rate was twice that observed in the syncope group older than 75 years. This is unexpected too because the long-term (1 year) prognosis of syncope was found to be related to aging and comorbidities (3) that are likely to be more frequent in elderly individuals.

The authors hypothesized that there could have been an underdiagnosis of unrecognized cardiovascular diseases in syncope patients 25 to 44 years of age (1).

Here we propose an additional potential explanation. A preliminary analysis of our STePS (Short-Term Prognosis of Syncope) (3) database indicated that 1-year syncope recurrence in working-age (18 to 65 years) patients was as high as 14%. Significant hazard ratio values of recurrent syncope were also reported by Ruwald et al. (1) in the 2 groups of syncope subjects 25 to 44 years of age and 45 to 74 years of age. In a setting of hazardous occupations, syncope recurrence might lead to fatal work accidents in the group of syncope patients 25 to 44 years of age in the present study who, potentially, are active workers. According to the EUROSTAT Health and Safety at Work in Europe report (4), most of work fatal accidents are classified as occurring after “loss of control,” “slipping,” “stumbling,” and “falling.” All these conditions might be the consequence of an occult syncope, producing a sudden loss of consciousness and postural tone.

We wonder whether work accidents, produced by a hidden syncope recurrence, might play any role in the increased risk of death found by Ruwald et al. (1) in patients 25 to 44 years of age. A potential answer might be found by matching the present study data with occupational accidents recordings. Should this hypothesis be confirmed, an unexplored scenario characterized by remarkable social implications could be opened.

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Does the Absence of Comorbidities Really Identify Low-Risk Syncope Patients?

We read with great interest the paper by Ruwald et al. (1) on the prognosis among healthy individuals discharged with syncope as the primary diagnosis. However, we wonder whether the subjects enrolled in the study can be effectively considered low-risk patients. Syncope is, most of the time, the first manifestation of several diseases, some of which are life-threatening, such as pulmonary embolism (2). In our opinion, the absence of comorbidities cannot by itself identify low-risk patients. Indeed, none of the risk stratification tools for syncope in the emergency department (ED), derived in both the short or long term, consider comorbidities as risk factors, except for cardiac diseases (3). On the contrary, patient admission to the hospital might be due to the clinical perception that the patient is not at low risk of adverse outcomes. During hospital admission, a specific cause of the syncopal episode is identified in only 30% to 50% of the cases (4,5).

Thus, syncope as primary discharge diagnosis is not necessarily consistent with the benignity of the clinical presentation.

Moreover, Ruwald et al. (1) observed an increased mortality rate from syncope after some years, whereas in the first year, data were inconclusive. If syncope itself were responsible for the increased mortality, should we not observe such increase in the short term rather than in the long term when comorbidities (3) seem to play a more relevant role?

Finally, from our point of view, low-risk patients should be those without comorbidities but who were also discharged from the ED. It would be of great interest to re-analyze the data from this point of view. We think that this analysis could strengthen the study results.

There might be a misprint in Table 3 because the 95% confidence interval of the point estimate does not include the point estimate itself.

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Reply

We read with interest the comments on our study (1) by Drs. Barbic and colleagues and Dr. Bonzi and colleagues.

Dr. Barbic and colleagues got the impression from our paper that patients 25 to 44 years of age had twice the risk of death than those older than 75 years of age. This misunderstanding is related to the hazard ratios mentioned in our study in which the hazard ratio in the 25 to 44 age group is 2.29, and that for those older than 75 years is 0.98. This is, of course, not the case, and the mentioned hazard ratios are given for “syncope” compared with “controls” of the same age group; thus, healthy individuals in the 25 to 44 age group with syncope have twice the risk of the corresponding control group without syncope. The relative relationship between the age groups and the cumulative incidence of death can be seen in Figure 1, where it is evident that with advanced age, the risk of death is much higher.

Dr. Barbic and colleagues propose an intriguing hypothesis that the increased mortality rate in these otherwise healthy patients with syncope may be caused by work-related accidents. We share their thoughts and are currently in the process of investigating the impact of syncope on subsequent accidents, falls, and accidents involving motor vehicles or large machinery compared with a matched control population by linking the data provided in the study with a database containing all data on accidents reported to the Danish medical system. Hopefully these analyses will provide data that could be useful for an evidence-based approach to guidelines on the use of heavy machinery and motorized vehicles after episodes of syncope.

Dr. Bonzi and colleagues whether our definition of low-risk patients was appropriate and states that syncope most of the time is the first manifestation of several diseases in healthy people. We do not agree that this has been shown before in the referred literature. We agree that the risk stratification developed is based on many other factors than comorbidities, but the registries used for our study do not include this information. Nevertheless, we find that our study provides valid contemporary data and prognosis on a very large population of “healthy” patients with syncope. We also want to emphasize that our definition of healthy individuals included that they were free of taking medications. Bonzi et al. state that the results on 1-year mortality were inconclusive, but we disagree with his interpretation because we clearly demonstrate that in patients 25 to 74 years of age, “healthy or low-risk” people with syncope have a significantly increased 1-year mortality rate compared with matched controls. Finally, Bonzi et al. requests the data re-analyzed on patients who were only seen in the emergency department. Our study combined those discharged from the emergency department with those discharged after admission. We did, however, also look into those patients who were discharged from the emergency department only, and the results were consistent with the overall results. As you noted, there was a typographical error in the online edition of the paper in the confidence intervals of the age group younger than 25 years of age; this was corrected in the print version.

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Reducing Hospitalizations for Acute Decompensated Heart Failure

The Infusion Room Approach

Collins et al. (1) provide valuable insight into the complex process and current dilemmas in managing patients who present to the emergency department (ED) with acute heart failure (HF). We would like to suggest an additional venue for the management of this special population: the outpatient infusion room (OIR), which is very similar to the observation unit concept proposed by Collins et al. It too has the intended goal to reduce hospital admissions and 30-day readmissions in a safe and effective manner. At our institution, we have implemented a Transitional Care Medicine program that works closely with cardiologists in the outpatient setting in an effort to meet the clinical and utilization management challenges that are associated with HF patients recently discharged from the hospital (2). As one of the major components of the program, enrolled patients are educated to contact the transitional care provider via telephone at the earliest sign of decompensation (weight gain, edema, shortness of breath, and/or increased fatigue). Such early communication of symptoms leads to in-person evaluation by the transitional care provider to help determine the optimal disposition of the patient. Similar to Collins et al. (1) viewpoint, we do not believe that all cases of decompensated HF require hospital admission. Thus, we use the observation unit for patients who require a moderate level of escalated care (i.e., 24 h of intravenous [IV] diuretic therapy) because we recognize the cost savings to the healthcare system by using this approach (3). In even milder cases that still require more than traditional outpatient care, we use the OIR in cases in which patients may only need <12 h of IV diuretic therapy to relieve dyspnea, for example. This decision is often guided by the familiarity that the transitionalist has with the patient and past presentations of that specific individual. Other institutions have used this...