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Wage Differentials: Theory and Measurement

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THIS paper reports an investigation of certain aspects of wage differentials. The array of earnings of individuals from work may be arranged in many ways: in this paper we consider only two arrangements, by skill (or occupation) and by industry. In analyzing the behavior of wage differentials, it is difficult to avoid drastic simplification in their measurement; we frequently treat the number of workers at each level of skill and in each industry as though it were a quantity, and either their mean or their median wage as though it were a price.

The violence this does to a very complicated set of facts is obvious. Though some of the appropriate warnings are given where needed, others are not; therefore it may be helpful if, at the outset, a few precautionary remarks are made.

1. As usually measured, wages exclude the pecuniary value of fringe benefits. This may well distort interindustry or interoccupational wage comparisons for the postwar period. However, an appropriate set of corrections is not at hand, and, willy-nilly, we have written as though we believed fringe benefits were distributed more or less proportionately to wages. This could affect some of our results; all we can do is hope that the necessary amendments would not prove catastrophic.

2. When we speak of the wage level of an occupation or an industry, we refer indifferently (unless otherwise specified) to straight-time average hourly earnings; average hourly earnings, including overtime premiums; mean or median weekly or annual earnings. These various concepts of earnings do not vary proportionally from one industry or occupation to another, and we must take care not to make statements, based on data referring to one concept of earnings, as though they were necessarily applicable to data pertaining to other concepts.

3. We indicate whole frequency distributions of industrial or occupational wage payments by a single measure of central tendency, a mean or a median. This leaves completely unexplored the effect of industry or occupation on other characteristics of earnings distributions.

NOTE: This paper is part of a longer study on wage differentials, which has been financed by a grant from the Ford Foundation to the Economics Department of Stanford University.

4. Finally, in treating the mean or median of an industry's or occupation's earnings distribution as an indicator of the price of a particular kind of labor, we abstract from all variations in hiring requirements—labor "quality" as judged by the employer, and all variations in job attractiveness as judged by the worker. However, these variations reflect an important aspect of labor-market behavior.

Certainly we attempt to take these various factors into account at places where they are especially pertinent. But there are places, particularly in the section on industrial differentials, where paucity of data and the usages of an existing literature lead us to speak of "average wages" in an industry as though it were much more closely related to the "price of a factor of production" than it actually is. Obviously, in individual cases where the consequences of abstraction are known, they are taken into account. However, there are many cases where these consequences are not known and we proceed to apply economic theory without, each time, mentioning all the necessary reservations. For this one can only point to the need for reasonable brevity, and explain in advance.

Occupational Wage Structure

Since J. S. Mill invented the concept of noncompeting groups, if not before, economic theorists have tended to explain occupational differences in wages by differences in costs of training or other obstacles to supply. But this explanation is, at most, supposed to account only for long-run differences. In the short run, as usually defined, the number of persons in one occupation is assumed to be virtually fixed, and earnings are therefore presumed to be affected by recent changes in labor demand and wage rigidities, as well as by supply influences. It will therefore be convenient to separate the long- and short-run aspects of the matter and discuss them separately, though giving their interrelation due attention.

In discussing occupational or skill differentials, we adopt a particular, though customary, definition of the wage recipients whom we designate as skilled and unskilled. Here, the skilled workers are usually some sort of manual craftsmen, e.g., carpenters, millwrights, electricians, etc., while the unskilled are laborers, sweepers, watchmen, and the like. The records of the wages paid in the specific occupations chosen suffer from the usual limitations of samples and, in addition, the occupations used to represent skilled and unskilled workers are

themselves samples of skilled and unskilled occupations generally. The existence and relative increase of intermediate grades of skill create problems of demarcating skilled from unskilled occupations. One study, Keat's, measures skill differentials by the coefficient of variation of the ratio of the wage rates in 141 specific occupations to the average hourly earnings in all manufacturing; this largely overcomes the aforementioned difficulties except for those resulting from the limitations of manufacturing as a sample of the whole economy.

We measure the skill differential as the *percentage* difference between the hourly earnings of workers designated as skilled and those designated as unskilled. This is not the only possible measure that could be used; an alternative would be to measure the *absolute* differential—the dollar amount of the differential. We have chosen to measure the differential in percentage terms because we are primarily interested in the skill differential as one indicator of the relative economic well-being of two groups of earners. Though neither the absolute, the percentage, nor any one-dimensional measure of the earnings differential will be completely satisfactory for the purpose, we believe that changes in the percentage differential far more closely approximate that by which economists and others judge relative economic well-being than changes in the absolute differential do.

In an unpublished study, Gary S. Becker (properly) argues that the percentage differential is not always relevant in analyzing resource allocation. He contends that it is appropriate when we are attempting to explain variations in relative demand for skilled and unskilled workers. But if we should be interested in explaining variations in the resources devoted to investment in "human capital"—to education and training—as compared with those devoted to other uses, then it is the absolute difference that is relevant. This is because the percentage return on a given dollar investment in human capital is identified with the skill differential.

This view implies that if the dollar (i.e., absolute) skill differential between occupations A and B declined from t_0 to t_1, but in proportion with a decline in the difference in training costs, the skill differential (as measured) would be unchanged. However, we would still be interested, from a "welfare-distribution" point of view, in the differential earnings of the *persons* engaged in these occupations as well as in the return on the differential (dollar) investments they embody. Since we

have, *inter alia*, this welfare-distributive interest, we concentrate upon the behavior of percentage differentials.

Now let us turn to a consideration of the long-run characteristics of the wage structure.

SECULAR BEHAVIOR OF THE OCCUPATIONAL WAGE STRUCTURE

Most writers, including the present one, have contended that occupational relative (i.e., percentage) wage differentials, on an hourly basis (hereafter called the "skill margin"), have shown a secular tendency to diminish.[1] One important reason for this belief is empirical; the available evidence has seemed to support this view.[2] But, as we shall see, the facts do not tell a completely unambiguous story. If they did, we should expect to find that close substitutes of unskilled *urban*[3] workers would also have experienced a relative increase in hourly earnings—and some have not done so. For example, we should expect that the hourly wages of farm workers, who are closer substitutes for urban unskilled and semiskilled than for urban skilled workers,[4] would

[1] M. W. Reder, "The Theory of Occupational Wage Differentials," *American Economic Review*, Dec. 1955, pp. 833-852, especially n. 1, contains a fairly extensive bibliography of the literature before that date. The more important contributions since then are L. G. Reynolds and C. H. Taft, *The Evolution of Wage Structure*, New Haven, 1956; *Economic Survey of Europe in 1955*, United Nations, 1956, pp. 153-157; M. Rothbaum, "National Wage-structure Comparisons," *New Concepts in Wage Determination*, G. W. Taylor and F. C. Pierson, eds., New York, 1957, pp. 299-327; *The Theory of Wage Determination*, J. T. Dunlop, ed., London, 1957, especially the papers of Clark Kerr and Lloyd Reynolds; R. Perlman, "Forces Widening Occupational Wage Differentials," *Review of Economics and Statistics*, May 1958, pp. 107-115; M. Yanowitch, "Trends in Soviet Occupational Wage Differentials," *Industrial and Labor Relations Review*, Jan. 1960, pp. 166-191; William Goldner, "Labor Market Factors and Skill Differentials in Wage Rates," *Proceedings of the Industrial Relations Research Association*, 1957, pp. 207-216; and an unpublished doctoral dissertation by P. G. Keat, "Changes in Occupational Wage Structure, 1900-1956," at the University of Chicago, 1959. An article by Keat, "Long-run Changes in Occupational Wage Structure, 1900-1956," *Journal of Political Economy*, Dec. 1960, pp. 584-600, is based on his thesis.

[2] The best data summaries are in Reynolds and Taft, *The Evolution*, pp. 32-38; 59-63; 92-96; 108-128; 213-218; 240-242; 269-275; 293-298; and 319-327; and in Keat, "Changes in Occupational." Perlman, "Forces Widening," also contains some useful discussion of the data.

[3] The data on skill differentials refer almost exclusively to urban workers.

[4] Since one critic has challenged this assertion, we submit the following data from the 1950 Census: Among employed persons (both sexes) who resided in urban or rural nonfarm locations in March 1950 and were employed in nonfarm occupations, but had resided on farms in 1949, 55.8 per cent were occupationally classified as operatives and kindred workers; laborers except farm and mine or service workers—these are what are usually considered unskilled or semiskilled jobs. Classified as craftsmen, foremen, etc.—i.e., as skilled manual workers—were 13.8 per cent. For all other employed persons, the corresponding percentages were 40.3

rise relative to skilled earnings, but it is not clear that they have done so. The data in Table 1 clearly imply that in 1956 the ratio of their earnings to average hourly earnings in manufacturing was well below what it was in 1929, which was less than it had been in 1914. How-

TABLE 1

AVERAGE GROSS HOURLY EARNINGS IN MANUFACTURING, LAUNDRIES, AND AGRICULTURE, SELECTED DATES, 1929-58

	All Manufacturing	Index	Laundries	Index	Agriculture	Index
1929	$0.566	100	n.a.		$0.241	100
1934	0.532	94	$0.378	100	0.152	63
1939	0.633	112	0.422	112	0.166	69
1940	0.661	117	0.429	113	0.169	70
1941	0.729	129	0.444	117	0.206	86
1945	1.023	181	0.648	172	0.472	196
1948	1.350	239	0.817	216	0.588	241
1955	1.88	332	1.01	267	0.675	280
1956	1.98	350	1.05	268	0.705	292
1957	2.07	366	1.09	277	0.728	302
1958	2.13	376	1.13	299	0.757	314
1959	2.22	392	1.17	310	0.798	331
1960	2.29	405	1.22	323	0.818	339

SOURCE: Department of Labor and Department of Agriculture.

ever, these data are not beyond challenge: H. G. Lewis has shown me some unpublished computations of average hourly compensation in agriculture and manufacturing which show that with the ratio, $\frac{\text{average hourly earnings in agriculture}}{\text{average hourly earnings in manufacturing}}$, put equal to 100 in 1929, its value stood at 118 in 1956; i.e., contrary to the implication of Table 1, agricultural hourly compensation rose relatively to manufacturing. Fortunately, for the purposes of this paper, it is not necessary to choose between the two sets of estimates or even attempt to reconcile them.[5]

per cent in the various unskilled and semiskilled categories, and 15.7 per cent in the craftsmen, etc., group (data from *1950 Population Census*, Special Report P-E, No. 4C, *Population Mobility—Farm-Nonfarm Movers*, Table 3, p. 4C-14). In other words, recent rural-urban migrants were more prone to be employed as unskilled or semiskilled manual workers, and less likely to be employed as skilled—at least in 1950—than the remainder of the labor force.

[5] The main source of difference between Lewis's data and those cited in Table 1 is in the figures on agricultural earnings. The series in Table 1 is derived directly from the Department of Agriculture surveys, while Lewis's compensation data come from the Department of Commerce and from some unpublished hours-per-

But despite their differences, both series reveal a marked decline (about 20 per cent) in the above ratio during the 1950's, when the skill margin was about constant. This implies either that rural and urban labor markets are somewhat insulated from one another or that their interrelation is subject to a prolonged lag. In either event, there arises the possibility that the observed behavior of skill differentials in part of the economy may be a misleading guide to what has happened in other parts.

Laundry workers, like farm workers, are far better substitutes for unskilled than for skilled workers. And, in the absence of a competing series, we shall assert that their hourly earnings have declined relative to those in manufacturing from 1929 to 1957. Hence, if the trend toward reduced skill differentials is genuine, it is necessary to explain the growing gap between hourly earnings in manufacturing and in laundries, as well as the questionable relation between agricultural and manufacturing earnings.

One explanation of the growing wage disadvantage of the laundry workers is that, in terms of employment, laundering is a declining industry. From 1947 through 1958, employment in this industry fell 14 per cent (from 365,000 to 313,000). Thus it might be possible to rationalize the behavior of laundry wages as that of a declining industry in which some older workers are trapped but from which the more mobile are escaping; such an explanation is an entirely conventional application of short-run equilibrium analysis. No doubt there is validity in this explanation, but it is hard to believe that it is the whole story. For, in the postwar period, employment opportunities have usually been adequate to permit the escape of most workers into low-skilled jobs in manufacturing. A further explanatory factor that we believe should be taken into account is the probable relaxation of

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year data supplied by John Kendrick. The disagreement between the two sets of data lies mainly in their trends in hourly agricultural compensation: in Lewis's series the average rate of increase between 1929 and 1956 is about 1½ times as great as in that of Table 1. However, both series show (1) an appreciable (over 30 per cent) decline in the ratio of average hourly compensation in agriculture to that in industry during the early 1930's; (2) a very sharp rise in this same ratio during World War II; and (3) a sharp fall in this ratio from 1950-51 through (at least) 1957.

In fairness to Lewis, it should be noted that he does *not* claim his series to be superior to that in Table 1; he merely believes that, on available information, there is not an adequate base for choosing.

hiring standards to accept increasingly less eligible workers,[6] and some related phenomena to be discussed.

The declining industry hypothesis—i.e., relative declines in hourly earnings in an industry result from an uncompleted adjustment to a reduction in employment—could also be applied to explain the behavior of agricultural earnings, if such earnings have shown a relative decline. But, once again, the opportunities for interindustry mobility have raised a question as to whether there has been a differential change in hiring standards as between agriculture and the rest of the economy.

It might seem that the declining industry hypothesis, with a slight modification, could account for the relative wage behavior in both agriculture and laundries. That is, the conventional short-run analysis of a declining industry's relative wages implies that they will fall because some workers cannot or will not shift industries, though when they retire they will not be replaced. As the immobile workers disproportionately represent the older segment of the industry's labor force, it is to be expected that in the short run there will be a decline in the quality of the labor force as well as in employment; this will appear as a change in hiring standards.

The main factors which this analysis leaves out of account are such "institutional" ones as the differential effect of minimum wage laws, trade union pressure, curbs on child labor, etc. Clearly, minimum wage laws affect far larger segments of manufacturing than of either laundering or agriculture. Consequently, the relative decline of hourly earnings in the latter two industries may simply indicate the fact that market forces were free to set low wages in many of these industries' local markets but were prevented from doing so in manufacturing.

In other words, the usual measures of skill margins refer to differences in hourly earnings of different occupational groups in a large section of the economy, but do not reflect events in another part which is of some importance. The available statistics refer to urban areas and are collected disproportionately from large firms which are fairly long-lived; conversely, they underrepresent firms with the opposite characteristics. This creates the possibility that divergent movements

[6] It is difficult to test this surmise on census data since the Census Bureau does not publish detailed characteristics of laundry workers separately, but combines them with those of persons employed in cleaning and dyeing establishments. However, the matter can surely be investigated further.

in the "premium for skill" in these two parts of the economy may render data from either sector a misleading guide to skill margins in the economy as a whole.

Did this possibility materialize in the United States and elsewhere during the past twenty-five to fifty years? In our judgment, the answer is probably not. The compression of differentials in annual earnings since 1929, noted by Kuznets, Goldsmith, and others, would imply that this distribution behaved as though there had been a contraction in the margin for skill. Moreover, from 1939 to 1949 (at least), the dispersion of the medians of annual earnings of (full-year) wage and salary workers in different occupations declined during that period; this would suggest the same conclusion, though for a shorter period.[7]

There is further evidence, though of a different kind, which also suggests that there was secular shrinkage in the skill margin. It will not be seriously disputed that persons engaged in what are normally called skilled occupations generally have a higher level of education (more years of schooling) than those in unskilled trades. It will also be agreed that, whatever the causal pattern, variations in years of school attendance are associated with variations in occupational levels. Clearly, in the past fifty years, if not longer, there has been a marked cumulative increase in the median number of years of school completed by members of the American labor force. This would, *ceteris paribus*, have the effect of increasing the fraction of the labor force able to hold skilled jobs. Since the fraction of the labor force in skilled employment has increased secularly,[8] it is tempting to relate this fact and the decline in the skill margin by a conventional application of price theory.

This argument is as follows: there has been a secular increase in the relative supply of skilled as compared with unskilled workers which has led—because of a failure of relative demand for skilled workers to increase as much (at relevant ratios of skilled to unskilled wage rates)—to a decline in the skill differentials. The relative increase in the number of skilled workers is thus consistent with the above theory and with a decline in the skill margin; therefore it would seem to pro-

[7] H. P. Miller, *Income of the American People*, New York, Wiley, 1955, Chap. 9, especially pp. 120-21 and Table 67.

[8] From 1900 to 1950, craftsmen, foremen and kindred workers (roughly, skilled manual workers) increased from 10.5 per cent to 14.2 per cent of the labor force. Operatives, etc. (the semiskilled) increased even more (in percentage terms) during that period, but laborers, both farm and nonfarm (the urban unskilled and their substitutes) decreased sharply (see D. J. Bogue, *The Population of the United States*, Glencoe, Illinois, 1959, Table 17-1, p. 475).

vide an additional reason for believing that the skill margin really has declined secularly.

But this argument is not free from difficulty: it is possible that the secular increase in the relative number of skilled workers is due mainly to changes in relative demand rather than supply. We do not have any independent measure of changes in relative demands for skilled and unskilled workers, and we cannot deny that the spreading of education may have been partly a response to relative changes in labor demand.[9] If this possibility is accepted as an "important" cause of the relative increase in the number of skilled workers, we cannot buttress the finding of a declining skill margin by the behavior of the relative numbers of skilled and unskilled workers.

However, we are inclined to deny that there have been important changes in relative demand for the following reasons: (1) below the college level—and it is with this range that we are concerned—increased schooling has been due in good part to laws compelling school attendance; (2) a substantial role in the relative decline of unskilled labor has been the sharp reduction in child labor (under legal pressure) and, in the United States, the reduction in immigration. Both of these developments tended to reduce the relative supply of the unskilled at the same time that they raised the median years of schooling of the labor force. (3) Of the increase in private expenditure for education that has occurred, the major explanatory factor has been the increased income (of parents); i.e., private educational expenditure *below the college level* has been viewed as a consumer luxury good rather than as a producer good. (4) Those ambitious persons who respond to the lure of higher incomes by increasing their training—at least in the United States—have not gone into skilled manual work but into business or the professions. Consequently, their effect on the supply of skilled labor would have been small.

While we believe that these assertions can be supported with empirical evidence (1 and 2 easily, 3 with a little effort, and 4 with considerable effort), for the present consider them merely as assertions. If all were true, they could not prove that changes in relative demand

[9] This possibility is indicated very clearly in G. S. Becker's unpublished manuscript, "Investment in Human Capital." For the purpose of our paper, delayed (supply) response to an initial skill differential in excess of the (long-run) equilibrium level would yield the same price-quantity behavior as a shift in the supply function with instantaneous adjustments; i.e., the relative quantity of the skilled will increase and their differential wage advantage will shrink. Hence, we shall not bother to distinguish this from a downward shift in the long-run supply function.

had no part, or even a smaller part than changes in relative supply, in narrowing the margin for skill. All they are intended to do is indicate the evidence that could be marshalled in support of the claim that supply factors are adequate to explain the secular decline in the skill differential. But it cannot be denied that an adequate alternative explanation stressing demand factors might yet be offered.

The explanation of a secular decline in the skill margin offered here would strongly suggest that the decline will continue. However, not everyone agrees: one recent writer[10] has argued that because of the possibility of relaxing hiring standards, it is likely that in the future skill differentials will be widening rather than narrowing. That is, employers will relax hiring standards for the unskilled more than for the skilled, thereby causing a widening of the skill margin. Moreover, as he contends, it is possible that technical progress will have the effect of increasing the ratio, $\frac{\text{DEMAND FOR SKILLED WORKERS}}{\text{DEMAND FOR UNSKILLED WORKERS}}$, more than broadened educational opportunities will increase the ratio, $\frac{\text{SUPPLY OF SKILLED WORKERS}}{\text{SUPPLY OF UNSKILLED WORKERS}}$, at a given set of relative wages.[11] This possibility could manifest itself by drawing an increasing fraction of full-time adult male earners toward skilled trades, with their places being taken (in part) by "the transient, the very young, the very old, and the physically, mentally and socially handicapped."

However, this "labor reserve" has always been available and, moreover, was in the past bolstered by elements no longer available, e.g., children and immigrants. Perlman offers no reason to suppose that the net result of these various factors should be a different secular trend in the skill margin than has existed hitherto. Nevertheless, it is possible that technical progress in transportation (e.g., the automobile) and in household appliances has shifted downward the supply schedule of the typical housewife's labor services.[12] In considering this possibility, it is necessary to recognize that many housewives, wishing employ-

[10] R. Perlman, "Forces Widening Occupational Wage Differentials," *Review of Economics and Statistics*, May 1958, pp. 107-115.

[11] This statement, and the one following, is my interpretation of Perlman's argument on p. 113.

[12] The secular rise in the fraction of the female population in the labor force is, of course, no evidence for or against this possibility; at least part of this rise has been simply a movement along a given supply function in response to increased demand; on this point the reader should see the paper of Jacob Mincer in this volume.

ment at going wage rates, have often been frustrated by employer hiring requirements or legal restraints on hiring, or both.[13] Relaxation of either of these restraints clearly may lower unskilled wage rates relative to others.[14]

Another possibility is that with rising family incomes it is possible that secondary earners will tend to substitute lower-paying, but pleasanter, jobs for jobs with the reverse characteristics. This would, *ceteris paribus*, reduce the wage rates on what have traditionally been considered pleasant jobs, and raise them on jobs traditionally considered unpleasant. If we assume unskilled manual labor to be "unpleasant," and white-collar jobs to be "pleasant," this would imply a decline in the relative earnings of white-collar to manual jobs. It seems likely that something of this sort has happened over the last half-century.[15]

In short, it is possible that the concomitants of economic progress will, as Douglas once surmised, transform the occupational wage structure so that the jobs at the bottom will be comparatively pleasant and their low remuneration a "compensating" differential. Whether this comes to pass will depend more upon employer hiring standards concerning part-time workers, especially married women, etc., and upon legal and customary restraints upon hourly wages, than upon the relative costs of educating such workers.

In other words, the secular behavior of skill differentials reflects not only variations in relative costs of "producing" skilled and unskilled workers, but also the restraints upon the labor market imposed by legal action (e.g., minimum wage laws, restrictions on child labor, etc.) and the variations in relative labor supply of skilled and unskilled labor services consequent upon rising real incomes. The operation of these latter factors *could* alter the trend in skill differentials and make it widen in the future; however, in our judgment there is no reason for thinking that these forces will be more powerful in the future than they have been heretofore.

So far in our discussion we have deliberately ignored the alleged influence of inflation on the skill differential. One writer (Perlman)

[13] For example, minimum wage laws, laws against industrial homework, etc.

[14] I have discussed this point elsewhere: "Theory of Occupational Wage Differentials," pp. 838-840.

[15] Cf. K. M. McCaffree, "The Earnings Differential Between White Collar and Manual Occupations," *Review of Economics and Statistics*, Feb. 1953, pp. 20-30, especially pp. 20-21; also P. H. Douglas, *Real Wages in the United States, 1890-1926*, Boston, Houghton Mifflin, 1930, pp. 367-368.

contends that "our inflation-conscious public, government, and monetary authorities will exert every effort to stabilize prices, thus removing the strongest force in narrowing the differential—inflation."[16] Let us put aside the prognostication about the future of the price level and concentrate on the analytical issue—the relation of wage structure and inflation. Perlman's contention is that unskilled workers "need" larger percentage increases in hourly wages than skilled and, therefore, exert "more urgent upward pressure on wages."[17]

This argument is not peculiar to any one author,[18] but despite its currency, we find it simply implausible. Our objection to the argument does not refer to its use as an ad hoc explanation of events in a specific (short-run) situation; in such applications, one must judge it as best one can, case by case. The following remarks are directed solely to its use as an explanation of a secular trend in wage differentials.

As we have argued elsewhere, there is, in nearly every community, a minimum real hourly wage below which the hiring of labor services is not permitted.[19] The minimum is closely related to unskilled wage rates (but not to skilled rates) and variations in it may therefore have a marked—and possibly permanent—effect on skill differentials. The argument we are considering goes further: it alleges (in effect) that this minimum is subject to a money illusion; i.e., the relative value of the minimum rises with the price level in such a way that the greater the secular rise of the latter, the greater the secular decline in the skill margin. Testing this contention would not be difficult if we were free to assume that both the skilled and the unskilled labor markets were in equilibrium. However, the existence of a minimum real wage has frequently caused unemployment of the unskilled; this is one of the strongest arguments to support the hypothesis of a minimum real wage. This makes the hypothesis of labor market equilibrium doubtful, which, in turn, puts the above-mentioned money-illusion thesis beyond reach of simple tests.[20] For the present, one is free to accept it if he chooses; but there is no reason for doing so.

[16] Perlman, "Forces Widening," p. 115.

[17] This is a dubious contention for periods of full employment which frequently coexist with inflation. For with full employment, families whose principal earner has deficient wages can remedy the deficit by supplying secondary earners. That is, since unemployment hits the unskilled more than the skilled, full employment benefits them proportionately more.

[18] For example, it is also expressed by Knowles and Robertson, "Differences between the Wages of Skilled and Unskilled Workers, 1880-1950," *Bulletin of the Oxford Institute of Statistics*, Apr. 1951, pp. 109-127.

[19] Reder, "Theory of Occupational," pp. 839-840.

[20] What is needed, as a minimum, are data measuring excess demand or unem-

THE PATTERN OF SHORT-RUN FLUCTUATIONS IN SKILL MARGINS

It is generally agreed that in the short run skill margins change relatively little during "normal" periods, but contract sharply during periods of over-full employment. It is also possible that they widen during major depressions, though this is not clear.[21] We have attempted elsewhere to account for the sharp reduction in skill margins during the two World Wars when there was excess demand for all types of labor.[22] What is not accounted for is why the sharp narrowing of occupational wage rates that has occurred during war periods has been only partially reversed subsequently.

If we grant that there is a secular decline in the skill margin, and that little or no decline occurs in "normal" periods, it is a matter of arithmetic that wartime declines must exceed postwar increases. The analytical question is why a trend that presumably reflects more or less steady changes in supply should manifest itself in short violent movements followed by long periods of comparative quiescence. There may be many possible explanations of this phenomenon; let us consider two.

The first of these is as follows: the relative supply of skilled and unskilled workers is only indirectly affected by the educational attainment of the labor force. Having more schooling does not automatically fit a man for a more skilled job; it merely increases his ability to absorb the specific training that qualifies him for such a job. To obtain this training it is necessary to serve an apprenticeship, attend a trade school, or get it on the job. In some cases (e.g., where training must come through a union-influenced apprentice program), availability of training may be limited by the current employment situation. In others, workers seem uninterested in taking advantage of training programs unless they are connected with the imminent prospect of promotion or unless they involve on-the-job training. This lack of interest largely reflects the apparent reluctance (or inability) of employers

ployment for skilled and unskilled workers, separately, under conditions of secular inflation; i.e., we need long-time series or at least a few observations well separated in time.

[21] Keat ("Changes in Occupational," Chapter II) finds evidence of this in both the 1920-21 and 1929-32 depressions. P. W. Bell ("Cyclical Variations and Trends in Occupational Wage Differentials in American Industry since 1914," *Review of Economics and Statistics*, Nov. 1951, pp. 329-337), however, found a widening of the differential only in the 1920-21 contraction. Knowles and Robertson ("Differences Between the Wages," especially p. 111) found that skill differentials did not widen in Great Britain during 1929-32, though they did in the early 1920's.

[22] Reder, "Theory of Occupational," pp. 840-845.

to accept inexperienced or partially skilled workers merely in order to reduce the wage premium on skilled jobs. According to this piece of speculation, in "normal" times, supply and demand for skilled workers is kept more or less in balance at current skill margins by the various obstacles (and discouragements) to acquiring requisite skill.

To elaborate a bit: Much of the difference between a highly competent skilled worker and one less competent lies in the greater range of tasks that the more competent worker can perform. Assuming, realistically, that labor turnover involves expense,[23] the differential advantage of the more competent skilled worker to his employer will become greater the more varied are the tasks that the firm's output pattern imposes on him. Conversely, long runs of one particular product minimize variation in a skilled worker's productive role, and hence in his differential advantage.

During a war period, huge orders create the possibility of producing long runs of one particular product. This makes it possible to keep partially trained workers continuously occupied at a narrow range of tasks; consequently, it may—though it need not—absolutely reduce the demand for the broadly trained. Put differently, the elasticity of substitution between partially (narrowly) and fully (broadly) skilled workers is increased. Furthermore, in wartime the premium on speed of delivery makes it more profitable than normally to hire workers whose limited skills compel them to be idle—"unproductive"—for part of the time spent on the job. The relatively short and simple training needed to learn one or a few specific skills further encourages training and use of partially skilled workers.[24]

In short, in a war period, there is a relative shift in labor demand from more to less broadly skilled workers, with the less broadly skilled tending to acquire the titles but only part of the functions of those more fully trained. The effect is (1) to increase the supply of workers able to fill jobs with skilled titles and (2) to curb the increase in demand for workers with specific high-level skills. While (1) acts as a brake upon wage increases to those holding jobs with skilled titles,[25] (2) curbs wage increases to those who have broad skills.

[23] The importance of this point is brought home by an unpublished doctoral dissertation of W. Y. Oi of the University of Chicago.

[24] This statement implies that firms become short-run monopsonists with regard to fully trained workers, but not with regard to the partially trained (see fuller discussion below).

[25] Which is what the data reflect.

After the war, labor demand reverts, more or less, to its prewar composition in response to the relative decline in large orders which require long runs and permit the use of limited skill workers. However, the partial training acquired during the war reduces the prime cost of becoming fully trained, as compared with starting from scratch. Furthermore, many of the workers promoted during the war display such industry and aptitude that they are offered continued employment on skilled jobs if only they acquire the full range of skills needed for peacetime employment; sometimes employers will even defray the training costs involved. The effect of a job promise, combined with successful experience, is greatly to reduce the uncertainty of getting employment which is a real, though nonpecuniary, cost of training. Thus, the relative supply of skilled workers is increased, which tends to prevent a return to the prewar skill margin.

A second explanation is suggested by the behavior of "experience differentials." By experience differentials I refer to the wage differentials that are associated with length of service in a given firm or organization. There is a very extensive overlap of skill and experience; in many cases skill, and the reward thereof, is simply a by-product of long experience in a particular line of work, and is acquired more or less by osmosis. Often, when an employer specifies that he wants an "experienced" worker, what he means is that he wants a relatively skilled one, and considers successful experience as an indication of skill. Consequently, it is not surprising that skill and experience should be confused, both in practical and theoretical discourse.

For reasons that will become apparent, we believe that the wages of unskilled or inexperienced workers or both are apt to be more sensitive to the state of the labor market than those of workers with the reverse characteristics. In support of this proposition, let us consider a rather rare type of data: the behavior of earnings data classified by years of experience of the earner. Such data are quite rare: one of the few good sources is the data for engineers presented by Blank and Stigler.[26] These data are the monthly salaries of engineers, classified by years of

[26] D. M. Blank and G. J. Stigler, *The Demand and Supply of Scientific Personnel*, New York, National Bureau of Economic Research, 1957; see Appendix Tables A-3 (p. 117), A-9 (pp. 133-134), A-14 and A-15 (pp. 140-141).

Since this section was written, an excellent study by Robert Evans, Jr. ("Worker Quality and Wage Dispersion: An Analysis of a Clerical Labor Market in Boston," *Proc. Industrial Relations Research Association*, Dec. 1961, pp. 246-259) has appeared. In his paper, Evans presents strong evidence of experience differentials among Boston stenographers (see pp. 250-251).

experience, for selected years in the period from 1894 to 1953; they reveal a pronounced upward trend in the ratio (1) of the salaries of starting engineers[27] to those of experienced engineers and (2) of the salaries of less experienced engineers to those of more experienced. What is more to our present purpose, these ratios rose especially rapidly in periods when the over-all demand for engineers rose with unusual speed but then subsequently declined, contrary to trend, when the demand for engineers declined. For example: during World Wars I and II and during the Korean episode (periods of sharp increase in demand) the salaries of inexperienced engineers rose markedly relative to those of engineers with more experience.[28] And, in each case,

[27] That is, those with less than one year of experience. Years of experience refers to years elapsed since start of first professional job, though in some cases this is estimated (imperfectly) by the difference between an engineer's age and the median age at which graduate engineers receive their first degree. Thus defined, experience is not length of experience with one firm as the argument requires; however, it is likely to be strongly correlated with it.

[28] As Blank and Stigler summarize the data for the World War I period, "the increase in earnings at the starting level and at one and two years experience ranged between 61 and 80 per cent between 1914-1916 and 1919-1921, while the increase for engineering graduates with 10, 15 and 20 years experience ranged between zero and 20 per cent" (*The Demand and Supply*, pp. 124-125, and Table A-5).

For the World War II period, the data in Table A-8 (p. 130 of Blank and Stigler) show that the median salary for engineers with less than 1 year of experience rose by 80 per cent between 1939 and 1946, but rose by only 58 per cent (during the same period) for those with 9-11 years of experience and by only 27 per cent for those with 30-34 years experience. (Tables A-10 and A-11, pp. 135-136, show that a similar relation existed within each field of engineering.)

During the Korean War, average monthly salaries for engineers of varying levels of experience behaved as indicated in the following table, which shows the annual percentage changes in average monthly salaries of research engineers and scientists with B.S. degrees, by years of experience, 1948-55. The relevant pairs of years are 1951-52 and 1952-53.

	Percentage Changes, by Years of Experience				
	0	1	9	10	11
1948-49	−3.2	−0.3	2.8	1.1	0.9
1949-50	1.8	1.7	2.0	3.5	3.2
1950-51	6.9	5.0	7.6	6.7	6.9
1951-52	11.3	12.7	8.2	9.4	8.1
1952-53	6.8	6.7	3.8	4.0	4.4
1953-54	3.7	5.5	1.6	2.1	3.4
1954-55	7.2	5.0	16.6	8.9	8.0

SOURCE: National Survey of Professional Scientific Salaries, Los Alamos Scientific Laboratory of the University of California, 1949 through 1955. Each set of percentages derived from data collected in a single survey, to avoid the effects of changes in coverage. Taken from Blank and Stigler, Table A-15, p. 141.

soon after the demand for engineering service slackened, the salaries of inexperienced engineers underwent a relative decline.[29] Furthermore, during the depressed 1930's (1929-39), the salaries of inexperienced engineers declined relative to those with more experience.[30]

It is possible that this argument rests upon a statistical mirage. That is, the tendency for the experience differential among engineers to diminish in periods of sharp increase in demand might be due mainly to the fact that in such periods demand is concentrated on certain newly developed specialties (e.g., in the early 1950's, the strong demand was for electronic engineers, aeronautical engineers, and so forth) in which there is literally no stock of experienced practitioners. If so, the relative increase in wages of the inexperienced has occurred only because the pressure of increased demand has been greatest in those specialties where the number of experienced engineers was extremely small relative to demand. If this were the whole story, we would expect that there would be no tendency for the experience differential, within given categories of engineers (especially those little affected by tem-

[29] In the post-World War I period, from 1919 to 1924, the median monthly starting salary for engineers rose by 14 per cent, while that of engineers with 9-11 years experience rose by one-third (Table A-6, Blank and Stigler, *The Demand and Supply*, p. 126).

Data for the post-World War II period are given in the table in footnote 28. These data show that in each of the pairs of years between 1948 and 1951 (with the exception of new graduates as compared with those of 10 and 11 years experience in 1950-51) the salaries of engineers with zero or one year of experience declined relative to those with 9, 10, or 11 years of experience. In the post-Korean period, 1953-54 and 1954-55, the wages of the engineers with zero and one year of experience declined relative to those with 9 or more years. This decline does not appear in 1953-54, but it is marked in 1954-55 and also in the two-year period 1953-55.

[30] Between 1929 and 1932, the median salary of engineers with less than one year's experience declined relatively more than that of all engineers; salaries for those with one year's experience fell 26.5 per cent as compared to 18.7 per cent for all engineers. For the decade 1929-39, the median for beginning engineers declined 14.1 per cent; for all engineers the decline was 4.2 per cent.

From 1932 to 1934 the median salary of new engineers declined less than for all engineers (Blank and Stigler, *The Demand and Supply*, Table A-8, p. 130). This accompanied a marked decline in the ratio of the interquartile difference to the median from .43 to .35 for engineers with less than 1 year of experience, while the corresponding ratio for those with 9-11 years of experience was virtually unchanged (.45 in 1932 and .44 in 1934) and increased from .72 to .75 for those with 30-34 years of experience (Blank and Stigler, Table A-3, p. 117). I would suggest that this reflects the upward pressure upon very low wages that stemmed from the National Industrial Recovery Act and related phenomena; this interpretation is supported by the fact that, between 1932 and 1934, the lowest quartile of salaries for engineers with less than 1 year of experience rose from $89 to $91 per month, while the median fell from $111 to $110 and the highest quartile fell from $137 to $129. For experienced engineers, all quartiles fell from 1932 to 1934.

porary "shortages"), to contract. However, the available data are inconsistent with this surmise; during World War II, at least, the "margin for experience" contracted in each of the five specialties for which data are available.[31]

Therefore, for the present we shall assume that the behavior of the experience differential is not entirely due to a statistical mirage and, in part, reflects other factors. One of these may be that the relative demand for inexperienced (as against experienced) engineers increases when total demand for engineers increases, and vice versa.

One possible explanation of why it is more profitable, given the initial difference in salaries, to train and promote inexperienced engineers to senior positions than to recruit experienced engineers *in a tight labor market*, but that it is more profitable to do the reverse in other kinds of markets, is as follows.

In a normal labor market, the individual firm can hire both experienced and inexperienced engineers in the quantities it desires from the unemployed resulting from normal labor turnover. But in a tight labor market, experienced engineers cannot be hired from the ranks of the unemployed, but must be bid away from their current employers. (The reverse is true of newly graduated and, by definition, inexperienced engineers.) There are two reasons why this is more costly than hiring comparable engineers who are currently unemployed: (1) it is time-consuming to find an appropriately skilled and experienced engineer who is currently employed and "pirate" him and (2) it is costly to induce him to leave a situation where he has known and favorable prospects for advancement, good personal relations, etc. and, where done, it is usually at a substantial increase over his current salary. Both

[31] Blank and Stigler, *The Demand and Supply*, p. 136.

Despite these facts, there may be a connection between the sharp rise in demand for certain specialties, and the contraction in the experience differential. Consider: the sharp rise in the entrance rates for certain types of engineers may have led to a switch to those specialties by students—which could occur within two years—thereby decreasing the relative supply of graduates in other fields and (*ceteris paribus*) causing a rise in their entrance rate. Whether such a shift took place during (say) the Korean episode is not easy to determine: the data on percentage of engineering degrees awarded in various fields reveal a suspicious tendency for the percentage in "other" to rise from 1950 to 1953. (Cf. Blank and Stigler, Tables C-5, C-6, C-7, and C-8, pp. 160-165. "Other" is other than civil, mechanical, electrical, chemical and mining; specifically it includes aeronautical and electronic engineers.) However, it is obvious that a more detailed analysis is necessary to see if the data will bear this interpretation; but even if they do, it is far from clear that this "mirage argument" provides a complete explanation of the behavior of the experience differential.

of these factors make the *marginal cost* of hiring *additional* experienced engineers rise substantially when the labor market tightens, but do not affect the wage rate that must be paid to those experienced engineers already hired, provided no new ones are added. And they do not apply to new engineering graduates. Hence, the marginal cost of recruiting experienced engineers rises relative to that of hiring inexperienced ones, even though their relative wage rates remain unchanged.[32] This tends to reduce the demand for additional experienced engineers (relative to that for inexperienced) at given relative wage rates, thereby (given the supply functions) driving up the relative wages of the inexperienced.[33]

Conversely, when the market for engineers loosens, it becomes easier for an employer who needs an experienced engineer to find one currently looking for a job so that the marginal cost of hiring him is no greater than the wage paid those already employed. Thus, when the over-all demand for engineers falls, the marginal cost of hiring an experienced one falls relatively (to that of hiring an inexperienced one), if relative wage rates are unchanged; therefore there is a tendency for the relative demand for and wages of the inexperienced to decline.

This explanation applies directly to the case of engineers. Superficially, at least, it also seems consistent with the behavior of teachers' salaries. And we believe it is applicable to a wide variety of labor market situations. Indeed, whenever a firm has an incentive to prefer previously hired workers to new ones—either because of a desire to minimize the costs of labor turnover or because it has made an investment in training its workers—it will try to keep its present employees from desiring to leave. Since it does not have a parallel incentive for the newly hired, it may follow the market for that group, but maintain a given wage rate for each job among the more experienced.[34] This

[32] That is, in a tight labor market, the supply curve of experienced engineers to a firm develops a discontinuity at the quantity currently hired. However, this discontinuity disappears—or is greatly reduced—in a "normal" or "loose" market; this is a species of monopsonistic behavior.

[33] One would expect that the (few) experienced engineers who do get onto the labor market when it is tight would receive higher wages than those who do not. Perhaps they do, but two contrary possibilities must be considered: (1) that the experienced engineers appearing on the market may be an inferior sample of the relevant population and (2) that, because of the market situation, employers have made such *short-run* arrangements that they cannot effectively use additional experienced men. Furthermore, it is notoriously difficult to discriminate in favor of newcomers without creating grave discontent among old employees. The need to extend the "recruitment price" to those already hired may be a serious deterrent to recruiting experienced personnel; this is typical of monopsony.

[34] To do this a firm need not juggle its starting rate continuously. It will suffice

will generate a pattern of fluctuations in experience differentials in response to labor demand similar to that which students have observed. Given the association between skill and experience, which is implied by the practice of promotion from within, the skill differential is likely to move with the experience differential.

Interindustry Differentials

LONG RUN[35]

Most discussions of interindustry wage differentials proceed without much explicit consideration of economic theory. The literature abounds in ad hoc hypotheses, some of which are consistent with neoclassical price theory but many of which are not. However, these various hypotheses are usually treated as being equally plausible, a priori; consistency with the implications of price theory has counted for very little in appraising the merits of a theory. Our attitude is somewhat different; we believe that if a theory is inconsistent with the implications of price theory it is cause for concern, and that an explanation is in order. Consequently it will be helpful if we begin our discussion by spelling out what is implied by price theory for interindustrial differentials.

In the long run, under competitive conditions,[36] any industry will pay the same price for a given grade of labor as any other industry hiring in the same location. This remark must be qualified for differences in the nonpecuniary attractions of different industries and locations, but let us abstract from these at first. Therefore, in the long run, real wage differentials among industries will reflect differences in the skill mix. Money wage differences among locations, for given skill, should be no greater than can be rationalized by differences in living costs.

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simply to have two or more similar entering job titles with different rates, and hire for the job title consistent with the current market rate.

[35] In this section, the distinction between long and short run is drawn very sharply. The theoretical basis of the distinction is the usual Marshallian one. In practice, we interpret a period as long, for the purpose in hand, if shifting the initial or terminal date to any other year in the same reference cycle would not alter the argument. Specificially, we regard 1899-1953 or 1909-53 or even 1929-53 as "long periods," but not 1929-38.

In the Methodological Appendix, the argument of this section concerning the relation of long run and short run, the "competitive hypothesis," and related matters is stated in more detail.

[36] We shall assume, except when the contrary is specifically stated, that competitive conditions exist.

This means that there should be no association of industry wage levels either with the amount of labor employed or with the amount of capital employed (total or per worker) except insofar as either of these quantities is correlated with the skill mix. This absence of association between industry wage level and quantity of labor utilized is an important distinguishing characteristic between long- and short-run situations (see Appendix). In the short run, the greater the increase in employment over the recent past, the more likely is an industry to encounter rising wages because of short-run inelasticities of labor supply; hence the theory implies a positive association of increase in labor quantity used and wage increase in short periods, but not in long periods. Moreover, it seems reasonable to suppose that it will be more likely that skilled labor will become relatively scarce[37] to an expanding industry than nonskilled. Therefore, in the short run, skill differentials should be positively associated with changes in employment.[38]

That is, we interpret price theory as saying that in the long run each industry's wage level will, *ceteris paribus*, vary in the same direction as its skill and locational mix (see below) and, in particular, will not be related to changes in the quantities of labor or capital employed. Now if *ceteris* were exactly *paribus*, and our sample were large enough, the correlation coefficient (among industries) between long-run changes in wage levels and those in (any) factor quantity would be exactly zero. But our samples are limited and ceteris is never exactly paribus; hence the theory will be considered "not inconsistent with the evidence" if the above mentioned correlation coefficients are approximately zero. Inconsistency with the evidence will emerge if ceteris is insufficiently paribus in the sense that forces affecting long-run relative wage changes are significantly correlated with long-run relative changes in factor quantities.[39]

[37] That is, it will take longer to train workers with skills peculiar to the industry than unspecialized workers and hence, for a time, their elasticity of supply will be less.

[38] This is analogous to Clark Kerr's contention that, "The lesser the degree and the greater the rate of industrialization, the wider will be the occupational differentials and the greater the premiums for skill." (See "Wage Relationships—The Comparative Impact of Market and Power Forces," in *The Theory of Wage Determination* [cited in footnote 1], p. 187, especially no. 2.)

[39] Now a word about the nonpecuniary attraction of different industries. It is hard to believe—though imaginable—that industries *as such* have differing degrees of nonpecuniary attractiveness to labor force members. Most of the apparent nonpecuniary differences among industries would seem to boil down to differences in the relative attractiveness of different locations and of the specific jobs offered. For example, we submit that a job as bookkeeper in the New York office of a coal

Now, how do these inferences square with available evidence? One body of evidence is presented by Fabricant[40] in a study of average growth rates of real hourly wages, labor employed, and capital utilized in 33 industries from 1899 to 1953.[41] Let us suppose that in a period of 54 years the long-run forces that affect the relative levels of industries' wages make changes sufficiently large to permit us to treat differences between 1899 and 1953 as reflecting mainly these forces and, only to a minor degree, random and short-run forces.[42] That is, the differences between 1899 and 1953 are assumed to be explicable on the hypothesis that they are, save for random disturbances, positions of comparative statics. If so, there should be no association between either the relative growth in the quantity of labor utilized, or the relative growth in the stock of capital employed in a given industry, and the relative growth in wages (measured by average hourly earnings) in that industry. The rank correlation coefficients between (a) wages and labor employed and (b) wages and tangible capital owned,[43] with each industry taken as a single observation, are +.21 between wages and labor quantity and +.29 between wages and capital quantity. The standard error of the rank correlation coefficient with 33 observations is .17, and hence neither coefficient is statistically significant at the 5 per cent level.[44]

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mining firm is no less attractive than a similar job in the same location in an electronics firm. However, coal mining will offer proportionately more jobs in mining towns, and underground, than (say) electronics manufacturing and therefore might well face a higher *pecuniary* supply price for its labor.

It would not be correct, conceptually, to identify unskilled jobs with unattractive ones, but historically there has been a strong positive association. In general, as industries have shifted away from unskilled labor they have also improved working conditions and reduced nonpecuniary disutilities. And since it is obviously very difficult to measure or indicate the relative nonpecuniary attractiveness of different industries, we have assumed that the *rank* of the various industries with respect to nonpecuniary utilities varies with the percentage of its workers employed in unskilled jobs. Clearly, this is a rough approximation which must later be improved upon.

[40] S. Fabricant, *Basic Facts on Productivity Change*, New York, NBER, Occasional Paper 63, 1959, especially pp. 29-37.

[41] These data are presented in extended form in J. W. Kendrick, *Productivity Trends in the United States*, Princeton for NBER, 1961.

[42] See Methodological Appendix.

[43] Perforce, we use the Kendrick-Fabricant definitions of labor, capital, and output. The data used are contained in Table B, pp. 46-47, of Fabricant, *Basic Facts*. "Wages," "labor," and "capital" mean here percentage change in each of these variables between 1899 and 1953.

[44] It might be contended that, because we have two coefficients differing from zero, and with the same sign, the two coefficients together differ significantly from

These findings are compatible with the competitive hypothesis.[45] Indeed, the fact that both of the correlation coefficients are positive, as well as small, is what might be expected because of the tendency for rapidly growing industries to locate (as of 1953) in relatively high-wage urban centers. There is, moreover, further evidence that is also favorable to the competitive hypothesis.

(1) Contrary to much of the recent literature, there was only a slight correlation between productivity[46] and average hourly labor compensation among 33 industry groups during the period 1899-1953. The rank correlation during that period was +.24 (insignificant at the 5 per cent level); in various shorter periods the coefficient was appreciably higher.[47] Confirming this is the fact that during 1899-1947, among 80 manufacturing industries, the rank correlation coefficient between output per man-hour and average hourly labor compensation was 0.26—not quite significant at 5 per cent; during individual decades of that period, the coefficient was invariably higher than this.[48]

It would be possible to hide behind the insignificance of the above coefficients and say that the competitive hypothesis is not disconfirmed. However, it seems more plausible to suppose that the two coefficients (noted above) together indicate the operation of some rather weak force systematically correlating average hourly labor compensation and productivity. One explanation of this that would not be incom-

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zero (to which theory implies they are both equal). However, output, capital, and labor are all highly correlated so that we cannot suppose the two coefficients to be independent, and combining the tests is therefore extremely difficult.

[45] By "competitive hypothesis," I mean the hypothesis that prices and quantities behave as though they were in long-run equilibrium under conditions of pure competition. When we speak of the short-run competitive hypothesis we mean the same hypothesis except for the modifications introduced by the substitution of Marshallian short-run equilibrium for long run (see Appendix).

[46] "Productivity" is total productivity as defined by Kendrick; i.e., output per unit of input of both labor and capital. However, output per unit of labor input is highly correlated with total productivity (rank correlation coefficient among 33 industry groups is +0.94) and, as Kendrick says, "Thus analysis of productivity change based on output-per-manhour measures should give results comparable to analyses based on total factor productivity" (p. 155). Therefore, we shall consider Kendrick's results, where "total productivity" is interchangeable with man-hour productivity.

The competitive hypothesis implies that there will be no correlation *in the long run* between (average) productivity and wages. That is, industries in which average productivity grows relatively to others will show an increasing ratio of average to marginal (labor) productivity because all industries must pay the same for given grades of labor *in the long run*. This, of course, is not true in the short run.

[47] Kendrick, *Productivity Trends*, Table 55, p. 198.

[48] *Ibid.*

patible with the competitive hypothesis is that there is a tendency for industries with a greater than average increase in productivity to experience a greater than average "improvement" in skill mix[49] and, therefore, to have a greater than average increase in hourly labor compensation. Though not directly testable, this explanation seems to have considerable plausibility. Another possibility consistent with the competitive hypothesis is that increases in productivity are weakly associated with a tendency toward urbanization and higher wages. Last, but not least, in the short run a positive correlation between relative wages and employment is to be expected (see below). This coefficient may approach zero in the long run, but it may remain positive and finite for a very long time—long enough to generate (at least) some of the positive coefficients reported in this section.[50] Obviously, failing empirical tests of these and rival hypotheses, there is room for doubt and debate.

(2) Another finding consistent with the competitive hypothesis is that the ratio of capital compensation per unit of capital service is only slightly correlated with changes in average hourly labor compensation among 33 industries in the period 1929-53; the rank correlation coefficient was only +.12.[51] The competitive hypothesis implies that this coefficient be zero. Though the coefficient is insignificant at the 5 per cent level, we are inclined to take its positive sign seriously and rationalize it as follows: in industries with a higher than average rate of increase in productivity, there is a slight tendency for both labor and capital "quality" to increase more than the average.[52] A further finding that tends to support this conclusion is the very slight positive correlation (+.05) between (1) factor compensation (of both labor and capital) per unit of input, and (2) productivity among 33 industries in 1899-1953.

[49] That is, a greater than average increase in the percentage of high-earning and presumably skilled workers employed. "Skill mix" is defined more precisely below.

[50] This possibility is discussed in more detail in the appendix. The point was raised in discussion by both M. J. Bailey and H. G. Lewis.

[51] Kendrick, *Productivity Trends*, Table 55.

[52] By "capital quality," I refer to the intangible (and unmeasured) inputs that add to the nonlabor income of an enterprise, but are not included in its measured capital stock. Included in these would be entrepreneurial skill and investment in research and development. In this connection, Kendrick reports (Ch. VI, p. 183) a rank correlation coefficient of +.68 between research and development expenditures, as a per cent of sales in 1953, and the average annual rate of change in total factor productivity in 1948-53.

Contrasted with these slight positive correlations is the very substantial negative rank correlation coefficient, during 1899-1953, between unit prices of output and factor productivity, —.55, which is significant at 5 per cent.[53] The sign of this coefficient is what the competitive hypothesis would lead one to expect. Combined with the other findings cited it bears out the view that, as between industries, the relative gains of factor productivity are passed on to buyers and none accrue to the factors employed.

(3) Still a third finding that bears upon the competitive hypothesis is the behavior of the interindustrial wage structure itself. We have seen that there has been a secular decline in skill differentials in the economy as a whole. What has been said of skill differentials also applies to geographical differentials.

We also know that the ranking of industries with respect to their level of earnings per worker is quite stable over long periods of time. That is, the rank correlation of an industry's position in the industrial wage hierarchy in one year (or period) with another very distant in time is "quite high." For example, Cullen[54] found a rank correlation coefficient of +.66 for 76 manufacturing industries between ranks of per-worker annual earnings in 1899 and 1950. In Kendrick's data, the rank correlation between average hourly earnings in 1899-1909 and 1948-53 was +.46.[55] Slichter found a coefficient of rank correlation of +.7289 between the average hourly earnings of male unskilled labor among 20 manufacturing industries in 1923 and 1946.[56]

Because of the secular decline in skill margins and in regional differentials, the competitive hypothesis implies that there would have been a secular decrease in interindustry relative wage dispersion if the skills and geographical mix had remained more or less unchanged.[57] The evidence that there has been a secular decrease in interindustry relative wage dispersion is far from conclusive; and Cullen's scepticism of this evidence as proof of secularly[58] reduced dispersion seems fully

[53] Kendrick, *Productivity Trends*, Table 57.

[54] D. E. Cullen, "The Inter-industry Wage Structure, 1899-1950," *American Economic Review*, June 1956, pp. 353-69, especially Table II, p. 359.

[55] Kendrick, *Productivity Trends*, computed from Table 54.

[56] S. H. Slichter, "Notes on the Structure of Wages," *Review of Economics and Statistics*, Feb. 1950, pp. 80-91, especially p. 88 and Table 7.

[57] Kerr ("Wage Relationships," pp. 189-191) argues this very strongly, though without indicating the crucial role of the competitive hypothesis.

[58] The very marked and undisputed declines since the late 1930's are irrelevant for long-run analysis, as it seems clear that at that time these differentials were abnormally large.

warranted.[59] It is possible that further investigation will show that dispersion has indeed been reduced. But if it does not, certain more or less alternative inferences may be drawn: (1) despite the general decline in skill margins, the relative wage premiums that must be paid by industries that are expanding their labor forces rapidly were as great in the late 1940's as at the turn of the century;[60] (2) there was an increasing dispersion in the "richness" of industrial skill mixes[61] which offset the reduced skill margins; (3) there were offsetting inter-industry changes in skill mixes, locational mixes, etc.; and (4) the competitive hypothesis is wrong. These inferences are not mutually

[59] Cullen, "The Inter-industry Wage Structure," p. 361. Further evidence to the same general effect is provided by correlating the percentage wage change between 1899 and 1953 (from Kendrick's data) with the index of "richness of skill mix" by industry, in 1950 (see n. 63). If the relative richness of skill mix of the various industries had been unchanged over time, the percentage wage increase should have been the smaller, the richer the skill mix, because of the secular decline in skill margins. However, the rank correlation coefficient was only —.086 (between richness of skill mix in 1950 and percentage change in wages between 1899 and 1953). The sign is in accord with the hypothesis of no change in relative skill mix, but far too small to be taken seriously.

[60] That is, the industries that are "very high" wage payers in any given year include a disproportionate number of those expanding rapidly, and therefore trying to increase their total labor force; the converse applies to those industries that are "very low" wage payers. Industries at either end of the rankings include a disproportionate fraction of those in temporary disequilibrium. Naturally, those industries need not be the same ones in 1899 and 1950.

[61] By "richness of the skill mix," I refer to the relative numbers of skilled, semi-skilled, and unskilled workers employed. An industry's skill mix is richer, the greater the fraction of the first, and the smaller the fraction of the last in the work force.

We can measure the richness of the skill mix of different industries in 1950 from the statistics of *Occupation by Industry* which, so far as we are aware, has not been published for any other Census. The measure of the richness of an industry's skill mix is defined as the following weighted average:

$$R_i = \frac{\sum_{j=1}^{n} a_{ji} W_j}{E_i} \text{ (males)} + \frac{\sum_{j=1}^{n} a_{ji} W_j}{E_i} \text{ (females)}$$

This weighted average refers to the i th industry; W_j is the median annual earnings of persons in the j occupation throughout the economy; a_{ji} is the number of persons of given sex employed in the i th industry, and the i th occupation; and E_i is the number of persons employed in the i th industry. R_i is a weighted average of the nation-wide median occupational earnings of the employees in the i th industry with the fraction of the i th industry employment in the various age-sex classes serving as weights.

Sex, as well as occupation, is treated as a determinant of skill mix because women, even in the same occupational category and industry, tend to be paid less than men (for whatever reason). It would have been better to have corrected our weights for degree of unemployment, but we were unable to do so. The richness of skill mix in 1950 was rank correlated with median industrial annual earnings in 1949 by a coefficient of +.613.

exclusive, and they could all be true to a degree; however, none of the first three has yet been tested, though it is far from impossible to do so.

We have already presented some evidence which tends to reject (4); i.e., which tends to support the competitive hypothesis. And there is some further evidence to the same effect: both Cullen and Woytinsky find evidence of diminishing secular dispersion of interindustrial earning among the particular industries that happened to be at the upper and lower extremes of the distribution in a particular year.[62] This means that, although the over-all interindustry dispersion among a collection of industries may not have diminished appreciably over time, the spread among the group of industries that happened to be paying very high and very low wages in a given base year (e.g., 1899 or 1929) diminished. In other words, the particular industries that are toward the high and low extremes in the interindustrial earnings hierarchy in a given year tended to regress toward the mean with the passage of time.

This is what the competitive hypothesis implies will happen; for, in any given year, part of the interindustry dispersion of wages is due to disequilibrium of industries expanding and contracting employment more than the average, and this source of interindustry wage dispersion is reduced over time by the operation of the price system. The competitive hypothesis implies nothing concerning the long-term trend in interindustrial wage dispersion among a particular group of industries as a whole, except that it should depend solely upon variations in skill and locational differentials and random disturbances.

One further hypothesis, not strictly of a long-run variety, should be mentioned. The rise of an economy from a less- to a more-full utilization of its labor force (including its reserves) may cause a reduction in interindustry differentials, as happened when the economy emerged from the depression of the 1930's to the full employment of

[62] Cullen ("Inter-industry Wage," Table III, p. 361) found a reduction between 1899 and 1947-50 of 8-12 per cent in the difference between the median annual earnings in industries in the upper and lower quartiles of the distribution of 84 manufacturing industries, in 1899. W. S. Woytinsky (*Employment and Wages in the United States*, New York, Twentieth Century Fund, 1953, Chap. 39, pp. 460-462 and 507-509) found a tendency for low-wage industries in 1929 to have climbed relatively to high-wage industries by 1950.

Cullen (pp. 364-365) notes that most of the narrowing in dispersion in his data occurred before 1921. This, of course, would suggest that the short-run disturbances had been washed out before that date. This interpretation of Cullen's findings is different from (and possibly inconsistent with) his own.

the 1940's.[63] Such behavior would follow from the narrowing of skill differentials during such periods. Whether this limited experience can be generalized to a proposition relating level of employment, or growth rate in labor demand, to the interindustry dispersion of wage rates is not clear. However, it is a possibility.

Several other hypotheses concerning the long-run equilibrium industrial structure of wage rates have been advanced by Slichter:[64]

(1) "The average hourly earnings of male unskilled labor (U) tend to be high where the average hourly earnings of male semiskilled and skilled labor (S) are high." Slichter found, in 1939, a rank correlation coefficient of +.7098 (among 20 manufacturing industries) between U and S. If this correlation is interpreted as resulting from a tendency for industries using relatively expensive types of skilled labor also to use expensive types of nonskilled, then it is compatible with the competitive hypothesis. Slichter accepts this interpretation in part,[65] but also contends that the correlation is partly due to company wage policy, which presumably is independent of market forces; on this point, see below. It should be noted that it is also possible that Slichter's observation reflects short-period and not long-period forces; i.e., expanding industries are more likely than others to encounter increasing supply prices (as a function of rate of increase of employment) for all kinds of labor.[66]

(2) "The hourly earnings of male common labor (M) have some (not pronounced) tendency to be low where the percentage of women (W) among wage earners is high." The coefficient of rank correlation between M and W in 1939 (for 19 manufacturing industries) was +.4491, and in 1929, +.5224.[67] This, as Slichter (in effect) argues, may well reflect the operation of the competitive mechanism; i.e.,

[63] This has been stressed in two studies of English data: P. Haddy and N. A. Tolles, "British and American Changes in Inter-industry Wage Structure under Full Employment," *Review of Economics and Statistics*, Nov. 1957, pp. 408-414; and P. Haddy and M. E. Currell, British Inter-Industrial Earnings Differentials, 1924-1955," *Economic Journal*, Mar. 1958, pp. 104-111. This tendency also appears in Cullen's data ("Inter-industry Wage") for World War II; however, it does not appear during World War I.

[64] Slichter, "Notes on the Structure of Wages." Slichter does not distinguish carefully between long- and short-run relations; consequently, the interpretation placed on his findings is entirely our own.

[65] *Ibid.*, p. 84.

[66] This possibility would seem less likely in 1939, to which Slichter's data refer, than in the 1920's, 1940's, or 1950's. It is also possible that the correlation reflects the common effect of locational factors.

[67] Industries are ranked in inverse order of male common labor earnings.

women are hired mainly in low-wage industries and men, in order to compete with them, must accept less than the average male wage. That is, the correlation is presumed to reflect competition for similar jobs, and not osmosis. If this explanation is correct, then the industries where women are most highly concentrated should be those in which the unfavored (by the market) males, e.g., Negroes, are also concentrated; and this seems to be the case.[68]

(3) Slichter also found substantial rank correlation between net income after taxes, as a percentage of sales, π, and average hourly earnings both of unskilled and of skilled and semiskilled workers.[69] Slichter interpreted π as an index of profitability. Accepting this interpretation, we could easily rationalize the observed rank correlations as short-period phenomena resulting from the short-run association between increased labor demand and profitability. However, Slichter, like many other writers, contends that this phenomenon "reinforces the view that wages, within a considerable range, reflect managerial discretion, and that where managements are barely breaking even, they tend to keep wages down."[70] This interpretation is incompatible with the competitive hypothesis.

We believe that the importance of this possibility can easily be exaggerated. Nonetheless, the field work on our study of interfirm wage differentials has confirmed the oft-expressed view that large and profitable firms will often ignore local labor market situations by overpaying on certain jobs in certain areas in order to avoid undesired intercompany differentials. Such firms also manifest a desire to be toward the top of any labor market in which they hire, both for reasons of prestige and quality selection.

To be sure, there is a tendency for out-of-line wages to be corrected "as soon as the opportunity presents itself," but it is also true that large firms are more dilatory about correcting overpayment (e.g., red circle rates) than correcting underpayment. This, together with a preference for selective recruitment policies, creates an upward bias in wage level relative to the market as of any given moment. Thus, we would be inclined to agree that large and profitable firms do tend to pay more

[68] The rank correlation coefficient between percentage of women and percentage of Negroes (among males) employed (from 1950 Census data) for 14 industries was +.386; when finance and agriculture are excluded, the coefficient is raised to +.662. However, the osmosis hypothesis requires further investigation.

[69] Slichter, "Notes on the Structure of Wages," p. 88.

[70] *Ibid.*, see also p. 90.

at any one time than could be explained by the competitive hypothesis. However, this cannot explain *movements* in relative wages; at most, it can explain relative wage levels as of a given moment.

These remarks pertain directly to individual firms, and not to entire industries. Their relation to the industrial wage structure results from the fact that in some industries the percentage of workers employed in large firms is greater than in others. Industries concentrating relatively large fractions of their labor forces in large firms should tend to exhibit relatively high concentration ratios;[71] hence there might well be an association between high concentration ratios and high wages at a *given moment of time.*

However, this is no reason to suppose there would be an association between *changes* in relative industrial wage levels over time and the index of concentration as of a given moment, as some writers have argued.[72] These writers contend that the index of concentration is a rough (inverse) indicator of the relative degree of competitiveness of an industry;[73] and that noncompetitive industries tend to raise wages more than others. But, since it is not alleged that the indexes of concentration for different industries have changed during the relevant time period, it cannot be permanent differences in industry structure that are responsible for differential wage behavior; it must be differential *changes* in industry behavior. That is, what must be explained are differential changes in the willingness or ability or both of highly concentrated industries (relative to others) to grant wage increases; to our knowledge this has never been attempted. It should also be noted that to relate *levels* of concentration with *increases* (in favor of concentrated industries) in wages implies a secular increase in wage dispersion which is grossly inconsistent with known facts.

Because the hypothesis that interindustrial differences in degree of monopoly are an important factor in explaining the interindustrial differences in wage behavior has had wide currency, and is obviously a rival to the competitive hypothesis, we have attempted one rather simple test of it. We have taken Nutter's data on the relative extent

[71] As measured by (say) the percentage of the industry's employment concentrated in the four or eight largest firms.

[72] For example, H. M. Levinson, "Post-war Movement in Prices and Wages in Manufacturing Industries," *Study Paper No. 4,* Joint Economic Committee, Congress of the United States, 1960, pp. 2-5 and 21; also J. W. Garbarino, "A Theory of Inter-industry Wage Structure," *Quarterly Journal of Economics,* May 1950, pp. 282-305, especially pp. 299-300.

[73] This is highly debatable but, for the sake of argument, let us concede it.

of monopoly in 1899 and 1937 by major industry groups,[74] and correlated the change in the rankings between those dates with the change in the rankings of wages paid by those groups.[75] The correlation coefficient of these rank changes was —.05, indicating a slight (negligible) tendency for a decrease in monopoly to accompany an increase in wages—inconsistent with the hypothesis.[76]

(4) Slichter alleges a strong inverse association between hourly earnings of unskilled labor and the ratio of payrolls to sales. He explains this by saying: "Managements naturally are more concerned about the rates which they pay for labor when payrolls are large in relation to the receipts of the enterprise than when payrolls are small."[77] One (slightly astonishing) implication of this is that vertical disintegration, per se, leads to high wages. But leaving this aside, let us concede that, in the absence of competition, a low ratio does make it easier for a benevolent employer or an aggressive union to raise wages than otherwise. However, before accepting this as an important determinant of industrial wage differences, we would urge consideration of the following alternative: high ratios of payrolls to sales are more likely to be found in industries that specialize in fabricating operations, and are associated with low wages because the likelihood of such specialization is greater where the fabrication can be performed by low-wage labor.

But at the very most, the above relation obtains only at a given instant. It provides no warrant for a long-run interpretation of Dunlop's contention that "wage and salary rates would be expected to increase most . . . where labor costs are a small percentage of total costs."[78]

[74] G. W. Nutter, *The Extent of Enterprise Monopoly in the United States, 1899-1939*, University of Chicago Press, 1951, Tables 10 and 11.

[75] The wage figures were obtained as follows: 1953 (annual average) hourly wages were extrapolated back to 1899 by means of Kendrick's data, and ranks were obtained; these were compared with the ranks of median annual earnings per worker in 1939 as reported in the 1940 Census (see H. P. Miller, "Changes in the Industrial Wage Distribution of Wages in the United States, 1939-1949," *An Appraisal of the 1950 Census Income Data*, Studies in Income and Wealth, Vol. 23, Princeton for NBER, 1958, Table B-2. It is assumed that the 1937 and 1939 rankings would be virtually the same.

[76] David Schwartzman ("Monopoly and Wages," *Canadian Journal of Economics and Political Science*, Aug. 1960, pp. 428-38) reaches a similar conclusion on the basis of comparing United States and Canadian industries with varying concentration ratios.

[77] Slichter, "Notes on the Structure of Wages," p. 87.

[78] J. T. Dunlop, "Productivity and the Wage Structure," *Income, Employment and Public Policy, Essays in Honor of A. H. Hansen*," New York, Norton, 1948, p. 360. In fairness to Dunlop, it should be noted that he has not indicated whether

So far as we are aware this contention has never been substantiated *for the long run.*

(5) One determinant of an industry's place in the interindustry wage hierarchy at a given moment is the relative richness of its skill mix. For 1950, we ranked industries by richness of skill mix and correlated this with rank in the interindustry wage hierarchy; the rank correlation coefficient was +.612.[79] This cross-sectional relationship reflects departures from long-run equilibrium, crudeness of industrial classifications, etc. Nonetheless it indicates a substantial degree of relation between the two sets of rankings.

SHORT RUN

Let us begin our discussion of the short-run behavior of the interindustry wage structure by considering the relation of its variations to those in employment. The competitive hypothesis explains such variations as due to wages rising in industries where employment is expanding because of short-run inelasticities of labor supply, and falling in industries where employment is shrinking because of labor immobility. In the short run, *differential* changes in skill mix are assumed to be uncorrelated with differential changes in employment.[80]

There has been a number of studies of the relation of variations in the interindustry wage structure to changes in employment. Unfortunately, not all of their findings are mutually consistent. For example, Garbarino[81] found a rank correlation coefficient of +.48 between percentage changes in hourly earnings and employment (for 34 manufacturing industries) in 1923-40; Ross and Goldner found that in three of four periods studied there was a strong positive association of percentage increases in hourly earnings and percentage increases in employment.[82] Ostry found that in Canada there had been

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he intended this relationship as long or short run. The short-run version is discussed below.

[79] This coefficient was computed from an analysis of 14 major industry groups.

[80] See Appendix.

[81] Garbarino, "Theory of Inter-industry Wage Structure," p. 304.

[82] A. M. Ross and W. Goldner, "Forces Affecting the Inter-industry Wage Structure," *Quarterly Journal of Economics*, May 1950, pp. 254-281, especially Table VI, and pp. 272-276. The four periods studied were 1933-38, 1938-42, 1942-46, and 1933-46; the deviant period was the wartime interval 1942-46. The authors present no correlations but merely place industries into four quartiles in accordance with the percentage increase in employment.

F. C. Pierson (*Community Wage Patterns*, University of California Press, 1953, Chap. VI) also finds a positive rank correlation between average hourly earnings

an appreciable correlation between percentage changes in hourly earnings and in employment; among 36 industries, the correlation coefficient in 1945-49 was +.44; in 1949-56 it was +.53, and for 1945-56, +.56.[83]

Moreover, Hansen and Rehn, in a study of wage differentials from 1947 to 1954 among eight industries in Sweden,[84] found substantial interindustry correlation between wage drift[85] and excess demand[86] for labor, which is consistent with the hypothesis that short-run wage differentials result mainly from differing rates of increase in labor demand. They found virtually no correlation of wage drift with gains in average man-hour productivity, but were unable to use Swedish profit data for interindustry analyses.

But the data do not all point to one conclusion: Slichter found among 20 industries, during 1923-39, a coefficient of rank correlation (between percentage changes in hourly earnings and percentage changes in employment) of only +.2812.[87] Eisemann found that in 1939-47, percentage increases in manufacturing wages were negatively correlated with percentage increases in employment; however, the absolute increase in average hourly earnings was positively correlated with percentage increases in employment.[88] Levinson[89] has found that in 4 of the 11 year-to-year changes between 1947 and 1958 there was

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and employment for manufacturing industries among several cities between 1929 and 1939, but not during the war period, 1940-48.

[83] S. W. Ostry, "Inter-industry Earnings Differentials in Canada, 1945-1956," *Industrial and Labor Relations Review*, Apr. 1959, pp. 335-352, especially pp. 341-343.

[84] B. Hansen and Gosta Rehn, "On Wage-Drift: A Problem of Money-Wage Dynamics," *Twenty-five Economic Essays in Honour of Erik Lindahl*, Stockholm, 1956, pp. 87-133, especially pp. 105-106 and 128-133.

[85] That is, wage increase in excess of what was implied in collective bargaining agreements.

[86] That is, unfilled vacancies minus unemployment.

[87] Slichter ("Notes on the Structure of Wages," p. 90) argues very explicitly that the relation between hourly earnings and profits is due to wage policy and not labor-market pressure. He found a small *negative* coefficient of rank correlation between changes in employment and changes in average hourly earnings in 1923-39 for *unskilled* workers (as contrasted to the positive coefficient for all workers). Somehow, this argument is not very impressive. (1) As argued above, one would expect the supply of unskilled workers to a given industry to be more elastic in the short run than that of semiskilled and skilled. (2) Slichter's period is almost identical with that of Garbarino ("Theory of Inter-industry Wage Structure"), who found evidence of a stronger relationship than Slichter, and with better data.

[88] Doris M. Eisemann, "Inter-Industry Wage Changes, 1939-1947," *Review of Economics and Statistics*, Nov. 1956, p. 446.

[89] Levinson, "Post-war Movements," Table 1, p. 3.

a negative correlation among 19 manufacturing industries between percentage changes in straight-time hourly earnings and percentage changes in production worker employment. He also found a negative partial correlation coefficient between this pair of variables for 1947-53 and a negligible positive one (+.0046) for 1953-58.[90]

Bowen[91] computed correlation coefficients between percentage changes in average hourly earnings, w, and percentage changes in employment, e, during six subperiods of the interval 1947-59. These various coefficients reflect the association between w and e among 20 two-digit manufacturing industries. Bowen computed both simple and partial correlation coefficients. The partial coefficients between w and e held constant some or all of the following: (1) average level of profits in the industry; (2) the concentration ratio (in 1954); and (3) the percentage of the production workers unionized (in 1958). All possible first and second order partial correlation coefficients between w and e (holding constant the other variables, both singly and in pairs) are presented. The coefficients show a positive correlation between w and e in the three subperiods when unemployment was relatively low,[92] and this relation is generally stronger in the partial than in the simple coefficients. In the three subperiods in which unemployment was relatively high, the coefficients showed a different pattern: in two of these three subperiods the simple coefficients were negative; in one of them all of the partials were negative; and in another, half of them were negative.

Thus Bowen's findings (on this point) tend to support the competitive hypothesis for periods of "low unemployment," but not for those of higher unemployment. That the relation between w and e should be stronger in periods of low unemployment is in the spirit of the competitive hypothesis (though not its letter);[93] i.e., in periods of low unemployment, short-run elasticities of labor supply to indus-

[90] *Ibid.*, Table 2, p. 4. A. H. Conrad ("The Share of Wages and Salaries in Manufacturing Incomes, 1947-1956," *Study Paper No. 9*, Joint Economic Committee of Congress, Washington, 1959) obtained similar results on *Census of Manufactures* data for all 61 three-digit industries, for the period 1949-56.

[91] W. G. Bowen, *The Wage-Price Issue: A Theoretical Analysis*, Princeton University Press, 1960, pp. 59-66 and Table E-1, pp. 134-135.

[92] The subperiods of low unemployment are characterized by an unemployment percentage (of the civilian labor force) that was "generally below 4.3." The subperiods of high unemployment are those where the unemployment percentage was always above 4.3. Bowen, pp. 24-29.

[93] The letter of the competitive hypothesis makes no provision for unemployment as a variable in supply or demand functions.

tries are likely to be smaller, and differential increases in employment therefore more likely to produce differential wage changes. But if Bowen's findings are accepted, then the competitive hypothesis is uninformative, if not invalid, as an explanation of short-run wage movements in the presence of "appreciable"[94] unemployment.

In short, the evidence does not give unqualified support to the view that short-run variations in labor demand are a major cause of variation in straight-time hourly earnings. Some of the contrary evidence can be "explained away." The adverse findings of Ross and Goldner for 1942-46 and of Eisemann for 1939-47 may well be due to the fact that the war industries which expanded most rapidly were the very ones where dilution of the skill mix was greatest. However, it is harder to explain away the findings of Levinson, Conrad, and especially Bowen. Let us now turn to alternative explanations.

PROFITS, CONCENTRATION, AND RELATED VARIABLES

Levinson suggests that relative industry wage levels have varied either with (industry levels of) current profits or with profits lagged one year.[95] He measures profits as return on stockholders' equity both before or after taxes. This alleged relation is not, of itself, inconsistent with the competitive hypothesis, for the level of current profits would be expected to be associated with recent increases in employment. However, Levinson computes partial correlation coefficients between percentage wage changes, w, and percentage increases in employment, e (average profit level, P, constant), for 1947-53 and 1953-58 and also between w and P (e constant) for the same interval. In 1947-53, the coefficient between w and e was negative, while that between w and P was positive; in 1953-58, the latter coefficient substantially exceeded the former though both were positive.[96]

These findings were similar to those of Bowen, who finds a consistent positive correlation (among 20 manufacturing industries) between percentage change in average hourly earnings and percentage change in average level of profits.[97] This positive relation is found in the

[94] Using Bowen's 4.3 per cent as a criterion for distinguishing years of appreciable unemployment from others, 33 of the first 58 years of this century were years of "appreciable unemployment." Even if we exclude the 11 years, 1930-40, 22 out of 47 years showed appreciable unemployment. (These figures are Stanley Lebergott's as quoted by Bowen in Appendix A, pp. 99-101.)

[95] Levinson, "Post-war Movement," pp. 2-7.

[96] Levinson, "Post-war Movement," Table 2, p. 4.

[97] Bowen, *Wage-Price Issue*, pp. 67-69 and 134-135.

simple correlation coefficients in all of Bowen's subperiods; it is also found among the partial coefficients (save for three small negative ones).

What are we to make of these findings? Barring some unperceived differential change in hiring requirements (among industries), we would seem driven to accept Slichter's judgment that "wages, within a considerable range, reflect managerial discretion, that where managements can easily pay high wages they tend to do so, and that where managements are barely breaking even, they tend to keep wages down."[98] This judgment is, of course, incompatible with the competitive hypothesis for the short run.

It is important to distinguish sharply between levels and movements. It is entirely in keeping with the competitive hypothesis that more profitable firms should find it advantageous to demand superior personnel, and pay more to get it. This is essentially what Slichter, Reynolds, and others have contended. What is more difficult to accept is the finding that differences in profit levels also explain *movements* in interindustry differentials.

For certain periods, the *level* of current profits may well be related to the *change* in wage rates. One such period seems to have been 1947-58, and perhaps there have been others. But unless we are to infer a secular trend toward increasing wage dispersion in favor of the high-profit industries—a trend which no one has alleged and which would be inconsistent with the available evidence—we are left with the problem of explaining why the profit-wage relation is so intermittent. To say that the relation may well hold for one period but not for another is merely to state the facts. The problem of theory is to indicate the differential characteristics of the periods when it does hold and those when it does not. Let us consider a possible explanation.

Despite the evidence he presents on the association of wage change and profit level, Bowen distrusts differentials in profit levels as an explanatory factor of differential in wage changes among industries.[99] He does so mainly because he feels that the partial correlations in the 1954 and 1958 recessions were very small, and that the dominant factor in the simple correlation was the high intercorrelation among wage changes, concentration ratios, and degree of unionization.

Because of the behavior of the partial correlation coefficients Bowen

[98] Slichter, "Notes on the Structure of Wages," p. 88.
[99] Bowen, *Wage-Price Issue*, p. 68.

(rightly) rejects the possibility of a consistent *ceteris paribus* relation between wage changes and either concentration or unionization, taken separately. However, he contends that when we consider the combined effect of concentration and unionization (which are strongly intercorrelated in his sample), we find a stable relationship.[100] In discussing this, Bowen abandons correlation analysis and instead divides his industries into two groups: a "market power" sector (consisting of industries that are both highly concentrated and highly unionized) and a "competitive" sector in which the industries have the reverse characteristics. He argues that, with one exception,[101] the percentage change in average hourly earnings was greater in the market-power sector in all of the subperiods between 1947 and 1959. Although recognizing that the average level of profits was generally higher in the market-power sector, he says "that the importance of profits in this picture ought not to be exaggerated."[102] He also rejects the possibility that different rates of growth in employment are a differentiating characteristic of the two sectors.[103]

Bowen does not allege that what he has observed is part of a secular trend, but neither does he attempt to indicate what special characteristics of the period, 1947-59, are responsible for the unusual behavior observed, or why such behavior cannot persist indefinitely.[104] However, one possible explanation seems to be fairly obvious: the market-power sector contains a relatively large number of firms that respond to market stimuli rather slowly as compared with firms in the competitive sector. This relative sluggishness reflects the fact that *investment* decisions are expensive; can be made only infrequently and cannot easily be reversed. This would seem to be most characteristic of those sectors of the economy where "productive capacity" per unit of output is relatively expensive and long lived. Firms with these characteristics typically produce durable goods and are disproportionately found in

[100] *Ibid.*, pp. 74-81.

[101] The period January 1949-October 1950.

[102] Bowen, *Wage-Price Issue*, p. 78.

[103] Whatever the validity of this contention, it is not consistent with his own data. From Table 13, p. 77, we can compute (putting initial employment in Jan. 1947 = 100) that employment in June 1959 was 102.5 in the market-power sector but only 97.7 in the competitive sector.

[104] Bowen does offer an explanation, in spirit similar to what follows, of wage behavior in the 1954 and 1958 recessions (pp. 82-84). However, he does not recognize that if there is growth in differentials in recessions, there must be either (1) contraction of the differentials in periods of high employment or (2) a secular trend (in the differentials).

Bowen's market-power sector. Investment and output decisions in these firms respond not so much to current profits, as in the competitive sector, but to (moving) averages of current and past sales and profits which serve (along with other indicators) as a guide to the future.

On this view, the wage behavior described by Bowen is explained thus: at the end of World War II both sectors were confronted with situations of strong demand and high profits. The competitive sector acted to eliminate its excess demand faster than the market-power sector where long gestation periods of capital goods combined with a cautious outlook to hold back the investment program. This made the market-power sector's period of high profits, full capacity operation, and "strength" in product prices last longer than the competitive sector's period did. This, in turn, facilitated the payment of relative increases in earnings (in the market-power sector) during most of the 1950's.

If this explanation is correct, the growth of excess capacity and price weakness in the market-power sector during the late 1950's will soon end its relative wage gains, if it has not already done so. Also, if true, there should have been similar periods to the 1950's in the past—e.g., in the 1920's, as an aftermath of World War I. This particular hypothesis implies the same response pattern of wage changes to current profits or to current labor market conditions in Bowen's two sectors; it requires (1) that the two sectors both start from initial positions of excess product demand; (2) that the response mechanism of the market-power sector to excess product demand be slower, so that it takes a longer period to reach equilibrium; and (3) that relative wages in the competitive sector be high enough to obviate the need for short-run adjustments under pressure of a growing relative labor scarcity. It is quite possible that the wage adjustment mechanism in the market-power sector is also more sluggish; the differential incidence of long-term contracts with automatic deferred wage increases would suggest this.

We also suspect that, in the above explanation, the role of product prices is crucial. That is, when firms believe that cost increases can be passed on to buyers, they are more inclined to grant wage increases than when the reverse is the case. This is consistent with some findings of Dunlop on the relation of changes in wages and product prices,[105]

[105] J. T. Dunlop, *Wage Determination under Trade Unions*, 2nd ed. New York, Kelley, 1950, Chap. VII. Dunlop found a strong positive association (among industries) between declines in wage rates and product prices during the recessions of 1929-32 and 1937-38, and presents substantial evidence to support this observa-

which should be investigated further and brought up to date, especially the interrelation among changes in wages, product prices, and profits.

But if there is anything to the idea that there are two important sectors of the economy in which the ratio of *current* wages to the *current* marginal productivity of labor[106] behaves differently, it is incompatible with the competitive hypothesis as an explanation of wage behavior in the short run. This is because shifts in the ratio of current wages to current marginal productivity of labor can always be expressed as changes in the elasticities of (imagined) supply of factor or demand for product curves or both, whose alleged shifts are the staple of noncompetitive explanations of relative wage and price behavior. Such short-run shifts are simply the obverse side of a pattern of delayed response to market stimuli; it may also be true that a pattern of delayed response—especially in adding to productive capacity—requires some restriction upon entry and a substantial degree of concentration. Failure of either of these conditions to obtain may make it impossible for any one firm to hold off on expansion because of its inability to keep communicating with—or even to keep track of—all its potential rivals. The reader will understand that this is to suggest an hypothesis; testing it is another and far more difficult matter.

There are a number of other ad hoc short-period hypotheses concerning the behavior of industry wage levels. For example, it has been suggested that changes in industry wage levels tend to be more closely related in absolute than in percentage terms. While more careful writers have usually agreed that neither percentage nor absolute measures of changing differentials was ideal, the argument is that, because of union or governmental pressure, or both, industries tend to obtain equal absolute wage increases rather than equal percentage hikes.[107] This argument has been widely discussed in recent years, and appears to have had considerable validity for the period 1933-45, when

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tion for the two depressions in question. However, Levinson ("Post-war Movement," p. 15) found that from 1947 through 1951-52 "price changes were unrelated to changes in gross hourly earnings—after that point, however, the correlation became very much stronger."

[106] That is the value of the marginal physical product as reflected in the output records for the *same* accounting period to which the wages are imputed. The reason for this rather narrow view of the competitive hypothesis is indicated in the Appendix.

[107] A very large number of writers have argued in this fashion. One of the earliest was A. M. Ross, *Trade Union Wage Policy*, University of California Press, 1948, Chap. VI.

interindustry (like other) differentials were narrowing. However, for the period since 1947, the hypothesis does not seem so plausible.[108] It is worth noting that this hypothesis implies, contrary to the competitive hypothesis, that in the short run relative wage levels are altered by variations in supply determinants (union and government policies) rather than by variations in demand determinants.

To argue that movements in relative wage levels are strongly correlated with levels of relative profits or changes in relative product prices is not to contradict the competitive hypothesis, per se. For both of the aforementioned independent variables may be correlated with variations in the level of employment and reflect only the influence of this variable on relative wage levels. Moreover, industries with high current profits might well be industries in process of an unusually marked tendency to be hiring workers to operate new processes or to work in newly developing high-wage areas, or both. Either or both of these tendencies could create (upward) labor market pressure on wage rates despite a tendency for over-all employment to decline. None of the studies to which reference has been made has attempted to control against these possibilities.

PRODUCTIVITY

Some writers have found that the increase in average hourly earnings was greater in industries where the increase in physical production per man-hour was greater; e.g., Dunlop[109] and Garbarino.[110] Barring a correlation of skill mix and/or location with productivity, such a relationship is incompatible with the competitive hypothesis in the long run; whether it is compatible in the short run depends upon whether increases in man-hour productivity are positively correlated with increases in employment via correlation with the *value* of labor's marginal physical product.

The alleged factual relation between man-hour productivity and wages has itself been disputed by Levinson,[111] Meyers and Bowlby,[112] and Perlman.[113] These authors, especially the last, rightly stress the

[108] For example, Levinson's data would not seem consistent with it.
[109] Dunlop, "Productivity and the Wage Structure."
[110] Garbarino, "Theory of Inter-industry Wage Structure," pp. 298-300.
[111] "Post-war Movement," Table 1, p. 3.
[112] F. Meyers and R. L. Bowlby, "The Inter-industry Wage Structure and Productivity," *Industrial and Labor Relations Review*, Oct. 1953, pp. 93-102.
[113] R. Perlman, "Value Productivity and the Inter-industry Wage Structure," *Industrial and Labor Relations Review*, Oct. 1956, pp. 26-39.

importance of product price movements in determining the relative average value productivity of labor in different industries. Despite the dispute about whether the various correlation coefficients are significant, and which periods should be studied, it seems that the coefficients are usually positive,[114] which suggests the existence of a positive short-run association, but one which is disturbed by extraneous factors whose intensity varies from one period to another.

How one is to interpret this association is another matter. Garbarino found that in Dunlop's data (where the correlation between increases in man-hour productivity and wages was strong), the coefficient of rank correlation between increases in employment and in man-hour productivity was only +.08.[115] Obviously this militates against the short-run competitive hypothesis that there is a positive association between changes in hourly earnings and changes in man-hour productivity, because of an empirical association of the latter with rising output and employment. Another possible explanation, of pertinence in the long run as well as the short, is that industries in which man-hour productivity increases most are those in which the skill mix is likely to improve most. Yet another possible explanation of this phenomenon posits the existence of a link between wage increases and rises in productivity via profits and ability to pay, à la Slichter, Levinson, et al. But there is no good reason, either in theory or fact, for accepting any of these hypotheses.[116]

UNIONS

Our discussion of interindustry wage differentials has obviously left out unions; the omission is intentional. The main reason for exclusion is the failure of previous research to obtain very satisfactory results in relating them either to the levels or movements in interindustry wage differentials. The well-known conclusion of Douglas and of Ross and

[114] But not always: Meyers and Bowlby turned up some negative coefficients ("Inter-industry Wage Structure," p. 98) and so did Levinson.

[115] "Theory of Inter-industry Wage Structure," p. 285.

[116] In a recent paper, L. Johansen ("A Note on the Theory of Inter-industrial Wage Differentials," *Review of Economic Studies*, Feb. 1958, pp. 109-113) concludes that "we may expect not changes in wage differentials, but wage differentials themselves to be correlated with the changes in productivity." This result, however, refers only to differentials that reflect labor market disequilibrium; i.e., his results depend on labor market disequilibrium embodied in his equation (4) on p. 110. For the short run, his conclusion is identical in empirical content with the conventional Marshallian one, where productivity is reflected in a parameter of the industry labor demand fraction.

Goldner[117] that new unionism is associated with differential percentage wage gains to an industry, but long-established unionism is not, was about as far as anyone had been able to go before the work reported on by H. G. Lewis in this volume. We shall not attempt to appraise this work here but only note its relevance to our discussion.

One possibility of detecting the influence of unionism is to analyze the association among industries between wage changes and profit levels, holding employment changes constant. If unionism is effective in making wages higher than they would have been in its absence, this should be reflected in a forced sharing of profits[118] which should be, in the short run, over and above the influence of labor market conditions. That is, the positive partial association between wage changes and profit levels should increase with the strength of unionism—however measured. Of course, the influence of extraneous factors such as changes in skill and locational mix must be somehow taken into account.

Conclusion

This paper's point of departure is that relative wage levels, both by skill and industry, behave more or less as though they were market prices reflecting predominantly the interplay of changing tastes, techniques, and resources—the competitive hypothesis. The implications of this hypothesis, however, are not so simple as they might seem because tastes, techniques, and resources interact in peculiar and complicated ways. Moreover, the basic hypothesis has required amendment to allow for the effect of changes in minimum wage laws, etc., for secular rural-urban migration, and for the gradual broadening of educational opportunities.

The competitive hypothesis is at its best, both in explaining skill margins and interindustry differentials, when it is used to explain variations over long periods of time. It can hardly be said to be firmly established as an explanation of wage phenomena even for long periods;

[117] P. H. Douglas, *Real Wages in the United States*, p. 564; Ross and Goldner, "Forces Affecting," p. 267.

[118] To test our hypothesis, it is necessary that unions be not "too strong"; i.e., unions must compel relatively more profitable firms to *share* their "excess profits" with wage earners (but not obliterate them), so that there are still greater than average profits to be observed. It is conceivable—though not likely—that unions could be so effective in raising wages that all potential supernormal profits were transferred to wages, completely obscuring the hypothesized relation.

but it has at least survived (reasonably well) the tests to which it has so far been put.

For the short run the competitive hypothesis does not appear very reliable. There are a number of findings concerning interindustry differentials which simply are not consistent with its short-run implication that relative industry wage rates vary in the same direction as relative changes in employment, in any given short-time interval (see Appendix). We are not without alternative short-run hypotheses; but these either break down during one time interval or another, or still are in a primitive state of formulation and testing.

In this paper we have discussed only skill and industrial differentials. No attempt has been made to analyze interfirm, interplant, and interregional differentials which, incidentally, may be associated with skill or industry differentials. However, this task will be attempted in the near future.

Appendix on Methodology

This brief appendix was written because of numerous criticisms and misunderstandings of my remarks on the "competitive hypothesis," its implications and their tests. This statement is not intended as original, in any fundamental sense; and obviously it is cursory. The views expressed, or similar ones, have been in the atmosphere for some time. Though the spirit of the exposition is similar to that of Milton Friedman's famous essay on the methodology of positive economics,[119] its content is different. In short, all of the blame for possible misstatement must rest here; much of whatever credit is due should go elsewhere.

When we speak of the "competitive hypothesis" we are referring to a hypothesis which states that the behavior of relative prices and quantities can be explained as though these prices and quantities were equilibrium values in a static economic model,[120] in which all pur-

NOTE: I am very grateful to my colleagues K. J. Arrow and Marc Nerlove for their criticism of an initial draft.

[119] Milton Friedman, *Essays in Positive Economics*, Chap. I, University of Chicago Press, 1952.

[120] This is not the entire content of static general equilibrium theory, but it is the part with which we are concerned. In particular, we are abstracting from those aspects connected with the determination of money prices.

In private discussion, objections have sometimes been made that the competitive hypothesis is not—or ought not to be—defined so as to apply only to cases where concurrent (unlagged) relations obtain among the variables. There can, of course, be no quarrel with a definition, but if the competitive hypothesis is defined so as

chases and sales are made by units bent on maximizing satisfaction[121] subject to individual budgetary restraints, having access to the same technology, and treating prices as parametric constants, independent of the quantities they (individually) purchase or sell. The prices in question, when taken as a set, make all excess demands equal to zero. As this argument is very well known, it is not considered necessary to spell out its details. The competitive hypothesis discussed here could be called, with propriety, the long-run static competitive hypothesis.[122] (The short-run version is mentioned briefly below.)

The utility functions of the households in our model shift with the tastes of the individuals who compose them; i.e., changes in a household's tastes are reflected in shifts of one or more parameters of its utility function. Similarly changes in the technology available to a firm are reflected in shifts of one or more parameters of its transformation function. Consequently, we may write the excess demand function for the j th kind of labor by the i th industry as

$$q_{ij} = D_{ij}\,(p_1 - p_n;\ v_{i1} - v_{im};\ a_{i1} - a_{iz};\ \beta_{i1} - \beta_{ih};\ \gamma_{i1} = \gamma_{id})\,(1),$$

For simplicity, we assume each of these industries to produce only one commodity; hence we have n prices and the subscript of the p_i's runs from 1 to n.[123] v_{ij} is the wage rate of the j th kind of labor in the i th industry, and $j = 1, 2 - - m$. Because our argument will be concerned not with the v_{ij}'s, but with their ratios, let us define $w_{ij} = v_{ij}/v_{tj}$, where v_{tj} is the wage paid the j th kind of labor in the t th industry. This means that in long-run equilibrium, $w_{ij} = 1$; i.e., all industries

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to permit lagging relations then, in the short run, there are no generally applicable theorems concerning concurrent price-quantity relations. The character of the concurrent relations will vary both with the structure of the lagged system and the time period. Because we prefer that the competitive hypothesis have some specific short run implications, we define it as applying solely to static relationships.

Kenneth Arrow has said he believes widely applicable theorems on price-quantity relations involving lags can be deduced from individual behavior functions which include weighted lags. That is, with only "reasonable" restrictions on the weights, theorems can be deduced which will apply almost as widely as do those of comparative statics in the long run. When these results are published it will be possible to push further the argument of this appendix; but they will not invalidate what is stated.

[121] Profit maximization is clearly a special case of this.

[122] This statement applies either to an economy in stationary equilibrium or to one always on its growth path. Some of the relative prices will vary from a stationary state to a situation of balanced growth, but not those discussed in this appendix.

[123] Capital goods are treated as outputs of particular industries and their prices are included in the p's.

must pay the same wage rate for a given kind of labor. $\alpha_{i1} - \alpha_{iz}$ are parameters reflecting changes in the tastes of the households; $\beta_{i1} - \beta_{ih}$ are other parameters reflecting changes in the technologies of the firms and $\gamma_{i1} - \gamma_{ir}$ reflect the resources owned by each decision-making unit. (For simplicity, when there is no danger of confusion we shall refer to the set, $\alpha_{i1} - \alpha_{iz}$, by α — without subscript; similarly, for the sets $\beta_{i1} - \beta_{ih}$ and $\gamma_{i1} - \gamma_{rd}$.)

Barring corner solutions, static equilibrium requires that $q_{ij} = 0$ $(i = 1, 2 - n;\ j = 1, 2 - m)$. It also requires that $v_{ij} = v_{rj}$ and $w_{ij} = 1$, where r is any industry other than i. Obviously, these are not sufficient conditions for equilibrium; however, they are necessary, and they are the only ones with which we shall work. Let us designate the value of w_j at which $w_{ij} = w_{rj}$ $(i = 1, 2 - n)$ as w_j^* — the equilibrium value of w_j. (As we have seen, $w_j^* = w_f^* = 1$, where f is a kind of labor other than j. However, to avoid confusion, we shall not utilize this fact and write the equilibrium value of w_j as w_j^*.) Let us also designate $q_{ij}^* = 0$ as the value of q_{ij} corresponding to w_j^* and $Q_{ij} = F(q_{ij})$ as the equilibrium quantity of j employed in the i th industry. (Q_{ij} is the observed employment of the j th kind of labor in the i th industry, and Q_{ij}^* is its equilibrium value.[124]) Neither w_j^* nor Q_{ij}^* is observable; they are intellectual constructions whose *raison d'être* is to explain the behavior of the observed w_{ij} and Q_{ij}. The competitive hypothesis, in effect, states that the behavior of w_{ij} and Q_{ij} is "approximately" the same as that implied by static economic theory concerning the behavior of w_j^* and Q_{ij}^*.

w_{ij} and Q_{ij} vary both from time to time and place to place. (For the sake of exposition, let us speak only of variation from one date to another, remembering that date and place are, in this discussion, interchangeable.) At any given date,

$$\log w_{ij} = \log w_j^* + \log \epsilon_{ij} \qquad (2)$$

[124] To avoid formal indeterminacy let us suppose that workers are not indifferent about the industry in which they work, at given relative wages, and that in equilibrium the industry of each worker is determined. However, to preserve the equilibrium condition, $v_{ij} = v_{rj}$, let us assume that workers' nonpecuniary preferences for industries are such that, *at equal wages*, the number preferring to work in a particular industry is proportional to the demand of that industry.

This assumption is, of course, restrictive and unrealistic. But to relax it would greatly complicate the exposition and obscure the point of the appendix. Moreover, to do so would not generate any interesting new possibilities; it would merely embody in the model the nonpecuniary preferences from which compensating wage differentials arise.

where ϵ_{ij} is a "disturbance" term whose log is assumed to be uncorrelated with log w_j^* and which reflects all the forces bearing upon log w_{ij} except those affecting log w_j^*; i.e., $w_{ij} = w_j^* \epsilon_{ij}$.

We assume that $E(\log \epsilon_{ij}) = 1$ and that $\sigma_{\log \epsilon_{ij}} = \lambda w_j^*$, where $\lambda < 1$; i.e., the standard deviation of the disturbance term is proportional to the equilibrium value. (Hereafter, for brevity, we shall write $\overline{w}_{ij}$ for log w_{ij}, $\bar{\epsilon}_{ij}$ for log ϵ_{ij}, etc. for all logarithms; we shall also assume all variables to be measured as deviations from their mean values.) The assumption embodied in (2) is, of course, arbitrary. Its only defence is that it is similar to those usually made in econometric research, and that some arbitrary assumption of this kind must be made to begin any argument relating theory and observation.

The movement of $\overline{w}_{ij}$ between any two dates (0) and (1) is represented by $\Delta\overline{w}_{ij}$; i.e., $\Delta\overline{w}_{ij} = \overline{w}_{ij}^{(1)} - \overline{w}_{ij}^{(0)}$. Similarly, we define $\Delta\bar{\epsilon}_{ij} = \bar{\epsilon}_{ij}^{(1)} - \bar{\epsilon}_{ij}^{(0)}$, and will adopt analogous conventions for all other variables. This gives us, by substitution in (2)

$$\Delta\overline{w}_{ij} = \Delta\overline{w}_j^* + \Delta\bar{\epsilon}_{ij}. \tag{2a}$$

We also have

$$\Delta\bar{Q}_{ij} = \Delta\bar{Q}_{ij}^* + \Delta\bar{\zeta}_{ij} \tag{3}$$

where $\bar{\zeta}_{ij}$ is a disturbance term. The analogue of each relation among $\overline{w}_{ij}$, $\overline{w}_j^*$ and $\bar{\epsilon}_{ij}$ that has been posited, is assumed to hold among $\bar{Q}_{ij}$, $\bar{Q}_{ij}^*$ and $\bar{\zeta}_{ij}$, and

$$E(\Delta\overline{w}_{ij}\,\Delta\bar{Q}_{ij}) = E[(\Delta\overline{w}_j^* + \Delta\bar{\epsilon}_{ij})\ (\Delta\bar{Q}_{ij}^* + \Delta\bar{\zeta}_{ij})] = E(\Delta\overline{w}_j^*\,\Delta\bar{Q}_{ij}^*) + E(\Delta\overline{w}_j^*\,\Delta\bar{\zeta}_{ij}) + E(\Delta\bar{Q}_{ij}^*\,\Delta\bar{\epsilon}_{ij}) + E(\Delta\bar{\epsilon}_{ij}\,\Delta\bar{\zeta}_{ij}). \tag{4}$$

Since $\overline{w}_j^*$ is the same in all industries it is a constant which implies, because we are measuring all variables as deviations from their means, that the first two terms in (4) vanish. And, as we interpret the competitive hypothesis, it implies that the second two terms vanish also. This means that the competitive hypothesis implies that $E(\Delta\overline{w}_{ij}\,\Delta\bar{Q}_{ij}) = 0$; i.e., that it implies an absence of correlation between percentage changes in wage rate and percentage changes in employment among industries *when the positions compared are those of long-run equilibrium.*

Crucial to this contention is the assumption that $E(\Delta\bar{Q}_{ij}^*\,\Delta\bar{\epsilon}_{ij}) = E(\Delta\bar{\epsilon}_{ij}\,\Delta\bar{\zeta}_{ij}) = 0$. To assert $E(\bar{Q}_{ij}^*\,\Delta\bar{\epsilon}_{ij}) = 0$ is to say that the per-

centage change in the long-run equilibrium volume of employment of the j th type labor in the i th industry is uncorrelated with the percentage change in the disturbance component in the wage rate paid it. In a literal sense, there is nothing in economic theory that implies this statement. But if it were not true, economic theory would be very different than it is or ever has been. It would have to be so constructed that transient disturbances to the parameters of the excess demand functions for factors would have permanent effects (i.e., would change long-run equilibrium values). This would mean that *equilibrium* prices and quantities would reflect not only the states of tastes, techniques, and resource endowments but also the whole history of prices including the effects of accidental disturbances. I doubt that one could construct useful economic models from such premises, and certainly no one has seriously tried to do so. Consequently, I shall interpret the competitive hypothesis as implying $E(\Delta\overline{Q}_{ij}^{*}\ \Delta\bar{\epsilon}_{ij}) = 0$. For similar reasons, we assume $E(\Delta\overline{Q}_{ij}^{*}\ \Delta\bar{\zeta}_{ij}) = 0$.

In the short run there is good reason to suppose that $E(\Delta\bar{\epsilon}_{ij}\,\Delta\bar{\zeta}_{ij}) > 0$. That is, the short-run disturbances that make measured employment exceed long-run equilibrium levels (e.g., unanticipated increases in demand) will also tend to make factor prices exceed long-run equilibrium levels. As $\bar{\epsilon}_{ij}$ and ζ_{ij} are disturbance terms, and there is no reason to suppose that disturbances become smaller with the passage of time, it follows that in the long run, as well as in the short,

$$E(\Delta\bar{\epsilon}_{ij}\,\Delta\bar{\zeta}_{ij}) > 0.$$

For concreteness let us assume that $E(\Delta\bar{\epsilon}_{ij}\,\Delta\bar{\zeta}_{ij}) > 0 =$ constant in any time period. This assumption is arbitrary, but necessary; to assume that the expression increased indefinitely with time would open the possibility that actual prices or quantities, or both, might deviate ever further from equilibrium ones—i.e., a long run might never be reached. To assume that the expression diminishes with time would add to the strength of long-run forces, but such an assumption would be unwarranted and unnecessary. In fact, all we need assume is that the expression does not increase so fast as to upset the following argument; to assume constancy is to impose—for simplicity of statement—a somewhat stronger condition than necessary.

If the competitive hypothesis is true, then $E(\Delta\overline{w}_{ij}\ \Delta\overline{Q}_{ij})$ will behave as $E(\Delta\overline{w}_{j}^{*}\ \Delta\overline{Q}_{ij}^{*})$ when Δ is so chosen as to generate a long run. As

we have seen, $E(\Delta\overline{w}_j^* \Delta\overline{Q}_{ij}^*)$ is zero, but $E(\Delta\overline{w}_{ij} \Delta\overline{Q}_{ij})$ is not because of $E(\Delta\bar{\epsilon}_{ij} \Delta\bar{\zeta}_{ij})$. But the correlation coefficient between $\Delta\overline{w}_{ij}$ and $\Delta\overline{Q}_{ij}$ will, under competitive conditions, approach zero asymptotically in the long run. By the usual argument, the closer the approach to zero, the smaller is the relative importance of $E(\Delta\bar{\epsilon}_{ij} \Delta\bar{\zeta}_{ij})$ in explaining the behavior of $E(\Delta\overline{w}_{ij} \Delta\overline{Q}_{ij})$. The correlation coefficient is

$$r = \frac{E(\Delta\overline{w}_{ij} \Delta\overline{Q}_{ij})}{\sigma_{(\Delta\overline{w}_{ij}^* + \Delta\bar{\epsilon}_{ij})} \sigma_{(\Delta\overline{Q}_{ij}^* + \Delta\bar{\zeta}_{ij})}} = \frac{E(\Delta\bar{\epsilon}_{ij} \Delta\bar{\zeta}_{ij})}{\sigma_{\Delta\bar{\epsilon}_{ij}} \sqrt{\sigma^2_{\Delta Q_{ij}^*} + \sigma^2_{\Delta\bar{\zeta}_{ij}}}}$$

The equality of the numerators follows from our argument concerning (4) which showed all its terms to be zero except $E(\Delta\bar{\Sigma}_{ij} \Delta\bar{\zeta}_{ij})$. The denominators are equal because $\sigma_{\Delta\bar{w}_{ij}^*} = 0$ *ex hypothesi*, and the expression under the radical sign is simply the expanded form of the standard deviation of a sum of two variables which are assumed to be uncorrelated.

Now in the long run $\sigma^2_{\Delta\bar{Q}_{ij}^*}$ increases indefinitely (see below). Since $\sigma_{\bar{\zeta}_{ij}} = \mu\bar{Q}_{ij}^*$, it is conceivable that it could shrink despite the rise in $\sigma^2\Delta\overline{Q}_{ij}^*$ if the predominant direction of movement in the $\overline{Q}_{ij}^*$'s was downward; i.e., if the relative importance of the error component in observed quantities secularly increased. In the data discussed in this paper, most of the $\overline{Q}_{ij}$'s increased markedly. This does not prove that the $\overline{Q}_{ij}^*$'s (not observable) also increased, but for the sake of the argument we shall assume that they do. (A complete treatment must consider other possibilities.) Therefore, given our assumptions, the denominator of the correlation coefficient grows with time, but the numerator remains unchanged, so that the quotient eventually approaches zero.

To reach this conclusion—that the correlation between $\Delta\overline{w}_{ij}$ and $\Delta\overline{Q}_{ij}$ diminishes (algebraically) over time to approach zero asymptotically—in the long run—requires an hypothesis about how the $\Delta\overline{Q}_{ij}$'s behave in the long run. The hypothesis is that the $\overline{Q}_{ij}^*$'s, predominantly, have long-run monotonic trends and that their variance increases with the length of the interval considered. This hypothesis would be satisfied, for example, if each $\overline{Q}_{ij}^*$ increased at a constant percentage rate and the variance at any given moment was non-zero.

This hypothesis implies that $\sigma_{\Delta\overline{Q}_{ij}^*}$ will increase with the passage

of time and that, provided the covariance of the disturbances is not "perversely" correlated with time, the correlation between $\Delta\overline{w}_{ij}$ and $\Delta\overline{Q}_{ij}$ will eventually approach zero. How much time must elapse before we have any specified degree of approximation to a long run depends upon the divergence in the rates of trend movement among the $\overline{Q}_{ij}$'s; the greater the divergence, the less time required. If there is no predominant tendency toward monotonic trends among the Q_{ij}*'s, then the long-run forces may never dominate the behavior of $E(\Delta\overline{w}_{ij}\,\Delta\overline{Q}_{ij})$.

R. H. Strotz has suggested a very useful simile to explain this point: if a pair of rifle targets is far apart, the difference in the locus of observed shots may be explained by the difference in aim of the rifleman. But if they are close together, the error component may make it impossible to infer at which target a given shot was aimed, and force us to ignore the difference in targets as an explanatory variable.

Now let us suppose that the correlation coefficient between $\Delta\overline{w}_{ij}$ and $\Delta\overline{Q}_{ij}$ shows no signs of approaching zero with an increase in the length of the period, casting doubt on the competitive hypothesis. How should this eventuality be interpreted? It would *not* mean that we should reject price theory as invalid; rather we should say that the assumptions of price theory were violated in such a way as to make the competitive hypothesis inapplicable to the data in question. To say this would not, in any sense, "save" the hypothesis; a hypothesis is valued according to its fruitfulness, and one that is frequently found to be inapplicable is inferior to another which is inapplicable less frequently.

If the competitive hypothesis is found inapplicable, there is a variety of possible alternatives:

1. It is frequently argued that the absence of pure competition invalidates the competitive hypothesis. Surely it is possible that there is correlation—in either direction—between ΔQ_{ij} and $\Delta\epsilon_{ij}$ because of a correlation of ΔQ_{ij} with changes in the parameters of (1). The competitive hypothesis asserts that these parameters reflect only tastes, techniques, and resources;[125] but if the hypothesis is found inapplicable, one reason might be that the parameters also reflect, *inter alia*, the ratio of the value of a factor's marginal product to its rate of reward.

[125] Put in a slightly different way, the competitive hypothesis asserts that the Δw_j*'s reflect only changes in parameters reflecting tastes, techniques, and resources, and from no other forces. Among the forces thus excluded are changing union wage policies which might alter the supply functions of given grades of labor differently in different industries.

It should be noted that the competitive hypothesis can be applied successfully, even though there is monopoly in one or more markets. It suffices for the argument of this paper that there should be no *changes* in the degree of monopoly power;[126] i.e., for the present purpose, the competitive hypothesis does not say that observed prices are approximately those that would obtain under pure competition,[127] but only that *changes* in these prices are the same as those that would have occurred if the degree of monopoly power in every market had been (approximately) constant. That is to say, in terms of equation (1), there are parameters which reflect changes in the degrees of monopoly power, but for the period in question they are approximately constant. Indeed, for the purpose of this paper, it is not necessary that there should be *no* changes in the degrees of monopoly power, but only that these changes should be uncorrelated with ΔQ_{ij} (or other variables whose covariance with Δw_j is being studied). On this last interpretation of the competitive hypothesis, monopoly could be an important factor in explaining changes in any individual price, but this would not imply that it would be useful in explaining the relative long-run changes among any "fairly large" group of prices.

2. The test of the competitive hypothesis we have been discussing is not a very powerful instrument for discriminating between monopolistic and nonmonopolistic situations because—at the level of industry data[128]—the price-quantity behavior which is characteristic of a changing degree of monopoly power is often consistent with two or more very different interpretations. For example, (1) if industries have quantity responses to price changes that involve interindustrial differences in the "true lags" of price behind quantity (or vice versa) and (2) if these lags are subject to change, then the recorded concurrent association of prices and quantities will be identical with what it would have been had there been a changing pattern of monopoly behavior.[129] To say the same thing in a different way: if one is given a set of prices and quantities, it is always possible—in the absence of knowledge about costs or imagined demand elasticities or both—to invent a set of imagined demand elasticities which vary (if necessary) from one

[126] The degree of monopoly power of any seller or buyer is the ratio of price to marginal revenue or marginal expenditure, as the case may be.

[127] This remark applies only to the particular aspect of the competitive hypothesis discussed here. In other contexts—e.g., discussions of relative economic efficiency—the competitive hypothesis implies that observed prices actually approximate competitive ones.

[128] As distinguished from the data for individual firms.

[129] That is, a changing degree of monopoly power, as defined in footnote 126.

period to the next that will "explain" the price-quantity relations perfectly and without reference to any lags.

If firms do not consistently attempt to maximize profits, then variations in the strength of their "propensity to maximize" also could simulate the effects of a changing degree of monopoly power. Yet another possibility arises where there are technological lags of output behind inputs; variations of these lags (either because of changes in technology or in interest rates) will simulate the effect of a changing degree of monopoly power on concurrent price-quantity data.

All of these possibilities are known to have materialized in varying (and disputed) degrees. To disentangle their separate effects in the event the competitive hypothesis is rejected is often very difficult and, in this appendix, we shall make no attempt to discuss the problems that arise.

At this point, it would be well to emphasize that in this discussion of the competitive hypothesis we are considering only those aspects that are relevant to explaining the behavior of a set of industrial wage rates and employment quantities, or, more generally, industry prices and quantities. As such, it is a useful test to apply to historical records of wages, prices, and related quantities of the sort that can be obtained from censuses and which have recently been extensively utilized by by Kendrick and others. Its utility lies mainly in the fact that it requires no data beyond series of prices (factor or product) and quantities—data that are comparatively easy to obtain. But it is a test that is biased in favor of accepting the hypothesis.

Consider: we have, in effect, proposed accepting the competitive hypothesis if the correlation coefficient between $\Delta\bar{w}_{ij}$ and $\Delta\bar{Q}_{ij}$ does not differ significantly from zero. The reason is that, if the coefficient were zero, $E(\Delta\bar{Q}_{ij}^{*}\ \Delta\bar{\epsilon}_{ij}) = E(\Delta\bar{\epsilon}_{ij}\ \Delta\bar{\zeta}_{ij}) = 0$. But $(\Delta\bar{\epsilon}_{ij}\ \Delta\bar{\zeta}_{ij})$ reflects the effect of all forces acting on industry wages and employment except those implied in the competitive hypothesis. Suppose that, contrary to the hypothesis, $E(\Delta\bar{Q}_{ij}^{*}\ \Delta\bar{\epsilon}_{ij}) \neq 0$, but is small relative to the variance of $E(\Delta\bar{\epsilon}_{ij}\ \Delta\bar{\zeta}_{ij})$, whose mathematical expectation is zero; in this event, we might well accept the hypothesis despite its falsehood. To put the matter in another way: what the hypothesis asserts is also what the absence of correlation between the disturbance terms implies; therefore, increases in the variance of the covariance of the disturbances increases the probability of accepting the hypothesis even though every term in (4) is unchanged.

Nevertheless, when we have only industry averages for wages (prices) and quantities at two dates, this test is the only one available. As the number of dates for which data are available increases, at least one other test becomes possible. Because $E(\Delta\bar{\epsilon}_{ij}\,\Delta\bar{\zeta}_{ij}) > 0$, we should find the correlation between $\Delta\overline{w}_{ij}$ and $\Delta\overline{Q}_{ij}$ tending to be positive, but (as we have seen) steadily diminishing as the period considered becomes longer. This test is not subject to the aforementioned bias. Further tests are also possible, but they require additional data. Some of them have been discussed in the text above. However, these tests usually relate relative industry wage levels, or changes therein, to the skill-mix or other aspects of the composition of the industries' labor forces and do not directly involve the methodological issues raised in this appendix. There are still other implications of the competitive hypothesis, and other types of tests; it is not suggested that these are less important than those discussed here.

3. If, on any particular set of data, the competitive hypothesis is rejected, it is sometimes plausible to suppose that the dates to which the data refer are insufficiently separated to constitute a long run; i.e., the hypothesis may be valid for the long run, but the particular period studied should not be considered as such. But if such an approach is not to render the hypothesis inaccessible to empirical confrontation, it is necessary to specify how long a period must be before it is considered a long run.

A rough and ready answer to this question is as follows: a time period is a long run, for the purpose of a particular hypothesis, if extending it further without limit or reducing it slightly[130] does not alter the outcome of any relevant test. In the context of this paper, this criterion means that we have reached a long-run situation when the series of correlation coefficients between $\Delta\overline{w}_{ij}$ and $\Delta\overline{Q}_{ij}$ generated by extending the length of the period from (0) to (1), by increasing (1) indefinitely or by reducing it slightly or both, becomes indistinguishable from a random sample from a normal universe with a zero mean.[131]

[130] Put differently, a long-run relation is one that holds irrespective of the time interval chosen, provided the interval is picked from an admissible set of intervals. Given (0), defining a long-run is akin to choosing the lower bound of the set of admissible dates for (1)—a set for which there is no upper bound.

[131] If the series of coefficients "stabilizes" around a non-zero value, this would suggest that a long run has been reached and the competitive hypothesis disconfirmed. However, the possibility of a spurious long run should never be neglected.

The importance of distinguishing between short- and long-run relationships arises from the fact that in the short run, *ceteris paribus*, there is likely to be a correlation between Δw_{ij} and ΔQ_{ij}. That is, when (0) and (1) are close together, industries for which ΔQ_{ij} is relatively large will almost certainly be those which are growing relatively rapidly at (1). And, because of short-run inelasticity of labor supply to individual industries, the wage rate in the i th industry will tend to rise more, the greater the increase in employment; this induces positive correlation between $\Delta \epsilon_{ij}$ and ΔQ_{ij}. Also, rapidly growing industries tend to experience (temporary) high profits, bursts of growth in productivity, capital stock, etc. However, as (0) and (1) become further removed, the relative size of ΔQ_{ij} becomes decreasingly correlated with its relative growth rate at (1); i.e., industries that experienced great relative growth between 1899 and 1953 are not, to any appreciable degree, those that were experiencing great relative growth in 1953. Moreover, the correlation between $\Delta \epsilon_{ij}$ and the growth rate of Q_{ij} at (1) tends to disappear as we lengthen the period over which differential rates of employment growth are compared; this is because the main reason for short-run inelasticity of factor supply to an industry is failure to anticipate demand accurately. And if an industry's employment has been growing at a more or less steady rate for a fairly prolonged period, both sides of the market will have time to adjust, thereby eliminating the correlation; i.e., $E(\Delta \epsilon_{ij}\, \Delta \zeta_{ij})$ tends to zero.

In short, lengthening the period between (0) and (1) tends to reduce the correlation between $\Delta \epsilon_{ij}$ and ΔQ_{ij} until for some length—and all longer ones—it becomes approximately zero. How long such a period may be, or if it is finite, can be determined only empirically, and ad hoc. As we have seen, the length of the period will depend upon the variance among the trends in Q_{ij}* as compared with the intercorrelations of $\Delta \epsilon_{ij}$ and $\Delta \zeta_{ij}$. The appearance of a set of (small) positive correlations[132] may well provoke a suspicion that we have been studying a period shorter than that necessary to apply long-run theory. However, it must be remembered that there is no guarantee that lengthening the period, even indefinitely, will eliminate the correlation; i.e., the competitive hypothesis may be inapplicable even in the long run.

[132] Generated by varying the length of the period (0)—(1).

The reader will note that in discussing the competitive hypothesis, long-run version, we have hardly mentioned *ceteris paribus*. This is deliberate; as we interpret it, the competitive hypothesis does not assert that certain relations hold, *ceteris paribus*; this is what economic theory asserts. The competitive hypothesis says that other things *are* sufficiently equal for the implications of competitive price theory to apply. If these implications do not hold, then there is need for an alternative hypothesis—possibly including the competitive hypothesis as a special case.

So far, we have been considering only the long-run version of the competitive hypothesis. The short-run version of the competitive hypothesis is in a somewhat different position (from the long run). This is because the short-run version is, as we interpret it, supposed to apply to any and all periods.[133] Therefore, because there is a number of variables associated (for short periods) with relative increase in an industry's wages, the relationship implied by the competitive hypothesis[134] is not the only one that will be found. Hence, the competitive hypothesis may be interpreted in (at least) two ways: (1) In the strong form, it states—parallel to the long-period version—that the relationship it implies is the only one that will be found consistently.[135] The repetitive finding of any other relation is inconsistent with the theory. (2) In its weak form the competitive hypothesis does not state that there is a simple positive correlation between Δw_{ij} and ΔQ_{ij} but merely that this relationship exists *ceteris paribus*; i.e., appropriate partial correlations will have positive signs. Intermediate versions of the hypothesis—e.g., that the *ceteris paribus* relation between Δw_{ij} and ΔQ_{ij} is stronger than that between Δw_{ij} and any other variable—can also be formulated.[136]

So far all our remarks in this appendix have referred to one specific kind of labor employed in a number of different industries and whose wage (in each industry) was recorded.[137] In practice, much of the

[133] It should be emphasized that this is one writer's conception. Other economists may—and some surely will—define terms differently.

[134] That is, that supply price of a factor to an industry, occupation, etc. is a nondecreasing function of quantity; and for quantities "large enough" it is a monatomically increasing function.

[135] That is, evidence of any other relationship will be explicable as a result of random disturbances or sampling fluctuations.

[136] However, it would seem reasonable to insist that the short-run competitive hypothesis be stated so as to imply that the partial correlation coefficients (with all other relevant variables constant) between Δw_{ij} and ΔQ_{ij} are positive.

[137] A further implicit restriction is that all employment occurred in one location

data with which we deal are industry averages of wages in various occupations or of individuals working in different places. Consequently, as the text indicates, much of the explanation of changes in relative industrial wages runs in terms of changes in the composition of the items affecting these averages. The argument of this appendix does not apply directly to these averages, but only to their components and the relationships of substitution and complementarity among them. Where the competitive hypothesis is found to hold for industrial averages, it implies either that the relative changes among the components are unimportant or that they cancel one another.[138]

COMMENT

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I should like to direct most of my remarks to Melvin Reder's treatment of interindustry wage differentials, for I find his defense of the competitive hypothesis less persuasive in that area than in the case of the

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or some unvarying set of locations, thus eliminating interlocational variations in excess demand functions.

[138] It has been argued (privately) by H. G. Lewis that there is a priori reason to suspect long-run correlation between Δw_i^* and ΔQ_{ij}^*. As I interpret the argument, such correlation might arise in several ways, one being the following. Suppose some industries require two or more kinds of labor. Also suppose, for simplicity, that the long-run supply curve of each kind of labor is an increasing function of its price, and that some or all of the industries are sufficiently important as users of some (or all) of the kinds of labor they hire for the long-run equilibrium price of any one kind (which is the same for all industries) to increase with the amount the industry hires. Then, *ceteris paribus*, industries growing relatively more (or declining less) than others will tend to create higher relative prices for the kinds of labor they use most intensively, and hence in their average wage rates. The pressure to substitute against the kinds of labor that have become relatively more expensive mitigates, but does not eliminate, this tendency. *Ceteris paribus*, this factor would lead to a positive correlation in the relative average wage of an industry and its relative employment. Its practical importance will depend on the size of the industries considered; the smaller the industries, the less the importance.

There are other strong forces relating these two variables, e.g., some industries utilize relatively large quantities of man-hours of dirty, rough work which tends to have a negative income elasticity of supply. Consequently, these industries tend to face secular increases in relative (average) supply price of the labor employed per unit of output, which tends (*ceteris paribus*) both to raise relative product prices and therefore to reduce employment, and to increase average wages; this introduces a correlation opposite in sign to that discussed above. The net correlation between employment and wages, even in the long run, will obviously depend upon the relative strength of these and possibly other linkages between the variables. In the text we assumed, *faute de mieux*, that this net correlation was zero. However, this is an assumption that may prove misleading.

skill margin. Orthodox theory, as he points out, would lead one to predict that occupational differentials will be narrowed as educational opportunity is equalized; this narrowing has certainly occurred, and to a very marked degree, in most Western countries; and since he has shown that the spasmodic nature of this secular trend can also be explained in competitive terms, I agree that there is no reason to search for an alternative theory of occupational differentials. When Reder turns to a consideration of interindustry differentials, however, he does not offer such decisive evidence in support of the competitive hypothesis; in fact, the available data suggest that these differentials have not narrowed much at all. Much of his analysis therefore turns upon the question of whether anyone has come up with a better explanation of this wage structure than that offered by the competitive model, and while he inflicts some mortal wounds upon some of the alternatives which have been put forth, that in itself is not a positive confirmation of his model.

We now have a great deal of information on three aspects of interindustry wage differentials: (1) the structure of these differentials in local labor markets over short periods of time; (2) the dispersion of these differentials on a national level over both the short run and, within manufacturing, the long run; and (3) the relation of short-run changes in the national structure to several other factors, such as productivity and employment changes, concentration, etc. Reder concentrates upon the last two of these aspects and offers several valuable insights to which I shall return. First, however, I should like to consider the problems posed by interindustry differentials in local markets.

The competitive hypothesis, he points out, predicts that "In the long run, under competitive conditions, any industry will pay the same price for a given grade of labor as any other industry hiring in the same location." But if all labor economists agree on anything (which is doubtful), it is that a wide diversity of rates for the same jobs characterizes the average local labor market, and that a uniformity of rates is usually a sure sign that the market in question is not "free" but rather is dominated by either organized labor or organized management.

It is this apparent failure of local rates to equalize which has long been seized upon by critics of competitive theory as their prime exhibit. You will recall that Marshall referred to the "naive" assertions of this nature by Cliff Leslie and other critics of that day, and that he retorted

that differences in money rates (for the same job in the same market) were actually evidence of the effectiveness of competitive forces, since workers of different efficiency must necessarily receive different money wages in a competitive market.[1] As our discussion yesterday showed, this argument continues unabated today. On the one hand, the interfirm differentials examined by Shultz were not widely dispersed and appeared to be closely related to quality differences. On the other hand, it was pointed out that this market more closely approached the competitive "norm" than most others which have been studied, and that the narrowness of these largely intraindustry differentials should not be taken as evidence of a similar narrowness of *inter*industry differentials for the same type of labor.

Since no one has been able to measure labor quality differences with any degree of precision, nearly every set of wage data we use to test the competitive hypothesis is subject to conflicting interpretations on this crucial score. I would agree, however, with the view that many of these local wage differentials for the same jobs are so large—50 to 100 percent—that they cannot be explained away as reflections solely of quality differentials. If you grant this, then haven't you denied the validity of the competitive hypothesis? No, says Reder quite properly, since the hypothesis would predict wage variations arising from disequilibrium situations in the short run. To test this possibility, he then examines the several studies that have compared wage and employment changes over the short run but justifiably concludes that the evidence on this point is as conflicting as the evidence adduced to test alternative models stressing concentration, profits, productivity, and union organizations.

One way of resolving this question might be to apply to the local-market level the type of analysis, comparing wage changes with several variables, which to date has largely been employed at the national, industry-wide level. Unlike the gross industry averages used in most studies, local wage data are often available for identical jobs, are obviously not affected by interindustry differences in location, and afford a good test of whether variations within industries (e.g., between large and small firms) are not more revealing than those between industries.

For example, consider the provocative data which Dunlop cites to

[1] Alfred Marshall, *Principles of Economics*, 8th ed., London, Macmillan, 1936, pp. 548-549.

illustrate his concept of wage contours: in Boston in July 1953, the union rate for truck drivers ranged among twenty-two industries from $1.27 to $2.494.[2] Dunlop argues persuasively that this great disparity in the rates paid for the same job in the same market, particularly when organized by the same union, cannot be satisfactorily explained either by orthodox theory or by models which emphasize market frictions or union power. Similar data can be found in other local market studies and in various BLS publications, and these data in some ways pose the issues involved in interindustry wage differentials more sharply than the national, all-worker industry averages. Unfortunately, perhaps for lack of data on the nonwage variables, there has been little if any attempt made to analyze these local wage data with the rigorous, quantitative methods employed in most of the interindustry wage studies.

Also, in keeping with the make-work-for-authors spirit of this conference, I would suggest that the local market has advantages as the setting for the long-run test that Reder correctly points out is the most fitting for his purposes: the determination of whether the differentials existing between the high- and low-wage industries in a given base year narrow over the long run. I don't know if any of the Census or Labor Department occupational and industry data from the turn of the century could be followed through for a single city, but this procedure, if at all feasible, would naturally avoid the problems posed by long-run shifts in skill mix and location.

Difficult as such a test would be, I nevertheless suggest that something more persuasive is needed as a proof of the long-run validity of the competitive model than the evidence Reder can now offer. As he notes, we mildly disagree about the interpretation of my own findings on this point. In analyzing annual earnings data for workers in 84 manufacturing industries over the 1899-1950 period, I found that the percentage differential between the industries which were at the top and the bottom of the structure in 1899 narrowed only from 162 per cent in 1899 to 149 per cent in 1950. While it is true that this slight narrowing "tends to support" the competitive model, it is hardly overwhelming as evidence of the leveling effects of competition over a fifty-year period, and certainly it is not as convincing as the drop in the skill margin from 200 per cent to 138 per cent over a comparable

[2] John T. Dunlop, "The Task of Contemporary Wage Theory," in *New Concepts in Wage Determination*, Taylor and Pierson, eds., New York, McGraw-Hill, 1957, p. 135.

period—a drop, incidentally, which in itself could have produced a narrowing of interindustry all-worker differentials (assuming little change in skill mix) without narrowing the "real" differentials between wages for comparable jobs.

Of course, all-worker data can greatly overstate interindustry differentials because of the effect of skill mix. One of the few sources of wage data by industry and occupation over time is the Conference Board figures for unskilled rates by industry for 1923-46. My impression is that these data are derived from a sample of highly dubious value, but for what they are worth they show the percentage differential between the unskilled rates in the high- and low-wage industries in 1923 to have been 134 per cent in 1923, 133 per cent in 1939, and 121 per cent in 1946.[3] Perhaps better data of this nature over a longer period would show the substantial narrowing necessary to support Reder's argument, but the available evidence is weak indeed.

When he turns from this "positive" test to considering the alternatives offered by others, however, I feel he makes at least two very significant contributions. The first is his vigorous reminder to fellow economists that there is, after all, a difference between the short and long run, a first principle frequently forgotten by some of us in our absorption with the turbulent labor markets of the 1930's and 1940's. Second, he points out that some of the studies of one aspect of this wage structure—short-run wage changes compared with several variables—proceed quite oblivious to the results of studies of a different aspect, the structure's dispersion over time. If I read him correctly on this point, he is the first to warn that many studies have suggested in effect that wages increase most rapidly *over time* in those industries that tend to be the highest-paying *at any point in time*—which would result in a steadily expanding structure, although all the evidence is against this. Thus, the data seem to indicate that, at least in some recent periods, wages increased most rapidly in industries such as iron and steel, petroleum refining, rubber, and automobiles, which have long been high-wage industries in an absolute sense, and the laggards are such industries as cotton textiles and boots and shoes, which are low-wage industries to begin with.

But might this be simply a short-run phenomenon—that, as Reynolds

[3] Median earnings of the top quarter of the wage structure divided by median earnings of the bottom quarter. For the NICB data, see Sumner H. Slichter, "Notes on the Structure of Wages," *Review of Economics and Statistics*, Feb. 1950, p. 89.

and Taft suggest,[4] most industries pass through a "life cycle" in which they rank at the top of the structure in their growth years and are then supplanted by other, newer industries? This possibility has not been adequately tested, but again the burden of my own findings is to the effect that the high-wage industries of fifty years ago tenaciously maintained their position against most comers.

And here we can return to the local market studies. Imperfect as our evidence now is, most of it is consistent in suggesting that: (1) substantial interindustry wage differentials exist for the same jobs in local markets and tend to persist in the short run, and (2) to a lesser extent, all-worker differentials exist between industries on the national level and these tend to persist even over the long run. If this is true, the explanation can lie in a combination of the competitive hypothesis *and* the findings of local market studies. That is, local differentials arise for a variety of reasons—frictions, monopsony, the entry of new industries, union pressure, management philosophy, etc., and once established, these differentials tend to persist, partly for "noneconomic" reasons (for instance, the practical difficulty of abolishing differentials accepted as customary) and partly for the "economic" reason that, as Reynolds and Taft put it, "the wage structure never gets a chance to approach static equilibrium."[5] If the poor do not get poorer, that is, the rich certainly get richer; they do not sit by idly until the low-wage industries catch up with them, and consequently the state of the arts is not frozen for any substantial length of time.

For example, Reynolds and Taft suggest that the persistence of regional differentials over time is the result not only of the growth of labor supply in the South but also of the continuing industrial expansion in the North and West, which has served to offset the "eroding effect of labor and capital migration."[6] Reder suggests that the failure of agricultural wages to catch up with manufacturing wages is also the result of a continuing disequilibrium over the long run. Can this not also be true of many other interindustry differentials? If it is true, however, it is difficult to isolate statistically the effects of the competitive hypothesis, and it also loses much of its predictive value at this particular level.

[4] Lloyd G. Reynolds and Cynthia H. Taft, *The Evolution of Wage Structure*, Yale University Press, 1956, pp. 356-357.
[5] Reynolds and Taft, *Evolution*, 369.
[6] *Ibid.*, p. 370.

To sum up: With respect to interindustry differentials, the competitive hypothesis has not been validated in the short run—nor, as Reder shows so well, has it been conclusively disproved. However, neither has it yet been proved or disproved for the long run.

Finally, two footnotes. First, it is to be devoutly hoped that we can soon incorporate fringe benefits in interindustry wage studies, for I share the suspicion that these have served to widen this type of differential during the very period when the structure of *rates* alone appears to have narrowed (and, of course, during the period when unions have greatly increased in strength).

Second, I hope someone will tackle the problem of our overreliance upon manufacturing data in these studies. Perhaps this is not a serious problem, since manufacturing industries certainly do vary greatly among themselves in certain respects. Yet, in view of the decreasing relative importance of manufacturing, I am uneasy over the studies in which the wage patterns among the 30 or 40 or 80 manufacturing industries for which we can get detailed data overpower the wage movements among a few nonmanufacturing industries. In spite of the hazards obviously involved, the relation of all-manufacturing wages to wages in agriculture, construction, mining, and particularly the service industries deserves more attention than it has received.