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Not Exactly the Path I Had Planned

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Series Editor: Diane Labrosse | Production Editor: George Fujii

ESSAY BY EDWIN E. MOISE, CLEMSON UNIVERSITY

My father, also named Edwin E. Moise, had more influence on the way I think about history than any of the professors who were formally my teachers. He was a mathematician, but had a wide range of other interests, including history both ancient and modern. I always planned to follow him into academia, but the path I followed took some unexpected turns.

Probably by late 1962, definitely in 1963, we both got very interested in Vietnam, and critical of U.S. policy there. But my academic focus was on math and the physical sciences. I graduated from high school in 1963 with four years of French, two years of Russian, AP credit in math and chemistry, and just enough history to get me past the graduation requirement.

During my freshman year at Harvard I was expecting to focus on second-year calculus and a notoriously difficult physics course, but what really caught my interest was the American History survey. By the end of the year I had decided to major in history. Sophomore year I took a wide range of courses, but none dealt with Asia. In my junior and senior years I focused on Asia, mostly on China. Harvard faculty had a lot of expertise on China and Japan in those days, but very little on India, and essentially none on Southeast Asia. When I decided to write an undergraduate honors thesis on French imperialism in late-nineteenth century Vietnam, the History Department gave me the specialist on Manchuria and Mongolia, Joseph Fletcher, as a supervisor.

I took courses from some of the top China scholars in the United States—John Fairbank, Benjamin Schwartz, and Ezra Vogel—but I did not work closely with any of them.

In the fall of 1963, I joined Tocsin, the anti-war student group at Harvard that was unofficially affiliated with Students for a Democratic Society (SDS). It formally became a branch of SDS in 1964. I knew considerably more about Vietnam and the war than most other members, so when an undergraduate student was needed to speak for the anti-war movement in a public forum, I was sometimes the choice. On one occasion, I debated the war against an assistant professor of government from Wellesley College, in front of the student body of a Catholic high school in Weston, Massachusetts.

My father was one of the two most conspicuous anti-war figures on the senior faculty at Harvard. He debated the war in front of larger audiences than I did, and against more important opponents. In one televised debate he was up against Zbigniew Brzezinski, professor of government at Harvard and future national security adviser to President Jimmy Carter. My father and I cannot have had precisely identical views, but I do not recall any disagreements about the war. We shared books and information to our mutual benefit.

I decided to go the University of Michigan for graduate study, rather than staying at Harvard, because I wanted a place where I could study not just China but also Southeast Asia and India, starting with an M.A. in Southeast Asian Studies. There I soon got my first exposure to diplomatic history, in a course on US relations with Asia. Every day, in the center of

the front row, I sat next to a uniformed U.S. Army major. We got along fine, but the professor seldom had to look far to find contrasting student opinions.

There did not seem to be enough anti-war activity going on in Ann Arbor for me to be drawn into it.

I was in a two-year M.A. program, but after the first year I joined the Peace Corps and went to Sarawak, in East Malaysia, as a teacher. I could make this sound pretty exotic; the headhunting days of the Iban and some other tribes were not that far in the past. I once spent two nights in an Iban village that still had heads hung from the rafters. But I lived in a town with a mostly Chinese population. The government was decent and competent, and local standards of sanitation and health were good.

I was on the job in the Peace Corps for calendar years 1969 and 1970, but Peace Corps training took three months in late 1968, and I travelled in Asia for several months in early 1971 before returning to the United States, so overall I missed three academic years at Michigan, 1968-1971. I had assumed when I went to Malaysia that this would significantly impact, and benefit, my career as an Asia scholar. I got a start on learning the Malay language, and in Singapore in early 1971 I spent some time gathering data on Malaysian politics that I expected to use in a research project at Michigan. But when I got back to Ann Arbor in the fall of 1971, I found that the university had hired John Whitmore, an actual historian of Vietnam, which was pretty rare in American academia in those days. I surprised myself by dropping all thought of Malaysia, and focusing on China and Vietnam for the rest of my graduate career. I worked with Whitmore on the Vietnam side, and with Albert Feuerwerker, Ernest Young, and Norma Diamond on the China side.

I wrote my M.A. thesis on the origins of the National Liberation Front in South Vietnam, 1954-1960. It was while researching this that I recognized the falsehood of a story that I and many others in the anti-war movement had long accepted, that the guerrilla struggle had begun in the late 1950s as a purely South Vietnamese initiative, without the support of and indeed against the wishes of the Communist leadership in North Vietnam. According to this story, the Communist leaders in Hanoi had only come around to accepting and supporting the struggle in the South late in 1960. This turned out to have been Communist disinformation. I was embarrassed, but not very embarrassed; I felt I had not done too badly. I had accepted the reality—that Hanoi had decided early in 1959 to begin promoting guerrilla warfare in South Vietnam—as soon as I encountered serious evidence for it during academic year 1971-1972. Many on the other side clung much longer than that to a false narrative that understated the role of South Vietnamese Communists, treating the Communist struggle for control of Vietnam as an essentially North Vietnamese enterprise.

My Ph.D. was officially in History, but I seldom set foot in the History Department offices at Michigan. My academic home was Lane Hall, the building housing the Center for Chinese Studies and the Center for Southeast Asian Studies. My dissertation examined Communist land reform in China (roughly 1930 to 1953, with most of it occurring from 1950 to 1952) and North Vietnam (1953 to 1956, with most of it between mid-1955 and mid-1956). There was no possibility of doing research in either China or Vietnam, and the sources available in the United States were limited. But I actually got some use out of my two years of high-school Russian; there were Vietnamese documents that I could not find in Vietnamese but was able to read in Russian translation.

I described the Chinese land reform as massively brutal, with a high death toll, but most of the brutality had a point. The Communists were killing people they needed to kill in order to consolidate their power. The Vietnamese land reform was characterized by pointless brutality. The Communist Party was not just harming Vietnamese society, it was harming itself by attacking and in many cases executing people it had no reason to attack. There had been anti-Communist propagandists who exaggerated the death toll of the North Vietnamese land reform, and to some extent I looked like a defender of the Communists when I pointed out these exaggerations, but I was not denying—I was pointing out strongly, and at length—that what the Communists actually did had been extremely bad.

Clemson University hired me in 1979 as a historian of East Asia, primarily China. I published my dissertation in 1983, and a history of modern China in 1986,¹ but by that time three things had happened that were changing my trajectory, making me primarily a historian of the Vietnam War.

First, the man who had been going to teach military history in my department died. The department head came to me and asked whether I would take on the course. I said yes, if given time to prepare for it. A very good one-month summer program at West Point, for professors teaching military history to Reserve Officers' Training Corps (ROTC) cadets at civilian institutions, helped. That was the first formal training I had ever gotten in military history, although I had done quite a bit of reading, mostly on World War II, as a teen-age war buff. I had disposed of most of the war books when my parents moved from a large house to a small one in 1971, which was something I came to regret in the 1980s. Teaching military history pushed me toward making it a research subject.

Second, CBS television broadcast a documentary titled "The Uncounted Enemy" on the way the U.S. military had underestimated enemy strength in South Vietnam in 1967, in an effort to show that the United States was winning the Vietnam War. General William Westmoreland sued for libel. The lawsuit led to public release of much information that otherwise would not have become available for many years, if ever. I published one article on the dispute, then set the issue aside to focus on other matters. I finally returned to the topic in 2007, and the result was *The Myths of Tet: The Most Misunderstood Event of the Vietnam War* (2017).²

Finally, teaching military history led me to join the Military Book Club. One month I forgot to send in the postcard to say I did not want the selections of the month, so Admiral Jim Stockdale's memoir³ showed up in the mail, and I started reading. The first chapter described how on the night of 4 August 1964, while two U.S. destroyers were firing at what looked on their radar screens like hostile torpedo boats, in what came to be known as the second Tonkin Gulf incident, Stockdale had been flying low overhead, looking for torpedo boats and not finding any. He said he could not have failed to see the torpedo boats if any had been present. That looked interesting enough to merit further investigation. The research took me into unfamiliar territory, both in the sense that I finally did some actual research in Vietnam, and in the sense that I had to learn to conduct interviews, do archival research, and use the Freedom of Information Act.

The interviews presented me with a challenge. I spoke with about twenty Americans who had been participants in the incident of August 4. At the time, most had believed that an actual torpedo boat attack was occurring. By the time I interviewed them, decades later, there were more doubters than believers, but the ones who believed in the attack were more certain in their opinions. If I factored in degree of certainty, I could say that my eyewitnesses were evenly split on this rather fundamental question. Whether I decided that the destroyers had been firing at genuine attackers or that they had been firing at ghost images on their radar—essentially at empty ocean—I would be rejecting the judgment of a lot of my eyewitnesses. But the evidence from sources other than my interviews was so strongly on the side of no attack that I was confident in reaching that conclusion. I published *Tonkin Gulf and the Escalation of the Vietnam War* in 1996, and a revised

¹ Edwin E. Moise, *Land Reform in China and North Vietnam: Consolidating the Revolution at the Village Level* (Chapel Hill: University of North Carolina Press, 1983); Moise, *Modern China: A History* (London and New York: Longman, 1986).

² Moise, *The Myths of Tet: The Most Misunderstood Event of the Vietnam War* (Lawrence: University Press of Kansas, 2017).

³ Jim and Sybil Stockdale, *In Love and War: The Story of a Family's Ordeal and Sacrifice during the Vietnam Years* (New York: Harper & Row, 1984).

edition, incorporating new information both from the National Security Agency and from Hanoi, in 2019.⁴ This is the best book I have written, probably the best I will ever write.

I enjoyed A. G. Hopkins's quotation from Erasmus in his contribution to this series to the effect that revising and reworking one's writings over a very long period is an exercise in self-torture.⁵ For me, however, taking a long time offers an opportunity to get things right. A large part of the reason my two most recent books (*The Myths of Tet* and the revised edition of *Tonkin Gulf*) are so much better than my first is that my first book was published only nine years after I began serious work on it. On each of the two recent ones, I had worked off and on for well over thirty years.

My anti-war stance clearly was crucial to the initial development of my interest in the Vietnam War, but it has little influence on how I write about the war as a scholar. I have not suggested in print either that the war was unwinnable for the U.S. or that it was immoral and illegitimate. The closest I have come to taking positions on such issues was in two minor articles that actually pointed in opposite directions: one arguing that the belief of many hawks in the domino theory was foolish, and one arguing that the belief of many doves, that the war was unnecessary because an acceptable solution would have been available through negotiations, was foolish.⁶

When I wrote about the dispute over estimates of Communist troop strength, in *The Myths of Tet*, I was taking sides in a dispute between two groups of hawks, not in a dispute between hawks and doves. My book on Tonkin Gulf demonstrates that there was no attack on U.S. ships in the August 4 incident, but does not use this as an argument against the legitimacy of the Vietnam War. It rejects the idea that the incident caused the escalation of the Vietnam War, and rejects the idea that the report of a North Vietnamese attack on US ships on August 4, 1964, was a deliberate lie by the U.S. government. Frankly, I find the truth—that the incorrect report of an attack on August 4 began as an honest mistake—to be more interesting than a narrative treating the incorrect report as a deliberate lie would have been.

There has recently emerged a cohort of scholars, most considerably younger than I am (though Merle Pribbenow is an exception), who are using a wider range of Vietnamese-language sources than I do in studies of various aspects of the Vietnam War.⁷ Their language skills are better than mine, and they get to Vietnam more often than I do. I have been eagerly awaiting Lien-Hang T. Nguyen's book on the Tet Offensive, which I hope will be published this year. She surely will have found important things I did not find, and I will not be astonished if she finds that I got something wrong in my book on Tet.

⁴ Moise, *Tonkin Gulf and the Escalation of the Vietnam War* (Chapel Hill: University of North Carolina Press, 1996); Edwin E. Moise, *Tonkin Gulf and the Escalation of the Vietnam War*, revised edition (Annapolis: Naval Institute Press, 2019).

⁵ A. G. Hopkins, "How I Got from There to Here," Essay Series on Learning the Scholar's Craft: Reflections of Historians and International Relations Scholars, 12 February 2020, <https://hdiplo.org/to/E190>.

⁶ Moise, "The Domino Theory," in Alexander DeConde et. al., eds., *Encyclopedia of American Foreign Policy*, 2d ed. (New York: Scribner's, 2002), vol. 1, 551-559; Moise, "The Mirage of Negotiations," in Lloyd Gardner and Ted Gittinger, eds., *The Search for Peace in Vietnam, 1964-1968* (College Station: Texas A&M University Press, 2004): 73-82.

⁷ See, for example, Lien-Hang T. Nguyen, *Hanoi's War: An International History of the War for Peace in Vietnam* (Chapel Hill: University of North Carolina Press, 2012); Edward Miller, *Misalliance: Ngo Dinh Diem, the United States, and the Fate of South Vietnam* (Cambridge: Harvard University Press, 2013); and Philip E. Catton, *Diem's Final Failure: Prelude to America's War in Vietnam* (Lawrence: University Press of Kansas, 2002). Merle Pribbenow has been important as a translator, but also has written articles such as "General Vo Nguyen Giap and the Mysterious Evolution of the Plan for the 1968 Tet Offensive," *Journal of Vietnamese Studies* 3:2 (Summer 2008):1-33.

When the U.S. went to war in Iraq in 2003, I shocked and distressed my wife by not opposing the war. I was on the fence, neither for nor against, but very interested. I have not yet published anything significant on the U.S. involvement with Iraq, but I will be saying a good bit about it in a book on which I am now working, *An Asymmetric Power: The United States and Its Asymmetric Wars*.

I am amused to note a certain similarity between the way I look at my own career, with its unpredictable trajectory, and the way I look at the policies of the governments and policymakers I study. If there were a “Moise theory” of international relations, it would be that policymakers have a very limited understanding of the situations they face. They cannot even predict their own future actions as well as they, and we as scholars, often think they can. This is not just because their actions are often responses to events they had not anticipated. Even if they did think about a possible future event, the way they planned to react to it may not be what they actually do when the event arrives.

Edwin E. Moise is a professor of history at Clemson University. His primary fields of interest are the modern history of Vietnam and China, and modern military history. His greatest expertise is on the Vietnam War.