

**The Place of Cultural Explanations and  
Historical Specificity in Discussions of Modes of  
Incorporation and Segmented Assimilation**

by

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## ABSTRACT

This paper serves as an opportunity to pull together some thoughts and questions about modes of incorporation as an explanation for ethnic differences in behavior. Specifically, I ask just what is the status of cultural explanations for ethnic behavior if ethnic behavior is approached from a modes-of-incorporation perspective. I ask this question both in connection with individuals of the immigrant generation as well as in connection with the second generation; the concern with the second generation leads me to consider the status of cultural explanations for ethnic behavior in connection with the related conception of segmented assimilation. My argument proceeds through four steps. 1) I note that the modes are introduced as a way out of being left with a large ethnic residual (or unexplained difference) from individual-level analysis and as one more way of contradicting the claim that the residual reflects the operation of independent cultural differences among groups. 2) I stress how far we can push the corollary that living in different modes can effect not only the structural opportunities available to a person but also the attitudes, values, and outlooks common in people from different groups. 3) I also stress the possibility that many specifics of an immigrant group's historical experiences are not captured by the modes of incorporation (as would be true of any typology), and that such historical specifics ignored by the typology might matter a great deal. Moreover, such historically specific features may involve cultural characteristics as well as other characteristics, cultural characteristics related not at all or only tangentially to the aspects of experience discussed in the typology of the modes. 4) A big question, from this perspective, then, is: how well do the modes in fact explain the residual ethnic differences unexplained by the individual-level variables? And how do we answer that question empirically?

I want to use this opportunity to pull together some thoughts and questions about modes of incorporation as an explanation for ethnic differences in behavior. My approach is highly idiosyncratic, in that I first describe briefly the logic of my own efforts (of a decade ago) to sort out cultural and structural influences upon ethnic behavior (Perlmann, 1988), and then show that the concept of modes of incorporation is meant to surmount just the sort of constraints inherent in approaches such as the one I had taken. For this reason the concept of the modes and the development and elaboration of that concept is of great interest to me; and so I try to go a step farther and ask explicitly just what is the status of cultural explanations for ethnic behavior if ethnic behavior is approached from a modes-of-incorporation perspective. I ask this question both in connection with individuals of the immigrant generation as well as in connection with the second generation; the concern with the second generation leads me to consider the status of cultural explanations for ethnic behavior in connection with the related conception of segmented assimilation.

Another issue also runs through this paper, namely the contrast between the demands of historical specificity and those of a broad explanatory framework (in this case the modes of incorporation) that relies on a relatively small number of explanatory elements (those in the typology). The questions about the status of cultural baggage leads back to this issue of historical specificity; when I ask about the existence of premigration cultural patterns that seem to have originated in historical circumstances one could not have foreseen simply by invoking the modes-of-incorporation typology. And more broadly, the issue of historical specificity arises when I ask just how much of what needs to be explained can in fact be explained by the modes of incorporation. To put it another way, the historical specifics of each group's

premigration situation can hardly be fully described by the typology; surely it is plausible that some of these historical legacies might continue to influence the group's members after migration. If these historical legacies do continue to influence the group's members, and if these historical legacies are not merely aspects of the class structure in the country of origin, than they may well turn out to be cultural sources of behavior unrelated to the class features stressed in the typology. And in any event, just how important are the historical legacies not captured by the modes (whether cultural sources of behavior or not)?

For the sake of simplicity I focus almost entirely on the discussion of modes of incorporation as it appears in the second edition of Alejandro Portes and Ruben Rumbaut's *Immigrant America* (1996), which I think is a recent, self-conscious and subtle effort to summarize an evolving body of remarkable research and reflection. I want to make it clear that there are considerable gaps in my reading of related scholarship; for that reason too focusing on the single exposition is useful, but it seems fair to warn the reader that this strategy may not protect against all blunders.

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Much early work on ethnicity tried to show that differences in ethnic group behavior could be explained by appeals to differences in attitudes, outlooks and values that were thought characteristic of different groups. Other, typically later, work stressed the empirical limitations and internal contradictions of these cultural explanations -- or still worse faults, such as self-congratulation, patronization, and arguments that were disturbingly parallel in nature to older biologically-based racial theories. The critiques of the cultural interpretations focused on ethnic behavior as a reaction to discrimination in the wider society and especially on ethnic behavior as

a result of structural location, and most especially the social class location of ethnic group members. A long tradition in explaining differences in ethnic behavior has wavered between, or tried to weave together, these competing forms of explanation, cultural and structural.

In empirical terms, the challenge often takes the following form. Measures of social structural characteristics are taken – father’s occupation, number of siblings, years of schooling and so on. If measures of cultural values can be obtained, those can be added to a multivariate analysis; if measures of cultural values are not found (and this is all too often the case) then the residual ethnic difference observed in ethnic behavior – that is, the difference in ethnic behaviors remaining unexplained when structural characteristics of individuals in different ethnic groups had been taken into account -- needs to be interpreted. Interpreted as -- as what? Well, one possibility is to follow Barry Chiswick, and refer to the result as “the ethnic effect.” As Chiswick may have meant to imply, and as *Immigrant America* stresses, to say that the residual is associated with the ethnic group is tautological; to say that the residual is proof of cultural differences between ethnic groups is a highly questionable theoretical leap.

This is as far as many discussions of the explanations for ethnic differences go. One further step in the logic of explaining ethnic differences is worth mentioning before turning to *Immigrant America*, namely the relevance of contextual factors as opposed to individual-level factors. At the individual level, we might compare second generation Mexican and Korean immigrants and take into account the fact that the Koreans are far more likely to have had parents who were professionals and parents who were petty proprietors than are the Mexicans. If we then compare second generation Mexicans and Koreans whose parents were in roughly the same social class positions, we eliminate the impact of this difference in individual level

characteristics, but not the difference in the contexts within which the Mexican and Korean second-generation members will grow up. There are many ways in which being part of a community of professionals and petty proprietors is different from being part of a community of immigrant laborers – even if the class position of individual families from each group are taken into account. These differences in contexts could well be both structural and cultural in nature (Perlmann, 108-112, 211-13).

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One can appreciate, I hope, what the modes of incorporation offers to someone who approached ethnic behavior in the manner I just described; the modes explain why individual-level social structural characteristics do not capture all that needs to be explained about ethnic differences, and give a meaning to the ‘residual ethnic difference’ observed in multivariate analysis of individual-level data. The concept of the modes elaborates the notion that the social context is important in ways the individual-level data cannot capture -- elaborates, specifies and systematizes that previously vague ‘notion.’

So the residual ethnic difference need not be attributed to a distinctive cultural characteristic distinguishing one group from another; rather, the residual may reflect differences in the benefits that particular contexts offer individuals. I will first show briefly that approaching the modes of incorporation as an alternative to the limits of individual-level explanations (including cultural explanations) is exactly how *Immigrant America* in fact introduces their value, and then raise some questions that follow from this approach.

The modes are introduced in Chapter 1 of *Immigrant America* and are typically referred

to as a rough and preliminary typology (rather than a full explanatory theory<sup>1</sup>). However, the powerful presentation of the modes, as an explanation of ethnic differences in socioeconomic outcomes, and as a way of transcending the limits of individual-level analysis, is left to Chapter 3, entitled 'Making it in America.' The first half of Chapter 3 comprises a survey of the evidence on immigrant (and in some cases later-generation) socioeconomic attainments – especially in education, occupation and income. And in connection with each of these measures of attainment, the survey includes a subsection presenting a review of multivariate evidence from individual-level analyses. The point is always the same: the controls for individual level variables do not explain the "ethnic effect" at all well. Education: "These persistent differences suggest the existence of broader cultural or social factors, not captured by the analysis of individual variables that affect the collective performance of each group." Occupation: "As in the case of education, these factors [i.e.: microlevel factors] do not account at entirely for differences in occupation among either individuals or nationalities, a result that suggests again the presence of broader cultural or structural forces." Income: "This low ability of predictive models based on individual variables to explain differences within and across immigrant groups indicates, once again, the need for an alternative and more encompassing explanation. This task must necessarily focus on factors other than those employed by prior studies, incorporating variables at a broader level of analysis."

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<sup>1</sup>In a recent article Portes (1997) makes the distinction explicit ("typologies are not theories" and the modes are a typology. However, I do not think the distinction is important for my purposes here; rather, as I emphasize below, the relevant section of *Immigrant America* is called 'Explaining the Differences.' Whatever form of 'explanation' is intended there is the form of explanation I am discussing here.

The very next words comprise the title for the second part of the chapter: "Explaining the Differences: Modes of Incorporation." Shortly thereafter comes the elaboration: use of these modes "is a way to overcome the limitations of exclusively individualistic models of immigrant behavior... [The different modes] can help explain differences ... among immigrants who are statistically 'equal in a host of individual characteristics.'" From here the authors detail what it is about each mode that gives it explanatory power.

I want to make several interrelated points about the modes. 1) I've already argued that they are introduced as a way out of being left with a large ethnic residual (or unexplained difference) from individual-level analysis and as one more way of contradicting the claim that the residual reflects independent cultural differences among groups. 2) I now want to stress how far we can push the corollary that living in different modes can effect not only the structural opportunities available to a person but also the attitudes, values, and outlooks common in people from different groups. 3) I also want to stress the possibility that many specifics of an immigrant group's historical experiences are not captured by the modes of incorporation (as would be true of any typology), and that such historical specifics ignored by the typology might matter a great deal. Moreover, such historically specific features may involve cultural characteristics as well as other characteristics, cultural characteristics related not at all or only tangentially to the aspects of experience discussed in the typology of the modes. One could try to generalize about these ignored specifics, in a more complex theory, of course, but I don't see the reliance on the modes encouraging that complexity. 4) A big question, from this perspective, then, is: how well do the modes in fact explain the residual ethnic differences unexplained by the individual-level variables? And how do we answer that question



empirically?

The crucial domain of life that distinguishes one mode of incorporation from another is the premigration social-class position prevalent in each immigrant group, and still more to the distinction between waves of working class immigrants and waves of immigrants that include enough higher-class members to help create a distinctive socioeconomic environment. Now, there is a difference in cultural conditions in working-class and higher-class immigrant communities:

In addition [to the economic features of the labor migrant mode], there is often a kind of collective expectation that new arrivals should not be 'uppity; and should not try to surpass at least at the start, the collective status of their elders .... Ethnic-network assistance comes at the cost of ethnic pressures for conformity and the latter often reenforce employers' expectations about the 'natural' position of the minority in the labor market. These dynamics help explain the self-perpetuating character of working class immigrant communities. [In the opposite kind of community the dominant feature is] "that the support of ethnic networks is not contingent on acceptance of a working-class lifestyle or outlook".

Terms such as 'life style' and 'outlook' suggest that the working class "pressures for conformity" are often internalized; these internalized characteristics, then, can become features observable at the individual level of behavior. Furthermore, the members of such a "working-class community," those immigrant groups characterized by a working-class mode of incorporation, are typically working class themselves, or at least members of a working-class community prior to immigration too, in the country of origin.

Thus it would seem that these theoretical formulations about the context of working class communities at least open up the door to, and perhaps anticipate, differences in 'life style' and 'outlook' that could be observed at the individual level, differences that would precede immigration. If we gave a test that measured motivation in arriving immigrants, a test that measured say a belief as Nathan Glazer might have phrased it "that the world is open to their initiative" then these formulations of *Immigrant America* would seem to imply that we might expect to find (i.e.: expect on the basis of the formulations in *Immigrant America*) that labor-migrant immigrant nationalities would score lower on such a test. I mention this hypothetical test because Portes and Rumbaut, comment derisively at the end of this chapter that

Afterwards, apologists of successful groups will make necessities out of contingencies and uncover those 'unique' traits underlying their achievements; detractors of impoverished minorities will describe those cultural shortcomings or even genetic limitations accounting for their condition. Both are likely to affirm that in the end, 'if there is a will, there is a way.'

Fair enough; we've all heard such repelling self-congratulation and denigration. But the point I want to stress is that the discussion of life style and attitudes in *Immigrant America* also seems to suggest that, or at the very least allow for, the possibility that, there is more of "a will" in the middle-class compared to labor-migrant "life style and outlook" -- and that the greater 'will' is in fact part of the 'way' found later.

I think the authors might say not that I have misunderstood but rather "yes; so what?" That is, cultural differences related to upward mobility may indeed emerge from differences in modes of incorporation; but such cultural differences stem ultimately from social class positions

(class differences following and very likely also preceding migration). As such, these cultural differences should be understood as mere by-products of what really matters: the structural realities that lie behind the modes of incorporation. It is important to stress, however, that the framework of *Immigrant America* in fact opens the door to this sort of cultural difference (the byproduct of the modes), and in fact the book admits some discussion of such cultural differences in the passages I quoted; moreover, it is important to see that once the door has been opened to this sort of explanation, it is possible for others to utilize the same explanatory typology in order to throw the door open wider and stress such cultural differences derived from the modes more than the authors of *Immigrant America* have done.

So far I have discussed only cultural differences that might be thought to emerge as byproducts of the modes, the type of cultural differences that the discussion of the typology recognizes, although it does not stress. But what of other sorts of cultural differences, those that do not arise from the modes of incorporation. The sort of such cultural explanations cited as examples in *Immigrant America* typically date back to Weber's idea of the Protestant Ethic. That is, in order to pose a theoretically interesting challenge, cultural theories must derive from aspects of experience that are not rooted in social class location, and typically must derive from the domain of ideas. Here is an old example. In explaining American Jewish achievement Nathan Glazer referred to premigration class background as well as various sorts of cultural legacies (Glazer, 1955). One of those cultural legacies he believed to have been derived directly from the religion of the Jews.

But what is the origin of these values that are associated with success in middle class pursuits? Max Weber argues that they originated in a certain kind of

religious outlook on the world, the outlook of Calvinism. There is no question that Judaism emphasizes the traits that businessmen and intellectuals require, and has done so since at least 1,500 years before Calvinism. We can trace Jewish puritanism at least as far back as the triumph of the Maccabees over the Hellenized Jews and of the Pharisees over the Sadducees.

I assume that this "Jewish Puritanism" would qualify as an example of the kind of cultural explanation critiqued in *Immigrant America*; it is based in ancient religious differences, raised post-hoc, found to coexist with all sorts of other (structural) group advantages, etc. But now consider the formulation of economist and economic historian Simon Kuznets. During his lifetime he made several efforts to summarize the social and demographic characteristics of Jewish immigrants to the United States. In 1975 he published the fullest of these in *Perspectives in American History*, an exhaustive, thoughtful review of the demographic evidence on the characteristics of Russian Jews (especially age and sex structure, occupational background, and literacy levels). Yet listen now to how Kuznets closes his hundred-page review of the demographic record.

Our account dealt mainly with the measurable characteristics of the base population and selectivity of Russian Jewish immigration to the United States. These records do not reflect directly the major features of the historical heritage of Russian Jewry that shaped the human capital transferred to the United States by immigration. It is this transfer of human capital that constitutes the essential content of immigration, internal or international; and while sex, age, occupational structure, and literacy tell us much about this human capital, they do not help us to

distinguish the more fundamental characteristics of capacity for social organization and for adjustment to the challenges of a new environment. Nor do they describe the long-standing scale of priorities inherited from the past and likely to shape the goals of immigrants and their descendants for several generations after their arrival in the country of destination. One may assume that after centuries of coexistence with hostile majorities, after migrations from one country to another in Europe and the Middle East, and after self-selection over time by the loss of some of its members, the Jewish people in Europe, and especially its largest subgroup in Tsarist Russia, must have acquired a distinctive equipment of human capital. Such equipment is transferable to new surroundings and maybe of great value in making the necessary adjustments. If one could establish the characteristics of this heritage of human capital other than the basic demographic and economic characteristics, one might be able to explain, in tracing their consequences in the history of the Jewish community in the United States, aspects of American social history that are otherwise obscure. But the tools needed for such a study of the historical heritage of Russian or East-European Jewry are not those of economics and demography; and the account above, long as it is, must be left incomplete.

Thus Kuznets stresses the specific history of a particular kind of oppressed minority. It is that legacy that led to individual and communal 'human capital' (or 'ethnic capital', or 'ethnic effect'). My point is simply to stress that such historical specifics could indeed lead to influential differences in values, habits or outlook that may not derive from the class base of a

group's mode of incorporation, but from some other historically specific feature of the group's premigration life.

The point is not, of course, whether Kuznets is right to stress this factor; only to ask what the place of such a factor would be in the modes of incorporation typology, and to recognize that the answer is that such a factor has no place in that typology (that, after all, is what makes it a typology, and not an historical narrative). Can such factors -- historically specific, and in this case cultural in nature -- be added to the explanatory discussion in *Immigrant America*? Of course; but to do so will inevitably complicate the goals of a typology -- and the explanatory framework that rests on the typology.

I don't answer, as an historian might, 'well then, to hell with the typology.' But I do want to be reassured that the modes are going to dispose of much of the mess of ethnic diversity. To put it differently, if there are historically specific features of premigration life that are relevant to a full explanation, and if these are not captured in an explanation that rests on the modes of incorporation, *how much* of the whole of ethnically diverse behavior do the modes in fact capture? Here we are at the fourth issue I mentioned earlier, the question of empirical tests. The section of Chapter 3 in *Immigrant America* called "Explaining the Differences" might more fairly be entitled "Explanatory Hypotheses that Might Explain the Differences." The first part of the chapter shows only that much remains unexplained by individual-level social structural characteristics; the second part of the chapter offers the elaboration of modes of incorporation. But this elaboration only shows that the modes are a plausible and well-developed explanatory hypothesis; that is not the same as an empirical analysis. The chapter (the book) in fact does not offer an assessment of how well the modes explain what they are called on to explain. To say

that the residual from individual-level analysis establishes the impact of the modes on ethnic behavior is to appropriate the residual ethnic difference to support the modes -- just as the residual was earlier appropriated as support for the 'cultural values' explanation for behavior.

There are some empirical techniques waiting in the wings, namely tests for contextual effects. Some of these tests are very sophisticated and have data requirements that probably cannot be met; others are cruder, but applying them should at least be suggestive. In their recent paper, Portes and Dag Macleod (1996) test for the power of school context, using sophisticated methods. Their Florida and California sample includes 42 schools. They use the proportion of children receiving free lunches at the 42 schools as a measure of each school's SES context. In a similar way, one could consider measuring aspects of the ethnic context that are hypothesized to be critical to the modes of incorporation. Here the data requirements may turn prohibitive; Portes and Macleod, for example, do not have adequate numbers of children in 42 nationality contexts as they do in 42 schools. But maybe a dozen nationality contexts; eight? It may be that the number of nationality contexts is too small to apply the sophisticated HLM methods that they use to measure school contexts. But work with cruder, yet still suggestive methods would be helpful; I used such tests very briefly in my own book, and George Borjas has used them much more extensively (Perlmann, 1988; Borjas, 1992). If what matters is the proportion of entrepreneurs in the group, why not take the proportion of entrepreneurs among the gainfully employed as a measure (is that measure so much cruder than taking the proportion of the student body getting free lunch as a measure of average SES?). A continuous variable, the proportion of entrepreneurs in a group, could be substituted for the ethnic dummy variables and results (variance explained and coefficients for ethnic groups) compared using this variable in a model

rather than using the ethnic dummy variables in a model. I see no reason why such a test would be impossible; but if for some reason it is impossible to test the modes explanatory power directly -- to test whether they will explain the residual ethnic difference -- the implications of that impossibility would surely deserve close consideration.

One might think that a related sort of test arises in connection with the impact of working within an ethnic enclave, compared to working elsewhere. If the wages of otherwise statistically comparable individuals are higher in the enclave, that would be of interest. Nevertheless, that cannot be the end of the demonstration of the power of the modes (even leaving aside differences between an enclave and a mode of incorporation). We should still ask, *how much* of the difference in income across an ethnic divide can be explained by considering the contextual effect. If Cubans working in the enclave on average earn more than Cubans outside the enclave, is the difference large enough to explain most of the residual difference that was found among “statistically comparable” Cubans and Mexicans in individual-level analysis?<sup>2</sup>

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The discussion so far pertains especially to the immigrant generation, although most of what has been said could apply to the second generation as well. Yet the social context influences the development of the second generation in some ways that are generationally distinct. Before turning briefly to the concept of segmented assimilation, which in *Immigrant America* is self-consciously related to the concept of modes of incorporation, it is worth considering one further question about the modes of incorporation and about the issue of

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<sup>2</sup>Such a test also ignores the additional complexity of whether selection for an enclave job reflects some unmeasured personal characteristics relevant to income.



contextual-level vs. individual-level variables as explanations for differences in ethnic outcomes. The modes are enlisted to help us make sense of the differences among immigrant outcomes not captured by individual-level characteristics of immigrants. Over time, it is argued, the contextual variables influence the outcomes of groups members; and so the impact of the modes is to be observed eventually at the individual level. Now consider the second generation; some children have parents who were the recipients of benefits flowing from membership in a middle-class mode of incorporation; other children have parents who were limited by membership in a working-class mode of incorporation. To put it differently, *some effects of context have been transferred to the individual level, to the parents' social class position -- to variables such as occupation, education, and income.* And thus we can say that what were contextual effects for understanding the progress of the immigrants are now folded into standard SES measures for understanding the progress of their second-generation children. Now clearly the issue is again *how much* of the effects of context have in fact been folded into the measures for parents' SES and how much of the effect of context continues -- or becomes even greater -- as a distinctive influence upon the second generation. The answer may well differ across the modes of incorporation, and across specific ethnic groups. Thus an added assumption seems to have worked its way into the extension of the typology to the second generation, namely the assumption that this sort of transfer of effects from the contextual to the individual level *has not* critically reduced the importance of the contextual influences arising from modes of incorporation.

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The new chapter in *Immigrant America's* second edition on the second generation, is I

think the most complex in the book; the process described, as I understand it, is this. Forms of acculturation are offered as a new typology here, a typology for understanding the second generation. The parents' mode of incorporation has a good deal to do with which kind of acculturation will occur. And then the types of acculturation, once established, interact with several features of the social context within which the youth live. The specific features of the social context mentioned in the book are: the way the host society treats relevant phenotypes, the job structure and the geography of settlement. The geography of settlement in turn derives partly from the modes of incorporation. So: modes partially determine both acculturation and social context, and the interaction of acculturation and social context in turn provide the framework for our tentative expectations regarding which segment a youth will assimilate into (see especially pages 247-53 of *Immigrant America*).

The cultural issues I want to discuss come up in connection with segmented assimilation on two levels. The first is in connection with acculturation. Some groups preserve or modify premigration cultural forms that serve as a buffer to over-rapid acculturation -- the Vietnamese Church, the Sikh emphasis on family and tradition, the Cuban private schools seem ways to maintain premigration cultural patterns. Still, here culture, at least in the formulation in *Immigrant America*, does not mean distinctive premigration legacies of outlook that are especially conducive to making it in America. Rather, the implication is that all groups are about equal in terms of the sorts of cultural elements discussed in the chapter. At any rate the theory ignores any possible ethnically-distinctive differences among cultural legacies: the theory is not about (for example) whether Confucianism or Buddhism works better than Catholicism as a buffer against the dangers of acculturation. Nor is the point for Portes and Rumbaut that some

parts of the cultural baggage of Confucianism or Buddhism will be remarkably well-suited to American life and that part of the cultural baggage will be unpacked. I stress this distinction in the uses of 'culture' because Min Zhou's review of segmented assimilation in the recent *IMR* (Zhou, 1997) seems to stress the importance of ethnically-distinctive differences in the internal characteristics of the cultures serving as buffers. Thus Zhou comments (in the context of stressing that cultures are in fact transplanted selectively), "For example, most of the Asian subgroups ... whose original cultures are dominated by Confucianism, Taoism, or Buddhism often selectively unpack from their cultural baggage those traits suitable to the new environment, such as two-parent families, a strong work ethic, delayed gratification, and thrift" (p. 994). This passage can be read to mean that virtually any old-world culture can be drawn on for those values (a reading close in spirit to formulations in *Immigrant America*), or that Confucianism, Taoism and Buddhism are especially good cultural baggage to unpack (a reading closer to Glazer's "Jewish Puritanism" cited earlier, and close to what is treated with derision in *Immigrant America*). I don't see why the elaboration of modes of incorporation and segmented assimilation in *Immigrant America* necessarily *must*, on theoretical grounds preclude the second reading of Zhou's formulation; but the view in this second reading is at a minimum excluded from the typology, and at a maximum alien to the spirit of the book's discussion of cultural explanations.

Thus, for *Immigrant America*, more or less any old-world culture could act as a buffer against the destructiveness of ending up in the inner city ghetto cultures of resistance. So why then, is the Vietnamese Church strong and some other Church (say the Mexican) weak? One important reason is that the Vietnamese are not a working-class community, but (at least partly)

an 'entrepreneurial' community. Still, we may ask, are Vietnamese church arrangements typical of every entrepreneurial immigrant church, or of most, or of only a few entrepreneurial immigrant churches? Surely the churches of entrepreneurial groups are likely to vary; what then accounts for the strength of the Vietnamese church in particular? Part of the answer must be found in the nature of church history in the country of origin; at least that was the case with regard to the loyalties of labor migrant groups to Church institutions at the turn of the century -- compare the Italians and the Poles, for example. Thus again we are veering back toward the historical specificity of cultural baggage.

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Finally I want to turn to the second and more obvious way in which the issue of culture shows up in the *Immigrant America* chapter on the second generation. The children of the immigrants may be influenced by a process of cultural diffusion: the inner-city dysfunctional subculture may become increasingly appealing to them. I want first of all to raise the question whether Roger Waldinger and I went too far in offering a particular critique of this of cultural diffusion. We noted that working class immigrant youth in question are coming from communities with low joblessness whereas the inner-city native minority youth, described in terms similar to Wilson's underclass, come from a community plagued by joblessness. So we argued that in order to believe that the culture of joblessness can be transmitted to immigrant communities characterized by high labor-force participation, one must put great weight on independent cultural dynamics. However, I now wonder if there is not a simpler answer. The argument that the immigrant parents (and quite possibly the second generation children) have high labor-force participation does not necessarily undercut the observation that these second

generation kids may have high levels of dissatisfaction -- driven by the contradiction between their perceived chances of getting a *decent* job. If they live in a world of unappealing jobs and missing rungs on the mobility ladder, and an hour glass economy, then the such second-generation members may indeed be responsive to a dysfunctional culture that emerged nearby in a situation of joblessness. It is the issue of life chances, not the specifics of employment rates that provides the underlying shared structural condition for the two sets of youths. I am not arguing that the relevant descriptions of structure and culture are true (i.e.: the prevalence of ‘unappealing jobs and missing rungs on the mobility ladder, and an hour glass economy’); nevertheless our argument was that *even if* these structural descriptions are true, the culture of joblessness would not be likely to spread to the context of high labor-force participation. That argument of ours may give too little weight to the commonality in the structure of life chances across the two kinds of communities.

There is another feature to this cultural diffusion that I want to consider in closing. It seems to me unclear exactly who ‘the inner-city minority youth’ are. The reference clearly refers first of all to ghetto blacks. But what other groups are meant to be included? It seems that native-born Mexicans in southwestern barrios are included, and I suspect Puerto Rican children in New York City and in one or two other metropolitan areas would qualify too. But are any Asian groups included? Perhaps Chinatown gangs? The answer is unclear. I ask because a prerequisite (which I think weakens the adequacy of the segmented assimilation idea, but which may be essential to it) is that these native-born minority groups are members of racial minorities (treating this term loosely enough to include Mexicans).

Why does this matter? I think it matters because there is a semi-articulated belief here

that being non-white is enough to link the diverse minority races so that a dysfunctional subculture of resistance can spread from one group of youth to another. At a minimum, this assumption requires more attention than it receives in *Immigrant America*. To put it differently, a) in general, Asians would seem to me excluded from this downward form of assimilation; b) blacks, the prime subject, form 12% of the second generation according to *Immigrant America* and c) a great deal of the concept's power (its applicability beyond that 12%) would seem to hinge on its applicability to the Mexican barrio youth. On the other hand, the position can be taken that cross-race working-class linkages are possible (that blacks influence Vietnamese, for example); but then the issue arises, are such cross-race working-class linkages really limited to minority races? But here I am in danger of slipping into the historical questions about working-class youth culture and immigrant groups that Roger Waldinger and I have dealt with elsewhere (Perlmann and Waldinger, 1997 and Waldinger and Perlmann, 1998).

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