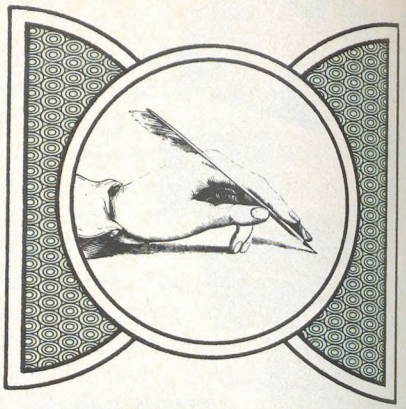
Letters to the Editor



The Journal welcomes Letters to the Editor; if found suitable, they will be published as space allows. Letters should be typed double-spaced, should not exceed 400 words, and are subject to abridgment and other editorial changes in accordance with journal style.

Cardiopulmonary Resuscitation Training

To the Editor:

I read with interest the article on "Cardiopulmonary Resuscitation Training in a Family Practice Residency" *(J Fam Pract 12:1013, 1981)*. I spent about two years at the University of Alabama in Tuscaloosa on the staff of the residency program. I find that the training of residents at the level of basic cardiac life support is inadequate for general medicine, at least in the family practice I am engaged in and have been exposed to during my ten years in family practice.

At Tuscaloosa we initiated a two-stage cardiopulmonary resuscitation program. The first stage involved the incoming first year resident class in their indoctrination, during which they received advanced cardiac life support instruction and were expected to pass it (or pass it in the following year if they failed during this period). The second stage was a refresher and recertification course in advanced cardiac life support at the onset of the third year.

We found that this worked quite well. Our success rates using the American Heart Association criteria ran around 85 to 90 percent in the first year and 100 percent in the third year. This enhanced our residents' credentials in the emergency room.

My experience as a rural physician and as a Navy physician has indicated that anybody doing this type of family practice needs to be absolutely comfortable in handling advanced cardiac life support resuscitation as well as advanced trauma support management until adequate transfer can be made.

My feeling is that the caliber of family practice should be such that any of the faculty members should be willing to and capable of teaching advanced cardiac life support. The article brings attention to the necessity to maintain high levels of clinical competency by the family physician. Advanced cardiac life support is a competence that should be an expected skill of the residency trained family physician.

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Continuity of Care

To the Editor:

Having recently reviewed the continuity of care literature with an emphasis on researchable issues, I was particularly interested in the article by Godkin and Rice *(Relationship of physician continuity to type of health problems in primary care. J Fam Pract 12:99, 1981)*. The authors focus on provider continu-

ity and suggest an association between chronic illness visits and increased continuity. Unfortunately, the study has serious shortcomings, both methodologically and conceptually.

A number of measurement indices of provider continuity exist currently. The continuity index proposed by the authors would appear to be a crude and unvalidated instrument. By their definition, a patient seen on six visits by three different physicians (continuity index of two) would be presumed to have the same continuity as if he or she had been seen on two occasions by the same physician. Shortell[1] and others[2-4] have provided more sophisticated and sensitive measures of provider continuity. Steinwachs, in the most comprehensive review to date of these measurement techniques, proposes that several indices may be more informative and valuable than one single continuity measure.[3]

Like many studies in this area, the affect of the setting (a residency training program) on the level of continuity has all but been ignored. Discrepancies already exist in the literature[5-7] as to the continuity achieved in the training environment. This fact would imply that provider continuity is influenced significantly by such variables as practice organization and setting, population characteristics (demographic data, health beliefs), and so on.

More importantly, to demonstrate that those with chronic diseases have a higher degree of provider continuity does not permit the assertion that these individuals are more effectively managed by one provider. First, one would have to separate informational continuity (ie, the medical record) from provider continuity. Second, one would need to have well-defined outcome measures that would be altered by differential continuity levels before making the leap of faith the authors share.

Further studies are most certainly called for to answer these issues so central to the theoretical framework of our burgeoning discipline.

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References

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3. Steinwachs DM: Measuring provider continuity in ambulatory care: An assessment of alternative approaches. Med Care 17:551, 1979
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The preceding letter was referred to Dr. Godkin and Ms. Rice, who respond as follows:

We would like to take this opportunity to respond to the critique by Dr. Wall of our recently published article.

In a recent unpublished study the Continuity Index (CI) has been shown to be a valid measure of physician continuity. Physician continuity was compared between a

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nonresidency setting and three residency training sites using the CI and two previously developed measures.[1,2] Each measure demonstrated a similarly higher degree of continuity in the nonresidency setting. We agree with Dr. Wall that valid comparisons of continuity between different sites are difficult to achieve because of variations in practice organization or catchment populations, ie, factors that will influence continuity. It is for this reason that we would like to suggest that continuity indices are most appropriately used by clinicians in practice (not researchers) for internal management purposes. Thus any index must be simple and calculated easily. One of the major problems with the aforementioned indices is their more complicated mode of calculation and, in one case,[1] difficulty of interpretation. The COC[1] (Continuity of Care) and SECON[2] (Sequential nature of provider Continuity) indices, for example, both require more detailed data collection than the CI.

Dr. Wall points out, correctly, that the CI cannot differentiate between six visits to three physicians and two visits to one physician. It must be remembered, however, that other sophisticated continuity indices have similar problems. For example, the COC index does not differentiate between sixteen visits to one physician and two visits to one physician, nor does it differentiate between sixteen visits to sixteen physicians and two visits to two physicians. An argument could be made, I guess, that differentiation is not appropriate, since the first example represents perfect continuity in the two situations and the second example reflects perfect discontinuity. A case could be made just as easily, however, which would suggest that, given the differences in numbers of visits in each pair of situations, there are considerable differences in the degree of continuity. Even so, it should be noted that using the COC, eight visits to five physicians is considered a lesser degree of continuity than four visits to three physicians, a result of questionable validity.

The major point to be drawn from these examples is that continuity indices are most appropriately applied to a whole practice population or a particular sample making an equal number of visits to the same number of physicians. It is very difficult and somewhat arbitrary to interpret differences in continuity between couplets representing different ''visits made/physicians encountered'' ratios. This is especially the case in the aforementioned examples and others that could be suggested.

Finally, we did not assert that a higher level of continuity *is* more appropriate or necessary for treating chronic illnesses. Rather, we say that *if* the above premise is valid (and intuitively it makes some sense), then the distribution of provider resources in the study sites is probably appropriate.

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