Notes on the Historical Study of Social Mobility
Author(s): Stephan Thernstrom
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NOTES ON THE HISTORICAL STUDY OF SOCIAL MOBILITY *

In recent years sociological work in the field of stratification and social mobility has become, in at least one sense of the term, impressively cosmopolitan. National sample surveys which include data on inter-generational occupational mobility have been carried out in every major Western nation and in a good many non-Western societies as well, and those have inspired some ambitious comparative analyses of social mobility. This development, as S. M. Miller puts it, has had the virtue of making "the study of mobility one of the few fields of sociology which has overcome national parochialisms".[1] True as this is, however, it must be said that there are forms of parochialism other than national. Much contemporary research into social mobility suffers from one of these – a parochialism of time rather than of place, as it were, the parochialism of presentism. My purpose here is to suggest what is lost as a result of that parochialism, and to argue that a sense of the past, an ability to see his subject in historical depth, is not a luxury but a necessity for the student of social mobility.

I

Let me begin with a simple, obvious, uncontroversial point – so obvious, indeed, that I would blush to make it but for the fact that so few students of social mobility seem to have taken it to heart. This is simply that some of the most interesting questions we might ask about the nature of a class structure today cannot be answered without reliable information about the nature of that class structure yesterday – and the day before yesterday, and even the century before yesterday! And that nowhere has the research necessary to supply such knowledge about class and mobility been carried out in sufficient historical depth. Despite the recent avalanche of empirical research on social mobility, appallingly little is known about the process of

* An earlier version of this paper was delivered at the 1964 annual meetings of the American Sociological Association. I am very much indebted to Charles Tilly for critical suggestions.

[1] S. M. Miller, "Comparative Social Mobility: A Trend Report and Bibliography", *Current Sociology*, 9 (1960), 2.

social mobility in the past and about long-term mobility trends in any society – certainly not in our own.

Consider the familiar controversy over the blocked-mobility hypothesis advanced by the Lynds, Warner and others some decades ago.[2] The question of whether or not American society is on the verge of succumbing to arteriosclerosis has been repeatedly discussed ever since. Clearly this is a question of considerable interest – to the general public as well as to the scholarly world, if the success of such books as *The Status Seekers* be any index. And yet the issue remains largely unresolved, despite some recent studies demonstrating that there has been no dimunition in mobility rates in the past two decades.[3] Valuable though this work is, its time perspective is so foreshortened as to be irrelevant to the issue of long-term changes in the openness of the American social order. Rogoff's Indianapolis study reaches farther back into the past, but even that begins no earlier than 1910, while the explicit or implicit point of comparison chosen by proponents of the blocked-mobility theory was *nineteenth-century* America, and there is the further limitation that Rogoff did not study intra-generational mobility at all.[4] Systematic studies of social mobility in nineteenth-century America are still woefully absent. True, the social origins of members of the national business elite of the era have been examined in some detail, and Aronson has recently explored the social composition of the higher civil service in the first four decades of the Republic, but little, regrettably, can be learned about the range of mobility opportunities at the lower and middle levels from surveys of those who rose to the very top.[5]

The debate about mobility trends in the United States has been conducted without any solid grasp of the nature of the American class structure in the past, or indeed any knowledge that much the same argument about the level of opportunities has been going on in this country for approximately a century! Thus this typical contribution to the debate:

> The man at the bottom of the ladder leading up to the social heavens may yet dream that there is a ladder let down to him; but the angels are not seen very

[2] W. Lloyd Warner and J. O. Low, *The Social System of the Modern Factory* (New Haven, 1947); Robert S. and Helen M. Lynd, *Middletown: A Study in American Culture* (New York, 1929) and *Middletown in Transition: A Study in Cultural Conflict* (New York, 1937); Elbridge Sibley, "Some Demographic Clues to Stratification", *American Sociological Review*, 7 (1942), 322-330; D. H. Anderson and P. E. Davidson, *Occupational Mobility in an American Community* (Stanford, 1937); J. O. Hertzler, "Some Tendencies Towards a Closed Class System in the United States", *Social Forces*, 30 (1952), 313-323.

[3] E. F. Jackson and H. J. Crockett, "Occupational Mobility in the United States: A Point Estimate and Trend Comparison", *American Sociological Review*, 29 (1964), 5-15; O. D. Duncan, "The Trend of Occupational Mobility in the United States", *American Sociological Review*, 30 (1965), 491-498.

[4] Natalie Rogoff, *Recent Trends in Occupational Mobility* (Glencoe, Ill., 1953).

[5] Sidney Aronson, *Status and Kinship in the Higher Civil Service* (Cambridge, 1964). The business-elite literature is conveniently summarized and analyzed in S. M. Lipset and R. Bendix, *Social Mobility in Industrial Society* (Berkeley, 1959), Chapter IV.

often ascending and descending; one after another, it would seem, some unseen yet hostile powers are breaking out the middle rungs of the ladder.[6]

These are not the words of Lloyd Warner or the Lynds observing Newburyport or Muncie in the Great Depression, but the gloomy verdict of an obscure Boston minister in 1885. Similar complaints about declining opportunities were voiced by artisans threatened by economic change in the Jacksonian era.[7]

That the blocked-mobility hypothesis advanced by modern social scientists is not a blinding new discovery but a restatement of an age-old American complaint does not necessarily discredit the notion, but it should make us wonder a little. If the middle rungs of the social ladder were being wrenched out in the 1880's or in the age of Jackson, how many could have been left for the later destruction described by Warner and the Lynds? If, on the other hand, Reverend Smyth and Jacksonian labor leaders were mistaken in their diagnosis, victims of an innate American tendency to judge the imperfections of the present against a fictitious vanished Golden Age of perfect opportunity in the past, might not the same predispositions shape the perceptions of later American social scientists as well? The ironic coincidence that the idyllic past conjured up by Lloyd Warner in *The Social System of the Modern Factory* was just the period in which Reverend Smyth was lamenting the death of the American Dream strongly suggests that this was indeed the case. Popular mythology about the character of the social order is well worth careful study in its own right, to be sure, but some writers have reflected it instead of reflecting upon it.[8]

A large-scale, systematic quantitative study of social-mobility patterns in nineteenth-century America will be required to allow us to gauge whether any of these dire prophecies of constricting mobility opportunities had any foundation in fact, and, more important, to assess the influence of a host of different variables singled out by those who have speculated about long-term mobility trends in the United States. Was it the mechanization of industry and the consequent destruction of older craft hierarchies which produced the changes Reverend Smyth deplored, and if so, had these processes advanced as far and in the same form in Boston in the 1880's as in Muncie and Newburyport four to five decades later? What of the closing of the frontier, the blocking of mass immigration to our shores, or the narrowing of class differences in fertility? The first of these, difficult though it is to date

6 Newman Smyth, *Social Problems: Sermons to Workingmen* (Boston, 1885), 12-13.

7 Norman Ware, *The Industrial Worker, 1840-1860* (Boston, 1934; paperback edition, Chicago, 1964), *passim*. For similar fears in the latter half of the nineteenth century, see the documents in Leon Litwack, *The American Labor Movement* (Englewood Cliffs, N.J., 1962), 3-14. Both Ware and Litwack were insufficiently critical of the testimony they cite, and assumed that if contemporary witnesses *thought* that opportunities were declining, they must have been in fact, an assumption questioned below.

8 For further development of this point, see Stephan Thernstrom, "'Yankee City' Revisited: The Perils of Historical Naïveté", *American Sociological Review*, 30 (1965), 234-242.

precisely, was taking place at just about the time at which Reverend Smyth wrote, while the other two had yet to occur at all. Since the *timing* of these and other historical developments which might have influenced the shape of the class structure varied greatly, historical inquiry affords us the opportunity to assign priority to certain variables and to dismiss others.

It is a truism, of course, that the comparative method serves this end. What is not a truism, however, at least not one which conspicuously influences the actual course of social research today, is that additional depth of knowledge about one society can be as fruitful for comparative purposes as additional cross-cultural breadth.[9] Perhaps more fruitful, for it does not rest upon the questionable premise that all societies pass through similar stages of development and that in the absence of sufficient historical knowledge about social patterns in the early years of industrialization in the West we may apply models derived from the study of the class structure of underdeveloped countries today. Such models may or may not be relevant; their possible relevance can only be demonstrated on the basis of thorough acquaintance with the historical record. The two research strategies are complementary, of course, but what requires emphasis is that the one is a poor substitute for the other.

III

The blocked-mobility hypothesis provides a convenient illustration of another point which underlines the importance of understanding social mobility in historical context. The question of how the Industrial Revolution and a host of subsequent economic and technological changes have affected the social position of the ordinary workingman has generated an enormous historical and sociological literature, much of it marred by the failure to grasp that in a mobile society a decline in the status of a particular occupation is often accompanied by a corresponding shift in the social stratum from which the occupation draws its labor force. Thus writers like the Hammonds, Norman Ware, the Lynds, and Warner misunderstood the implications of their discovery that the position of the semi-skilled operative in a modern textile mill or shoe factory was in many ways inferior to that of the artisan who produced similar products prior to industrialization. They saw industrial change as the engine of "status degradation" for a large sector of the working class. But this assumed a simplistic model of an occupational structure in

[9] For a good example from the field of demography, see Aaron Antonovsky, "Social Class, Life Expectancy, and Overall Mortality", *Milbank Memorial Fund Quarterly*, 45 (1967), 38-39. For further discussion of the advantages of longitudinal studies over static cross-sectional ones, see Nathan Goldfarb, *An Introduction to Longitudinal Statistical Analysis* (Glencoe, Ill., 1960), and Norman B. Ryder, "The Cohort as a Concept in the Study of Social Change", *American Sociological Review*, 30 (1965), 843-861.

which all skilled crafts were being wiped out and in which there were no opportunities for upward social mobility, so that the artisans and their children had no alternative but to suffer status degradation and accept a semi-skilled factory job.

This model, however, is of doubtful validity. Convincing evidence that it was the skilled craftsmen of old or their children who made up the new semi-skilled factory labor force has never been produced by adherents of the cataclysmic view of industrialization. Recent research suggests that status degradation was a rare phenomenon, that the skilled have commonly been able to preserve their position and that their sons have been likely to find other skilled niches or quite often to enter a white-collar position of some kind. The new factory labor force, it appears, has characteristically been recruited by a process overlooked by earlier observers, a process with very different implications for the social structure. By and large it was not declassé artisans but unskilled newcomers – immigrants and migrants from rural areas – who moved into the factories, men for whom factory employment generally meant improved status. An essential aspect of the complex of changes we refer to as urbanization and industrialization has been a cycle of migration and social mobility which has filled the least attractive and least well-rewarded industrial positions with successive waves of newcomers, who appraise their situation with standards formed not in the proud world of the independent artisan but in a subsistence agrarian economy.

We cannot speak too dogmatically about this matter, given the paucity of evidence currently available, but this generalization does hold for the United States, I think. (More about the rest of the world in a moment.) My investigation of working class social mobility patterns in Newburyport, Mass. in the 1850-1880 period reveals this mobility cycle at work there,[10] and my current work with a sample of 8000 residents of Boston between 1880 and the present, as yet unpublished, has yielded much the same result. Few skilled workmen in the community suffered downward mobility as a result of technological and other developments at any time in this period. Furthermore, the sons of skilled craftsmen rarely dropped down into the ranks of the unskilled or semi-skilled themselves; close to half of them, indeed, attained middle class status.

The same conclusion is suggested by Rogoff's Indianapolis study. This goes back no earlier than 1910, unfortunately, but it is the most comprehensive inquiry available for the period with which it deals, and it has the further advantage of dealing with a Hoosier city quite similar to nearby Muncie, the site of the Middletown research, thus providing a rough check of the accuracy of the Lynds' assumptions about the status degradation wrought by industrialization. Rogoff's tables reveal that fully 49 percent of

[10] Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth Century City* (Cambridge, 1964).

the sons of skilled craftsmen in the Indianapolis sample for 1910 were themselves in skilled callings, and another quarter of them had moved into the rapidly expanding non-manual occupations. Few of them had been downwardly mobile in the way foreseen by proponents of the blocked-mobility hypothesis.[11] We can infer from Rogoff's tables and what the Lynds tell us about the precipitous growth of the Muncie population that the semi-skilled factory labor force in both cities was actually composed predominantly of newcomers of lowly origin.

This mobility cycle, in which newly-created jobs of rather lowly status tend to go to those who previously held even lowlier jobs, is easiest to observe in cities with two distinguishing features. If a city's population increased dramatically during industrialization, and if many of the newcomers were members of highly visible ethnic groups, this relationship between in-migration, industrialization and social mobility should leap to the eye. Doubtless it was not accidental that the two major community studies which advanced the blocked-mobility theory – the Yankee City inquiry and the Middletown volumes – were carried out in American cities which happened to lack one of these traits. In the case of Newburyport, whose total population was little more in 1930 than it was in 1855, it was natural – though utterly mistaken – to assume that the *composition* of the population had changed very little, and that this was a self-contained, static "old New England community". In fact, however, this was a radical misconception. Though its total population levelled off in 1855, the composition of the population underwent a number of fundamental changes. The stable net figure concealed staggeringly high rates of gross movement. A substantial fraction of the city's inhabitants left Newburyport each decade; others – the Irish, later the French-Canadians, the Italians, and so forth – poured in to take their places, a steady stream of newcomers to occupy the lower rungs of the occupational ladder.[12]

That the total Muncie population grew very rapidly during the years of the Middletown study was not conducive to the illusion that the community was sealed off from the larger society in the manner dear to the heart of the anthropologist. But the fact that the Lynds selected a community without a large foreign-born or Negro population – they sought to exclude racial change as a variable and to isolate the social effects of industrialization – blinded them to this mobility cycle in much the same way. In fact, Kentucky, Tennessee, and rural Indiana served as the functional equivalent of the Old World as a source of migrants with little status to lose; it was much easier to overlook newcomers like these, however, and to assume that the sons of the highly-skilled glassblowers of old Muncie were necessarily a prime source of the new semi-skilled factory labor force.

[11] Rogoff, *op. cit.*, 44.
[12] Thernstrom, *op. cit.*, 84-86, 167-168, 195-196.

The bald assertion that factory employment generally meant improved status for migrants like these does raise a troubling problem sociologists are only now beginning to grapple with, the problem of how to evaluate movement from an entire social setting to one utterly different. It seems simple enough to say that a foreman enjoys higher status than a factory laborer, and that the owner of the factory ranks above both of them, but the shift of agricultural workers into urban industrial employment obviously defies easy evaluation. There can be a series of distinct clusters of positions which differ in their relationship to the market, life chances, etc. and yet which are roughly equivalent in power, wealth, or prestige. The concept of "situs" has been developed to describe such a situation.[13] The movement of workers from the agricultural situs to the industrial one may sometimes entail no vertical mobility in either direction, but only horizontal movement into a slot of equivalent rank. This means that it is necessary to investigate in detail the social milieu from which the migrants came in any particular instance, to specify the distribution of social types – large landlord or small, tenant or farm laborer – within the migrant stream, and to employ these categories in examining the experiences of these men after they enter the industrial world. Such a procedure will make it possible to distinguish cases in which migration brought no improvement or even status loss from what I suggest is the more common pattern of general advance.

The sketchy evidence cited above pertains to the United States in the past century. That a similar process has been at work in other societies as well we know from the work of Lipset and Bendix, Morris D. Morris, and others, but this work constitutes the barest beginning toward a full understanding of the matter.[14] One wonders if some of the societal differences which were invisible through the crudely-ground lenses of Lipset and Bendix's microscope, and which they consequently interpreted as variations in national values unsupported by actual differences in social structure, might not be rooted in differences in the way this process operated in different countries. One suspects, for example, that the pace and volume of immigration and internal migration was spectacularly high in the American case, and that the proportion of the urban labor force with prior exposure to the artisan

[13] Paul K. Hatt, "Occupation and Social Stratification", *American Journal of Sociology*, 55 (1950), 539. For an excellent historical illustration, see Charles Tilly's discussion of the dual class structure of the Vendée in the 1790's; *The Vendée* (Cambridge, 1964), 79-80, 98-99.

[14] Lipset and Bendix, *op. cit.*, 203-226; Gideon Sjoberg, "Rural-Urban Balance and Models of Economic Development", in Neil Smelser and S. M. Lipset, *Social Structure and Mobility in Economic Development* (Chicago, 1966), 235-261; Gunnar Boalt, "Social Mobility in Stockholm: A Pilot Investigation", *Transactions of the Second World Congress of Sociology* (London, 1954), II, 67-73; Morris D. Morris, "The Recruitment of an Industrial Labor Force in India, with British and American Comparisons", *Comparative Studies in Society and History*, II (1960), 305-328; Ronald and Deborah Freedman, "Farm-Reared Elements in the Non-Farm Population", *Rural Sociology*, 21 (1956), 50-61.

and yeoman traditions that inspired the militant labor protest of the Industrial Revolution in Britain was distinctively low, and that this has a good deal to do with the continuing popular belief that New World society has been uniquely open.[15] Too little is known at present to press this argument very far, but the whole issue demands study by sociologically-inclined historians and historically-minded sociologists.

II

Something should be said about the problem of finding data from which historical studies of social mobility can be written, and about the historian's approach to such data. It has often been assumed that systematic historical studies of social mobility are rare because of the absence of satisfactory evidence. In a great many instances, at least, this is more a rationalization than a reason. True, there are times and places irrecoverably lost to history. But for most relatively modern societies, and some traditional ones (such as ancient China), vast amounts of usable material are still untapped. Thus, for the United States there is a wonderfully rich and virtually untouched source for mobility research – manuscript schedules of the federal census, which from 1850 on provide a primitive social survey of the entire population of the United States. There are city directories, well used by Sidney Goldstein and his colleagues in the Norristown study, which for many communities extend back a century and a half or more; the problem of reliability with early city directories is severe but not insurmountable, and they offer the special advantage for sample studies that respondents are arranged by alphabetical order.[16] Reaching even farther back into the past are local tax and voting records – even when these lack adequate occupational information they made it possible to stratify a community according to property ownership and political participation. In many Western European countries, of course, historical records with relevant fragments of data have survived even longer.

There are maddening difficulties in employing this data, to be sure. Occupational designations are sometimes appallingly vague by modern standards. Until well into the nineteenth century in England and the United States, a "manufacturer" could be a manual employee or his employer; it was not until the twentieth century that the French "ouvrier" received its present definition.[17] Where the available records do not supply further information to make the distinctions which we regard as crucial, analytical possibilities are obviously sorely limited.

[15] Cf. E. P. Thompson, *The Making of the English Working Class* (London, 1963).
[16] Sidney Goldstein, *Patterns of Mobility, 1910-1950: The Norristown Study* (Philadelphia, 1958).
[17] George Rudé, *The Crowd in History, 1730-1848* (New York, 1964), 196-197.

Instead of bewildering vagueness, we sometimes encounter puzzling concreteness. Consider the difficulties of devising an occupational classification scheme to employ in analyzing Patrick Colquhoun's enumeration of the London population at the close of the eighteenth century, enumerations which include such intriguing occupations as mudlark, scufflehunter, bludgeonman, morocco man, flash coachman, grubber, and bear baiter.[18] Obviously the findings of Inkeles, Rossi, Reiss and others who have investigated the prestige ranking of occupations is of little avail in grappling with the problem. If the example is not very serious, the point is. Whatever the difficulties they create, however, there is the consolation that the curiously antique or exotic labels which often appear in the sources convey a very useful warning. The problem of the mudlark and the scufflehunter suggests two general principles: (1) the need to employ finely calibrated instruments in reconstructing a social structure now vanished; (2) the necessity of paying close attention to the entire social context in which the particular phenomenon under consideration was embedded.

At this level of abstraction, these principles seem singularly harmless platitudes, but they are not, I think, without a cutting edge. If we abide by them in examining some vast problem – changes in the openness of the American social structure since 1700, let us say – I think we would proceed in a manner somewhat different from that in which many contemporary sociologists would proceed. Rather than taking the currently fashionable index of social mobility, the rate of inter-generational movement between maual and non-manual occupations, computing occupational mobility rates at selected intervals, and constructing a simple time series, the historian would insist that a scrupulous examination of the class structure at several strategic points in time and an assessment of the extent of social mobility in terms of categories appropriate to each point in time would be required, and that to arrive at a simple conclusion about trends might be impossible because of the lack of comparability of the historically specific categories used. Did the Polish peasants in the mills of Gary in 1910 hold the same relative position in the class system as the indentured servants of Salem in 1710? What was the mobility equivalent in 1910 of the purchase of a farm in the Connecticut River Valley in 1710? Merely to contemplate the multitude of meanings of the occupational designation "farm owner" through the course of American history is to see some of the inescapable complexities of the problem.

This is not to say that there would be nothing of interest in a crude table which purported to summarize changes in the rate of inter-generational movement between manual and non-manual occupations in America since 1700, any more than that there is nothing of interest in a similar table summarizing crude occupational data for post-World War II Europe and

[18] Thompson, *op. cit.*, 55.

the United States. But this would be but a small first step, and it may not be the wisest step to take first. The concept of social mobility, after all, is an exceptionally rich and complex one, and simple one-dimensional indices which facilitate immediate comparisons of social mobility in radically different social orders may not yield the most rewarding comparisons. The alluring comparability attained by passing disparate sets of data through a sieve so crude that it allows essential features of each set to trickle away is purchased at a very heavy price.

In my own work on working class mobility in nineteenth-century America I quickly found that some of the most important elements of the problem could not be discerned through the lenses of a two-class occupational scheme – or, indeed, any occupational scheme at all. Not only was it useful to distinguish unskilled, semi-skilled, and skilled positions within the broad rubric of manual occupations; it was essential to devote extensive attention to another dimension of social mobility entirely. By far the most important form of upward mobility in the setting I examined was movement between the floating, unstable, propertyless sector of the working class and the settled, respectable, property-owning working class group. Whether these were indeed distinct social classes or different strata of the same class seems to me a verbal rather than a substantive problem. Certainly they were distinct social groups, with different life chances and different styles of life, and movement between them must be regarded as social mobility. To move from the first of these into the second was a less dramatic upward advance than to leap directly into the world of the middle class, but it happened much more often and was in this sense a more important feature of the social scene. Without attention to social mobility of this kind, which requires investigation of patterns of home ownership, savings, and residential continuity among other things, we would know very little about social mobility in this milieu. For other times, other places – seventeenth-century England, let us say – a somewhat different conception of social mobility would be required, as the famous controversy inspired by Tawney's paper on "The Rise of the Gentry" suggests, and categories appropriate to that specific historical configuration should be utilized.[19] In developing these categories, major boundaries of the stratification system of the particular society – power, wealth, style of life – can be specified, and then inquiry can be made about rates of movement and processes of movement across these boundaries, as well as about shifts in the boundaries themselves which take place in the course of historical development.

Such concern for fidelity to a particular historical context implies a certain

[19] A judicious review of the controversy and selections from the leading contributions is available in Lawrence Stone, ed., *Social Change and Revolution in England, 1540-1640* (London, 1965). For splendid examples of the sort of historical analysis I have in mind, see Stone's essay on "Social Mobility in England, 1500-1700", *Past and Present*, 33 (1966), 16-55, and his book *The Crisis of the Aristocracy, 1558-1641* (Oxford, 1965).

chastening of the aspiration to construct a general theory of social mobility through comparative research, but certainly not an abandonment of sociological models and systematic comparative analysis. My aim is not to urge the inviolable uniqueness of each historical moment, but rather to argue that historical data should be employed to edit and refine social theory to make it more sensitive to social reality past and present.

STEPHAN THERNSTROM
Brandeis University