

COMMENTARIES

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I am convinced that Professor Mamalakis' paper presents a framework which is potentially useful for the social analysis of developed as well as developing societies. The framework generates models. Models cannot, of course, be true or false, but *predictions* derived from their applications to concrete societal situations can be borne out to a greater or lesser extent. If predictions are found true in a variety of cases, then the model is productive, useful.

The sector notion seems to me to be rather clear, but there are, of course, problems that arise when we try to decide how to *delineate* sectors in given societies. Should, for instance, one talk of an *ejido* sector in Mexico, in contrast to the private agrarian sector, *la pequeña propiedad*? I suppose one should, using the following criteria of delineation:¹

1. There is a financing system (the Banco Ejidal) which helps finance many ejidos (not all of them by far). It *only* works with ejidos.
2. The peasant sector of the ruling party (the PRI) claims to represent the *ejidatarios*, among others.
3. There is a set of laws which defines the rights and obligations of ejidatarios and people who want to become ejidatarios.
4. Ejidatarios perceive themselves, at least regionally, as a group with common interests.

The extent to which a proposed sector is a social system and not merely a convenient statistical "aggregate" is of course of great importance when we want to study *the politics of sectoral clashes*. It is with this political aspect of the Mamalakis framework that I want to deal in this presentation.

When a sector becomes dominant or suppressed, or even when it stays neutral, it is because certain political decisions have been made. The amount of effective *power* and *influence* a sector can generate and apply to this decision-process is obviously a relevant research (and maybe researchable) question. But when a political decision has been made, it also needs to be *implemented*: the question then arises about the power and influence a sector can wield inside the public administration branches concerned with its affairs.

One important task, when it comes to applying the Mamalakis framework to concrete situations would be, then, to do detailed studies of the interplay of sectoral power and influence with the political authorities and the administrative apparatus on the macro- and well as on the micro-level. Suppose that

urban workers in, say Mexico, are interested in cheap staple products, like corn, and beans. Do they see these interests threatened if the government were to commit large resources toward support for the poor ejido peasantry, when funds instead could go to support capital-intensive and presumably more productive commercial farming? My hunch is that urban workers and poor peasants only make common cause under rather exceptional conditions: these need spelling out obviously. But the normal state of affairs would seem to be that the urban working class puts *sector-consciousness* before *class-consciousness*. But many urban workers come from rural areas (how much do we really know about the origins of the industrial workers in developing countries?), and this may predispose them to sympathize with the peasants. As a tentative hypothesis one could then state:

Workers' organization will be sector-loyal in a clash between industry and peasant sectors in agriculture if they recruit members from urban strata; if they recruit from rural strata their loyalties will be divided and their sector-loyalty weaker.

I am offering this hypothesis as an example of the kind of specific and testable propositions that empirical studies of the politics of sectoral clashes would be concerned with.

The Scandinavian countries have histories of "red-green" coalition politics. In Sweden, for instance, the Agrarian Party has several times been a partner in governments dominated by the Labor Party. Finland currently has a government with Agrarians, Socialists and Communists in it. The Labor Party in Norway has appealed to the small-holders as well as to the urban workers. During the late sixties agriculture is to some extent a suppressed sector in Sweden, but not in Norway or Finland. A comparative study of this particular sector-clash in these countries should prove useful. The countries are very similar with respect to political and administrative structure, culture and international status.

For Norway there exists some rather detailed research on how the agrarian sector has been able to keep a great deal of control over policy. Since the last century the training of agronomists and agrarian economists has been conducted at the State Agrarian College, which is very much committed to a "populist" ideology. This involves a desire to protect the interests of local, agrarian communities even at the expense of "technocratic" concerns with the "over-all growth" of the economy, GNP, "rational allocation of resources" and similar considerations. The bureaucrats in the Department of Agriculture and the whole agrarian administration is permeated with this view.²

Some preliminary research has dealt with the problem of how resistance

on the part of small-scale Norwegian agriculture to sector-suppression is rooted in the social structure of the farming communities.³

In Sweden, the attitude to agriculture is definitely 'technocratic' and not populist. An agrarian enterprise is useful only if it gives a rate of return that is competitive with manufacturing. "Unproductive" enterprises should be phased out.

Finland seems to be more like Norway than Sweden.

An interesting question is: Can Finland and Norway continue their populist approach to agriculture as the integration of the Common Market proceeds? Will they be forced to take the Swedish course? What kinds of tensions would that create within the countries?

I do not intend to pursue this Scandinavian problem much further in my presentation: my point is to offer it as an example of the kind of macro-political questions the Mamalakis framework gives rise to. Just as sectoral clashes have a foreign trade aspects to the economist, so do they have a foreign policy and international relations aspect to the political scientist and sociologist. (In fact, a great many of the tricky problems of the Common Market are not due to de Gaulle, as many Americans like to think, but to political aspects of sectoral clashes.)

We should also consider the "sociology of knowledge" aspect of sectoral clashes. How does a point of view that reflects only the interests of a particular societal group get defined as the "only rational thing to do", "non-political, purely expert considerations" and the like? Economics concerns itself with figuring out the balance between utilities and disutilities of policies. But the set of factors they take into account is often very small. In discussions of policies one often finds sparse reference to hidden human costs of, for instance, industrialization or suppression of small-holder agriculture. The farmer-fishermen in Norway, for instance, who prefer to fish in their own small boats rather than becoming crew-members on big trawlers are said to be irrational. Yet it is possible to show that they use a calculus which includes some "non-economic" utilities, and that they, from their own point of view, behave very rationally. Decision-makers on the top level need to rethink the set of factors that should enter into their cost-gain calculations. From this point of view I would like to add one point to Dr. Mamalakis' policy recommendations: Decision-makers who are concerned with balancing the various sectors in a society against one another should take pains to develop *explicit welfare criteria* that take into account a wide range of factors, including some that are traditionally viewed to be outside the province of economics proper. Central planners who do not, may someday have a populist revolt on their hands.

NOTES

1. T. S. Rao, "El crédito rural en México," *Investigaciones Económicas*, XXII, No. 88 (1962), 1061–1064.
3. Knut Dahl Jacobsen, *Teknisk hjelp og Politisk Struktur* (Oslo: Universitetsforlaget, 1964), and an article by the same author, "Informasjonstillgang og likebehandling i offentlig virksomhet," *Tidsskrift for Samfunnsforskning*.
3. See Ottar Broz, "Recruitment and Organizational Stability in Industrially Underdeveloped Areas," *Acta Sociologica*, XII,1 (1969).

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I would like to ask the political scientist's question of the theory of "sectoral clashes:" Does this approach help us to describe and explain the nature of political conflict and conflict resolution in Latin America? I realize that Mamalakis' purpose was not this, but to propose a model for the process of growth. However, I feel that in this case the political question may help to test the economic theorem. Furthermore, the purpose of the symposium is to test the relative utility of sectoral and class approaches to all aspects of the problem of development and change.

Mamalakis' target, the class conflict model of Latin American political economics, is not merely a straw man. Whether he is correct in thinking that this mode of analysis is central to contemporary economic development theory or not, it is certainly true that the conventional wisdom of political economic analysis about Latin America relies on it.

Simply as a taxonomy of the relevant actors in Latin American politics, the sectoral clash theory is an improvement on the class conflict model. Perhaps this is only because Mamalakis' model has sixteen boxes rather than the three or four used in class analysis and therefore permits more complex differentiations. Nonetheless, it is true that class analysis is an incredibly blunt instrument for describing or explaining political conflict in Latin America. Most of the interesting things politically in Latin America happen within the conventional classes rather than between them. Finding a workable classification scheme for the units of analysis of Latin American politics has been a frustrating task. The more discrete, and in a sense "true to life" we become in identifying the units, the more we are tempted by descriptive journalese ("clique of generals. . ."). On the other hand, highly abstract units, such as classes, often lead to trivial or misleading generalization ("the masses demand . . . the oligarchy controls. . .").

Sectors, as a unit of analysis, have some nice characteristics. They are relatively discrete. Certain public policies are apt to affect their hypothetical

self-interest in identifiable ways. Some might argue that the sector is an inappropriate unit of political analysis because it is not collectively self-conscious of a political interest. However, political economic analysis need not rely only on organized, self-aware groups or movements. If the category includes groups or individuals who tend to behave politically or economically in similar ways, that is sufficient.

Impressionistically, it seems to make sense to read the political economic history of Latin America in terms of sectoral clash analysis. As a point of departure, Mamalakis postulates what I might call a Mercantilist government which advantages one sector over another for reasons of state. This would seem to permit a more accurate reading of the course of Latin American development politics than the presumption that the state characteristically responds to demands from established interests. From the mineral concessions and liberal trade policies of the nineteenth century to the Prebischian economics of the postwar period, the development policies of most Latin American states cannot very well be explained as responses to extant economic elites.

Similarly, analysis based on class or ideological alignment is of little help in explaining the patterns of political conflict over economic policies unless one has a bent for irony and paradox. There are simply too many strange bedfellows in the record of Latin American politics. The presumption of a "ruling class" conceals too much of the maneuver and bargaining that are at the heart of normal politics in Latin America. The complex molecular structures of governing coalitions in Latin America are more inter-class and inter-interest than is presumed in the conventional wisdom. The sectoral clash thesis is helpful in describing and explaining some good portion of these politics.

However, I do not believe that sectoral clash constitutes a new unifying theme for the analysis of conflict in Latin America. Nor do I believe that it replaces class conflict analysis. Rather, it simply calls attention to another axis around which conflict may occur in Latin America. Mamalakis does not imply that all political economic conflict can be described as sector based. Certainly, a scheme that can account for both sectoral and class conflicts, as that proposed by Mamalakis does, is to be preferred to one that only deals with one of these dimensions. However, I can conceive of any number of other ways in which Latin Americans may choose to define friend and foe for purposes of political conflict, any of which may override commitments to sectoral or class advantage or interest. For example, suppression of guerrilla movements is not an inter-sectoral clash. Nor can it be explained in all of its variants (Bolivia, Venezuela) as an example of class conflict. It would rather seem to represent an alliance of all political contenders with a stake in and access to the existing

political system to thwart the emergence of a new contender for power whose impact on the system would be highly uncertain.

Sectoral clash, then, provides a new way of looking at one arena of Latin American politics, that of conflict and competition for the productive incentives and disincentives that the state can offer. Better, it provides a new level of analysis for the examination of this problem. Lower the level of abstraction, and the story is written as a contest between firms, interest groups, and institutions. Raise it, and one portrays the politics of the "modern" and "traditional" or urban and rural sectors, the politics of dualistic economies.

The politics of development policy have not been central to the concerns of students of Latin American politics. This field has largely been left to the economists. However, this world, where central bankers, technocrats, entrepreneurs, and interest groups play for the stakes of exchange rates, fiscal policies, and price controls rather than the more familiar ground of the political scientist, where parties, the military, intellectuals and others compete for power, is probably the most salient arena of contemporary Latin American politics. The theory of sectoral clash could provide the political scientist with a most useful handle on this problem. Class conflict analysis cannot touch this problem at all, simply because it postulates a unity of interest among the putative contestants. The tools used by Marxists and others to deal with such matters are clearly jerry-rigged appendages to a theory that doesn't fit the data. Such constructs as "the progressive forces of the national bourgeoisie" have neither precision nor theoretical elegance.

As presently formulated, the theory of sectoral clash does not make the political elements of the problem explicit. Portraying Latin American development primarily in its economic dimension, Mamalakis draws a picture in which the state acts to restructure the economic advantages and disadvantages of the several sectors. The sectors then adapt to this changed economic environment. In real life, of course, they also fight back, through bargaining, negotiation, and the deployment of political resources. The capacity of the state to make good on its design for development is conditioned by the political demands made upon it. The straightforward strategies of development offered by Mamalakis are always watered down by the need to compensate economically disadvantaged, but politically significant groups. Furthermore, the intentions of governments are conditioned by the capacity of non-compliance with these intentions by all economic actors.

From this point of view, all strategies of sectoral dominance must be economically sub-optimal. This is so because such strategies must be jointly political and economic, that is, they must simultaneously resolve the problems of the generation of growth and of coalition maintenance in what is usually a complex

political environment. Because of the political uncertainties that attend the formulation and execution of development policy I cannot accept the notion that the theory of sectoral clash unlocks the genetic code behind Latin American history and development any better than the class conflict model. Until persuaded otherwise, I will continue to view the problem of development as essentially pragmatic and open-ended, and abjure affiliation with any theory of historical movement from stage to stage.

Let us examine one problem of generalizing political consequences from the theory of sectoral clash. Mamalakis hypothesizes that class struggle is most likely to occur within the "losing sector". I would have supposed that the more plausible hypothesis would be just the other way around. It would appear to me that intersectoral class conflict is first activated in the dominant sector. The emergence of military labor organizations in the mines of Bolivia, the oil fields of Venezuela, the banana plantations of Honduras and Costa Rica, the later appearance of industrial unions would suggest this. Even *campesino* militancy and unrest seems to follow from public incentives to the agricultural managerial class. Thus, peasant rebellion follows the land policies of Díaz in Mexico, or the commercialization of the latifundia under Gómez in Venezuela. To postulate vertical clash within the dominant sector would seem to square best with sociological theories of "relative deprivation" and the propensity of revolutionary action to occur when new opportunities are partially opened rather than when things look most hopeless.

What are the limits of sectoral clashes in describing and explaining political conflict in Latin America? At first glance, it would appear that the theory of sectoral clash does not fit the conditions of "revolutionary" politics. In the Mamalakis model, the political actors seem totally to respond to the initiatives of the state. They seem to recognize the legitimacy, or at least the authority, of the established political system. No actor in the Mamalakis scheme seems to see the capture of power as a way out of its "suppressed" position.

However, it does appear that revolutionary politics in Latin America can be described more neatly in terms of the sectoral clash categories than the class conflict ones. To analyze the Bolivian revolution, for example, simply as a class phenomenon, misses the interesting shades of conflict and competition between miners and campesinos. Similarly, the complex and evolving patterns of coalitional politics in Mexico are made trivial by class analysis.

The problem, of course, is that class conflict models have no very persuasive theory for dealing with the government of societies that have had a revolution. The sectoral clash theory seems at least suggestive in this respect. For example, the Cuban case could be analyzed as one in which agriculture became the dominant, with services the suppressed and industry the neutral sector.

Incidentally, Cuba would seem to confirm Mamalakis' corollary concerning vertical clash. It was in the suppressed sector that antagonism toward the revolution became most acute, partially reconciled by a migration of labor to other sectors or abroad.

The theory of sectoral clash has manifest advantages over the class conflict model in the analyses of Latin American politics. However, in political analysis, class conflict is not really the closest competitor. Rather, students of Latin American politics seem increasingly to be working with a complex coalition-management model. I believe that the dimension of sector could profitably be incorporated into this approach. Despite Mamalakis' suggestion that parties and basic political institutions could be added to his sectoral scheme, I find it incredibly hard to perform the operation he suggests. The interactions of Latin American politics are simply too rich and diverse to be squeezed comfortably into this mold. However, in some specific arenas of politics, such as economic policy-making, the sectoral clash theory would seem to have considerable power.

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The purpose of this comment is to raise some social and political issues explicit or implicit in Markos Mamalakis' presentation of the theory of sectoral clashes.¹

In general, one of the more important questions to be determined is whether the theory of sectoral clashes is primarily an analytical tool or also a close description of empirical reality. Do people actually behave "sectorally" or is it that one finds it useful to conceptualize at least an important part of their behavior in such sectoral terms? Although a great deal of theory-relevant research would be necessary to settle that question, it may nonetheless be possible to suggest some lines of approach. The theory of sectoral clashes, as presented thus far, rests on at least five preliminary assumptions:

- a. that the economy is multi-sectoral, so that its various components may become dominant, neutral or suppressed;
- b. that national, religious or cultural integrative cleavages are of secondary importance for conditioning behavior, and that they are, therefore, neither the primary cause nor the ultimate purpose of state action;
- c. that the government's main policy orientations and concerns are domestic rather than foreign;
- d. that the level of government capabilities is high, that the range of its effectiveness is broad, that the scope of the behaviors which authority may affect covers all the

- relevant issues for the economic implications of the theory, and that the domain of government activities includes most people in the polity;
- e. that such a government is motivated to intervene actively and effectively in the economy, that is, that the dominant ideological or symbolic structure leads the government to such action, primarily out of motives which are predominantly "economic."

To stress the existence of assumptions is not to deny the validity or utility of a theory, but merely to indicate that the theorist himself has chosen to embrace less than the universe of data in his search for useful explanations. Since the theorist did not explicitly limit the scope of his theory to a regional grouping, even though its special applicability to Latin America was indicated, many of the comments will be presented from the point of view of a general framework, although toward the end of the discussion some more specific suggestions will be made with regard to the theory's applicability to Latin American countries. The five assumptions which have been stated do limit the empirical scope of the theory. But it would still be possible to use the theory of sectoral clashes as an analytical tool in cases where the assumptions are not met, provided the researcher is fully aware of the difficulties in establishing a link between his analytical tool as a research "map" and actual behavior.

To a large extent, the most important limiting assumption is the second one, that is, the primacy of economic or functional cleavages in a society, of a class or sectoral nature, over other kinds of cleavages, of a national-cultural-linguistic-religious nature. The effect of this second assumption is increased by the existence of the fourth and fifth assumptions on governmental capabilities and motivations. These comments, in turn, affect the relationship between sectoral and class factors in behavior.

Sociological and political research has repeatedly emphasized the persisting importance of integrative factors in individual and collective behavior in a very large number of countries throughout the world at different levels of economic development. There are dramatic historical and contemporary examples of the importance of non-economic factors in motivating behavior or, more precisely, of a perception of common interests (including economic interests) neither on class nor on sectoral bases but on national or communal bases. Among such dramatic examples of the prevalence of national perceptions of common interests are the collapse of the Austro-Hungarian empire and the Nigerian civil war. The partition of India and Pakistan is yet another such example. The process of African independence offers a continuing series of examples of the formation of cleavages on national or tribal bases. To most observers, it appears that cross-pressures on nationally oriented behavior which may arise out of cross-national or cross-tribal membership in different sectors

or classes are not sufficient to break up national or tribal solidarity, that is, the cross-pressures between national pulls and sectoral-class pulls are generally resolved in favor of the former. Class or sectoral conflicts do appear within national or tribal groups, but they tend to remain secondary to integrative cleavages.²

If national or cultural cleavages are important, one might expect that governmental behavior might reflect them. If one were to adopt a narrow sectoral approach, one might speak of a sectoral clash stimulated by the Tanzanian government against the trade service sub-sector, or by one of the Ceylonese government against landed estate agriculture, or in the middle ages by the French and English governments against banking. But such a view would be probably misleading. A more powerful explanatory hypothesis would point to another variable: that "Asians" dominated the trade service sub-sector in Tanzania, that Indian Tamil-speakers were the main laborers in the landed estates, and that Jews were the bankers of France and England in 1300. The goals and motivations of these governments were primarily national: to have Africans in charge of the Tanzanian economy, Sinhalese workers in the Ceylonese estates, and to open up banking in England and France to "nationals."³

There is no need to be esoteric in the choice of examples. Religiously based parties continue to play an important role in continental western Europe in the middle of the twentieth century. In Germany, Italy, Belgium and the Netherlands, they are indispensable to the workings of the political system.⁴ In Chile and Venezuela, Christian Democratic parties now run the government. Religious, linguistic, regional as well as economic factors are of great importance to Mexico's PAN. The characteristic of all of these parties is that they cut across practically all functional lines: they are multi-class and multi-sectoral in appeal and composition. Therein lies their electoral strength as well as their cohesive weaknesses once they take charge of the government. The multiplicity of their functional constituencies tends to weaken the capabilities of these parties for decisive governmental action.

Yet another aspect of cleavages of integration which is more related to distinct economic differences is the urban-rural cleavage. Such a cleavage can be perceived as one between agriculture and industry—a view which a sectoral approach might prefer. The problem with such a sectoral view of the urban-rural cleavage is that one of the important corollaries of the theory of sectoral clashes would require some modification: although the theory predicts a clash between landowners and poor farmers, political research stresses the support which landowner-dominated traditional conservative parties have received in the rural areas prior to the social and political mobilization of the countryside. Among the reasons for this, two stand out. The first is that such a conservative

party has had greater access to the rural vote in countries (and at historical moments) where techniques of political manipulation do not necessarily stop short of fraud. A conservative vote in rural areas has often been an indicator of access rather than real support. But secondly, conservative parties can often claim real support in the countryside, although not necessarily in response to the party's economic program; such support is given because the conservative party has often been the party of religious and cultural defense. From this perspective, the urban-rural cleavage is not only a clash between primary and secondary economic activities but also a clash between different cultural life styles and world-views.⁵

This brings us to a problem which is relevant to any mode of analysis, whether one chooses to emphasize functional class or sectoral cleavages or cultural, national or religious cleavages. The problem focuses on the concept of social and political distance. Do members of the same income group, or the same economic sector, or speakers of the same language, or believers in the same religion perceive themselves to be "close" to or "distant" from each other? It is a characteristic of the human condition that one is likely to fit in more than one of the researcher's "boxes;" thus one is likely to belong to a language group, to take a religious (or secular) position, to belong to a functional sector, and to a class within that sector. The critic's cry, under such circumstances, is usually for the much desired and insufficiently performed multi-variate analysis, quantitative or otherwise. The usual aim of such analysis is to determine the variables which are significantly important for an explanation of behavior, their interrelations, the cross-pressures under which a given individual is likely to find himself, and the relative explanatory power of the given variables under consideration. Without performing such an analysis here, it does appear, nevertheless, that a great deal of social and political behavior in a large number of countries tends to follow more closely both cultural and class cleavages rather than sectoral cleavages. In short, although economic analysis may point to what can be called "objective" sectoral clashes inferred from the distribution of income, political behavior on a permanent basis through political parties is not often organized along these lines; although economic analysis may point to an "objective" common interest within the dominant (often industrial) sector and to increasing conflict within the suppressed or neglected (often agricultural) sector, political behavior analysis generally points to political conflict between labor and management, and to political cooperation in the rural sector prior to the advent of effective social and political mobilization.⁶

Must one, then, abandon the empirical relevance of the sectoral approach for sociological and political studies, and relegate the theory's utility only to the level of heuristic devices, especially for economists? I think not. As suggested

earlier, in countries where these assumptions can be approximated, particularly in certain Latin American countries, there are at least four levels in which a sectoral view may be important and useful. First, the theory of sectoral clashes, as Markos Mamalakis has presented it, has an important time dimension. It seeks to explain not merely a particular array of forces at a given point in time but also to suggest changes in coalitions, levels of governmental activity, and the appearance or disappearance of particular sectoral clashes. It may be hypothesized, therefore, that particular kinds of sectoral clashes—of the more extreme sort—may be characteristic of the period of transition from mostly primary economic activities to greater reliance on secondary economic activities. If this is the case, then more extreme forms of sectoral clashes leading to discrimination against agriculture in Perón's Argentina and Gomecista Venezuela, or against copper under the Chilean Radical governments, may be significantly modified subsequently, such as the post-Perón governments have done to some extent in Argentina, such as the new deal for Chilean copper and then Frei's Chileanization policies suggest, or the revitalization of Venezuelan agriculture during the last decade also supports.⁷ During these particular periods in the process of modernization in these countries, there has been a greater tendency for sectoral behavior in politics than at other times. The theory of sectoral clashes includes a prediction that such clashes will tend to moderate in more advanced phases of economic development.

Secondly, sectoral behavior may appear on an intermittent basis as a feature of interest group politics. Otherwise opposed political groups may coalesce temporarily for limited goals, such as approval of a tariff which may protect both management and labor in a particular branch of industry, or construction of a road which may help both rich and poor farmers of a given area. Thirdly, although Markos Mamalakis' presentation includes a great deal of group disaggregation (much more than can be usually found in the literature), one of the more intriguing and useful features of his approach may lead in the direction of further disaggregation. As Mamalakis suggests, it is important to distinguish domestic-owned and foreign-owned sectors of the economy; it may also be necessary, particularly in countries with large Indian populations where a goal of the government is to increase incentives for linguistic and cultural assimilation, to distinguish between subsistence and commercial agriculture (this is but another example of the way in which cleavages of a cultural or national sort affect what might otherwise appear to be a strict economic affair), to distinguish within the service and industrial sectors between public and private sub-sectors, to distinguish within the industrial sector between artisan, labor-intensive enterprises and capital-intensive modern manufacturing (particularly with respect to such issues as tariff policies on importation of machin-

ery and equipment, applicability of costly welfare legislation) and so forth. These conflicts within particular sectors may be as important, from a political point of view, as conflicts among sectors. John Johnson's hypothesis on the genesis of Radical parties in Argentina and Chile,⁸ for example, stresses a coalition of "middle sectors." Although Johnson's use of the term "sector" is different from Mamalakis', the two conceptions would approach each other more if Mamalakis' process of disaggregation within sectors is systematically extended. Such an extended disaggregation, in short, would entail making use of the concept of social and political distance in addition to one of economic distance. For example, although the economic distance between bureaucrats of the service sector and small industrialists may be great, their social distance may be small. This may lead to a sectoral clash against mining or agriculture. But large industrialists, particularly if they are foreigners, need not benefit from such a coalition. White-collar and blue-collar workers may benefit temporarily from such arrangements producing a sectoral coalition; but they may also perceive a greater social distance from their economically close sectoral partners.

And fourth, the earlier part of the commentary indicated the existence of agglomerative parties formed on non-functional cleavages, such as ethnic or religious parties. That analysis also indicated that the appeal of these parties, in general, is multi-class and multi-sector, even if, for example, blue-collar workers may tend to vote more for working class parties. The importance of a sectoral view for the analysis of these parties is the emphasis that would be placed on coalition formation and bargaining within each such party. Cross-pressures within non-functionally based parties are strong because of the pull effects of other functionally based parties in the same party system. Thus pressures on Belgium's or Italy's or Chile's Catholic parties are strong both from the right and the left, and appeals are made to professionals and businessmen and to blue-collar workers to break their religiously based pattern of political behavior. The survival of the Catholic party under such circumstances depends on its ability to reconcile the conflicting functional interests within its midst, including symbolic stress on harmonious, trans-class, trans-sector, cooperative and communitarian ideologies. But the distribution of benefits within the parties, nonetheless, includes the need to reconcile multiple interests, both sectoral and class. The contribution of a sectoral perspective is, therefore, to add a new dimension to the analysis of bargaining within these non-functionally based parties.

In summary, this social and political commentary on Markos Mamalakis' essay on the theory of sectoral clashes has focused on two main problems. The first is the tendency to neglect the entire array of non-functionally based cleavages in a society, which may lead to non-functionally based motivations for

individual and governmental behavior. The second is the tendency to go much beyond the analytical value of the theory of sectoral clashes to suggest or imply that sectoral behavior is the principal mode of social behavior, relying on the concept of economic distance to unite sectors within and to separate sectors from each other to the expense of the concept of social and political distance which stresses linkages across sectors and differentiation within sectors. The commentary has stressed cultural and class factors in order to sharpen the discussion. But then at least four ways to use sectoral theory for social and political analysis were suggested. The first would link extreme sectoral clashes and sectoral behavior with a particular period in the process of modernization; the second suggests interest group sectoral coalitions for limited purposes at particular points in time including (but not limited to) the period of transition to a more industrial economy; the third points to a continued process of group disaggregation to benefit from at least a limited convergence of the theory of sectoral clashes and other prevalent sociological and political approaches; and the fourth suggestion focuses on the utility of a sectoral perspective for the study of coalition formation and bargaining in non-functionally based political and social organizations. Throughout the entire discussion, the concept of cross-pressures has been an underlying theme: individuals are subject to pressures from different sources which result from multiple loyalties and multiple membership in different social categories. The importance of presenting a sectoral theory is to suggest the need to consider carefully yet another aspect of cross pressures which is not emphasized often enough.

NOTES

1. The basis of this commentary has been, primarily, the essay by Markos Mamalakis entitled "The Theory of Sectoral Clashes," Center Discussion Paper No. 19, of the Latin American Center of the University of Wisconsin-Milwaukee, dated April 18, 1969 which is published in the present issue of LARR.
2. See Karl Deutsch, *Nationalism and Social Communication* (Cambridge, Mass.: M.I.T. Press paperback, 1966); Rupert Emerson, *From Empire to Nation* (Boston: Beacon Press paperback, 1962); and Dankwart Rustow, *A World of Nations* (Washington, D.C.: Brookings Institution paperback, 1967).
3. R. R. Palmer and Joel Colton, *A History of the Modern World* (New York: Alfred A. Knopf, 1964), p. 63; Howard Wriggins, *Ceylon*, (Princeton, N.J.: Princeton University Press, 1960).
4. Robert Dahl, ed., *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966), Chapters 5-7, 10.
5. For an excellent discussion of cleavages and party alignments, see Seymour M. Lipset and Stein Rokkan, eds., *Party Systems and Voter Alignments* (New York: The Free Press, 1967), Chapter 1.

6. This is not the place to give a bibliography of cleavages and party systems. Nonetheless, the books edited by Dahl, *op. cit.*, and Lipset and Rokkan, *op. cit.*, not only cover most of the party systems in Western Europe, North America, the "old" British Commonwealth countries and Japan, but also give more extensive bibliographical references for these and other areas. For political parties in Africa, see James Coleman and Carl Rosberg, Jr., eds., *Political Parties and National Integration in Tropical Africa* (Berkeley: University of California Press, 1964), which also includes a longer bibliography on the subject. On a broader comparative perspective, see Gabriel Almond and James Coleman, eds., *The Politics of Developing Areas* (Princeton, N.J.: Princeton University Press, 1960). This book includes analyses dealing with Asia, Africa and Latin America. For some considerable attention to Asia and Africa, see also Emerson, *op. cit.* On parties in Latin America, again the list could be exceedingly long but we will merely suggest a few titles which bear on these propositions, most of which have useful bibliographies: John J. Johnson, *Political Change in Latin America* (Stanford: Stanford University Press paperback, 1965); John D. Martz, *Acción Democrática* (Princeton: Princeton University Press, 1966); Robert Scott, *Mexican Government in Transition* (Urbana: University of Illinois Press, 1964); Federico Gil, *The Political System of Chile* (Boston: Houghton Mifflin Co., 1966); Robert Dix, *Colombia* (New Haven: Yale University Press, 1967); Gino Germani, *Política y sociedad en una época de transición* (Buenos Aires: Paidós, 1962); John D. Martz, ed., *The Dynamics of Change in Latin American Politics* (Englewood Cliffs, N.J.: Prentice-Hall, 1965), Chapters 18–27.
7. Markos Mamalakis and Clark Reynolds, *Essays on the Chilean Economy* (Homewood, Ill.: Richard D. Irwin, 1965); Arthur P. Whitaker, *Argentina* (Englewood Cliffs, N.J.: Prentice-Hall, 1964); Tomás Fillol, *Social Factors in Economic Development: The Argentine Case* (Cambridge, Mass.: M.I.T. Press, 1961); Federico Brito Figueroa, *Historia económica y social de Venezuela* (Caracas: Universidad Central de Venezuela, 1966), Vol. II; and Oficina Central de Coordinación y Planificación de Venezuela, *Plan de la nación, 1963–1966* (Caracas: 1963), pp. 25, 34.
8. Johnson, *op. cit.*

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Economists have long felt the need to *disaggregate* our existing conventional national income accounts into relevant sub-groups sharing a common technology of production or a common market for product-disposal. The broadest possible set of such groups is furnished by the "sectors," i.e., agriculture, manufacturing, mining, export, etc., which have the special advantage of wide international use and hence provide a ready basis for international comparison. Economic development by its very nature, as Schumpeter urged in nearly all of his writings, is uneven, involving thrust-outs of new activities and techniques which will usually be indicated, if not clearly exposed, in broad sector accounts.

Government fits into sector analysis in two special ways. Government first carries on the process of production for the satisfaction of collective needs and wants and to a considerable extent for individual wants, thus displacing pri-

vately conducted activities. In this capacity government defines the locus of control or ownership of what may be called the *public economy* which defines a sector of the economic system. But the influence of government in our time extends far beyond performance of production activities. The influence of government is also regulative, and redistributive, achieved by many means: taxation, export subsidy, import control, use of credit and money, limitation, prohibition, etc. In these respects the activity of government is not economy-wide in its impact but is often or typically interventionist in form and impacts unevenly, promoting some activities, industries, classes, areas, neglecting others, suppressing still others. This *selective* mode of operation of government is brought forward in the Mamalakis theory to express the basic fact that government is not only an *agent* of social control but an *agency* through which social and interest groups operate and by means of which they seek to realize their objectives.

This struggle for the control of government to realize group interests and aspirations is by no means confined to issues of economic development. The purposes for which this control is sought are usually redistributive and protective: to increase the share of income going your way and to improve the environment and facilities by which group life is carried on. But though growth may not be the dominant objective of struggling social groups, the effect of group dominance and control on growth is a crucial consequence since it conditions the entire economic process as it moves into the future. This is a major justification for linking the theory of sectoral conflict with growth.

I use the term "sectoral conflict" rather than "clashes" because the latter term connotes violent collision and encounter. In certain other respects a modification of semantics may be in order. Thus, "flight" of resources invested in ground and buildings and other improvements on land can only occur by the slow process of under-maintenance and withholding of replacement investment. Only liquid capital and funds invested in inventories of staple goods are really capable of rapid territorial transfer. Similarly, some highly problematic inferences are labelled corollaries, a term traditionally reserved for propositions requiring no additional proof following upon a more basic proposition just demonstrated.

The social and interest groups which compete for control of government are not always national or nationwide *in content*. They may be *regional* in form where a given mode of production or industrial activity is concentrated within a certain region. The regional dimension is often reinforced by lines of political jurisdiction which permit provincial government a certain range of authority and by ethnic and racial division which often reinforce the tendency to regionalism. Proper regional breakdowns are not always provided in income

accounts but the analyst must use his materials and not let his materials use him.

The interest groups which may be regional in layout will oftentime come to a focus in an industrial activity or product area which may cut into a number of sector accounts or be concealed only in one. Thus activity in the *construction* industry, which is the basic sector of national accounts, will oftentimes be dominated by the activity of housing both because demand for new residential housing will generate a complementary demand for an outfitting of ancillary urban structures of commercial, educational, and public character. Housing is also frequently a strategic objective of government because it concerns fulfillment of a basic want for shelter. In a similar way British industry of the 16th and 17th centuries was dominated by activity relating to wool, both in the animal husbandry which made sheep farming profitable, and the sequence of activities which made English yarn and cloth prime exports. Yet the woolen industry was only a branch of textiles but in its farthest reach it comprehended farmers and landlords, craft processors, dyers and weavers, clothiers, and merchants and shippers, the whole drawn from many sectors. A more modern example of how a dominant industrial activity comes to focus in a *product* area will draw together processors and producers located in many sectors of national accounts is that of the automobile. Automobile manufacturers are the dominant parties in the auto interest but they lead a broad following: highway builders and road builders, public highway and road departments, parking garages, repair garages and car service stations and auto shops, used and new car dealers. The some \$60 billions of used cars owned by consumers in 1960 generated a byproduct medical-care industry in hospitals and cemeteries to take care of the accident loss generated by the automobile. Police departments increasingly turned to traffic control as one of their major duties; and courts of law have been turned into arbitration tribunals for accident claims. All of these make up a totality of interests servicing directly and indirectly the automobile and involved in an automotive interest.

Groups within sectors, groups with a regional focus, groups interlacing across sectoral lines dominate the surface play of group interest in the day-to-day work of government. Over a long period of time it will be seen that groups coalesce on certain questions and that running through certain group activities are broad themes which persist in time and develop a focus of their own. That focus whenever it is touched or brought into action is called a *class interest* which, though it often lies undisturbed, exerts a steady gravitational pull on the state and its activity. It is the class interest which governs the basic layout of institutions, the powering of an economic system, the protection of future interests and status positions.

Sectoral conflict, group struggle and class interest are concerned about

control of the state for the regulation and redirection of economic life. The state however is not all-powerful for these purposes even in communist-controlled societies. Certain kinds of activity cannot be coerced and the communist state has learned to use rather than suppress the market, wherever that is possible. Where official bureaucracies are more loosely constructed or where the sense of individual freedom is more fully cultivated or where the power of the state is not regimented as throughout the Western world, the power of the state over economic life will be even more limited. The ability of the Western state over a prolonged period of time to enforce wage and price controls over producers and workers in a national economy is very limited, especially where family producers, or small firms are concerned. There is a limit too in what can be collected in taxes once income is apportioned out among workers, business men, and property owners. There is not much scope for income redistribution among sectors and classes when income tax laws are poorly enforced and where there is little or no property taxation. The theory of sectoral and group clashes for control over government must in each case come to a proper estimation of the real scope for government control.

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The Theory of Sectoral Clashes of Markos Mamalakis represents an important attempt to return to one of the fundamental issues of sociology and political economy: how to meaningfully and usefully characterize societies in change. In so doing Mamalakis is reopening the issues raised by Marx's effort to classify changing societies in terms of their modes of ownership and means of production. Mamalakis' concern with income groups inside sectors is similar to Marx's interest in the impact of ownership upon the social relations of productive groups, and Mamalakis' focus on economic sectors and their transactions raises issues which Marx related to the means of production and the division of labor.

This theoretical effort seems to be particularly appropriate at the present time, since empirical studies are focusing attention on very similar phenomena, namely, the emergence of coalitions and conflicts between various social groups each of which is "class" in Weber's sense of sharing a common economic position. The outcome of political struggles has been found to depend in large part on the emergence of such coalitions and clashes and upon their impact on the distribution of national resources.

The highly regarded work of Barrington Moore on the social origins of dictatorship and democracy is one such example. Moore analyzes the development of political systems in Germany, Japan, France, England, the United

States, and India in terms of the emergent coalitions which led either to the dominance or destruction of estate agriculture and serfdom. His analysis bears strong similarities to Trotsky's work on the social origins of the Russian Revolution. Other studies concerned with such coalitions and conflicts include the work of Clifford Geertz on Indonesia, Dudley Seers on Africa, Harry Eckstein on Norway, and James Malloy on Bolivia.

In all of these studies, the emergence of socio-economic groups with a conscious identity and particular economic interests is treated as an empirical or historical given, rather than as a phenomenon expected on the basis of a theoretical model. Nevertheless, the congruence of such empirical findings suggests that theoretical formulations which account for such developments should not be far off. The Mamalakis Theory of Sectoral Clashes is a highly promising step in this direction.

The role that conscious income-group or sectoral identity plays in the development of conflict and coalition should not be overemphasized. Some criticism of the Theory of Sectoral Clashes has suggested that insofar as sectoral consciousness is one of the types of consciousness which forms social groups and influences their relationships, it may play some role, but that sectoral consciousness is generally not an important aspect of political struggle. The thrust of the Theory of Sectoral Clashes, however, is in another direction. As it is developed, it may help us predict which social groups will emerge as a result of economic change, and what the underlying conflicts of interest are likely to be. As classical economics has shown us, conflicts of interest, or coalitions of interest, need not be conscious to have a real impact upon the nature of the society in which they operate. In fact, they may be more often unseen than recognized. Thus industrial unions and industrial management may fight each other like cats and dogs, but the end result is likely to be an increase in industrial prices which benefits both at the expense of groups in other sectors. The Theory of Sectoral Conflict therefore helps focus our attention on the way in which economic conflicts and coalitions influence the availability and distribution of resources in the nation, which in turn help form the context within which overtly political action takes place.

To be sure, the consciousness of sectoral interests is itself an important variable which can decisively alter the nature of political life when it does in fact occur. Since Mamalakis devotes considerable attention to this development, it is understandable, though perhaps regrettable, that it has been seen by a number of scholars as *the* primary, rather than *one* of the primary, effects of sectoral coalition and conflict.

My own criticisms of the Theory of Sectoral Clashes have to do with whether or not it is in technical terms a theory as it now stands. Generally

speaking, a theory sets forth a taxonomy, or a number of key terms, which are rigorously defined and related to each other by assumptions and axiomatic statements. From these theoretical statements, assertions are logically derived, which specify relationships or outcomes in the form of hypotheses to be tested with empirical data.

In its present form, the Theory of Sectoral Clashes is primarily a taxonomy, on the one hand, and a series of hypotheses, on the other. As a taxonomy, or a series of concepts which assert to organize reality in a useful manner, it is extremely valuable, for, as I have suggested, it focuses our attention upon the most basic aspects of social change. As a series of hypotheses, it suggests a number of stimulating and insightful relationships between political and economic phenomena in developing nations. Nevertheless, it lacks the rigorous theoretical definitions, postulates, and axiomatic statements which allow us to derive the hypotheses from the taxonomy. As a result, it is still dissatisfying. Many of the statements of relationship which would transform the taxonomy into a theory are implicit in Mamalakis' articles, but much work remains to be done before this material is in fact formalized into a theory.

Somewhat paradoxically, perhaps, it also seems to me that the development of the Theory of Sectoral Clashes stands to gain a great deal from more empirical research. We have much to learn about the manner in which economic interests are formed and in turn form a nation's resources. Continuing investigation into these problems is likely to play an important role in specifying the relationships between the variables included in the theory.

Despite these reservations, the Theory of Sectoral Clashes is an important and impressive attempt to reopen the key issues of economic and social change. Some of the most valuable work now being done in this area is based upon the analysis of coalition and conflict, and the Theory of Sectoral Clashes may provide us with the theoretical underpinning needed to significantly advance this work.

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Mamalakis' valiant effort to present an inter-disciplinary explanation of the patterns of growth in developing economies must have been born from the author's dissatisfaction with various sets of analytical tools presently used for that purpose. This dissatisfaction has been shared by many who realize that, while alternative structuralist, monetarist or Marxist approaches (to mention a few) offer useful insights into some aspects of economic behavior of the under-developed societies, none of them explains it fully.

To this writer the novelty of Mamalakis' approach consists of looking

upon the process of economic growth from the viewpoint of the impact of policy decisions upon resources allocation. Since, on the one hand, policy decisions have roots in both 'economic' and 'non-economic'—often contradictory—motivations, and, on the other, resources are finite, the intense struggle for control over resources develops among competing social groups, here called sectors. This struggle determines the relative position of different sectors over time and, consequently, sets the given path of growth or stagnation.

The Mamalakis image of struggle and conflicts underlying the process of economic modernization or its absence differs considerably from the simplistic Marxist notions of class struggle for income shares with the predictable outcome. This image is intuitively perceived by most students of economic growth processes. The fact that we are still unable to dissect the maze of mutual conflicts, to reconstruct their sequence and to analyze their impact upon growth patterns is another matter. It is not that the economic tools are missing. It is that most probably economic analysis, whether neoclassical or any other, assumes away too much under the *ceteris paribus* clause.

Under these conditions those interested in the general problem of why—given comparable initial endowment in production factors—some economies grow while other stagnate are left in uncomfortable positions. To give an example: we can measure the growth of the Australian economy and the stagnation of Argentina in the past thirty years, but we are at a loss to explain the differences in respective performance.

Assuming that we do not want to limit ourselves exclusively to the measurement and the description of growth, it may well be that we would advance considerably in the search for explanations of economic processes by following Myrdal's dictum, contained in his *Economic Theory and Underdeveloped Regions* to the effect that "the distinction between factors that are 'economic' and those that are 'non-economic' is, indeed, a useless and nonsensical device from the viewpoint of logic, and should be replaced by a distinction between 'relevant' and 'less relevant'."

This is what Mamalakis attempts in his theory of sectoral clashes. While the introduction of certain 'non-economic' factors into Mamalakis' work gives it refreshing quality, his theoretical exercise still ascribes to these factors a priority lower than one might hope for. Mamalakis makes his position clear at the very beginning of his essay: "I single out—he says—the competition between economic sectors—sectoral struggle—as the moving force behind growth. . ." But, what if the patterns of growth are the result not only of the competition among economic sectors (assuming the subordinated presence of some 'non-economic' factors) but of the interplay of 'non-economic' but relevant forces as well? While the Mamalakis paper suggests that this is what in

real life happens, he stops short of the 'non-economic' abyss. Applying the theory to its author one might say somewhat facetiously that Mamalakis (taking upon himself the role of "governmental-final arbiter") gives preferential treatment to one of the sectors participating in the process of growth—the purely economic sector—and discriminates (by neglect perhaps and not by overt suppression) against the remaining ones. The result is—to use again Mamalakis framework—the suboptimal growth of his theory, not in the terms of its internal logic but in terms of its confrontation with "observable" facts in what economists very conveniently for themselves qualify as backward or underdeveloped economies.

Speaking in a more serious vein, three major aspects of the Mamalakis *tour de force* call for further comments:

- a) "non-economic" explanations of the particular sectoral preferences reflected in Latin American economic policy decisions;
- b) the definition of a "sector," and
- c) conclusions and policy recommendations.

Apparently it is not enough—as Mamalakis does—a) to note the appeal of a "forced" industrialization concept in Latin America; b) to postulate, for example, that suppression of agriculture, the sector normally confined to a subordinate role in less developed economies, "has been partially the result of resentment against the conservative, landed oligarchy, partially an expression of anti-imperialist feeling (whenever there exist foreign-owned plantations), partially the result of disillusionment with agriculture as a growth and export sector during the depressions", or c) to state that "suppression of the mining sector, with its heavy foreign investment has been inspired by anti-imperialist doctrines especially as it is identified as a 'foreign' sector". While there is some truth in all these observations, the rationale behind the sectoral growth priorities has been in Latin America as elsewhere considerably more complicated. The explanation might be sought not only in a perhaps distorted vision prevailing in Latin America in respect to the relative importance of different sectors for the purpose of achieving accelerated growth, but in the performance of the world economy since the Industrial Revolution and in the changes in power structure of Latin American societies during the past fifty years.

After all, it is not Latin America that invented the slogan "power to industrial entrepreneurs". The notion that industrialization is the main road to growth and to power (in the 'non-economic' sense as well) has been borrowed by Latin Americans from the industrial countries of today. Not only was Latin America hardly inventive in the matter of developmental doctrines, but the region showed particularly few inventive skills in respect to policy tools that

led to the appearance of the industrial dominant sector in most of the region. If Latin Americans were (and perhaps still are) mostly poor imitators in respect to development ideology, quality of policy tools, and long-term policy performance, then the problem of taking the region out of the bog of underdevelopment may be solved not so much by restoring inter-sectoral balance through the more neutral behavior of the “supreme arbiter”, as Mamalakis seems to suggest, but by changing non-economic, historically-conditioned parameters responsible for poor policy tool-making and poor growth performance. This would take us into the field of study such non-economic factors as social and political structures prevailing in Latin America, the “suppressed” sector of the Mamalakis theory.

Parenthetically, one may note that Mamalakis seems to weaken his own case for stressing economic factors by stating with fairness that both dominance and suppression-directed policies may be efficient or inefficient as promoters of growth in concrete cases. While recalling in one of the earlier versions of his essay the British enclosure movement and Japanese agriculture, he is nevertheless not too optimistic in respect to the working of the mechanisms of domination or suppression in Latin America. Some interesting, albeit scattered, thoughts appear in the paper on the subject of technological change and institutional transformation. One might perhaps suggest to Mamalakis that going deeper into these matters would enrich considerably his exercise. It might well be that the proven inability for institutional transformation—due to the combination of the socio-political colonial heritage and the neo-colonial framework within which most of the Latin American societies operate—and the very peculiar patterns of technological change (due to value systems and consumer preferences, for example) might help us to understand why “dirigist” economic policies give such disappointing results in the region. It is particularly fascinating to note that in few other areas is there so much rhetoric about growth and so little growth at the same time.

The question of defining a sector seems very difficult to tackle. A sector has been originally defined as “a group of factors of production producing one or a series of similar products” or, presumably, services. Most of the general analysis and most of examples of sectoral clashes and conflicts and their consequences for the patterns of growth, as offered in the essay, fit this definition. The first conceptual difficulty arises, however, with his treatment of government, because government both is, and is at the same time not, a sector. It is obviously a sector in some situations, because it commands a particular set of production factors. But it is not a sector when it starts acting exogenously as policy maker, “supreme arbiter,” or an active partner of a sectoral team in allocating resources between different sectors. This double economic and non-

economic role of government does not seem to be clearly analysed. There is an urgent need for such analysis and for more careful definitions particularly because the Mamalakis landscape is inhabited not only by sectors as originally defined but by others as well: the public, the private, the domestic and the foreign sector, all of them sectors in a sense different from that of the original definition.

A further and very important question is left unanswered: what is the source of the power of a government acting as "supreme arbiter"? A possibility of additional serious conceptual difficulties is suggested by passages which assume the existence of an *ex-ante* and an *ex-post* government (before and after being taken over by a dominant sector). Is a government a technocratic entelechy with a life of its own; is it a reflection of balance of power between different socio-political groups; or is it just an instrument of some dominant power groups which it serves in various ways including by "nationalization"? Can government ever become neutral (as the author would like it to be)? Mamalakis may believe that such questions should be posed to political scientists rather than to economists like himself. But since his theory enters in the field traditionally reserved to other social disciplines he is liable to answer the questions he raises.

The problem of handling sectors becomes more and more complicated as one proceeds to study the Mamalakis paper carefully. The author's knowledge of the socio-economic reality tells him that the division of the economy into *major* sectors may be construed as a crude abstraction. Consequently, he introduces other actors on the scene. The banking system and central banking authority appear, for example, as participants in coalition and clashes. Again, one is prone to ask: who are they? The banking system has all the characteristics of a subsector, and the central bank is obviously a subsidiary organ of government—the "supreme arbiter." But how they fit into the general scheme of clashes as separate participants is not clear, at least not to this commentator.

In respect to the essay's conclusions and recommendations, it seems that Mamalakis has abandoned his earlier "radical" position that all sectoral clashes should be eliminated as all affect negatively the process of growth. Not only does he accept in the concluding chapter that "sectoral competition is likely to persist," but ventures an opinion that sectoral competition (presumably mild sectoral clashes) should persist "since it provides moving force." A few pages later, he elaborates further on the same point, stating that "sectoral clashes, like inflation and balance of payment deficits, can be tolerated as long as they are the lesser of the two evils," another one being "the employment of growth decline which their elimination might entail." Mamalakis would thus remove only those sectoral clashes that may sharpen constraints upon savings, entre-

preneurship or shortages of capital goods. In brief, he would like to see more 'natural' resource allocations through market forces and less unbalanced economic growth patterns.

In one of the footnotes he admits that both his theory and his policy preferences have roots in the neoclassical image of the world of economics. "There is a basic similarity—Mamalakis states—between post-Marshallian neoclassical value theory and the theory of sectoral clashes. The former stresses deviations from the norm of pure competition caused by non-governmental action, while the latter stresses the implications of government intervention wherein a particular sector is promoted far beyond the limits set for allocation of resources in conformity with the existing market structure."

While it might be a fascinating exercise to probe deeper into these doctrinal underpinnings of Mamalakis theory, a double question might be perhaps raised in respect to their applicability to Latin American conditions. Could intersectoral equilibrium and better growth performance be achieved in the region by decreased government intervention in the economy and by leaving to market forces the allocation of resources, or are the distortions observed mostly due to the "wrong" kind of government intervention reflecting short-run interests of particular power group coalitions that may be interested in things other than growth? If the second proposition were true, then as Charles Anderson notes in his comments, Mamalakis' postulation of a "mercantilist government, which advantages one sector over another for reasons of state" would represent a simplification, at least for the study of the politics of development in Latin America. Before deciding upon policy recommendations, Mamalakis may be forced to try harder to work out some kind of marriage between his theory of sectoral clashes, neo-Marxist class conflict theory, and other approaches based upon case studies of Latin American politics. Eventually, we may have a more complicated although less original theory that would have the advantage of squaring better with "observable" facts.

Without any malice, this comentator would like to point out that the essay is weakest when it tries to support certain models of sectoral clashes with available Latin American evidence. Mamalakis seems to be working with a particular vision of a Latin American country that clearly suggests the Chilean urban multi-class society which he knows from first-hand experience. While no such thing as a "typical" Latin American society exists, Chile is perhaps more atypical than others.

Since the task of working out an interdisciplinary approach to the politics of economic development is very difficult, at some point Mamalakis will have to opt, perhaps, for joining forces with his "non-economic" colleagues to make his theory more applicable to Latin America.