
The ethnic enclave debate revisited*

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Assimilation was once the grand theme of American immigration research. In the classic view, the immigrants arrived as 'ethnics', and the crucial questions involved the pace at which the newcomers and their children shed their home country identities and assumed the values and orientations of the country's core cultural group.

Though it retains its defenders, assimilation theory no longer shapes the direction of current work on immigration to the United States. Researchers have little faith in either the predictive power or the desirability of the 'straight-line' trajectory of ethnic change inherent in the assimilationist view. More importantly, the research agenda has shifted, to focus on the mechanisms and potential for social mobility, now seen as the linchpin on which other facets of immigrant adaptation depend.

But with this new direction, sociological research on US immigration has taken a curious turn: at the centre of an extraordinary amount of scholarly attention and controversy stands . . . the lowly immigrant firm. The reasons why ethnic business should have gained the spotlight are not difficult to divine: the prospects for moving up through the primary sector, as did earlier immigrants, seem poor; restructuring has created a better opportunity for small firms of the kind that immigrants can establish; and immigrants have been going into business in large numbers anyway — with or without the endorsement of salaried academics.

If the ethnic business phenomenon has a real-world side, it is also a scholarly event. The contemporary discussion about ethnic enterprise dates back to the early 1960s. However, the contours of contemporary sociological inquiry about ethnic enterprise have been shaped by a single publication that appeared more than 13 years ago — Franklin Wilson and Alejandro Portes's pioneering article on the Cuban 'Immigrant Enclave' in Miami (Wilson and Portes, 1980). Reporting on the initial wave of a longitudinal survey of newly arrived Cuban refugees and their labour-market experiences in Miami from 1973 to 1976, Wilson and Portes found that a sizeable proportion of the newcomers went to work for coethnics. They also discovered that those who worked for immigrant bosses were doing better than refugees employed in white-owned, secondary sector firms — which in turn prompted a piece of scholarly revisionism that became known as the 'ethnic enclave hypothesis'. What earlier observers had seen as a sweatshop, Wilson and Portes recast as an apprenticeship: low wages for a couple of terms of labour in the ethnic economy — dubbed the 'enclave' — in return for which one learns the tools of the trade in order to set up on one's own and thus move ahead.

The 'ethnic enclave hypothesis', as the intellectual history of recent immigration research shows, has proved to be a seminal idea. But it has also stirred up much controversy. The debate over the 'enclave hypothesis' has not just been framed by a single idea, but by the package of ideas, concepts and arguments in which it came. By package, I mean

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the concept of 'ethnic enclave' itself, as well as its defining characteristics. What I'd like to argue in this paper is that this particular package of ideas has served its role; it is now time to leave behind the preoccupations that arose in response to Wilson and Portes's pioneering contribution and move on to other concerns and conceptualizations. And so what I propose to do is revisit the 'enclave debate', suggest how it has gone wrong and offer some alternative ideas for making progress in the 1990s.

The argument summarized

Before proceeding to a critique, let me just summarize what the ethnic enclave hypothesis says. To be brutal, but fair, the enclave hypothesis started as a result in search of an explanation. Writing in *Latin Journey* (1985), which traced the labour-market experiences of Cuban refugees for a six-year period from their arrival in the United States in 1973 through to 1979, Portes and his collaborator, Robert Bach, underlined the extraordinary effect of Miami's burgeoning Cuban business sector. During these six years, the proportion of refugees who ran businesses of their own rose from 8% to 21%, and the single most important predictor of self-employment at the end of the study was employment by another Cuban three years before. About 37% of the refugees who were still employees in 1979 worked for other Cubans, and working in an ethnic firm provided a better return on experience than Cubans received in comparable non-ethnic firms in the secondary sector:

In the enclave, Cuban education contributes to occupational gains very early in the resettlement experience, with the magnitude of that advantage increasing over time. Enclave workers also benefit initially from their work experience in Cuba and, subsequently, from additional U.S.-acquired education. No such benefits are available, however, to refugees who become part of the secondary sector. Instead, this latter group competes with other ethnic minorities in firms that provide few rewards for individual skills and that tend to penalize past educational achievement. Attainment in this sector appears primarily determined by the inertial effect of economic resources transferred from Cuba, and, over time, acquired in the United States. (Portes and Bach, 1985: 239)

The finding that immigrants in the enclave did better than their counterparts in the secondary sector flew in the face of accepted theory. Many of the industries comprising the enclave also make up the 'secondary sector'. The same structural factors that impede skill acquisition, attachment (to a particular firm, industry, or labour market) and upward mobility in the secondary sector also characterize the ethnic enclave. Yet workers in the enclave appeared to enjoy some of the advantages associated with the primary sector.

Unravelling the puzzle of the 'enclave effect'

How to account for this puzzling result was the question that led Alejandro Portes and his associates to what economists would call a theory of wage determination in the enclave. But if we look closely at the writings on the enclave, we see that at least two theories of wage determination are sketched out. One is essentially a story about the role of ethnic solidarity and the way in which it 'modifies the character of the class relationship ... within the enclave' (Portes and Bach, 1985: 342). As Portes and Bach wrote in *Latin Journey*:

Ethnic ties suffuse an otherwise 'bare' class relationship with a sense of collective purpose ... But the utilization of ethnic solidarity in lieu of enforced discipline also entails reciprocal obligations. If employers can profit from the willing self-exploitation of fellow immigrants, they are also obliged to reserve for them ... supervisory positions ... to train them ... and to support their ... move into self-employment. (Portes and Bach, 1985: 343)

This story is plausible, of course, but *Latin Journey* provided no evidence that ethnic solidarity operated in the hypothesized way. For all their emphasis on the effects of ethnicity on relationships within the immigrant concern, Portes and Bach had little to say about the immigrant firm itself: with the exception of an item on the ethnicity of a worker's employer, they collected no information about the internal organization of ethnic firms, their recruitment and training practices, or their connections to other firms. The argument about ethnic solidarity also suffers from its *ad hoc* character, providing an account of wage determination in the enclave only, and not in the primary or secondary sectors to which it is contrasted.

The second theory involves an attempt to conceptualize the enclave as if it were fundamentally similar to the primary sector. Thus, Portes and Bach wrote that immigrant 'entrepreneurial activities . . . are able to reproduce some of the features of *monopolistic control* that account for successful firms in the wider economy' (Portes and Bach, 1985: 203; italics added). Taking note of Portes's findings on the advantages of enclave employment, Wilson and Martin contended that 'the enclave labour market is similar to the primary labor market' (Wilson and Martin, 1982: 139) and proposed the hypothesis that the enclave economies 'reproduce crucial features of the center economy'. What made the enclave resemble the centre economy was vertical and horizontal integration of enclave firms. This pattern yielded 'higher initial profits per unit of demand . . . higher levels of production in related industries (caused by the initial demand for the first industry's product) . . . higher wages and . . . more jobs (again because of the initial demand) (*ibid.*: 138). To support this argument, Wilson and Martin used input-output analysis and data from the 1977 *Survey of Minority-Owned Businesses*; they concluded that 'a high degree of vertical integration (within the Cuban enclave) is possible', producing the potential for an additional 50% increase in spending within the Cuban community after the 'injection of initial demand' (*ibid.*: 154).

This line of explanation reduced the enclave to a special case of the primary sector. The difficulty, of course, is that the enclave normally lacks those structural characteristics of the primary sector that make for higher returns to investments in human capital — unless the assertions of Wilson and Martin hold true. Close scrutiny calls the Wilson and Martin contentions into question. Their evidence, such as it was, mainly pertained to vertical integration. But the increased profit rate needed for higher wages in the primary sector comes from horizontal, not vertical, integration. Moreover, horizontal integration's effect on the profit rate has other correlates — namely, *lower* production, *lower* employment and *higher* prices — attributes that neither square with the characteristics of the immigrant sector, with its many small, labour-intensive firms in avid competition with one another, nor produce the employment effects hypothesized by Wilson and Martin.

As to vertical integration in the absence of horizontal integration, the question is whether it alters production efficiency, thereby profits, and through enhanced profits, possibly wages. But Wilson and Martin never addressed this issue; their concern was with the effect of integration on keeping money and profits within the Cuban economy. While encapsulation would increase the volume of profit, it wouldn't necessarily enhance the profit rate: as long as all Cuban firms are earning normal profit, whether they are in a chain of Cuban firms or they supply to or order from other Cuban firms does not alter the wage determination process.

There are other grounds for rejecting the idea of vertical integration, most importantly the fact that barriers to entry differ significantly along the supply chain. It takes little to start up a garment shop, and a good deal more to begin a textile mill. Not surprisingly, there are many Cuban garment shops but no Cuban textile mills, not many Cuban apparel retailers, and even fewer Cuban apparel wholesalers.

So we are left with the original, unhappy situation of a result in search of an explanation. This quest for a theory of wage determination in the ethnic economy is the issue that I addressed with Thomas Bailey in our article on training systems in the ethnic enclave

(Bailey and Waldinger, 1991). Starting with the assumption that immigrant firms are unlikely to develop primary sector-type structures, we asked how and why workers and employers in the ethnic economy would invest in skills. Our analytic strategy sought to answer this question in terms of a single, consistent account of skill acquisition processes in primary, secondary and ethnic sectors. We proposed the concept of *training systems*, structures that reduce the risks of investment in skills or training by increasing the probability that firms and/or workers will be able to make productive use of the skills in which they have invested. We classified these structures, which organize the circulation of information, the recruitment of labour, and the transmission of needed skills to new entrants, along two dimensions: their institutional context, whether occurring through formal channels or informal processes; and their relationship to the firm, whether internal or external.

Ethnic businesses generally lack internal training structures, which reduce the risks of skill acquisition in the primary sector. The fact that workers in the ethnic economy have to move from firm to firm to get ahead further aggravates the problem of risk-reduction. But the ethnic economy compensates because the ethnic community is brought inside immigrant firms. Hiring through the immigrant network, immigrant employers gain more knowledge than their secondary sector counterparts about the workers that are hired; this gives immigrant employers greater confidence in evaluating the prospects for employment stability and the related probability of their earning a return from investments in workers' skills. Risks are similarly reduced for workers: ethnic networks widen workers' contacts, improving the changes for successfully moving through a variety of jobs and firms and thereby obtaining appropriate skills.

Formal rules and procedures comprise a second mechanism for reducing the risks associated with skills investment in the primary sector. In primary sector firms, the impact of rules is complemented by the effects of custom, which arise out of long-standing, on-the-job interactions among workers and employers (Doeringer and Piore, 1971). While immigrant firms lack formal structures, they can make use of informal structures that, though not generated inside the firm, can be imported from outside. Network hiring brings previously existing relationships into the workplace; these relationships not only increase the quantity and quality of information available to workers and employers, they also create the normative basis for implicit contracts.

Bailey and I sought to buttress our argument by developing an example from the garment industry — and there is other empirical research, for example in Bailey's book on immigrant and native workers in the restaurant industry (1987), which provides additional support. But our main concern was to develop a theory of wage determination that accounted for differences among the primary, secondary and ethnic economies in terms of a single, sustained account. Consistency is one virtue of our attempt; so too are the affinities with other lines of sociological theorizing about economic life, most notably Granovetter's (1986) emphasis on economic embeddedness. In the end, however, the training systems approach remains a theory, waiting to be tested, with uncertain results.

What's in a name?

If we think of the enclave hypothesis as a theory of wage determination, it becomes apparent how greatly the discussion has been framed by the particular choice of name — the *ethnic enclave* economy. Part of the name has changed: what was an *immigrant* enclave economy in 1980 became an *ethnic* enclave shortly thereafter and has remained so ever since. But both words — immigrant and ethnic — are used to modify the noun enclave, which in turn is used to modify the word economy. And since we can just as easily invoke the concept of 'ethnic economy' to describe the same phenomenon — as indeed sociologists writing prior to Wilson and Portes did — it's useful to ask what additional meaning arises when one interposes this third word.

The dictionary offers us a little help: here, an enclave is 'a country or an outlying portion of a country, entirely or mostly surrounded by the territory of another country'. This definition lacks sociological content but calls up images that reappear in the enclave debate.

To my knowledge, the word enclave has no conceptual status in the urban or immigration literatures. However, the concept of 'enclave economy' is meaningful in development studies — a field of relevance to the intellectual biography of Alejandro Portes. The development literature defines an enclave economy as a sector that has virtually no connections with the domestic economy, for example tin mining in Bolivia, or banana plantations in Guatemala (see Gallo, 1991). With the exception of labour, which is sourced domestically, an enclave economy imports every input and exports every output. Most importantly, profits are exported; consequently, enclave economies fail to generate growth in other sectors.

The debate with which I am concerned here turns the concept of 'enclave economy' upside down. As in the case of developing economies, the ethnic enclave economy is detached from the general economy. But instead of a highly specialized economy, the concept of the ethnic enclave calls forth the image of a full, self-supporting economy that generates a variety of inputs and outputs within itself. The ethnic enclave idea further suggests that profits and earnings are also ploughed back into the ethnic community, producing a multiplier effect, as wage earners and firms buy from coethnic suppliers.

Better understanding of the nature of urban economies would have cast the enclave idea into question right from the start: if the largest cities are far from self-supporting, how could small ethnic enclaves do any better? Indeed, our knowledge of immigrant economies shows that they are highly specialized in a few industries or business lines where ethnic firms enjoy competitive advantages (see Waldinger *et al.*, 1990). But the tendency towards academic hermeticism proved too strong in this field, as in so many others. Before anyone had a chance to question the plausibility of the enclave idea, the ease with which it could be operationalized and put to a test kept much ink spilling in a number of US scholarly journals.

The enclave fallacy

In retrospect, conceptualizing Miami's Cuban ethnic economy as an enclave had a decisive effect on the turn of debate. An enclave, as indicated above, denotes separation, both the physical kind suggested by the dictionary definition and the structural kind found in the development literature. And thus we find that in his article with Robert Manning, Alejandro Portes specified 'physical concentration of enclaves' as a defining characteristic:

Once an enclave has fully developed, it is possible for a newcomer to live his life entirely within the confines of the community. Work, education, and access to . . . a variety of services can be found without leaving the bounds of the ethnic economy. This institutional completeness is what enables new immigrants to move ahead economically . . . (Portes and Manning, 1986: 330)

By thinking of the ethnic economy as an enclave, Portes and his associates transformed a variable — spatial concentration — into a defining characteristic, providing an opening for a critique successfully launched by Jimmy Sanders and Victor Nee (1987). Sanders and Nee aligned the enclave with metropolitan boundaries: they reassessed the experience of Cubans, whom they defined as enclave participants if they lived in Miami or Hialeah, and added the case of Chinese immigrants in California, who were defined as enclave participants if they lived in San Francisco. With this new definition in hand, Sanders and Nee asked how Chinese in San Francisco and Cubans in Miami and Hialeah compared with Chinese and Cubans who lived in any other location in the two states, and with a reference group of non-hispanic whites living outside these central cities in the two states.

The answer, no surprise to readers, was that these enclave workers did not do particularly well: 'immigrant-minority workers in the open economy tend to receive higher returns to human capital than immigrant-minority workers in an ethnic-enclave economy' (Sanders and Nee, 1987: 762).¹ In reaching this conclusion, Sanders and Nee were helped by a quiet change in the terms of comparison. Whereas *Latin Journey* mainly emphasized the advantages of the ethnic economy relative to the secondary sector, Sanders and Nee made the open economy (consisting of secondary and primary firms) the point of reference. This framing of the question seems to verge on straw-manning: to make the case for the ethnic economy one only need compare mobility opportunities in a Chinese restaurant to McDonald's — not to General Motors. And it is precisely the fact that, in contrast to days of old, immigrants are unlikely to find employment among the dwindling General Motors-type employers that makes the enclave an alternative worth taking seriously.

There is also the argument made by Portes, that the Sanders/Nee approach confounded 'participation in an ethnic enclave with living in an ethnic neighborhood' (Portes and Jensen, 1987: 768). Unfortunately, the controversy stirred up by Sanders and Nee did not lead to much progress on substance. Instead, readers have been treated to a continuing exchange with Portes and his associates over the proper definition of the enclave, a dispute joined by other researchers who have operationalized and then invoked alternative definitions of the enclave.²

These efforts have proved inconclusive, though they have certainly introduced more scepticism about the beneficial effect of employment in the immigrant firm. Unlike Portes's

1. This terminology should give readers some feel for the very particular way in which this debate was framed, and the influence exerted by the rhetorics considered appropriate by the leading journals of American sociology. To be fair, I must note that Sanders and Nee's critique developed a second line of attack: they pointed out that the original regressions reported in *Latin Journey* provided results for a pooled sample of enclave entrepreneurs and workers, without using a dummy for employment status, which very possibly biased the results. In their own study, Sanders and Nee ran separate regressions for ethnic employers and employees, finding that the enclave did yield benefits for the self-employed. All the subsequent studies, summarized in n. 2 below, followed the Sanders/Nee approach of separating employed from self-employed.
2. While a blow-by-blow account of these exchanges would undoubtedly exhaust readers' patience, the briefest summary is probably necessary. The basic technique has been to regress the log of annual earnings on a series of human capital and other variables. In most cases, separate regressions are run for each sector, with further tests for the significance of differences in the coefficients. In a study utilizing data from the 1980 US Census of Population, Zhou and Logan (1989) use three different definitions of the enclave (by place of residence, by place of work, and by industry), add a new case, that of Chinese immigrants in New York, and extend the analysis to include women. They find that male enclave workers gain positive returns in income from education, labour-market experience and English-language ability, regardless of the definition employed; none of these human capital variables, however, is positively related to the income of female enclave workers. Portes and Jensen (1989) reply to the issues raised by Sanders and Nee with another study focusing on Cubans in Florida, this time using 1980 Census data and a 1983–6 longitudinal survey of 'Mariel' immigrants. In the analysis of the 1980 data, Portes and Jensen look at all Cubans in the state of Florida, identifying enclave employees as all Cubans who gave Dade County as their place of employment (though not necessarily place of residence). The Mariel survey included a question about employers' ethnicity. The end result is a modest retreat from the more sweeping conclusions of *Latin Journey*: 'enclave enterprises do not treat their employees any worse than do outside firms, in terms of either gross earnings or net payoff on the workers' human capital' (Portes and Jensen, 1989: 945). Model (1992) returns to 1980 Census data and the cases of Cubans in Miami and Chinese in San Francisco, staying faithful to the original Portes/Bach approach of contrasting the enclave with primary and secondary sectors, and defining the enclave by industries in which immigrant owners and workers are over-represented. This study shows that workers in the primary sector do better than those in the enclave, who do no better, but no worse, than those in the secondary sector. Gilbertson and Gurak (1993) examine the case of Dominican and Colombian male immigrants in New York City, using a 1981 survey which allows them to identify both sector (primary v. secondary) as well as the ethnicity of employers. In a pooled regression, with primary, secondary and enclave sectors as dummies, they find that enclave workers do the least well. Portes and Jensen and Sanders and Nee make use of 10 pages of the October 1992 issue of the *American Sociological Review* (Jensen and Portes, 1992; Sanders and Nee, 1992; Portes and Jensen, 1992) to have a final attempt at defining the enclave, without bringing any really new findings to light.

original survey, where questions were asked about the ethnicity of respondents' employers, all but one of the reassessments (Gilbertson and Gurak, 1993) rely on the US Census of Population, which does not collect information about the ethnic characteristics of employers. Thus, all of the census-based studies define the enclave in terms of characteristics that are likely to converge with the phenomenon of interest — namely the employment of immigrants by their coethnics. But the procedure inevitably involves all sorts of questionable inferences, which not only lead to distortions but prolong treatment of the immigrant firm as a black box.

Methodological problems aside, there is good reason to think that the debate has pushed the discussion towards a dead end. If we think of the ethnic economy as a particular form of social organization, in which immigrant entrepreneurs employ coethnic workers, there is no reason to assume that the particular factors that distinguish the informal training system among immigrant workers and entrepreneurs are uniquely a product of their placement in space. To be sure, spatial concentration may affect the wage determination process in both positive and negative ways. For example, concentration may generate agglomeration economies that occur when ethnic firms proliferate and attract additional customers who are drawn by the size and diversity of the physical marketplace — this is the New York Chinatown phenomenon that Min Zhou (1992) has analysed so well in her recent book. Alternatively, concentration may increase the number of workers converging on an ethnic business district, thus increasing intra-immigrant competition and dampening wages.

Either way, the analytic issue concerns the impact of concentration on the wage determination process. From this perspective, the informal training systems operating in concentrated ethnic economies are likely to function in similar ways in ethnic economies which are dispersed across space. For example, Korean entrepreneurs who principally sell to non-Korean clients in non-Korean neighbourhoods are characterized by a high level of organizational density, multiple and criss-crossing social networks, and a continual flow of new workers and potential entrepreneurs. These are precisely the same ingredients that Alejandro Portes identified in his 1987 article in *Sociological Perspectives* as the source of Cuban entrepreneurial growth. And if we consider the ethnic economy as a special form of community, there is little theoretical reason to think that it cannot be maintained without propinquity — which is what the vast literature on urban communities more generally shows.

Conclusion

In its present incarnation, the ethnic enclave hypothesis is unlikely to be with us very much longer. One problem is simply the nagging empirical issues, to which I have alluded above. But as economists are fond of repeating, you can't kill a theory with facts alone. And, as I have argued, it is precisely on theoretical grounds that the ethnic enclave hypothesis, at least as initially formulated, seems on shakiest ground.

Regardless of the fate of this particular hypothesis, it is unlikely that interest in the immigrant business phenomenon and its consequences will die down. Immigration to the United States has increased substantially over the last 15 years and the steady growing infusion of newcomers has led to burgeoning immigrant economies in such cities as New York, Los Angeles and Miami.

Thus, the question is how to improve the quality of the debate. One modest step forward would be to drop the term enclave, and simply refer to ethnic economies, as Ivan Light (forthcoming) has suggested. The ethnic economy concept has the advantage of being more encompassing, especially since, as Light has pointed out, many immigrant firms have no employees. Having eliminated the referent of 'enclave', researchers could cease quibbling over spatial definitions and instead proceed to the work of seeing whether spatial factors affect wage and mobility processes among immigrant firms.

For still more progress we will need to expand the narrow frame of the debate. Recent scholarship has been too prone to take an economic view, relying on off-the-shelf models of labour-market segmentation that can be easily operationalized, yielding the coefficient-centred quality that characterizes much of the literature. Thirteen years after Wilson and Portes's original contribution, we have only begun to move towards more theoretically sophisticated accounts of the distinctive traits of ethnic enterprise that connect the research to the broader field of economic sociology of which it is a part. Portes's own recent contributions, in which he shows how 'bounded solidarity' and 'enforceable trust' promote collective economic action in immigrant communities, promise to turn the discussion in a much more fruitful direction (Portes and Zhou, 1992).

Better data will also help, and these will only come from studies that will take us directly into the immigrant firm. A new round of large-scale surveys, following in the tradition established by Portes, while providing insight on other groups than the well-studied Cubans, would certainly move us along. But my own sense is that the most pressing need is for more institutionally focused, fine-grained research — the best tool for telling us what lies inside the immigrant firm's black box.

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