even disrupt the term cardiorenal, and perhaps the term renocardiac is more appropriate, because HF therapies may not be as effective or as often used in patients with a poor renal function.

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REFERENCES

The “Obesity Paradox” Is Not a Paradox: Time to Focus on Effective Treatments

I recently read the editorial comment by Lavie and Ventura (1) with interest but also with disappointment. My disappointment is because several papers from different authors have illustrated that the reported relationship between obesity and survival from heart failure occurs because all of the observational analyses suffer from a very serious bias if one is exploring causal effects—for 2 examples, see Banack and Kaufman (2) and Preston and Stokes (3). The explanation was even provided in a letter to the editor on a recent publication by Lavie that is cited in this editorial (4). Dr. Lavie did not respond to the letter and it remains unclear why he has not incorporated such a simple and plausible explanation into this recent editorial.

As one example, one can divide causes of heart failure into those due to obesity and those not due to obesity. If the nonobese causes of heart failure (e.g., genetics, viral myocarditis) have a worse prognosis than obese causes of heart failure, one would expect to see the observed data even if obesity worsens survival in heart failure. This is expected and not a paradox.

Heart failure is a tragic illness. Focusing attention on the unjustified fascination that obese patients with heart failure do better than nonobese patients with heart failure may be diverting attention from proper prevention and treatment. This is especially true given that Lavie himself reviewed research on weight reduction programs in heart failure patients and concluded that weight reduction improves outcomes (5). I look forward to future academic efforts toward best treatment practices rather than repeating a theory that promotes unnecessary confusion.

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REFERENCES

REPLY: The “Obesity Paradox” Is Not a Paradox: Time to Focus on Effective Treatments

We were delighted at Dr. Shrier’s interest in our editorial (1). Referring to the letter regarding our Heart editorial (2), a response was posted almost immediately (3). Support for the obesity paradox, however, does not promote obesity, which adversely impacts many of the cardiovascular (CV) disease...
(CVD) risk factors, CV structure and function, and
does not any of their products). Dr. Ventura has reported that he has no relationships

CONCERNING THE ROLE OF GENDER DIFFERENCE IN OBESITY PARADOX IN PATIENTS WITH HEART FAILURE

Several studies demonstrate the presence of an “obesity paradox” among patients with several chronic diseases, and this obesity paradox shows that lower (rather than higher) body mass index is a risk factor for increased mortality. In particular, the presence of a significant degree of obesity (i.e., at least up to a body mass index of 30 to 35 kg/m²) is associated with lower mortality (1–3). However, when Vest and colleagues (4) analyzed their data by gender, they surprisingly found that well-established obesity paradox is valid for women but not for men. Most studies with a considerable amount of patients could not show such a gender-specific difference in obesity paradox (1,2).

Although Vest et al. (4) have adjusted their data for a variety of potential confounders, they did not adjust for coronary artery disease (CAD). It should be considered that CAD was more frequent in men than women (4). This may have had a significant impact on the survival rate in the male study cohort.

In this regard, implementation of other established survival markers in heart failure, such as N-terminal pro-B-type natriuretic peptide, as von Haehling et al. (5) discussed in their article, would be of help to shed more light on the conflicting results in this study.

The study protocol by Vest et al. (4) allowed cardiopulmonary exercise testing using either treadmill for most patients and bicycle exercise in a minority of patients. However, it is well-known that the results differ significantly depending on exercise method, and we question the validity of this mix of test methods. Did the authors adjust for this effect? Because values gained from bicycle and treadmill are not directly comparable, how did the authors overcome these differences?