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ABSTRACT

The current dispersion of stratification research into several seemingly unrelated topics such as social mobility and various kinds of inequality (racial, gender, income, etc.) is unfortunate and unnecessary. It is unfortunate because a good theory can provide the means by which to improve its various special applications. It is unnecessary because a general theory of stratification in fact exists, though this is not immediately obvious because its expressions have evolved with the passage of time, and because its basic unity is obscured by terminological differences among its contributors.

The theory’s original form was written in the Year 1377 by Ibn Khaldun as the basis of his theory of human organization (sociology), and was buttressed by his profound knowledge of pre-15th Century societies. His form of it is a conceptually simple framework consisting of a two-class hierarchy of absolute power, together with an analysis of the rise, dissolution, and replacement of power holding groups.

Beginning around 1900 this framework was augmented and elaborated both in terms of a more refined set of dimensions of types of power and of modern statistical concepts by which to describe the structural variations of power in societal stratification systems.

The paper reviews the evolution of the theory and brings it up to date. Measurement methods and applications to several of its currently fragmented expressions are discussed.
Introduction.

An overview of the various lines of research that are considered to be analyses of the hierarchical phenomenon sociologists call stratification gives the impression that the field may be hopelessly fragmented, little more than a number of unrelated activities, each going its own way with no apparent relation to the others. Social mobility, active and influential for a half century, is one. So also do analyses of racial inequalities, as well as those of gender. The same may be said of so-called ‘class analysis’. And it is also true of status attainment research.

This paper, however, presents a contrary view: that underlying and contribution to each of such specific lines is a fundamental set of concepts, one that has had a very long and evolving history that began centuries ago with the work of Ibn Khaldun. After this it lay dormant until the late 19th and early 20th Centuries when it was reinvigorated by Max Weber and Pitirim A. Sorokin. Then after a lapse of a few decades it reached its latest plateau in the 1960s or so. The present paper provides an extension of these.

As is well known, a powerful theory tends to generate both its own corrections and new, fruitful, and (at least in principle) testable hypotheses about the behavior of the phenomena to which it is addressed.

The paper sketches the theory and its evolution, its relation to some of its offshoots; its relevance for understanding how real-world stratification phenomena vary from time to time and society to society; and for improvements in the analysis of the causes and personal and social consequences of a society’s stratification structural states and changes, drawing upon the types of statistical devices required to describe such structural states and variations of stratification systems. It also comments on current methods by which variables composing its substantive (as distinct from structural) dimensions are measured.

The theory is called ‘empirical’ to distinguish it from so-called ‘theories’ in sociology whose referents are too obscure to be measured, at least at this time. It is called ‘societal’ because it is held to be applicable to societies as wholes, and because it is projected to be valid (perhaps with minor future modifications) for all time, rather than being limited to certain specific historical areas or epochs.

The concepts of ‘content dimensions’ and ‘structural dimensions’ (Haller 1970, rev. 2000) are invoked to clarify a fundamental difference between the substantive factors of stratification phenomena (content dimensions and content variables) and the forms (structural dimensions) by which each of the content dimensions may vary.
As the theory stands today, the conceptual content dimensions of power are four in number: political, economic, social, and informational. These are as specified by Svalastoga (1964), who added the last to those implied by Weber (1946) and made explicit by Sorokin (1927), and ratified in principle by Lenski (1966), Duncan (1968) and Haller (1970). They are thus matters of a consensus that emerges from historical evidence. Well known measurement devices, applicable everywhere, exist for each, except for political power. (Preliminary experiments have been made to learn how to measure the latter.)

There are six conceptual structural dimensions, some explicitly stated by one or another of Sorokin, Svalastoga, Lenski or Duncan, with two added by Haller. They are: general level, degree of inequality, degree of inheritance, crystallization, mode structure, and skewness (asymmetry). Logically, each of these applies to each content dimension. Each of these may be measured or otherwise assessed by standard statistical devices or by observing the frequency distribution.

**Empirical Theory.**

The concept ‘empirical theory’ is intended to call attention to systems of concepts proposed for the purpose of describing the ways the elements of a given limited domain of phenomena work together, which concepts are intended to have measured variables or other direct or indirect observations as their mirror images. At their best, such theories consist of conceptual variables that are measured by valid and reliable instruments. When this is impossible, as was the case for stratification theory up to a half century or so ago, careful empirical theorists often depended on indirect but credible recorded information, including certain historical writings.

In the natural sciences empirical theory has become so routine that it is simply taken for granted. Measurement and theoretic concept are so close to each other that they are interchangeable. The same holds true for the unmeasured observational data so often used by such researchers. And evidence published by previous writers of their research procedures and results is routinely built upon by subsequent researchers. For natural scientists there is no need to qualify the word ‘theory’ by the term ‘empirical’. It is literally unthinkable that theory would not have mirror image empirical referents, or if lacking today they are assiduously sought for, as in the current case of string theory.

Matters differ, however, in the field of sociology. Here empirical theory exists along side of widely regarded philosophical efforts, so-called ‘theories’ whose empirical referents are obscure, and along with points of view that are little more than ideologies that are assumed be valid merely because they have large followings.

Sociology’s field of stratification has had its share of both empirical theory and of so-called ‘theory’ whose empirical referents are seemingly absent (Parsons 1953).
The Present Paper.

This paper offers a few central contributions to today’s empirical theory of stratification beginning six centuries ago with the publication of Ibn Khaldun’s (1377) monumental, *The Muqaddimah: An Introduction to History*, in which he lays out a theory of stratification by power as the corner stone of the system of sociology that follows in the same volume—the thought system he calls, interchangeably, ‘sociology’, ‘the science of civilization’, and ‘the science of human organization’. This line of stratification theory picks up again in the late 19\textsuperscript{th} and early 20\textsuperscript{th} Centuries, with contributions by Max Weber (1946, 1925) and Pitirim A. Sorokin (1927). Then it picks up again around the 1960s, with the rise of quantitative analysis in sociology in writings by Svalastoga (1964). Duncan (1968), and Haller (1970, 2000). Duncan’s essay clarifies Svalastoga’s to some extent (albeit with a cloudy vocabulary), and Haller’s adds a bit more clarification, especially regarding a distinction between substantive hierarchies (‘content’ dimensions) and their statistical expressions (‘structural’ dimensions) that measure the ways stratification systems may vary from time to time in a given society and from society to society. The paper goes on to discuss measurement issues and the connections between the theory of power and other theoretic concerns of stratification, including Marxian theory, status allocation processes, and social mobility research.

Ibn Khaldun

As Toynbee has said, Ibn Khaldun’s is “undoubtedly the greatest work of its kind that has ever yet been created by any mind in any time or place. (Toynbee, 1935).

Looked at as a whole, his project is to explain history. This, he held, requires a theory of human organization. To explain human organization (i.e., sociology) a theory of stratification by power is needed. So the theory of stratification by power is basic to the field of sociology. More exactly, to Ibn Khaldun, sociology is the theory of history, and the theory of power is the key to understanding sociology.

Power, however, is not a one-sided concept. To him, the sultan or caliph as the case may be, had absolute power. That is, he had the power to inflict sanctions completely arbitrarily if he so decided. Yet delegates were needed to carry out his will. The power they exerted is in the form of authority, as authorized by the supreme power holder. The structure of the Sultan’s power system was two-layered: he that had absolute power and all others. The structure of the underlings’ authority was graded. (And note that neither is authority one-sided; the mirror image of authority is responsibility. Authorities are required to carry out the duties of their positions. This is only implicit in Ibn Khaldun, as it is also in Weber and subsequent theorists.)

The overriding power structure of the ruler’s society was fixed. The ruler, with his followers, held it or lost it. Though the structure didn’t change, its personnel composition could and did change. These changes occurred when a competing group succeeded in overthrowing the incumbents. In his time and before, this usually happened because a group of fierce and unsophisticated nomads, often Bedouin tribes wishing to taste the
fruits of the cities, would mount an attack. The leader of such groups maintained his authority by means of what Arab writers called ‘asabiyah’. The usual English translation of this term is ‘group feeling’. But it is this and more. Asabiyah also means ‘mutual affection and willingness to die for each other’ (Cartledge 2006).

During the early years of the new ruler, his power continued to rest on the asabiyah of his revolutionary followers. But if he or his dynasty stayed in power, the security provided by his original followers gradually withered away, being replaced by underlings in more or less like ‘bureaucrats’ who were presumably more competent. Also with the passage of time, the ruler and his clique became more accustomed to the delights of urban life and, feeling secure in their positions, less attentive to the maintenance of power. If things went on long enough they would become increasingly aware of their own vulnerability, then tending to rely more and more on the military for security. Yet every ruler or dynasty would sooner or later be overthrown by newer, more aggressive usurpers.

In short, Ibn Khaldun’s theory consisted of a single content dimension consisting of absolute power, with two strata as its only structural dimension—the ruler and everyone else. Those who carried out his will did so with the authority granted by him. The structure itself was fixed. But its composition could change. Such changes were effected by attacks from outside groups whose leaders maintained their standing in the group by their own deep personal attractiveness, which in turn generated the group feeling called ‘asabiyah’.

Thus power cycled from group to group throughout all time.

The empirical base of this theory rested partly on Ibn Khaldun’s knowledge of his own Arab societies. But there was more. Arab scholars of the day were quite familiar with what we today call ‘ancient history’. Along with others, he was well informed about Greek, Byzantine, and Persian history. He may also have read western Roman history. Today’s scholars may read much of the same literature. Much of it is thoroughly objective. But, unlike Khaldun, it shows no evidence whatsoever of a theory behind the sequence of changes it recounts. Neither, for other examples, does the Greek ‘father of history’, Herodotus, although Thucydides comes close when he attributes the cause of the earthshaking Athenian-Peloponnesian War to Athens’ rise in wealth and power and the ‘alarm’ this raised in Sparta. But Khaldun had even more to go on than recorded history. Some of his evidence was from his own direct observation, for example, the discussions he held with Tamerlane (Hourani 1991), the conqueror of an empire from India to Syria and Anatolia.

Apart from his knowledge there is also his obvious objectivity. He lets nothing stand in the way of recording his observations as he sees them.

In general, the basic lines of the empirical theory of stratification by power were laid down many centuries ago by Khaldun.
Max Weber.

The objectivity and profound historical knowledge of Khaldun is matched by that of Weber (e. g. 1950, 1951, 1976). He of course had advantages that the former lacked. Five hundred years of history had passed and considerable historical research had accumulated between the times of the two, which no doubt added even more depth to Weber’s store of knowledge. Besides this, European historians and anthropologists had contributed well documented information about world societies that was not available in the times of the great Arab thinker.

Weber’s main contributions to the theory were four. First is his recognition that the underlying phenomena of stratification are instances of power. The second is the specification of three content dimensions of power, rather than Khaldun’s single one. These were the power held by each of three types of collectivities: the power of political parties, of economic classes, and of the honorific standing of ‘status groups’ (Weber 1946). The third is his definition of power. “(T)he probability that one actor within a situation will be in a position to carry out his own will despite resistance, regardless of the basis on which this probability rests” (Weber 1925:153). The fourth is his recognition of authority, a type of power (Weber 1925: 324-423).

Nevertheless, more needs to be said about ‘power’ and ‘authority’. Though he appears to be quite clear about the relationship between the two concepts, at least implicitly, his statements about them could use a little improvement. The problem is that the word ‘power’, despite his careful definition, is ambiguous. On the one hand it means the same sort of power that was the primary concern of Khaldun: power that is absolute and arbitrary. On the other hand, the term refers to a general concept that includes authority as a special case. Authority, as he knew very well, expressed and effected within a set of norms that specify its limits and the sanctions that are applied when they are overstepped. That is, authority implies responsibility: authorities are held responsible for their applications of the authority they possess.

The consequence of this that it is not clear exactly which form of power (in his thought) is held by his three types of power holders, or under what conditions might they hold one form or the other (Weber 1946, p.195). For example, the power of economic classes is usually authority in that their range of action is limited by legislation. But is it always so? Then, too, the power of the parties of totalitarian governments may be applied without any limitations. And the power of status groups appears to be a form of authority (Weber 1925, pp. 428-429).

Was Weber influenced by Khaldun? There is a hint in ‘Class, Status and Party’ that he may have been, but one seeks in vain for any direct evidence of it. The hint is his concept of ‘communal action’: “…action which is oriented to the feeling of actors that they belong together” (Weber 1946, p. 183). This appears to be much like Khaldun’s ‘asabiya’.
For present purposes, Weber’s three types of power holders may be seen as the vehicles for three content dimensions of power: political, economic and status (social honor (Weber 1946: 187) which is the same as prestige (Weber 1925:428).

Despite his nuanced discussion of a number of aspects of power, Weber says little or nothing about structural dimensions. This may be due to his dependence upon his well known ‘ideal typical’ form of analysis. Thinking in terms of structural variability rests, it is obvious, on variables. Structural dimensions, like those of content dimensions, are conceptual variables whose levels are represented empirically by measured variables.

Pitirim Sorokin.

For better or worse, Sorokin is best known as the father of a long and distinguished line of research on social mobility. This line, however, is not his most profound contribution to understanding stratification phenomena. That contribution lies in his more fundamental concepts of stratification. As Otis Dudley Duncan has noted, it was Sorokin who pulled the field together into most of its present form (Duncan, personal communication).

Whether by intent or coincidence, Sorokin (1927) transformed Weber’s three content dimensions of power into conceptual variables, each of which is a form of a more general concept of social stratification. “Social stratification means the differentiations of a given population into hierarchically superposed classes. It is manifested in the existence of upper and lower social layers. Its basis and very essence consist in an unequal distribution of rights and privileges, duties and responsibilities, social values and privations, social power and influences among the members of a society….If the economic status of the members of a society is unequal, if among them there are both wealthy and poor, the society is economically stratified…(I)f there are rulers and ruled, then whatever their names (monarchs, executives, masters, bosses), these things mean that the group is politically stratified, regardless of what is written in its constitution or proclaimed in its declarations…If the members of a society are differentiated into various occupational groups, and some of the occupations are regarded s more honorable than others, if the members of an occupational group are divided into bosses of different authority and into members who are subordinated to the bosses, the group is occupationally stratified….”.

His general concept covering all of the three content dimensions is social stratification. His specific content dimensions are economic stratification, political stratification, and occupational stratification.

He also has two structural dimensions. One of them is the height (or range) of the content dimension, or what we are calling the ‘overall level’. The other is inequality, though that is not the way he labels it; he is obviously thinking of pyramids—some higher than others, some lower; some wider at the bottom, some narrower. Each of these applies to each of his content dimensions.
But he does not see the states of the dimensions as fixed. They vary from time to time: he speaks of ‘heightening’ and ‘flattening’. Yet he has no underlying theory that tells why and in what ways stratification structures change through time. Indeed, he says there is no pattern to such changes.

Another of his contributions lies in his attempts to use quantitative data to support his analyses. For example, he quantifies occupational stratification levels by resorting to the IQ data his University of Minnesota colleagues in the department of psychology calculated on the civilian occupations of the American soldiers in World War I. (No one would do that today because it confuses prestige and intelligence, and because we now have more appropriate measurement devices.) Nevertheless, his efforts at quantification were ground breaking.

For another contribution, he is apparently the source of the famous (or infamous, depending on one’s point of view) ‘functional’ hypothesis (Davis and Moore, 1945). “In any given society, the more occupational work consists in the performance of the functions of social organization and control, and the higher the degree of intelligence necessary for its successful performance, the more privileged is that group and the higher rend does it occupy in the interoccupational hierarchy, and vice versa” (p. 101).

Svalastoga.

Something as simple and commonplace as differences in wording may have obscured underlying identities of some of the concepts of stratification theory. For example, that which Weber calls the distribution of power is the same concept as Sorokin’s stratification. At other times a writer may use a general term, like power, when he really means authority, a particular form of power. Such complications are also to be seen in Svalastoga, Duncan, and beyond.

At his hands, Sorokin’s ‘stratification’ (1964) becomes ‘status’. The words he uses for his content dimensions are political status (also called power), economic status, social status, and informational status. To some extent the meanings of these are clarified by his or others’ efforts at measuring the variables for which they stand.

For political status, he comments on earlier work in which Dahl (1957) tried to measure the influence of senators. (This, however, provides little if any help in measuring this variable among the population at large, a topic he avoids.) He is equally vague about the measurement of economic status. His discussion of social status measures is much broader, with an already long history. In the end he settles for the prestige of occupations, with a comment on Duncan’s (1961) socioeconomic index of all occupations. Regarding informational status he, like most researchers since, falls back on the number of years of education of formal education.

Svalastoga’s concepts and measures of what he calls parameters (the content dimensions of stratification systems) is a distinct advance over Sorokin. He lists three of them—in addition to a brief remark about what are really two more. The three are degree of inequality, intercorrelation, and permeability/social mobility.
In fact it is income inequality that he treats, ignoring the inequalities of his other status content dimensions. For income he relies mostly on the Gini coefficient. Intercorrelation (our crystallization) is a parameter that he develops rather fully. This is one way of seeing the degree to which a stratification system is open or closed. High intercorrelations mean that those in the highest level of the stratification structure hold a monopoly on all the four content dimensions, while those at the bottom have none whatsoever, and so on up and down the line. All the rich are politically powerful, well educated, and of great prestige. And the humble are deeply so. Such high correlations would also mean that everyone is locked into a fixed position (which of course could change with time, as the society itself changes.) Naturally, these two extremes are extremely unlikely to be encountered in any society anywhere. Individuals’ standings on this structural dimension have implications for other domains of behavior, such as status inconsistency (Lenski 1956) and its own possible consequences for social behavior. (For example, in a society with a highly crystallized status structure, anyone whose statuses are greatly out of line with each other would be seen as a social deviant. Presumably, social deviants are more likely to exhibit unusual or even bizarre behaviors, perhaps such as dysfunctional personalities or alienation, etc.) It is to Svalastoga’s credit that he recognized that status crystallization is a structural phenomenon of society, and to Lenski’s that he saw that persons whose statuses are out of balance is likely to show unusual behavior.

The last of the three structural dimensions Svalastoga highlights is what he calls permeability. The basic idea is good. But he seems to have difficulty in knowing how to treat it. He seems to have had individuals’ movements across status lines in mind. This led him to leave permeability as such and to devote his attention to mobility. And even for that, he wrote only about occupational (‘social’) mobility. As will be seen later, permeability is best treated as the degree to which individuals’ positions are free from those of their forbears. And status mobility, as will also be seen, is really combination of the overall level of a given content dimension and the degree of permeability. Moreover, each (mobility and permeability) is a concept that logically applies to all content dimensions, not just to occupational status.

Having said this about the third of his parameters or structural dimensions, it must be said that he served the field well by calling attention to it in ways that help other theorists to think about it.

Earlier, it was noted that in passing he brings up two additional structural phenomena that have turned out to be important and which logically belong to his list of parameters even though he seems not to have seen this. They are the mode structure and the degree of skew of the distribution of a content dimension.

Mode structure refers to the possibility that the frequency distribution of a given content dimension may display ‘hills and valleys’, and that the hills may be close to each other or far from each other. That is, the distribution may be monomodal, bimodal, trimodal, etc. These may vary in size (the portion of the population so contained). And the distance between these modes may vary from time to time within a society or from
society to society. Modes on the distribution of a power variable are representations of real-world classes that differ in both the degree of power each holds vis a viz each other and of the size of the numbers within each. History is replete with instances in which such imbalances of power have sparked social discord and revolution. That is why mode structure is an important structural dimension of a stratification system.

In passing, Svalastoga mentions skewed (or asymmetric) frequency dimensions of status variables (content dimensions). Though he discusses instances of these, he says nothing about their consequences. It is this that matters. The degree to which the power structure of a society is skewed is its own special form of equality and inequality. The greater the skew, the more unequal. Some societies have relatively low levels of asymmetry, others high levels. In Brazil in 1973, common sense had it that the developed South was the least unequal region of the nation, and that the north was the most unequal. In this, common sense was not alone. It has been held by stratification experts (Lenski 1966; Treiman 1970) that the higher the level of development, the lower the degree of inequality. But frequency distributions of three content variables on Brazilian data (Haller 2000) show a different pattern. As measured by the variability, the south was clearly more unequal, and a pair of northern states seemed to be the most equal. The distributions of the two northern states showed wildly right-skewed asymmetries—the ‘inequality’ of the northerners was of an ironic form, an equality of the ‘outs’ in which a tiny elite towered over all the rest.

At Svalastoga’s hands the theoretic base he drew upon from Weber and Sorokin was rendered amenable to modern statistical analysis. He introduced concepts that are fair to the theory and that are measurable. In this, he added a quantitative foundation for the theory to the historically sophisticated evidence of Khaldun, Weber and Sorokin.

Yet even his omissions are instructive. They tell us where to look to fill in the gaps in his operationalizations of the theory, such as his failure to develop the parameter he called permeability. Similarly, he didn’t carry his comments about modes and asymmetry far enough. Also, he seems to have missed Sorokin’s point about the states and changes of the overall level (‘height’) of societies’ stratification systems. Beyond all this, his analysis implies the existence of a matrix defined by his four content dimensions and his five structural dimensions (three clearly presented, two mentioned by not developed.) But he failed to discuss the phenomena of some of the cells of the matrix.

**Duncan.**

To begin with it is well to point out that Otis Dudley Duncan was among the very most influential sociologists of his time, and that his impact on the field was due not only to the volume of his research production but more importantly to the extreme care he took with the quality of reasoning (i.e., theory) and of evidence. His essay on the structure of stratification (1968) was written as a contribution to a book on social
indicators (Sheldon and Moore 1968), a major word that was concerned with change in
American society.

His view of the nature and structure of stratification systems draws on both Sorokin and
Svalastoga. But in view of the book’s focus on the changing structure of America society,
his main inspiration in this essay came from articles by Elbridge Sibley (1942) and J. O.
Hertzler (1952). Sibley was concerned with upward social mobility as a defense against
possible increases in class consciousness and class hostility. In particular, he reasoned
that such class antagonism would increase unless the educational system could attract a
greater volume of talented individuals. Hertzer’s concern, that the class system might be
closing, was another way of looking at the issue raised by Sibley.

The end product of Duncan’s essay is a version of his well known model of the
process by which occupational statuses are allocated to individuals (Blau and Duncan
1967, Chapter 5), with an emphasis on how it might be used to record trends in the
degree of stratification. His discussion of stratification theory, however, begins with
comments on Svalastoga.

Regarding content dimensions, he acknowledges Svalasoga’s four types of status. But
he says two of them are not specific enough. One of the two is political status, which he
sees as having two forms, political and civic. The other is economic status. He divides
this into production and consumption. He also calls attention to the difference between
the flow and stock of a status variable, which applies most clearly to economic status:
income as flow, wealth as stock. His structural dimensions are the same as those of
Svalastoga. But is words for two of them are different. They are degree of inequality,
rigidity of inequality (Svalasoga’s intercorrelation), and rigidity of stratification.
Although he doesn’t make it explicit, it is clear enough that he implies that on logical
grounds the description of a stratification system requires the application of each
structural dimension to each content dimension.

In a way it is obvious that nations differ in degree to which they are stratified. But
like so many other sociological phenomena, a vague awareness of this is not sufficient.
Duncan provides a definition of this aspect of society which definition flows directly
from the structural dimensions that he fleshed out from those partially presented by
Svalastoga. The degree of stratification of a society depends, he says, on three things: the
dispersion of the measures of each content dimension, the degree of crystallization
(intercorrelation) of each, and the ‘coefficient of intergenerational status transmission’ of
each.

He devotes a little attention to classes, their boundaries, and the issue of class
consciousness, but leaves research on them as “an agenda for the future of sociology”.
Having thus disposed of upward and downward mobility, he then moves to his key
illustration of ways to think about assessing trends in the structure of society—
measurement of the rigidity of stratification, Svalastoga’s permeability.
His discussion of the measurement for content dimensions is sketchy, dealing only with income and political status. Beyond doubt, he saw the question of valid and reliable measuring instruments as rudimentary at best. Income inequalities and inequalities of wealth he left to economists. Regarding political status he simply points out that no way to measure it had yet been devised. Though he doesn’t say so, it’s clear enough that he sees his socioeconomic index (Duncan 1961) as—for now—a reasonable way to measure social status. And he uses educational attainment as the indicator of informational status.

Looked at as a whole, what he did in his efforts to show how to think toward the measurement of trends in the stratification system of the United States, was to clarify the theoretic position of Sorokin as it was explicated and elaborated by Svalastoga. In the continuing evolution of stratification theory that was begun by Ibn Khaldun, Duncan’s statement of it is not the last word. But it is a distinct improvement.

**Haller.**

The present writer, too, has tried to clarify the theory (1970, 2000) by building upon and fulfilling the partial conceptualizations offered by Weber, Sorokin, Svalasoga, and Duncan. This began as an effort to find a way to handle an question that raised itself in the early development phase (mid-1950s) of status attainment research. At that time sociologists had little notion of the structure of stratification systems and even less of the changes such structures could pass through within as short a mere generation. If this were true, status attainment behaviors of individuals would be affected by such changes. But nobody knew how to reason systematically about them. By chance, weak but suggestive evidence of short term rural stratification changes in Brazil showed itself. It seemed reasonable that if this were true in that country, then something like it was probably true everywhere. This led to original field research in Brazil and to use it to try to learn how to think about changes in the structure of stratification. In 1964 Svalastoga’s book came out, which along with Sorokin (1927) helped to understand what had been seen in Brazil. As planned, the results fed into conceptual improvements of status attainment processes (Haller and Portes 1973; Haller 1980), which themselves were built on models published around the late 1960s (Duncan 1967; Sewell, Haller and Portes 1969; Sewell, Haller and Ohlendorf 1970).

As it stood in its latest print publication (Haller 2000), the basic form of the theory of stratification structures remained as it was when it was first published in 1970 (Haller 1970), and was the result of combination of the work of Svalastoga (1964) and Duncan (1968) on the one hand, and the field experience in Brazil on the other. It was the 1970 article that introduced the concepts of content and structural dimensions of stratification systems.

Like Svalastoga, it held that there are four content dimensions of status: political, economic, social (prestige) and informational. Clarifying Sorokin, it took ‘height’ to mean the central tendency of the instruments measuring each content dimension. Again like Svalastoga, it suggests variability statistics to measure inequality and
intercorrelations of measurements of the content dimensions to measure the degree of crystallization. For the latter’s ‘permeability’ it took as its measure the degree of offspring-from-parent coefficient of alienation (1 minus the square of the correlation coefficient). It also added the degree of skewness (asymmetry) and the mode structure. So it held that in any society the structure of the stratification system could be described by the values of each of the structural dimensions, five for each of the four content dimensions plus crystallization, 21 different parameters ((4 x 5) + 1). The only change published since 1970 (Haller 2000) drops the word ‘status’ and following Weber replaces it with ‘power’. This is in recognition that all content dimensions are expressions of one form or another of the power to effect changes in others’ behavior, whether through absolute power, authority, influence (legitimate or illegitimate).

A later publication (Haller and Sharda 2005) carried this formulation one step further by adding a definition of the degree of stratification of a society. (To the extent to which the same components contributing to the measurement of this variable, they are consistent with Duncan’s (1968) definition.)

Stratification structures with a low degree of stratification would have each of the following characteristics:

1. A low level of absolute inequality (dispersion);
2. A low level of intergenerational inheritance of position;
3. A concentration of individuals on a single mode;
4. A low level of skewness;
5. A low degree of crystallization (intercorrelation) among the content dimensions.

Stratification structures with a high degree of stratification would have these characteristics:

1. A high level of inequality (dispersion);
2. A high degree of intergenerational inheritance of position;
3. More than one mode on each content dimension, and a marked tendency for the same individuals to occupy the same mode on each content dimension;
4. A high degree of skewness;
5. A high degree of crystallization (intercorrelation) among the content dimensions.


As has been seen, there has been a consistent development of the theory from its first statement in the remarkable work of Ibn Khaldun centuries ago, through the 1960s. This, though there is little if any evidence that his work was drawn upon, even by historically sophisticated scholars like Max Weber or Pitirim Sorokin. Weber, like Khaldun, saw its fundamental content dimension as one of power, and he advanced the theory in several ways, notably by calling attention to different forms of power held by three types of actors, and by distinguishing between absolute power and authority, the latter of which is
effected under sanctioned rules. Sorokin transformed Weber’s three forms of power holders into conceptually equivalent content variables, and showed that stratification structures vary in terms of overall level (‘height’) and inequality. Svalastoga added a fourth content dimension—informational differences. The net result of these elaborations of the substances of stratification hierarchies was to define them as political, economic, social and informational. These, with minor qualifications, were also agreed upon by Lenski and Duncan. Along with Sorokin, Svalastoga contributed to understanding of the structural dimensions of stratification (the ways each content dimension may vary) by introducing the concepts of intercorrelations (crystallization) and permeability (the degree to which such levels of content dimensions are inherited). But he didn’t do this systematically; Duncan did this for him. Duncan’s list of structural dimensions is identical to Svalastoga’s though his names for them are different. Svalastoga also commented on the degree to which the frequency distributions of content dimensions may be skewed (the degree of asymmetry of each) but he didn’t elaborate on this. He also hinted at mode structure as another. He didn’t discuss this either. So in the last publication (Haller 2000) accepts each of the four content dimensions, all of Svalastoga’s structural dimensions (with Duncan’s clarifications) and adds just one other to them: Sorokin’s level (‘height’). Like Weber, he sees them as power variables. With these specifications, and with the proper data, it is possible to measure the variations within and among stratification systems in any society.

The question of the mechanisms that induce changes in the structure of stratification systems remains open. True, there are important historical cases of such changes that seem to be driven by economic development. Particularly significant are the political emancipations of slaves and women that have occurred in many societies. Besides establishing legal political equality these have provided openings that at least have helped large numbers of formerly disbarred people to enter the competition for higher standings on each of the content dimensions. Still, apart from their effect on the legitimate political empowerment of these sectors of societies’ populations, the extent to which these events have changed the structure of stratification of such societies remains unclear.

With somewhat different emphases, Lenski (1966) and Treiman (1970) have proposed that development induces destratification. Apart from the revolutionary effect of development on the emancipation of vast numbers of people, the validity of this hypothesis in more recent times remains in doubt.

Lenski’s form of the hypothesis appears to hold that economic development is driven by improvements in technology, and that before industrialization such development raised the average level of a society’s population on most or all of the content dimensions (his ‘power, privilege and prestige’) while increasing the degree of stratification. After industrialization, however, this trend was reversed and development reduced the degree of stratification. Empirical tests of Lenski’s concept of privilege appear to apply mostly to the degree of inequality of economic power, but logically it may also be applicable to inequality of social and informational power. In any case it is extremely difficult to test at the level of the individual member of a society because only some societies have the data to do it and these tend only to be the most developed.
In addition, what is called ‘development’ may not be just one thing. According to evidence adduced by Sarda, et al.(1998) and Haller and Sharda (2005), the most dependable published indicators of development are composed of two factors, ‘Domestic’, or socioeconomic development, and ‘International Authority’, and these two may often be at cross purposes. Beyond this, using major socioeconomically disparate regions of Brazil, Haller (2000) showed data that amounts to a test of the Lenski hypothesis. It does not support it.

Treiman’s (1970) form of the hypothesis holds that development—industrialization—reduces the degree to which power (his ‘status’) is transmitted between generations. Again, the Brazilian data just mentioned fail to support it.

Naturally, the best tests of these two hypotheses would have to compare nations, rather than regions. But this is impossible with the available data sets for at least three reasons. First, good data are not available for most less-developed nations. Second, where it exists the data are not always comparable from nation to nation. Third, development appears to be two different things, as pointed out above.

Thus, if any conclusion can be drawn concerning the ‘development-induced destratification’ hypothesis, is that it appears to be partially right as it applies to the effects of emancipation, but it is that it may be wrong as it applies to the forms of it offered by Lenski and Treiman.

Nevertheless, fundamental problems remain with all 19th and 20th Century statements of the theory. First, they all deal with stratification systems as if they were in societies without internal changes and as if each exists in a vacuum. In the real world any society undergoes demographic and ethnic changes of composition as well as changes economic and other changes. Besides this, nations are often in competition with one another—to paraphrase, ‘no society is an island unto itself’. That is, very society is impacted to one degree or another by two kinds of forces: some that are external to it and some that are due to internal changes. Both types of forces may induce one degree or another of conflict or the threat of conflict. External provocations include warfare, economic pressure, perhaps cyberwar. Internal provocations include economic changes such as severe depression, unequal population growth among seriously competing ethnic groups, etc.

Inevitably, such actual or feared conflict affects the orderliness of a society, and the degree of orderliness affects the form of authority employed by its rulers. Ruling groups will routinely act either with absolute power—which is arbitrary and operates under the acquiescence of the population; or authority—which is legitimate in that it operates under rules and sanctions established with the consent of the population; or some combination of the two. As Weber (1925) pointed out but failed to develop, “In wartime…authorities are in a position to make use of powers which would generally not be tolerated in peacetime except in cases where the subjects are the ‘slaves’ of an authoritarian state.”
Because provocations impinging on a society increase and decrease all the time, any society's degree of orderliness will vary along with them. This will have two consequences of importance to the form of its stratification system. The first is that the level of orderliness may oscillate over long periods of time or shorter ones, and at greater or lesser levels of amplitude. Oscillations that are fast or slow and of different degrees of amplitude will exhibit curves more or less like sign waves. Abrupt changes would show irregular waves. Besides oscillations of the degree of orderliness there is also the question of the overall level: the rises and falls may vary over a generally higher or lower average level. That is, the level of orderliness of a society may be rather high for long periods despite its ups and downs.

These considerations suggest four sociological laws—offered tentatively—of the kinds of power employed by rulers and of the structure of stratification as these vary under different conditions of societal orderliness.

Law 1. The more orderly a society is at a given period, the greater is the tendency of its rulers to govern by authority and to be held responsible for their actions, and the lesser the tendency for its rulers to govern with arbitrary exercises of power.

Law 2. The less orderly a society is at a given period, the greater is the tendency of its rulers to govern by arbitrary exercises of power, and the less is the tendency for its rulers to govern by authority.

Law 3. The more orderly a society is at a given time the lower its degree of stratification.

Law 4. The less orderly a society is at a given time the higher its degree of stratification.

There is still another issue that must be addressed in the interest of clarifying the empirical theory of stratification. It is the meaning of the concepts of authority and of influence.

The current form of the theory recognizes three different kinds of power, two of which have already been introduced. The concept of arbitrary power needs no further clarification. That of authority does, however, and so does that of legitimate influence.

Measuring legitimate political influence by valid and reliable techniques applicable to the citizenry as a whole has always posed one of the most vexing conundrums facing stratification researchers. This is a serious problem because the power of political authorities, and the structural variations of such power, are perhaps the most important among the content dimensions describing stratification systems. Efforts at such measurement have been frustrated from Sorokin’s time to the present. A spate of efforts was tried shortly after World War II. For various reasons they were failures. And as noted in the beginning of this paper, any measurement device that is a faithful mirror of the
conceptual variable it purports to represent must be based on a clear understanding of the conceptual variable itself. This is fundamental to empirical theory in sociological research, just as it is in the natural sciences. The failure to confront this directly was the basic source of the failures of the past. Because this paper deals (later) with the measurement of political power, it is essential that the conceptual questions be addressed.

Positions of authority are bounded by rules and sanctions that are supported by those to whom such positions are responsible, including to those in positions with supervisory authority over them and to those who are subject to their incumbents’ directives. In a word, such positions are legitimate in the sense that they are held by the consent of those affected by the actions of their incumbents. This means that each level of authority in an organization is mirrored by a corresponding level of responsibility. It is these aspects of the power of authorities that distinguishes it from the arbitrary and often capriciously executed power of absolute rulers.

Any organization, most notably governmental, may experience attempts to influence it. Sometimes such attempts are legitimate and sometimes they are illegitimate. Legitimate influence is that which is communicated to and acted upon by an authority in accord with a set of sanctioned rules. For political power these derive from the citizens; in large private organizations they are set by boards, sometimes with the consent of the bulk of the participants.

Thus this paper’s stance on the current theory of stratification accepts all the parts of the theory as these were gradually brought together in earlier writings, and it adds two major parts. That is, it accepts the specification of the four content dimensions and the structural dimensions of its predecessors, and goes on to add certain structural dimensions that had been neglected earlier. It criticizes previous statements for ignoring the fact that no society stands alone but is influenced by others, and that such influences shape the degree to which it is or is not orderly, a phenomenon that affects its stratification system. Finally, it offers an explication of the centrally important concepts of authority and influence as parts of the theory.

Measurement

Content Dimensions: Social and Informational Power. As is well known, a substantial consensus has grown up on ways to measure these two content dimensions.

For social power, the two most widely used seem to be Duncan’s (1961) SEI (socioeconomic index) scores and Treiman’s (1977) occupational prestige scores or some derivative of them. As Featherman and Hauser (1978) have shown (Table 2.4, p. 327) the two were shown to be quite highly correlated on American data (r = .84). Both have undergone minor modifications by other researchers. Duncan argued that the social status
of an occupation is a function of the demand for the persons to fill it and the rewards that it provides. This led him to choose the average education of those in an occupation and the average income its occupants obtained. He calculated their respective weights by regressing certain census occupations on a small comparable on the values of those in a set of job titles whose standing in the population had been measured earlier (North and Hatt 1947). This rationale and the components it suggests have been accepted by many researchers ever since, sometimes with updated weightings of education and income.

Treiman’s occupational prestige scores are taken from the average evaluations of job titles by respondents in country after country. He concludes that the resulting hierarchy of occupational prestige is the same everywhere. SEI-type scales work well in the United States. Treiman’s may too. The extent to which Duncan’s SEI is valid abroad is an open question. For example, Brazilian researchers (Pastore 1982; Neves, Fernandes and Diogo 2005) prefer to use scores like SEIs which are calculated especially for Brazil (Silva 1973). Treiman’s scores or minor variants of them are used by some researchers in comparative international studies (e.g. Kelley and Evans, 2006, personal communication).

For informational power, it would appear that educational attainment may be the only informational power indicator used by stratification researchers everywhere. The general consensus seems to be that it’s best assessed in terms of the actual number of years of formal education successfully completed, from zero years through university, and on up through study toward advanced degrees. Obviously, there are differences in the quality of education among schools in any given country, and differences in the selection of students and of the forms and quality of education among countries. It appears to be assumed that the total informational effect of the number of years accumulated is so powerful as to offset the effects of such differences. Even for geographically isolated peasants this variable has been shown to be highly reliable over two months—test-retest: \( r = .93 \) (Haller and Saraiva, 1972). On the other hand, the validity of data on education is mostly assumed on the basis of experience rather than being tested.

Economic power is measured either by data on income or on wealth—usually the former. For income, respondents may be asked to tell the interviewer in any one of several different ways: total annual income, income annualized from monthly or weekly pay, total earnings, total household income, etc. Wealth may be trickier and is rarely if ever sought, even though as Duncan (1968) remarks it is probably the best measure of economic power (his ‘status’), because it is a steadier indicator than income.

Legitimate influence in the political arena and in formal organizations has been a stumbling block for stratification research ever since Sorokin raised it as an issue. This measurement problem is given extended treatment as follows because the issue is so important to stratification theory and research and because it has been so difficult to resolve.

Just after World War II, while attempts to determine the power holders in communities (Aiken and Mott 1970) were in vogue, a number of researchers made
attempts to measure political power. The main three of these efforts were voting or not voting, position in a hierarchy of community authority, and the so-called ‘snowball technique’. Each had basic flaws. They all shared one flaw of overriding importance—which we will get to below.

Voting has the advantage of allowing a score for each citizen. One of its flaws is that the level of political influence it measured is very low. Another is that it’s values are merely one and zero; multiple point continuous variables are much superior. A third is that while most of those who say they voted probably told the truth, so the level of power so expressed is valid enough, there is no way of knowing the level of power of the those who didn’t vote, and indeed of the higher levels of power enacted by those who did vote. In short this method fails to identify the political influence levels actually employed by individuals, except for a low level reported by some but not all.

Office holding has the advantage of locating some people’s level of influence in a hierarchy of positions. Also this could be done for samples representative of entire populations, not to mention the smaller communities were it was most often employed. On the negative side, it misses a great many people whose political influence is expressed in ways other than in office holding.

In the snowball technique the researcher first identifies one or more persons who thought to have a good knowledge of who does what in the community. These peoples’ indications lead to others, and so on. This provides a pool of influentials.

There are several problems with it. It is haphazard in its identification of power holders. It normally yields values of only one and zero (those with power, those without). And it is useless among populations larger than small communities.

The problem basic all of three is that they assume the researcher can either map the structure of legitimate applications of power by inferring it from positions actually held by some or from reported activities of some whose names happened to fall into the hands of the researcher. Voting or not simply ignores all the truly influential.

This paper holds that it is a mistake to begin with individuals: that the way to proceed is first to map the hierarchical structure of authority within the organization whose participants’ legitimate successful exercise of it occurs, and only after that to locate a given participant’s influence as the highest level at which that influence was initiated. Within such an organization, it is the office holder who exercises authority. It is perfectly legitimate for some—or sometimes all—others to ask an authority holder to take an official action (provided the action would be legitimate). If acted upon successfully by the office holder (the one with authority) the petitioner would have exercised legitimate influence at the level of the one in authority.

Years ago research teams at the University of Wisconsin and at a pair of Brazilian universities carried out two published ‘experiments’ on the measurement of legitimate influence. The data for them were collected in Brazil. One, on political influence (LPI)
was a part of a larger field research project (N = 468) in an isolated rural region. (Haller and Saraiva 1972). The other was on organizational (LOI) influence levels within firms in Sao Paulo’s industry. This one had data on about 22, 587 specialized personnel in 688 firms (Pastore, et al. 1977).

For the first, LPI, a rank order of elective offices was determined at the outset. It had these positions: governor of the state, state representative, prefect (‘mayor’) of the municipio (‘town and county’), and municipio council member. There are other such positions of course. These have the advantage of a definite rank order. The resulting scale had two components. For the first, the scores that were possible were zero through five. For it the interviews the respondents were asked to state whether they had ever presented a request, with a successful result to an elective official, beginning with council member. The highest position chosen, if any, was the one that was scored. The second was an arbitrary scale of elective office holding, with scores of zero for those who were never candidates nor holders of and office, three for one who had ever been a candidate for the council only, four for one who had been a candidate at least twice or had been elected to the council, and five for one who had held two or more elective offices. The respondent’s total score was the sum of the two.

Unsurprisingly, almost all, 86 percent, had scores of zero. Another six percent scored either one or two, seven percent scored between three and five. Two percent scored six or more. The correlations (pearsonian) of these scores lay between .23 and .44 for education, income, occupational prestige, and a multiple-item socioeconomic status scale. The test-retest reliability of the LPI scale (data taken two months apart) was r = .84.

It seems fair to say that this method of measuring one’s level of legitimate political influence (at least as claimed by the respondent) avoided all of the faults of the types of earlier LPI devices that were criticized above. Indeed by normally accepted criteria, it appears to be a rather good instrument. But ‘normally accepted criteria’ are not enough. For one thing, it is a composite of two different scales whose interrelations should have been checked. On the surface the first of the two seems adequate, possibly marred a bit by being the respondent’s report of his behavior rather than having been checked by more objective means. The second component seems to be a combination of items that are not clearly comparable.

For present purposes, the conclusion is that the first of the two components is a promising way to measure legitimate political influence in an agricultural population that is poor and isolated, and that the scale’s apparent success in this circumstance suggests that analogous systems, if properly constructed for their own milieu, might offer a solution to the vexing problem of the measurement of LPI.

The LOI (level of organizational influence) scale is a combination of two concepts of LOI, those of line and staff (as in the military, where some one kind of rank order commands in battle versus the other, that provides technical services but cannot command in battle). In other words, of positions of authority on the one hand, and of support positions for those of authority on the other hand (Dahrendorf 1957). In this
system, the class of influence (line and staff) is cross classified by span of influence. This yields six cells, three of the line of authority and one each for the support personnel of those in the respective cells of the line. Each of the 22,587 specialized workers in the 688 firms was assigned to one or another of the six cells. Each cell was assigned a score representing its position in the intraorganizational downward flow of control (i.e., of influence). The top score is six, the next is five, and so on to one. Six, wide span = authority: directors and managers. Five, wide span = indirect: experts—engineers, attorneys, economists, consultants. Four, medium span = department heads and supervisors. Three, medium span = technicians. Two, narrow span = foremen. One: narrow = auxiliary office personnel. The resulting variable, LOI, was included in a path analysis of five regressors that were intended to explain variation in total remuneration (wages, salaries, and fringe benefits). Correlation coefficients are not available, but a good idea of the relevance of LOI can be seen by comparing its direct path coefficient to that of occupational preparation (OP: job specific education). For LOI, p = .32. For OP, p = .33. But since OP’s effects are both direct as well as indirect through LOI, the total effect of OP is greater, at p = .46.

The conclusion of this LOI exercise is that even despite differences among nearly 700 of Sao Paulo’s companies, a conceptually simple method of ordering the downward flow of legitimate influence within them (as an average over all of them) has been shown to be effective. Its explicit range was short—one to six—though there is one more (implicit) level of zero, which would have provided a score for each of the 94 percent of the employees at the bottom of the power structure.

That is, the strategy of first mapping the structure of influence; next, providing a score for each of the levels so ordered; and last of all, locating the employee in his appropriate level, made it possible to assign a score of legitimate influence for each of the 22,587 specialized personnel of this huge and variegated sample of 688 manufacturing firms.

The general conclusion to be draw from this presentation of the strategy of mapping the structure of the formal organization or type of formal organization in which authority is exercised may provide a way to break the barrier that has so long impeded advances in the measurement of authority in government and other types of formal organizations. This is not to say that it will be easy. Each type of organization—often, each particular organization—may have its own relatively unique structure of authority.

Democratic governments may be one of the more complicated. Here, only the barest suggestion may be made. Perhaps best, if most difficult could be based on the span of influence—the number of persons actually or potentially influenced by actions of each one in elective authority. It might offer a way to begin to form a continuous variable for legitimate political influence. Another, less complicated and possibly of somewhat lower reliability of measurement, might be the simple rank order of each position of authority, rather like the rank system discussed above for the rural group in Brazil. There could be one such variable for the United States as a whole. But each individual state would have its own such power structure.
Measurement: summary. Scales to measure two of the four power content dimensions have more or less agreed upon since Svalasotoga added informational ‘status’ to the three implied by Weber and stated clearly by Sorokin. They are educational attainment for informational power and occupational prestige power scales or occupational socioeconomic power scales. A third is economic power. It has two forms, income and wealth. There is more than one way to measure income—salaries, totals, etc.—but they are probably so highly correlated as to be interchangeable for many purposes. In times past this was too touchy to get from interviews, though today it is gathered routinely. Wealth may be more difficult, though except for the very wealthy, a family’s total assets might not be difficult to obtain.

Legitimate political influence is another matter altogether. For one thing, it is probably the most important of all four. For another, so far the attempts to measure it have either failed or are must be considered experimental. The failures shared one fault: they began with the individual when they should have begun by mapping the hierarchical power structure of the organization in which such influence is exercised. For this reason ways to think about this were discussed at length. The conclusion is that ways to measure may well be possible but they might not be as easy as those of the other three content dimensions.

Implications.

This section considers the relevance of the theory for selected lines of stratification research.

Class Analysis. As has doubtless been noticed, this presentation of the empirical theory of stratification by power did not include Marxian theory and measurement. This is for two basic reasons. First, as held by Marx it appears to be applicable only to two supposed historical power situations. One of the latter is ‘feudalism’, which essentially means agrarian hierarchies of all sorts and shapes. The forms of stratification theory presented in this paper allow for even these structures to vary. The other is that the theory is unclear about what happens to power structures after the fall of capitalism. Again, the lines of the theory presented herein written so as to allow for descriptions and analyses of stratification structures everywhere and at alltimes.

There is yet another problem with this line of theory. That is that in its ‘least common denominator’ it holds that all the four content dimensions of stratification theory are really are but one: the combination of economic power and political power that is fully monopolized by owners to hold down the life situations of workers. This indeed may happen in the course of history. But it is a mistake; first, to assume that it will always be the case that power rests in the hands of classes as we know them today; and second, to assume that this is the only factor at work in the distribution of benefits within capitalist societies. For a case in point, it has been shown that in one such society Marxian-type class differences work along side routine ‘status attainment’ variables to help to determine income differences (Bills, et al. 1985).
A more general way to put the problem with Marxian theory is that it appears to be rather inflexible as compared with the lines of theory presented herein. For example, the latter lines allow for a wide range of variation among stratification structures, including those predicted by class analytic theory but by no means limited to them. The current theory’s flexibility lies in the many ways structures of stratification ‘are allowed’ to vary in terms of the six structural dimensions of each of the four content dimensions.

Social mobility research. Research on intergenerational patterns of social mobility in Western society has been quite useful—not the least to governments bent on gauging the likelihood of class conflict. In this regard, it will be recalled that in the great depression of the 1930s, many in these capitalist democracies feared the possible rise of communism, both because some (including labor unions) thought that the capitalist system might be overthrown and because some thought Soviet style dictatorships might replace democratic regimes. Social mobility research provided a way to monitor this possibility.

This is not to say that the researchers themselves were motivated by such political aims. They probably were not. Instead, this line of research offered chances to do serious quantitative research on what was obviously a topic of great interest to many people—and not only to those with specific political agendas, but also to a much wider public. Also, ways to determine how to analyze mobility tables so that secure information could be extracted from them offered tantalizing challenges to the rising numbers of more mathematically sophisticated sociologists. There may also have been other reasons why so many capable researchers concerned themselves with mobility analysis. But it is doubtful that political biases were much involved. One such might be that the tables can easily be read by almost any educated person.

It is intriguing the Duncan (1968) was not particularly interested in this line of research. Probably the main reason was that he saw the careful measurement of trends in the degree to which one’s own position is determined by that of his or her parents as more useful than time series comparisons of mobility tables. Another may be that he saw intergenerational mobility as a composite of two more fundamental factors, each of which could be measured directly—the degree of circulation mobility (call it CM) and the degree of structural mobility (call it SM). CM is the same as the inverse of the more basic content dimension, status (power) inheritability, and CM is the difference between the statuses of the respondents and their parents.

Social and psychological consequences. These implications appear to be wide ranging and substantial. Consider, for example, the hypothesis (Lenski 1954; Goffman, 1957; Landecker, 1963). It holds that the degree of inconsistency of individuals’ positions on the content dimensions helps to determine prejudice and attitudes toward inequality. Hard evidence on this, however, has been hard to come by. Indeed, Treiman (1966) seems to have put the hypothesis to rest by showing that its apparent effects are spurious. So it has seems to have been taken for granted the whole notion status inconsistencies have consequences was wrong. But does this be true for all shapes of stratification
systems? Perhaps not. Status inconsistency may be so common in the United States that it really doesn’t have any such effect here. But in a society with a highly crystallized stratification system, such inconsistencies would be much rarer.

Carrying this reasoning a bit further, it might be the case that violent attempts to overthrow governments may be more likely to occur in societies with highly crystallized systems than in those with systems that are less crystallized. And would this be followed by repression and maybe even civil war? This may not be farfetched. Indeed it is high time for stratification researchers to use their theory and methods to look into the more violent effects of states of the structure of stratification systems. But a caveat goes along with this. Careful sociological research on such topics, may threaten to upset the myths of powerful vested interests.

Returning to psychological consequences, the question of effects varying with different forms and degrees of stratification may also apply to many other social attitudes and perhaps to forms and degrees of personality problems.

The point of all this is that empirical theory of stratification opens up many new possibilities for understanding behavior patterns in societies with different forms of stratification.

Development policy in Brazil. At one point it was decided to use the socioeconomic development regions of Brazil as proxies by which to test the ‘development-induced destratification hypothesis’. In principle it is possible to use the nation’s five official macroregions, which are defined by the boundaries of states, for this. However, anyone who knows much about the geography of Brazil is aware that some of its states are huge and some are tiny, that the population of some would dwarf that of most nations, while others are sparsely populated. But a good comparison requires that each region be more or less homogeneous. So it was decided to perform a special regionalization for this project. A different set of five regions were thus delineated (Haller 1982a). This was done with no thought of practical ramifications. Even so, it is known that the Brazilian government used it to determine new directions for development efforts. In addition its methods and results were picked up for possible use in other parts of the world. In one instance that is known, it was the United Nations Institute for Science and Technology for Development that did so. In another it was an agency of India’s national government. The diffusion of basic research findings and methods is, for the most part, cannot be traced without special research efforts. So the extent of the wider spread of these, if any, is unknown.

Status attainment research. Contrary to the seemingly volunteristic implication of its name, this theory is entirely deterministic. Another possible misconception is that its variables were selected ad hoc. Actually, theory rests on secure bases of prior theory and research as these were applied to original data. It contains both social psychological and social structural variables. The social psychological bases come from C. H. Cooley (1964), G. H. Mead (1934), H. S. Sullivan (1953) and K. Lewin (1951). From the first three comes the theory of the socioenvironmental development of individuals’ self
conceptions (sets of which are status-level aspirations) and the related theory of the influence of ‘significant others’ (who, by suggestion and illustration, help to form aspirations). From Levine comes the concept of level of aspiration. From research on intelligence and personnel selection comes the concept of academic ability. From research on status measurement comes the concept of status origins. Status attainment theory was developed by a University of Wisconsin group over many years, and long predated the name applied to it (Haller and Portes 1973). Its first publication showed the effect of parental status on youths’ levels of educational and occupational aspirations (Sewell, Haller and Strauss 1957). Its next important publication (Haller and Miller, 1963) presented the key concepts that were later incorporated into the emerging theory, and weighed the evidence for and against them from a total of nearly 184 available bivariate relationships from ten different studies. One of these studies (by Sewell and Haller, unpublished) was the first longitudinal status attainment project (1948-1955, Haller and Sewell). In 1965 Duncan sent the group a prepublication (Blau and Duncan 1968) copy of his path analysis of status transmission. This suggested the use of the causal model published in the next main writings on the theory (Sewell, Haller and Portes 1969; Sewell, Haller and Ohlendorf 1970). Later research (with Woelfel and Fink, cited in Haller 1982) yielded concepts and methods for identifying youths’ significant others and measuring their influence. Still another (Otto and Haller 1979) summarized results from several different longitudinal status attainment projects. They were all consistent with the theory. Indeed, the better the measurement of their variables, the more powerful the theory was seen to be.

As discussed earlier, the main lines of the theory of the structure of stratification had been worked out by various writers by the end of the 1960s. Implications of it were first applied to status attainment research in the early 1970s (Haller and Portes 1973), but without supporting data. This application predicts the impact of various degrees of stratification on the status attainment process. Specifically it holds that the status attainment process of highly stratified societies will be conceptually simple: all antecedent variables will ‘coalesce’ into a single line of influence, which line will determine the fully crystallized status hierarchy onto which the young are projected. At the other extreme, a completely destratified system would be one in which everyone is equal. Societies whose stratification structure stand at a middle level (neither highly stratified nor nearly unstratified) would be the most complex, with four main lines, each element of which would be cross-linked to others.

In its final statement to date (Haller 1982b) the theory holds that among youths the status origin variables of all four content dimensions, together with the status indications and/or illustrations of the youths’ significant others, will determine the levels of status aspiration they hold for themselves, and that these in turn will determine the levels of the four status trajectories into which they are projected. In goes on to predict that the process as a whole will vary (as Haller and Portes theorized) according to the degree of stratification of their society during their lifetimes. The increasing inequality of American society may offer an opportunity to test this hypothesis.
Minorities and gender. Much has been written about inequalities of race, ethnicity and gender, both in the research literature and in the popular press. Much of it is concerned with their empowerment.

It seems obvious that the application of the theory of stratification would improve understanding of the ways empowerment and its changes work. A research program that would systematically determine the changing levels of each structural dimension of each of the four content variables would serve to pinpoint the exact nature of the impediments to the advancement of women or any minority group.

Beyond this, the systematic application gender and minority differences as measured with data using the social psychological theory of status attainment processes could provide a better understanding of the mechanisms of discrimination.

Conclusion.

The framework of today’s stratification theory was constructed Ibn Khaldun six centuries ago. Over the past century it has been elaborated and refined in contributions made by several other empirical theorists. Sorokin’s were particularly important. He expanded Khaldun’s single content dimension into three—political, economic, and social. Svalasoga added informational. With boosts from them, the rise of quantitative analysis increased the number of structural dimensions by which to describe the structure of any stratification system from Khaldun’s two strata to six: central tendency, dispersion, heritability, crystallization, mode structure, and asymmetry. Weber’s reintroduction of power (absolute and authority) as the common element in the content dimensions—now seen as four—is, in a sense, a return to Khaldun’s power as his single content dimension.

As a result, the current theory of stratification provides a powerful and flexible undergirding for all of the different kinds of social inequalities of human life. This same flexibility also makes it easy to generated new hypotheses by straightforward deductions from parts of it.

This is not to say that empirical research employing the theory is easy or inexpensive. It is neither. Analyses of temporal changes in the structure of stratification usually occur over many years. So, except when they are abrupt, observing them and their consequences may take a long time, and the costs of appropriate data can be quite high. Comparing the stratification structures of different societies may not require as much time. But the costs of data for this are also likely to be high, particularly when problems of data comparability are encountered. (Experience in using developmentally diverse macroregions of Brazil as proxies, thus overcoming the data comparability problem, illustrate this: collecting and preparing the government’s excellent data must have costs many millions of dollars. To this were added the stratification research groups costs.)
Still, these realities should not block research on their use in understanding societal stratification. These phenomena and their variations are too important to the quality of the lives of everyone to allow the analysis of them to languish.
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