to hypoxemic subjects during various activities, including rest, exercise, and sleep.[3–5] These studies demonstrated the clinical efficacy of the devices evaluated and proved the clinical equivalency to continuous flow.

Gallegos and Shigeoka used the air-dilution equation to illustrate how respiratory rate affects F_{IO_2}, but in their discussion they failed to fully account for how anatomical dead space and the changes in inspiratory time impact the net oxygen delivered via a continuous-flow system. In their example they compare a *total flow* of 1 L/min continuous to a *minute volume* of O_2 delivered via pulse-dose (using a 10-mL-per-breath bolus model) and suggest that a patient breathing 20 breaths per minute receives one fifth (200 mL) the O_2 they get from a 1 L/min continuous flow. This example fails to account for dead space and the fact that oxygen flowing during exhalation and the pause between breaths does not participate in gas exchange.

In modern, fixed-volume, pulse-dose devices, the net minute volume of O_2 delivered is the product of *respiratory rate* $\times$ *bolus volume*, independent of the inspiratory-expiratory ratio or inspiratory flow demand. Newer pulse-dose conservers deliver oxygen at higher flows and for shorter durations, limiting delivery to the first 100 ms of each breath and thus maximizing alveolar oxygen delivery. Using Gallegos and Shigeoka's example, a patient breathing 30 breaths/min with exercise on the same device (10 mL/breath) would get a total of 300 mL of O_2 per minute. Breathing 1 L/min continuous flow, maintaining a consistent inspiratory-expiratory ratio of 1:2 and assuming anatomical dead space of about 33%, the same 30-breaths/min patient would inspire about 7.3 mL of O_2 per breath, yielding a minute volume of 219 mL of oxygen, which is *81 mL less than the pulse-dose device*. Even when correcting for a slightly reduced O_2 percentage (eg, 89%), the pulse-dose device still provides 267 mL of O_2, which is 48 mL more net O_2 to the lungs.

A recent study by McCoy et al evaluated the performance of pulse-dose oxygen-conserving devices under various respiratory rates.[6] They found that as respiratory rate increases, pulse-dose devices more consistently maintain a target F_{IO_2} than does continuous flow, because the pulse-dose devices deliver a larger net minute volume of oxygen (respiratory rate $\times$ bolus volume). These results have also been supported by several clinical trials.[7–11]

Gallegos and Shigeoka's emphasis on the gas-mixing equation and calculation of F_{IO_2} is accurate and highlights the variability of oxygen concentration common to low-flow oxygen devices. Oxygen device manufacturers have recognized this for years, which is why most pulse-dose-device manufacturers recommend patient- and product-specific titration to ensure appropriate oxygen delivery. It is also the reason many pulmonary experts urge titration of *all* low flow oxygen systems to the patient's specific activity level.

Gallegos and Shigeoka state, "Clinicians have ignored the consequences of less-than-pure O_2, because of the shape of the hemoglobin-O_2 dissociation curve, limitations of pulse oximetry, and the ease of raising the flow to compensate." We disagree with that statement and note that, while the variables listed may explain why patients can clinically tolerate various devices, the patient's oxygen saturation has really been the driver of clinical acceptance and tolerance.

Technological advances in LTOT have resulted in a number of lighter, quieter, more efficient, and longer-lasting systems that, when properly matched to the patient's clinical requirements and lifestyle needs, essentially offer an unlimited supply of portable oxygen, with proven clinical performance. The goal is to improve the patient's quality of life by cutting the tether of the stationary oxygen device that has, historically, anchored the patient at home.

While we recognize that not all new oxygen devices are appropriate for all patients, the same holds true for all oxygen systems. Technological advances play an important role in improving the quality and cost of care provided. We strongly agree that oxygen-technology users should be thoroughly familiar with the function and application of the devices they employ. Misunderstandings, misconceptions, and the traditional dogma that so often plagues health care must be overcome. As clinicians we must spend more time understanding and adapting to systems and technology that can improve the quality of care and the lives of our patients.

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The authors respond:

We appreciate the comments of Lewarski, Messenger, and Williams about our editorial. We are pleased they agree with our conclusion that O_2 equipment should be evaluated with each patient, to ensure it provides adequate oxygenation: the "test drive." It is gratifying because they represent manufac-

turers and marketers of novel LTOT equipment. Editorials are, by design, expressions of opinion. All LTOT stakeholders should have a chance to express their opinions.

We expressed our concerns for several reasons. These include known limitations of concentrator-produced O_2, variability in demand O_2 valve performance, difficulties with "equivalent flow," problems with air-entrainment, and the dearth of published studies. Lewarski and colleagues acknowledge that published information is *still* limited, and of the 3 studies they cited (their References 4, 5, and 6), two existed only as abstracts when we saw our patient (though one, their Reference 3, has since been published in full form, in the March 2006 issue of RESPIRATORY CARE), and the studies involved small numbers of patients. Their Reference 6 was the *sole* full report when we saw our patient. We can imagine Lewarski and colleagues' frustration, because they have insider knowledge of both product development and preliminary clinical studies. They must be anxious for the respiratory community to learn more about their products. We hope they can imagine our frustration when the *sole* full report described D-size cylinders and did *not* mention an integrated demand valve (ie, the equipment appeared to be different from our patient's). We look forward to this second full report. We referenced the O_2-user's Web site because it contained the *only* easily accessed, *nonproprietary* descriptions of this novel equipment. It also provides the user's perspective (ie, someone who has to live with this novel equipment).

Novel O_2 systems are descendants of traditional systems, so we began our discussion by describing traditional equipment familiar to all respiratory therapists. We did not mean to imply that the equipment was the cause of the problem. We clearly described our patient's unrealistic expectations for ambulatory duration with his LTOT equipment, his medically unwise "solution" for extending duration, and his incredible assumption that our center had compatible novel equipment to refill his unique cylinder for his long drive home. We described the proprietary fittings and provided 2 illustrations to inform readers who may not be familiar with this novel equipment. We restricted our comments to 2 types of concentrator-based equipment, because a third device that produces liquid O_2 from concentrator-produced O_2 at home was (and is today) *too new*.

To expand on the problem of air-entrainment, we used a simple calculation familiar to students of respiratory care: the gas-mixing equation. We are pleased that Lewarski and colleagues used a form of this equation and obtained results similar to those in our editorial's Table 1, at setting "3" (approximately 30 mL) under columns A and B, for O_2 concentrations of 100% and 85%, with which the final delivered O_2 concentrations are 25.7% and 24.8%, respectively, which is a difference of less than 1%. These tiny O_2 concentrations are one way to demonstrate how well demand valves conserve O_2 under conditions established by the manufacturer. Manufacturers commonly describe demand-valve "efficiency" (ability to conserve O_2) by comparing O_2 *dispensed* by their valve with O_2 *dispensed* by conventional continuous-flow valves (eg, 200 mL/min at "setting 1" vs 1,000 mL/min at 1 L/min, which is a 5:1 savings). Clinicians, suppliers, and patients understand that simple comparison.

The important problem of "equivalent-flow" settings was addressed by McCoy[1] and revisited by Bliss et al,[2] who proposed a volume-referenced (flow) setting system to help patients and clinicians compare devices. It is unclear if manufacturers have agreed to adopt a uniform system.

Lewarski and colleagues describe a sophisticated approach to reduce the effects of air-entrainment; they account for dead space, O_2 rebreathing, and the timing of the O_2 bolus during inspiration. However, this requires highly specialized knowledge, is considered arcane by many, and is often a proprietary secret that promises performance advantages over a competitor's product. The average clinician and patient often do not possess this knowledge. The problem of flow equivalency and lack of published evaluations, discussed above, are pertinent.

Clinicians commonly face a big problem: that of trying to account for variability between patients, their illnesses, and circumstances (such as exercise) when more O_2 is needed to meet increased metabolic demand. This is more complicated than air-entrainment. A patient walking briskly on a treadmill may need to raise (pure) O_2 flow by 2 L/min above baseline resting flow with conventional equipment, may *not* have to raise flow with one brand of demand valve, and may *fail* to achieve adequate oxygenation with another brand of demand valve at the highest setting. When that patient requests a third brand of equipment, which provides less-than-pure O_2 through an integrated de-

mand valve that is different than previously tried demand valves, the clinician and supplier may not be able to extrapolate performance. Potential and reality may be different. This explains our recommendation for a test drive.

We apologize for confusing people with our terse statement that clinicians have ignored the consequences of less-than-pure O_2 because of the shape of the oxyhemoglobin curve, limitations of pulse oximetry, and the ease of raising flow. The context was conventional 100% O_2. Implicit in the following sentence and Reference 3 in our editorial[3] was the use of P_{aO_2} (oxygen partial pressure measured via blood-gas analysis), *not* pulse oximetry (S_{pO_2}). These factors raise problems and controversies about the limitations of pulse oximetry too complex to be addressed in a short editorial. We refer interested readers to the report by McGovern et al.[4] This may be seen in practice when patients who wish *not* to use LTOT *hyperventilate* just before staff obtain S_{pO_2} readings. Finally, experienced clinicians recognize that Medicare oximetry values that determine LTOT support span 3 saturation values (88%, 89%, and 90%), which is the same as instrument tolerance ($\pm 3\%$) of commonly used pulse oximeters!

We appreciate the opportunity to participate in an exchange of opinions about novel O_2 equipment. We are pleased that others agree with our recommendation to carefully match patient and equipment; that is, to take a test drive! We look forward to more published information about this novel equipment.

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