

HISTORICAL PERSPECTIVE

ON THE MUSLIM ARMED STRUGGLE?

(Critical Review of Samuel Tan's *The Filipino Muslim Armed Struggle, 1900-1972*.)

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Samuel K. Tan's *The Filipino Muslim Armed Struggle, 1900-1972* (Filipinas Foundation, 1977) addresses a topic of broad academic and public concern. Armed conflict in the southern Philippines has cost thousands of lives, created hundreds of thousands of refugees, drained the Philippine economy, hampered development efforts, complicated international relations, and contributed to the continuation of martial law. It is a factor in nearly every aspect of Philippine national life. The appearance of Dr. Tan's book raised many hopes. He appears uniquely qualified to shed new light on this subject. He is of Tausug-Samal-Chinese ancestry, was born in Siasi, Sulu, raised in the southern Philippines, and is presumably familiar with many aspects of the Muslim situation through intimate personal acquaintance. He studied aspects of Philippine Muslim history for both his master's and doctoral degrees. He collected Philippine archival materials for his thesis on American military rule in Sulu while studying at the University of the Philippines, and had wide access to American archives while studying for his Ph.D. at Syracuse University. He has published articles on several aspects of the Muslim struggle. He has a prestigious academic position, and his book is published by an equally prestigious private foundation. This book might have been expected to be a culmination of Dr. Tan's career to this point, as well as a thorough and authoritative history of the armed struggle. Instead, it is a disappointment. While Dr. Tan assembled information on a large number of incidents, his book adds little to understanding of the subject. In addition it contains numerous errors of fact and documentation.

This volume is the third Filipinas Foundation publication concerning Philippine Muslims, and it is a logical companion to its predecessors.

The first, *An Anatomy of Philippine Muslim Affairs* (1970), presented a general view of Muslims in the Philippines from a broadly sociological perspective. Aside from summarizing the available literature on the subject, this study presented survey results on socioeconomic characteristics and attitudes in Muslim areas, and analysis of the prospects for national integration. The second volume, *Philippine Majority-Minority Relations and Ethnic Attitudes* (1975), included Muslims in a general survey of Philippine ethnic attitudes. This study centered on ethnic stereotypes and invidious attitudes, offering comparative measures of intergroup distance and prejudice. On all dimensions, according to this study, Muslims are uniformly viewed as the least desirable Philippine group by all other groups.

Historical analysis of the Muslim situation is a logical successor to the earlier sociological and socio-psychological studies. Dr. Tan's book was apparently intended to fill this role. In his introductory comments he notes the lack of "a comprehensive Muslim historiography" and the need for "an integrative study of the Filipino Muslim armed struggle." He offers his book as an attempt to fill these gaps in the literature and writes that, "It offers a basis for understanding disturbances in Mindanao and Sulu and their relationship to national policies and programs."¹ The idea is to detect patterns in the Muslim disturbances and trace continuities between past movements and recent ones, the better to understand "Muslim conflicts and Muslim aspirations."

To accomplish these goals Tan describes, usually quite sketchily, a large number of incidents of armed conflict. Some forty-one are listed in an appendicized table. We may thank Tan for providing some information on a larger number of incidents than were heretofore available in a single volume. We may also be grateful for the assembling of interesting material on Kamlun's rebellion in Sulu, and for his inclusion as appendices of several interesting documents reflecting American views of Muslims during the colonial period. The resulting historiography, however, is not comprehensive, and Tan's attempt to provide an integrative study of the armed struggle cannot be reckoned very successful.

Tan's study comes in two parts. The first and largest comprises his doctoral dissertation, almost entirely unrevised except for some

retitling of chapters. This section covers the period 1900-1941 and centers on a catalogue of Muslim armed disturbances. They are classified as anti-American, anti-Christian, anti-government, and inter-Muslim. After parcelling out incidents among these categories, Tan sets out broad patterns and trends he finds in these disturbances and their relations to government policies.

Added to this first part are several new chapters. Recent developments in the armed struggle are presented in a narrative fashion, abandoning the categories of the earlier discussion. Tan also briefly reviews the literature on Muslims in the Philippines, discusses the "historical consciousness" of Tausugs, and offers recommendations for government policies toward Muslims.

To satisfy his goal of understanding Muslim conflicts, Tan adopts a method he attributes to the Arab historian Ibn Khadlun. According to this method, one proceeds from study of external data to an understanding of the rational structure which underlies them. According to Tan, "External data should, therefore, be collected and classified and then the causes and nature ascertained." In applying this method, the choice of criteria and categories for classifying data will strongly influence, if not determine, any assessment of causes and nature. The categories embody the rational structure the researcher is striving to discover. The method itself seems questionable, as historical material does not usually lend itself to taxonomic treatment, but Tan's choice of categories mitigates what effective use he might have made of it.

Tan's problem is one of oversimplification. He classifies all Muslim disturbances under the general causal motivations, "anti-American," "anti-Christian," "anti-government," and "inter-Muslim." No one would argue that these factors were not involved in armed disturbances, often all at the same time, but many others were involved, too. Tan immediately reduces all other causes to secondary status. The result is an overgeneral, counting the cases, type of analysis.

Tan's method forces him to oversimplify the motivations at work in the incidents he cites. He views anti-American feeling as a response to colonialism, "the American system of control which reduced the traditional leaders to virtual impotence (19)." He states it as a broad fact that, "the Muslim ruling classes resisted the enforcement of American

sovereignty (20).” Nevertheless, his accounts of the earliest anti-American incidents in Lanao (21) indicate considerable collaboration with American forces. This appears from the beginning of American military rule. Obviously there is more at work here than simply anti-Americanism. Similar difficulties are evident in Tan’s report of Datu Ali’s revolt in Cotabato, 1904-5. Having classified Datu Ali’s revolt as anti-American, Tan feels no need to elaborate on specific motivations. He writes simply that the revolts of Datu Ali and Datu Alamada were, “similar in motivation to the anti-Spanish revolts of Sultan Kudarat and Datu Utu (23).” Apparently all these broadly fall under the rubric of anti-colonialism. In his appendix, Tan lists the cause as US militarism. While this is at least partially correct, it neglects other important factors. Reynaldo Ileto, whose study of Datu Uto Tan cites approvingly later in the book (137), emphasizes the crucial importance of the slave trade of Uto’s position in Buayan (Ileto, 1971: 36-7), and recounts Spanish attempts to constrict it in Mindanao and Sulu through blockading of trade routes and garrisoning of trade centers such as Jolo and Sarangani Bay. It was largely the restrictions on slavery, one of the main sources and embodiments of his wealth and power, that led Datu Uto to revolt. Ileto (1971: 64) attributes a similar motive to Datu Ali, Uto’s successor as the most influential leader in Buayan. This analysis is corroborated by American reports of the period, and it is notable that Ali apparently got along well with American authorities until Gen. Leonard Wood determined to strictly enforce the anti-slavery policy in 1903. Tan’s description ignores these factors. It does, however, mention the rivalry between Ali and Datu Piang, an “inter-Muslim” element in this “anti-American” disturbance.

In a similar vein, Tan’s chapter on anti-Christian disturbances includes incidents proximately caused by the wounding of a man by a Muslim headman for allegedly having “an improper relationship with his wife (Palawan, 1911; 30),” and the appearance of a spirit seeking revenge for its owner’s death in a massacre by government troops (Pandak movement in Lanao, 1924; 32). These do not evince anti-Christian sentiment so much as responses to specific grievances. In fact, it is not clear in Tan’s accounts that any anti-Christian element was common to all the disturbances he lists under this category. What does link them all

is a "mystical" element, ranging in the different cases from the use of *anting-antings* to divine revelations. Mysticism, however, does not necessarily imply anti-Christianity.

Tan cites Acbara's 1923 rebellion on Pata Island as illustrative of "anti-Christian reaction in Sulu (30)." Among the factors to which he attributes the revolt are the cedula tax, compulsory education, land surveys, the influence of Hatib Sihaban (a religious leader), the sultan's detention of Acbara's wife because of Acbara's impoliteness to him during a visit, economic reverses in the Sulu-Bisayan trade, and belief in his divine mission. The divine mission was revealed in a command Acbara received to "drive all the Americans and Christians into the sea," and to "establish a government responsible only to heaven (30)." Clearly there are many motivating factors here besides an anti-Christian sentiment. There was anti-Americanism, rejection of the authority of the sultan, an attempt to purify or revitalize religious practice, and rejection of specific government practices and policies. Tan does not indicate if Acbara's attacks were directed against Christians. Their largest encounter was with a Constabulary detachment, and Acbara was finally killed by a Muslim councillor (31). It is simply not possible to adequately understand such a movement only in terms of religious sentiment.

The other two categories are equally objectionable. Many of the "inter-Muslim" conflicts hinged on objections to government policies enforced by Muslim leaders (e.g. the Sanda uprising in 1903; 47). Some incidents listed as "anti-government" involved large battles between opposing Muslim forces (e.g. the Battle of Pampang in Sulu, 1904; 38). Even Tan seems to sense the futility of trying to separate these incidents into monocausal categories. He writes that, "The various disturbances are difficult to categorize under one definite causal motivation (43)." The obvious reason is that there are many motivations in each.

Given Tan's own doubts, we may wonder why he used this approach. The payoff for the classification scheme appears to be a graph on page 55 which shows the number of disturbances by type during five-year intervals 1900-1940. From this graph Tan concludes that anti-American activity peaked during the the early years, and was transformed into anti-government activity after 1913. This transition is an

artifact of the classification scheme. Initially Tan equated anti-Americanism with anti-militarism. During the American military government, any resistance of government force was, by definition, anti-American. After 1913 and the abolition of the military government, even though many of the issues involved actually remained the same. The anti-American/anti-government distinction thus appears rather arbitrary. The supposed trends revealed by the different lines in the graph tell very little about changes in the Muslim situation. The disturbances could have been as easily categorized in some other way, and a graph of quite different shape would have emerged. The reader is left with the feeling that Tan's categories were chosen arbitrarily and with no clear rationale.

Another problem with a "sort and count" analysis lies in the selection of data for inclusion. Either the listing must be exhaustive or there must be some systematic method for deciding which items to include and which to omit. Tan admits that his listing is not exhaustive. His criteria are, "(1) treatment in official records, communications, and daily newspapers; (2) popular support and effect on society; (3) causal relation to past significant incidents; and (4) causal connections with established institutions." While these may appear reasonable, Tan's application of them results in some curious omissions. Among others he omits the Bayan expedition (April-May 1902) in which over 1200 American soldiers campaigned for the overt purpose of crushing armed resistance in Lanao and compelling cooperation by Muslim leaders. In the single Battle of Bayan, some 300-400 Lanao Muslims were killed while defending three *cotas* (Gowing, 1977: 84-8). By any standard, this armed encounter appears significant. Similarly, Tan includes Pershing's campaign against defiant residents in the vicinity of Taraca, Lanao in May 1903, but omits the Battle of Bacolod the preceding month in which 120 Muslims died in a two day artillery barrage and assault on the strongest *cota* in Lanao (Gowing, 1977: 90-1). Under inter-Muslims disturbances, Tan omits the dispute between the brothers Datu Calbi and Datu Jokanain and Sultan Kiram II of Sulu. While this rivalry had been simmering for some time, for much of 1901 and 1902 it threatened to engulf Jolo in open warfare, and contributed greatly to

outlawry and disorder on the island (see Majul, 1973: 309; Gowing, 1977: 80).

In some cases, Tan does attempt to show the connection between different incidents. He intimates a link between Datu Ali's revolt and that of Datu Alamada eight years later, though the character of the connection is unclear. Pala, a Sulu slave trader, is said to have carried on Hassan's revolt, because he admired and supported it (39). However, Pala also had compelling reasons of his own for rebelling. Tan gives the background to Tahil's revolt in Sulu in 1927. However, most of the disturbances are treated as isolated instances, with no connections beyond common membership in Tan's categories. Most of the descriptions lack detail, and the categorization does not lead to an understanding of the incidents described.

The lack of detail is not entirely Tan's fault. Inevitably, in a history covering so wide a topic, individual incidents are treated somewhat sketchily. That is true here, with a few exceptions such as Kamlun's rebellion in Sulu (114-17). While such details as Tan offers are tantalizing, the reader is frustratingly denied more. One means of compensating for this drawback in general studies is to include references to sources of more comprehensive information on specific subjects. Tan does not do this. While he briefly lists available sources concerning Muslim Filipinos, he does so for the purpose of establishing their authors' viewpoints, most of which he finds biased in one way or another. Instead of indicating what useful information can be found in these, he comments mainly on their irrelevance to the historiography he envisions. It would have been useful, for example, to note that for more detailed information on uprisings in Sulu during the American military regime (e.g. Hassan's uprising, the Pala rebellion, the Bud Dajo massacre, etc.), the reader could refer to Tan's own master's thesis, "Sulu Under American Military Rule, 1899-1913," which was published as volume XXXII of the *Philippine Social Science and Humanities Review*, 1968 (see especially Section III— Armed Disturbances, pp. 54-87). In this study, Tan's discussion of aggravating circumstances displays more detail and gives the reader a better grasp of the subject than does his discussion in the more recent book. Tan unfortunately includes no such reference, and similarly discards with only brief mention other useful

sources of information, including books by American and Filipino observers of the armed struggle, recent anthropological and historical studies, and the works of Muslim Filipino authors as well. Instead of noting the information on the Muslim Independence Movement contained in Alunan Glang's *Muslim Secession or Integration* (1969), Tan writes, "a more useful contribution would have been a presentation of the Magindanao aspects of Muslim history and culture (138)." Because Tan fails to acknowledge other sources of information, the reader is left with no way of extending his knowledge of conflicts involving Muslims beyond Tan's cursory treatment.

Doubts about the material and its presentation are reinforced by examination of the factual material and its documentation. The book contains some outright mistakes, and numerous erroneous, impossible, and missing citations. So faithfully does the book reproduce Tan's dissertation that all the mistakes are included as well. In his account of Datu Ali's revolt, Tan writes that American troops shelled Ali's *cota* at Kudarangan in March 1903 (23). The attack actually took place in early March 1906. This is unlikely as he was killed at Simpetan on October 22, 1905. Tan reports of this encounter that most of Ali's men were killed with him (24) and in Appendix A (162) lists the outcome as "leader, sons, datus, (and) several Muslims killed." That "several" was actually several hundred, in a battle described by Gowing as "one of the bloodiest battles ever fought with the Moros."² In his account of Datu Alamada's resistance (24-5) Tan writes that Alamada surrendered in 1913. The actual date of his surrender was May 19, 1914 (Gowing, 1977: 277). To indicate uncontrolled growth in the "Muslim population" Tan cites census figures 1903-1960 for "Muslim Provinces (125)." He neglects to point out that a large portion of the increase was not in the Muslim population at all, but reflected in-migration of Christian settlers, particularly in Cotabato, Lanao, and Zamboanga/Basilan.

These errors of fact indicate that the manuscripts was not carefully checked before publication. This impression is confirmed by errors in references. For example, in his brief discussion of "amuck-running" Tan cites a statement made by one Henri Fancounier in 1931 (13). The corresponding footnote lists the source from which Tan obtained this statement as a manuscript prepared by J. McD. Sheridan in December

1906! This apparent testimony to Sheridan's prophetic qualities is repeated from Dr. Tan's dissertation.

In at least two places, listing of Sulu and Lanao "anti-American" disturbances (22) and Datu Santiago's 1923 revolt in Parang, Cotabato (41-2), footnotes are out of order in the text. The confusion this creates in deciphering references heavily burdened with "op. cit.," "infras.," and "Ibids." is hard to overestimate. Elsewhere statements attributed to Dean Worcester (30), Gen. Leonard Wood (38), Prof. Maximo Kalaw (83), and Nathorst (84) are included without any citation of sources at all. These difficulties are added footnotes which are obviously incorrect. If the reader checks a footnote on page 94 concerning the transition from the Bureau of Non-Christian Tribes to the Office of the Commissioner for Mindanao and Sulu, he finds no source, but a list of payments apparently made to some Muslim leaders for an unnamed purpose. No discussion of this topic is evident in the text.

Even more troubling are the numerous items of information for which sources should have been cited but are not. One instance should show the problem. In discussion of Tahil's revolt in Sulu in 1927, Tan lists several possible motives attributed to Tahil by different people. He writes, "Some attributed it to Tarhata's³ love for luxury....," a Major Malone who disliked Gov. Moore, "was said to have given Tahil rifles and ammunition....," "Tahil, however, declared that his reason for revolt was reflected in his demand....," etc. (40). Tan goes on with this analysis for nearly a page without once indicating where he obtained the information on which it is based. The reader knows neither who cited Tarhata's love for luxury and consequent economic pressure on Tahil, nor who said Malone gave rifles and ammunition to Tahil. We do not know whether these people were in a position to know these facts. We do not know for whom Tahil declared his motives.

Aside from indicating a lack of scholarly care, all these deficiencies seriously compromise the book's authority. Documentation in an historical account is crucial, because the credibility accorded a "fact" depends largely on its source. Lacking knowledge of the source, a fact is credible only insofar as it fits the reader's preconceptions of what is true, or what at least, is likely. Historical writing should hope to accomplish more than that.

Lack of documentation is an even more serious problem in the added chapters. In discussion of what he calls the "Mindanao Crisis, 1971-72," "The Sulu Movement, 1969-72," and "The Independence Movements of the 1970's" (117-23) Tan disregards the need for documentation almost entirely. He refers to the "Corregidor Massacre," difficult living conditions, ineffective and unimplemented reforms, indifference of Muslim leaders, the Sulu movement, Nur Misuari's "easy" assumption of leadership in this movement, a hidden Marxist-Leninist program for the MNLF, use of *jihad* for revolutionary activities, MNLF connections to the KM (Kabataang Makabayan), Udtog Matalam's secessionist declaration, and the revolutionary turn to a Bangsa Moro Republic, all without a single substantiating bit of evidence or a single reference. Many of these events are publicly known, although that does not excuse the author from his responsibility to cite his sources. More serious is his assertion of dubious or arguable propositions as unassailable facts. For example, this reader has never seen any reference to a "Sulu Movement" in any other published or unpublished work besides this one. That does not mean it did not exist, but Dr. Tan gives no details of its origin, or of who was involved, what they did, or anything else. The reader is given no reason to take the "Sulu Movement" as fact instead of Dr. Tan's fancy. The same is true of a purported radical Filipino nationalist element Dr. Tan says exists in contemporary Muslim movements (127-8). If there is such an element, where does Dr. Tan find evidence for it? He does not say.

Perhaps most serious in this regard is Tan's failure to offer support for his characterization of the MNLF as a Marxist-Leninist movement manipulating religious sentiment for revolutionary political purposes. He writes, "Therefore, the MNLF, whose leadership was somehow related to the Kabataang Makabayan, was not principally a secessionist struggle, secession being merely a practical strategy to unite various Muslim groups for a more comprehensive movement of peasants and workers in the Philippines (119)." What evidence has he to indicate that secession is merely a front for some more insidious and comprehensive plot? He presents no evidence to support his claim. If he had written that he had seen some MNLF or government documents, that he had found evidence to this effect in other published sources, that he had

inferred it from observing MNLF activities, or that he had heard rumors from his neighbors; if he had included any evidence at all, it would be preferable to this bald assertion of unsupported opinion as authoritative history. As it is, Tan's account reads like an attempt to discredit the MNLF and other Muslim movements by painting them Marxist with a broad brush. Since the question of the MNLF's ultimate goal is such a central one to understanding the recent armed struggle, we could have expected Tan to sort out the substance in these charges from the specious, perjorative, and politically expedient. He fails to do so.

Of course the book is not entirely without value. One chapter which contains some interesting ideas is that relating Tausug "historical consciousness" to patterns of conflict.⁴ While it is not clear in what sense Tan uses the phrase "historical consciousness" he does make a credible case that oral history, as embodied in narrative stories and myths, has an influence on the way people see themselves, and on the way they act. He overextends the argument, however, finally asserting that the Tausug "portray the traits of characters in the epics, ballads, and other narratives (143)." While the lives of culture heroes in ballads and epics may reflect cultural ideals and aspirations, they do not often serve as direct role models. The picture Tan draws of the Tausug, as a result of this presumption, is heroic but largely inapplicable to the masses he sees as most influenced by oral traditions. While he indicates that members of the elite attained a "higher level" of consciousness through their access to written records, members of elites typically have to adhere more closely to cultural ideals, at least publicly, than do members of the "masses." This is particularly true in factional social structures where elites must depend at least partially on personal followings for their power. Tan writes of the ideal Tausug, "He is a fanatical defender of his freedom and builds around himself an image and aura of self-sufficiency and importance...Shame, however trivial, is taken as a serious offense...He seldom provokes but a slight challenge is seldom left unanswered (143)." All these characteristics would be more exemplary of a Tausug leader than a common person.⁵ Nevertheless, Tan's suggestion that oral traditions contain a rich vein for understanding cultural values underlying behavior does have merit.

This topic of oral history, cultural themes, and values brings up

another difficulty with Tan's book. This topic is discussed twice, once in the part taken from his dissertation, and once in the added chapter on historical consciousness cited above. These treatments differ in important details. In chapter 6, Tan writes of "Muslim literature (60)" but in chapter 10 he limits the discussion to the literature of "Tausug society...because Tausug society differs in many ways from other groups (132)." This question of whether to treat Muslims as a single group, or to insist on more particularistic treatment of groups, individuals, and events arises repeatedly. Throughout that portion of the book taken from his dissertation, Tan refers to "the Muslim struggle," the reactions of "Muslim ruling classes" to American sovereignty (20), "Muslim attitude and strategy (45)," patterns in "Muslim disturbances," "Muslim collaboration" with the colonial power "chapter 6) as if all of these were unitary phenomena. In all this there is no intimation that the proper frame of reference should be the ethnic society (e.g. Tausug, Yakan, etc.), the region, or some other more specific unit. Yet in his discussion of historical consciousness, he no longer writes of "Muslims" but of "Tausugs." He goes on to list elements differentiating Muslim societies, including different "historical experiences (126)." Even so his entire analysis of the armed struggle indicates a preponderance of similarities of experience, and few significant differences. Curiously, after arguing in chapter 10 the uniqueness of each Muslim ethnic society, he reverts in chapter 11 to writing of "Muslims" as a whole again (150).

This switch to the specific from the more general emerges clearly in Tan's listing of implications and recommendations for government policies (chapter 12). Here he recommends a "pluralistic assumption which recognizes the differences rather than the similarities of ethnic groups in Philippine society (155)." He then goes on to propose that ethnic differences are more basic than ethnic similarities, the latter being "external and superficial because they exist through constitutional and legal means (155)." It is hard to find a non-absurd meaning for this statement. Is he arguing that a common religion, common elements of social structure, common mythic characters, and other similarities are legislatively imposed? Nevertheless, it is clear that Tan now believes that the differences between Muslim societies are more salient than

their similarities. He thus brings into question his whole historical analysis, which established overriding commonalities in the Muslim experience, in which he treated Muslim societies as uniform, with scant mention of the differences between them.

Tan's analysis and conclusions are questionable in other ways as well. While many of his thoughts are straightforward and sensible, some are not well conceived. Early in the book he writes that colonialism, "reduced traditional leaders to virtual impotence (19)." Later he writes, "the advent of *colonialism helped reinforce the status of the ruling class* which became the only avenue through which colonial rule could be imposed on the Muslim masses (140)."⁹ These two statements are plainly contradictory. He writes that both Christian and Muslim revolutionary movements, "developed toward an increasing dependence on a mystical approach to the armed struggle rather than toward change in strategy and/or improvement in leadership and weaponry (106)." Aside from a cluster of "mystical movements" shown on his graph during the period 1922-25 (55) there is little evidence in Tan's account to indicate such a trend. In fact, in a later chapter, he writes, "the modern technology of the Christian north changed the nature of Muslim armed conflicts. This is shown in the attempts to import western arms, to solicit foreign aid, and to enter into temporary compromises with the enemy (149)." In any event these were characteristic of Muslim strategy all along (see Majul, 1973). Tan writes that after World War II, adherence to syncretic Islam as a basis for unity, "weakened existing bonds between Christians and Muslims and allowed aliens to profit from this condition (127)." Perhaps these bonds consisted of the partial coincidence of interest found among Christian and Muslim anti-Japanese guerrillas during the war, but Tan does not say. He also does not identify the aliens who profited from the Christian-Muslim dichotomy or how they did so. This statement, moreover, appears to make Muslim religious sentiment the scapegoat for postwar neocolonial economic penetration of the country. Later Tan writes of the colonial system that "education and professional efficiency became the only basis of authority, wealth, prestige, and influence (149)." Not only has no such society, colonial or otherwise, ever existed, but if it did we would be quite unsure what it would look like. Finally, Tan refers to, "the col-

lapse of Muslim power after the disastrous battle of Bud Bagsak in 1913 (149).” Though Bud Bagsak was the last massive resistance to colonial forces, it is well established that “Muslim power” had been effectively curtailed in all regions (with the possible exception of Lanao) even before the arrival of American forces in 1899 (see Majul, 1973: 283-316; Gowing, 1977: 12-3; Iletto, 1971).

Finally, Tan’s recommendations are not sufficiently well thought out. In chapter 11 he sets the course to his desired goal, national integration, as proceeding from consolidation into an ethnic, then a Muslim society, and finally unification of Muslim societies with the national society (151). In chapter 12 he reverses the order, advocating elimination of terms linking Muslims together in favor of more specific ethnic labels, and finally submerging all ethnic labels under the term Filipino (155). The progression here is Muslim-ethnic-national. His recommended program for achieving this goal does not appear well integrated. In the section on historical consciousness he endeavored to show how cultural themes, values and personality, and behavior are all integrally related (143-4). Yet the “radical nationalist” program with which he sympathizes depends on a fragmentation of culture and society, with the pieces being reassembled in the nationalist mold. The program aims to “preserve Muslim history and culture” while at the same time advocating reforms including, “promotion of a national language, development of economic opportunities, abolition of feudal and alien control, elimination of religious teachings in schools, secularization of sectarian institutions, etc. (127).” Essentially what is proposed is a radical reworking of society and culture. It is not possible to preserve Muslim history and culture, in any meaningful sense, under these conditions.

Tan’s recommended program is essentially assimilationist, different in specifics from those of the Spanish and Americans, but the same in principle. He contrasts two concepts: The first is an “ethnic” Muslim identity based on increasingly sophisticated knowledge of Islam, having Arabic as a common medium of communication, and working at the revival of “traditional or semi-traditional” political structures. The second is a preferred nationalist identity which subordinates religious to social needs, promotes Pilipino as a national language, and aims at developing a new political structure (154). He suggests that a datanship or

barangayic structure "rooted in modern concepts" would be an appropriate basis for the new political system. Attaching new meanings to old structures is, however, not likely to be a simple or to significantly ease the thorough reformulation of Muslim societies and cultures which is at the core of the envisioned program. Reduction of the role of religion, insistence on a uniform national language, and reordering of social structures will inevitably threaten the traditional bases for social standing in Muslim societies. These changes will almost certainly be resisted, just as they have been in the past. The lack of sensitivity in some of these proposals may be seen in Tan's idea that inter-cultural exchanges be encouraged through "intermarriage (155)." This does not take account of the explicit restrictions in Islamic law on intermarriage, according to which a Muslim man may marry a non-Muslim woman, but not the reverse.

It is, of course, unarguable that "offensive and divisive terminologies" should be eliminated from use, as Tan recommends. It is not clear what terms should be included on this list. Tan suggests that all epithets referring to religion be eliminated, including such straightforward labels as "Christian" and "Muslim." He favors "Tausug, Maranao, Yakan, etc." He does not see that all ethnic distinctions are potentially divisive, whether they refer to religion, language, dress, or any other arbitrary criteria of distinction. The pertinent factor Tan neglects is that whatever level of ethnic differentiation is perceived as being relevant for resource distribution will tend to take on indidious and divisive characteristics (see Barth, 1969; Despres, 1976). The continuing prominence of Muslim/Christian distinctions in the Philippines is not just an artifact of history. They continue because interests are seen by members of both groups as divided along religious lines. The salience of such distinctions is, of course, relative and Tan is correct in noting that there are divergences of interest between different Christian groups.⁷ Government promotion of more particularistic units as relevant for distribution of economic and political goods may simply encourage divisive ethnic competition on a different level. While lower level competitions may have the advantage of making individual groups more tractable and less able to wage armed struggle, this is essentially the same divide-and-conquer strategy Tan deplored in colonial times. Instead of

dividing Christians from Muslims or Muslim datus from each other, Muslim ethnic groups are divided from their co-religionists.

Much of Tan's demand for more particularistic treatment seems to stem from grievances of Sulu residents who have felt neglected in government programs to aid Muslims. He writes of a "new ethnocentrism" reflected in Lanao attempts to dominate Muslim affairs, complacency in Cotabato, and Sulu's struggle for "equitable representation for all Muslim groups (121-2)." Apparently, Tan feels Sulu residents would get a better break from a pluralistic approach, with "sectoral" representation as advocated by the Sulu-Tawi-Tawi Professionals, Inc. Elsewhere in the book Tan describes the evils of "ethnicism (151)." One wonders what the invidious distinctions he draws between Lanao (domineering), Cotabato (complacent), and Sulu (equity-seeking) Muslims constitute other than "ethnicism." In such characterizations, Tan perpetuates the disunity he says has afflicted the Muslim armed struggle from the beginning (151).

There certainly are geopolitical differences between the various areas of Muslim habitation, and these should be attended to. It is not clear, however, that the path of more particulate ethnicity leads to national integration, which Tan desires. Inevitably his proposals contain a coercive element, an ultimate subjugation of local and regional interests to the "national interest." Those whose particular interests are impinged upon will resist. It is unwise to underestimate the costs of such a program. They may ultimately be necessary to bear, but they are likely to be high.

Tan's analysis does not seem to take account of the Muslims as rational actors operating according to what they perceive as their best interests. His account of the armed struggle, while listing many instances of apparent injustice and hardship which precipitated violence, inevitably disregards these as causal factors in favor of high level "causes" such as colonial militarism, opposition to all forms of government authority, etc. (43). In arriving at a realistic assessment of the Muslim struggle, considerable weight should be given to what the participants themselves say. One of the remarkable aspects of the Muslim struggle is that demands for justice and self-determination have been consistent throughout the colonial and post-colonial periods.⁸ Their

demands not having been satisfied, the people have rebelled. There is no reason to believe that rebellion will stop until their grievances are redressed. As Tan (157) correctly notes,

The physical capacity of a discontented people can be destroyed by military might but the lessons of history show that the desire and will of the people to resist cannot be eradicated by physical force. It can only be delayed in its realization.

Many of the sources of Muslim discontent are apparent in Tan's descriptions of incidents in the armed struggle. These include interference in local affairs by outsiders, forced labor by the government, imposition of alien systems of justice, lack of government recognition, landgrabbing, etc. Tan's study does not portray reasonable people coping as best they know how with offenses committed against them. It portrays irrational intractable people mired in backward traditions who, when threatened, retreat into mysticism or are duped by religious and political fanatics. This only perpetuates the negative stereotypes which already mark off Muslims so strongly from other Filipinos.

In arriving at a practical and just solution to the southern Philippine conflict, the first step should be to ascertain just what the people want. After having proposed a comprehensive program ostensibly leading to national integration Tan finally suggests that, "General studies should be initially and silently conducted in both Christian and non-Christian areas to determine whether or not the people desire integration (156)." Tan's proposed program appears to assume that there is no doubt about the outcome of such studies. I suggest that instead of being an appended afterthought, assessing the will of the people who will be affected should be the first step in policy and program design.

I derive no pleasure from finding so many faults with Tan's book. The subject is fully as important as he believes. It is crucial to come to a "realistic assessment of history and...a meaningful perception of contemporary conditions (150)." Tan wanted his book to help attain this goal. The gap between the vision and its realization is just too large. Despite his accumulation of data and his attempts to give an integrated view of the armed struggle, the result is an oversimplified analysis which generates as much confusion as light. Future researchers in this field will have great difficulty using Tan's study as a base on which to build.

NOTES

- ¹ These quotations would be referenced but for the lack of page numbers of any sort in the introductory parts of the book. All subsequent unreferenced quotations are also taken from this portion of the book. References to the body of the book will take the form of parenthesized page numbers only.
- ² For details on Datu Ali's revolt see Gowing, 1977: 153-4 and Majul, 1973: 314.
- ³ Tarhata, Tahil's wife, was Princess Tarhata, daughter of Sultan Kiram II of Sulu.
- ⁴ As an anthropologist, I am unabashedly partial to studies relating to elements of culture and history.
- ⁵ See Kiefer, 1969, for details on this matter.
- ⁶ Italics added.
- ⁷ As is commonly known, ethnic distinctions between Christian groups are salient in Philippine politics, as are those between Muslim groups (see, for example, Cordero and Panopio, 1967: 230-1). Note also the perceptions of intergroup differences and distance in Filipinas Foundation, 1975. In some cases these are nearly as great between some Christian groups as between the same Christian groups and Muslims.
- ⁸ Compare the grievances in Tan's accounts, those in the 1963 Senate report on cultural minorities (Senate Committee, 1963), and those in transcripts of the Zamboanga Peace Talks, 1975.

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