the above-noted methodologic flaws not present, the finding that intravenous therapy was no more beneficial than oral medication would still leave us in the dark given the study's low statistical power to detect anything but large differences in outcome measures.8

In choosing study outcomes and subject groups, clinical investigators must respect that nearly all asthmatic patients presenting for emergency treatment eventually recover. Consequently, studies should focus on meaningful endpoints such as survival in previously intubated patients, duration of hospitalization in those admitted with severe obstruction (FEV1 25 percent predicted), hospitalization in those presenting with moderately severe obstruction (FEV1 40 to 50 percent predicted), and relapse in those discharged home from the emergency room. Recent studies9 addressing the latter two endpoints are in need of validation given their potential major impact on clinical practice. On the other hand, it is time we stopped studying change in FEV1 as an endpoint unto itself.

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To the Editor:

Dr. Kern maintains that our patients were destined to recover without steroid therapy, as most asthmatic patients eventually would. Yet in his criticism of our methods he complains that we used too little methylprednisolone and did not give the iv group a bolus. He also feels that the patients responded too quickly for the steroids to have had any major impact, although the steroids were the most significant addition to therapy given the nearly-therapeutic mean theophylline levels on admission and the fact that the patients were all using inhaled or oral beta-agonists.

The above contradictions aside, I feel our major disagreement is the concept that, in order for patients with acute asthma to be hospitalized or receive steroid treatment, they must be screened in the emergency room with vigorous bronchodilator therapy. This approach to treating acute episodes of asthma has become questionable after recent studies have clearly shown the great value of steroids in improving treatment results and preventing relapses of patients thus treated.14 As a result of better understanding of asthma as an inflammatory disease, treatment results are likely to improve with a more liberal use of steroids, both short term in acute asthma and as maintenance therapy in the inhaled form.

In Iceland there have been less restrictive admission criteria for asthmatic patients and this is likely to account for the fact that our hospitalized patients were, on the average, not as severely ill as patients in some other studies. We feel that this in no way compromises our ability to draw conclusions from our data, which was carefully collected and included assessment of dyspnea and wheezing (not just spirometric data as implied by Dr. Kern). The steroid dose proved to be appropriate for the severity of disease and patients with the lowest FEV1, appeared to do quite well on either treatment program. We made no attempt to belittle the value of therapy with a larger iv dose of steroids in severe obstruction which we feel is likely of benefit but stand by our conclusion that oral administration of steroids and theophylline is just as effective as iv use in moderate disease.

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Flow Volume Loop

To the Editor:

It is with great interest that I read the article, "The Shape of the Maximum Expiratory Flow Volume Curve" by Kapp et al (Chest 1988; 94:799-806). I share their opinion that the usefulness of the flow-volume loop could further be enhanced by taking into account the configurational information contained in this relatively simple and widely used test. The difficulty is in expressing this configurational information—which is readily available by visual inspection of the shape of the flow-volume loop—in numerical indices to quantify and compare various shapes. In this respect, the proposed

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**FIGURE:** Three MEFV curves, one with a straight descending limb (—) and two curves with opposite sigmoid descending limbs (— and —) have been superimposed. The three MEFV curves shown, although having widely different shapes, will yield the same value for angle B because they are determined by similar values for VC, PEF and FEF50%. Expiratory flow (Vex) on the y-axis, volume (V) on the x-axis.
angle $\beta$, however valuable, has a major shortcoming in that it does not distinguish between the various shapes that are determined by the same relationship between the three determinants of angle $\beta$ (the vital capacity, peak expiratory flow and mid-vital capacity forced expiratory flow). This is illustrated in the Figure, where completely different and realistic maximal expiratory flow-volume (MEFV) curve shapes will yield similar values for the angle $\beta$. The same problem exists for previous attempts at quantifying the complex MEFV curve shape by a single variable such as the index proposed by Green et al. or Landau et al.'s mid-vital capacity curvilinearity score. In my opinion, adequate quantification of the MEFV curve shape requires a more complex analysis such as Mead's slope-ratio-volume relationship that analyzes the curve on a point-by-point basis, or a method that quantitates various segments of the curve rather than its overall contour, such as the chord slope analysis I proposed previously. It nevertheless is encouraging to see that, with other authors, Kapp et al. point to the usefulness of the analysis of the configurational aspects of the MEFV curve, showing that configurational parameters may contribute to the clinical utility of this test.

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To the Editor:

Dr. Vincken raises interesting observations which deserve further comment. We agree with his statement that "the difficulty is in expressing configurational information, which is readily available by visual inspection of the shape of the flow-volume loop, in numerical indices to be able to quantify and compare various shapes." The approach which we chose was to reduce this configuration to a single index (angle $\beta$) which could be easily derived from available parameters (in this case peak flow, Vmax50 and FVC). This index has intuitive geometric meaning and our analysis has shown that it is useful in documenting or defining individual and group respiratory abnormalities.

The specific criticism that Dr. Vincken raises is eminently plausible although its actual importance remains to be tested. Certainly we do not mean to argue that characterizing the configuration of the MEFV curve by a single index uniquely characterizes the shape of the MEFV curve. Clearly, such a characterization would have to take into account many more (if not all points) on the MEFV curve, as does chord slope analysis. What we hoped to avoid in selecting a single parameter was an analysis so cumbersome as to discourage its usefulness. After all, FEV$\text{a}$ does not characterize all flow rates on the MEFV curve yet its determination and study has proven empirically quite useful in measuring respiratory function. While Dr. Vincken's figure suggests that a given angle $\beta$ would, in fact, characterize a number of curves with dissimilar shapes, we would like to qualify this criticism. For a given individual the variation between maneuvers of angle $\beta$ within a series of blows or even between days is small (see Tables 6 and 7 of our study), suggesting that $\beta$ may serve as a useful parameter for comparisons in a single individual.

The question of the variability of angle $\beta$ between individuals is more complex and as can be appreciated from Dr. Vincken's figure depends to a great extent on the position of the volume at which peak flow occurs (relative to TLC). In a preliminary analysis of this question we have reviewed 34 MEFV curves from separate individuals and have found that the volume below TLC at which peak flow occurs (expressed as a percent of FVC) is restricted to a narrow range: 16.1 ± 7.3 percent (mean ± SD). This restriction implies that not all configurations of the MEFV curve are probable and that, in fact, some are distinctly unlikely. This observation suggests to us that $\beta$ may be more specific for a given flow volume shape than is implied by Dr. Vincken. Clearly, the issues which Dr. Vincken raises demand further careful analysis. We hope that his thoughtful reflections will stimulate more work on a measurement which we consider will prove useful both conceptually and empirically.

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Catheter Complications

To the Editor:

We read with great interest Scott's review of "Complications Associated with Central Venous Catheters, A Survey" (Chest 1988; 94:1221-24). Recently we reported a very unusual complication of CVCs—namely venobronchial fistula. This occurred six months after insertion of a Hickman catheter for systemic chemotherapy. The patient suffered transient episodes of hemoptysis after removal of catheter, but this rapidly disappeared and she subsequently required no major intervention.

Although Scott's findings indicate "that complications were primarily health professional technique-related", we believe that our case illustrates that a complication may develop as a result of the infusate through the CVC itself.

In addition, we seriously question the findings that infection is not a reported complication of CVCs. Those of us who insert and utilize these devices on a regular basis know that everything from simple insertion-site cellulitis to full-blown septic shock can occur. In this regard the MDR system seems to have failed.

Clearly the benefits of CVC use for a variety of diagnostic and therapeutic procedures are obvious. Although complications do exist in both short- and long-term central venous catheterization, we believe that this should not deter their use in clinical practice.

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