



Warlord Studies

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Source: *Modern China*, Vol. 6, No. 4 (Oct., 1980), pp. 439-470

Published by: [Sage Publications, Inc.](#)

Stable URL: <http://www.jstor.org/stable/189036>

Accessed: 18/02/2011 06:37

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Warlord Studies

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Things fall apart; the centre cannot hold;
Mere anarchy is loosed upon the world,
The blood-dimmed tide is loosed, and everywhere
The ceremony of innocence is drowned;
The best lack all conviction, while the worst
Are full of passionate intensity.

W. B. Yeats, *The Second Coming*

Yeats was writing about his beloved, strife-torn Ireland, but his words describe just as well the agony of China during the warlord period, that period of chaos, confusion, anguish, and absurdity which followed the end of the Qing dynasty, when China lay at the mercy of her enemies, like Gulliver assailed by the Lilliputians.

China passed through the darkest period of her modern history under the warlords. Though some outsiders could laugh at the "comic-opera" aspects of warlordism, at silver bullets and at swaggering Ruritanian generals laden down with medals and gold braid (whoever supplied the uniforms must have made a fortune), few Chinese ever did. The participants were deadly serious about their activities; if they were comic, it was not to themselves, nor to those upon whom they preyed. For its victims, warlordism was a profound tragedy, an endless menace and degradation. For many

peasants, warlordism was a burden which made hard lives unendurable. For merchants and businessmen, warlordism meant intolerable interruption and harassment. For China's politicians and intellectuals, warlordism carried the despairing connotation of reducing the country to wrack and ruin without promise of any salvation. It confounded their hopes for the future by showing that a country could continue to exist in degradation, without unity, without law, without order. Even foreigners who were committed to China regarded warlordism as an unmitigated evil. It disrupted the trade of businessmen, ruined the lives of missionaries' converts, and created an atmosphere of terror in which even the cosseted foreigner became fearful for his own safety.

Warlordism entrenched the military at the center of Chinese life, it introduced hundreds upon thousands of young men to the military life as officers and as soldiers. By its own actions, and by the adjunct actions of banditry, it institutionalized violence. It reduced politics and political solutions to impotence and ridicule; it substituted fear and force for due process. It undermined China's economy, threw her fiscal system into disarray, and retarded efforts at economic advancement, except in areas under foreign control.

Warlordism also created an atmosphere of great instability, made all previously solid institutions tenuous and shaky, and in the process created a fluidity and a flexibility not known in China before or since. Under warlordism, a great deal of social change occurred. The armies created new patterns of social and economic advancement, while a wide variety of new avenues opened up for civilians, whether opponents of or collaborators with warlordism. This was a great period for writers, for artists, for entrepreneurs, for eccentrics, and above all, for radicals. Traditional career structures melted away, and individuals were forced to carve out new niches for themselves, unimpeded by established patterns or by concern for security, in a world where that concept was redundant. The Guomindang and the Chinese Communist Party were only two among the many radical political organizations formed during this period; women's movements, student movements, workers' movements all appeared and pursued their strident (if

tenuous) careers. All these movements were the products of the profound sense of despair which accompanied warlordism, a despair for which only radical solutions offered an assuagement.

It is hard to find anyone who will say a good word for the warlord period. The almost universal judgment of commentators—whether contemporary or later—is negative. And yet it remains a period of great fascination, of anguish and misery for the many, but also the progenitor of the new China, the context out of which the new society would emerge.

This article will look at some of the approaches to warlordism in Western languages, at the present state of the field, and at what work is still to be done. First, some questions of definition.

WARLORDISM

The term warlordism has a long and dishonorable pedigree. It is in itself such a strongly descriptive term that the need to define it more precisely may seem a rather exaggerated semantical exercise. And yet most of the people who have worked on Chinese warlordism have felt it necessary to give some more precise definition to the term. Some have approached the matter by listing the variant dictionary definitions of the term “warlord” (in English) (Wou, 1970: 12). Others have noted the Japanese origin of the Chinese term *junfa* (Gunbatsu), which means “military house” rather than “warlord.” Most have attempted to identify the key elements contained in the term “warlord.” Rather than list a number of subtle, often minute variations, I have chosen here only to give the elements which recur in the majority of definitions: the possession of autonomous military force, the control of a base region, the use of force as the final arbiter, the reliance on personal rather than impersonal patterns of rule, and a ruthless and extractive attitude towards society and the economy.

Some writers on the subject have felt uncomfortable with the term “warlordism,” insofar as it obliges them to call the proponents of warlordism warlords. They regard the term, quite

rightly, as derogatory, and prefer to use more decorous, value-free terms. Thus Chi Hsi-sheng calls his protagonists "militarists" (Chi, 1976: 1), Hall calls them "members of a provincial faction" (Hall, 1976: 2), and Kapp combines the two notions by calling the men he studied "provincial militarists" (Kapp, 1973: 33). I do not share these sensitivities. While it is true that no warlord ever referred to himself as a warlord, but used much more high-sounding honorifics and titles, few of the individuals whom other Chinese called warlords manifested the kind of personal characteristics which would give them the right to have their sensitivities pandered to. A spade is a spade, and a rose is a rose.

THE WARLORD PERIOD

Periodization is a favorite game of historians, though not one practiced with great sophistication. We all feel the necessity to divide, to give beginnings and endings to historical processes. But we tend to accept unquestioningly division by century or by decade. We talk about nineteenth and twentieth-century China, even about China in the 1920s (Chan and Etzhold, 1976), without establishing whether these periods have any relevance to what was actually happening in China. We accept revolutions as major turning points, though only in certain aspects of history do they have any direct meaning.

It should be easy to set dates for warlordism, which was a phenomenon of mercifully brief duration, but there is a substantial division of opinion. If we leave aside the question of the origins of warlordism (which will be dealt with below), there is disagreement about the advent of warlordism as a full-fledged historical phenomenon. Some writers have dated it from 1912, from the rise to national power of Yuan Shikai, on the ground that thereafter China was clearly under military rule, that military might had become the ultimate arbiter and legitimator of power holding (Ch'en, 1979). Most wait for Yuan's demise in 1916, seeing the end of centralized military power as the beginning of full-scale fragmentation, the move from militarism to warlord-

ism. The key distinction here is between centralized and fragmented military power. I prefer the earlier date; I see during Yuan's rule the initiation of a process which may have reached its lowest depths after his disappearance, but did not leap out of nowhere.

If there is disagreement over the beginning of warlordism, there is fiercer contention over its ending, a contention which has strong political overtones. Wilbur asserts unequivocally that warlordism came to an end with the end of the Northern expedition in 1928 (Wilbur, 1968). Chi Hsi-sheng states that in 1928 "particular patterns of interaction among the political actors came to an end" (Chi, 1976: 9), presumably meaning that men in power ceased to behave as warlords. Thereafter, in this view, China returned to centralized rule, and those who opposed the center were rebels, not warlords. This view falls on two grounds: first, large areas remained under warlord control and were not subject to the programs of change envisaged by Nanjing (Lary, 1975); second, of those many warlords incorporated into Nanjing's armies, few departed significantly from their former practices.

Sheridan's concept of residual warlordism goes some of the way to coping with these anomalies, but it still assumes that Nanjing itself had definitely departed from the practices of warlordism (Sheridan, 1966: 15). Other harsher writers see the "Nanking Decade" (1928-1937) as an intensification of warlordism rather than of its disappearance, at least for those areas only nominally under Nanjing's flag; Kapp, for example, sees an incremental growth of militarization and fragmentation during that period (Kapp, 1973: 61).

One might expect that the Japanese invasion of China in 1937 would put a final full-stop to warlordism, but one has only to look at the continued existence of Yan Xishan and Long Yun to see that this was not the case. While much of China passed under the brutal militarism of the Japanese, other areas remained in the control of their familiar warlord masters. Warlordism can only be terminated in reference to specific areas; that date might be anywhere between 1925 (the reunification of Guangdong) and 1949 (the Liberation).

The question of whether Jiang Jieshi himself was a warlord is sometimes raised, usually to vilify that gentleman yet further. Though he remained true to many of the basic concepts of military rule, he nevertheless presided over a government and a Party which had pretensions—and some real claim—to establishing itself as a new national government, interested in the future of China rather than simply in the future of its own members. To call Jiang simply a warlord is not fair.

Whatever the periodization one uses, there is no doubt that in the warlord period one is looking at a period of interregnum, a period connecting two periods of strong, unitary government, a period which saw the final demise of the old order and the beginning of the new. This notion of interregnum, with its overtones of instability, of decay and birth, is probably more important than specific dates.

SOURCE MATERIALS

Having looked at the rather picayune question of definitions, I want before we turn to the various studies of warlordism to look at some of the source materials which exist for the study of warlordism. Warlordism is a subject for which vast, rich, and sometimes overwhelming arrays of source materials exist.

While warlordism held sway in China, a great number of newspapers and journals were published. This period saw in fact the greatest period of blooming and flourishing which the Chinese press has known to date; the freedom of the press related not only to the chaos and consequent inefficiencies of the period, especially in so far as censorship was concerned, but also to the fact that censorship could be easily avoided by publishing from the safety of the foreign concessions. The variety and quality of daily, weekly, and monthly newspapers and journals varies enormously. Official government gazettes (*gongbao*) published by Beijing governments, by Nanjing, and also by provincial governments provide important running information on what these governments were doing and what they intended to do. As

mouthpieces of the holders of power, they must, of course, be read very critically. Other independent newspapers and serials were published in large numbers. Some of them, such as *Dongfang Zazhi* (*Eastern Miscellany*) and *Huazi ribao* (*China Mail of Hong Kong*) endured for long periods; more came and went like mayflies. Whether short or long lived, they are treasure houses of information on the period and often a pleasure to read, since they were produced in a climate of competitive journalism in which readership had to be wooed and retained.

In addition to the press, there are vast quantities of government documents, emanating from every level of government—statistical compilations, government blueprints, government communications. Many of these government documents were produced by warlord governments; warlords competed with each other not only on the battlefield but also in print. These publications vary widely in terms of quality and veracity, but with careful handling they can be made to yield valuable information.

Missionary records, consular records, and the foreign press also provide excellent resources for the study of warlordism, for warlordism was a phenomenon which the foreign world in China could not ignore. In some instances, missionaries were the only direct commentators on events, since they were the only people who had access to publication sources. I am thinking, for example, of the French Jesuit missionaries in Western Guangxi, who provide invaluable insights not available elsewhere.

Chinese novelists and short-story writers provide the most penetrating and anguishing descriptions of warlordism, from the point of view of its victims. A writer such as Shen Congwen gives a more immediate and direct entry into a world dominated by a rapacious military than almost any other source.

The warlord period is still so recent that numbers of the participants and their cohorts are still with us to be interviewed by scholars or to publish their bowdlerized memories in the monthly *Mingbao* or in the biographical magazine *Zhuanji wenxue* (both published in Hong Kong).

The scholar of warlordism labors under an overabundance rather than a dearth of source materials, the bulk of which are in

Chinese or Japanese. Most scholars have not been deterred by this bulk, nor by the languages they appear in. Though one sometimes sees an overreliance on consular sources, without regard for their inbuilt biases, only one Western scholar has ignored Asian language sources, relying instead on French and Russian sources (Bouissou, 1974). There remain, though, great reservoirs of materials which have been untapped or only scratched, especially local sources. Many high-quality gazetteers were published in the period, right into the 1940s, and other local materials exist which have scarcely been touched. A very large collection of materials on the period is in process of publication in China (*Minguo ziliao*), and others should become available to scholars now that access to Chinese collections is easier. Use of these local sources may present the necessity for major reinterpretations of the period.

WARLORD STUDIES

The recognition of the critical nature of the warlord period has led a considerable number of scholars into the field, working from a number of different view points. The development of the field has been haphazard and far from homogeneous. No single scholar has created or defined the field, as Mary Wright did for post-Taiping Imperial China, or Fairbank for the study of foreign penetration. Jerome Ch'en has done important work, but his synthetic work on the period is only now being published (Ch'en, 1979). Lucien Pye has produced a macrolevel approach that has proved too sweeping for others to follow (Pye, 1971). Most work on the period has been done by fairly young scholars working under a number of different mentors (or none); the absence of "a top party person in authority" has encouraged diversity and originality but has also given the field a curiously diffuse tone.

There are several recurrent and erroneous tendencies which I should like to discuss before I come to look at specific types of approach. The first is a tendency to balkanize, to carve up and tactily subdivide the field, especially in terms of individual areas

of China. This tendency, which I call "my provincism," simply recapitulates the fragmentation of the period itself. A second tendency is one identified by Ezra Vogel as "right-adventurism," which attempts to impose Western analytic models on the fabric of Chinese society; though analytic models in themselves are valuable, those derived from a study of Western societies may be massively irrelevant. A third tendency, the most dangerous one, is a desire to reduce complexity to simplicity, to find common features and common characteristics of warlordism. This must be one of the fundamental aims of the student of the period, but where it takes precedence over the detailed and exhaustive study of the period, or where it is based on a single case study, it can only produce confusion rather than clarity.

These erroneous tendencies have not been fully exposed, partly because scholars in the field are gentle with each other in reviews and partly because they are the kind of tendencies which are a natural part of the development of a field, its growing pains, errors which have to be committed before they can be overcome.

I have chosen to look at the current state of the field by dividing the works written to date into six categories (some works will appear only in one, others in two or more):

- (1) The origins of warlordism
- (2) Military history
- (3) Warlord biography
- (4) Regional studies
- (5) Political science approaches
- (6) Socioeconomic approaches

As yet, there exists no single work which attempts to combine all or the bulk of these approaches, with the exception of Jerome Ch'en's forthcoming work. Nor is there anything like equal representation in all categories. Far more work has been done in categories 3-5 than in the others, and far more remains to be done in category 6 than in any other. This is where I would hope to see the greatest future development of the field.

THE ORIGINS OF WARLORDISM

Marc Bloch remarks that there is a strong tendency among historians to confuse origins with causes, to look for origins, to push origins as far back in time as possible, and then to assume that one has found the causes (Bloch, 1954: 29). That tendency has shown itself in the study of warlordism. In a large sense, the causes of warlordism are not hard to identify—they lie in the breakdown of the civil order and the appearance of contending military figures to fill the vacuum of power; the historian is not obliged to go back much before the end of the Qing in his identification of causes. But several have, in fact, wanted to go back further, to identify origins. What they are actually talking about is not a measured development toward warlordism, but either recurrent patterns within Chinese society or the development of military institutions which facilitated the later development of warlordism but did *not* cause it.

The identification of recurrent patterns can be dealt with fairly summarily. It involves reference to the various periods of division which recurred through traditional history, in which case warlordism becomes simply another cycle of Cathay, or (a slight variation only) a last fling of a recurrent feudal phenomenon in a “société immobile et bloquée” (Bouissou, 1974: 84-85). Wang Gungwu has neatly knocked such theories on the head by pointing to the fundamental differences between earlier periods of division, which were breakdowns of central *administration*, and the warlord period, which was marked by the novel phenomenon of a “disintegration of political authority” (Wang, 1968: 267). In other words, the context of twentieth-century warlordism was quite distinct from that of earlier periods of division.

As far as the study of military institutions is concerned, Franz Michael goes back to the regional armies raised during the Taiping Rebellion and traces then an irrevocable process in which Zeng Guofan enters one end of a tunnel and Wu Peifu pops out the other end (Michael, 1971).

Later writers have made less general claims. Richard Smith has shown the first faltering attempts to train a core of military professionals (Smith, 1978). Ralph Powell has looked at the

gradual transformation of China's standing armies, and especially at the rise of the New Army, giving particular attention to the period after the disastrous defeat of 1895 (Powell, 1955). Hatano Yoshihiro has described, with concise clarity, the rise of the new armies (Hatano, 1968). Ernest Young has examined the overall change in attitude toward the military as a career (Young, 1970: 160). After 1895, the emphasis in historical studies is on Yuan Shikai and his Beiyang Army; Odoric Wou describes Yuan's organizational skills and his ability to corral funds (Wou, 1978). MacKinnon discusses Yuan's development of an officer corps, which emerges not as a personal, protowarlord clique, but as a group of men whose relationship with Yuan was essentially *quid pro quo* (MacKinnon, 1973).

From the late Qing development of military institutions and of military leaders, which created a climate in which the military—but *not* militarism—became a respectable institution, we move to the Xinhai Revolution and the emergence of Yuan Shikai as hegemon of China. To Yuan has usually been ascribed the discreditable role of the father of warlordism, ousting the father of the Republic. To quote Jerome Ch'en, "Yuan's most powerful single legacy was the creation of a large number of warlords" (Ch'en, 1961: 214). Yuan's latest biographer, Ernest Young, gives a more generous interpretation. In his study, Yuan becomes a Chinese Eisenhower, a man who abandoned the military once he became president and cast himself in the mold of a civilian and a centralizer. Yuan comes across as a much maligned and misunderstood man whose main failing was that he was duped and betrayed by his lieutenants, who became warlords in defiance, rather than in pursuit of, his own designs (Young, 1977). I find this interpretation disingenuous, closer to whitewash than to revisionism. Ch'en's estimation of Yuan as a man "who can command our respect neither as a statesman nor as a man of integrity" seems much closer to the mark (Ch'en, 1961: 215). Though Yuan cannot be held responsible for the actions of his former subordinates, he can be held responsible for fostering an atmosphere and a political-military system in which their transfer from generals in a centralized army to independent warlords was as smooth as tobogganing down a hill.

The critical permissive factor in the sudden blossoming of warlordism after Yuan's death was the militarization of China, fostered by Yuan at the top and by countless others at lower levels of Chinese society. It is at these lower levels that work remains to be done. The growing instability of Chinese rural society fostered the growth of local militias, of self-protective units raised by local gentry and officials, of irregular armies (*minjun*), and of bandit gangs. This process was strongly underway before the demise of the Qing, so strongly in fact that local power bases with a military backing, which had previously been rare, became a widespread phenomenon. This process involved the detachment of increasingly large numbers of men—on a permanent or temporary basis—from civilian society, the body of men who were to become the soldiers of the warlord armies. It is in this process, which grew out of the social and political fragmentation of China, that we must look for the breeding ground of warlordism, for the context which made its rampant growth possible.

MILITARY HISTORY

Military history would seem to be an obvious subspecies of historical study for discussing warlordism. If one thinks of military history as the study of officer corps, of technology and logistics, of military training and skills, or strategy and tactics, as a branch of history which draws widely on social science disciplines, this would indeed be the case. But this is not what military history has been until very recently. Military history has been dominated by those who describe specific campaigns and battles, by those whose desire has been to transfer the smoke and blood of the battlefield onto paper. Its practitioners have often been retired professional soldiers, or academics who have accepted their definition of what military history is all about. Though this type of writing has a huge market among the devotees of warfare, it is not much use as an explanatory or analytic technique. This type of military history, with some refinement in terms of the discussion of military institutions, is

represented in the study of Chinese warlordism by the work of Ralph Powell (Powell, 1955) and F. F. Liu (Liu, 1956), and by a large number of contemporary accounts which now have the flavor of period pieces, telling us more about the cast of mind of Western military gentlemen than of warlordism itself. This approach also appears in Donald Jordan's (1976) account of the Northern Expedition, which concentrates strictly on campaigns and victories, even at the expense of derring-do and blood and thunder.

Work in the first type of military history outlined above is now developing strongly for other areas of the world, but not yet for China. Historians such as Michael Howard (1957, 1976) and sociologists such as Morris Janowitz (1959, 1974, 1975) are only the leaders in a field where fascinating work is being done—by M.R.D. Foot on war and society (1973), by Jacques van Doorn on the military and social change (1975), by J. M. Winter on warfare and the economy (1975), and by Martin van Crefeld on logistics (1977). These works and many others suggest interesting ways in which warlordism can be examined from new perspectives. The only scholar of warlordism who has yet moved seriously into this field is Chi Hsi-sheng (1976), whose chapters on the structures of military life, recruitment, training, finance, and armaments all have valuable perceptions, though they are of more use as starting points for further research than as definitive statements.

What the field of warlord studies needs now is a number of studies on such aspects as: strategy and tactics (were warlords really as incompetent in these areas as they are often assumed to be?); on military technology and the impact that developments in this area had on China's overall technological growth (in other societies, military technology often paves the way for other forms of technological innovation); on arms procurement and the arms trade (Anthony Chan is just completing a dissertation on this subject at York); on soldier recruitment and training (my own area of research); on army finance; and on a host of other subjects which link the military world to the rest of Chinese society. It seems to me that an understanding of these aspects of military life is essential for an overall understanding of warlordism.

WARLORD BIOGRAPHY

History as biography has come to be viewed as a rather passé form of history, unless it is psychobiography—and even that once trendy approach seems to have faltered badly, once historians realized that it must remain the province of psychologists and psychoanalysts. (In Canada, no historian has dared to apply the technique to an obviously “different” former prime minister, Mackenzie King, who kissed the marble bust of his deceased mother every day on return from government business, and consulted her regularly on affairs of state through séances.) Biography is regarded as the activity of old-fashioned historians, of filial sons, and of limited minds who cannot rise to the analytic heights of models and structures. A stress on individuals is even equated with ‘psychological reductionism’ (Hall, 1976: 3).

And yet for warlordism, biography remains one of the most useful approaches, one which has produced much of the best work in the field. The whole warlord period reeks of personalities, larger-than-life figures, deviants, charlatans, braggarts, who, given sufficient ambition, talent, and luck, could pursue careers which would have been unthinkable in more stable periods of Chinese history. There is a great tradition in Chinese culture of such characters as Cao Cao and Zhuge Liang, both of whom (as warlords) displayed sufficient cunning, stratagem, and daring to land them solidly in folk tradition. Many warlords were not averse to identifying themselves with this tradition, seeking a historical respectability more plausible than an identification with the Confucian tradition.

The demise of the traditional systems of advancement with the end of the Qing, the disappearance of clear-cut career patterns, and the delay before new systems became firmly entrenched made it inevitable that men who might otherwise be outsiders or criminals should rise to high positions; men whose only common characteristic was that they were in some way exceptional.

Few warlords were drab or faceless men, and it should therefore be impossible to portray them as such. But those who write on warlords—especially those who treat them collectively

rather than as individuals—have been hesitant about describing the color, bombast, and idiosyncracies of their characters. The hesitation stems in part from a fear of being unacademic and journalistic, and in part from a fear of obscuring the essential venality and cruelty of warlords by focusing on their picturesque aspects.

There is also, I think, a larger concern that the concentration on an individual warlord, with all his quirks and foibles, may prevent the scholar from establishing common characteristics, from standardizing warlords. This seems to be an unreasonable concern. One cannot look at warlordism without being aware of the immense importance of individual characteristics, and of the great variety in which they presented themselves. The warlord period was not one in which a straightforwardly competent, well-trained soldier could rise to great heights; while one may attempt to describe a typical Confucian bureaucrat or a typical Communist cadre, in a period as fluid and unstable as warlordism one finds oneself looking at men from a wide range of backgrounds, with a wide range of training, talents, and abilities. Each of them owed his rise in part to particular personal capacities for panache, bravado, scheming, and stratagem, without which they would have remained other warlords' subordinates. These capacities are at least as important as their personal ties, their regional origins or their educational experience in explaining their prominence.

It seems to me then that the biographical approach is a major entrée to an understanding of warlordism, and it is works of this nature which are the best studies of warlordism to date—James Sheridan's (1960) book on Feng Yuxiang and Gavan McCormack's (1977) work on Zhang Zuolin. Sheridan looks at the most colorful and most bombastic of all the warlords, a towering giant who, though ruthless and brutal when he felt the need, was also capable of capturing the imagination of both Chinese and foreigners with his flamboyance and histrionics. Through its focus on the career of this extraordinary individual, who could only have existed in the warlord period (in any other period he would have been a sure candidate for execution), Sheridan's book captures the atmosphere and flavor of the times. McCormack describes the

improbable career of Zhang Zuolin, a man who rose from the dregs of Chinese society to occupy a throne of his own design and to lord it over a vast, rich area of China, running rings round all who crossed his path, until the exasperated Japanese, tired of being outsmarted by a mere Chinese, blew him to kingdom come.

Within the continuum of warlords, Feng Yuxiang stands near the better end; the lower end is occupied by such crude brutes as Zhang Zongchang (not yet the subject of a biography) and the sordid characters who dominated hapless Sichuan—the stubby melon, the two-headed snake, and the rotten pig (Kapp, 1973). Slightly less squalid are those who managed to establish stable regional governments but were still devoid of refinement or any but the most pragmatic approach to rule, men such as Zhang Zuolin, “a man unmarked by modern ideological considerations” (McCormack, 1977: 253). Yan Xishan, whose long rule over Shanxi provided some element of continuity for that province, occupies a position on the spectrum somewhat more elevated than Zhang, since he did attempt to introduce a modicum of good government to his province (Gillin, 1967). Yan started his career as a warlord very early, as did some of the other more distinguished warlords—such as Chen Jiongming and the early Yunnanese warlords (Hsieh, 1962; Sutton, 1979). There seems to have been a marked decline in the caliber of warlords as the period wore on, not a decline from a very high starting point, but one whose consequences for the people of China were grim (Kapp, 1973; Sutton, 1979).

The biographical study of warlords to date has been very patchy; there are numbers of important warlords who still remain shadowy, cardboard figures (for example, Duan Qirui, Cao Kun, Zhang Xun, Feng Kuochang, Zhang Congchang, Sun Chuanfang), in fact, many of the major northern warlords. While it might seem exaggerated to ask for studies of these figures, it should be noted that most of the biographical studies we now have deal with individuals whose location was peripheral, whose role was less central than some of those who have been ignored.

Whatever the value of biographical approaches, there are also drawbacks. First, there is the danger of overidentification with

one's subject, less of a danger for Western civilians working on Chinese militarists than it was for Isaac Deutscher working on Lenin, where one had the impression that Deutscher had actually come to see himself as Lenin, down to the goatee. It is also difficult to write hagiography about individuals who clearly do not merit paeans of praise. Where the tendency to overvalue one's own subject creeps in is with the desire to measure him against others of his ilk. Sutton has done this to some extent with his study of the early Yunnan warlords (1979) as has Lary with her study of the Guangxi Clique (1975). McCormack does it quite explicitly when he refers to Zhang Zuolin as "the greatest of that strange group of men known as warlords" (McCormack, 1977: 1). We should remember that although a skunk smells worse than a polecat, they both stink.

A second drawback of biographical studies is a tendency to see the period of warlordism through the eyes of one's own protagonist, a natural tendency, since much of the documentation comes from sources directly related to him and even from him himself (see Gillin on Yan Xishan). This tendency may lead one to give a much greater importance than is deserved to the figure under study, to place him in a position which *he* may have well felt he merited, but which is not deserved.

I have paid little attention here to autobiographical accounts, though several exist, notably in the Columbia Oral History Project. The reason is that—with the exception of Zhu De's account of his life as told to Agnes Smedley (1956)—these are characterized by a self-serving pomposity which makes them useful only to establish points of detail, not to give a well-rounded or objective picture of the period.

As far as future work is concerned, it would be ludicrous to suggest biographical treatment for every warlord who flitted across the Chinese stage—though such work would be taken for granted in English or American history. What we need now is a few more representative studies, certainly one of a major northern warlord in addition to Odoric Wou's useful study of Wu Peifu (1970), and also one of a minor warlord, someone such as Zhao Hengdi, who, though not a man of outstanding significance,

appears again and again in the history of Hunan and of Central China, allying now with this clique and now with another, showing great capacities for dealing, double dealing, and tenacity. A study of a strictly mercenary warlord would also be most valuable, for example, a man such as Fan Shisheng, who popped up all over Southwest China for more than a decade, whose survival itself, if analyzed, might tell us a great deal about the mechanics of warlordism.

REGIONAL STUDIES

Central power and authority disappeared during the warlord period; China was divided into chunks of varying and changing size. Individual warlords or groups of warlords held regional control and ruled over their satrapies as autonomously as they could. A number of scholars have chosen to approach the study of warlordism by looking at one of these individual satrapies, at a regional base: Gillin on Shanxi (1967), Sutton and Hall on Yunnan (Sutton, 1979; Hall, 1976), Kapp on Sichuan (1973), McCormack and Suleski on Manchuria (McCormack, 1977; Suleski, 1974), and Lary on Guangxi (1975). With the exception of Manchuria, all the regions studied are provinces on the periphery of China, each of which was a geographically self-contained and clearly demarcated entity; Manchuria, with three provinces, was an equally distinct entity. These regions were clearly defined as such before the advent of warlordism.

But this is not the case with all the provinces of China; in Central China and on the North China Plain, provinces are less clearly demarcated from each other than on the periphery, and in these areas, during the warlord period, the phenomenon of provincial warlordism was less marked. It is much more difficult to equate the Anhui or Zhili Cliques with a region than it is the Guangxi Clique; to ascribe a specific regional setting to a warlord as peripatetic as Feng Yuxiang is impossible.

The province, then, cannot be equated with the region in terms of warlord regional power. Sometimes the warlord held power

over a multiprovince block. Sometimes he moved frequently, always having a base from which to draw revenue, but never controlling a region permanently. Sometimes he dominated only a small area, a part of a province, a garrison area. There are obviously problems involved in looking at regionalism, not the least of which concerns the size and nature of the region. The work of definition to date has been very haphazard, and far more needs to be done, to identify supraprovincial regions, provincial regions, and intraprovincial local regions.

Many of those who have embarked on regional studies have chosen to ignore these questions of definition, assuming that in their case at least, the province was the region. This assumption is not unwarranted, given that the holders of power in the provinces studied made this assumption themselves, and on the basis of it fostered provincial ties to serve their own ends. Chen Jiongmeng, for example, in Guangdong, advocated a regionalism so strong that it amounted to local patriotism (Hsieh, 1962). This kind of intense provincial regionalism predated the rise of warlordism—see, for example, Esherick's work on Hunan and Hubei (1976) and Lewis's study of Hunan (1976)—and was strong enough to fuel the Federalist Movement of the early 1920s (Chesneaux, 1969).

But we must not be misled by the power of provincial warlordism into assuming that it was a universal phenomenon, nor indeed that provincial loyalties were equally strong throughout China, or shared by the whole population of a province. In many cases, provincial loyalties were the preserve of a provincial elite that drew on a strong regional tradition—Hunan is a clear case of this. But there is no necessary correlation between the strength of this provincial sentiment and the ability of a warlord to manipulate it. Hunan passed through the hands of a long succession of warlords, some of whom were Hunanese, some of whom were not, some of whom ruled the whole province, some of whom ruled only a corner.

Individual regional studies reveal a great deal of variety. Gillin's study of Yan Xishan and Shanxi shows the kind of tenacity and flexibility needed to hold on to a province for more

than thirty years, the longest single warlord rule. But it also shows that Shanxi was far from the model province some commentators made it out to be; its only advantage during the warlord period was that it was spared much of the brutality which accompanied changeovers in warlord rule elsewhere.

Sutton's and Hall's studies of Yunnan look at the same province, in sequence, but with very different approaches. Sutton's work (1979) is a meticulous study of provincial militarism and deals with the rise and fall of a fine military force. In great detail and with great clarity, he charts the internal mechanisms of a regional military system; his work reveals more clearly than any other study the internal dynamics of a provincial army. Hall's study (1976) is more ambitious, in that he attempts to examine the social and economic aspects of warlordism as well as its military aspects, but given its brevity, the treatment is superficial.

The two studies of Manchuria also take different approaches. Suleski (1974) is concerned principally with the civilian administration subordinate to Zhang Zuolin, while McCormack looks at a much larger picture encompassing all of Zhang's activities, both within Manchuria and inside the Pass (1977). Lary's principal focus in her study of Guangxi (1975) is on the activities of regional warlords, and she only focuses specifically on the province when that was the principal focus of the warlords themselves.

Pause's study of regionalism (1978) is not concerned with a specific region at all, but rather with those military figures who were in the Guomindang after the Northern Expedition but not of it. In a treatment which is strictly chronological and descriptive, he discusses the ways in which Nanjing and Jiang Jieshi dealt with the unruly generals, the practitioners of what Sheridan describes as "residual warlordism." Military regionalism is both a cause and a product of weak central government.

My comments above should indicate that the notion of a regional approach can be deceptive. Although the scholar is apparently dealing with a nicely defined entity, the approach determines what aspects of that region will be looked at. One may

only be dealing with warlordism in a regional contest, rather than with a region in all its aspects.

What is needed now is a different kind of regional study, not the study of a provincial region but of a local region, a town or locality which experienced warlordism in an acute form, by the ill fortune of being a locality over which warlords regularly fought, or a town in which troops were regularly stationed. Many such localities can be identified: Xuzhou, Luoyang, and other key centers on the North China railway network; Chengdu, Changsha, and other provincial capitals, which changed hands repeatedly; the Huaibei region and the Guangdong North River Valley, which were subjected to far more than their quota of military activity. Such a regional study would approach warlordism not from the point of view of the participants, but from the point of view of those who lived under it, those few who benefited from warlordism and the many who suffered.

The element that would be critical in this kind of study is the role of the middlemen of warlordism, the permanent local powerholders, those members of the gentry and of the *shanghui* who facilitated warlordism, who collected taxes for warlords, raised money for them, carried out requisitions for them, recruited troops for them. Without their support, the survival of warlordism would have been impossible.

Some scholars have already tackled this relationship between the gentry, *shanghui*, and the military. Bouissou, in a very general treatment devoid of substantiation, refers to warlords simply as the “bras armées” of the gentry and the merchants (Bouissou, 1974: 159). Jerome Ch'en, in a much more sophisticated and detailed treatment, examines several distinct aspects of the warlord/gentry relationship (1979). What remains to be done is a study of how these relationships evolved and solidified over a long period of time in a specific context. Hall promises to undertake such a task for Yunnan, but in fact only devotes two pages of his book to the question (1976: 63-4). We now need a study of a small region in which the mechanisms of the relationship and the decisions which lie behind them can be revealed.

We also need a study of a supraprovincial region (such as the North China Plain or the region centered on Wuhan), which might deal with the strategic and economic factors involved in the control of such a region.

POLITICAL SCIENCES APPROACHES

The works discussed above are almost all the works of historians. But history is not the only discipline represented in warlord studies; much work has been done by political scientists, whose aim is to bring to the field approaches which are more analytic and more systematic than those of historians. For a period as chaotic as the warlord period, there is often an impression that all the historian can do is reproduce that chaos. The aim of the political scientist is to pummel that chaos into order, usually by providing an overarching model or an explanatory construct that will reveal the underlying patterns of warlordism.

Many such models have been used. Nathan has defined the activities of those warlords associated with Beijing in terms of factionalism and has examined the structure, composition, and workings of warlord factions (1975). Wou has posited a parallel between the internal working of warlordism and the Chinese clan system, using a model of fictive kinship and the Krech-Crutchfield-Ballachy star-shaped model of authority (1970). Chi Hsi-sheng (1976) has gone to the other end of the spectrum in terms of population groupings and has used concepts from international relations theory to study warlordism; China under the warlords thus becomes an international system (Chi, 1976). (Chi also makes use of the Krech et al. model of authority.)

These models are intrinsically attractive. Nathan's in particular, which is crystalline in its clarity, gives a real impression of having laid bare the underlying patterns of warlordism. But a nagging question assails the non-political scientist: which came first—the model or the empirical research? One fears it is sometimes the former—that political scientists, committed by

their discipline to the proposition that underlying patterns do exist and therefore can be found, sift through the store of models which have been developed by Western political science and apply the one which seems to fit most neatly the phenomenon they have chosen to study. Where the model takes precedence over the historical reality, one sees Ezra Vogel's "right adventurism," a deviation from reality in favor of abstract order.

Another question which is raised by the model-building approach is the scope of applicability. Most of the work of model building is set in the context of northern warlordism, of the great cliques of North China. The factional model may be entirely appropriate for the Anhui, Zhili, and Fengtian Cliques; Wu Peifu may indeed have run his clique on the basis of fictive kinship ties. But when one attempts to apply these models in other contexts, they do not fit, or they fit so imperfectly that real liberties have to be taken with historical truth in order to force recalcitrant facts into molds not made for them.

The desire for clarity, for imposing order on confusion espoused by the model builders is a laudable one. Anyone who has suffered the pain of reading Tao Zhuyin's monumental study (1957-1958) of the Beiyang warlords, an eight-volume string of names and details, will know how little illumination comes from a recitation of "what actually happened." The problem is to prevent the pendulum from swinging too far in the other direction, to the point at which reducing complexity to simplicity becomes reductionism, an assumption that all complexity has at its roots simple, clear-cut structures susceptible to presentation in model forms. This danger is easy to recognize when one is presented with crude class categories, but much more difficult when the model building is in the hands of sophisticated political scientists.

None of the model builders shows complete security in the use of models. Nathan's book divides neatly into two halves, both of which are very well done, but the relationship between the empirical half and the theoretical half is not as clear as the two halves are individually. The Fengtian Clique in practice looks closer to the ramshackle, makeshift, and heterogenous outfit described by McCormack (1977) than it does to Nathan's

immaculate model. Chi Hsi-sheng's descriptive chapters on training, recruitment, weapons, and so forth are strong and suggestive, but they do not relate clearly to the model of international systems, which is tacked on rather lamely and without great reference to the earlier, empirical sections (Chi, 1976). The same is true of Wou's treatment of Wu Peifu, which is excellent in detail, especially on finance and local administration (Wou, 1970: 159-186), but contrived and artificial when it comes to seeing Wu as a clan elder, presiding over a collection of stems and branches (Wou, 1970: 203-218).

I have left to the last a mention of Pye's sweeping theoretical treatment of warlordism, which is most bold in terms of generating an overarching theory and most limited in terms of relating theory to fact. Pye is not a model builder, but an architect of theory. His theory is that warlordism was an essentially positive phenomenon, one which led China into healthy competition, away from an unhealthy, closed, and autocratic system of the kind that preceded and followed the warlord period (Pye, 1971). Pye sees the warlords as "Chinese pathbreakers," men whose original intention of introducing China (for its own good) to competitive politics was botched by their inability to match their style of behavior to their real objectives. I find this theory so abstract, and so little concerned with the anguish and suffering generated by warlordism, as to be rather insulting. No Chinese who lived through the warlord period could take such a detached view.

One approach which has only been touched upon by political scientists, but which seems to me to contain potential merit, is the study of the political behavior of warlords, of the "rules of the game" of warlordism. These rules were not spelled out, but they clearly existed: there were implicit rules of dress (only broken by nature's gentlemen such as Feng Yuxiang), of communication, of recruitment, and alliance formation; there were rituals of warfare and combat; there were understood modes of dealing with defeated enemies (warlords very seldom killed each other off, but allowed the defeated party to retire to the comfort of a foreign concession or Buddhist monastery). Jerome Ch'en has touched

on some of these aspects of warlord behavior (1968), but more by way of composing checklists and defining categories than by delineating the rules of the game. Chi Hsi-sheng starts to get into this approach when dealing with the “normative aspects” of warlord behavior (Chi, 1976: 181-189), but gets sidetracked into an examination of Northern and Southern patterns. In other fields, this approach of looking at political behavior rather than at political structure has proved of great value—I am thinking of the studies of mass behavior for eighteenth and nineteenth-century England and France—and could surely be tried for China with great benefit.

SOCIOECONOMIC APPROACHES

The last category of warlord studies which I define is the least well represented in the current literature, that is, those studies which deal with the relationship of warlordism to Chinese society and to its economy.

Few segments of Chinese society were left untouched by warlordism. I have already mentioned the relationship between the gentry and warlordism in speaking of regional studies. There I spoke of the gentry without internal definition. What we need to know—and what Fei Xiaotong’s field work in Yunnan began to investigate (1953)—is: what process of subdivision went on during the warlord period in a previously well-defined sector of Chinese society; which of the members of the gentry moved to the cities, and why; what directions did those who remained in the rural areas take. Once some work of definition has been done, some of the roles of gentry members in relation to warlordism will become clearer. There are fascinating questions to look at here: the extractive function of gentry as agents of warlords, the gentry role in supplying officers and common soldiers to warlords, the gentry role in local militarization, the raising of militia and irregular forces, the gentry role in local government. Many of these questions have been touched upon in existing works, but few have been studied systematically.

Turning from the top to the bottom of rural society, another segment which has received little specific study—especially in its relationship to warlordism—is that of the bandits, whose rapid multiplication in the warlord period was noted grimly by all contemporary commentators. Banditry was intimately linked to the military; ex-soldiers became bandits and ex-bandits became soldiers, the one phenomenon fed off the other (Billingsley, 1974; Perry, 1979), but much more remains to be done.

A third segment of Chinese society which deserves greater attention in its relation to warlordism is that of the civilian bureaucracy, at the capital level as well as at the provincial and local levels. The fate of the old-style bureaucrats is intriguing; the reemergence of long-gowned figures to serve the Japanese suggests that their disappearance was less rapid and less absolute than might be assumed. For many, the relationship with warlordism was intimate—they formed what were in effect the *mufu* of the warlords, composed warlord communications in *guwen* (to the sorrow of students of the period), and gave a touch of Confucian class to army headquarters which might otherwise have been boorish barracks. One also comes across much more modern-minded bureaucrats in the ministries of Beijing and in the employ of individual warlords—Zhang Zuolin used the talents of Wang Yongjiang (Suleski, 1974) and Chen Jiongming those of Chen Duxiu (Hsieh, 1962). One also sees apparent progressive civil administrators in Guangxi and in Shanxi, though Gillin (1967) and Lary (1975) have overestimated the degree of good government they were able to engender (principally because in comparison to anywhere else in China those provinces looked so good). What is needed now is some more penetrating study of the relationship between warlords and their civilian employees, and of the degree to which the warlords actually controlled the civilian world. Though we assume that many warlords were all powerful in the areas they held, little work has actually been done on the extent to which they may have been circumscribed by local gentry and mercantile power. This seems to me a very important area of enquiry. Bureaucratism in China has been identified—both before and after the warlord period—as one of the major

impediments to innovation and to decisive government; the inertia and caution of bureaucrats caused difficulties both at national and local levels. It would be strange if such an ancient and all-pervasive phenomenon went completely into abeyance during the warlord period.

Surprisingly little comment has been made on the class affiliations of warlords other than in terms of blanket epithets—"feudal remnants, running dogs of imperialism." This may be because the approach is too daunting. Warlords did not share a common class origin, nor did they display a common class position in terms of ideological stance. Among them were convinced conservatives (Wu Peifu), judicious eclectics (Yan Xishan), the ideologically poor and blank (Zhang Zuolin), wavering Christians (Feng Yuxiang), and mild progressives (Chen Jiongming). How one would chart clear patterns of class affiliation and interest through this ideological morass defeats imagination.

The economic impact of warlordism is an even more complex area than the social. We have an excellent study of warlord finance (Wou, 1970) and many partial treatments in larger studies (Gillin, 1967; Sheridan 1966; Hall, 1976; McCormack, 1977; Nathan, 1975). We also have general studies of the economy of Republican China, but little work has been done on the effects of warlordism on the economy of China in terms of disruption, extraction, and retardation. The first and third of these areas are hard to handle, since they involve looking at what did not happen rather than what did. I am not a devotee of counterfactual approaches to history; it should, nevertheless, be possible to give some indications of the extent to which warlordism deterred economic development in China and limited modernization to those areas under direct foreign control. The second area is more susceptible of study, and indeed much work has already been done. The milking of local economies has been admirably discussed by Sheridan and by Kapp, but far more detailed source material exists than has yet been tapped, and much more work remains to be done, not only on specific economic depredations, but also on the psychological climate produced by the fear, and

the reality of deprivations which reduced the level and the quality of economic activity and turned men of property into people whose main concern was to husband and guard their resources rather than to invest them in risky undertakings.

Another aspect of warlord involvement, which others regard as essentially a political question, but I see much more as an economic question, concerns warlord relations with the foreign powers. The notion that warlords were simply the running dogs of imperialism has never been specifically proven, and it has in some cases been explicitly debunked (McCormack, 1977), but the economic relationship between warlordism and imperialism was very real. Foreign sources supplied much of the munitions which fueled Chinese warfare, foreign banks provided loans and safe havens for warlord gains, foreign aides (usually of a distinctly shifty nature) provided contacts and purchasing services for warlords. The complex web of arms deals, loans, and financial services which foreigners provided is hard to penetrate; the source material is patchy and often nonexistent, since few of the participants felt the need or desirability of keeping good records of their shady activities. But a study of these areas of economic cooperation, however partial, will reveal the real dimensions of the warlord-imperialist relationship.

Indirectly, the role of warlordism in weakening China's ability to defend her economic independence is also of great significance, though at the same time warlordism helped to make the conditions of trade and investment in China so precarious as to be inimical to foreign economic penetration. The one nation which was not deterred was Japan, which used China's manifest weakness and instability to strengthen its own hold over the country, first through economic inroads and then through military aggression. Warlordism gave to Japan the critical psychological encouragement of believing that it was looking at an utterly decadent and incompetent country.

FUTURE DIRECTIONS

I have indicated as I have gone along some of the areas in which I feel further work is needed, not with the idea of criticizing the

inadequacies of people already working in the field, but with the intention of showing what remains to be done. We seem now to be in a period which is favorable to new endeavors. Access to sources in China is opening up significantly, and the study of Republican China seems not to be a sensitive area. We also have the benefit of recent work in other fields, where very suggestive methodologies have been developed (which cannot be applied directly to the Chinese context, but which may well offer new ways of looking at questions). This is especially true of the fields of military history, of political behavior, and of social and economic history.

Let me recapitulate by listing some of the areas which will benefit from further study:

- (1) Local militarization, especially in the early years of the Republic; the transformation of rural China from an essentially civil society into one in which military and paramilitary forces were omnipresent.
- (2) Warlord strategy and tactics, in terms of innovation and of differential levels of skill.
- (3) Military technology and logistics, both in terms of its use by warlords and in terms of its impact on China's economic development.
- (4) Soldier recruitment and training, in terms of the creation of a new stratum of Chinese society.
- (5) Biographical studies of some of the types of warlords who have so far escaped the scholarly searchlight—a major Northern warlord, a minor warlord, and a wandering warlord.
- (6) Regional studies, both at the micro and the macro level; the first in terms of local power relations between the military and the gentry, the second in terms of the economic and political systems of a large, supraprovincial region.
- (7) A study of the political behavior of warlords, of the rules of the games of warlordism, and of the rituals of warfare.
- (8) Work on the nature and composition of the post-Imperial gentry, of its division into several strands, and of the relationships between these strands.
- (9) Work on the relationship between gentry and the military, and on the commonality (or otherwise) of interest between the two groups.
- (10) Work on banditry and its relationship to the military and to local power holders.

- (11) A study of the civilian arm of warlordism, and of the fate of bureaucratic government during the warlord period.
- (12) An economic study of the disruption to and economic retardation of the Chinese economy at the hands of warlords.
- (13) More detailed study of the direct impact of warlordism on the Chinese economy, in terms of deprivations and damage.
- (14) Work on the role of imperialism in relation to warlordism, not in terms of diplomatic history, but in terms of economic cooperation.

These are some of the directions I hope the study of warlordism might take in the future. Whatever the specific direction, all will, I hope, show the profound impact of warlordism on Chinese society and will show that warlordism must be seen not as a ghastly aberration, but as a phenomenon which exerted a deep influence on postwarlord China. The warlord stranglehold on the military, the politics, and the economy of China demanded that its opponents fight it on its own terms, with military might. In doing so, they ran the danger of perpetuating the abuses of warlordism. The number of paths for the regeneration of China was starkly reduced. Purely political and ideological solutions were reduced to impotency; transformation through economic or educational change became irrelevant. Military solutions and militarization became a permanent part of the fabric of Chinese society, one which even the Communists have not entirely reversed. The warlord legacy was a grim and a heavy one. China's dark night of warlordism did not give way to a new golden dawn, but to an overcast day.

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