

Letter to Editor

Letter to editor on noninvasive ventilation to prevent reintubation

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Abstract: We would like to reply to the comments are as follows: 1- Our patients were published in Letter to Editor entitled "Non invasive ventilation to prevent reintubation. Key methodological concerns in cardiothoracic unit" by the authors Beyoglu C.A., Ozdilek A., Esquinas A.M. in Int J Clin Exp Med. The main issues that the authors Beyoglu and her colleagues have put forward and our answers to these comments are as follows: 1- The patients in our study have a history of cardiac dysfunction and they underwent cardiac or thoracic surgeries and because of these reasons they are not hemodynamically in stable state, 2- The materials and methods have been well presented in our study, 3-The inclusion criterias are well established in our study. We do not wait on room air oxygen until patients have an acute respiratory failure before application of noninvasive ventilation and patient's clinical deterioration while receiving oxygen therapy via face mask is necessary to decide whether patient has respiratory distress or not. These decisions are made depending on acute respiratory failure criterias on textbooks. 4- Cardiac and thoracic surgeries are both included as these operations are involved significantly with postoperative pulmonary dysfunction because of incision on chest wall causing atelectasis, pleural opening, possible phrenic nerve injury, pain, prolonged recumbent position and reduction of diaphragmatic movement, 5- Pain is an important factor for postoperative pulmonary complications and we thank you for your comment and describe our pain relief methodology during our study, 6-The complication rates were statistically not different from each other between the groups, 7- Carbondioxide retention, hypoxia and bradycardia are a part of the definition of failure of noninvasive ventilation and bradycardia is not listed in acute respiratory failure criteria in our study.

Keywords: Noninvasive ventilation, reintubation, cardiac surgery, thoracic surgery

We would like to reply to the comments that were published in Letter to Editor entitled "Non invasive ventilation to prevent reintubation. Key methodological concerns in cardiothoracic unit" by the authors Beyoglu CA, Ozdilek A, Esquinas AM. in Int J Clin Exp Med [1]. We are glad that our work has been discussed at Istanbul University Cerrahpasa Medical Faculty as this institute is a major teaching and academic hospital in Turkey. We would be interested to receive an interaction through letter, messages and collaborative work with institutes like Istanbul University rather than a discussion about our weaknesses or discouraging negative critical issues in our study because we have included our limitations into the study on page 3445 of our study [2]. I am writing this letter together with Dr. Gönül Sağıroğlu and Dr. Elif Çopuroğlu. I waited for a few months to receive further concerns from other academic insti-

tutes before answering the comments of the authors listed above and I was not able to receive any further discouraging and negative concerns regarding our study. The main issues that the authors Beyoglu and her colleagues have put forward and our answers to these comments are as follows:

In our study the methodology is not written on page 3439 however there is an introduction on page 3439. In the introduction part the concern of Dr. Beyoglu and her colleagues is that noninvasive mechanical ventilation (NIV) does not cause hypotension in normal hemodynamic state and the reason for our reference to the review article of Jaber et al [3]. In our study our sentence on pages 3439 and 3440 are as follows; "The main concerns during use of NIV are; 1- A reduction in the left ventricular preload and afterload that may cause hypotension, 2- An

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increase in pulmonary compliance due to recruiting of previously collapsed alveolar units [3, 8]". Jaber et al is a review article and it includes many information regarding the use of Bilevel Positive Airway Pressure (BIPAP) on cardiac patients and Lenique F et al describes the main concerns regarding the use of NIV in patients undergoing cardiac surgeries [3]. Our patients usually have a history of cardiac dysfunction and they underwent serious cardiac or thoracic operations and for these reasons they are not in normal hemodynamical state and we do not agree with their comments.

We do not agree with your comment on methodology of our study because the material and methods are described on Page 3440 thru 3443 in detail including the use of BIPAP in our patients. It is very clear from the sentence that 273 patients were enrolled into the study, after 11 patients were excluded 261 patients were randomized however 7 of them could not complete the study due to the reasons listed and only 254 patients were allocated for intervention and this is described on page 3440.

On page 3442, the first group of patients inclusion criterias were included and these include; "1- Spontaneous respiratory rate (RR) < 25/min, 2- Spontaneous respiratory volume (Vt) > 0.005 L/kg of body weight, 3- Heart rate < 140/min, 4- Body temperature < 37.5°C, 5- Partial arterial oxygen pressure (PaO₂) > 60 mmHg with inspired oxygen fraction (FiO₂) ≤ 0.4, 6- No need for vasoactive and/or inotropic support, 7- PaO₂/FiO₂ ratio > 200, 8- pH > 7.34, 8-No clinical signs and symptoms of acute respiratory distress (dyspnea, respiratory rate more than 24 breaths/minute, use of accessory muscles of respiration, presence of paradoxical breathing)". We did not referenced it to any article as these are normal values. The criterias for acute respiratory failure in Group 2 is explained on Page 3443. In Group 2 of patients acute respiratory failure is diagnosed with the following criteria after extubation within fourty eight hours and these include: 1- Spontaneous respiratory rate > 25/min, 2- SpO₂ < 90%, 3- Heart rate > 140/min (or more than 20% change from the initial heart rate), 4- Systolic blood pressure > 200 mmHg or < 80 mmHg, 5- PaO₂ ≤ 60 mmHg, 6- pH ≤ 7.30, and 7- Restlessness. These were not referenced either as these are parameters for acute respi-

ratory failure. The comment of Dr. Beyoglu and her colleagues as "The data given in Table 2 does not suggest the existence of acute respiratory failure in any of the two groups" is not true because the main concern in acute respiratory failure after cardiac surgeries are hypoxia and a PaO₂ less that 60 mmHg for a short period with a respiratory rate greater than 24/minute is sufficient for a diagnosis of acute respiratory failure and we apply 60% to 100% O₂ on these patients depending on the clinical status. All arterial blood gas values are on 60% to 100% O₂ in Table 2 before BIPAP and also we do not wait until patient has a pH of 7.2 or even 7.3. Our patients are critical cardiac or thoracic surgery patients after a serious operation and BIPAP is applied immediately when we saw clinical signs within fifteen to thirty minutes and this time period is not mentioned in our study because it is applied as soon as possible and in every patient similar time period was needed. There is no need for further references as these criterias are well described in related text books and review articles. There is an increase in PaO₂ values after application of BIPAP and this suggests that BIPAP provides a better oxygenation without causing hemodynamical deterioration in both group of patients. (Table 2) [2].

The reason for including cardiac and thoracic surgeries into the study is explained on page 3439 and it is clear that there is more than one reason for postoperative pulmonary dysfunction after cardiac or thoracic surgeries and in both of these operations there is incision on chest wall causing atelectasis, pleural opening, possible phrenic nevre injury, pain, prolonged recumbent position and reduction of diaphragmatic movement and for this reason both of these operations were included into the study and the numbers of these patients and their diagnosis were included. The use of cardiopulmonary bypass is not necessary to be enrolled into the study however, the main concern is the development of postoperative pulmonary dysfunction secondary to several chest wall and lung related causes (References are included in [2] as (1-4)). We do not agree with the concern of Dr. Beyoglu and her colleagues.

Pain is an important factor for postoperative pulmonary complications. In our study, pain control was provided by the protocol of the cardiothoracic unit and this include; intravenous

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paracetamol 1 gr every six hours during the first 24 hours after surgery if needed depending on verbal pain analog scale (VPAS) score that was measured every hourly by the nurses after extubation. Other pain modalities were not used. We did not include this in our study and we agree that this needs to be included into the protocol. We thank you for this concern.

The complication rates were statistically not different from each other between the groups and because of this I do not understand your paragraph regarding Table 3. There can be differences due to many other factors because of this we investigate and record our findings to show that there can be differences between similar groups although they may look like identical. Atelectasis has been written in the methodology and discussed in the introduction and discussion extensively and in results it is stated that "Postoperative complications related to respiratory and cardiovascular systems showed no significant differences ($P > 0.05$) (Table 3)". There can be differences in intensive care unit stay due to many other reasons and these do not have to be related to pulmonary causes so it was not our goal to investigate these issues in this study.

It is clearly stated in the sentence that carbon-dioxide retention, hypoxia and bradycardia may occur in some patients showing signs of acute respiratory failure and a reference was given for this. These are a part of the definition of failure of NIV. The level of carbondioxide rises and the other pathophysiological changes occurs after this [5]. Bradycardia is not listed in acute respiratory failure criteria in our study. We do not agree with Dr. Beyoglu and her colleagues comments.

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