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Circulatory Effects of Weaning from Mechanical Ventilation: The Importance of Transdiaphragmatic Pressure

WHAT IS THE MAJOR difference between the effect of spontaneous ventilation and that of intermittent positive-pressure ventilation on the circulatory system? During spontaneous ventilation, the airway pressure remains relatively constant, but, during conventional mechanical ventilation, the airway pressure rises. In pulmonary edema from acute left ventricular failure, our intuition tells us that positive pressure in the airways should be beneficial. There is a notable history of the use of continuous positive-pressure ventilation in the treatment of pulmonary edema from left ventricular failure beginning with the work of Emerson early in this century¹ and later popularized by Barach.² The mechanism proposed by Emerson and by Barach to account for its effectiveness was that positive airway pressure pushed fluid from the lungs back into the blood. When I ask a nurse, resident, or medical student why a patient with left ventricular failure often does better receiving positive-pressure ventilation, the answer I am most likely to get is along the lines proposed by Emerson and by Barach. Ironically, the major circulatory differences between spontaneous and mechanical ventilation have little to do with the difference in airway pressure and nearly everything to do with the difference in pleural and abdominal pressure.

When the respiratory muscles take over the work provided by the mechanical ventilator, the airway pressure relative to pleural pressure, the transpulmonary pressure, is unaltered because transpulmonary pressure depends only on the mechanical properties of the lungs. While it is true that mean airway and pleural pressure are lower during spontaneous than during intermittent positive-pressure ventilation, they are not changed relative to each other. As long as transpulmonary pressure remains unaltered, there is no direct effect on the heart or pulmonary vessels. When I travel from Baltimore to Denver, both my airway and my pleural pressure decrease by approximately 130 mmHg, a change orders of magnitude greater than the changes that occur between mechanical and spontaneous ventilation, but my lungs

are not congested and there is no added impediment to the ejection of blood from my left ventricle. The reason there is no change is that there is an equal decrease in the pressure around my systemic vessels. If I took my thorax and its contents to Denver and left the rest of my body in Baltimore (assuming low resistance and non-compliant conduits!), nearly all the blood would be pushed into the heart and pulmonary vessels, and my left ventricle could hardly eject any blood. In addition, my diaphragm could not generate enough tension to descend because of the high abdominal pressure relative to thoracic pressure. The high pressure surrounding the systemic blood vessels relative to the pressure around the right ventricle causes an increase in preload of the right heart, and the high pressure around the systemic vessels relative to the pressure around the left heart causes an increase in afterload of the left heart.

In the paper of Lemaire *et al.* in this issue of ANESTHESIOLOGY,³ patients with left ventricular dysfunction weaned from mechanical ventilation reduced the pressure around their heart and lungs by an average of 7 mmHg. The authors feel that it was this decrease in pressure that produced acute left ventricular failure through an increase in preload and afterload. It is refreshing to see that they emphasize changes in pleural pressure and not the more apparent changes in airway pressure. Indeed, the changes in pleural pressure are not at all apparent, and the authors had to quantify these changes by the special technique of measurement of esophageal pressure.

Lemaire *et al.* present compelling evidence that left ventricular failure occurred during spontaneous ventilation, and the measurements of heart volumes and vascular pressures strongly suggest that there was an increase in both preload and afterload when mechanical ventilation was terminated. If, indeed, the changes in preload and afterload produced by such small changes in pleural pressure were the cause of the acute left ventricular failure, this work emphasizes the great importance of ventilatory mechanics in the setting of compromised heart function. Further, if such small changes in one direction can produce failure, small changes in the opposite direction can be used for therapy.⁴

Is a change in pressure around the heart of a few mmHg sufficient to cause the large change in preload and afterload? There has been considerable controversy concerning the magnitude of the effect of pleural pressure on afterload, but general acceptance of a major

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Address reprint requests to Dr. Permutt: Department of Medicine, Francis Scott Key Medical Center, 4940 Eastern Avenue, Baltimore, Maryland 21224.

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effect on preload. We know from Guyton's work that a small change in the right atrial pressure can have a large effect on venous return because the gradient of venous return is so small.⁵ If the only effect on preload were from the decrease in the pressure around the right heart, there could be no increase in the gradient of venous return without a decrease in right atrial pressure relative to atmospheric pressure. From the data of table 2 in the paper of Lemaire *et al.*, surprisingly, it appears that the right atrial pressure rose relative to atmospheric pressure, for the right atrial transmural pressure rose more than the pleural pressure fell. This was so unexpected that I contacted one of the authors (Zapol) about this question. He said that this often happened, and he sent me a recorder tracing from one of the patients showing a large rise in right atrial pressure relative to atmospheric pressure during spontaneous ventilation, *i.e.*, the same pattern as the pulmonary artery occlusion pressure shown in Lemaire *et al.*'s figure 1. How, then, could the decrease in pleural pressure be responsible for the increase in preload? It cannot be; therefore, something else must be going on, and I believe the paradox can be resolved by a consideration of abdominal pressure.

During mechanical ventilation, the diaphragm is relaxed. During spontaneous ventilation, the muscles of the diaphragm contract, and this requires an increase in active tension. It is this increase in the active tension of the diaphragm that is responsible, in part, for the fall in pleural pressure. The magnitude of the increase in active diaphragmatic tension is only partly reflected by the fall in pleural pressure, but is more directly a function of the difference between pleural and abdominal pressure, the transdiaphragmatic pressure. During mechanical ventilation with the diaphragm relaxed, the rise in pleural pressure during inspiration is accompanied by a parallel increase in abdominal pressure with little change in transdiaphragmatic pressure. During spontaneous inspiration, the descent of the diaphragm produces a fall in pleural pressure and an increase in abdominal pressure.

We do not know the values of abdominal pressure in the patients of Lemaire *et al.*, but it is likely that the pressure in the stomach (as an indicator of abdominal pressure) changed in parallel with esophageal pressure during mechanical ventilation, but rose at the same time that esophageal pressure fell during spontaneous breathing.

The splanchnic circulation has a much greater compliance and resistance to venous return than the rest of the systemic circulation.⁶ These special mechanical properties allow the splanchnic circulation to play a profoundly important role in the regulation of venous return through changes in its arteriolar and venous

tone.⁷ In essence, the splanchnic circulation acts as a functional reservoir to adjust venous return and preload during various types of physiological stress, such as exercise and changes in posture and environmental temperature. The mechanical properties of the splanchnic blood vessels make them especially sensitive to changes in the pressure on their outer surface.⁸ A small increase in abdominal pressure is capable of causing a marked increase in preload, and this becomes exaggerated when the systemic circulation is congested, as in conditions where hepatajugular reflux is present.

On the basis of these considerations, it is more likely that the major cause of the increase in preload in going from mechanical to spontaneous ventilation was the increase in transdiaphragmatic pressure rather than the fall in pleural pressure. The strongest support for the importance of transdiaphragmatic rather than pleural pressure comes from the comparison of the two episodes of mechanical weaning. During the second episode, the fall in pleural pressure was even greater, but it seems likely that the increase in transdiaphragmatic pressure was considerably less. There is little reason to believe that 1 week of diuretic therapy had a significant effect on myocardial contractility, but the significant decrease in weight of 5 kg and blood volume of 1 kg between the first and second episodes could have resulted in a much smaller translocation of blood from the systemic circulation to the heart and lungs if we consider the role of transdiaphragmatic pressure.

It is highly likely that a large portion of the 5-kg weight decrease came from a loss of edema and blood volume from the abdominal viscera. Slight adjustments in vascular tone could maintain preload constant during the slow loss of blood volume in the week of diuretics. These are the expected physiological adaptations to loss of blood volume, and it is not surprising that there were not significant differences in the circulatory measurements while the patients were receiving mechanical ventilation. A decrease in abdominal volume during the week of diuretics could have had a huge effect on abdominal compliance, such that, with descent of the diaphragm during the second episode of spontaneous inspiration, the abdominal pressure rose considerably less than during the first. Compatible with the smaller increase in abdominal pressure during the second episode is the reversal of the direction of the change in right atrial pressure relative to atmospheric pressure. The transmural right atrial pressure now rose less than the esophageal pressure fell (from analysis of data received from Dr. Zapol).

While no measurements of abdominal pressure were made, further analysis of the data from Dr. Zapol lends support to the concept that when a patient is weaned from mechanical ventilation: 1) the major circulatory

changes are due to both an increase in preload and afterload, and 2) the increase in preload and afterload are largely the result of an increase in transdiaphragmatic pressure rather than a decrease in pleural pressure. These conclusions are based on the following considerations.

If the increase in transdiaphragmatic pressure caused an increase in both preload and afterload, we would expect a significant increase in both right atrial transmural pressure and decrease in left ventricular ejection fraction in going from mechanical to spontaneous ventilation. Furthermore, if the major difference in the circulatory response between the two episodes were due to the difference in abdominal pressure, we would expect to find a smaller increase in right atrial transmural pressure and a smaller decrease in left ventricular ejection fraction per mmHg fall in esophageal pressure between the first and second episodes. From the data I received from Dr. Zapol, these expectations are met. The right atrial transmural pressure increased 1.6 mmHg per 1 mmHg decrease in esophageal pressure during the first episode, and only 0.8 mmHg/mmHg during the second ($P < .05$ by paired t analysis). (Note that the ratio was greater than 1 on the first episode and less than 1 on the second, related to the directional change in right atrial pressure relative to atmospheric pressure.) The decrease in left ventricular ejection fraction in relation to the decrease in esophageal pressure was 2.3%/mmHg and $-0.1\%/mmHg$ ($P < .05$ by paired t analysis) between the first and second episodes, respectively.

Because the transdiaphragmatic pressure is directly related to the muscular tension of the diaphragm, it is a major determinant of the energy requirements of the diaphragm. Furthermore, there is evidence that diaphragmatic tension impedes diaphragmatic blood flow through mechanical compression of the muscular blood vessels. The magnitude of the transdiaphragmatic pressure is probably the single most important factor in the relation between supply and demand of energy, and, thus, plays a major role in determining whether respiratory muscle fatigue will or will not be present.⁹

If the abdominal viscera are congested and the compliance of the abdominal cavity reduced, the transdiaphragmatic pressure will have to increase more to provide the same tidal volume during spontaneous breathing. The magnitude of the decrease in pleural pressure during spontaneous inspiration for a given tidal volume is proportional to the elastic and resistive properties of the lungs. With no change in the mechanical properties of the lungs, the greater the rise in abdominal pressure, the greater the energy requirements and the less the diaphragmatic blood flow (unless adjusted through autoregulation). If we now add to these factors an increase

in preload and afterload in the presence of compromised heart function, the burden on the diaphragm becomes even greater; for it is not unlikely that the pulmonary congestion causes the lungs to be stiffer, and the transdiaphragmatic pressure will have to be even greater to provide the same tidal volume. It is not difficult to see how this vicious circle will lead to diaphragmatic fatigue and inability to sustain spontaneous ventilation.

I believe that just such a vicious circle was occurring in the patients studied by Lemaire *et al.* during the first attempt at weaning. If the study had not been repeated after a short time of diuretic therapy, the significance of that vicious circle could have been suspected, but would have been only one theoretical factor among a multitude of others that could be responsible for why a patient could not be weaned. Even the convincing demonstration of the development of left ventricular failure on the first attempt at weaning could have been interpreted as the result of the inability of the patients to maintain adequate ventilation. It is not unreasonable that the rise in P_{CO_2} and increase in oxygen consumption could have been the cause of the failure. The authors state that they "cannot dissociate cause from effect."

In my opinion, the major contribution of the study of Lemaire *et al.* is that they repeated it after diuresis. The importance of the repetition is not that the patients could now be weaned or that left ventricular failure did not occur. This could have been related to improvement from factors other than diuresis. From my point of view, the great contribution is that the data can be used to test our speculations on why left ventricular failure occurred on the first weaning attempt and not the second. I am completely unable to explain the differences without invoking the role of transdiaphragmatic pressure to account for the major circulatory and respiratory changes between mechanical and spontaneous ventilation in patients with compromised heart function. But once I am forced to think about transdiaphragmatic pressure, I now see a most attractive picture clearly for the first time, and this picture gives me new insight into the complicated and important interactions between circulation and respiration.

SOLBERT PERMUTT, M.D.

Professor of Medicine

School of Medicine

Professor of Environmental Health Sciences

School of Hygiene and Public Health

Professor of Anesthesiology and Critical Care Medicine

School of Medicine

Director of Research

*Division of Pulmonary Medicine
Department of Medicine
The Johns Hopkins University
School of Medicine
Francis Scott Key Medical Center
4940 Eastern Avenue
Baltimore, Maryland 21224*

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