The study concluded that "(1) a significant level of extrinsic contamination of intravenous infusion deliv-

ery systems occurred on the intensive care unit; (2) documented clinical significant nosocomial bacteremias occurred less often in those patients who had a 0.22 micron bacterial retention filter on all possible intravenous lines." Quercia further stated that "intensive care units patients are particularly vulnerable to infections acquired as a result of hospitalization. The host defenses of this patient population are modified, bypassed, or eliminated by their underlying disease states, by the diagnostic and therapeutic modalities routinely administered, and by numerous invasive monitoring devices such as intravascular or bladder catheters." Quercia clearly describes a "compromised patient," one of a select group for whom I recommended the "routine use of IV filtration to reduce pain, risk, and expense of otherwise preventable complications." Simmons himself called Quercia's work "one of the best articles supporting the use of IV filters." Quercia also demonstrated a proven cost savings as a result of the use of IV filtration in the SICU; nosocomial infection previously would have increased costs by $168,000 yearly (42 patients at a cost of $4,000 each). Use of filters was estimated to cost $5,700 in his institution annually.

Gurevich is correct in her statement that "proven methods of prevention should be followed." Large volume filtration of admixtures in the pharmacy is an ideal solution; unfortunately, there are hospitals throughout the country that still delegate the responsibility for admixture to nurses on the nursing units. Slow administration of such admixtures is an ideal solution, providing that the rate of flow is consistent with the physician's orders and other parameters for an individual patient. While Tanner addresses the influence of heparin on intravenous infusion, routine buffering of IV solutions with heparin sodium or hydrocortisone as a means to prevent phlebitis could be harmful. Consideration would have to be given to the harmful as well as clinical effects of these two drugs and correlation of same with a patient's clinical diagnosis and other medications ordered; the potential for drug interaction and incompatibility would be an important factor.

My statement concerning a "myriad of conditions" came from a paper by Turco and Davis (Clinical significance of particulate matter: A review of the literature. *Am J Hosp Pharm* 1973; 8:137-140). I caution Gurevich not to misjudge the significance of literature citations that are "classic" in a specific area; animal experimentation has been the forerunner of much research seen today in our field. Of course, effects of infusing particles into human beings may be studied retrospectively. However, Garvan and Gunner's studies related several case reports of humans who appeared to have suffered increased morbidity and mortality due to the use of heavily contaminated infusates. It is true that such particles are filtered during the manufacturing process; one cannot dismiss the potential for contamination in shipping, handling, and use.

I am uncertain as to what Gurevich means by "centrally-delivered TPN solutions." Is this reference made to total parenteral nutrient solution delivered via the central vein or solution that has been prepared in a centrally located hospital pharmacy? If she is referring to central vein infusion, much of this infusion is taking place outside the confines of the hospital today—an even greater justification for the use of an inline IV filter.

I urge Ms. Gurevich to take her concerns relevant to pH directly to those who can respond—the drug manufacturer. Dr. Simmons is correct in inviting further discussion on this topic.

Again I state: "cognizant of the high cost of health care, and the subsequent higher cost of IV complications, why not advocate the 'routine' use of IV filtration in select patient populations?"

Sharon M. Weinstein, CRNI, MS
University of Health Sciences/
The Chicago Medical School
North Chicago, Illinois