mune system and inflammatory response, with subsequent release of the latter initiating the clinical evidence of the infection. This correlation may not apply to therapy for tumors, but I am curious about this relationship and about the immediately preceding immune therapy in the patient of Byrd and Hornt wonder if this correlation has been noted by others.

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REFERENCES

To the Editor:

We appreciate the helpful comments of Reuland in regard to infection with Pneumocystis. In reply to his query, our patient had not had any changes in his chemotherapy or therapy with corticosteroids immediately prior to his developing symptoms of infection. We have not been aware that reduction in immunosuppressive therapy might be a precipitating factor in opportunistic infection but would be interested in the comments of other investigators in this regard.

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Right Ventricular Performance

To the Editor:

In a recent report in Chest, Steele et al1 presented their data on right and left ventricular ejection fractions in coronary artery disease (as determined by the radionuclide angiocardiography).

This study provided additional useful information about the right ventricular performance under the stress of chronic ischemia, yet the conclusions reached by Steele et al1 could be questioned not only on the basis of the methodology used, but also from the standpoint of the criteria that were selected to include the patients in this study.

Biplane cineventriculographic studies were used to substantiate the validity of the radionuclide method for determining right ventricular ejection fraction, yet in spite of a rather substantial error caused by the geometric model that Steele and associates1 selected for their determination of right ventricular cineangiographic ejection fraction (r = 0.94), no regression equations were used to compensate for the error introduced by the cineangiographic model or for the error inherent in their radionuclide approach (r = 0.80). Further, it is quite probable that the injection of contrast medium into the superior vena cava (rather than the preferable selective injection directly into the right ventricle) compounded the errors inherent in the methodology of Steele et al.1

In their study, Steele et al1 have included all patients who had coronary arterial lesions that exhibited greater than 50-percent narrowing. This is a highly questionable practice. Various studies indicate that only obstructions in excess of 75 percent are hemodynamically significant.2,3 Indeed, a recent report4 dismisses 50-percent coronary arterial lesions as being of insufficient magnitude to cause myocardial ischemia even during the maximal stress test. A substantial number of the patients considered by Steele et al1 to have coronary artery disease may therefore be functionally normal. This could further (and quite seriously) distort the conclusions reached by the authors.

Finally, Steele et al1 quote us5 as stating that the right ventricular ejection fraction is impaired in patients with right coronary arterial obstruction. They contradict our findings by pointing out that all of their patients with isolated right coronary arterial disease had a normal right ventricular ejection fraction. A conclusion of this nature can be made only if our data5 were either misinterpreted or analyzed only in a very superficial fashion. Only one patient in our entire series had an isolated right coronary arterial obstruction, and her right ventricular ejection fraction was normal. We5 specifically stated that it is unlikely that the coronary artery disease frequently impairs the right ventricular function directly through ischemia or prior infarction. Indeed, our subsequent investigations6 have confirmed this hypothesis. Unfortunately, Steele et al1 have looked at our data and mistakenly introduced further disagreements into the already abstruse field of right ventricular performance—unnecessarily.

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REFERENCES

To the Editor:

I was surprised that the correlation between right ventricular ejection fraction obtained from radionuclidic data and this measurement made from cineangiographic data was as good as observed ($r = 0.80$). Since count rate is a function of chamber volume, regardless of chamber geometry, the radionuclidic method might be more accurate than contrast angiographic studies for an irregularly shaped chamber like the right ventricle. Radionuclidic time-activity data must be corrected for the contribution of activity from tissues surrounding the chamber of interest, a complex and empirically solved problem.

Supraventricular injection of contrast medium was selected to prevent the occurrence of ventricular premature beats, which, in our hands, commonly occur with right ventricular injection. In our studies, there has been no difference in right ventricular volume calculated from contrast studies following injection into the superior vena cava, right atrium, or right ventricle.

I do not think that the critical point of reduction in luminal diameter with respect to the occurrence of myocardial ischemia is, as yet, an established fact. Gould and Lipscomb began to observe a reduction in hyperemic response to contrast injection at a 40-percent narrowing in arterial diameter in dogs.

I can appreciate why Dr. Ferlinz thinks that we have misinterpreted his data, but I am not sure that this is the case. We found impaired right ventricular ejection fraction on occasion in patients with isolated obstruction of the left anterior descending coronary artery, whereas the study of Ferlinz et al did not (0.60, 0.59, and 0.58 in their three patients; normal $\pm 1 SD$, 0.66 $\pm 0.06$). The single patient with isolated right coronary obstruction in the study of Ferlinz et al had a normal right ventricular ejection fraction (0.63), and all of ours had a normal ejection. Perhaps our phrase, "their data are in agreement with ours . . .," was too strong, in that there appear to be some areas of agreement.

I look forward to reading the forthcoming publication of Ferlinz and associates.

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REFERENCE
3 Ferlinz J, DelVicario M, Gorlin R: Incidence of right ventricular asynergy in patients with coronary artery disease. Am J Cardiol, to be published

Thoracoscopy and Intrathoracic Extrapleural Lipoma

To the Editor:

Concerning the "Roentgenogram of the Month" entitled "Tapering Shadow in Anterior Right Thorax" by Villegas and de Goldenberg in the July 1976 issue (Chest 70:103-104, 1976), a suspected intrathoracic extrapleural lipoma can usually be confirmed by a simpler technique than open incision via thoracotomy or even via mediastinotomy. Thoracoscopy from a somewhat distant region of the thoracic wall can allow an intrapleural view of the lesion. Assuming that there are no pleural adhesions that would prevent the lung from collapsing away from the chest wall, the tip of the instrument can be placed in close approximation to the lesion, so that the yellow color becomes clearly discernible with the excellent visualization provided by the optics of the presently available equipment. Probing can determine the typical soft consistency. Finally, actual biopsy for tissue confirmation is easily accomplished.

I have had personal experience with two cases similar to the one described by Villegas and de Goldenberg; each patient was an elderly woman for whom any type of thoracotomy was relatively contraindicated because of diminished cardiopulmonary reserve. Three-year and four-year follow-up examinations with serial films in each case have confirmed the benignity of the lesion.

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Chicken Soup

You can't tell the players without a scoreboard!

EDITOR

To the Editor:

Second thoughts on these latest publications on chicken soup (Chest 69:572, 1976) suggest that for those not of Litvak or similar origin, a glossary has become pertinent:

Booby: The classic form of bubee or bubbe, an "affectionate term of endearment . . . between a husband and wife, parent and child . . . " with the diminutives of bubeleh or bobeleh, meaning "little grandma."

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