hydrocortisone for 53 of 63 days after myocardial infarction. At autopsy, it was noted that the healing had been slowed to the 10- to 14-day level after infarction, with development of a large ventricular aneurysm. These investigators also cited several reports supporting the detrimental effects of corticosteroid therapy in delaying healing and in the formation of aneurysms after infarction in both humans and animals. It is interesting that in the paper by Toole and Silverman, three of their six autopsied cases had ventricular aneurysms; however, it is not stated whether these patients had received steroid therapy.

In light of these observations, the use of steroid therapy for a probably self-limited condition may be unwarranted when less toxic analgesic and anti-inflammatory agents are available. It seems unlikely that the questionable benefit of decreased narcotic use in this setting justifies the risk of delayed healing of the infarct and aneurysm formation.

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REFERENCE

1 Bulkley BH, Roberts WC: Steroid therapy during myocardial infarction. Am J Med 56:244-250, 1974

Interpretation of Chest X-ray Films

To the Editor:

The lead article in the September issue, "Disagreements in Chest Roentgen Interpretation" by Herman et al (Chest 68:278-282, 1975), is right on target. Responsible radiologists have been very honest about the problems in proper interpretation of chest films. Perhaps if more clinical information and the patient's age and sex had been given, the authors' interpretation accuracy would have been higher. Nevertheless, several points must be emphasized: (1) responsible radiologists are keenly aware of problems in film interpretation; (2) when ordering a chest film at most institutions, you are asking for a radiologist's consultation, and this should be carried out as other forms of medical consultation, with dialogue between physicians, adequate clinical information, etc; (3) it is irresponsible for the referring physician to request a chest film without supplying adequate clinical information; and (4) a written radiologist's report is not equivalent to tissue diagnosis.

It is my impression that practicing physicians, particularly on the primary-care level, do not listen to the radiologists discuss their own problems with chest film interpretation and simply rely on a typewritten report as the be-all and end-all. A patient with chest symptoms and abnormal findings on the chest film needs careful interpretation and consultation on his chest film. Dr. Herman and his colleagues have written an honest, straightforward prospective report, and its implications and conclusions need strong emphasis before the community of practicing physicians.

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Phlebotomy, Hemodilution, and Autologous Transfusion in Open-Heart Surgery

To the Editor:

I would like to compliment Cohn and associates on their article entitled "The Effects of Phlebotomy, Hemodilution and Autologous Transfusion on Systemic Oxygenation and Whole Blood Utilization in Open Heart Surgery," which appeared in the September 1975 issue (Chest 68:283-287, 1975). This is certainly the correct approach, and I am sure it is not only beneficial to the patient, but also helps to save on the amount of blood used for open-heart surgery.

Since late 1972, we have used phlebotomy, hemodilution, and then replacement of the blood from the heart-lung machine to the patient. We then return to the patient his own blood that was withdrawn prior to cardiopulmonary bypass. By using such a technique for the last 726 consecutive patients who had vein graft surgery, 80 percent received no blood during the operative procedure nor at any time during their hospitalization. For those patients undergoing valvular surgery alone, 140 (65 percent) of 214 patients received no blood. And, for those having valvular surgery along with vein graft surgery, 132 (54 percent) of 244 patients received no blood. The technique was published in our article entitled "Coronary Artery Surgery: A New Technique with Use of Little Blood, If Any," which appeared in the August 1974 issue of The Journal of Thoracic and Cardiovascular Surgery (68:283-287, 1974). We believe that this technique is now a proved one, and we strongly urge other cardiac surgeons to use it.

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Sterilization of the Flexible Fiberoptic Bronchoscope

To the Editor:

I have read the letters from Dr. Johnson and from Dr. Miner entitled "Sterilization of the Bronchoscope" (Chest 68:807, 1975).

I would like to answer them as follows: My original article in Japanese covered nine pages of report and references, including some about 2-bromo-2-nitropropane-1,3-diol (Bronoslol; bronopol). The version printed in Chest was very much condensed and, therefore,
may not have been fully understood by readers. Furthermore, it represents only the initial and basic stages of a study which is still proceeding. The final goal of my studies is to discover a rapid sterilization method which is as effective as sterilization with ethylene oxide gas or formalin gas. As to probable ineffectiveness of benzalkonium chloride mentioned by Dr. Johnson, I can say that I began using it two years ago, and in about 112 cases, I have not had one single case of complications after fiberoptic punch biopsy. I believe that this article was very helpful not only for me but also for all fiberoptic bronchoscopists.

Since my article was printed in *Chest*, I have been continuing my studies and have developed a new piece of sterilizing apparatus for this purpose. This apparatus, together with my latest findings, were presented to the Congress of Digestive Endoscopy in Tokyo on Nov 15, 1975.

My further studies will include investigatory work on the following: (1) antiseptic agents and bactericidal agents, and development and combination of their use; (2) improvement of the structure and mechanisms of the fiberoptic bronchoscope; and (3) further improvement of my sterilizing apparatus. I believe that these studies will be successful and hope that every fiberoptic bronchoscopist will follow the studies and exchange of ideas.

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**Chicken Soup**

*To the Editor:*

It astounds me to see that such eminent chicken soup experts as Drs. David H. Spodick and Lawrence F. Greene are ignorant of the well-established mechanism of action of this effective treatment. It is well known that the antibacterial effect depends directly on the content of Bobbymycetin, the most potent naturally occurring antimicrobial drug in existence. Bobbymycetin is very soluble in sodium cytarabine hexamethyl-acetyl lututria tetra-zolamine and is, therefore, also effective for gonorrhea. Tablet form is also available under the trade name, Crebena. A. Jay Block, M.D., F.C.C.P.

*Schmaltz (Upjohn).*

**REFERENCES**

1 The chicken soup controversy. Chest 68:604-606, 1975
3 Rosenberg, Rebecca: Personal communication
4 Kasdan S: Love and Knishes. Greenwich, Conn, Fawcett World Library, 1956, p 54

*To the Editor:*

I read with interest the provocative commentary (*Chest* 68:604-606, 1975) elicited by our case report on chicken soup and was gratified to learn that so many of my colleagues have directed their attention to this important area; however, I feel constrained, owing to inferences about my hospital affiliation, to defend my credentials and those of my coauthor as regards our authority to speak on chicken soup. I should note at the outset that while the address listed for reprint requests did indeed cite Presbyterian-University Hospital, I am also on the staff of Montefiore Hospital in Pittsburgh. Furthermore, my Litvak origins, as any student of Western culture will attest, render me an authority on virtually anything, chicken soup included. My coauthor, Dr. Schwartz, was born in Vilna, so his credentials are clearly impeccable. I hope this information will be reassuring to the Mount Sinai group in New York.

With regard to the other correspondence, I would commend Dr. Spodick on his selection of beverages and suggest only that he offer a draught to his medical school applicants; it is never too early to teach the coming generation of physicians basic principles of hygiene. I congratulate the diligent staff of the Chicken Soup Institute in Philadelphia and acknowledge their pioneering work in this field. With regard to chicken-soup-resistant strains of bacteria, I can only echo the alarm expressed by your correspondents and encourage them to pursue their work in this area; their studies suggest that chicken soup should not be used where less potent agents will do but should rather be held in reserve for treatment of patients infected with multiply-resistant strains of bacteria. I do not feel qualified to comment upon the potential of chicken soup as a new energy source, the use of other chicken derivatives in the treatment of male impotence, or the use of sheep hemoglobin as adjunctive therapy; surely, however, these are important areas of research and should not be neglected.

Dr. Schwartz and I wish to thank the scores of concerned physicians who have communicated directly to us their heuristic comments and suggestions. One physician, for example, is currently engaged in research on the use of his mother-in-law's matzoh balls as self-destructing esophageal bougies. It would appear that chicken soup is a concept whose time has arrived.

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