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The 1911 Revolution has long been eclipsed by the far greater revolutionary struggles which followed it. And justifiably so. It was not much of a revolution. Some have even questioned whether it deserves to be called a "revolution" at all (Rankin, 1971: preface). But it did end several millennia of imperial rule. The monarchy was replaced by a republic—even if that republic was not all that its Chinese and Western advocates would have liked it to be. The Confucian basis for imperial legitimacy was destroyed forever—even if no recognizable consensus emerged on what should replace it. Taking the term 1911 Revolution in its usual broad sense to refer to the entire period from 1900 (or even 1895) to 1913, the significance of the era becomes even more manifest. The abolition of the examinations destroyed the traditional criterion for gentry status. The establishment of Chambers of Commerce allowed and even encouraged merchant participation in the political process. The constitutional reforms institutionalized the power of local elites to an unprecedented degree. The New Army gave military men a "modern" prestige which rivaled that of their civilian counterparts. And impelling all of these changes forward was a pervading nationalism fueled by an intense fear that imperialism would carve up China or even exterminate the Chinese race.

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An appreciation of the importance of these changes has made the 1911 Revolution a recently popular topic in the historians' search for the origins of modern China. The books and articles under review are largely the product of a flurry of activity in the 1960s, but they are only the beginning. Others will soon appear and add significantly to our understanding. The field, in fact, has now developed to the extent of producing studies whose sources and subjects overlap. Though the modern China field has long grown on the contrary principle that dissertations and books are written only on previously untouched subjects, the advantages of the present state of 1911 studies should be evident. We begin to see that authors with different viewpoints and approaches project radically different images of the same event. And I, at least, emerge with the conviction that, as much ground as has already been covered, there are few topics which could not be done again.

To say that much remains to be done and done again is not to disparage the advances that have already been made. In fact, the fundamental organizing principle of this lengthy review is an attempt to trace the development of 1911 studies away from an older orthodoxy and in the direction of increasingly sophisticated analyses of sociopolitical change in this period. However, increasing sophistication brings enhanced complexity. As old problems are solved or discarded, new ones arise in their place. My focus in this review is on the new problems raised by each successive contribution. I am as much interested in what remains to be done as in what has already been accomplished. Thus while the framework of this essay is developmental, the content is largely critical. I hope that in this way I can combine the functions of a review article and a summary of the state of the field.

THE SUN-CENTERED ORTHODOXY

One need only glance through Winston Hsieh's useful bibliographic survey of *Chinese Historiography on the Revolu-*

tion of 1911 (1975) to appreciate the interpretive advances that have been made. Hsieh describes the manner in which the "orthodox school" of historiography grew from a unique set of historical circumstances faced by Sun Yat-sen at the nadir of his political career: exiled from China following the failure of the "Second Revolution" of 1913 and abandoned by many of his former revolutionary colleagues, Sun required a myth upon which to base his claims to political legitimacy. That was to be the myth of Sun's contribution to the founding of the Republic. In Hsieh's words, "the challenge to his [Sun's] leadership from his own ranks . . . spurred Sun's group to claim his undisputed leadership and seniority in the whole revolutionary movement" (Hsieh, 1975: 18).

Sun's writings at this time laid the foundation for an orthodoxy which dominated pre-1949 Chinese historiography, and strongly influenced both Western and recent Chinese accounts. At the heart of the historiography was an elaborate conspiracy theory, starring Sun Yat-sen, in which his hearty band of radical emigrés in the Tongmenghui single-handedly brought down the alien Qing dynasty of the Manchus and ushered in the era of republican government. Hsieh's description of the growth and elaboration of this orthodoxy is excellent. His argument that post-1949 Chinese historiography of the Revolution should be regarded as a "neo-orthodoxy" is also well-founded. As he notes, all the major documentary collections published in the People's Republic of China are organized according to the chronology of the revolutionary organizations which Sun joined.

But Hsieh's lack of sympathy for the new Marxist content of this historiography is evident in his references (p. 59) to "the dogmatic application of the Marxist-Leninist doctrines" and "the gap between doctrine and reality" which resulted therefrom. He is a little old-fashioned in maintaining a good liberal belief in "historical objectivity" (pp. 4, 33) and "the historian's duty to search out and to present the truth" (p. 1). It is presumably these liberal convictions which make it difficult for him to acknowledge what his own summary (pp. 41-63) of

historiography in China shows: Marxist class analysis and Leninist concern for the problems of imperialism have raised some of the most significant new issues in studies of the 1911 period. In particular, significant attention has been devoted to the class character and political role of such crucial groups as the secret societies, the New Army, the revolutionary parties, and the constitutionalists.

To me, these are serious shortcomings from the standpoint of historiographic theory. But the book is intended to be a useful bibliographic introduction to the field, and it is that. Anyone about to embark on major research on the field should start here. Only two caveats are in order: regrettably, Hsieh's Wade-Giles romanizations are not to be trusted. Errors are all too frequent. Secondly, the exclusion of purely primary sources—while unquestionably necessary to keep the bibliography manageable—means that the newspaper and archival sources which have been so essential to the advances made in the other studies under review are not mentioned. Scholars wishing to go beyond the orthodox historiography which Hsieh so deftly exposes will have to go beyond the orthodox sources which make up his bibliography.

The need to go beyond the orthodoxy is easily seen as we move to other studies on 1911. Six of the eight monographs under review focus on a member or members of the revolutionary parties in the treaty ports and overseas. Later in this review, when I move to a consideration of the Qing reforms, constitutionalism, the bourgeoisie, popular discontent and what I consider the significant sociopolitical issues of the period, I will be talking largely about articles. Most of the monographs have yet to appear.

Of the present group of monographs, the closest to the orthodox tradition is Harold Z. Schiffrin's *Sun Yat-sen and the Origins of the Chinese Revolution* (1968). By seeking the origins of the Chinese revolution in the person of Sun Yat-sen, Schiffrin is necessarily accepting the central tenet of the old orthodoxy. Yet this is a book which cannot easily be dismissed. In the narrow sense of the term, Schiffrin's scholarship is

excellent: his book is well-researched, analytical, literate, significant. Most importantly, while extremely careful in stating his conclusions, Schiffrin does not hesitate to tackle the hard issues. An excellent example is his treatment of an embarrassing aspect of Sun's revolutionary style—his proclivity for alliances with the most adventurous of Western and Japanese imperialists in the effort to carry out his revolutionary schemes.

Marius Jansen first detailed Sun's extensive contacts with Japanese expansionists both inside and outside the government in his book *The Japanese and Sun Yat-sen* (1954), published over 20 years ago. Albert Altman and H. Z. Schiffrin himself have recently added to our understanding of these contacts during World War I, when Sun refused to oppose the Twenty-one Demands the Japanese had made on Yuan Shi-kai and instead accepted a total of 1.4 million yen to organize one of several Japanese-sponsored military efforts to overthrow Yuan (Altman and Schiffrin, 1972). Josef Fass (1967) has called our attention to the $2 million Sun extracted from the Germans in 1917, to help set up his Canton government in opposition to Duan Qi-rui in Beijing [Peking]. Finally, back on the pre-revolutionary period, an article by J. Kim Munholland (1972) and a more extensive dissertation by Jeffery Barlow (1973) have explored Sun's reliance on French expansionists in both Paris and Vietnam, especially in the 1906-1907 period.

These last incidents are all outside the time frame of Schiffrin's book (which ends with the founding of the Tongmenghui in 1905), but Schiffrin does cover extensively Sun's involvement with an aggressive group of British treaty port journalists in 1895. Here he does not hesitate to ask whether Sun and his group were "Nationalists or 'Running Dogs of Imperialism' " (Schiffrin, 1968: 77). Though he understandably (if regrettably) fails to answer explicitly in favor of one of these alternatives, he clearly leans toward the former view by reconstructing the "nationalist rationale for the Hsing Chung Hui's [Xingzhonghui's] pro-Western orientation" (p. 79). Denying that Sun and his group were "willing to sacrifice Chinese sovereignty merely to satisfy their political ambitions," Schiffrin (1968. 79) argues that

> their other distinguishing trait was that they were intellectual and spiritual products of the Western, Christian tradition. They were Chinese who critically appraised their country's institutions through eyes which had already been accustomed to Western criteria of evaluation. They sincerely felt that it was necessary to impose upon China European standards of administration and justice with the help of foreign advisors, at least in the beginning stages.

This seems a lucid and convincing rationale until we get to the point where Sun's foreign allies are Japanese instead of British. Then Schiffrin argues that in contrast to Sun's "intellectual commitment" to Western values, there was an "emotional" attachment to Japan: "Japan would always receive his special consideration and tolerance, for despite her active participation in the imperialist attack upon China, Japan was for Sun a 'natural ally,' sharing a common East Asian cultural heritage and a common victimization by the West" (p. 146). But if Sun was a "spiritual product of the Western, Christian tradition," one wonders what appeal he found in the "common East Asian cultural heritage" that China shared with Japan.

Political biography is an extremely difficult genre of history. To maintain interest, enthusiasm and empathy for his or her subject, the historian is constantly tempted to let explanation become apology, and to allow the subject who is necessarily central to the book to appear to be central to events. Schiffrin has perhaps surrendered too much to these temptations, and to the orthodox view of Sun's role in the Revolution. For example, the orthodoxy requires that Sun be regarded as the founder and leader of the Xingzhonghui, which must, in turn, be seen as a party advocating republican revolution. Schiffrin endorses the view that in 1895, Sun, "having already created the organization in Hawaii . . . was accepted as its leader in Hong Kong as well" (p. 51). He also offers a number of arguments for the notion that the Xingzhonghui was republican, even in its Hawaiian phase, even if there are no documents to prove the case (p. 43). But he later is forced to admit that Yang Qu-yun[1] had "control of the movement's finances" and consequently was elected president of the society (pp. 68-70),

and that "in matters of policy and program" both Sun and Yang deferred to He Qi (p. 70). Seven pages later we learn that in this deference to He Qi, the republican goal was deferred (p. 77). Further doubt as to Sun's commitment to republicanism is cast when we learn (slightly mysteriously) that: "There is evidence too that Yang, rather than Sun, was more insistent upon republicanism at this time" (p. 69), and that Sun was simultaneously conspiring with the avowedly monarchist *jinshi* Liu Xue-xun in Canton (pp. 64-65). Schiffrin undeniably does a great service in openly presenting all of these contradictions in Sun's plots and programs. But it is a little bit disconcerting to have them explained always in terms of his "flexibility" (p. 77) or "the stong pragmatic tendency in Sun's personality" (p. 65). Where is the line between *personal* pragmatism and *political* opportunism?

Schiffrin's book's greatest flaw lies in its structure and argument. It is, essentially, a book on the founding of the Tongmenghui and it exaggerates both Sun's role in that founding and the historical significance of the event. "By mid-1905," Schiffrin writes "he [Sun] had created a new revolutionary combination out of the disparate elements which no longer accepted Manchu rule under any conditions" (p. 347). This assertion of Sun's single-handed creation of the Tongmenghui is repeated elsewhere (p. 8). In addition, the power and unity of the Tongmenghui are exaggerated. For Schiffrin it was "an important innovation . . . the prototype of a modern political party . . . [T]he new organization commanded multi-provincial, national support. It was also multi-class" (pp. 8-9). "For the first time in his life, Sun commanded the manpower and talent for an organized, national political effort" (p. 362). Unfortunately, this perception of the Tongmenghui as a unified, national political organization is largely an illusion, and it is uncharacteristic of Schiffrin that he should accept it in such an unqualified manner.

THE PERILS OF REVOLUTIONARY BIOGRAPHY

If we seek to get away from the orthodox Sun-centered view of the Revolution, there are two immediate biographical alternatives: one older and poorer work on Huang Xing and one more recent and much better book on Song Jiao-ren. In 1961, Huang Xing's son-in-law, Chün-tu Hsüeh, published his *Huang Hsing [Huang Xing] and the Chinese Revolution*. It was a frankly filial act and Hsüeh openly sought to elevate Huang to equality with Sun Yat-sen as "co-founder of the Republic of China" (1961: vii). But the argument is partisan and quite unconvincing. Every failing of Huang is swept under the rug, most notably, perhaps, Hsüeh's denial, in the face of a dozen eyewitness accounts to the contrary, that in November 1911, Huang advocated the abandonment of Wuchang after the Qing armies had recaptured Hanyang, across the river (pp. 118-119). A much greater fault of the book is its adherence to the letter of the orthodox school's notion that the Revolution resulted from a string of "ten unsuccessful revolutionary attempts" led by the Xingzhonghui and the Tongmenghui. All that Hsüeh attempts to do is assert that it was Huang and not Sun who led most of them (p. 56). The myth of Tongmenghui unity and leadership of the revolutionary movement is totally accepted.

The considerable contribution made by K. S. Liew's *Struggle for Democracy* (1971) is his utter destruction of that myth of Tongmenghui unity. His chapter VI (pp. 68-84) should be read by anyone still tempted to believe that the Tongmenghui was, except perhaps very briefly in 1905-1906, what Schiffrin has called "an organized, national political effort" "commanded" by Sun Yat-sen. In Liew's words, the Tongmenghui was "more a federal union of the provinces rather than a unitary organization composed of individuals. . . . The first loyalty of party members was to their respective provincial leaders rather than to the central authority" (p. 68). Consequently, centrifugal tendencies quickly tore the organization apart. When Sun and Huang Xing quarrelled in February 1907 over the design for the

national flag, Song Jiao-ren reacted to Sun's leadership as "almost dictatorial and intransigent to an unbearable degree" (p. 71). Soon thereafter, when Sun failed to report 8,000 of 10,000 yen that the Japanese gave Sun on his expulsion from Tokyo, Zhang Bing-lin (then editor of the Tongmenghui party journal, the *Min Bao*) led an effort to expell Sun from the Tongmenghui (pp. 71-73). Later in 1907, another "separatist movement" emerged in the Mutual Advancement Society (Gongjinhui, an organization which I translate Forward Together Society) composed mainly of revolutionaries from the Yangzi [Yangtze] Valley. This was one of the key groups which initiated the ultimately successful Wuchang Uprising of October 1911, as well as the movement in Hunan–the first province to join the Revolution. The Mutual Advancement Society was, as Liew puts it, "for all intents and purposes . . . a rival to the Chinese League [Tongmenghui]" (p. 76). With these challenges to his leadership, Sun, by 1910, simply abandoned the Tongmenghui and began enrolling new members in San Francisco and South East Asia in a new Chinese Revolutionary Party (pp. 80-82). During a brief stop-over in Tokyo in mid-1910, Sun was challenged by Song Jiao-ren and others to explain these actions. His response, according to a participant in the discussions, was

> "The League has long since been dissolved. Those who felt strong enough to do so could organize independent establishments of their own." When Sung [Song] asked him to explain those remarks, he [Sun] said, "Party members attacked the *Tsung-li* [*Zong-li*, the leader, i.e. Sun], how could there be a League without me? As all funds are raised by me, party members have no right to question me about them, still less to make them the object of attack" [p. 80]

Largely in reaction to this imperious attitude of Sun, Song Jiao-ren and others in Tokyo conceived in 1910 and created in 1911 a Central China Office of the Tongmenghui. The Central China Office was the sole link of the Tongmenghui to the revolutionaries who ultimately launched the successful Wuchang Uprising. Thus orthodox historiography has been at pains to

portray it as a regular part of the Tongmenghui organization. In fact, as Liew shows, it was a fractional group, whose declaration made no mention of Sun Yat-sen as Zongli but instead endorsed an alternative principle of collective leadership (pp. 96-98). Financially and operationally, it was totally separate from the rest of the Tongmenghui.

Liew, then, does more than anyone before him to expel Sun and the Tongmenghui from their usurped position at the center of the revolutionary stage. Unfortunately, however, he also commits the classic biographer's sin of placing Song Jiao-ren at the center of history, and viewing his Central China Office as "the pivot of the Wuchang uprising" (p. 100). The attempt fails. In his presentation of "more conclusive evidence of the office's authority" over the revolutionaries in Central China, he offers a Central China Office resolution of October 7, 1911: "it directed that the activities of the revolutionaries should be slowed down, and the plan for the uprising should be suspended until further developments" (p. 101). What Liew fails to stress is that the resolution was simply ignored, and the Revolution broke out four days later. Song Jiao-ren's Central China Office had no more authority over the revolutionary groups acting within China than did Sun Yat-sen and the Tongmenghui.

Despite all their shortcomings, the three biographies by Schiffrin, Hsüeh and Liew do give us a fair range of viewpoints from which to analyze the revolutionary parties overseas. No one of these viewpoints is sufficient in itself, a point easily brought home by reading contrasting accounts of the same event. In, for example, the crucial instance of the founding of the Tongmenghui Schiffrin naturally traces it to Sun's recruitment of students in Europe, and throughout Sun is unquestionably the leading character (1968. 344-366). Hsüeh, however, stresses Sun's eagerness to seek out Huang Xing, notes that Sun's desire for the term "revolutionary" *(geming)* in the name of the Chinese (Revolutionary) Alliance was rejected, and emphasizes the domination of Huang's group in the leadership of the new organization (1961: 40-45). Liew's account stresses the tendencies toward unity among the revolutionary students

before Sun's arrival in Tokyo in 1905, Sun's initial isolation from the students, and his hesitancy to answer directly when a student asked whether democracy or monarchy was the goal of the revolution (1971: 40-48). If one had to read (or assign to be read) only one account of these events, there is no question that Schiffrin should be chosen. On the other hand, Hsüeh and especially Liew offer important corrective views to help balance the account. And no one need fear that any of these books has given us the final word on the subject.

IN SEARCH OF RADICALISM

With Michael Gasster's *Chinese Intellectuals and the Revolution of 1911* (1969), we move from political biography to political-intellectual history, but we still do not escape the domination of the Tongmenghui. The principal focus of the book is on such men as Wang Jing-wei, Hu Han-min and Zhang Bing-lin—all Tongmenghui spokesmen. The subtitle of Gasster's book is "The Birth of Modern Chinese Radicalism" and his conclusion is that: "In twentieth-century China 'radicalism was trumps'" (p. 248). More specifically, Gasster argues that: "The radicalism which has dominated Chinese thought and politics in the twentieth century first saw the light of day and won its first victory between 1903 and 1908" (p. ix). His description of the radicals' "victory" in those years focuses on the development and elaboration of nationalism, republicanism, anti-Manchu racism, anarchism, and finally Zhang Bing-lin's unique blend of classical racism, Buddhist egalitarianism, and philosophical anarchism. This is all done in very competent fashion, and if one wishes a summary of views expressed in the Tongmenghui's Min Bao, Gasster is a good place to start.

The flaw in Gasster's analysis, I believe, lies in the assumption that what was published by revolutionaries in Tokyo should be taken as an indication of political trends in China. In particular, the notion that radicalism had scored some notable victory in

1908 strikes me as absurd. As we have seen, the Tongmenghui, organizationally, was in utter disarray by that time, and within China the ranks of those who were willing to give constitutional monarchy a serious try increased daily. There was unquestionably a growth of nationalism (of various sorts), and a trend toward constitutional government (whether monarchial or republican in form) in this period. For Gasster, this is probably evidence of a growth of "radicalism," for he is willing to include Liang Qi-chao's writings within that category because of Liang's commitment to "goals of modernization" and his "desire to modernize thoroughly" (p. 236). But this, it seems to me, stretches the definition of the term to intolerable lengths. With Liang a "radical" on one side because he wishes to modernize China, and Zhang Bing-lin and Liu Shi-pei "radicals" on the other, despite (or because of?) their skepticism of the whole process of modernization, one begins to doubt the analytical usefulness of the term "radical."

In addition, what intellectual historians will always have to demonstrate to skeptics like myself is that their ratiocinations are somehow useful in explaining the behavior of their subjects. What "radicals" write in Tokyo is of little interest to me unless it helps me to understand what they *did* in China. In this case, we have a number of intellectuals who wrote in undeniably radical fashion while in exile in Japan. But their actions upon their return to China indicated that they were willing to compromise many of these radical principles. Liu Shi-pei, who served the Qing and Manchu official Duan-fang from 1908-1911 is a prime example. Even Zhang Bing-lin in 1912 joined with several reformers and former Qing officials to form a party in opposition to the Guomindang [Kuomintang]. Wang Jing-wei, after his release from jail in Beijing in 1911, seemed able to converse amiably with Yuan Shi-kai, seeking a definition of republicanism acceptable to that veteran bureaucrat. In fact, by 1911 there seems no separate camp of "radicals" at all, as all join the political struggles on more or less equal terms. By 1912, Sun Yat-sen and most of his colleagues are appealing for national unity behind Yuan Shi-kai. Thus one begins to wonder:

is radicalism "trumps" in political action? Or is it merely "trumps" in the publications of radical intellectuals?

Before such doubts go too far, one should pick up Mary Backus Rankin's *Early Chinese Revolutionaries: Radical Intellectuals in Shanghai and Chekiang [Zhejiang], 1902-1911* (1971). Here at last is a study of revolutionaries which does not overstate their role, and of radicals who all seem to deserve the label. Rankin begins with a critical distinction between the "revolutionary movement and the Revolution of 1911," and acknowledges that not revolutionaries but "constitutionalists, army officers and others had the power to dominate the Revolution" (1971: preface, 2). She recognizes the decline of radicalism within China after 1907:

> The romantic, radical trend in student politics, therefore, was an early phase of the 1911 Revolution. It never died out, but it declined in significance after 1907, and the 1911 Revolution was not the result of the radicals' plotting. [p. 6]

Even more importantly, she suggests some fundamental answers to the revolutionaries' failure in the victory of the Revolution. In the Su Bao case of 1903, when Zhang Bing-lin and Zou Rong provoked and then awaited arrests for sedition—arrests which they could easily have escaped—the revolutionaries "gained publicity for their cause, but at the cost of their institutional base" (p. 78). Similarly in 1907, the commitment to "heroic sacrifice"—very consciously in the tradition of the knight-errant (*youxia*)—of "revolutionary romantics" like Xu Xi-lin and the great female revolutionary Qiu Jin won widespread public support for the anti-Manchu cause, but necessarily destroyed the revolutionaries' own organization. They were, in this sense, "the exact opposite of the Leninist type of revolutionary" (p. 176; see also pp. 187-190). The very tactics by which they sought to hasten the coming of the revolution guaranteed that they would be organizationally incapable of controlling the revolution when it came.

These classically educated upper-class revolutionaries were a different breed from the Westernized Christian Sun Yat-sen. Nor did they adhere to the cautious, patient, methodical approach to revolution of a man like Song Jiao-ren, who often opposed his colleagues' tendencies toward dramatic sacrifice (Liew, 1971: 49, 104). But we have, in the Zhejiang group, individuals who were probably culturally and politically much closer to other revolutionaries active within China than was either Sun or Song. If this is the case, then several themes of Rankin's narrative are worth examining as possibly representative of China as a whole.

First, the conventional historiography makes a sharp distinction between revolutionaries and reformers. This view probably grows from the sharp verbal battles that the revolutionaries fought with men like Liang Qi-chao in Japan. K. S. Liew has even gone so far as to posit a "permanent and unbridgeable gulf" between revolutionaries and nonrevolutionaries (1971: 159). Rankin demonstrates that within China no such division ever existed. Officials conspired to protect known revolutionaries (1971: 86-87), powerful local gentry sponsored the revolutionaries' schools (pp. 157-160), and revolutionaries passed bribes and approached relatives in the bureaucracy to gain entrance to military schools in Japan (pp. 169-170). In Rankin's words, the revolutionaries were able to establish their front organizations by "simultaneously taking advantage of opportunities afforded by the modernization movement and of the relative degree of freedom traditionally enjoyed by members of the local elite" (p. 157).

This stress on the elite status of the revolutionaries allows Rankin to examine another critical question: their relations with the peasant masses. This question is not only important in itself, but it is important because many scholars have attempted to link the revolutionaries of this era with later communist revolutionaries whose movement was predicated on the support of the peasantry. Rankin asserts that: "The student revolutionaries of 1911 and the peasants lived in different worlds and

neither was entirely in sympathy with the other" (p. 156). While the students naturally approved of the modern schools and self-government institutions, the peasantry often attacked them as the cause of an unbearable increase in taxation. In this and other respects, the radical intellectuals could not "bridge the social gap and identify themselves closely with the attitudes and problems of the peasantry" (p. 157; see also p. 12). If the revolutionaries were so divorced from the peasantry, one might justifiably ask how they mobilized the secret societies of Zhejiang. Rankin's answer is simple and direct. In Zhejiang, many of the secret societies were led by members of the gentry, including a "transitional group of scholars . . . [who] had already been exposed to 'modern,' Western views." The revolutionaries "were able to approach the educated society leaders, particularly the transitional segment, from the basis of fairly similar backgrounds and certain shared attitudes. There was no distinct barrier between secret societies and intellectual circles" (p. 139). Thus, the elite status and elitist attitudes of the radicals did not in any way impair their ability to mobilize the secret societies. On the contrary, it facilitated the process.

If I were to find fault with this book, I would have to confine my comments almost exclusively to the preface and conclusion. In the former, Rankin states her commitment to modernization theory with this first "assumption" of the book: "that the basic aim of the 1911 Revolution was modernization" (preface, p. 2). That I neither like nor understand the concept of modernization is my problem. But that Rankin fails to define it is her problem. Then, in her conclusion, Rankin identifies her radical intellectuals with this modernization: "The ultimate demand of the 1911 revolutionaries was for modernization. . . . They had irretrievably abandoned tradition as a whole" (p. 228). This, I believe, is nonsense and is contradicted both by her earlier sensitive portraits of the revolutionaries and by her discussion two pages later. As "revolutionary romantics," many of the radicals' actions and beliefs were informed by the continuing influence of their traditional education and upbringing. At the

other end of the social spectrum, we have Rankin's questionable reference to the "conservatism, superstition, and ignorance of the peasantry," which prevented any effective link with revolutionaries until, à la Chalmers Johnson, "the Japanese invasion awakened peasant nationalism" (p. 231).

Clearly, Rankin's conception of "modern" revolutionaries and "conservative" peasants is analytically linked to her recognition of the social gap separating radical intellectuals from the masses. But here she has overdone it. The problem is perhaps better approached by asking on what basis radicals sought to ally with peasants. Earlier she noted Tao Chengzhang's proposals of "common land ownership, light taxation, small armies and sufficient food and shelter for all," but rather cavalierly dismissed them as "merely a composite of utopian goals of traditional visionary reformers and programs generally pursued by a new dynasty anxious to pacify the countryside" (p. 153). This seems a bit too glib. It would appear more reasonable to recognize that insofar as radicals of this era attempted to and succeeded in allying with the peasantry, they did so on the basis of rather traditional formulae which answered long-standing peasant demands. Conversely, insofar as radicals were "modern," they cut themselves off from the peasantry.

REVOLUTION AS RELIGIOUS RESTORATION

A theme similar to this informs Edward Friedman's *Backward Toward Revolution: The Chinese Revolutionary Party* (1974). Friedman accepts neither Rankin's notion that radicals were hopelessly divorced from the peasantry, nor her suggestion that the conservatism of the peasantry made them unrevolutionary. On the contrary, the conservatism and superstition of the peasantry could become the very fount of revolution. In Friedman's view, revolution, to the peasants, was a "restoration" (1974: 120), a "return to the mythical starting

point," to "renewed community" (p. 121). Nor did the peasants' peculiarly "backward" notion of revolution preclude an alliance with the group of revolutionaries around Sun Yat-sen. "China's radical intellectuals could help China's rural dwellers remake themselves. . . . Revolution would require a union of a new community of radicalized intellectuals of ultimate concern with the actual and felt religious needs of rural dwellers, an explosive mass joining energy and vision" (p. 87). A significant point, to which we shall return presently, is Friedman's stress on the necessity for radicals to unite with the peasantry on the basis of their *religious* needs. This is in contrast to Rankin's approach to the problem through Tao Cheng-zhang's *social* remedies of common land ownership, light taxation, and so on. In any case, Friedman finds in the Chinese Revolutionary Party such a group of "radicalized intellectuals of ultimate concern" who were willing to move "backward toward revolution."

Friedman's theories, I believe, should be taken with the utmost seriousness. His book is outstanding, in our field, in its attempt to combine primary research with the latest in Western social science theory. I am not convinced that his primary materials can bear the weight of his contemporary theory, and his conclusions often seem outrageous. Nonetheless, his analysis is never dull, and (though this comparison will discomfort some) I often find Friedman a lot like the late Joseph Levenson: suggestive, even if he proves to be wrong. What is necessary here is to see what Friedman is arguing about (and even more, what he is arguing against), and then to see whether the evidence will bear him out.

The Chinese Revolutionary Party (CRP), which Sun Yat-sen formed after the failure of the Second Revolution of 1913, has conventionally been regarded as a step backward from the liberal parliamentarianism of the Guomindang, which Song Jiao-ren had formed from the Tongmenghui and several smaller parties in 1912. The CRP's return to secret society formulae and the oath of personal loyalty to Sun himself have been noted as particularly regressive. Friedman begins by arguing, as does

K. S. Liew's biography of Song Jiao-ren, that parliamentary democracy was unworkable in China at this time, that revolution required a "high degree of discipline" (Liew, 1971: 198). Though admitting that Sun "obviously has a fixation on oaths," Friedman contends that the oath's "symbolic use may have served a rational purpose"—instilling "iron military discipline" (1974: 59-60). He has little use for "condescending academic criticism of the Sun Yat-sen group" (p. 207), and insists that: "The language and lives of self-styled new people should be treated in a serious, sympathetic manner" (p. 85)—two admonitions he might well direct at this review. In Friedman's serious, sympathetic treatment, Sun is first a revolutionary socialist (pp. 10 ff.), second an advocate of national unity within an inclusive single party (pp. 30 ff.), and finally (in the CRP), the creator of a "religious communal world of all-encompassing single commitment" (p. 84).

The assessment of Sun as a revolutionary socialist in this period seems both sound and useful. It rescues us from the notion that Sun's socialism was simply the product of his alliance in the 1920s with the Soviet Union. But we should remember that Sun was many things to many people. The new sources which Friedman has uncovered are themselves largely socialist or they are newspaper accounts of speeches before socialist gatherings. It is possible that Sun's socialism was largely for the benefit of Chinese socialists, and meant for the eyes of his beholders in the Second International.

Friedman's image of national consensus on "the fearful need for unity against Western imperialism" (p. 29) in the years immediately following the 1911 Revolution is also an important discovery. It certainly disposes of Liew's mistaken notion of a "permanent and unbridgeable gulf" between revolutionaries and nonrevolutionaries in this period (1971: 159). Friedman notes Huang Xing's efforts to enroll Yuan Shi-kai's entire cabinet in the Guomintang (p. 36), and the eight-point program signed by Huang, Sun, Yuan and Li Yuan-hong in September 1912 according to which Sun "agreed to abandon politics" (p. 35). But he does not, I believe, adequately explain the contradiction

between this act and Sun's "bid for leadership of a politically active Socialist party" one month later (p. 25). He seems on particularly shaky ground when he argues that "Western-style political liberals" like Song Jiao-ren, organizing a party for electoral competition, "increasingly seemed more like foreigners, traitors to the national cause" (p. 34). From the standpoint of political centralizers like Sun or Huang or Yuan this may have been the case, but in the provinces where the elections were held, Song's party won an overwhelming victory. I do not believe that seeming traitors could have accomplished that feat.

But it is Friedman's interpretation of the CRP and rural revolution in religious terms that is the most notable and the most troublesome part of his argument. Once again, it is important to see what Friedman is arguing against in order to understand what he is arguing for. While the first part of the book derives from a critique of the liberal defenders of parliamentarianism, the object of attack in the second half of the book is revealed in this perceptive footnote:

> Chinese anthropologists, archeologists, philosophers, etc. anxious to prove China's modernity to their significant others, their allegedly more advanced Western counterparts, have interpreted Chinese society so as to explain away its religious basis. [p. 84n][2]

Later he admits that he finds it

> difficult to believe the singularly secular accounts of village calculus found even in outstanding works such as William Hinton's *Fanshen* and Jan Myrdal's *China The Revolution Continued.* I suspect that people in the countryside who had been acting for centuries on millenial, religious, and magical notions did not suddenly undergo a transvaluation of values. [pp. 130-131]

The implications of this expression of faith are important Friedman obviously sees his religious conception of the Chinese revolution applying even to the revolution's communist phase (see also pp. 219-224). This raises certain theoretical problems.

One need only glance at Mao's report on the peasant movement in Hunan or the much-praised writings of Lu Xun to appreciate the doubts that many Chinese revolutionaries had about the religious conceptions of the peasantry. Furthermore, as Friedman himself notes, the CRP brand of revolution could lead to fascism at least as easily as to communism:

> The Chinese Revolutionary Party with its narrow elite, its notion of mission, its willingness to use all force, and its appeals to vested elites has a huge potential for repressive rightwing or fascist dictatorship. [p. 214]

Since the single member of the CRP most frequently cited in Friedman's discussion of the organization's religious nature is the "CRP ideologue" and later leader of the Guomindang's right-wing, Dai Ji-tao, I would take that potential very seriously. The similarity between Friedman's theory of revolution and the fascist conception is too great for my comfort. By focusing on religion and psychic phenomena instead of on economics, class, or social structure, Friedman cannot analytically distinguish a progressive from a fascist revolution. It is never clear how, in his mind, the CRP revolution with all its fascist potential, differed (if at all) from the communist revolution that Hinton and Myrdal describe.

But these are objections on the level of theory and ideology. We still must ask: does his theory fit the facts? To a degree it certainly does: there was a spiritual aspect to the revolutionaries' fearlessly remaking themselves into selfless soldiers "who didn't recognize the word impossible" (p. 85). And I am willing to see some poor villagers joining a bandit brotherhood and finding in it "a more universal familial-religion" (p. 136). But let us probe Friedman's fascinating narrative of the northern Chinese bandit known as the White Wolf for some evidence, in action, language, or symbol, of these religious conceptualizations of the revolutionary process. The exercise is useful in evaluating his method. We first meet the White Wolf demonstrating "political tendencies" in his concern over Huang Xing's abandonment of Nanjing [Nanking] in 1913. In the next

year he is publishing proclamations of "political revolution" advocating the old Qing official, and then ally of the revolutionaries, Cen Chun-xuan, as President (p. 145). Then we get a North China Herald report of a rumor among the White Wolf's band that Huang Xing had married a Japanese princess. From this Friedman reasons that "ex-villagers understood . . . the alliance as making Japan one of the family" (p. 146). This in turn leads to an extended discussion of "the familial crisis in China" and "my own feeling that the villagers experience the revolution as a reknitting of the family" (p. 147), all of which seems rather heavy stuff to lay on one rumor of marriage to a Japanese princess. After an extended discussion of the symbiotic relationship which existed between the White Wolf's bands and the government's soldiers, and the White Wolf's decision to redirect his attack toward Xi-an, we have the bandit leader appealing to the people of Shenxi [Shensi] and invoking the name of Liu Bang, the peasant founder of the Han dynasty (p. 156). As natural as that invocation would seem to me anywhere in China, to Friedman it is evidence of a "popular myth" peculiar to Shenxi, and the basis for an argument that "each area would experience the revolution in its own particular existential terms" (p. 157). This in turn provokes a discussion of children's games (to which there has been no reference in his cited sources), in which, in his own words, "I quote people such as Thoreau and Emerson and merely footnote Mao Tse-tung [Mao Ze-dong] (p. 158). Next, Fujian [Fukien] missionaries, using moral dramas as the functional equivalent to Chinese storytellers, give "independent historical corroboration" of his treatment of rural revolution "in terms of a response to a familial-religious crisis" (p. 159). Finally, Friedman concludes his narrative of the White Wolf's decline, citing evidence that the White Wolf's "major message" was his "ethno-nationalist" message directed against Yuan Shi-kai as a former official of the Manchus (p. 160), quoting references to the "semi-political character" of his bands (p. 163), but still referring without any cited documentation to "the religious quality of the secret society beliefs of the band members" (p. 162).

Friedman has certainly presented a novel and provocative approach to rural revolution in China. It is a useful challenge to us all to attune our ears to religious language and religious symbolism in the Chinese revolutionary experience. Perhaps we will find something. But I submit that if we find no more evidence than Friedman has presented here, then we will have a theory fertilized only by willful imaginations.

THE GENTRY. PROGRESSIVE OR CONSERVATIVE?

Having examined six monographs, we must pause to ask what we have learned about the causes and the significance of the 1911 Revolution. The answer, I fear, is "very little." For these books are all about revolutionaries, and as Rankin has warned, the Revolution and the revolutionaries were two different things. This concentration of scholarly energies on the revolutionary camp is understandable. In the first place, the old orthodoxy that the revolutionaries made the Revolution is difficult to shake off. In the second place, even those who recognize that the revolutionaries failed in their own time find in these losers the origins of later and more successful Chinese revolutionaries. Rankin and Friedman, who calls his study a "story about losers" (1974: 3), fall into this category. And finally, I fear, these frank, open, verbal revolutionaries are both easier and more interesting to study than the complex internal dynamics of late Qing politics and society.

But those studies of events in China are on the way. In particular Ernest P. Young's study of Yuan Shi-kai and his Presidency will soon shed important light on a complex man and period. In addition there are provincial studies, still in dissertation form, by Charles Herman Hedke (Berkeley) on Sichuan [Szechwan], William R. Johnson (Washington) on Yunnan and Guizhou [Kweichow], Samuel Kupper (Michigan) on Jiangxi [Kiangsi], Keith Schoppa (Michigan) on Zhejiang and Donald Sinclair Sutton (Cambridge) on Yunnan. Hopefully, these will ultimately be revised and published as monographs.

But even before these monographs appear, we have a number of articles which significantly advance our understanding of the social dynamics of the Revolution, many of them collected in Mary C. Wright's *China in Revolution: The First Phase, 1900-1913* (1968).

It would be foolish to try to summarize and assess each of the contributions to this rich volume, and I will not make the attempt. But there are certain themes and controversies that run through this volume which help to organize our understanding of the Revolution. For example, the volume as a whole–and Chang P'eng-yüan's article on "The Constitutionalists" in particular[3] –directs our attention away from the revolutionaries abroad and toward the constitutional reformers at home. Chang is able to demonstrate the significant contribution that the gentry reformers–impatient with the Qing's gradualist plan for transition to constitutional monarchy–made to the founding of the Republic. He summarizes the important role the Provincial Assembly members had in leading their respective provinces to independence of the Qing, and the prominence of the constitutionalists in the new revolutionary governments.

Of these facts, there can be little dispute. Several interpretive difficulties emerge, however, from this recognition of the critical role of the gentry reformers in the Revolution. Were the gentry progressive and was the Revolution evolutionary? Or were the gentry conservative and the Revolution devolutionary? Was this a real revolution, the "first phase" (as Wright's title claims) of China's larger revolutionary experience? Or was it simply another instance of dynastic collapse, obscured by a Western constitutional facade which lacked redeeming social value?

Ichiko Chūzō and Mary Wright emerge as the protagonists in this debate. Ichiko argues that "the gentry who held the local self-government posts were conservatives and not at all interested in Westernization. They were only interested in constitutional reforms for their own self-preservation" (p. 302). To him, 1911 was merely a "dynastic revolution . . . no great economic and social changes can be detected between the periods before

and after the Revolution" (p. 313). Mary Wright takes the opposite point of view. She argues that: "Rarely in history has a single year marked as dramatic a watershed as did 1900 in China" (p. 1). The decade before the Revolution marked "a new era, indeed . . . a new world" (p. 2), dominated by nationalism and a dynamic effort at reform: "The gentry do not seem to have been isolated from the main currents of the time. . . . They were the leaders of the local self-government associations and dominated the provincial assemblies. . . . The records show that these organizations concentrated on the great national issues of the day: resistance to Western encroachment, domestic social reforms, constitutional government" (pp. 39-40).

In part, the debate is over motivation (and as such, almost impossible to resolve): Ichiko sees the gentry reformers motivated by an instinct for self-preservation; Wright sees them responding to a nationalist urge to strengthen China through constitutional reform. In part, the debate is over the significance of social change in this period: Ichiko is impressed by the tenacity of gentry strength right up to 1949 (pp. 308-309); Wright finds China after 1900 to be "a new world," "a new society in the making" with enlarged roles for youth, women, military men, overseas Chinese, and industrial workers (pp. 30-38). In part, the debate is over the viability of the Qing reforms: Ichiko stresses popular opposition to the elite's reforms (p. 302) and argues that: "Social order at the end of the Ch'ing [Qing] was extremely unstable" (p. 312); Wright discounts peasant resistance (p. 29) and argues that: "In the long run—if there had been a long run—China might have followed the Japanese pattern" of successful self-strengthening under a monarchy (p. 26). If China had had a "monarch with imagination" instead of the "feeble, frightened" Prince Regent, she might have been able to avoid the perils of revolution (p. 29).

The Ichiko-Wright debate is so stark and so fundamental that a number of scholars, either implicitly or explicitly, have aligned themselves with one side or the other. Probably the

most impressive support for Mary Wright's position comes from Mark Elvin, at the conclusion of his study of the Shanghai Municipal Council—China's first Western-modeled local self-government body, and apparently one of the most successful. Elvin emphasizes the adaptability of the Chinese elite and the Qing court's failure to keep pace:

> Change was necessary and inevitable; but if the late traditional elite was generally capable of the creative energy it showed in Shanghai, then the revolution, which led to the rapid breakdown of the framework for peaceful change, was a disaster. It is not unreasonable to speculate that, if the Imperial Court in 1910 and 1911 had possessed one or two politicians with either the intelligence or the flexibility to have conciliated the constitutional movement, not only might there have been no overt revolution, but a new political order, of which the municipal council studied in this paper may stand as an exemplar, might have had the time to establish foundations that could not have been so easily swept away. [Elvin, 1974: 261-262]

The wistful yearning for a gradualist alternative to revolution is too evident here to require further comment.

Further support for Wright's general position comes from one of her students, Roger Des Forges, in his study of the Mongol provincial official and reformer, *Hsi-liang [Xi-liang] and the Chinese National Revolution* (1973). I frankly find Des Forges' principal thesis absurd. He insists, throughout, on portraying Xi-liang as a "radical" whose policies sought "support for the larger national revolution" (1973: 85). He was a "patriot" and a "populist" who at one point is seen "urging the upper classes to carry out a revolution toward an egalitarian society" (p. 176). By making such claims, I fear that Des Forges has perpetrated a gross debasement of language, for Xi-liang was in fact—as the narrative shows—an uncorrupt but otherwise rather common late Qing reformer who supported the dynasty to the end.

If I had to compare him to one of his contemporaries, it would be to the Manchu normally blamed for the Wuchang uprising, Rui-zheng. Despite Rui-zheng's image as an incompetent who fled his post at the first sign of trouble, he was

a vigorous reformer, interested above all in eliminating corruption, rationalizing administration and resisting imperialism. Xi-liang was much the same, and Des Forges' description of his efforts at military reform, his resistance to Western mining schemes, his promotion of education and constitutional reforms, his suppression of opium smoking and cultivation is really a description of what most provincial officials were doing at this time. Taken in this light, this is a useful book. Above all, it demonstrates that with people like Xi-liang, the imperial bureaucracy was as committed to a program of reform as were the gentry. In addition, if one reads carefully, one sees that many of the reform projects failed to bear fruit because of inadequate financing or imperialist pressure. Thus we have evidence both of the energy committed to reform and the inadequacy of energy alone.

Against these supporters of Wright's sanguine view of the Qing reforms, several scholars have allied themselves with Ichiko's position. David Buck concludes his study of "Educational Modernization in Tsinan [Jinan]" with an endorsement of Ichiko's position that "the gentry proved able to adapt to the challenges of the late Ch'ing [Qing] and early Republic by assuming whatever political coloration was needed to maintain power" (Buck, 1974. 211). Robert Kapp discusses the emergence of "bad gentry," "militia lords," and "local bullies" in Sichuan as part of "a metamorphosis of the traditional local sub-official elite that had commenced long before 1911 (Kapp, 1974· 169). In this, he seems much closer to Ichiko's position than to Mary Wright's. And most impressively, Philip Kuhn concludes his spendid study of local militarization in nineteenth century China with an endorsement of Ichiko's "thesis that 'local self-government' was an opportunity for the conservative local elite to expand their influence" (1970: 217n).

But Kuhn also suggests an hypothesis to narrow the gap between Ichiko and Wright. He argues the need to distinguish between "national, provincial, and local levels" of the elite, and suggests that "modernization produced, or widened, cleavages within the elite" (p. 217n). Thus Ichiko's local elite could be

composed of conservatives interested only in expanded influence, while the national and provincial elites covered by Wright could be interested in Western-style urban modernization. This, it seems to me, is an extremely useful hypothesis. It allows us to recognize the achievements of Elvin's Shanghai merchant and gentry elite, without accepting his hypothesis of a "national system of gentry democracy" (Elvin, 1974: 250) sharing the Shanghai Municipal Council's supposed goal of a "municipal welfare state" (p. 259). It helps us to explain Buck's phenomenon of educational modernization in the cities, while in the rural areas government schools closed when parents preferred to send their children to illegal, clandestine traditional schools. There was, as Buck notes, a "widening gap between rural and urban standards (1974: 211).

There is little doubt that the next phase of our research on this era of Chinese history will have to be centrally concerned with Kuhn's hypothesis of a cleavage within the elite—of, in fact, a split in the ruling class. But there are several problems raised by this approach which have to be faced. In the first place, the cleavage is quickly diminished if we recognize what was unquestionably the case: that the local elite's interest in expanded influence did not preclude acceptance of a degree of institutional modernization (schools, police, self-government councils), and the urban and provincial elite's interest in modernization did not entail an abandonment of ambitions to influence. Thus the difference between the levels of the elite becomes one of degree of commitment and of initiative. I would suggest that the initiative for such innovations as constitutionalism and local self-government came from the cities, where the threat of imperialist partition was both more visible and more real. Largely in response to that threat, the urban elite sought to strengthen *both China and itself* by Westernizing local and national political institutions. The local elite of the towns and *xian* capitals, on the other hand, was not so active in pressing for local self-government as an answer to the national emergency, but did see in these new political forms an ideal opportunity to strengthen and institutionalize its local political power.

A second problem arises over the popular reaction to the reforms. On this issue, I side unequivocally with Ichiko in stressing the seriousness of mass opposition to and violence against the Qing reforms. It seems to me that Wright's and Elvin's sanguine appraisals of the reforms' chances for success must be questioned given the extent and depth of this popular opposition. But since most of this opposition expressed itself outside the major urban centers, we must ask: was it directed at the reforms themselves, or only at sham reforms carried out by local gentry interested primarily in preserving their own power? If the latter was the case, then the weakness of the reform program was less serious, being more of execution than of design. If, on the other hand, the population regarded the reforms as costly, culturally alien and benefiting only the elite—institutionalizing its power in local self-government, educating its own sons in modern schools, protecting its position with modern police—then the weakness of the reforms was inherent in their design. I suspect that both types of opposition occurred, but we shall have to do a great deal more local research to discover which was more important and what implications this opposition has for our assessment of the reforms and the reformers.

A NASCENT BOURGEOISIE?

Turning to the other side of Kuhn's cleavage in the elite—to the provincial and national elites in the major urban centers—we must ask what social category best characterizes this group. Here the crucial question concerns the degree of articulation of a bourgeois class. Marie-Claire Bergère has tackled this question most directly and writes that "at the turn of the last century, a new class arose in China: the commercial bourgeoisie" (Bergère, 1968: 237). How distinct was this new class? Bergère argues that on the one hand "the bourgeoisie had begun to display unity and its own particular characteristics" (p. 239); while on the other:

> The gentry, whose social position had become less clearly defined with the decline of the Manchu government [resulting in the increased sale of examination degrees] after 1870 and the abolition of the Examination System in 1905, and the nascent bourgeoisie who had not yet attained a clearly defined social position, merged to form the merchant-gentry *[shenshang]*, a class composed of influential people of each locality, closely connected with land ownership, but not above taking the opportunity to derive profit from investment in modern business ventures. [p. 240]

She further argues, and I think correctly, that while in the interior the fused merchant-gentry stratum (I would hesitate to call it a class) tended to prevail, along the coast the bourgeoisie had achieved greater articulation as a distinct class (pp. 240-241). Certainly Edward Rhoads' study of "Merchant Associations in Canton, 1895-1911" (1974: 97-117) gives powerful support to the notion of a merchant class with interests and institutions distinct from, and even in competition with, those of the gentry.

In the years before 1911 a "bourgeois ideology" of nationalism and constitutionalism gained currency (Bergère, 1968: 242-244), and the bourgeoisie played an important role in the boycotts of foreign goods and the agitation for parliamentary government (pp. 251-257). But Bergère argues persuasively that when the Revolution finally came, the bourgeoisie proved too weak to play a leading role. "The weakness of the provincial bourgeoisie was particularly conspicuous. There were too few of them and they were too little differentiated from the gentry to be able to free themselves from subordination to them" (p. 279). The Revolution, therefore, was not a "bourgeois revolution." "Since the bourgeoisie had not yet attained its full strength as a class, its role could not be anything but subsidiary" (p. 295).

In general I find Bergère's conclusions sound and well-stated. In particular, her admonition to pay attention to regional differences is well taken. In every area in which the Revolution is studied, the configuration of participating elite groups seems different. In Canton, gentry and merchants seem to be in

separate camps. In Shanghai, the two seem well merged, although in that most commercialized of Chinese cities, the role of the bourgeoisie in the alliance seems stronger than in any other place. In the interior, the urban gentry were unquestionably the senior partners in the merchant-gentry alliances which emerged. In the North, as one moves further from China's great commercial centers, the domination of officials and gentry seems even more pronounced.

No one, however, should believe for a minute that such easy regional stereotypes as these will solve the problem of identifying and analyzing the Chinese elite in 1911. Lest any be tempted to leap to easy conclusions, we should look at the difficulties encountered in the first provincial study to be published on 1911: Edward J.M. Rhoads' *China's Republican Revolution: The Case of Kwangtung [Guangdong]* (1975). Although he works from a theoretical framework which includes Chalmers Johnson's "disequilibrated social system" and James C. Davies's "revolution of rising expectations"—two rather psychological approaches to revolution for which I have little sympathy—Rhoads presents a compelling narrative of nationalism, reform, and revolution in one province. He stresses the menace of imperialism, which was "far more rapacious" (1975: 29) in this period, and which played, as "China's paramount concern" (p. 267), a critical role in stimulating nationalism and reform (pp. 29-33, 59-65). His descriptions of the local workings of educational reforms, anti-American and anti-Japanese boycotts, the impact of the new and increasingly radical press, the activities of the Provincial Assembly and local self-government bodies, and peasant reactions to gentry-led reforms are excellent and based on the soundest, detailed local research. But if we sort through this narrative and analysis in search of the identity of the political elite in the reforms and the Revolution, we will perhaps be a bit disappointed.

By and large, Rhoads supports Ichiko's view that the gentry emerged from the Revolution strengthened. The Qing reforms served "to increase the power of the local elite at the expense of the masses, expecially the rural masses" (p. 153). Though

"anti-gentry tax resistance" and other popular disturbances produced, on the eve of the Revolution, an "anarchic situation in the Kwangtung [Guangdong] countryside" (p. 213), the gentry managed to suppress opposition and emerge from the Revolution "perhaps stronger than ever . . . and, even in Kwangtung the dominant group in society" (p. 258). "Nationally," Rhoads writes, "the heirs to the revolution were the old imperial elite, the scholar-officials" (p. 234).

This is all well and good, but Rhoads is also anxious to explain to us the manner in which Chinese society and its gentry elite were changing in this period. He sounds a lot more like Mary Wright than Ichiko Chūzō when he writes that the reforms "produced a fundamental transformation of Chinese society" (p. 50). Analyzing the impact of the educational reforms after 1905 on recruitment into the elite, Rhoads states:

> the abolition of the examinations had not done away with the gentry class but had merely subjected it to a different set of qualifications, to produce the novel subtype, the "new gentry." It was only with the 1911 Revolution, when all connection was severed between civil service and the schools, that the scholar-gentry as a distinct social group disappeared. [p. 75]

This is a little bit difficult to fathom Perhaps there is a distinction between the "scholar-gentry" which disappeared, the "scholar-officials" who were heirs to the Revolution, and the "gentry" who were the dominant group in postrevolutionary society. But I, at least, found myself at a loss to describe what that distinction was.

The problems are even more complex when we turn to the merchants and the bourgeoisie. First, there is the problem of the independence of the bourgeoisie. With their own Canton Merchants' Self-Government Society, existing in competition with the gentry's Association for the Study of Self-Government, the Cantonese merchants would appear to have been relatively independent, extraordinarily active in the nationalist agitations of 1907-1909 (pp. 148-152), and quite influential in the Revolution (pp. 222-230). In an earlier article, Rhoads claimed

that by their support of the Revolution in 1911 and the withdrawal of that support two years later, the merchants "determined the fate of the revolution in Canton" (1974: 117). Yet I have some doubts that gentry and merchants were quite as distinct, even in Canton, as Rhoads suggests. For example, I am struck by the fact that several of the key nationalist demonstrations involving the merchants were held at the Confucian Temple (pp. 141, 144), which was, of course, in the gentry's eastern half of Canton (Rhoads, 1974: 101-102). And it seems notable that the most prominent "gentry" activist in Guangdong's troubled maneuvers toward independence after the Wuchang Uprising was one Jiang Kung-yin, a *jinshi* who was also a leader of the Merchants' Self-Government Society (Rhoads, 1975: 216 ff.). Since Rhoads also speaks of "the socially prominent merchants, the merchant-gentry" in the Chamber of Commerce (p. 80), it is extremely difficult to know to what degree a distinct, self-conscious bourgeois class had emerged.

Secondly, to the degree that merchants did act as an independent political force, it is difficult to interpret what that action meant. This is particularly true of their actions in 1911-1913, actions which are critical in determining to what extent 1911 was a "bourgeois revolution." When, in October-November 1911, the gentry and the governor-general, equivocating between a monarchy and a republic, put forth a plan for provincial autonomy, the merchants emerged to press the republican revolution. Rhoads interprets this action as directed against "the gentry's traditional control of society," and in favor of "the revolutionaries, who were ideologically, if not in class origins, 'bourgeois' " (p. 229). But the narrative seems to indicate that the merchants were afraid the gentry equivocation would leave Canton prey to bandits and revolutionary-aided "people's armies" which were moving on the capital from the countryside (pp. 222-225). Their motivations, therefore, appear less those of a progressive bourgeoisie and more those of a frightened monied elite. Similarly in 1912-1913, the new Tongmenghui-controlled revolutionary regime attempted an

impressive series of public health, public works, anti-opium, anti-gambling, legal, social, economic, and religious (anti-Buddhist and Taoist, pro-Christian) reforms which Rhoads describes as "distinctly 'bourgeois' " (p. 258) and "an urban-oriented political and cultural revolution catering to the merchants" (p. 259). But the reforms were costly, and the Canton merchants, with a view toward halting inflation, were primarily interested in fiscal responsibility. Thus: "To satisfy the requirements for fiscal responsibility, the government had to give up some of its reform projects" (p. 241). That is, the merchants sabotaged the "bourgeois" reforms.[4]

In piecing together these apparently conflicting parts of Rhoads' narrative, it is not so much my desire to question the analytical accuracy of the book as to establish the great difficulty one should expect in any attempt to develop a consistent and simple description and analysis of the various elite classes and strata operating in the 1911 period. I believe that there are serious contradictions in Rhoads' book, but it is highly possible that they merely reflect the contradictions inherent in the society. The social categories overlap, and the members of certain social groups do not always act as anticipated: merchants do not act "bourgeois"; gentry call themselves "merchants" and join Chambers of Commerce. Rhoads has done a remarkable job in communicating the complexity of the various sociopolitical groups in Guangdong. But serious analytical problems remain, both in the Guangdong area and in comparing Guangdong with other areas.

THE MASSES

At this point the reader is entitled to object. where were the masses? Did the people not participate in the Revolution at all? Was this altogether an elite revolution, dominated by gentry (whether conservative or progressive), merchants (whether bourgeois or merchant-gentry), New Army officers and perhaps a "new intellectual elite" (for K. S. Liew makes a claim for this "identifiable self-conscious class of men"; 1971: 33, 34)? Have

we, by discounting the importance of revolutionaries in the Revolution, eliminated the only group with any contacts to popular revolutionary forces? The most serious challenge to the notion of 1911 as an elite revolution has been mounted by John Lust, who argues that: "The revolution from above so often depicted in sources is an exaggeration, growing out of the elitism of New Army officers and literati" (Lust, 1972: 195). Lust's important article on "Secret Societies, Popular Movements, and the 1911 Revolution" presents the most powerful argument yet put forward for the significance of "movements from below" (p. 165) in the revolutionary period.

Lust's argument begins with elements common in Japanese and Marxist histories of the period, and at odds with the sanguine Wright-Elvin view of the Qing reforms. He sees China gripped by a "profound social and economic crisis" in this period, brought about by over-population, rising taxes, neglect of irrigation systems and public granaries, and "the flight of the gentry to the towns, leading to the deterioration of their mediatory role between officials and people" (p. 166). This crisis was exacerbated by the Qing reforms, the "New Policies of modernization" which brought new tax burdens for the lower classes and new opportunities for the gentry to advance their administrative and economic positions. In response to these reforms, rioting and local outbreaks became commonplace (p. 167). "Ultimately, both the authority and the self-confidence of the establishment were undermined" (p. 170).

In this social context, "a Chinese form of populism, a limited osmosis between radicalism and old-style insurgence" became possible, largely on the basis of the "anti-Confucianism" of the *renxia* (knight-errant) tradition (p. 170). It is clear that Lust is speaking here of the sort of alliance which Mary Rankin's radical intellectuals formed with the secret societies in Zhejiang. But the emphasis is somewhat different. Rankin stresses gentry leadership and manipulation of the secret societies; Lust cautions against "underestimat[ing] the ability of the illiterate or semi-literate Chinese to size up political situations" (p. 172). Rankin notes the social and cultural gap which separated radical

intellectuals from popular elements, while Lust (though admitting that in some Tongmenghui leaders like Hu Han-min one can detect "the alienation of the bourgeoisie from the old system, and with it the old popular movements" (p. 176); stresses the union formed on the basis of radical populism.

These questions—the degree of elite control of the secret societies, and the degree of cultural and political fusion between radical intellectuals and popular elements—will certainly remain crucial during the next phase of research on 1911. My own guess is that while, as Rankin herself admits (1971: 139), gentry control of the secret societies was more pronounced in Zhejiang than anywhere else in China, the societies cannot too easily be taken to represent "popular" revolutionary forces. Many societies were dominated by locally powerful magnates, while others (especially the Ge Lao Hui in Central China) were largely composed of lumpenproletarian elements along the rivers who participated in revolutionary activities in what often resembled a mercenary capacity. Nonetheless, one must assume that lower-class secret society members who participated in political protests did not do so as passive pawns of elite manipulators, but because they saw some potential political or economic advantage to be derived from their participation. In urging that the political participation of popular elements in the secret societies be taken seriously, Lust is undoubtedly on the right track.

The question of elite-mass union on the basis of "radical populism" is more complex. It is easier to see this union, based on shared notions from the *renxia* tradition, during the period of revolutionary uprisings. But Lust also applies the term to the 1911 revolutionary government of Jiao Da-feng in Hunan and the Self-Government Society in Guizhou. It is noteworthy that it was in the interior, among the more traditional, less "bourgeois" revolutionaries that the most effective "populist" alliances were formed. In Canton, by contrast, the Westernized urban government of Hu Han-min quickly came in conflict with the "People's Armies" of the hinterland. Unfortunately, however, the Hunan and Guizhou regimes of 1911 were over-

whelmed so quickly by more conservative forces that it is difficult to know just what their "radical populism" might have amounted to.

My doubts about the viability of the "radical populist" characterization of these regimes are best explained by examining the connection Lust attempts to make between the 1909-1910 riots against the reforms and some aspects of the uprisings in 1911. He describes the 1910 anti-reform riot in Laiyang, Shandong [Shantung], in which "bad gentry" and officials were attacked for their self-interested manipulation of the reforms. He properly regards it as indicative of "the violence inherent in the polarization of classes" (Lust, 1972: 169). Then he argues that "the logic of the Laiyang riot had been carried to its conclusion" in Sichuan in 1911 when the gentry-dominated Railway Protection Society (with an undetermined degree of assistance from Tongmenghui members) mobilized the secret societies to oppose the Qing policy of railway nationalization and foreign loans (p. 187). Thus when Lust gets to 1911, "the polarization of classes" becomes a "radical populist" (or in Sichuan, and "nonradical") alliance of classes. This is a major transformation, and I frankly suspect the viability of these alliances. Not only were the "radical populist" alliances threatened by the conservative landlord-military forces which Lust sees overwhelming them, they were also threatened by internal contradictions. The "radical populists," after all, were fully in favor of most of the reforms—modern schools, a New Army, local self-government, modern police forces, and so on. Yet these were precisely the costly, elite-serving reforms that popular riots had been directed against in the pre-1911 period.

We can appreciate the problem most simply, perhaps, with a brief look at Winston Hsieh's articles on the People's Armies of Guangdong. There, opposition to the exactions brought about by the Qing reforms seems to have led directly to the secret society risings in the Huizhou region (Hsieh, 1972: 160-164). Similarly, in the Canton delta region, resentment against the reforms combined with a sudden reversal of economic fortunes in certain market areas to produce insurrectionary forces in

1911 (Hsieh, 1974: 131-133). Hsieh argues quite convincingly that these People's Armies were motivated by local concerns, operated almost exclusively within their own market areas, and sought to defend their locality from outside interference, whether by market disruption, increased taxes, or the incursions of government troops. Their alienation from the modernizers—whether Qing reformers or the Tongmenghui government of Hu Han-min—was rather complete, and the latter were ultimately impelled to disband them forcibly. Here, the "polarization of classes" seemed to carry over into 1911 itself.

In general, though Lust has done a great service in challenging elitist interpretations of the Revolution, he has probably gone too far in the other direction in stressing the direct popular contribution to the republican cause. My own feeling is that the secret societies, the peasants and the urban poor in the anti-reform riots, and all other popular anti-establishment forces contributed to the Revolution rather more indirectly. They demonstrated their power in the widespread riots of 1909-1911, and stood ready to take advantage of the weakness of established authority at the time of the Revolution. They impelled the Revolution forward, but not always by their direct participation. As often as not, they caused elements in the elite institutions—the Provincial Assemblies, Chambers of Commerce, New Army units, local self-government offices—to preemptively declare for the Revolution in order to co-opt the anti-Manchu issue for themselves and preserve stability and social order in the midst of the revolutionary transition. Thus, popular elements contributed significantly to the coming of the Revolution, but the manner of their contribution unfortunately gave the Revolution a rather conservative, "law and order" cast.

If we are to look for "popular" forces directly participating in the Revolution, the New Armies would seem the only remaining place to turn. Hatano Yoshihiro has argued that these armies "successfully channeled peasant discontent into organized, revolutionary form" (Hatano, 1968: 382).[5] But his own evidence is rather conclusive that it was the poor peasants who were discontent with Qing reforms, while the

revolutionaries in the New Armies were drawn largely from landlord, merchant, or rich peasant families—often educated in the new schools which the peasantry in general opposed. Furthermore, the New Army soldiers usually remained subordinate to their elite-recruited officers, who in turn showed a stronger tendency to ally with the civilian gentry and merchant elite than to strike out on any popular revolutionary course. Having said this, however, let me at least qualify to the extent of admitting that insofar as the Revolution has a radical republican wing—opposed to many compromises with the existing elite establishment—that wing does seem to have been based on soldiers of the New Army. But that wing was too weak to operate on its own, was easily crushed in the Second Revolution of 1913 (if not before), and was still a long way from representing the fundamental social and economic grievances of the peasantry.

CONTRADICTIONS AND DIALECTICS

How would one summarize the findings of this recent scholarship on the 1911 period? Roger Des Forges begins his book on Xi-liang by tracing what he calls a "revisionist consensus" emerging in studies of late nineteenth- and early twentieth-century China (1973: xii). I do not believe that any such consensus exists. As scholarship on this period has become more sophisticated, it has begun to tackle problems which are more intractable. Debates have emerged to mirror the contradictions of history. I would not have it otherwise. Were we all to seek consensus, history would be written in platitudes: either meaningless generalities or bundles of discrete and insignificant facts. We would all die of boredom.

Fortunately, then, there is little consensus on the 1911 Revolution or its place in history. In general, there are two schools; most (including Gasster, Rankin, Friedman, Des Forges, Wright and Lust) see in this period the origins of the later Chinese communist revolution. They find these origins largely

in the intellectual and ideological developments, in the "isms" of this period. radicalism, anarchism, populism, nationalism, feminism, republicanism. Gasster and Rankin locate these "isms" primarily in the revolutionary camp, and argue (in one fashion or another) that though these "isms" may have had limited influence on the 1911 Revolution itself, they helped shape the future course of history and spawned elements of the later Chinese revolution. Friedman makes a similar argument, but focuses on religion and community instead of "isms." Des Forges, Wright and Lust, on the other hand, are willing to see the "isms" acting through most if not all of Chinese government and society in the years following 1900, having a recognizable causal impact on the events of 1911, and contributing to the progressive development of a single extended Chinese revolution in the twentieth century.

The other major school of interpretation sees social developments which lead not to the communist revolution, but on the contrary, to the disjointed society of warlordism which later Chinese revolutions sought to end. Ichiko and his supporters are obviously the most prominent of this group, which stresses the enhanced power of the gentry and local elites in the process of dynastic collapse. It should be noted, however, that this school bears many similarities to the theories of Franz Michael and others on regionalism and provincialism in modern China. Michael has described a progressive devolution of power beginning with the regional armies which suppressed the Taiping and other rebellions and led down to warlordism in the 1910s and 1920s (1964: introduction). The provincial declarations of independence from Beijing in 1911 were a crucial stage in this devolution.

Some may be tempted to despair over this debate: my God! Can't we even agree whether China was building towards a great revolution, or crumbling toward the confusion of warlordism? But there is obviously no reason for distress. China moved from 1911 *both* to warlordism and to revolution, and the historian is entitled to find the causes of either or both in the 1911 period. In fact, one can find origins for both in the same aspects of this

period. Take, for example, provincialism. There are obvious links between the provincial unit of reform in the late Qing—which produced armies, mints, industries and arsenals built by and for the separate provinces—and the disunified polity of the warlord period. On the other hand, John Fincher has argued persuasively that the provincialism of 1911 was "oriented to the national polity" and "transitional to rather than an obstacle to nationalism" (Fincher, 1968: 220). Often the province was simply the most efficient unit for members of the elite to take as the basis for their nationalist projects—be they railway or mining rights recovery, industrial development, or political organizing for constitutionalism. Thus provincialism cannot be regarded as wholly devolutionary: some of it was the most potent and efficient form of nationalism that this period was to produce.

If provincialism can be a form of nationalism, we are on the road to recognizing the fantastic complexity of nationalism itself. Many have observed the "rise of Chinese nationalism" in this general period. But if the observation stops there, it is a most unhelpful one. We must go on to recognize that different types of "nationalism" implied different and indeed contrary policies. As we have seen, some, like Sun Yat-sen, could be both Chinese nationalists and "running dogs of imperialism." One step away were those who, in order to strengthen the nation, pressed schemes for foreign loans at substantial sacrifice to Chinese sovereignty. Usually these were advocates of political and administrative centralization—the court and Sheng Xuan-huai under the Qing, Sun Yat-sen and the Nanjing government of 1912, and Yuan Shi-kai in Beijing in 1913. In each case, the opponents of this sort of foreign-financed national centralization were the provincial interests with their own form of nationalism, building resistance to imperialism on the intense defensive interests of local elites.

Once we recognize that provincial gentry can be both sincere nationalists and defenders of their own interests and positions, we can begin to appreciate some other important contradictions in the Revolution. The fact is that for all their progressive

nationalist motivations, the provincial gentry and provincial military men (who acted in efficient concert in the revolutionary period) emerged from the Revolution with substantially increased power. That the masses neither benefited from nor welcomed that increased power was indicated by pre- and post-1911 attacks on the reforms of which these elites were both the architects and the benefactors. In this respect, then, both the reform and the Revolution were socially regressive.

This socially regressive quality of the Revolution set up two separate contradictions which impelled the course of Chinese history to later and larger revolutions. Constitutionalism, republicanism, and talk of "people's rights" were unquestionably a political advance over the Confucian monarchy of the Qing. Thus the Revolution, while socially regressive, was politically progressive. The later stages of the Chinese revolution would attempt to carry out the social revolution which was forgotten, feared, or frustrated in 1911. Secondly, there was the contradiction between the lofty ideals of the radical intellectuals—the nationalism, anarchism, and republicanism which Gasster and others have found so prominently displayed in the pages of the revolutionary journals of this period—and the paltry achievements of the Revolution. One theme of the May Fourth era was certainly that a more thorough social and cultural revolution than 1911 would be necessary to bring Chinese reality into phase with the ideals of China's leading intellectuals.

In my mind, the next stages of research on the 1911 Revolution will have to confront these and a host of other contradictions. It will not do to wish them away, or to choose only one horn of the dilemma. We must recognize a period of both revolutionary growth and dynastic collapse. We must see that the power of the elite grew, while the elite both widened and split. Between elite and masses there were both new alliances and new alienation. There was political progress and social regression. Provincialism could be nationalism, and nationalists, "running dogs of imperialism." Lofty ideals were furthered by self-serving deals. Each of these contradictions is

inherent in the history. Already they have served to produce productive debates in the scholarship under review. With luck, future debate will be characterized by greater precision, higher quality, and increased intensity. After all, not only history, but history-writing as well, must move forward in a dialectical fashion.

NOTES

1. On this interesting and forgotten figure, see Chün-tu Hsüeh (1960), "Sun Yat-sen, Yang Ch'ü-yün, and the Early Revolutionary Movement," Journal of Asian Studies 19, 3 (May): 307-318

2. It might be noted that more recently, anthropologists (largely Westerners) have begun to take Chinese religion more seriously. Their findings, however, do not necessarily bear out Friedman's contention that religion is at the heart of revolution. Note, for example, Arthur P Wolf's contention that Chinese gods are perceived by peasants as a hierarchy of bureaucrats, "a detailed image of Chinese officialdom." Thus: "Assessed in terms of its long-range impact on the people, [the Chinese imperial government] appears to have been one of the most potent governments ever known, for it created a religion in its own image. Its grip on the popular imagination may have been one reason the imperial government survived so long despite its many failings Perhaps this is also the reason China's revolutionaries have so often organized their movements in terms of concepts and symbols of such foreign faiths as Buddhism and Christianity The native gods were so much a part of the establishment that they could not be turned against it" (Wolf, 1974: 145)

3. It should be noted that Chang's article is an abbreviated version of a larger study in Chinese, *Lixianpai yu xinhai geming* (The constitutionalists and the 1911 Revolution), Taibei: 1969 Similar issues are further elaborated in Zhang Yu-fa's *Qingji di lixian tuanti* (Constitutionalist organizations of the Late Qing), Taibei: 1971.

4. See also Edward Friedman, "Revolution or Just Another Bloody Cycle? Swatow and the 1911 Revolution," which treats the Swatow merchants as "good burghers believing in law and order" (1970: 290), who ultimately "won by calling in outside forces" (p 296) in the form of British and American imperialism Friedman notes that because of this merchant defection, "progressive reforms ended long before the reform government of Canton was removed from power" (p 306)

5 This argument is more explicit in Hatano's Japanese works than in this article See especially his "Minkoku kakumei to shingun: Bushō shingun o chūshin to shite" (The republican revolution and the new army: with special reference to the Wuchang new army).

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