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.

HEALTH CARE IN NICARAGUA

To the Editor:

I was surprised and disappointed to see "A Report on Health Care in Nicaragua" in the Family Practice Grand Rounds (*Drickey R, Gin D, Rapp J: A report on health care in Nicaragua, J Fam Pract 1987; 24: 349–356*). I do not believe it meets your own definition of the purpose of Grand Rounds, and I do not believe the report is necessary so that physicians would understand that war causes bad health. Thus, what is the purpose of this article? My interpretation is that the article was presented out of political motivations and obviously deals with a very controversial topic in our society. If its purpose was to garner support for the Sandinista government in Nicaragua, only the readers as a group can tell us what the impact has been. After reading the article, I felt that at best it disinformed us, and at worst it was more propaganda from the Sandinista government. Here's why I believe this.

What is the source of information? Many statements were made that are judgmental, and the way in which these judgments were reached is not stated. On p 349 Daisy Gin makes the statement, ". . . health facilities that were targeted by Contra activity." How does she know that they were targeted? Could the facilities have been victims of an attack and not specifically designated as targets? Were health facilities marked as such, so that during an attack everyone, Sandinistas and Contras, would know what kind of facilities they were? This statement is highly political in nature, and if it is true, then it would be hard to justify continued support for the Contras regardless of one's political views on the US involvement. This assertion is again repeated on p 353 and even implies "health workers . . . are targeted." If this statement is not more propaganda, where is the support for these assertions? Finally, I believe the statement that "The health care system serves as a stabilizing force in the country" is either naive or arrogant because I do not believe that health care systems, other than offering improved nutrition, sanitation, and immunization status, have much impact on a culture's well-being.

Next, on p 350, Dr. Jonathan Rapp presents "some statistics reflecting the health of the Nicaraguan people under the two systems." The source of these statistics is not given. Why not? I am sure that the public health measures instituted by the government have had a positive impact on the health of those citizens who the "war during the revolution had a devastating effect on" (p 350). Thus, it appears to be self-serving to compare current statistics on health with data from a time when the country was in the midst of a revolution, as is done in Table 1. A more realistic comparison would be the current data compared with similar data taken from a time before the revolution was in full swing.

Again on p 353, numbers are given on casualties without sources of information, and a very political statement is made: "The Contras operate mainly by terrorizing the rural population with attacks and ambushes on small towns." If the sources of information are the citizenry, then how was the information obtained from them, ie, controlled by the government, through government interpreters, in their homes, etc? Nothing is said about the health of political prisoners or about those injured, killed, or missing because they opposed the Sandinista government.

In conclusion, I do not believe this article has a place in *The Journal of Family Practice* and appears to have a political agenda. Warfare is harmful to any culture, especially in the country where it takes place, and this article does nothing to further our knowledge of this fact.

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The preceding letter was referred to Dr. Drickey and colleagues, who respond as follows:

We appreciate the opportunity to respond to Dr. Mongan's comments regarding our Family Practice Grand Rounds on health care in Nicaragua.

Dr. Mongan believes that the article does not meet "your own definition of the purpose of Grand Rounds." As stated in the The Journal's Information for Authors, Family Practice Grand Rounds are "normally based on an interactive teaching conference . . . illustrating one or more of the basic concepts of family medicine." In the Family Health Center at San Francisco General Hospital, we serve many refugees and immigrants from Central America. To understand and work within the entire context of their lives is imperative. Central Americans in San Francisco remain very connected to their roots in their countries of origin. As community-oriented health workers and as responsible citizens, we are attempting to learn, therefore, about what is happening in Central America.

Dr. Mongan says he does "not believe the report is necessary so that physicians would understand that war

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causes bad health." The implication is that since it is known that war causes bad health, we do not need to talk about it further. We disagree. We believe that we must learn the facts about this war because it is a war in which the United States is involved.

Dr. Mongan asks how we know that health facilities were targeted by the Contras. The facts speak for themselves. As we stated in the article, 61 health facilities have been completely or partially destroyed since the war began (that number has risen to 68 since the article was written). We refer Dr. Mongan to a current article that substantiates the numbers that we cited.[1] Dr. Mongan goes on to say that if it is true that health facilities are being targeted, "then it would be hard to justify continued support for the Contras regardless of one's political views on the US involvement." We couldn't agree more fully.

Dr. Mongan believes that health care systems, other than offering improved nutrition, sanitation, and immunization, have little impact on a culture's well-being. We agree that preventive measures have more impact upon the long-term well-being of a society than do acute medical services. It is precisely these types of measures in which the reorganized health system in Nicaragua is most heavily involved. Its programs in nutrition, sanitation, and immunization have been lauded by the World Health Organization as model primary health care programs for the developing world.

The source for statistics on health care in Nicaragua, which Dr. Mongan requested, was the national Ministry of Health of Nicaragua.[2]

Dr. Mongan questions the comparison of statistics on health status and health care delivery from 1978, when the system was damaged by the war of revolution, with data from 1985. Garfield and Taboda[3] have shown that no significant changes occurred in the Nicaraguan health care system from 1974 to 1978. However, if one wishes to consider how special circumstances might impair health care delivery, it is worth noting that the US economic embargo of Nicaragua has made it impossible for that nation to obtain pharmaceutical and medical supplies from this hemisphere's major source of those items, the United States. As a result, vaccines, antibiotics, even spare parts for x-ray machines are in short supply.

As the Iran-Contra hearings have illustrated, our government is often the source of disinformation about events in Central America. We traveled to Nicaragua to see for ourselves what is happening there. We talked with people (unaccompanied by government officials or interpreters), we saw the devastation that is being wrought, and we cite our own observations as documentation for our comments in the article. We invite Dr. Mongan and others to go and see for themselves.

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3. Garfield RM, Taboda E: Health services reform in revolutionary Nicaragua. Am J Public Health, 1985; 74:1138–1144

INITIAL PRESCRIPTION FILLING

To the Editor:

I agree with Dr. Krogh and Dr. Wallner[1] that initial prescription filling is difficult to measure, especially when no central clearinghouse exists where all filled prescriptions can be collected and checked. Their survey of initial compliance using orange prescription pads was successful, but undoubtedly some compliant patients were missed because their pharmacists refused to participate.

Perhaps a more reliable method of

studying initial compliance is to visit patients in their homes a few days after their clinic visits. One can then ask to see the medicine bottles and know which ones were filled. Boyd and coworkers[2,3] studied compliance in this way in Oklahoma in 1974. They found that almost 94 percent of patients filled their prescriptions. This method is costly and time consuming and may overestimate initial compliance because compliant persons may be more likely than noncompliant persons to grant home interviews.

In the winter of 1986, I conducted a pilot study of initial compliance at the Family Practice Center of Charlotte Memorial Hospital and Medical Center (Charlotte, NC). The patients were free to fill their prescriptions at any pharmacy. I mailed each patient a survey concerning his or her initial compliance. If the patient did not respond, he or she was contacted by telephone. Eighteen patients filled 24 of the 27 prescriptions they received, achieving an 89 percent rate of initial compliance. Two patients did not comply because they felt they did not need the medicine. One said that he could not afford his medication.

Initial noncompliance leads to increased morbidity and mortality and is often more destructive than secondary noncompliance.[3] More research is needed to find the causes of not filling prescriptions. Health maintenance organizations and state Medicaid offices may provide the central collection areas needed to better study this issue. We physicians must know which factors we can change to ensure higher rates of initial prescription filling.

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RESULTS OF REMINDING FAMILY PHYSICIANS ABOUT ADMINISTRATION OF FLU VACCINE

To the Editor:

As influenza vaccination time approaches, we would like to share a study the nurses in our office did late last fall with the consent of one of our senior physicians. We decided to see what effect reminding the residents and staff physicians in our family practice office would have upon their requesting that influenza vaccine be administered to their patients.

For two weeks we attached a slip of paper on which was printed, "Do you want this patient to have flu vac-

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cine?" on the charts of all patients aged over 65 years. During that time 57 reminder slips resulted in 53 requests that we administer influenza vaccine. During the next two weeks, when we did not ask the physicians whether influenza vaccine should be given, only 14 influenza shots were requested.

As we did not make sure that there were as many elderly patients seen during the second period as during the first, we cannot attest that our experiment is statistically significant. However, we all feel certain that physicians will immunize more people against influenza when they are directly reminded to do so.

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PAINLESS SIGMOIDOSCOPY

To the Editor:

Flexible sigmoidoscopy permits the examination of more colon than the rigid instrument with less discomfort to the patient. Early in our experience with the new device, it was clear that one obstacle still needed to be overcome. Precisely because the tip of the sigmoidoscope is flexible and flat, it can be difficult and painful to insert into the anal canal of many patients, especially those with hemorrhoids, proctitis of any cause, obesity, or patients who are extremely anxious and have increased sphincter tone.

We have solved this problem by using a simple, disposable plastic anoscope. The anoscope is gently inserted to its full depth (about 10 cm) and the trochar withdrawn. The sigmoidoscope is then inserted through the anoscope to a depth of 10 to 12 cm. The anoscope is withdrawn over the sigmoidoscope back to the control head, where it is left until the completion of the examination. The sigmoidoscope is then advanced in the usual manner. We have used this technique in 150 examinations with no difficulties. In fact, when used in conjunction with good technique for the remainder of the procedure, many patients will have a painless examination.

We have been pleased with the effectiveness, ease, low cost, and safety of this "Warshaw manuever" and believe other physicians will find it very useful.

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SCREENING FOR URINARY TRACT INFECTIONS IN ELDERLY WOMEN

To the Editor:

The article by Bertakis and Ross (*Bertakis KD, Ross JL: Office evaluation of urinary tract infections in elderly women. J Fam Pract 1987; 24: 72–75*) is interesting but beside the point. To screen for an asymptomatic disease that has been present more than three months (by their definition) is probably important, but certainly does not require speed. A less complex schema is to await the results of culture, and if there is any question about the result, reculture the urine. There is no particular rush to diagnose a process that causes no short-term problem and certainly no advantage to using (and paying for) less than the gold standard when the gold standard is also to be done.

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WOMEN'S ADJUSTMENT TO MARITAL SEPARATION

To the Editor:

In the article "The Impact of Daily Stressors on Women's Adjustment to Marital Separation" (*J Fam Pract 1987; 24:507–511*), Oppenheimer concludes that "daily stressors are better predictors of psychological distress than the major life event of marital separation." However, the study findings do not support this conclusion, and in fact, the design of the study does not allow for an assessment of the effect of marital separation or divorce on psychological distress. The study group consisted only of women who had experienced a separation or divorce sometime during the preceding three years. No women who had not experienced separation or divorce were studied. To determine the impact of this major life event, women who were exposed to it would need to be compared with women who were not exposed. Considering the individual variability in the length of time necessary to cope with and adjust to such a major life event, recency of the separation or divorce is probably not an adequate proxy measure of the impact of the event.

Even if one accepts the assumption that recency of separation or divorce does reflect the impact of the event, there is a problem with the conceptual base of the analysis in this study. On the basis of the results of a multiple-regression analysis, Oppenheimer concludes that daily stressor frequency is associated with psychological distress on follow-up, while recency of separation is not. However, including stressor frequency and stressor intensity as independent variables in the multivariate model may obscure the association of recency of separation or divorce with distress. This confounding would occur if daily stressors constitute an intervening or intermediary variable in a causal pathway between recency of separation or divorce and distress.[1] In the introduction the author speculates that daily hassles or stressors might mediate the relationship between the event and distress; if this were true, including stressors as an independent variable in the regression analysis would partially or completely cancel out the variance in distress that is explained by recency of separation or divorce.

To explore the possible mediating role of daily stressors, additional analysis is necessary. The correlation of stressors with recency of separation or divorce should be determined. Then the association of recency with distress should be examined with and without the daily stressor variables included in the regression model. The hypothesis of a mediating effect of daily stressors would be supported by the finding of a correlation of stressors with recency and by the finding of an association of recency with distress that decreased or disappeared when stressor variables were added to the regression equation. Unfortunately, in this study daily stressors are conceptualized as mediators of the relationship between marital separation or divorce and distress, but are analyzed as confounders of this relationship.

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Reference

1. Rothman KJ: Modern Epidemiology. Boston, Little, Brown, 1986, p 91

The preceding letter was referred to Dr. Oppenheimer, who responds as follows:

It appears that Dr. Blake misinterpreted the purpose and therefore the design of my study. First, he faults the design by saying that ". . . the design of the study does not allow for an assessment of the effect of marital separation or divorce on psychological distress." He continues by asserting that a control group is necessary to answer this question. Blake is correct on this point, *if* I had wished to examine the effect of marital separation on psychological distress. However, this relationship is no longer an interesting research question, as it has

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been well-documented that persons who have experienced marital separation appear more pathological on virtually every outcome measure than persons who have not experienced marital separation.[1] In addition, implicit in the group comparison method is the assumption that persons who have experienced marital separation comprise a homogeneous group. Research has demonstrated that there is variability in the amount of distress reported following marital separation.[2] Thus, the interesting research question becomes: Given that marital separation has occurred, what are the variables that predict distress following marital separation? This question is exactly that which my study was designed to answer. Since I investigated predictors of distress within a group, no separate control group is needed.

Blake's second point, that "recency of separation or divorce is probably not an adequate proxy measure of the impact of the event," is not supported by the divorce literature or crisis theory, which served as the theoretical basis for my investigation. Within a crisis theory model, separation or divorce is a stressful life event that results in a temporary state of psychological disorganization and disequilibrium.[3] Generally, the acute distress lasts for a period of several weeks to a few months,[3,4] although research has shown that the disorganizing experience of divorce can extend over several years.[5,6] However, studies have suggested that the psychological sequelae of marital separation is greatest in the first six months following the event,[7] and that women who have been separated three or more years do not significantly differ from their married counterparts on indices of psychological distress.[8] Thus, the decision to operationalize the impact of marital separation by the recency of the event was both theoretically and empirically derived.

Third, Blake contends that there is a conceptual problem with my study. He stated, "Oppenheimer concludes that daily stressor frequency is associated with psychological distress on follow-up, while recency of separation is not." The key word here is association. A correlation between two variables does not suggest that they are causally related. The conceptual problem seems to be in Blake's apparent assumption that my study was designed to demonstrate that daily hassles *cause* psychological distress. My findings indicated that daily hassles are better predictors of psychological distress than the mere fact that separation has occurred, thus making this clinically useful information for physicians. Furthermore, I acknowledge in the discussion the potential confounding of these variables by stating that a limitation of this study was that both measures were self-reported, thus allowing the possibility that high levels of psychological distress may have resulted in increased perceptions of events as stressful (p 510). Thus, Blake's statistical argument seems superfluous given that invalid assumptions were made with respect to the rationale for the study, the hypotheses tested, and the research design.

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