

## VIEWS ON THE PLO

IN THE REVIEW of my recent book *The Palestinian Liberation Organization: People, Power and Politics* (Book World, March 25), Dr. Daniel Pipes made several serious charges which I consider injure my standing in my profession as a writer. These charges, as listed below, are untrue.

However, by far the most disappointing thing about the review you published was its failure to deal in any serious way at all with the central theme of the book, which was to try to analyze how the decision-making process in the PLO actually works.

This subject is, quite rightly, a matter of serious concern to all those interested in the fate of the Middle Eastern region. Yet such are the difficulties of tackling the subject, that no book-length work addressing it in all its dimensions had been published since 1973. And of course, momentous events have affected the PLO since then.

My book was the result of five years of covering Middle East affairs as Beirut correspondent of the *London Sunday Times*, and the *Christian Science Monitor*. That direct experience was augmented, in my work on the book, by a full year of research conducted with the support of Harvard University's Center for International Affairs, and Georgetown University's Center for Contemporary Arab Studies.

As a result of all that work, in my book I was able to reach certain conclusions about the PLO's power structure and decision-making which provide new insights into the analysis of PLO politics. But did Pipes' review describe and seek to evaluate these conclusions at all? He did not.

Instead, half of his review was filled with random lists of adjectives, taken from the book and presented outside of any contextual framework whatever. The other half contained charges against me which I would herewith like to refute. I am not, and never have been, a "court historian of the PLO," as he alleges—but which charge he is totally unable to substantiate. The book is not, as he claims, "replete with spelling mistakes in English and Arabic . . . and wrong dates." There are, as is almost inevitable in a work of this length, a small number of typographical errors in the book. I have counted six so far, in the entire 261 pages of text. But the serious slur on my professional competence implied by this charge is in no way backed up by the facts. Finally, Pipes claimed that a point I had made concerning the number of fighters on each side in the Arab-Jewish fighting of 1948, as well as "many other assertions" I had made—all unidentified—were "simply wrong." Again, he was unable to back up this claim with any solid facts.

It is a pity that a book on such a serious subject should have been treated by your reviewer in such a sloppy way. I hope that next time a book comes out on a similar subject you will be able to find a reviewer who appears able to deal with it on the basis of some familiarity with its central theme.

Helena Cobban  
Washington, D.C.

## Daniel Pipes replies:

MY REVIEW of Helena Cobban's book makes two main points: that the author's pro-PLO outlook permeates her study and that her research and writing are of poor quality. In her letter, Cobban addresses neither of these points; instead, she raises a great number of irrelevant matters.

First, what does it matter that her book deals with a

topic not addressed by anyone since 1973? And why does Cobban ignore the very study I cited in the review, Aaron David Miller's *The PLO and the Politics of Survival*, published in 1983? And what of John Amos' *The Palestinian Resistance* (1980) or Aryeh Yodfat and Yuval Arnon-Ohanna's *PLO Strategy and Tactics* (1981)?

That the book required five years to research and was subsidized by prestigious universities strikes me, again, as unconnected to the issue at hand, namely the quality of the book. Need one point out that no credentials can guarantee a good book?

The "random lists of adjectives" in the review—such as examples of the repeated uses of "harsh" in connection with Israeli policy—have the virtue of concisely and irrefutably indicating the nature of her bias.

I dubbed the author "court historian of the PLO" in a humorous and metaphorical way (the PLO has no court, Cobban is not a historian). By this, I meant to emphasize her unblushing adherence to the official viewpoint of the al-Fatah organization. Substantiation of this charge comes from the simple fact that she not once criticizes al-Fatah.

The book is indeed replete with mistakes. These did not enter into the review because of limitations on space—and again must mostly be omitted here. Here are, for example, four cases in which she made mistakes in dating: The Cairo Agreement was reached in November 1969 and an Arab summit meeting took place in Cairo in October 1976; the author refers to the time elapsed between these two events as eight years. Contrary to what is written, Anwar Sadat was not killed on the festival of Id al-Adha. Another passage indicates that King Farouq of Egypt was overthrown three years after 1951, or 1954—whereas the correct date is 1952. The current boundary between Israel and Egypt does not date to the first half of the 19th century but to a century later.

On the matter of Israel and the Arab troops in 1948, I again lacked the space in the review to back my challenge with "solid facts." Here they are. The author makes two assertions in her book: (a) that the "total number of Arab soldiers mustered in 1948 came to only 24,000," and (b) that this number was "far fewer than the number of fighters raised by Jewish groups in Palestine." Both of these assertions are demonstrably wrong.

Cobban supports her position by quoting Nadav Saffran, *From War to War*. But the very book she cites includes the following statistics for Arab soldiers: 60,000 "mobilizable" Egyptian troops in 1949-50; 38,000 Iraqis in 1949-50; 6,000 Jordanians "at the outset of the Palestine war in 1948"; and 8,000 Syrians "when the 1948 war broke out." In addition to these 112,000 soldiers were those of Lebanon and Saudi Arabia, Palestinian irregulars, and volunteers (such as the Muslim Brethren from Egypt)—perhaps another 15,000 troops. Even taking into account that some of these figures date from 1949-50, I fail to see how they can be reconciled with the figure of 24,000 soldiers Cobban refers to.

On the second point, that Israeli soldiers outnumbered the Arabs: Again, using the book that Cobban cites, *From War to War*, we find that Israel "mobilized for the 1948 war" 60-70,000 troops.

While it is understandable that Helena Cobban should object to a negative review, she fails to prove that I was in any manner misleading or inaccurate.

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