Book Reviews

The Historiography of Nuclear Diplomacy

A World Destroyed. The Atomic Bomb and the Grand Alliance. MARTIN J. SHERWIN. Knopf, New York, 1975. xvi, 316 pp. + index. $10.

Martin Sherwin has written what is really a double book, or two books in one. His first theme is the development of the first atomic weapons and the role they played in American foreign policy toward Britain and the Soviet Union up to the Potsdam Conference of July 1945, the Alamogordo test, and the destruction of Hiroshima and Nagasaki. His second theme is the conflict between the views of the nuclear scientists and those of the American, and to a lesser extent the British, policy-makers on the diplomatic uses to which American control of the development of nuclear weapons was to be put in planning a postwar settlement.

In covering the first theme, Sherwin is treading a path already trod by Gar Alperovitz and Herbert Feis, to name only the most sensational assailant of American policy and its most distinguished and determined defender. He has been able to make use of much more extensive materials than either of his predecessors in that, inter alia, he has benefited from the opening of the British records, which, being far more systematic and coherent than American records, throw a great deal more light on President Roosevelt's activities than American records can alone provide. He rises above his predecessors too in that he rarely confuses, as Alperovitz, for example, and his followers regularly do, documents proferring advice or urging a particular course of action upon the President with documents recording presidential decisions and the acts of policy-making that flow from them.

His book is thus as good a guide as we can ever hope to have to the development of Roosevelt's thinking to that moment of indecision which was overtaken by his death and to the clear and overrapid moments of decision which President Truman substituted for his predecessor's tortuous instinctual and largely subvocal deliberation. If fault can be found it lies in Sherwin's failure to integrate the theme of control of nuclear weapons and Roosevelt's thoughts on the subject with the wider issues of Roosevelt's thinking on the future pattern of world order, the mechanisms for protecting that order from disruption, and the ambiguities and unresolved conflicts which that thinking embraced. One obvious conflict lies in the contrast, which Sherwin mentions, between the concessions Roosevelt, unlike either his advisers or his successors, was prepared to make to Britain on the issues of shared knowledge and shared control and his constant hostility to the instruments on which the strength, if not the existence, of the British empire depended. The reverse side of this coin, to which Sherwin ought perhaps to have paid equal attention, is the degree to which Roosevelt courted Stalin at Teheran and Yalta by deliberately denigrating Britain's status and leadership and overruling British interests as seen by that leadership. It is at least possible that in nuclear sharing with a Britain by its own admission so concentrated on sheer survival as to be unable to afford the massive diversion of resources to the development of nuclear weapons, Roosevelt saw a means of control of British policy after the war, where his advisers could see no further than an America isolated in a Hobbesian world of spies and potential enemies. We simply do not know; and it is unlikely we ever will know. Nor is it likely that we will ever be able to answer the even more difficult question of what priority Roosevelt gave in his own mind to nuclear sharing and the control of nuclear weapons. This too needs to be looked at in a wider context than that employed by Sherwin before any hypothesis can be advanced.

Sherwin is distracted from any closer examination of these issues by his acceptance of two propositions, neither of which he subjects to any real examination. The first of these is that the American monopoly of the development of nuclear weapons and the maneuvers which American policy-makers went through in the effort to capitalize on that monopoly in the first few months after Roosevelt's death played a major part in the breakup of the "Grand Alliance" (a Churchillian hyperbole for the uneasy wartime cooperation between Britain, the United States, and the Soviet Union), out of which the Cold War developed. The second is that the proposals for international control of nuclear developments urged on Roosevelt and Churchill by the nuclear scientists, most notably by Niels Bohr, should have been adopted, could have been adopted, and, had they been adopted, would have made a firm basis for a continuation of the Grand Alliance into the postwar years. Of these two propositions, the second has been an article of faith with much of the American scientific community since the first proposals for international control were advanced; the first is in the process of becoming part of the conventional wisdom of historical writing on the Cold War. Both warrant the most serious examination and question.

Let us begin this examination by accepting that part of the process of scientific development consists in the validation of hypotheses by examination of the processes by which the hypotheses are tested. Work in the physical sciences is unique in that experiments designed to validate hypotheses can be repeated, and if the experiments are properly controlled the same results should be obtained. If the results are not repeated, then the processes of experimentation will be examined to see if they fulfill the requirements of scientific method; if they stand up to such examination, then the hypotheses will be discarded or amended if possible to comprehend the variations in result. The point I am making is that in the physical sciences hypotheses undergo a double process of validation, by the "scientific" character of the methods employed to validate the hypothesis and by the ability to repeat the process of validation.

It is a truism that historical hypothesization cannot be tested by repetitive experiments; but this does not mean that hypotheses advanced as a result of historical research cannot be subjected to the process of validation by examination of the methods of research and argumentation employed by the historian. What is depressing to a historian, especially any historian of international relations who encounters intrusions into his field by physical scientists, is their immediate abandonment of scientific method once they abandon the sure ground of their own expertise. Historians who express such feelings are often denounced as medieval guildsmen attempting to restrict entry into their guild by insisting on prolonged periods of apprenticeship and initiation ceremonies into the arcana of their profession. No doubt there are such historians. But the sense of depression expressed above does not arise from a fear that the arcana of the profession

may be seen through but from the spectacle of men of proven intellectual and creative ability, men whom one had admired and revered, revealing themselves, once they step outside their own fields of expertise, as ignorant, insensitive fools. Strong language perhaps, but let me justify it.

Any European historian of international relations has enough reason to be depressed when he contemplates what passes for historical method among his American colleagues. To refer for a moment to Sherwin's first proposition: he admits there is virtually no evidence on which we can assess the motives of Soviet policy-makers. Yet his first proposition rests essentially on an interpretation not of American policy (for which American evidence is available), but of Soviet perceptions of American policy, Soviet knowledge of American developments, the Soviet order of priorities in postwar foreign policy, and the nature or indeed the existence of any serious long-term Soviet thinking in the period 1941–45 about what was the most desirable type of international order after the defeat of Nazi Germany, matters concerning which there is little or no evidence of Soviet provenance. It is surely axiomatic that American evidence is evidence only of what American observers of Soviet policy thought about that policy. Sherwin's first proposition is untenable—not because it is wrong, though it is extremely difficult to reconcile with those examples of Soviet policy-making for which we do have some evidence of Soviet provenance—but because a proposition that cannot be validated by the methods of validation acceptable to historians is a nonproposition; though its tenure by historians is itself of historical interest, as is the cosmogony of Moses, St. Augustine, or the *Morte d'Arthur*. It might be possible to advance a series of propositions about the Soviet views of these issues by a careful comparison of what the Soviet Union's representatives said to British, French, and American diplomatists with their internal political directives and their behavior on the ground. A. J. Sharp of Dundee University has recently produced a most valuable study of wartime Soviet policy on the postwar control of Germany by the careful use of such comparative methods. But since the issue of nuclear control never became a subject of discussion between the members of the Grand Alliance this approach is barred. The restrictions placed on Peter Kapitsa on his return to Russia from the Cavendish laboratory in 1934 do suggest that Soviet views of the issue were then governed by the same restrictive and monopolistic nationalism as that produced by General Groves; but it is only a suggestion. In this section of his book Sherwin is abandoning the practice of history for that of cosmic mysticism.

The second proposition, put simply that Bohr was right and Churchill and Roosevelt wrong, is equally open to historical objection. The concept of "international control" in the minds of Bohr and others was essentially a cop-out, a flight into the higher mysticism away from the unpleasant and unacceptable world of politics. In Bohr's mind, the idea of science as the true basis of international confraternity was a survival of 19th-century European internationalism, that movement of upper-middle-class professionalism which in the decade before 1914 produced the great international scientific congresses and was perpetuated among nuclear physicists in the 1920's by the illusory international order established at Versailles. When Hitler and Mussolini (and Stalin) began imposing restrictions on the movement and practice of nuclear physics, the transnational society of nuclear physicists withered and died, and its members' loyalties polarized onto the surviving "free" societies. Internationalism of the pre-1914 cosmopolitan kind lost its basis with the destruction of transnational society in the 1930's.

When Bohr spoke of international control he was assuming that a basis for such international cooperation existed. He was also, though the humanity and nobility of his desire to avert the evils of a third, nuclear, world war blinded him to this, arguing for a definition of political responsibility and government which approached the authoritarian all too closely. Both Roosevelt with his studied vagueness and Churchill with his choleric reaction to Bohr's proposals understood this well. Roosevelt had been president long enough (and the presidency of the United States is the last surviving embodiment of the ethos of 18th-century benevolent despotism) possibly to have been tempted by the idea. Churchill, who, for all his faults, was devoted to the British view of democracy, to parliament and to respect for the views of his political colleagues, was not. Both men had spent large parts of their political careers gaining firsthand experience of the manner and nature of relations between sovereign states, deriving their sovereignty from the electorates of these states. Their first concern had to be for the interests of those from whom their sovereignty was derived. In the circumstances of 1942–43 to talk of international cooperation and international order outside the specific areas of wartime cooperation was to talk of proposals for the future creation of something at that time nonexistent. Moreover, the concept of "international control" as employed by Bohr in conjunction with his concept of a transnational confraternity of scientists was difficult to distinguish from that of "supranational control," an intellectual construct for the realization of which the world of actual political experience provided neither the preconditions nor anything else. Stripped of its belief in the transnational loyalties of scientists and its confusion between the ideally conceivable and the politically realizable, Bohr's proposal implied at least the transfer of control over the new weapon to a government from whose past behavior and present attitudes it was only too easy to infer the probability of future antagonism. Churchill's belief that Bohr was potentially a forerunner of Nunn May, Fuchs, or Pontecorvo is of course clear evidence that he did not, in any way, understand Bohr's system of values or personal integrity. Bohr's failure to see that the "international control" he was proposing depended at best on the continuing stability of that historically most unstable of institutions, a triumvirate, and lacked any realizable institutional base shows that he was as ignorant of politics as Churchill of nuclear physics but that, unlike Churchill, he was unaware of his own ignorance.

What is clear is that for Bohr as for his American colleagues the idea of international control was a flight into irrationality occasioned by their understanding of the terrible possibilities resulting from their work. It is of considerable historical importance as marking the penultimate stage of a certain set of attitudes to the external political world which originated with the rise of the higher educated professional classes within a European society now defunct. Our age is developing its own varieties of transnationalism, including that of professional bodies, but in the context not of a single dominant society and culture but of several mutually, if partially, antagonistic transnational cultures. Methods of international control are also being devised. They develop with painful slowness, they are far from universal in coverage, and they are inevitably outstripped by the technology they are designed to control. But then one cannot uneat Eve's apple or put the genie back in the bottle.

DONALD CAMERON WATT
London School of Economics and Political Science, London, England