Letter to Editor in Response to Case Report Titled “A Novel Treatment for Malignant Cough Syncope” Published in JAPI 2020;68:83–85

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We read with interest the article titled “A Novel Treatment for Malignant Cough Syncope” published in JAPI 2020;68:83–85. We would like to offer the following comments:

• Authors have used the term “malignant cough syncope” and have also defined it without quoting any reference. Cough syncope is a well-defined term but the use of the term malignant cough syncope has not been found in the literature search except in one case report, wherein the term has not been defined. The justification for using this term needs to be elaborated by the authors.

• The patient in the reported case presented with a chronic cough of 1-year duration. Idiopathic pulmonary arterial hypertension (IPAH) is a very uncommon cause of chronic cough and he should have been worked up to exclude most common causes of chronic cough like, asthma including cough variant asthma, nonasthmatic eosinophilic bronchitis, gastroesophageal reflux, and upper airway cough syndrome.3 Except for high-resolution computed tomography (which was done to exclude diffuse parenchymal lung disease), other investigations like spirometry, allergy workup, bronchoscopy, rhino sinus imaging, and gastrointestinal endoscopy were also required to be done before attributing the cause of chronic cough, leading to repeated syncope, solely to IPAH.

• Since the diagnosis of IPAH is one of exclusion, arterial blood gas analysis, polysomnography to rule out sleep-disordered breathing, abdominal ultrasound to rule out portopulmonary hypertension, color Doppler of lower limbs to rule out deep vein thrombosis (DVT) should also have been done to label it as IPAH. Authors have mentioned New York Heart Association (NYHA) class III for IPAH which instead should have been the World Health Organization (WHO) functional class (which is NYHA revised functional classification for patients with pulmonary hypertension) and which is one of the important criteria for risk stratification and also for planning specific treatment. Also the risk stratification should have been done for assessment of prognosis and response to treatment and should have included a 6-minute walk test, N-terminal pro-B-type natriuretic peptide (NT-pro BNP) apart from the workup done in the present case.

• Considering his symptomatology, this patient was probably in WHO functional class IV in view of right-sided failure and syncope. He was started on specific combination therapy with tadalafil, ambrisentan, and intravenous epoprostenol (class IIa evidence).3 However, the case report does not mention any postprocedure pharmacotherapy. The reported case did not receive maximal drug therapy for IPAH and usage of this intervention in this case may give a wrong message about the indications of this intervention in IPAH.

• The authors have used atrial septal stenting as a palliative procedure in this case. This invasive procedure with anecdotal evidence of its effectiveness may be offered to patients who have failed maximal drug therapy and as a bridge to lung transplantation.

• The authors have mentioned in the discussion (without quoting any reference) that specific drug therapy in IPAH has minimal effect on long-term survival and is very expensive. This may not be true since comprehensive analysis of survival from the time of diagnosis in a large cohort of patients with IPAH suggests considerable improvements in survival in the past 2 decades.5 So far as cost is concerned, except for epoprostenol, most of the drugs are affordable by the majority and would have been more cost-effective in the given setting, considering the lack of studies on the long-term prognosis, and course of disease posttrial septostomy and stenting.

• The title of the case report should have conveyed the correct purpose of this novel therapy primarily for IPAH and not malignant cough syncope. Also, the references quoted by the authors are very old.
REFERENCES