Second, it is interesting that absolute risk reduction in mortality from 23% to 6% is 17% in those symptom-free patients who were operated on. However, all variables in a scientific study must be controlled except the single variable that is being studied. Thus, when David and colleagues¹ attempted to modify a successful operative procedure, underlying background pathologies in the control and study groups needed to be similar. This was lacking.

Third, the demographic data (Table 1 in the original article¹) reveal an important cause with regard to the population sample. Atrial fibrillation (AF) was more prevalent in the symptomatic than asymptomatic group ($P = .0001$). Chronic AF carries greater risk than intermittent AF, doubling the risk of cardiovascular mortality. In other words, implications for symptom-free patients are only indirectly related, as opposed to a direct cause-effect relationship with AF.³ Moreover, diabetes mellitus in the symptomatic group approached a statistically significant difference ($P = .07$). More recently, it has been shown that all the patients with insulin-dependent diabetes mellitus with left diastolic dysfunction have evidence of definitive autonomic neuropathy. Such isolated diastolic dysfunction may be the principal physiologic mechanism in these patients. The annual mortality from diastolic heart failure varies widely, from 9% to 28%.⁴ This variable should be addressed in the context of patient cohort by reviewing echocardiographic evidence of diastolic dysfunction. The mortality rate among patients with diastolic heart failure is 4 times that among persons without heart failure but half that among patients with systolic heart failure.

Third, apart from the semantics of asymptomatic, of more interest at mean follow-up 93% of patients in the asymptomatic group were in New York Heart Association (NYHA) functional class I or II, which was not statistically significant from the symptomatic group (88%). This result argues that mitral valve surgery repair on symptom-free patients does not confer any further clinical benefit. This statistic is consistent with others and follows classic teachings. Also, the discrepancy between the reoperation rate and the incidence of significant mitral valve regurgitation indicates that not all patients having important valve incompetence late after repair undergo surgery again within the time

frame. This repeated observation creates a new argument that recurrent mitral incompetence lacks substance as an indication for operation and argues against the primary hypothesis and conclusion of David and colleagues.¹

Fourth, David and colleagues¹ did not mention the well-known limitations to the simple NYHA scale for cardiovascular disability. NYHA has a tendency to oversimplify an issue or a problem by ignoring complexities or complications. The criteria committee of the NYHA has provided a widely used classification that relates symptoms to "ordinary" activity. The term *ordinary* is subject to various interpretations, as are terms such as *undue fatigue*. Such terms have limited the accuracy and reproducibility of the NYHA classification.⁵

In the end, it is desirable to seek patients who need an operation. Offering such surgery to symptom-free patients on the basis of effect rather than cause, however, is simplistic and fuzzy. Better data analytic methodology and study design are needed to qualify for a new indication.

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Reply to the Editor:

Dr Shuhaiber makes some valid points about our study,¹ but many of his comments are irrelevant. He ignored the fact that ours was an observational study and did not establish causation but rather an association between functional class and

late mortality after mitral valve repair for mitral regurgitation caused by prolapse. The objectives of the study were clearly defined, and in addition to showing the late outcomes of mitral valve repair for floppy valves it also showed associations between certain preoperative variables and late mortality by multivariable analysis with bootstrap validation. Our study was not a prospective randomized trial, and consequently we could not control the variables before the operation as he suggested. This is, of course, a major limitation of all retrospective studies. One can only adjust for those factors that were measured. Residual confounding by unmeasured variables may potentially distort results. However, numerous studies have shown that careful, multivariable analysis of observational data produces treatment effects very similar to those of randomized controlled trials.²⁻⁴

Atrial fibrillation and diabetes were included in all multivariable models. Dr Shuhaiber's concerns regarding chronic atrial fibrillation may in fact provide further argument to operate on symptom-free patients. Atrial fibrillation may be a consequence of deterioration in ventricular function in patients with mitral regurgitation, and by the time it first occurs irreversible changes may already be present.

His remarks regarding postoperative functional class and the probability of recurrent mitral regurgitation in our patients are incorrect. One would not expect symptom-free patients to have lessened symptoms after the operation simply because they were already in functional classes 1 and 2 (ceiling effect). The analysis at follow-up was designed to demonstrate the improvement in the patients with symptoms. The mechanism of mitral valve prolapse was similar in both groups of patients, and recurrent mitral regurgitation after mitral valve repair is more dependent on the pathology of the mitral valve than on presence or absence of preoperative symptoms. That is reason why recurrent mitral regurgitation and reoperation rates were similar in the two groups.

We agree that functional class is a soft, subjective variable, especially between classes 2 and 3, but certainly not between classes 1 and 3 or 2 and 4. Moreover, symptoms are usually the reason a patient seeks medical advice. What our study showed is that postponing surgery until symptoms are more obvious (functional

classes 3 and 4) is not appropriate if the mitral valve can be repaired. Nowhere in the text of our article will the reader find a sentence stating, "Patients with asymptomatic mitral valve incompetence are candidates for surgery," as written in Dr Shuhaiber's letter. In fact, in the last paragraph in the discussion of our article, the reader will find the following¹: "In conclusion, surgical intervention should be considered in asymptomatic patients with severe MR caused by floppy valves if valve repair is feasible, and it can be done with low operative mortality and morbidity because the late survival is identical to that of the general population."

And the paragraph before the last reads as follows¹: "This is a retrospective study of a clinical experience of a single surgeon, and the results might to be generalizable. The prevalence of associated cardiac and non-cardiac diseases was relatively small in this series, and statistical values of certain variables might have been altered by chance alone."

We believe our conclusion was far softer than implied in the letter. However, we agree that a controlled randomized trial is needed to determine the appropriateness of mitral valve repair for symptom-free patients with normal left ventricular function.

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Esophageal perforation during left atrial radiofrequency ablation

To the Editor:

Doll and colleagues¹ reported an esophageal perforation incidence of 1% (4/387) after left atrial ablation with intraoperative radiofrequency ablation for atrial fibrillation. Risk factors could not be identified; therefore, they recommended against the use of intraoperative radiofrequency ablation for atrial fibrillation. In our opinion, however, a combination of various factors—such the device, the handling of device, the application time, the lesion pattern, and the surgical access—contribute to this complication, rather than the mere use of radiofrequency.

Doll and colleagues¹ used temperature-controlled radiofrequency ablation with a 10-mm T-shaped rigid ablation probe (Radios 504; Osypka GmbH, Grenzach, Wyhlen, Germany) targeting a temperature of 60°C for 20 seconds for each lesion without taking the variability of the local atrial wall thickness into account. This catheter has a temperature overshoot, which proved to be a concern in terms of safety and rapidity of feedback control. Excessive tissue temperature could result in necrotic perforation.² It is the overlap between two linear ablation lines where excessive tissue heating can occur. The Leipzig group did not mention this in their publication. The Leipzig group performed these procedures through a right lateral minithoracotomy; therefore, dissection of the doom of the left atrium was probably not done. Thus the relation ship between the left atrium and the esophagus was intense.

Several surgical centers have used temperature-controlled radiofrequency without reporting any esophageal or circumflex arterial injuries (Table 1). However, differences in technique can be distinguished.

All centers used a standard sternotomy. Williams and coworkers⁴ used a flexible ablation probe with seven consecutive electrodes (Cobra; Boston Scientific-EP Technologies, La Garenne Colombes, France), each independently regulated by the generator targeting an even higher temperature (70°C-80°C) and longer application time (1 minute) per lesion than used by Doll and colleagues.¹ Energy delivery was flexible but still up to 150 W. Ablation lesions were either made as separate ovals around the left and right orifices or as a complete circumferential island around all four pulmonary orifices. Nevertheless, Williams and coworkers⁴ did not report any injury, nor did Benussi and associates⁵ and Melo and colleagues,⁶ who also used temperature-controlled radiofrequency in a combined cohort of 105 patients.

In our own series of 124 patients treated with irrigated radiofrequency,⁹ the 30-day mortality was 4.8% (6/124). The causes of death were cerebral stroke (n = 1), atrioventricular dehiscence (n = 1), cardiac failure (n = 1), and low cardiac output (n = 3). Autopsies did not reveal any esophageal, pulmonary orifice, or circumflex arterial injuries. Neither were such injuries seen by Sie and coworkers⁷ in a series of 122 patients. We used a handheld, flexible pen catheter (Cardioblate; Medtronic Inc, Minneapolis, Minn). Formation of yellow-white blistering endocardial lesions, induced by oscillating catheter movements, were considered sufficient. Stable catheter-tissue contact was preserved without pressing the atrial wall against adjacent mediastinal structures.

We therefore believe that the cause of the reported complication was the use of a rigid T-shaped temperature-controlled radiofrequency ablation probe pressed against the atrial wall, which was not dis-

TABLE 1. Results of selected series

Reference	Esophageal injury	Circumplex arterial injury	Sinus rhythm (%)	30-d Mortality (%)
Mohr et al ³	1% (4/387)	0.4% (1/234)	67–78	6.4 (15/234)
Williams et al ⁴			81	12.5 (6/48)
Benussi et al ⁵			77	2.5 (1/40)
Melo et al ⁶			54	0 (0/65)
Sie et al ⁷			72	4.1 (5/122)
Gueden et al ⁸			71–95	3.2 (2/62)