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Chapter Author: Milton Friedman

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CHAPTER IV

Consistency of the Permanent Income Hypothesis with Existing Evidence on the Relation between Consumption and Income: Budget Studies

ONE of the aims of this and the next chapter is to document the consistency of our hypothesis with a number of broad empirical findings that strikingly contradict the impression that consumption is a stable function of absolute real income and that this function can be identified with the regression of consumption on income computed from either budget studies or time series. These findings, cited at the outset, are: (a) the rough constancy of the average propensity to consume in the United States over the past half-century, as measured by time series data, despite a substantial rise in real income; (b) the rough similarity of the average propensity to consume in budget studies for widely separated dates, despite substantial differences in average real income; (c) the sharply lower savings ratio in the United States in the period after World War II than would have been consistent with the relation between income and savings computed from data for the interwar period. Another finding of the same kind is (d) the apparent decline over time in the inequality of income despite the possibility of interpreting the consumption-income relation from time series or budget data as showing that the rich are getting richer and the poor, poorer.

But consistency with such broad findings would by itself not be much of a recommendation. Accordingly, these chapters examine the consistency of the permanent income hypothesis also with more detailed evidence on consumption behavior. This chapter compares it with evidence from budget studies; the next, with evidence from time series. Although the empirical evidence examined is by no means exhaustive, it covers a fairly wide span of time and a fairly broad range of phenomena. Its chief defect is that so much of the evidence is for the United States. This is partly because more empirical work has been done for the United States, particularly in recent years; partly because my knowledge of the work that has been done in other countries is more limited, and I have relied mainly on material that was fairly readily available.

CONSISTENCY WITH BUDGET STUDIES

As noted in the preceding chapter, the permanent income hypothesis accounts for the broader features common to observed regressions of measured consumption expenditures on measured income computed from budget data: the tendency for expenditures to exceed income at low incomes, and to fall short of income above some point; the uniform tendency for the ratio of consumption to income to be lower, the higher the income. We turn now to consider the consistency of the hypothesis with (1) temporal changes in the inequality of income; (2) differences among regressions for (a) widely spaced dates, (b) different countries, (c) farm and nonfarm families, (d) different occupational groups and (e) Negro and white families; (3) the relation between savings and age; and (4) the effect of changes in income on the relation of measured consumption to measured income.

1. *Temporal Changes in Inequality of Income*

A tendency for the inequality of income to increase over time has frequently been inferred from the regressions of consumption on income computed from budget data. These regressions show savings to be negative at low measured income levels, and to be a successively larger fraction of income, the higher the measured income. If low measured income is identified with "poor" and high measured income with "rich," it follows that the "poor" are getting poorer and the "rich" are getting richer.

The identification of low measured income with "poor" and high measured income with "rich" is justified only if measured income can be regarded as an estimate of expected income over a lifetime or a large fraction thereof. One step in this direction is taken when the computed regressions are regarded as estimates of a stable function relating consumption to income, since, in our terminology, this is equivalent to regarding them as estimates of the relation between the permanent components of consumption and income. It requires only the additional step of giving "permanent" the special meaning of expected income over a large fraction of a lifetime to make increasing inequality of income a valid inference from the observed regressions.

On the permanent income hypothesis, the observed regressions give no evidence on the secular behavior of the inequality of income. Negative savings at low measured incomes reflect precisely the fact that measured income is not a valid index of wealth; that many people have low incomes in any one year because of transitory factors and can be expected to have higher incomes in other years. Their negative savings are financed by large positive savings in

years when their incomes are abnormally large, and it is these that produce the high ratios of savings to measured income at the upper end of the measured income scale. The existence of large negative savings is a symptom that the observed inequality of measured income overstates substantially the inequality of permanent income. It is not a harbinger of a widening gap between rich and poor.

Empirical data show no tendency for inequality of income to increase. If anything, inequality seems to have been decreasing in recent decades.[1] This fact has been widely recognized, and so has the possibility of reconciling it with the observed regressions of consumption on income by considerations very similar to those embodied in our hypothesis. But this has not prevented these regressions from being treated as describing a relation between permanent components in other contexts where the contradiction with available evidence is less obvious.

2. *Consumption-Income Regressions for Different Dates and Groups*

Table 1 summarizes some of the salient findings of budget studies for a wide range of dates and groups of consumer units. The studies summarized in this table are a major source of the evidence examined in this section.

For the United States, the average propensity to consume is remarkably similar for different studies covering either nonfarm groups alone or all consumers (Table 1). Eight of nine values are between .89 and .92, and these are for years as much as six decades apart; the one deviant is for 1944, and reflects the extraordinarily high level of savings during World War II. Time series data show both the same similarity of the average propensity for different periods of time and an average propensity very nearly the same in magnitude (see Table 12 in the next chapter). On a definition of consumption like that used in these budget studies—one which treats as consumption, expenditures on consumer durable goods other than housing—the average propensity for the period 1897 through 1949 computed from estimates by Raymond Goldsmith is .892. It would be preferable for our purposes to use a definition of consumption that included only the use value of durable goods as consumption and treated increases in the stocks of consumer durable goods as increases in wealth. According to Goldsmith's

[1] See Simon Kuznets (assisted by Elizabeth Jenks), *Shares of Upper Income Groups in Income and Savings*, (New York: National Bureau of Economic Research, 1953); Selma Goldsmith, George Jaszi, Hyman Kaitz, and Maurice Liebenberg, "Size Distribution of Income since the Mid-Thirties," *The Review of Economics and Statistics*, XXXVI, No. 1 (February 1954), pp. 1–32.

CONSISTENCY WITH BUDGET STUDIES

TABLE 1

Relation between Consumption and Income Based on Budget Data, for Different Countries, Dates, and Groups of Consumer Units

	Date	Consumer Units	Average Income	Average Propensity to Consume	Marginal Propensity to Consume	Income Elasticity of Consumption
		United States (*income given in dollars*)				
1.	1888–90	Selected wage-earner families	682	.90	.67	.74
2.	1901	Selected wage-earner normal families	651	.92	.68	.75
3.	1917–19	Selected wage-earner families	1,513	.91	.78	.86
4.	1935–36	Nonrelief nonfarm families	1,952	.89	.73	.82
5.	1941	Urban families	2,865	.92	.79	.87
6.	1944	Urban families	3,411[a]	.82	.57	.70
7.	1947	Urban families	3,323[a]	.92	.78	.85
8.	1950	Nonfarm families	4,084[a,b]	.91	.73	.80
9.	1950	Spending units of one or more persons, urban plus rural	3,220[a,b]	.92	.75	.82
10.	1935–36	Nonrelief farm families	1,259	.87	.57	.65
11.	1941	Farm families	1,680	.83	.57	.69
		Great Britain (*income given in pounds sterling*)				
12.	1938–39	Middle-class families with one earner				.89
13.	1951–52	Income units of one or more persons, urban plus rural	369	.99	.86	.87
		Sweden (*income given in Swedish kronor*)				
14.	1913	Industrial workers and low-grade employees	744	.99	.90	.91
15.	1923	Industrial workers and low-grade employees	1,232	1.00	.96	.96
16.	1923	Middle-class families	2,692	1.00	.92	.92
17.	1933	Industrial workers and low-grade employees	1,236	.98	.94	.95
18.	1933	Middle-class families	2,341	.96	.88	.91
19.	1933	Small farmers	577	.95		
20.	1933	Farm and forestry workers	504	.99		

(cont. on next page)

TABLE 1 (cont.)

[a] After personal taxes.

[b] Difference reflects not only inclusion or exclusion of farm families and of single individuals but also difference in definition of consumer unit. Line 8 is for families defined by relationship, line 9, by pooling of income. The latter, "spending units" in the terminology of the Survey of Consumer Finances, are more numerous than the former.

General Notes: 1. Concepts of consumption and income vary among studies. Consumption uniformly includes expenditure on durable consumer goods other than owned homes. For United States data, consumption includes gifts, contributions, and personal taxes except where noted; it excludes life insurance premiums, which are treated as savings. Income is personal income before personal taxes, except where otherwise noted.

2. Average propensity to consume is ratio of average consumption of group to average income.

3. Income elasticity of consumption is slope of straight line regression of logarithm of consumption on logarithm of income. For most studies, regression was estimated graphically.

4. Marginal propensity to consume is product of average propensity and income elasticity. It is therefore marginal propensity at point corresponding approximately to mean income of group.

5. Definition of family varies among studies. Uniformly refers to group of two or more persons sharing same dwelling unit; beyond this, some studies use criterion of relationship, others, of pooling of income.

Source:

United States: Lines 1, 2, 3, 6, 7

Income and consumption by income classes used in computing elasticity, from Dorothy S. Brady, "Family Saving 1888–1950," Part II of Raymond W. Goldsmith, Dorothy S. Brady, and Horst Mendershausen, *A Study of Saving in the United States, III*, (Princeton University Press, 1956), pp. 182–183; average income and average expenditure for all income classes furnished by Brady, based on same data.

Lines 4, 5, 10, 11

See notes to Table 3 below. Income and consumption include nonmoney items, except that elasticities for lines 4 and 5 were computed from data by income classes excluding nonmoney items.

Line 8

Data provided by Division of Research, Board of Governors of the Federal Reserve System, based on the 1951 Survey of Consumer Finances. "Family" is defined by relationship, in contrast to line 9, where unit is "spending unit," as defined in Survey of Consumer Finances.

Line 9

H. F. Lydall, "National Survey of Personal Incomes and Savings: Part IV," *Bulletin of the Oxford University Institute of Statistics*, XV (October and November, 1953), p. 388, gives basic data by income classes, derived from Supplementary Table 8, "1952 Survey of Consumer Finances, Part III," *Federal Reserve Bulletin*, September 1952; Table 1, "1951 Survey of Consumer Finances, Part III," *ibid.*, August 1951; and Appendix Table 1, "1951 Survey of Consumer Finances, Part IV," *ibid.*, September 1951. Unit is "spending unit." Classification of units is by income before taxes; elasticity is computed from relation between consumption and income after taxes for such gross income classes; comparison of this result for corresponding British figures with result obtained from relation between consumption and income for net income classes indicates that use of gross income classes yields same result as use of net, presumably because of extremely high correlation between tax (as computed by surveys) and gross income.

Great Britain: Line 12

H. S. Houthakker, "The Econometrics of Family Budgets," *Journal of the Royal Statistical Society*, Series A (General) CXV, Part I, 1952, p. 20.

(cont. on next page)

TABLE 1 (cont.)

Line 13

Lydall gives mean income and expenditure, and mean income and savings for net income classes. The elasticity cited is computed from slope of graphically fitted log-log straight line.

Sweden:

Average income and average propensity for all studies, Herman Wold in association with Lars Jureen, *Demand Analysis* (Almquist and Wiksell; Stockholm, 1952), p. 20; elasticities, *ibid.*, p. 226. The elasticities cited are those obtained by "direct pooling."

figures, the average propensity computed on this definition is only moderately lower than those so far cited; for 1897 through 1949 it is .877 rather than .892.[2]

The average propensities are much higher for the British and Swedish studies than for the American studies, and very much the same for those two countries; for Sweden, the average propensities are much the same for different dates for comparable groups. Since these studies cover only personal consumption and savings, this difference between the United States and the other countries—even if statistically reliable—does not necessarily mean a corresponding difference in the fraction of aggregate income devoted to savings or capital formation. In all countries, capital formation is financed not only by personal savings but also by business and governmental saving, and the importance of the different sources of savings doubtless varies from country to country.

The marginal propensities vary somewhat more widely than the average propensities and so do the income elasticities of consumption, which are the ratios of the marginal propensities to the average propensities. The major differences are, however, between the farm and the other regressions and between countries, rather than among dates. For the United States, if we exclude the two farm regressions, eight of nine marginal propensities are between .67 and .79, eight of nine elasticities, between .74 and .87. The deviant is again 1944, for which both the marginal propensity and elasticity are decidedly lower than for the other studies. These differences are by no means negligible and we shall examine below (in section a) why they should have occurred. But they are small enough to justify regarding a marginal propensity of about .75 and an income elasticity of about .83 as reasonably typical values for broad groups of nonfarm families in the United States. For both Britain and Sweden, the corresponding values are systematically higher (section b). For farm families in the United States, the corresponding values are systematically lower (section c);

[2] See Table 12 below for sources of time series estimates.

unfortunately, no farm-nonfarm comparisons are available for the other countries.

a. TEMPORAL DIFFERENCES

For every regression in Table 1, the marginal propensity is less than the average propensity so that the elasticity of consumption with respect to measured income is uniformly less than unity. It is this feature, of course, that makes it impossible to regard these regressions as estimates of a stable relation between consumption and income. An income elasticity of less than unity implies that a rise in income produces a decline in the ratio of consumption to income; yet the average propensities are the same over the six decades spanned by the table despite a sextupling of average income. This stability in average propensities is therefore inconsistent with stability in the relations themselves.

For a more detailed examination of these temporal differences, let us restrict our attention to the first eight lines of Table 1—for the United States and for wage-earner or nonfarm or urban families. For these regressions, the marginal propensities and elasticities are quite similar, except for 1944, so the differences among them that produce the common average propensity is primarily in their height. The variation in height is shown by column (4) of Table 2, which gives consumption at a common dollar income of $2,000 as estimated from the regressions for the different studies. Computed consumption is lower for the 1901 study than for the 1888-90 study, along with average income, but then rises steadily—except for 1944—from $1,360 in 1901 to $2,160 in 1950 or from 68 per cent of income to 108 per cent.[3]

Much of this difference in heights of the regressions is accounted for by differences in the price level in the various years and thus reflects simply a difference in the unit of measure. The values of average income shown in Table 1 and repeated in column (2) of Table 2 are in current prices: prices of the date when the study was made. Expressing them in constant prices reduces drastically the differences among them—from a range of 6 to 1 to a range only a little more than 2 to 1 [column (3) of Table 2]. Similarly, expressing the regressions in a common unit—a dollar of 1935–39 purchasing power—reduces drastically the differences in their heights. Computed consumption at an income of $2,000 varies from $1,565 for

[3] No special importance is to be attached to the figure $2,000. It was chosen rather arbitrarily as a convenient figure somewhat in the middle of the span of observed income. Comparison at some such figure is better than comparison of the constant terms in the logarithmic regressions (i.e. logarithm of consumption at an income of $1), because the latter is subject to much greater sampling error.

TABLE 2

Comparison of Heights of Regressions in Current and Constant Prices, Eight Studies from 1888–90 to 1950

	Date	Average Income		Estimated Consumption at Income of $2,000[a]		Ratio of Consumption to Income at Average Income in 1935–39 Prices Computed from Regression for		Income Elasticity of Consumption
		Current Prices	1935–39 Prices	Current Prices	1935–39 Prices	1888–90	1950	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1.	1888–90	$682	$1,236	$1,385	$1,610	.91	1.07	.74
2.	1901	651	1,135	1,360	1,565	.94	1.09	.75
3.	1917–19	1,513	1,402	1,755	1,735	.89	1.04	.86
4.	1935–36[b]	1,952	1,980	1,875	1,880	.82	.98	.82
5.	1941[b]	2,865	2,723	1,945	1,930	.76	.93	.87
6.	1944	3,411	2,714	1,625	1,915	.75	.92	.70
7.	1947	3,323	2,082	2,055	1,920	.80	.97	.85
8.	1950	4,084	2,376	2,160	1,945	.77	.94	.80

[a] Computed from regressions of logarithm of consumption on logarithm of income.
[b] The entries in columns (2) and (3) are for money plus nonmoney income. The remaining entries are based on regressions and averages for money income and consumption only. The effect of excluding nonmoney income is minor.

1901 to $1,945 for 1950, or from 78 per cent of income to 97 per cent.

Though price changes account for the greater part of the differences in the heights of the regressions, sizable differences remain even after the regressions are expressed in the same units, as is shown by the steady rise from 1901 to 1941 in the entries in column (5). It is shown, also, in columns (6) and (7) in a slightly different way, a way that is more closely connected with the observed stability in the average propensity to consume. If the 1888–90 regression had been valid throughout the period and for the different groups, consumption at the arithmetic mean income would have fallen from 94 per cent of income in 1901 to 77 per cent in 1950; if the 1950 regression had been valid, consumption would have fallen from 109 per cent of income in 1901 to 94 per cent in 1950; in fact, of course, the shift in the regressions was enough to keep consumption roughly the same percentage of the mean income.[4]

On our hypothesis, it is no accident that the computed regressions

[4] It will be noted that the ratio of consumption to income at the arithmetic mean income is not equal to the average propensity, which we have defined as the ratio of the arithmetic mean consumption to the arithmetic mean income. The reason is that we are using logarithmic regressions. Arithmetic regressions necessarily pass through the point

shifted by just these amounts; the shifts do not indicate any change in tastes or in the underlying factors affecting consumption; they are a direct result of the change in average real income. The interpretation of the results by our hypothesis is suggested by Figure 4. Unlike the earlier figures, the scales in this one are logarithmic, since the logarithmic variant of our hypothesis conforms better to observed data. The heavy line is an assumed relation between permanent components:

$$\text{Permanent consumption} = .9\ (\text{permanent income})$$

or

$$\log c_p = \log .9 + \log y_p.$$

If mean transitory components of income and consumption were zero for each study separately, and if the value of k stayed the same over time, because changes in the determinants of k had been either negligible or offsetting (see section 2d below and section 1b of Chapter V for further comments on this point), the points defined by mean income and mean consumption would, on our hypothesis, fall on a line such as this. The crosses and solid symbols, which plot these mean points, clearly huddle very closely around the line. The one point that is decidedly off the line is for 1944, a year for which the mean transitory component of income was almost certainly positive and of consumption, negative; and this point is off the line in the direction that would be produced by mean transitory components with these signs.[5]

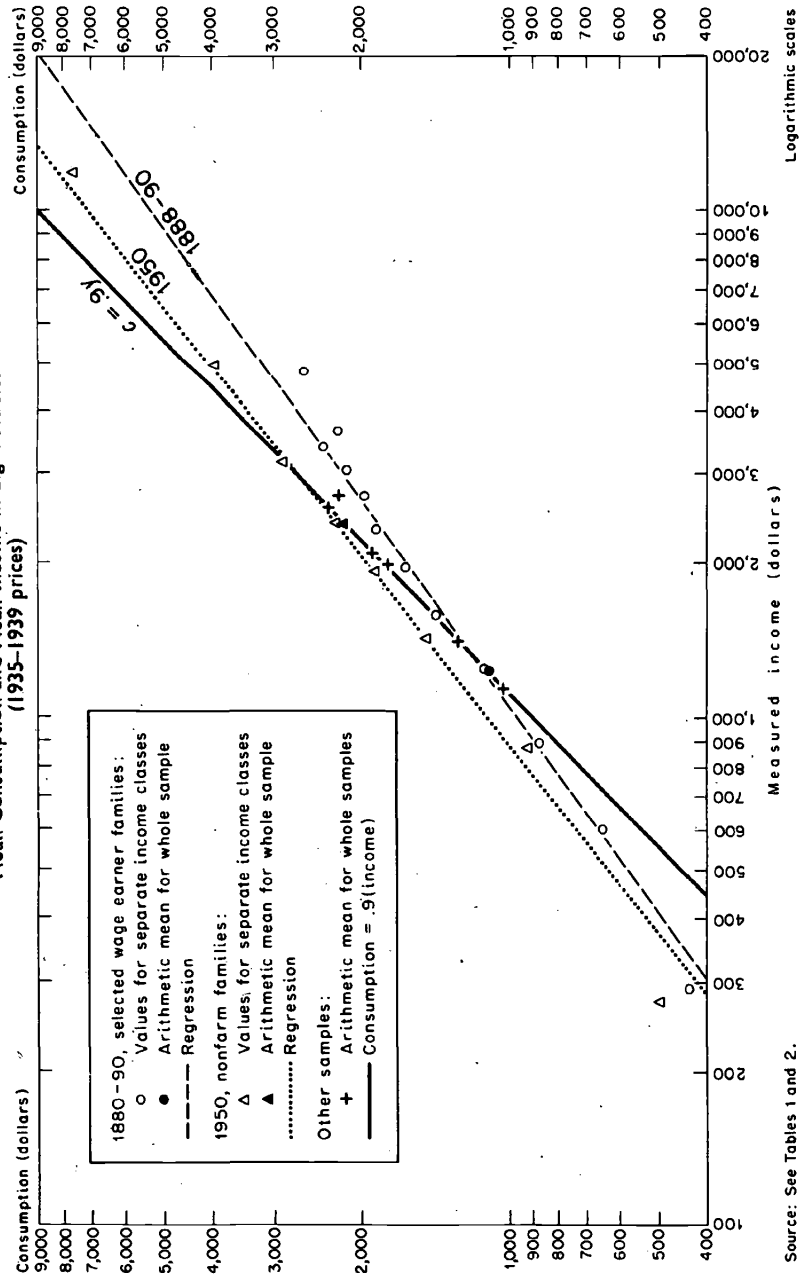
Consider now average consumption for different measured income classes in a single year, such as the points plotted in Figure 4 for 1888–90 and 1950. These cannot be expected to huddle around the heavy line since mean incomes for such classes are not mean permanent incomes; for the low measured income classes, mean measured income is less than mean permanent income but mean

defined by mean consumption and mean income so that the ratio of consumption computed from the regression at the mean income to mean income necessarily equals the average propensity. The logarithmic regression passes through the point defined by the geometric means but not in general through that defined by the arithmetic means, so the equality no longer holds.

[5] One feature of the figure and this interpretation should be noted. The means plotted are arithmetic means, whereas consistency with logarithmic plotting might seem to require geometric means. This is not, however, so; the question is solely what is to be meant by the "averaging out" of transitory components. Suppose the relation $c_p = ky_p$ holds for every consumer unit; then it holds *both* for the arithmetic mean of the *permanent* component and the geometric mean of the *permanent* component. The question then becomes how to estimate the one or the other of these means from data on *measured* income and consumption; and this depends on whether the arithmetic mean of the transitory components themselves or of the logarithmic transitory components can more appropriately be regarded as zero. In the present case, there is little choice; the data are not in a form in which geometric means can be readily computed with any accuracy.

FIGURE 4

Regressions of Consumption on Income, 1888–1890 and 1950, and the Relation between Mean Consumption and Mean Income in Eight Studies (1935–1939 prices)



Source: See Tables 1 and 2.

consumption is equal to mean permanent consumption, so the observed points will be above the heavy line, and conversely for the high measured income classes. The regression of consumption on measured income is flatter than the heavy line and cuts it in the neighborhood of mean measured income.[6] On our hypothesis the slope of these regressions tells us something about the importance of random factors in the distribution of income but nothing about consumption behavior. As mean measured income and consumption rise along the heavy line, they carry this regression along with them.

Points for individual measured income classes are plotted on Figure 4 only for the earliest and latest study in order to keep the figure readable. If the points for the other studies were plotted, they would for the most part fall within the area bounded by the two regressions, though some of course would be outside. The general picture would be the same but blurred by much random variation. For each study the set of points starts for low incomes above the heavy line and ends below it for high incomes, crossing in the neighborhood of the mean measured income; the later the time period, in general, the higher the set of points.

One detailed feature of the 1888–90 and 1950 points that is rather characteristic of these and similar regression data is the deviation of the points for the lowest and highest measured income classes from the pattern of the remaining points: for the lowest income class, consumption is abnormally high, for the highest, abnormally low. This pattern holds for six of the eight regressions covered by Table 2 and Figure 4, for one of the two points in one of the remaining regressions and for neither point in only one.

On our hypothesis, points like those plotted for 1888–90 and 1950 will fall on a straight line if random factors affect all income classes alike in the sense that the fraction of the deviation of the measured income of an income class from the mean measured income of the group as a whole accounted for by the deviation of the transitory component from its mean is the same for all income classes.[7] It should be recalled that a common effect in this sense

[6] Again, attention should be called to the difference between arithmetic and geometric means. The regressions are something of a mixture: the mean expenditures and incomes plotted for the separate income classes are arithmetic means; the regression is fitted to their logarithms and so passes through a point corresponding to the geometric mean of the class means rather than through the plotted arithmetic means. Again it might be—though it is not entirely certain that it would be—preferable to use logarithms and geometric means throughout; however, this is impossible without going back to the original data. The differences are not, however, large, as can be seen from the closeness of the arithmetic mean points to the fitted regressions.

[7] With logarithmic lines, all quantities should be expressed in logarithms.

implies that measured income itself (as contrasted with the deviation of measured income from the mean) understates permanent income by successively larger percentage margins the farther measured income is below the mean, and overstates permanent income by successively larger percentage margins the farther measured income is above the mean. The abnormally high consumption in the lowest income class means that permanent income for this class is even higher than the level consistent with a common effect of random factors. The abnormally low consumption in the highest income class means that permanent income is even lower than the level consistent with a common effect. Thus both deviations from the pattern reflect a disproportionate importance of transitory components of income at the extremes of the permanent income scale.

Two alternative explanations of this phenomenon suggest themselves: first, that it mirrors an important and basic feature of the income structure of the population; second, that it reflects the nonrepresentativeness of the samples or errors in recorded responses rather than a characteristic of the population.[8] While the first explanation can by no means be ruled out, I do not believe that the budget studies themselves can be regarded as providing much evidence for or against it, because the second explanation is so clearly important for such studies that the only question is whether it tells the whole story.[9]

Consider, first, the lowest income class. One source of bias is

[8] For the upper point, some part may also be played by a purely technical explanation which arises from the mixture of arithmetic and geometric operations in the figure and regression. Suppose that all income classes are affected alike by transitory factors when all quantities are expressed in logarithms. The log-log regression will then be a straight line throughout. Now compute the arithmetic mean income and consumption of the units with measured incomes above some value. The point defined by the logarithms of these two quantities will be below, on, or above the log-log straight line according as its slope is less than, equal to, or greater than unity. For the regressions in question, the slope is uniformly less than unity, so that if the relation between the logarithms were strictly linear, a point computed like our final points would fall below the line. This same bias affects all our points, but it is much smaller for the intermediate points than for the end point which is generally for an open-end class covering a rather wide range of incomes.

[9] There is some independent evidence on the first explanation. See in particular, Friedman and Kuznets, *Income from Independent Professional Practice*, pp. 309–319, 325–352; Horst Mendershausen, *Changes in Income Distribution during the Great Depression*, Studies in Income and Wealth, VII (New York: National Bureau of Economic Research, 1946), pp. 101–113; Frank A. Hanna, "The Accounting Period and the Distribution of Income" in Frank A. Hanna, Joseph A. Pechman, Sidney M. Lerner, *Analysis of Wisconsin Income*, Studies in Income and Wealth, IX (New York: National Bureau of Economic Research, 1948), pp. 241–250.

This evidence is mixed: that in the first reference cited is contradictory to the explanation; that in the second reference supports it rather strongly; and that in the third, supports it slightly. A thorough analysis of the scattered evidence now available on this question is much to be desired.

the so-called "eligibility requirements" used in deciding which consumer units to include in the sample. The 1935–36 study is the clearest example. The main body of expenditure data from this study is for families not on relief; relief families were excluded to eliminate the "abnormal" cases left in the wake of the Great Depression. Even with a representative sample, the lowest measured income classes are unduly populated with units that do not "belong" there permanently; this effect is already allowed for in the linear regression. With a sample restricted to nonrelief families, the lowest measured income classes must be almost exclusively populated with units that "belong" higher up the permanent income scale, and it is not surprising that their mean consumption is decidedly higher than the level computed from a regression fitted to the intermediate income classes. The three earlier wage-earner studies—for 1888–90, 1901, and 1917–19—all had eligibility requirements that had similar objectives though they were different in detail, and less clear in effect. It seems very likely that their effect would be much the same, though for the 1917–19 study, consumption for the lowest income class is not out of line with the pattern for the rest of the classes. Perhaps the reason is that the eligibility requirements for this study were more restrictive than for any of the others, going so far as to exclude families with large transitory incomes, so that the bias in this direction offset the bias in the direction of excluding families with unusually low permanent incomes.[10] The later studies—for 1941, 1944, 1947, and 1950—were directed more explicitly at getting a representative sample and hence deliberately avoided imposing eligibility requirements of the kinds used in the earlier studies. Yet even without explicit requirements, much the same result is likely to flow from the problems of field sampling and interviewing. The consumer unit with temporarily low income is more likely to be a temporally stable unit, to be included in the directory used for sampling, and so on, therefore more likely to be included in the sample than the unit with permanently low income. It may also be more willing and able to respond; the units that are abnormal by virtue of very low permanent incomes are likely to be abnormal in other respects.

In the highest income classes, a rather different problem arises. This is the problem of "refusals," "not at homes," and so on; and here my interpretation of the source of bias in the final sample is perhaps more conjectural. For family surveys of the kind under

[10] See *Cost of Living in the United States*, Bureau of Labor Statistics Bulletin 357, (Washington, 1924), or the summary in H. Gregg Lewis and Paul H. Douglas, *Studies in Consumer Expenditures*, (Chicago: University of Chicago Press, 1947), p. 7.

discussion, it is fairly well established that the refusal rate rises with measured income and is particularly high in the high income groups. The crucial question for our purpose is whether the key variable is measured income itself or permanent income so that the refusal rate rises with measured income only because permanent income rises with measured income. The latter is highly plausible, implying, as it does, that a person who this year has a high measured income thanks to good fortune, so that his measured income is substantially higher than his customary income, is more likely to respond than a person who has the same measured income but whose measured income this year is about equal to his customary income. The former tends to live in a different neighborhood, move in a different social circle, and in general to have the habits of his cultural and geographical neighbors rather than of his accidental measured income neighbors. If this interpretation of the differential refusal rate is correct, it means that those units with high measured income who reply have been even more favorably affected by transitory factors than the whole population of that measured income class. In consequence, their consumption tends to be lower than that of the whole class, which would explain why the observed point for the top income class tends to be below the regression fitted to the intermediate points.[11]

The final feature of these data that requires interpretation is the elasticity of consumption with respect to income—the slope of the regression of the logarithm of consumption on the logarithm of income. On our hypothesis, this elasticity measures the fraction

[11] These doubts about the significance of the initial and terminal points partly explain why graphic methods were used so extensively in determining the elasticities presented in the tables. I experimented with fitting least-squares regressions to all the points and to all but the first and last points. In general, my graphic fits seemed to me to be better and to come closer to giving the end points some weight without allowing them to distort the regression. The following comparison for three of the studies indicates the quantitative magnitude of the problem.

	Income Elasticity of Consumption from:		
	Computed Regression		
Study	*Using All Points*	*Excluding First and Last Points*	*Graphically Fitted Regression*
1935–36	.767	.840	.825
1941	.851	.892	.868
1944	.630	.698	.701

My trials and tribulations with these data are an excellent illustration of the dictum attributed to Frederick Macaulay: the best method of curve-fitting is the freehand graphical method; the only difficulty is that it takes too much time.

of the variance of measured income attributable to variation in the permanent component: the higher the elasticity, the smaller the importance of transitory factors relative to permanent factors in producing income differences in the group considered, and conversely. Except for 1944, the earliest study has the lowest elasticity, and the next earliest, the next to the lowest. One interpretation is that this reflects a secular decline in the importance of chance or transitory factors in our economy, and one that proceeded with special rapidity from 1888 to the first World War—a result that is most plausible in view of the changes in the character of the private economy in the period before World War I and in the governmental arrangements for providing security to individuals in the later period. We cannot, however, be sure that the difference between the imputed elasticities may not be more simply explainable by differences in the kinds of groups covered and in the way the data were collected. In both the first two studies, the eligibility requirements almost certainly operated to narrow the range of variation in permanent components of income more than in transitory components. For example, restriction of the samples to wage-earners. to certain industries, and to families of specified demographic characteristics would all have this effect. Some indication of the possible quantitative importance of this factor is available for 1901 from separate tabulations by income classes for "all" families and "normal" families; the latter including families which had "(1) a husband at work; (2) a wife; (3) not more than five children and none over fourteen years of age; (4) no dependent, boarder, lodger, or servant; and (5) expenditures reported for rent, fuel, lighting, food, clothing, and sundries."[12] (1), (2), (3), and (4) might all be expected to reduce the variation in permanent components; only (1), to reduce the variation in transitory components. As our hypothesis would lead us to expect, the elasticity for "normal" families is lower than for "all families": .75 as compared with .81.[13] Unfortunately, however, this difference cannot be regarded as the effect solely of the factors listed; the elementary observations for "all families" are averages for small groups of families, rather than data for individual families; and this, too, would tend to make the elasticity for all families higher than for "normal"

[12] Quoted from Lewis and Douglas, *op. cit.*, p. 6.

[13] The value for normal families (.75) is taken from Table 1. The value for all families (.81) is the slope of a straight line regression of the logarithm of consumption on the logarithm of income estimated graphically from data given by Dorothy S. Brady, in "Family Saving, 1888–1950," Part II of Raymond W. Goldsmith, Dorothy S. Brady, and Horst Mendershausen, *A Study of Saving in the United States*, III (Princeton University Press, 1956), p. 182.

families. The difference between the elasticities for "all" and for "normal" families is almost the same as the difference between the elasticities in Table 1 for the 1888–90 and the 1935–36 studies, and of the same order of magnitude as most of the other differences between elasticities. On the basis of this evidence, therefore, there is little firm basis for supposing any underlying secular change, though what evidence there is argues for a diminution in the relative importance of transitory variation in income.

The extremely low elasticity for 1944 is readily explained on our hypothesis. This was a wartime period involving unprecedented geographical, industrial, and occupational mobility of income recipients. It seems eminently reasonable that transitory components should account for some 30 per cent of the variation in income under such circumstances rather than for the approximately 15 per cent they account for in the preceding and succeeding peacetime years.

This interpretation of the 1944 elasticity makes the 1917–19 elasticity seem decidedly out of line. This, too, was a war period; yet the elasticity is higher than for the other wage-earner studies. It seems likely that the explanation is to be found in the eligibility requirements which were particularly stringent for this study and some of which operated especially to rule out large transitory components; for example, the requirement that the family have kept house in the locality for the entire year covered. The high elasticity for this study may well be simply a resultant of rules that retained a much larger fraction of the variation in the permanent than in the transitory component. One piece of evidence in favor of this explanation is a study not included in Table 1, a study of the money disbursements of wage earners and clerical workers for a year within the period 1934–36 but which varied in exact dating for different groups. This study is very comparable to the 1917–19 study, and used highly similar and equally restrictive eligibility requirements. The elasticity derived from this study is .89,[14] which is decidedly higher than the value of .82 for nonrelief, nonfarm families derived from the contemporaneous 1935–36 study and recorded in Table 1. The 1935–36 study was much broader in scope than the wage-earner study and had less restrictive eligibility

[14] Based on data from Faith M. Williams and Alice C. Hanson, *Money Disbursements of Wage Earners and Clerical Workers, 1934–36, Summary Volume*, Bureau of Labor Statistics Bulletin No. 638 (Washington, 1941), pp. 12 and 22.

This report also contains a special tabulation for a subsample selected so as to match, so far as possible, the 1917–19 sample in cities covered and type of family included (pp. 345–346). This subsample yields the same value of the elasticity (.89) as the entire sample.

requirements. Further, the elasticity for the 1934–36 wage earner study is moderately higher than the elasticity of .86 recorded in Table 1 for the 1917–19 study, so consistent with a reduced elasticity during a wartime period.

The lower elasticity for the 1935–36 study than for the 1941 and 1947 studies, which also cover all occupational groups, is readily explained by the exclusion of relief families and the consequent reduction in the variation attributable to permanent components of income.

This final point may well be gilding the lily. 1888–90 and 1944 aside, the outstanding fact about the remaining elasticities is that they are so similar. Though I have not tried to make a formal test of the proposition, I strongly suspect that the differences are within the range of sampling variation. One must, therefore, beware of trying to explain too much.

It should perhaps be explicitly noted that the constancy of the average propensity to consume over the period, while consistent with our hypothesis, is not required by it. On our hypothesis, this average—for groups and dates for which transitory effects can be regarded as averaging out to zero—is determined by such variables as the rate of interest, the ratio of wealth to income, the degree of uncertainty contemplated, etc., and there is no reason why these need either stay the same or change in such a way as to offset one another. This point is considered in more detail in section 2b below and in section 1b of Chapter V.

b. DIFFERENCES AMONG COUNTRIES

Both the elasticity of consumption with respect to income and the average propensity to consume are higher for the British and Swedish studies in Table 1 than for the United States studies. In addition, the recorded income elasticities are higher for Sweden than for Britain. The evidence on the average propensities is too meager to justify any conclusion about the direction of the difference between Sweden and Great Britain.

On our hypothesis, the differences in income elasticity reflect differences in the relative importance of transitory factors in producing differences in measured income. Taken at their face value, therefore, the elasticities in Table 1 suggest that transitory factors are more important in the United States than in the United Kingdom and more important in the United Kingdom than in Sweden—results that seem not inconsistent with casual observation.

However, the results for Sweden recorded in Table 1 cannot be regarded as strictly comparable with those for the United States

and Great Britain, for two reasons: (1) the groups covered are more narrowly defined, covering consumer units in a particular socio-economic class; (2) the regressions were computed from data adjusted for differences in family size by being converted into consumption and income per equivalent unit, rather than per consumer unit. Point (1) might be expected to make the elasticities less than for broader groups, since restriction to a homogeneous socio-economic class presumably reduces the variance attributable to permanent components without affecting systematically the variance attributable to transitory components. Point (2) might work in either direction, depending on the precise characteristics of the scale chosen. Judging from similar data for the United States, I conjecture that the most likely result is that the number of equivalent units assigned to consumer units of different size varies much more widely than their average incomes. If this is so, conversion to a per equivalent basis would widen the differences among the average incomes of families of different size. Since such differences are to be regarded as differences in permanent components, the final effect would be to increase the variance of permanent components relative to that of transitory components and so to make the elasticities higher than they would otherwise be.[15] If this conjecture is correct, points (1) and (2) affect the recorded elasticities in opposite directions, and there is no way of knowing whether their combined effect is to make the recorded elasticities higher or lower than elasticities directly comparable with the British and United States elasticities.

The comparison between Britain and the United States is free from these difficulties. Indeed, the two studies summarized in lines 9 and 13 are about as nearly comparable as could be. The United States study was conducted by the Michigan Survey Research Center under the auspices of the Board of Governors of the Federal Reserve System, the United Kingdom study, by the Oxford Institute of Statistics; the latter explicitly patterned its study on the Federal Reserve study and adopted very nearly the same definitions, sampling methods, schedules, and so on. The one difference that is worth recording is in the definition of the consumer unit: the United Kingdom definition places less emphasis on the "pooling" of income and so would in some cases yield two or more consumer units where the United States definition would yield but one (see section 3 below for a fuller discussion of this difference). It is not at all

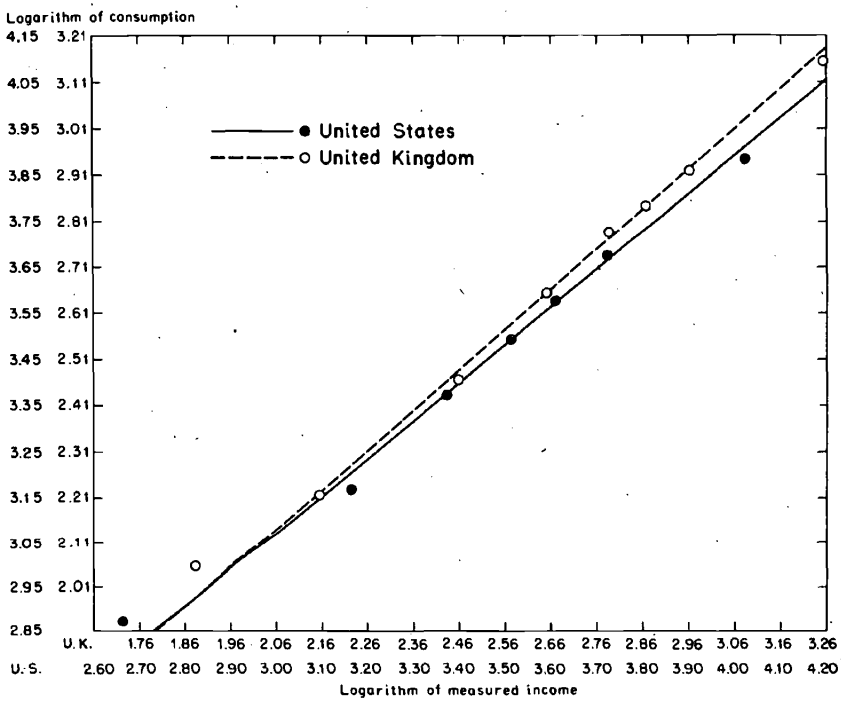
[15] Unfortunately, Wold does not present the basic data that could provide a check of this conjecture.

clear in what direction this would affect the elasticities, and it seems unlikely that the quantitative effect could be large.

Some of the data from these two studies are plotted in Figure 5. The scales on the figure have been chosen so as to make the arithmetic mean incomes coincide: the mean income in the United States

FIGURE 5

Regressions of Consumption on Income for the United States and the United Kingdom, Spending or Income Units of One or More Persons, Urban plus Rural, United States, 1950, United Kingdom, 1951–1952
(consumption and income in dollars for the U.S., in pounds sterling for the U.K.)



Source: See Table 1, sources for lines 9 and 13.

study in dollars is 8.73 times the mean income in the United Kingdom study in pounds; accordingly, dollar and pound values have been plotted as if the rate of exchange between them were $8.73 to the £. It is interesting that for both sets of data, the initial point is above and the final point below the regression suggested by the intermediate points—a characteristic of such data discussed in the preceding section.

The elasticities of .82 for the United States data and of .87 for

the United Kingdom data imply that some 18 per cent of the variance of measured income in the United States but only 13 per cent in the United Kingdom can be regarded as attributable to transitory factors. Since the total variance in measured income relative to the mean is not very different in the two countries or, if anything, somewhat less in the United Kingdom,[16] transitory variation in income must be less important in the United Kingdom not only as a percentage of total variation in income but also as a percentage of mean income itself. On the basis of our theoretical analysis in Chapter II, this would mean that the need for a reserve for emergencies would be less in the United Kingdom than in the United States, and hence might be expected to mean a higher value of k—or a higher average propensity to consume—in the United Kingdom than in the United States.[17] The observed value is decidedly higher, and the greater temporal stability of relative income position in the United Kingdom than in the United States revealed by the income elasticities may well be one important reason why it is.

Two other reasons for the higher average propensity in the United Kingdom come immediately to mind: (1) the more extensive provision of security through governmental channels in the United Kingdom; (2) the absence of a capital gains tax and the associated stimulus to investment by corporations. Both work against personal saving, which is the only kind covered in the studies summarized in Table 1.

One reason for the greater relative importance of transitory factors in the United States may be the higher frequency of farm units, who number some 10 per cent of the United States sample

[16] Compare the distributions of gross and net income for the two countries in H. F. Lydall, "National Survey of Personal Incomes and Savings," *Bulletin of the Oxford University Institute of Statistics*, XV, Numbers 2 and 3, (February and March, 1953), pp. 35–84, esp. Table 10, and in "1952 Survey of Consumer Finances, Part III. Income, Selected Investments and Short-Term Debt of Consumers," *Federal Reserve Bulletin*, (September 1952), esp. Table 8.

[17] There is a slip between the lower need for a reserve and the higher value of k that should be noted. Strictly speaking, the smaller magnitude of transitory factors implies, other things the same, a lower equilibrium level of nonhuman wealth relative to total wealth or to income. Given full stationary equilibrium, k would be unity whatever the magnitude of transitory factors, because full adjustment would have been made to it. The value of k depends on the difference between the equilibrium level of nonhuman wealth and the achieved level—or the significance attached to the difference. The assertion that k is higher the smaller the magnitude of transitory variation in income implicitly assumes (1) that we are speaking of a nonequilibrium position, (2) that the discrepancy between the existing position and the full equilibrium position is larger the higher the equilibrium level of nonhuman wealth, so that the higher the latter, the greater the pressure to devote current resources to increasing the level of nonhuman wealth.

and only 1 per cent of the British.[18] However, this can hardly account for much of the difference. The elasticity for United States nonfarm families alone is .80 (line 8, Table 1), compared to .82 for all United States units and .87 for all British units.

C. CONSUMPTION OF FARM AND NONFARM FAMILIES

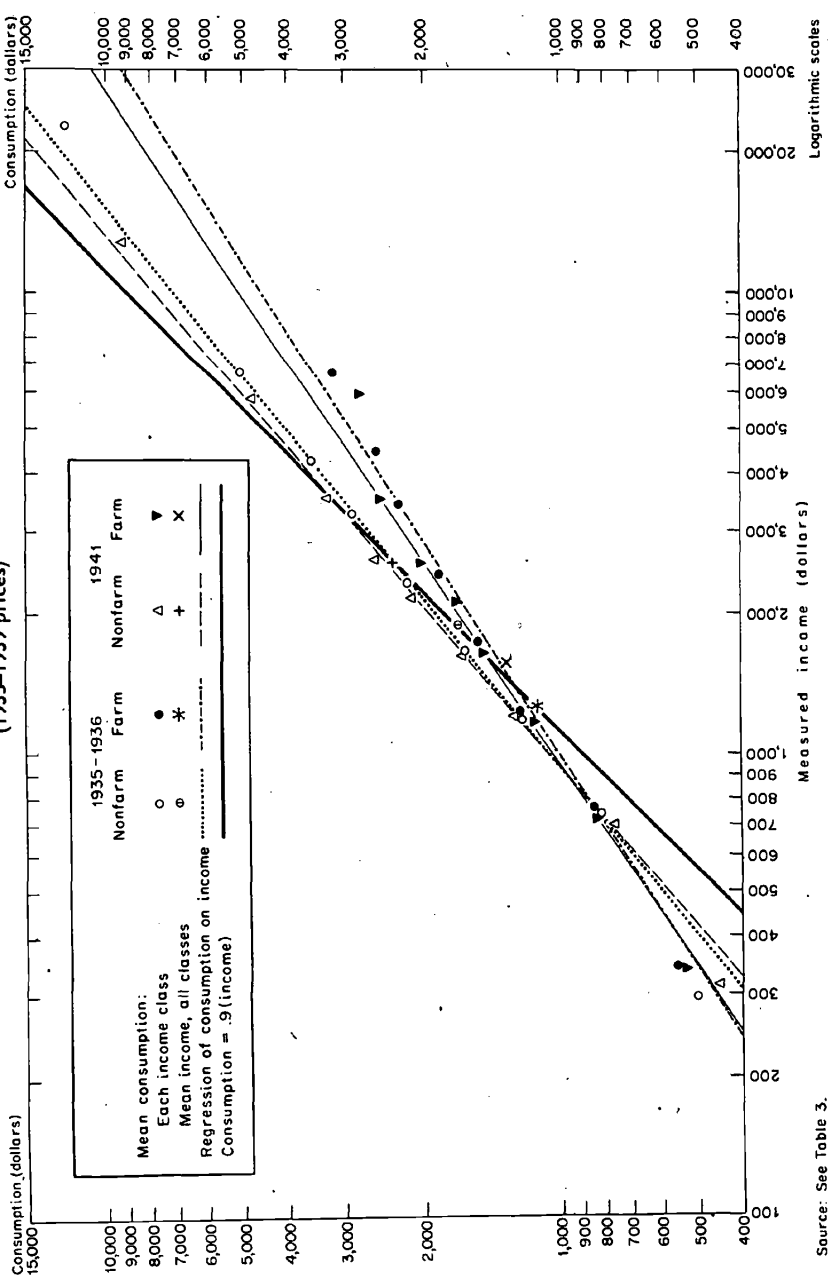
Figure 6 presents some comparative data for farm and nonfarm families from two studies for the United States—one for 1935–36, the other for 1941. These studies are comparable in coverage, method of collection of data, and concepts used. The only significant differences between the data from them plotted in Figure 6 are that (1) the 1935–36 data are for nonrelief families only, the 1941 data, for all families; (2) the nonfarm figures for 1935–36 are for all nonfarm families whereas those for 1941 are for urban families alone and exclude rural nonfarm families. Table 3 summarizes some numerical data for these two studies and adds some additional data for 1948–50. The 1948–50 figures are derived from the Federal Reserve Board Survey of Consumer Finances conducted by the Michigan Survey Research Center, and differ from the other figures in several important respects: (1) The data were collected differently. In the other studies, data were collected on consumption, savings, and income independently, in this study, only on savings and income. (2) The data are for spending units rather than for families. A spending unit consists of all related persons living in a single household who pool their income for "major" expenses, and it is on the average smaller than a family defined by relationship alone. According to Survey figures, there were about 15 per cent more spending units than families in 1950.[19] The use of the spending unit rather than the family has no effect on the average propensity to consume but does alter the level of mean income and consumption and can affect the regression of consumption on income. (3) The data are for spending units of all sizes combined, including one-person spending units, whereas the other data in Table 3, except for the 1941 farm figures, exclude single individuals. Both the use of the spending unit instead of the family and the inclusion of single individuals make decidedly more difference for nonfarm than for farm units,[20] which explains why only the average propensity to consume is recorded for nonfarm families.

[18] "1951 Survey of Consumer Finances, Part III," *Federal Reserve Bulletin* (August 1950), Table 7; Lydall, *op. cit.*, p. 61.

[19] "1955 Survey of Consumer Finances," *Federal Reserve Bulletin*, (May 1955), p. 472.

[20] Tables 7 and 8 of "1951 Survey of Consumer Finances, Part III," *Federal Reserve Bulletin* (August 1951) report that units headed by a farm operator accounted for 9 per cent of all spending units but 10 per cent of all family units. Unfortunately, these percentages are rounded to too few significant figures to permit a satisfactory estimate

FIGURE 6
Regressions of Consumption on Income for Farm and Nonfarm Families, United States, 1935–1936 and 1941 (1935–1939 prices)



Source: See Table 3.

(4) The data are for money items of income only. (5) Last but by no means least, the data available in published form are much less fully and less conveniently presented than for either of the other studies.[21] Despite these differences, the 1948–50 figures are included to provide some evidence for the period after World War II when the situation was decidedly different than in 1935–36 or 1941, particularly with respect to the income position of farmers.

The figures plotted on the chart are for money income and money consumption expenditures for nonfarm families, but include both money and nonmoney items for farm families. For nonfarm families, the only important source of nonmoney income and consumption is the imputed income from owned homes (the excess of rental value over current housing expenses and depreciation). In consequence, the difference between the money magnitudes and a broader concept of income and consumption that includes nonmoney items as well is not large enough to affect appreciably the kind of results we have been considering. As Table 3 shows, nonmoney items account for only about 5 per cent of total income; accordingly, we have not heretofore found it necessary to distinguish between

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of the excess of spending units over family units for farmers: taking the reported percentages to be precisely 9 and 10 would imply an excess of spending units of two per cent; allowing for errors of rounding, the excess could be anything at all under the 15 per cent for the country as a whole in 1951 implied by the estimates cited in the preceding footnote.

Footnote f of our Table 3 gives some figures on the relative importance of single individuals among farm consumer units. I have been able to infer no comparable data from the published data on the Survey of Consumer Finances.

[21] Indeed, there is no other American survey I have had occasion to use for which the published material on income and saving is so unsatisfactory for research purposes. The annual publications on the Survey of Consumer Finances in the Federal Reserve Bulletin give primarily frequency distributions, and even these are mostly only one-way distributions and the entries are given to too few significant figures to enable them to be used rather than simply repeated (a striking example is contained in the preceding footnote). Few averages or aggregates are published. Those that are published are mostly for the country as a whole. Even where averages are presented for subgroups, as for different occupational groups, the weights required to combine them into any other grouping cannot be inferred from the reported "number of cases," owing to the sampling methods used, but must be taken from a frequency distribution in which they are given to too few significant figures to be useful. No averages are given for income classes.

As a result of these deficiencies in the published data, I have had to rely almost entirely on data made available to me on special request or on data published in secondary sources in connection with special analyses of the Survey, mostly by members of the Michigan Survey Research Center. The research staff of the Board of Governors of the Federal Reserve System has been extremely cooperative and helpful in meeting such requests, and I am much indebted to them. At the same time, this arrangement is at best a poor substitute for having the basic data available in a form in which one can work with them himself. This is a major reason why these potentially valuable data have been far less useful in this study than they might have been, and why they are referred to so seldom.

the two concepts. For farm families, imputed income from housing is relatively more important than for nonfarmers; in addition, value of food grown for own use, which is of minor importance for nonfarm families, is even larger. The result is that nonmoney items—all of which count as both income and consumption—account for something like a third of total measured income including such items.[22] The broader concept seems more nearly comparable for the two groups of families, so is used in the chart. Table 3 contains data for both: money income and consumption alone, and total income and consumption including both money and nonmoney items.

The differences between the 1935–36 and 1941 studies themselves are primarily of the kind that, as suggested in section a above, would follow, on our hypothesis, from the higher average income in 1941 than in 1935–36: the 1941 regression of consumption on income is a trifle higher in every case than the comparable 1935–36 regression. The 1941 elasticity of consumption with respect to income (the slope of the regressions in the graph) is also a trifle higher in each case; these differences are minor and may well be a result simply of sampling fluctuations, though the consistency of the difference argues against this interpretation. Insofar as they are more than a reflection of sampling fluctuations, they imply that transitory factors were relatively more important in the 1935–36 data than in the 1941 data, a result that, as noted above, might be expected from the restriction of the 1935–36 data to nonrelief families.[23] The 1948–50 figures yield a higher average propensity to consume for both nonfarm and farm units than the earlier figures; it is not clear whether this is a result of differences in methods and definitions or is a significant difference between the two periods (see the additional comments on this point in the next section). The same question arises about the higher elasticity and marginal propensity for farm units in 1948–50, since the use of spending units instead of families would tend to produce higher values for these parameters. The differences among the several studies are, however, minor compared to the remarkably consistent story they tell about the differences between farm and nonfarm families:

[22] In addition to imputed income from housing and the value of food grown for own use, nonmoney income for farmers includes some minor items, such as fuel and ice, and gifts or pay in kind.

[23] This factor may also help to explain why the elasticities differ more for nonfarm than for farm families: about 17 per cent of all nonfarm families but only about 9 per cent of all farm families are estimated to have received relief during 1935–56. See U.S. National Resources Committee, *Consumer Incomes in the United States* (Washington, 1938), pp. 74–75.

CONSISTENCY WITH BUDGET STUDIES

TABLE 3

Relation between Consumption and Income for Farm and Nonfarm Consumer Units, 1935–1936, 1941, and 1948–1950

(dollar figures in 1935–39 prices[a])

Group and Year	Arithmetic Mean		Estimated Consumption at Income of $1,500	Average Propensity to Consume[b]	Income Elasticity of Consumption[c]	Marginal Propensity to Consume
	Income	Consumption				
			Money Income and Consumption			
Nonfarm or urban:[e]						
1. Families, 1935–36	$1,896	$1,676	$1,479	.88	.82	.73
2. Families, 1941	2,554	2,325	1,503	.91	.87	.79
3. Spending units, 1948–50				.94		
Farm:						
4. Families, 1935–36	816	657	1,035	.80	.63	.50
5. Families, 1941[f]	1,103	828	1,110	.75	.64	.48
6. Spending units, 1948–50				.88	.69	.61
			Money plus Nonmoney Income and Consumption			
Nonfarm or urban families:[e]						
7. 1935–36	1,980	1,760		.89		
8. 1941	2,723	2,494		.92		
Farm families:						
9. 1935–36	1,278	1,118	1,309	.87	.65	.57
10. 1941[f]	1,597	1,322	1,374	.83	.69	.57

[a] Figures in original sources all converted to 1935–39 prices by Bureau of Labor Statistics index of consumer prices.

[b] Ratio of arithmetic mean consumption to arithmetic mean income. For 1948–50, mean income is disposable income after personal taxes.

[c] Slope of graphically fitted straight line regression of logarithm of consumption on logarithm of income.

[d] Average propensity times elasticity.

[e] Nonfarm in 1935–36 and 1948–50, urban in 1941.

[f] Families plus single individuals. However, of the 762 farm consumer units in the sample, only 2 are single individuals, so the results for families alone would differ very little.

Source:

Lines 1, 4, 7, 9

Average income and consumption computed from National Resources Planning Board, *Family Expenditures in the United States* (Washington, 1941), pp. 119, 123, 124, 125, 127, 128, 130, and 13 Data for regression of consumption on income from *ibid.*, p. 51, for line 9; from Dorothy Brady in *Study of Saving*, Vol. III, p. 182, for lines 1 and 4. Consumption throughout includes gifts an personal taxes.

Lines 2, 5, 8, 10

Average income and consumption from U.S. Bureau of Labor Statistics, *Bulletin No. 822, Family Spending and Saving in Wartime* (Washington, 1945), pp. 71 and 73; income figure used is item recorde in these tables as income plus inheritances and other money receipts; consumption includes gifts an personal taxes. Data for regression of consumption on income from Dorothy Brady in *A Study Saving*, Vol. III, p. 182, for lines 2 and 5; from Department of Agriculture, Miscellaneous Publicatio No. 520, *Rural Family Spending and Saving in Wartime* (Washington, 1934), p. 161, for line 10.

Line 3

Based on weighted averages of average income and consumption figures in Table 5 for "independe business" and "others," with a weight of .07 for "independent business" and of .93 for "others." The

(cont. on next page)

TABLE 3 (cont.)

eights were roughly estimated from the averages for each of the three groups in Table 5 separately and l groups combined, plus the percentage of spending units reported as headed by "farm operator" in 1951 Survey of Consumer Finances, Part III," *Federal Reserve Bulletin*, August 1951, Table 7, and 1950 Survey of Consumer Finances, Part III," *ibid.*, August 1950, Table 15.

ine 6

See notes to Table 5.

consumption expenditures of farm families (1) are lower at any absolute income level except, perhaps, at the lowest levels of income observed in the farm samples; (2) increase less rapidly with measured income —both the marginal propensity to consume and the income elasticity of expenditures are decidedly lower; (3) are on the average a smaller fraction of average income—in our terminology, k is lower; this difference is much smaller for the comparison in terms of money plus nonmoney items than for the comparison in terms of money items alone; in all cases, however, it is in the same direction.[24]

Let us consider each of these in turn. On our hypothesis, point (1) follows from the lower average measured income of farm than of nonfarm families in the several studies. To bring this effect alone out in sharp relief, let us depart from the actual data in Figure 6 and instead consider the hypothetical Figure 7, in which differences

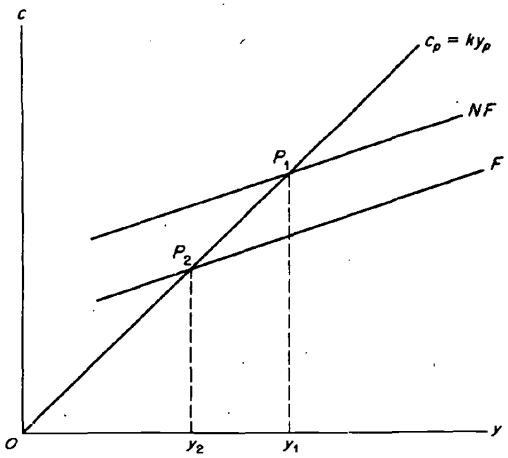
[24] For additional discussions of these data see Margaret G. Reid, "Effect of Income Concept upon Expenditure Curves of Farm Families," in Conference on Research in Income and Wealth, *Studies in Income and Wealth*, XV (New York: National Bureau of Economic Research, 1952), pp. 133–174; Brady and Friedman, "Savings and the Income Distribution," pp. 252–253; James N. Morgan, "The Structure of Aggregate Personal Saving," *Journal of Political Economy*, LIX (December 1951), pp. 528–534 especially p. 531.

In his time series savings study, which provides much of the evidence analyzed in the following chapter, Goldsmith estimates savings and the ratio of savings to income separately for nonagricultural and agricultural households. At first glance, his figures suggest that the relatively high savings of farmers shown by the budget studies are a special feature of the period after the mid-1930's and do not apply before then. His estimates give savings-income ratios for periods prior to 1934 that are decidedly lower for farm than for nonfarm families and for some periods even negative [see R. W. Goldsmith, *A Study of Saving in the United States*, Vol. I (Princeton University Press, 1955), pp. 76 and 102]. However, this contradiction is only superficial. It reflects primarily Goldsmith's exclusion of capital gains and losses from income and savings, which affects agriculture particularly strongly because of the large rise in land values during the early decades of the century. Whatever the validity of this treatment for the national aggregate, it is misleading in comparing the saving propensities of different groups. Goldsmith recognizes the difficulty and points out that the situation is rendered even worse by the inclusion, as a negative item of savings, of debt accumulated to purchase land that has risen in value. As some indication of the effect of his treatment, Goldsmith presents estimates of changes in net worth for separate saver groups for selected periods. It is clear from the relation of these to his savings estimates that use of change in net worth—and this seems closer than Goldsmith's concept of savings to the concept relevant in comparing different groups and implicit in budget studies—would yield higher savings-income ratios for agricultural than nonagricultural households for the whole of his period except possibly 1923–33. *Ibid.*, pp. 136–137.

(2) and (3) are supposed not to hold and which, for simplicity of exposition, is in arithmetic rather than logarithmic terms. Let $c_p = ky_p$ be the relation between permanent components, provisionally supposed to hold for both farm and nonfarm groups. Assume that transitory components of both income and consumption average out to zero for the group of farm families and also for the group of nonfarm families. Mean expenditures and mean income for the two groups would then fall on the line $c = ky$, say at P_1 for nonfarm families and at P_2 for farm families. As we saw in

FIGURE 7

Hypothetical Regressions of Consumption on Income for Farm and Nonfarm Families (k and P_y assumed same)



Chapter III, section 3 above, the observed regression of consumption on income tends to be flatter than $c = ky$ and to cut it at the group mean producing two regressions like those labelled *NF* (nonfarm) and *F* (farm) in Figure 7.

The explanation for this difference in observed results is therefore the same as for the differences between regressions for widely spaced points in time (see section a above). The interpretation is, however, somewhat easier to put explicitly in this case. Consider an income equal to y_1, the mean observed income of nonfarm families. Of the nonfarm families at this income, some "belong" there, in the sense that this is their permanent component, some are there because of favorable factors that have made their incomes abnormally high, some, because of unfavorable factors that have made their incomes abnormally low. Because y_1 is the mean income

of all nonfarm families, the latter two groups may be expected to be about equally numerous and to offset one another, so that on the average, y_1 correctly describes the permanent income of the class. Their consumption is, therefore, adjusted to this income, at least on the average. Consider now the farm families with an income of y_1. They fall into the same three groups—those who "belong" there, those who are there by good fortune, and those who are there despite bad fortune. But y_1 is an unusually high income for farm families, well above the average for all farm families. Those who are in this income class because of good fortune are therefore likely to be more numerous than those who are in it despite bad fortune. The average permanent income of the class is therefore less than y_1; their consumption is adjusted on the average to this permanent status; it is, therefore, less than the average consumption of the nonfarm families with an income of y_1. Similarly, consider an income equal to y_2, the mean observed income of farm families. This is, on the average, the "permanent" or "normal" income of the farm families classified at this measured income, so on the average their consumption is k times this income. But it is an abnormally low income for nonfarm families, so the average permanent income of nonfarm families classified at this measured income is higher than y_2, and their average consumption is higher than k times this income.

The lower marginal propensity to consume for farm families recorded in point (2) is, on our hypothesis, to be interpreted as a resultant of two more basic factors: the lower income elasticity of consumption and the lower average propensity to consume recorded in point (3). For our hypothesis tells us that the income elasticity is equal to P_v, the fraction of the total variance of income contributed by the variance of the permanent component;[25] and that the marginal propensity is equal to kP_v.[26] In discussing this point, therefore, we shall consider the lower income elasticity as its independent contribution. This lower income elasticity is, of course, reflected in Figure 6 in the steeper slope of the regressions for nonfarm than for farm families. When extended toward lower incomes, the regressions cross, a fact which explains the occasional finding of higher consumption expenditures for farm than for nonfarm families at low observed incomes.

On our hypothesis, the income elasticity itself is a resultant of

[25] If the regression is linear in arithmetic units, this is the elasticity at the mean income, when transitory components average out to zero.

[26] If the regression is linear in logarithmic units, this is the marginal propensity at the mean income when the transitory components average out to zero.

two more basic magnitudes: (a) the differences among families in measured income produced by factors regarded as transitory or temporary; (b) the differences in measured income produced by factors regarded as permanent. It seems plausible that (a) should on the average be larger for farm than for nonfarm families, perhaps not in dollar terms given the lower average income of farm families but certainly as a percentage of income. Variations in weather and the like affect farm income much more than nonfarm income; and farm income has a large entrepreneurial element whereas most nonfarm income is from such relatively stable sources as wages and salaries. The relative size of (b) is less clear: the greater heterogeneity of the nonfarm group tends toward wider differences in permanent components for them; the entrepreneurial character of farm income may well work in the opposite direction. The lower income elasticity for farm families tells us, or reflects the fact that, (a) is larger relative to (b) for farm than for nonfarm families, which is consistent with these speculations though not required by them. But, by itself, it does not tell us anything about each magnitude separately.

We can get estimates of each magnitude separately by combining the estimated income elasticities with the observed variation among families in measured income. Table 4 summarizes the results. Column (2) repeats the income elasticities from Table 3. These are here treated as measuring the fraction of the variance of measured income attributable to the variance of the permanent component. Column (3) is the standard deviation of the logarithms of measured income. Because computed from the logarithms, it is a measure of relative dispersion. It can be regarded as an estimate of the coefficient of variation of the original observations, or the ratio of the standard deviation to the mean.[27] The advantage of a measure of this sort is that it allows for differences in the unit of measure or in average income, and we have already taken account of such

[27] The standard deviation of the logarithms is a direct estimate of the coefficient of variation of the original observations provided that natural logarithms (logarithms to the base e) are used. If common logarithms (to the base 10) are used, the standard deviation of the logarithms must be multiplied by $\log_e 10$ to convert it into an estimate of the coefficient of variation.

If the observations are supposed to be distributed according to a logarithmic normal distribution, the maximum likelihood estimate of the coefficient of variation is $(e^{s^2} - 1)^{\frac{1}{2}}$, where s^2 is the variance of the natural logarithms of the observations. It will be seen that this is approximately equal to s for small values of s. I have not used the more precise estimate of the coefficient of variation simply because there is no special reason to prefer the estimated coefficient of variation of the original observations to the standard deviation of the logarithms as a measure of relative dispersion. It is convenient in exposition that the two can be regarded as estimates of one another.

TABLE 4

Dispersion of Measured Income, and its Permanent and Transitory Components Relative to Mean Measured Income for Farm and Nonfarm Families, 1935–1936, 1941

Group, Year, Income Concept (1)	*Fraction of Variance of Measured Income Attributed to Permanent Component*[a] (2)	*Relative Dispersion of:*		
		Measured Income[b] (3)	*Permanent Component*[c] (4)	*Transitory Component*[d] (5)
Nonfarm or urban families:				
1935–36	.82	.78	.70	.33
1941	.87	.82	.76	.29
Farm families:				
Money income:				
1935–36	.63			
1941	.64	1.04	.83	.62
Money plus nonmoney income:				
1935–36	.65	.70	.57	.41
1941	.69	.85	.70	.48

[a] Elasticity of consumption with respect to income from Table 3.

[b] Standard deviation of natural logarithms of measured income. This is an approximate estimate of the coefficient of variation (standard deviation divided by arithmetic mean income) of measured income itself.

[c] Square root of product of elasticity in column (2) and variance of logarithms of measured income [square of column (3)]. Result is an approximate estimate of coefficient of variation of permanent component in absolute units, on assumption that mean transitory income is zero.

[d] Square root of product of unity minus elasticity in column (2) and variance of logarithms of measured income. Result is an approximate estimate of ratio of standard deviation of transitory component to mean measured income.

Source of distributions of measured income: 1935–36, National Resources Planning Board, *Family Expenditures in the United States* (Washington, 1941), p. 120. 1941, urban, U.S. Bureau of Labor Statistics, Bulletin No. 822, p. 68; farm, U.S. Department of Agriculture, Miscellaneous Publication No. 520, pp. 26, 27, and 161. In lieu of detailed information, rather arbitrary approximations were used to distribute negative incomes and incomes in the top open-end class. Logarithmic variances cannot, of course, be computed when there are negative incomes. This difficulty was evaded by combining negative incomes with incomes under $500 and treating the corresponding families as if all had the average income of the group.

differences in our discussion of point (1). The square of the number in column (3) is the variance of logarithms of income, and the product of this and the fraction in column (2) is the estimated variance of the permanent components alone; its square root is the number entered in column (4). Similarly, the product of the variance of logarithms of income and the complement of the fraction in column (2) is the estimated variance of the transitory component

and its square root is the number entered in column (5). Our procedure forces equality between the square of column (3) and the sum of the squares of columns (4) and (5). This is, of course, a consequence of the assumption in our hypothesis that permanent and transitory components of income are uncorrelated. In interpreting these figures, it should be noted that the dispersion of permanent and transitory components is measured relative to total income. This seems the appropriate base. No significance can be attached, for example, to the dispersion of transitory components relative to their own mean, which might well be zero or negative.

There is considerable agreement between the studies for 1935–36 and 1941. The differences are in the direction to be expected from the more stringent eligibility requirements for the 1935–36 figures, especially their restriction to nonrelief families. In addition, the 1935–36 figures include nonfarm rural families plus urban families, whereas the 1941 figures include only urban families. Both factors would tend to produce wider variation in the 1941 data than in the 1935–36 data, and this is what Table 4 shows.

The more meaningful comparison between farm and other families is for money plus nonmoney income—only one set of figures is given for nonfarm families because the results are not appreciably different for the two concepts. This comparison shows the expected difference between the two groups in the dispersion of the transitory component: the standard deviation of the transitory component is about 30 per cent of mean income for nonfarm or urban families, and between 40 and 50 per cent for farm families. The difference is in the other direction for the permanent component: the standard deviation of the permanent component is at least 70 per cent of mean income for nonfarm or urban families; at most, 70 per cent for farm families. This means that the lower income elasticity of farm families is produced by both a larger variance of transitory components and a lower variance of permanent components—though the figures for money income show how the same result can occur despite a larger variance of permanent components.

For farm families, money income alone is relatively more dispersed than money plus nonmoney income. This is to be expected. Nonmoney income from housing and from food grown for own use can hardly be expected to vary anything like so much among families as money income.

The greater importance of transitory variation in income for farm than for nonfarm families may also help explain point (3),

the lower ratio of average consumption to average income for farm families. The reason is the same as that suggested above for the higher average propensity of British than of United States families: a larger variance in the transitory components of income means that farmers have a greater need for a reserve against emergencies than nonfarmers and so might be expected to have a lower k. Another factor that has often been cited, and that may well play a role, is the possibility that the farmer can earn a higher average rate of interest by direct investment in his own enterprise than nonfarmers can by indirect investment through financial intermediaries. However, it is my impression that savings as a fraction of income have been found to be higher for farmers than for nonfarmers even during periods when farming was a declining industry relative to other industries. If this is so, it suggests that the lower P_y is an independent and quantitatively significant factor producing a relatively high savings ratio.

d. OCCUPATIONAL CHARACTERISTICS OF FAMILIES

Two characteristics of farm families are the keystone of the preceding analysis of their consumption behavior: their decidedly lower average income, at least for the years studied, which accounts for generally lower consumption at given measured incomes; and the entrepreneurial source of their income, which helps to account for both the lower income elasticity of consumption and the lower average propensity to consume by making transitory components a relatively important source of income variation. Owners of nonfarm unincorporated businesses differ from farmers in respect of the first characteristic—their average income is higher than the average income of all families;[28] they are alike in respect of the second. Accordingly, if our hypothesis is valid, we should find that the average propensity to consume is lower for them than for nonentrepreneurial families and that our measures imply higher relative dispersion of transitory components—in this section we examine whether this is so. Negro families as a group are like farmers with respect to the first characteristic—they tend to have a decidedly lower average income than their white neighbors; but they differ with respect to the second. Accordingly, we should find that observed consumption is lower at given measured incomes for them than for white families; but there is no reason, on the basis of the characteristics cited, why the observed elasticities or

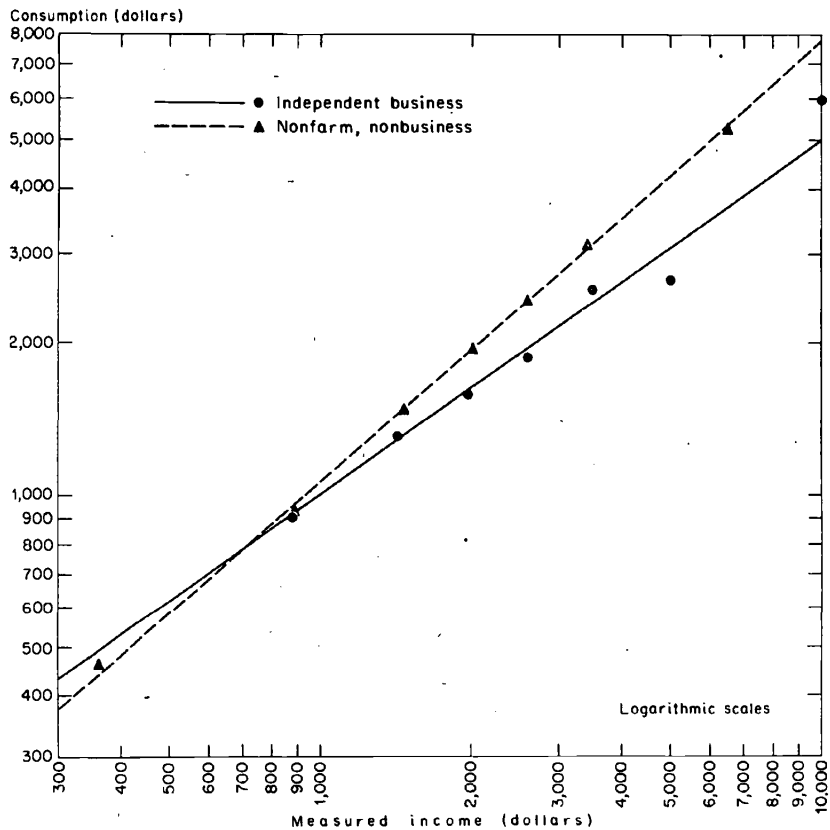
[28] See Table 5 below; *Consumer Incomes in the United States*, p. 26,1 "952 Survey of Consumer Finances, Part III. Income, Selected Investments, and Short-Term Debt of Consumers," *Federal Reserve Bulletin* (September 1952), Table 2.

average propensities should differ—in the next section we examine whether this is so.

Figure 8 and Table 5 summarize some evidence on the consumption-income relations for independent business spending units and other

FIGURE 8

Consumption-Income Relations for Independent Business and Nonfarm Nonbusiness Spending Units, 1948–1950
(1935–1939 prices)



Source: Based on L. R. Klein and J. Margolis, "Statistical Studies of Unincorporated Business," *Review of Economics and Statistics*, February 1954, Table 13, p. 41.

spending units. These data are from the nationwide Surveys of Consumer Finances covering 1948, 1949, and 1950 income. For comparability with earlier figures, the income and expenditure data in Figure 8 have been deflated to 1935–39 prices. The two regressions in the figure are very much like those for farm and nonfarm families in Figure 6: the business regression is flatter,

TABLE 5

Relation between Consumption and Income for Independent Business, Farm, and Other Spending Units, 1948–1950

Occupational Group	Average Disposable Income		Average Propensity to Consume	Income Elasticity of Consumption
	Current Prices	1935–39 Prices		
Independent business	$4,789	$2,795	.77	.70
Farmers	2,404	1,403	.88	.69
Others	3,038	1,773	.95	.86

Note: Figures are for money consumption and money disposable income.

Source:

Average disposable income and average propensity to consume: Based on data made available by the Board of Governors of the Federal Reserve System. These data gave average saving and average total income for each occupational group in each year but average disposable income only for 1948 and 1950. I estimated average disposable income in 1949 from the other figures plus average total income and disposable income in separate income classes. The average propensity entered is the ratio of average consumption for the three years to average disposable income for the three years, these averages in turn being simple averages of the corresponding figures for the individual years.

Elasticity of consumption for independent business and others: Slope of straight line regression of logarithm of consumption on logarithm of disposable income fitted graphically to data plotted in Figure 8. *Elasticity of consumption for farmers:* Slope of graphically fitted straight line regression of logarithm of consumption on logarithm of disposable income based on data for 1948, 1949, and 1950 made available by the Board of Governors of the Federal Reserve System. These data were for total income rather than disposable income classes and for each year separately.

starts above the other regression, then meets it and falls below it by increasing amounts. As Table 5 shows, the elasticity for independent business spending units is .70, decidedly lower than the elasticity of .86 for nonfarm, nonentrepreneurial spending units in Table 5, but a trifle higher than the values ranging from .63 to .69 recorded in Table 3 for farm families or spending units.

The similar elasticities for farm units and for independent business spending units is interesting; it is not, however, required by our hypothesis. We should expect both farm and independent business units to display a wider relative dispersion of transitory components of income than other units. The value of the elasticity also depends, however, on the size of the relative dispersion of permanent components. It is plausible that permanent components differ more widely for independent businessmen than for farmers. The independent businessmen pursue a wider diversity of activities than

the farmers and differ more widely in such attendant circumstances as the amount of capital invested in the business. For similar reasons, it is plausible that permanent components differ more widely for business spending units than for nonbusiness, nonfarm spending units. The crucial question for elasticities, however, is whether differences in the dispersion of permanent components are greater or less than the corresponding differences in the dispersion of transitory components, and about this there is not much a priori basis for formulating any precise expectations. What the elasticities tell us is that if the dispersion of permanent components for business units exceeds that for farm units, the dispersion of transitory components does so by a smaller percentage; whereas, if the dispersion of permanent components for business units exceeds that for nonfarm, nonbusiness units, the dispersion of transitory components does so by an even larger percentage. Unfortunately, there are no reasonably comparable estimates of total dispersion of measured income that could be combined with the elasticities to get estimates of the dispersion of each component separately, as we were able to do in Table 4.[29]

The decidedly lower average propensity recorded in Table 5 for business units (.77) than for nonfarm nonbusiness units (.95)

[29] Klein and Margolis consider explicitly what is in essence the explanation given here for the difference between business and other spending units in the elasticity of the consumption-income relations and in the preceding section for farmers and nonfarmers. They reject this explanation, writing, "Possible explanations for these findings (a lower marginal propensity to consume for farmers than nonfarmers) are differences in the rural way of life, high motivation to invest savings in productive farm assets, or participation by farmers in relatively large income increases just preceding the survey interviews. The last mentioned point would help to explain the differences if savings were positively correlated with income change. To some extent this is true, but the contribution of *large* income *increases* to lower savings rates has been found and can be established on plausible reasoning. Farmers, being entrepreneurs, are said to have more variable income than nonentrepreneurial groups; consequently, income change may be a more strategic variable for the former.

"Similar considerations carry over to our study of unincorporated businessmen. They too show a lower average propensity to consume and a lower marginal propensity than nonfarm, nonbusiness spending units" (p. 41). Later, they write, "The data in Tables 7 and 8, together with the fact that the marginal effect of income change on savings has doubtful size or sign, do not lead us to explain the observed discrepancies by differences in the variability of income for the two groups" (p. 42). Tables 7 and 8 of the Klein-Margolis paper give the distributions of past income change and of a number of other items for independent business and all spending units.

The evidence Klein and Margolis cite does not seem to justify their conclusion. We shall see later, in section 4, that data on the relation of income change to the consumption-income regression, including data from the studies Klein and Margolis use, are directly consistent with our hypothesis; it is hard to see why Klein and Margolis regard them as indirectly inconsistent with it. L. R. Klein and J. Margolis, "Statistical Studies of Unincorporated Business," *Review of Economics and Statistics*, XXXVII (February 1954), pp. 33–46.

conforms with expectations.[30] The propensity for farmers is much more difficult to interpret. It is closer to the propensity for "others" than for business units. Moreover, it is based on money items only. Inclusion of nonmoney items, which would render it more nearly comparable with the other propensities, would probably yield a still higher figure. If the ratio of nonmoney items to money items were the same in 1948–50 as in the 1935–36 and 1941 studies summarized in Table 3, and if, as in those studies, all nonmoney items consisted of consumption in kind, a propensity of .88 for money items would imply a propensity of .92 for money plus nonmoney items. It is likely that nonmoney items were less important in 1948–50 than in the earlier studies. In addition, the 1948–50 studies excluded some nonmoney saving items.[31] On both grounds, the required upward adjustment is smaller than that indicated. To add to the problem of interpretation, the 1948–50 propensity for farmers, whether or not adjusted for nonmoney items, diverges much more from the propensities computed from the earlier studies than the propensity for nonfarm units (see Table 3). These earlier studies yield propensities of .75 and .80 for money items, of .83 and .87 for nonmoney items. Does the divergence reflect a temporal change? Or does it reflect the sizable differences in the methods, techniques, and definitions of the several studies? I see no way to decide. Whichever interpretation is accepted, the studies agree in setting the average propensity lower for farmers than for nonfarm, nonbusiness units—though this difference is rather small for the 1948–50 data—and higher for farmers than for independent business units—though this difference is nonexistent if the money figures for the earlier studies are regarded as comparable to the figures in Table 5.

The only other extensive body of data classified by occupations that I know about is the 1935–36 data from the Study of Consumer Purchases, used as the source of the various data for 1935–36 cited earlier in this chapter. Some evidence from this source is summarized in Table 6. These data have some advantages over those from the Survey of Consumer Finances: data on consumption expenditures were collected directly, whereas in the Survey of

[30] Goldsmith gives estimates of the saving-income ratio of unincorporated business (*op. cit.*, I, p. 169) which are lower than his estimates for nonfarm households. However, these data do not contradict our findings for two reasons: (1) the exclusion of capital gains discussed above in footnote 24; (2) his estimates are for the businesses, not the owners thereof, and in consequence he does not include savings by the owners in forms other than investment in their business.

[31] In particular, changes in farm inventories, which were positive over the period 1948–50. See "1949 Survey of Consumer Finances, Part VIII," *Federal Reserve Bulletin* (January 1950), footnote 7; *National Income, 1954 edition* (Washington, 1954), p. 167.

TABLE 6

Income Elasticity of Consumption, and Relative Dispersion of Measured Income and its Components by Occupational Groups, Native White Nonrelief Complete Families in Three Cities, 1935–1936

Occupational Group	Income Elasticity of Consumption			Relative Dispersion[a] of: Measured Income			Permanent Component			Transitory Component		
	N.Y.C.	Col., Ohio	Atl., Ga.	N.Y.C.	Col., Ohio	Atl., Ga.	N.Y.C.	Col., Ohio	Atl., Ga.	N.Y.C.	Col., Ohio	Atl., Ga.
Wage earner	.94	.78	.83	.54	.54	.61	.52	.47	.56	.14	.25	.26
Clerical	.81	.83	.86	.51	.52	.56	.46	.47	.52	.22	.21	.21
Salaried business	.88	.78	.88	.72	.56	.57	.68	.50	.53	.25	.26	.20
Salaried professional	.87	.76	.78	.66	.54	.50	.61	.47	.44	.23	.26	.23
Independent business	.90	.73	.82	.85	.87	.90	.80	.75	.82	.26	.45	.38
Independent professional	.84			.92			.85			.37		
All families	.89	.81	.84	.77	.68	.70	.73	.61	.65	.26	.30	.28

[a] For meaning and computation of measures of relative dispersion see notes b, c, and d to Table 4

Source:

Income elasticity of consumption: Slope of graphically fitted straight line regression of logarithm of consumption on logarithm of income. Basic data from Department of Labor, Bulletin No. 648, *Family Expenditures in Selected Cities, 1935–36, Volume VIII, Changes in Assets and Liabilities* (Washington, 1941), Table 2.

Relative dispersion of measured income: Standard deviation of natural logarithms of income computed from frequency distributions in Department of Labor, Bulletin No. 643, *Family Income and Expenditure in New York City, 1935–36, Volume I, Family Income* (Washington, 1941), pp. 93–96; Bulletin No. 644, *Family Income and Expenditure in Nine Cities of the East Central Region, 1935–36, Volume I, Family Income* (Washington, 1939), pp. 137–40; Bulletin No. 647, *Family Income and Expenditure in the Southeastern Region, 1935–36, Volume I, Family Income* (Washington, 1939), pp. 147–150.

Consumer Finances they must be derived by subtracting reported savings from reported income; the original data have been published in much fuller and more detailed form; data are available for individual cities and for rather narrowly defined occupational groups. On the other hand, these data have two serious disadvantages for our present purpose. (1) For independent business families, measured income is defined essentially as withdrawals from business.[32] This must have the effect of reducing the relative importance of transitory components of income. When measured income, including business income, is abnormally low, consumption is likely to be

[32] See, for example, Bureau of Labor Statistics, Bulletin No. 647, *Family Income and Expenditure in Southeastern Region, 1935–36*, Vol. I, Family Income, pp. 505, 506 and 509.

financed, at least in part, by withdrawals from the business in excess of current earnings, and, conversely, when measured income is abnormally high, some part tends to be retained in the business. The restriction of recorded income to withdrawals is also likely to make the recorded average propensity to consume higher than one based on the alternative concept of income, since it seems likely that a substantial part of the savings of businessmen takes the form of business investment. The same definition of income is used for independent professional families. However, its effect seems less serious for them, in view of the generally much smaller scope for direct investment in, or withdrawal of funds from, professional than business activities. (2) Income data were obtained for a representative sample of units, albeit with some eligibility requirements. Consumption data, on the other hand, were obtained for a much smaller and designedly nonrepresentative sample. The result is that the basic reports on the study do not give figures on average consumption for a group as a whole, covering all income classes. Computation of such an average requires constructing a weighted average of estimated consumption for individual income classes; and since some income classes were not covered at all in the expenditure sample, estimates for these require extrapolation from other classes.[33] The computations involved are so extensive that I have not attempted to compute average propensities for the Consumer Purchases Study. I have reported such propensities only when they had already been computed as part of another study.

The evidence from the Study of Consumer Purchases in Table 6 is for three cities: New York; Columbus, Ohio; and Atlanta, Georgia. The particular selection of cities is arbitrary; similar data can be obtained for a considerable number of other cities. The effect on the income elasticity of using withdrawals instead of net earnings from business as a measure of income for independent business families is clear. Whereas the elasticity is .70 for the 1948–50 nationwide data which use net earnings, it is .90 in Table 6 for the New York data, which measure income by withdrawals, and this value is higher than for any other occupational group except wage earners. For income measured by withdrawals, only about 10 per cent of the total variance is contributed by transitory factors. The elasticity is lower for independent professional families than for independent business, whereas the reverse relation seems more plausible for the net earnings concept of income. None of the other

[33] This is the method employed by the National Resources Planning Board in constructing their estimates, which we have mostly used above.

elasticities show any regularities deserving of note; this may be because the data are for just three cities. Perhaps if a larger number of cities were included, some regularities would emerge.

The estimated measures of relative dispersion are more illuminating than the income elasticities themselves. There seems no reason why the use of withdrawals should systematically affect the variance of the permanent component, and the figures in Table 6 conform with this expectation. In New York, the dispersion of the permanent component is decidedly higher for business families than for any of the nonentrepreneurial groups, and almost the same as for the independent professional group; for each of the other cities, the two entrepreneurial groups have a decidedly higher dispersion of permanent components than any of the other groups. The only striking inter-city differences in the dispersion of the permanent components is for salaried business and salaried professional; these display a higher dispersion in New York than in the other cities; a result that seems entirely consistent with the differences in the economic character of the cities. New York offers greater opportunities for managerial and professional specialization than either of the other cities; the range of activities included under "salaried business" and "salaried professional" might be expected to be correspondingly broader. There is little difference among any of the other groups: the dispersion of the permanent component is in the neighborhood of 50 per cent for all four nonentrepreneurial groups in Columbus and Atlanta, and for wage earners and clerical groups in New York as well, compared with some 60 to 70 per cent for salaried business and professional families in New York and some 80 per cent for independent groups in all cities.

These remarks about the economic character of New York also seem relevant in explaining why the dispersion of the permanent component should be larger for all groups combined for New York than for the other cities. It is striking that the dispersion of the transitory component is almost the same in all cities, a result that seems entirely plausible, for there is nothing about the wider range of activities in New York that gives any reason to expect more uncertainty to be attached to any given activity. These considerations also explain why the elasticity should be higher for New York than for the other cities.

The main effect of the use of withdrawals for independent business families shows up in the estimated dispersion of the transitory component of their incomes in New York. At about 26 per cent, the dispersion is of the same order of magnitude as for the nonentrepreneurial groups in the table and for all nonfarm families

(see Table 4). The only differences that stand out in the table are: (1) the larger dispersion for independent professional families in New York and for the two independent groups combined in the other cities; this is in line with expectations and the numerical values are almost the same as those recorded in Table 4 for farm families; (2) the much lower dispersion for New York wage earner families than for other groups. I am inclined to view this result with suspicion, since the elasticity for this group seems out of line with other values.

To judge from the evidence of this and the preceding section, entrepreneurial families, whether farm or nonfarm, are alike in being subject to much uncertainty with respect to their income; the standard deviation of their transitory components of income appears to be something over 40 per cent of their mean income, or something like $1\frac{1}{2}$ to 2 times as large as for other families. The only substantial difference between farm and nonfarm entrepreneurs in the character of their income distributions is that the latter differ more widely than the former in respect of their permanent income status; the standard deviation of the permanent component is about 80 per cent of their mean income for nonfarm entrepreneurs, about 60 to 70 per cent for farm entrepreneurs.[34]

We saw in Table 1 that for nonfarm groups, consumption averaged about 90 per cent of income for studies ranging from 1888–90 to date. However, the earlier studies were restricted to wage-earner families, whereas the later studies covered a broader range of families, including in particular independent business families. If the figures could be taken at their face value, this would imply that the similarity in average propensity to consume concealed a significant change in wage earners' propensity to consume, since if independent business families were excluded from the later studies, the resulting average propensity would be higher than for the earlier studies. This possible conclusion is to some extent offset by the fact that the income figures for independent business families in most of the later studies are for withdrawals rather than net business earnings; and hence these studies doubtless understate the savings of these families. But this can hardly offset the conclusion completely, if only because, as we have seen, the use of withdrawals instead of net income has much less effect for independent professional families than for business families. In consequence, the apparent constancy of the average propensity must be regarded as concealing a secular rise. How this can be reconciled with time series data, which show no important secular

[34] This is for total income including nonmoney income.

change in the ratio of savings to income, is discussed in section 1b of the next chapter.

The substantial difference in the average ratio of consumption to income between entrepreneurial and other units, and between farm and nonfarm entrepreneurs, has important implications for the variables determining the k of our equation (2.6). We have already suggested that one variable accounting for the observed difference may be the larger dispersion of transitory components; if so, this is an important dimension of our portmanteau variable u. But it is clear that this is not the only variable. The dispersion of transitory components is of the same order of magnitude for farm and nonfarm entrepreneurs, yet nonfarm entrepreneurs apparently save a decidedly larger fraction of their income on the average. Another variable already mentioned is the rate of return that can be earned on savings—the i in our equation (2.6). It is likely that entrepreneurs are able to earn larger returns on the average through direct investment in their business—or at least expect to do so—than they or others can earn in other ways, if only because direct investment dispenses with the need for the services of brokers, financial institutions, and other intermediaries. Moreover, for the period spanned by Tables 3 and 5, farming was a declining source of income compared to independent business,[35] which suggests that the prospective earnings on capital were higher for nonfarm entrepreneurs. If, as seems plausible, this higher rate of return was a major factor accounting for the higher savings ratio of nonfarm than of farm entrepreneurs, this means that the rate of interest that can be earned on savings is an important determinant of the average ratio of savings to income, a conclusion that is entirely consistent with our earlier theoretical analysis but that runs counter to widely prevailing opinions about the effect of the interest rate on savings. Another factor that may help to explain the differences in the savings behavior of the various groups is a possible difference in tastes that accounts, on the one hand, for entry into independent business and, on the other, for high savings. The wide dispersion of permanent components in nonfarm independent business can be interpreted as meaning that this is a good route to high income status: people who place relatively great value on achieving high income status might thereby be attracted to it; by the same token they will place relatively great value on savings as another route to high income status. Still another factor, of a rather different sort, is the difference in the age and family composition of the entrepreneurial and other groups, a difference that might well be

[35] See *National Income, 1954 edition*, pp. 168–169.

designated a statistical bias. The clearest example is for people who are retired; these are necessarily excluded from the independent business group, yet are likely to have a relatively high ratio of consumption to income; this and similar exclusions of people who have not yet gone into business mean that the entrepreneurial group is likely to be rather heavily weighted with people or families that are in their peak earnings periods. These comments are, of course, purely suggestive. Detailed analysis for much finer groups would be required, and is highly to be desired, to support more precise and definitive conclusions.

e. NEGRO AND WHITE FAMILIES

The two panels of Figure 9 display the observed relation between the consumption and the income of white and Negro families for two cities. As in preceding charts, each point shows average consumption and income for a group of families in the same measured income class; the points are thus observations on the regressions of consumption on measured income. In both cities, the regressions for white and Negro families are roughly parallel and the regression for Negroes is below the regression for whites. At the same measured income, Negroes spend less on consumption than whites. As Table 7 shows, these results hold not only for the two cities covered by Figure 9 but also for other communities: in each of the six communities or group of communities covered by the table, the elasticity of consumption with respect to measured income is nearly the same for whites and Negroes—the minor differences show no consistency, the elasticity being higher for whites in two communities, higher for Negroes in three, and the same in one—and the regression for whites is above that for Negroes, as shown by the uniformly higher level of consumption for whites at a measured income of $1,000.[36]

[36] See Brady and Friedman, *op. cit.*, pp. 262–265; Horst Mendershausen, "Differences in Family Savings between Cities of Different Size and Location, Whites and Negroes," *Review of Economic Statistics*, XXII (August, 1940), pp. 122–137, and Richard Sterner (in collaboration with Lenore A. Epstein and Ellen Winston), *The Negro's Share* (New York: Harper, 1943), pp. 91–94, for similar analyses of the 1935–36 data.

The 1934–36 wage-earner study also contains information on Negro and white families separately. For all cities combined, the data, like those summarized in Figure 9 and Table 7, show lower consumption for Negroes than for whites at each measured income level within the range covered by the data. Like them also, they show little difference in the average propensity to consume for all classes combined, which is .99 for white families, .98 for Negro families; however, this slight difference is in the opposite direction from that recorded in Table 7. The one striking difference in results is with respect to the elasticity of consumption, which is .87 for the white families, .96 for the Negro families, a wider difference than for any of the cities covered in Table 7. (These estimates are based on data in Williams and Hansen, *op. cit.*, pp. 13, 14, 23, and 24.) I conjecture that the reason for this discrepancy between the two studies is the difference

These results are precisely those that, on our hypothesis, would be expected to follow from the decidedly higher average income of whites than of Negroes: the average income of whites is approximately 75 per cent higher than the average income of Negroes in the northern communities, and 140 to 235 per cent higher in the South. This higher average income means that a given measured income corresponds to a higher permanent income for whites than for Negroes, and therefore, on our hypothesis, to a higher level of consumption. In consequence, the higher consumption and lower savings of whites at each measured income level may reflect simply the inadequacy of measured income as an index of economic status.

We can come closer to a comparison of consumption habits proper by comparing not consumption at the same measured income but the ratio of mean consumption to mean measured income for all income classes combined. If mean transitory components of income and consumption were zero, these ratios would be estimates of our k. Unfortunately, such average propensities are readily available for only four of the six groups covered by Table 7 [column (5)]. To complete the picture, I have entered in Table 7, column (6), the ratio of consumption to income at the mean income of each group as computed from the regression of the logarithm of consumption on the logarithm of income.[37] As can be seen by comparing columns (5) and (6), as well as from other data for which we have both the ratio of averages and the ratio at the average income as computed from a regression between the logarithmic values, the ratio at the average income tends to

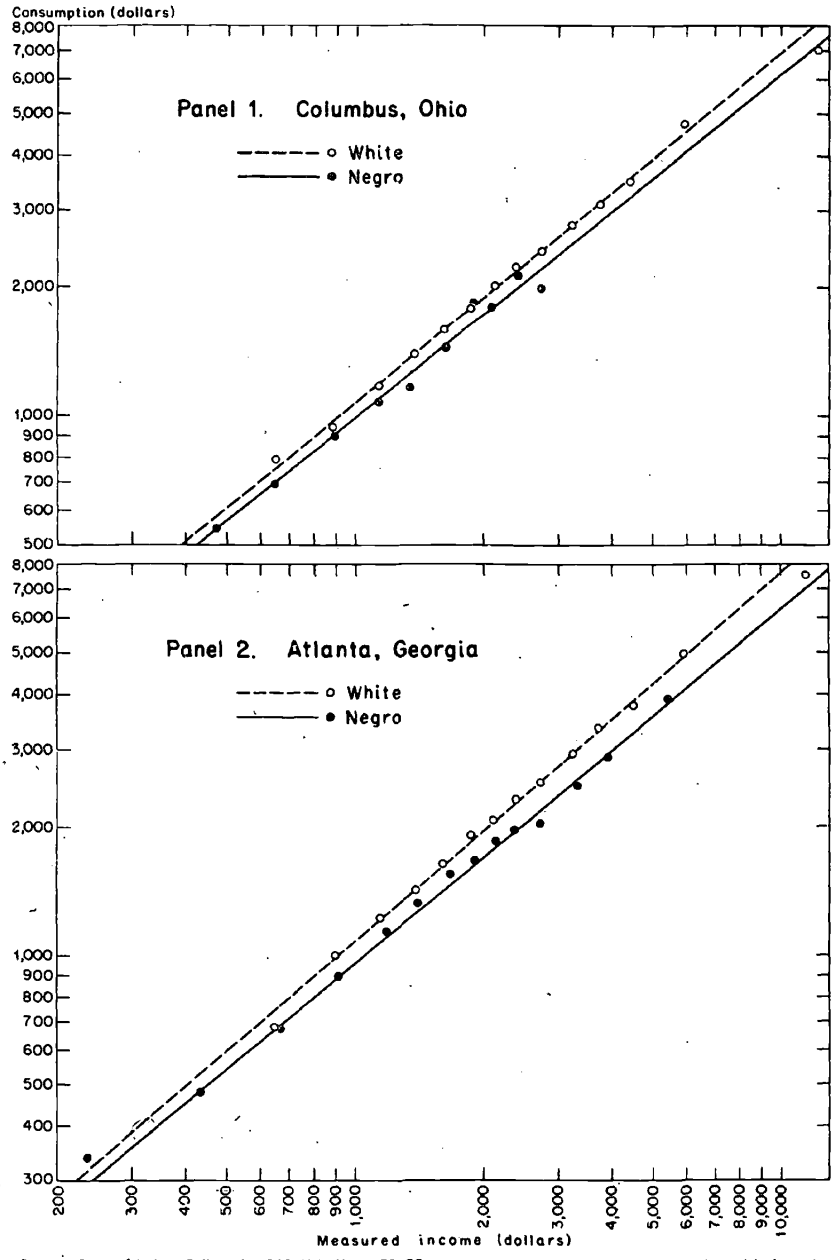
between them in eligibility requirements, which, as noted earlier, were more restrictive for the 1934–36 study. These requirements, which had the effect of excluding much of the variation in transitory components, disqualified a much larger fraction of Negro than of white families (*ibid.*, p. 30). By reducing the variance of the transitory component for Negroes more than for whites, they would tend to produce the observed difference in elasticities.

Results similar to those cited in the text are reported by Klein and Mooney for the North on the basis of data for 1947, 1948, 1949, and 1950 from the Surveys of Consumer Finances. However, for the South, they report a similar result only for lower incomes; for disposable incomes above about $2,000 they report higher consumption expenditures for Negroes than for whites. They suggest the possibility, and present some evidence to support it, that the different pattern for the South may arise from their inclusion of purchases of durable goods as consumption; while the Consumer Purchases Study also included durable goods as consumption, durable goods purchases may well have been more important for this group in 1947–50 than in 1935–36. See L. R. Klein and W. H. Mooney, "Negro-White Savings Differentials and the Consumption Function Problem," *Econometrica*, XXI (July, 1953) pp. 425–456, esp. pp. 425, 426, and 454.

[37] If the regression were a straight line between the arithmetic values of consumption and income, it would, of course, tend to go through the point corresponding to mean consumption and mean income.

FIGURE 9

Regressions of Consumption on Income for Native White and Negro Nonrelief Families, Columbus, Ohio, and Atlanta, Georgia, 1935–1936



Source: Dept. of Labor, Bulletin No. 648, Vol. III, pp. 52–55.

TABLE 7

Relation between Consumption and Income, and Relative Dispersion of Measured Income and Its Components, Native White and Negro Nonrelief Complete Families in Selected Communities, 1935–1936

Community and Group (1)	Arithmetic Average Income[a] (2)	Income Elasticity of Consumption[b] (3)	Consumption at a Measured Income of $1,000[a,c] (4)	Average Propensity to Consume (5)	Ratio of Consumption to Income at Mean Income of Group[c] (6)	Relative Dispersion[d] of: Measured Income (7)	Permanent Component (8)	Transitory Component (9)
New York, N.Y.:								
White	$2,645	.89	$1,110	.98	1.00	.77	.73	.26
Negro	1,500	.89	1,050	.99	1.01	.52	.49	.17
Columbus, Ohio:								
White	2,058	.81	1,070		.94	.68	.61	.30
Negro	1,130	.80	978		.96	.61	.54	.27
Atlanta, Ga.:								
White	2,158	.84	1,080	.93	.96	.70	.64	.28
Negro	888	.81	960	.96	.98	.89	.80	.38
Southeast, middle-size cities:[e]								
White	2,086	.86	1,040		.94	.72	.67	.27
Negro	686	.88	930		.98	.82	.77	.29
Southeast, small cities:[f]								
White	1,683[h]	.92	1,025	.95	.97	.52	.49	.14
Negro	620[h]	.94	960	.99	.98	.53	.52	.13
Southeast, villages:[g]								
White	1,674[h]	.88	1,015	.92	.96	.57	.53	.19
Negro	500[h]	.93	970	1.00	1.02	.48	.47	.13

[a] Dollar figures are in current prices. To convert into 1935–39 prices requires division by .986.

[b] Slope of graphically fitted straight line regression of logarithm of consumption on logarithm measured income.

[c] Computed from regressions described in preceding footnote.

[d] For meaning and method of computation see notes b, c, and d to Table 4.

[e] Columbia, S.C., and Mobile, Ala.

[f] Gastonia, N.C.; Sumter, S.C.; Albany, Ga.; Griffin, Ga.

[g] 34 villages in North Carolina, South Carolina, Georgia, and Mississippi.

[h] These averages are of somewhat more questionable accuracy than the others in this colum They are based on the data from the expenditure sample of the Consumer Purchases Study, where the others are based on data for the income sample. The expenditure sample was deliberately design to be nonrepresentative, and the Bureau of Labor Statistics in its publications gives no averages for income groups based on the expenditure sample. The Bureau of Home Economics does, saying that found only minor differences between averages computed directly for the expenditure sample and tho computed from it by weighting the observations in individual cells according to their relative frequen in the income sample. However, it warns explicitly that the least reliable averages are for all incon classes combined, precisely the ones we use. I have used them nevertheless because of the labor involv in using the data for the income sample.

(cont. on next page)

TABLE 7 (cont.)

Source:

Arithmetic average incomes and frequency distributions used in computing relative dispersion of measured income: New York, N.Y.—Department of Labor, Bulletin No. 643, Vol. I, pp. 93, 99, 156, and 162; Columbus Ohio—Department of Labor, Bulletin No. 644, Vol. I, pp. 137, 143, 185, and 191; Atlanta, Ga.—Department of Labor, Bulletin No. 647, Vol. I, pp. 147, 153, 195, and 201; middle-size cities—*ibid.*, pp. 243, 249, 270, 275, 296, 301, 322, and 327; averages for Columbia and Mobile combined by weighting by estimated number of native complete families in a 100 per cent sample as given in *ibid.*, pp. 138 and 140; small cities and villages—Department of Agriculture, Miscellaneous Publication No. 464, *Changes in Assets and Liabilities of Families, Five Regions* (Washington, 1941), pp. 88, 93, 95, and 96.

Consumption and income by income classes, used to fit regressions: New York, Columbus, Atlanta, and middle-size cities—Department of Labor, Bulletin No. 648, Vol. VIII, pp. 46, 47, 52–55, 62, and 63; small cities and villages—Department of Agriculture, Miscellaneous Publication No. 464, pp. 88, 93, 95, and 96.

Average propensity to consume: New York and Atlanta—derived from data given by Richard Sterner, *The Negro's Share*, (New York, 1943) pp. 92, 96–98; small cities and villages: Department of Agriculture, Miscellaneous Publication No. 464, p. 88.

be some one to five percentage points higher than the ratio of the averages, which explains why the numbers in column (6) are systematically higher than most of the average propensities we have so far cited.

The ratios in columns (5) and (6) tell a very different story from the absolute values in column (4): in each of the ten comparisons, the ratio is higher for Negroes than for whites. According to this evidence, it is the whites who are on the average more thrifty.

I hasten to add that this result cannot itself be interpreted confidently as reflecting the effect of race per se. (1) In each community, a decidedly smaller fraction of Negro families than of white are independent business or professional families. As we saw in the preceding section, such families tend to save a larger fraction of their income than other families, so their smaller relative importance tends to make the average propensity higher for all Negro than for all white families. This tendency is not fully reflected in these data, due to the use of withdrawals as a measure of entrepreneurial income, but even so, rough calculations suggest that this factor alone would make the average propensity something like one percentage point higher for Negroes. (2) The limitation of the data to nonrelief families might work in the same direction. A decidedly smaller fraction of Negro than of white families were classified as nonrelief. At the lower measured income levels, nonrelief families are on the average highly likely to have a negative transitory component of income, and certain to have a higher average permanent income than the relief families at the same measured income. This may well affect the average for all measured income classes, making for a negative mean transitory component

for all nonrelief families. If so, the effect would be greater for Negroes than for whites and would make the ratios in columns (5) and (6) even more of an overestimate of k for Negro than for white families.[38]

The final three columns of Table 7 give estimates of the relative dispersion of measured income and the permanent and transitory components. These show an interesting difference between the North and the South. In the two northern cities, the dispersion of both permanent and transitory components is wider for whites than for Negroes—only slightly wider in Columbus, decidedly wider in New York. In the Southern communities, the difference between Negroes and whites is generally in the opposite direction: for measured income and permanent components, in three of the four communities; for transitory components, in two. The differences are consistent for Atlanta and the middle-size cities and larger for these than for other southern communities. This result is most suggestive and not at all implausible—though again it must be regarded with some suspicion as possibly being simply a disguised reflection of the restriction of these data to nonrelief families. In the South, the whites and Negroes are more nearly two economically separated societies than in the North. The integration in the North means that the Negroes tend to be specialized; their lower average income reflects very largely the fact that they engage in lower paid occupations; they depend on the white community, as it were, for the services of the more highly paid occupations. In the South, being less fully integrated in this sense in the society, Negroes may tend to depend more on their own community for these more highly paid occupations; their lower average income reflects not only their concentration in lower paid occupations but also, to a much greater extent than in the North, a lower return for each activity separately. These effects would presumably tend

[38] It was noted in footnote 36 that for the 1934–36 wage-earner study the average propensity to consume is slightly higher for whites than for Negroes—.99 compared with .98. Two factors may explain this reversal from the pattern in Table 7: (1) The 1934–36 study covered only wage earners and lower salaried clerical workers, so point (1) in the text above does not apply. (2) Whereas the exclusion of families receiving relief might by itself tend to produce a negative mean transitory component of income, and to affect Negroes more than whites, the additional eligibility requirements in the 1934–36 study probably operated on balance in the opposite direction. In particular, the exclusion of all families with incomes under $500, which excluded a much larger fraction of Negro than of white families, clearly tended toward a positive mean transitory component. At the other end of the scale, the exclusion of families whose chief earner was a clerical worker earning more than $2,000 a year tended to introduce a negative mean transitory component and was much more important for whites than for Negroes. On balance, therefore, it seems not unlikely that the mean transitory component for Negroes in the sample is larger (in algebraic value) than for whites, just the opposite of the results conjectured for the Study of Consumer Purchases.

to be more marked in larger communities than in small cities or villages. A minor bit of evidence supporting this interpretation is that although the average income of nonrelief Negro families is more than 20 per cent lower in Atlanta than in Columbus, a slightly higher percentage of all Negro families, 10.3 compared with 9.8, are classified as business or professional families, independent or salaried.[39]

f. A DIGRESSION ON THE USE OF PARTIAL CORRELATION IN CONSUMPTION RESEARCH

One implication for consumption research follows so directly from the preceding analysis that it seems worth digressing briefly from our main path to consider it explicitly. A common method of analyzing the factors affecting consumption is to "hold income constant" while studying the effect of other variables. Income, it is argued, is the major factor affecting expenditures, so, unless its influence is first eliminated, the effect of other variables will be swamped. In this method, "income" is almost invariably taken to be what we have called "measured income" for a particular year, and it is "held constant" either by multiple correlation analysis in which income is one of the variables[40] or by a variant in which residuals from a consumption-income (or savings-income) regression are first calculated and then analyzed for the influence of other variables.[41]

If the preceding analysis is accepted, it is clear that these methods do not hold income constant in a sense that is meaningful for the determination of consumption behavior. What they hold constant is a mixture of income in such a sense—our permanent component—and accidental additions to or subtractions from current receipts that play little or no role in determining consumption behavior. And the particular mixture is likely to be related to the other variables being studied in a systematic way. Instead of eliminating the influence of income, these methods simply disguise its influence; it shows up as if it were the effect of other variables.

[39] Department of Labor, Bulletin No. 644, Vol. I, p. 122; Bulletin No. 647, Vol. I, p. 138. These percentages are for all families, relief and nonrelief, complete and incomplete. See also Gary S. Becker, "The Economics of Racial Discrimination," (1955) an unpublished Ph.D. dissertation at the University of Chicago; and Morton Zeman, "A Quantitative Analysis of White-Nonwhite Income Differentials in the United States," (1955), an unpublished Ph.D. dissertation at the University of Chicago.

[40] See, e.g., the equation set up by Lawrence Klein in Katona, Klein, Lansing, and Morgan, *Contributions of Survey Methods in Economics*, p. 203 and the subsequent analysis, as well as the various articles referred to in the bibliography appended to *ibid.*, especially those by Klein or by Klein and a collaborator.

[41] See, e.g., the two chapters by Morgan in *ibid.*

The Negro-white comparison perhaps brings this out most clearly. Let us hold measured income constant in analyzing the influence of color.[42] We shall then find that Negroes spend less on consumption and save more than whites. The reason, as we have seen, is simply that the same measured income systematically means a lower permanent income for Negroes than for whites. What this method leads us to call the effect of race for given income is predominantly the effect of the income differences which are concealed by a common measured income. As we have seen, when the comparison is made so as to eliminate this effect, namely, by comparing behavior at the means of groups at which transitory effects can be supposed roughly to cancel out, there is no evidence at all that Negroes are more "thrifty" than whites; on the contrary, such evidence as there is argues in the opposite direction. The appearance produced by holding measured income constant is an illusion attributable to the method of analysis.

The various earlier parts of this section are additional examples of the same point. The recent volume of the Michigan Survey Research Center, *Contributions of Survey Methods to Economics*, especially the chapters by Morgan and Klein, provides further examples that may be worth citing, particularly because these chapters are in the main such admirable pieces of work, revealing a high degree of sophistication and ingenuity in statistical technique and economic analysis, great care in examination of the basic material, and the loving and generous expenditure of time and effort in its analysis. Yet, if our hypothesis and its application along the lines of the earlier part of this section are accepted, their findings are rendered almost worthless because of the use of partial correlation techniques which hold measured income "constant."

Morgan first isolates groups that seem to differ appreciably in the slope of the regression of saving on income, and then computes a regression for each of these groups. For each consumer unit, he takes the difference between observed savings and savings estimated from the regression for the group to which it belongs as an estimate of the effect of variables other than (1) income and (2) those defining the groups, mostly home ownership status and size of liquid assets.[43]

[42] As Klein and Mooney do in their analysis of Negro-white savings differentials. See "Negro-White Savings Differentials and the Consumption Function Problem," esp. pp. 429–430.

[43] In Chapter III, he uses seven regressions: three for nonhomeowners, all for 1947 and 1948 combined, for three classes determined by liquid assets at beginning of year—0 liquid assets, $1–$499, and $500 or more; four for homeowners—two for 1947 and 1948 combined, liquid asset classes 0–$199, and $200–$2,999, and two for liquid asset class $3,000 or more, one for 1947 and one for 1948. In Chapter IV, where he uses

He then examines the effect of other variables on the residuals. This same type of analysis is carried out twice, once using regressions based on data for essentially all units in his sample, a second time using so-called "normal" regressions based on only about half the units.

One of the first variables by which Morgan classifies the residuals from the first set of regressions is size of city. From the average value of the residuals for each size of city, he concludes that city size has a significant effect on savings, savings at a given income being lower, and expenditures higher, in "metropolitan areas" than in nonmetropolitan cities or towns, and in the latter than in "open country nonfarm areas." It so happens that this is one of the few points at which Morgan reports average income and average savings. Average income is decidedly higher in metropolitan areas than in any of the three groups into which he classifies nonmetropolitan cities or towns—among which average income differs

"normal" regressions (see below), he uses sixteen regressions, all for 1947, 1948, and 1949 combined, for four liquid asset classes for each of four housing status classes.

Though it is aside from our own major point here, it is interesting to speculate on the differences he finds. He finds that the marginal propensity to consume (unity minus the marginal propensity to save that he computes) decreases as the size of liquid assets increases and is lower for homeowners than for nonhomeowners. The explanation of the liquid asset effect seems reasonably straightforward on our hypothesis. Units that are subject to a relatively large transitory component of income have more need for reserves against emergency than others; in consequence they might be expected to hold larger amounts of liquid assets on the average. As argued in our discussion of the effect of occupation, both k and P_y will tend to be relatively low for such units, and, in consequence, so will their product, which is the marginal propensity to consume.

Evidence supporting this interpretation is available from Survey of Consumer Finances data on the distribution of liquid assets by size for different occupational groups—[see "1950 Survey of Consumer Finances, Part V," *Federal Reserve Bulletin* (December 1950), Table 34, for 1949 and 1950, and "1952 Survey of Consumer Finances, Part III," *ibid.* (September 1952), Table 12, for 1951 and 1952]. For 1950, for example, if we use Morgan's liquid assets classes for nonhomeowners, the percentage of units with liquid assets of $500 or more was 67, 62, and 46 for professional and semiprofessional, managerial and self-employed, and farm operators, respectively; it was 48, 35, and 26 for clerical and sales, skilled and semiskilled, and unskilled and service, respectively. The former are clearly the groups subject to greater transitory variation. The one exception is for retired, for whom the corresponding percentage is 62; but this group is too small to affect the result significantly. We cannot duplicate from the tables cited the class intervals Morgan uses for homeowners. If we use instead liquid assets of $5,000 or more, the percentages for 1950 in the order just given are 18, 19, and 11; 7, 4, and 3; and 24 for retired. Much the same picture prevails for other years.

I have not thought of any equally plausible explanation for the difference between homeowners and nonhomeowners. Perhaps, as Morgan suggests, this difference reflects largely the omission of imputed income from housing as an item of income and consumption; like him, however, I doubt that this can be the whole story. It seems to me more likely that it is a disguised reflection of other variables; for example, homeowners seem to have larger liquid assets than nonhomeowners and presumably differ in age and family composition, etc. See Katona, Klein, Lansing and Morgan, *op. cit.*, pp.101–113; pp. 157–161.

little—and decidedly higher in these three groups than in open country nonfarm areas. The result is that the same measured income can be expected to correspond to a higher permanent income, and so to a higher consumption and lower savings, in the metropolitan areas than in the nonmetropolitan, and in these than in the open country. The average propensity to consume, as computed from the reported figures, is .84 in the metropolitan areas; .83, .84, and .84 in the three groups of nonmetropolitan cities and towns; and .83 in the open country nonfarm areas. These certainly give little sign of any city-size effect at all. The alleged city-size difference, like the Negro-white difference, is a figment of Morgan's procedure.

In his other analyses of residuals, Morgan does not report average income and average savings. But it is reasonably clear from other evidence that many of the effects he finds to be significant are, like the Negro-white and city-size effects, produced largely or wholly by differences in average income that make the same measured income correspond to different permanent incomes. To cite a few: (1) He finds "less saving by spending units with more than one earner."[44] We know that average family income tends to rise with number of earners, which could produce this pattern in observed residuals. (2) He finds "significantly lower saving for primary spending units and higher saving for secondaries."[45] Again we know that primary units tend to have higher average incomes than secondary units. (3) He finds "little difference in average residuals between different occupation groups, the only residual significantly different from zero being, strangely enough, that for unskilled and service workers,"[46] a group which has positive average savings residuals. It is also, of course, a group that has a relatively low average income, so this finding is not at all strange on our interpretation of it.

Morgan uses "normal" regressions at the second stage of his analysis in an attempt to solve the problem raised by transitory components of income, a problem of which he is aware though he does not, of course, use our terminology. He derives these regressions from data for "groups of people in reasonably 'normal' circumstances,"[47] the groups being obtained by excluding units with a head over 65 years of age, unemployed, or retired, and units that received large gifts or inheritances or experienced an increase in income of 25 per cent or more or a decrease of 5 per cent or more. He then uses

[44] *Ibid.*, p. 129.
[45] *Ibid.*, p. 129.
[46] *Ibid.*, p. 132.
[47] *Ibid.*, p. 157.

these regressions to compute residuals for both "normal" and other units. Unfortunately, despite its good intentions, this technique is either useless or makes matters worse. In the first place, the "normal" regressions cannot be regarded as relations between "permanent" components. The variance of transitory components for the included units is doubtless smaller than for all units but can hardly be zero; in any event, the variance of permanent components must also be smaller, though probably not so much so.[48] The result is some kind of hybrid that is difficult to interpret at all precisely. In the second place, suppose that the "normal" regressions could be regarded as relations between permanent components. They could then be used to eliminate the effect of the differences in permanent income only if some estimate of permanent income were available for the individual consumer unit. But no such estimate is available. Morgan simply uses measured income and thereby introduces an additional source of error into his residuals.

The crucial fallacy is the assumption that permanent, or, as Morgan would phrase it, "normal," consumption or savings is the same for the same measured income, provided other variables affecting saving behavior are held constant. Given this fallacy, it makes little difference what relation between savings and income is used to estimate "normal" savings; insofar as the same measured income corresponds to different permanent incomes, the residuals have not been freed from the effect of income.

I am not, of course, arguing that the variables Morgan considers may not affect spending behavior, or even that their effects may not be in the direction that he finds. My point is only that it is impossible

[48] As noted in footnote 43, Morgan uses data for 1947, 1948, and 1949 combined for his "normal regressions;" for 1947 and 1948 only, for his initial regressions; in addition, rather than 7 he uses 16 groups, of which it is not clear that any match precisely in definition. For these reasons it is not possible to make a valid comparison between the parameters of his two sets of regressions and so to test this conjecture directly or to estimate by how much more the variance of the transitory components is reduced than the variance of the permanent components. Somewhat more satisfactory evidence is furnished by a comparison of the regression coefficients for the included and excluded groups, though this too is blurred by differences in the classifications for which separate regression coefficients are reported. Crude calculations from these data support the conjecture that the variance of transitory components is reduced by somewhat more than the variance of permanent components. They suggest, however, that the differential effect is small in magnitude, so that the fraction of the variance of income accounted for by transitory components is about 75 per cent as large for the included as for the excluded groups (*ibid.*, pp. 159 and 160).

The liquid asset effect discussed in footnote 43 above is about as marked in the "normal" regressions as in the initial regressions. If the interpretation of this effect offered in that footnote is correct, this is a further bit of evidence that the "normal" regressions are not much closer to regressions between permanent components than the initial regressions.

to know from his analysis whether they affect spending behavior or in what direction.

Klein, in the first of his chapters in the *Contributions* volume, follows a method similar to Morgan's, except that he uses regression equations involving a number of variables simultaneously, and so multiple correlation rather than simple correlation, in getting his residuals; he uses as his independent variable the ratio of savings to income rather than savings alone; and he devotes much more of his attention to the equations themselves, and less to the residuals. In practically every equation, Klein includes income (or the logarithm of income) and liquid assets at the beginning of the year (usually as a ratio to income). One of his most consistent findings is that for given income (which means, of course, given measured income) savings decrease as liquid assets increase, which means that consumption increases. Now this finding, which has been pointed out in other studies of the effect of liquid assets, may, so far as this evidence goes, be simply another example of the bias introduced by the partial correlation technique. For there is ample evidence that income is on the average higher, the higher the level of liquid assets; in consequence, a given measured income tends to correspond to a higher permanent component, and so to higher consumption and lower savings, the higher the level of liquid assets. These correlations, therefore, give no usable evidence on the effect of liquid assets in their own right.

One incidental calculation which Klein makes is a correlation using income and savings for a two-year time unit rather than the usual one year. As he points out, lengthening the time period produces results that are consistent with our hypothesis: the ratio of savings to income is less highly correlated with other variables and the estimated marginal propensities to consume are higher.[49]

3. *Savings and Age*

There is an apparent discrepancy between our hypothesis and some results reported by Janet Fisher in a study based on data from the Survey of Consumer Finances. The first four columns of Table 8 are from this study. The income mean ratios in column (3) are estimates of the ratio of the average measured income of the age group in question to the average measured income of all spending units; and the savings mean ratios in column (2) are estimates of the corresponding ratios for savings. From the figures in column (3), it seems plausible to interpret the income ratios as estimates of—or at any rate as correlated with—the ratio of measured income to permanent

[49] See *ibid.*, pp. 220–221.

TABLE 8

Relation of Savings and Income to Age of Head of Spending Unit, United States, 1946

Age of Head of Spending Unit (1)	*Savings Mean Ratio* (2)	*Income Mean Ratio* (3)	*Mean Savings as a % of Mean Income* (4)	*Assumed Mean Age* (5)	*Estimated Mean Lifetime Income Ratio* (6)	*Adjusted Income Mean Ratio* (3) ÷ (6) (7)
18–24	15	57	−3	21.5	144	40
25–34	57	95	6	30	122	78
35–44	136	118	12	40	100	118
45–64	149	116	13	55	74	157
65 and over	55	57	10	70	55	104
All ages	100	100	10		100	100

Source:

Columns (1) to (4) reproduced from Table 10, p. 92 of Janet A. Fisher, "Income, Spending, and Saving Patterns of Consumer Units in Different Age Groups," *Studies in Income and Wealth*, XV (New York: National Bureau of Economic Research, 1952), pp. 77–102. They are based on a reported total of 3,058 cases collected in the Survey of Consumer Finances.

The income " 'mean ratio' was derived by dividing the percentage of total income received by spending units in a particular group by the percentage of the total spending unit population in that age group" (*ibid.*, p. 81). The savings mean ratio was derived in the same way. Savings are here defined to exclude purchases of durable goods, such expenditures being counted as consumption expenditures.

income; measured income might be expected to be less than permanent income in the early and late years of a working lifetime, and to be greater than permanent income in the middle years. On our hypothesis, we would then expect the income ratios in column (3) to be closely correlated with the ratios of measured savings to measured income in column (4). Since consumption is related to permanent rather than measured income, it should be high relative to measured income when measured income is lower than permanent income, and conversely. This relation would be reversed for measured savings, so columns (3) and (4) should be positively correlated. To some extent, the figures correspond with this expectation; the ratio of savings to income is lower for the terminal than for the middle classes. But the correlation is very loose indeed; the income ratio for the terminal class is the same as for the initial class; yet savings are 10 per cent of income for the one and −3 per cent for the other.

The apparent discrepancy between these results and our hypothesis is readily explained. Column (3) shows mean income ratios in a single year for spending units differing in age of head; it cannot be interpreted as showing directly the prospective lifetime income of a single spending unit unless the absolute real income of spending units of a

given age can be expected to remain constant over time. But per capita real income in the United States has risen over at least the past half century at a rate of about 2 per cent per year, and there seems no reason why it should not continue to rise in the future. If it does, the mean lifetime earnings which the youngest group can anticipate is decidedly higher than the average for the whole group covered in the table—the average that is set equal to 100. If, more or less arbitrarily, we assign this average of 100 to age 40, and assume per capita income to continue to rise at the same rate as in the past, the expected mean lifetime earnings of the youngest group would be 144, the figure entered in column (6) of the table. Similarly, the oldest group received a mean average lifetime earnings well below the mean for the whole group; we have estimated its mean as 55, by arbitrarily setting the mean age of the group at 70. The estimates in column (6) provide a rough basis for correcting the ratios in column (3) for the discrepancy that can be expected to arise in a progressive society between the pattern of contemporaneous incomes of units with heads of different age and the lifetime income pattern of a single unit.[50] The resulting rough estimate of the pattern of lifetime earnings is given in column (7).

The percentage of income saved is much more closely correlated with the adjusted income mean ratio than with the original income mean ratio. The consumption expenditures of an age group are adjusted to its own permanent income expectations, not to the average permanent income of a heterogeneous collection of age classes. While the close correlation between columns (4) and (7) is therefore in accord with our hypothesis, the numerical magnitudes of mean savings are not consistent with an interpretation of the hypothesis that would regard the figures in column (6) as the permanent component of income to which expenditures are adjusted. If k is approximately .9, as the average ratio of savings for all ages suggests, if 144 is the permanent component of income for the youngest class, then .9 $\times$ 144 or 130 would be permanent expenditures, so savings would be -90, or -225 per cent of measured income, instead of -3 per cent, and similarly with the other classes. The explanation—or rationalization—is the one referred to at an

[50] The adjustment is crude on many scores. (1) The 2 per cent figure is a rough estimate; (2) the assumed mean ages in column (5) are chosen arbitrarily; they probably should be lower for the two oldest classes and perhaps higher for the two youngest; (3) the assignment of the average income of the group to age 40 is arbitrary; (4) the average of this group is not really the relevant average for lifetime incomes; it is a weighted average of the averages for separate age groups, the weights being the actual percentage of units in an age group at a point of time, whereas the weights should be the fraction of units that can be expected to survive to a particular age. And there are doubtless other crudities as well.

earlier point. The permanent income component is not to be regarded as expected lifetime earnings; it can itself be regarded as varying with age. It is to be interpreted as the mean income at any age regarded as permanent by the consumer unit in question, which in turn depends on its horizon and foresightedness.[51] Accordingly, the entries in column (7) are to be regarded solely as indexes of the ratios of measured income to permanent income; they differ from unity in the same direction as the latter but not necessarily by the same magnitude.

The general consistency of these figures with other evidence from budget studies can be roughly checked by comparing the relation of column (4) to column (7) with corresponding relations for consumer units classified not by age but simply by the ratio of their actual incomes to the mean income of the group. Brady and Friedman plot the ratio of savings to money income against the ratio of measured income to average income for urban families, as computed from budget studies for 1901, 1917–19, 1941, and 1935–36.[52] The points defined by columns (4) and (7) fall closely in line with the results from these budget studies. Even more striking, they come closest to the points for 1917–19, almost precisely duplicating the relation for that year. And of the years plotted, 1917–19 seem most nearly comparable to 1946 in terms of the importance of mean transitory components of income and expenditure; both are at the end of or immediately following a major war and in the midst of a war or postwar inflation.

Further evidence is available from a study by Dorothy Brady based on budget data for a wide range of years in which she derived a relation between consumption expenditures, family income, and the average income of the community in which the family resides. If we interpret the ratio between family income and average income of the community as comparable with the entries in column (7), and modify Brady's function so that it gives the same average percentage saved of 10 per cent, the function yields the following estimates of the percentage of income saved for the successive values in column (7): −8,

[51] See Friedman and Kuznets, *op. cit.*, pp. 355–362. To justify changing permanent incomes with age in a rather different fashion, the expected lifetime income of the youngest class is the discounted value of expected future receipts, not the simple sum of them. If the capital market were perfect, the lifetime income so computed could be regarded as constant over the lifetime by proper positive and negative charges to the depreciation account. But the capital market is imperfect, so that—from the point of view of sums available for expenditures—the rate of interest that must be used is larger for borrowing than for lending and larger for borrowing over a longer term than over a shorter term. The result is that a pattern of actual receipts like that in column (7) would yield a pattern of permanent income components of the same shape but smaller amplitude, even if individual units calculated permanent income on a lifetime basis. A shorter horizon would not change the pattern but would increase the amplitude.

[52] *Op. cit.* p. 261.

+5, +13, +18, +11. The agreement between these estimates and the figures in column (4) is striking, in view of the crudeness of the estimates in column (9) of our table, as well as of the complete independence of the two sources of data.[53]

More recently, data similar to those in Table 8 have been published for Great Britain (Table 9). These data are derived from the Surveys

TABLE 9

Relation of Savings and Income to Age of Head of Income Unit, Great Britain, 1953

	1952		*1953*	
Age of Head of Income Unit (1)	*Income Mean Ratio* (2)	*Mean Savings as a % of Mean Income* (3)	*Income Mean Ratio* (4)	*Mean Savings as a % of Mean Income* (5)
18–24	64	0	62	1.2
25–34	112	1.3	114	.9
35–44	125	0	134	2.9
45–54	130	5.7	126	1.2
55–64	98	1.9	93	2.2
65 and over	57	9.4	55	6.0
All ages	100	1.0	100	1.0

Source:

Harold Lydall, "The Life Cycle in Income, Saving, and Asset Ownership," *Econometrica*, XXIII (April, 1955), pp. 131–50, esp. Tables III, XI, XII. Income is net income (i.e. after personal taxes) throughout. Savings exclude expenditures on durable goods which are treated as consumption expenditures. Column (3) is copied from Lydall's Table XII. Column (4) is computed directly from his Table III, and column (5) from figures on savings in his Table XI and on income in Table III. Column (2) is computed indirectly, since Lydall does not give income figures for 1952. In Table XII, he gives "durable consumption plus total saving" both in absolute amounts and as a percentage of net income. Net income was estimated from the ratio of the former to the latter.

[53] See Dorothy S. Brady, "Family Savings in Relation to Changes in the Level and Distribution of Income," *Studies in Income and Wealth*, XV (New York: National Bureau of Economic Research, 1952), p. 114, where she gives the formula: $\log y = -0.0295 + 0.8 \log x + 0.2 \log z$, where y is consumption expenditures, x is family income, z community income, and the logarithms are to the base 10. If we subtract $\log x$ from both sides, this becomes

$$\log \frac{y}{x} = -0.0295 - 0.2 \log \frac{x}{z}.$$

The constant term implies consumption expenditures of .93 of income when $x/z = 1$. To convert this to .90, the constant term must be $-.0458$, which gives as the equation from which the figures cited in the text were computed:

$$\log \frac{y}{x} = -.0458 - 0.2 \log \frac{x}{z}.$$

of Personal Income and Savings made by the Oxford Institute of Statistics, some of the results of which were used above in the comparison between Britain and the United States.[54] I have not made as detailed an analysis for these data as for the American data, partly because the difference in secular experience in Britain and the United States, particularly in war and postwar years, makes it uncertain how to correct for the secular effect. The British data show the same income pattern as the American data, though with a somewhat larger amplitude. The savings figures are much less regular than the American, though they show the same general tendency for savings to be higher in the middle than in the terminal years. The chief difference from the American figures is the tilt of the age pattern: in the American data, savings are decidedly higher relative to measured income for the oldest spending units than for the youngest; in the British data, they are decidedly higher for the youngest income units than for the oldest; in the American data, the only group with negative savings is the youngest, in the British, the oldest.

I can suggest three possible explanations for this difference, though, unfortunately, the data are not readily available to test their validity or relative importance.

(1) The difference in tilt may in part simply reflect the difference in the dating of the two bodies of data. The American table is for 1946, only one year after the end of the war, the British table, for 1953. Wartime shortages, particularly of durable goods, affected younger units more than older, and the data in Table 8 treat expenditures on durable goods as consumption expenditures. Perhaps a corresponding American table for 1953 would conform more closely to the British pattern.[55]

(2) The difference in tilt may reflect the difference in secular experience. The secular growth in real income per capita has been both larger and more regular in the United States than in Great Britain, particularly in the recent decades that might be expected to count for most in forming expectations. In consequence, less of a correction to the income mean ratios, or even no correction at all, may be required for the secular effect. But this cannot be the whole explanation, since the uncorrected income mean ratios for the first and last classes are too close together to explain the difference between a zero or positive savings percentage and a sizable negative percentage.

(3) The difference in tilt may also reflect a difference in definition of

[54] See Harold Lydall, "The Life Cycle in Income, Saving, and Asset Ownership," *Econometrica*, XXIII (April 1955), pp. 131–150.

[55] I am indebted to John Frechtling for this possible explanation.

units. The American spending unit includes all related persons in a household who are regarded as "pooling" their income; in the British data, "each single person of 18 years or over and each married couple is a separate income unit, children of under 18 being attached to the income unit of their parents or guardians," except that, "in the few cases in which an individual was found to have an income of less than £50 and was living with relatives, he or she was 'amalgamated' . . . into his relations' income unit."[56] As a result, the youngest age group in the British data presumably contains, as separate units, individuals or even young married couples, living with relatives and largely supported by them, who would have been counted as parts of other units in the American data. These units might be expected to have relatively high ratios of savings to measured income: they all have at least £50 of measured income; the recorded measured income presumably does not include the money value of the consumption in kind provided by the relatives with whom they live; what is counted as measured income and their own assets are therefore not the only, and, indeed, not the major means of financing consumption. By contrast, the units in this age group in the American data are mostly living separately and are mainly dependent on their measured income or their own assets to finance consumption. This difference in definition may account not only for the relatively higher saving in the youngest age group shown by the British data but also for the relatively lower saving in the oldest age group. For here the definitional difference may work in the opposite direction: the older units who are counted separately in the British data but as part of other units in the American data may be those who cannot afford to live alone and have moved in with their relatives as an economy move to reduce the drain they are making on their assets. That these definitional effects can be of significant magnitude is shown by the difference between the American and British data in the average size of the units classified in the different age groups: in the middle age group, the number of persons per unit is only slightly larger in the American data, in the younger and older age groups, decidedly larger.[57] It should be noted that the wider amplitude of the income ratios in the British data may reflect this same effect of the difference in definition.

[56] *Ibid.*, p. 134.

[57] Fisher does not give the average number of persons per spending unit in each age group but does give the percentage distributions by number in each spending unit. Janet A. Fisher, "Income, Spending, and Saving Pattern of Consumer Units in Different Age Groups," *Studies in Income and Wealth*, XV (New York: National Bureau of Economic Research, 1952), Table 7, p. 88. The estimates in the following comparisons were computed by treating the class "Five or more" as if it averaged six persons:

4. *The Effect of Change in Income*

It has frequently been suggested that the consumption behavior of consumer units is related to the change in measured income experienced by them in the recent past. Two very different lines of reasoning have been adduced in support of this expectation. One, which is very much in the spirit of our hypothesis, is that the change in income is evidence of the inadequacy of measured income as an index of income status; that a large rise or a large fall in income may imply that current income is "abnormal," whereas a stable income is evidence that current income is "normal." The second implicitly accepts measured income as an adequate index of income status, but supposes that there is a lag in the adjustment of consumption to a change in income status, so that units that have just risen to a given measured income are not adapted to it as fully as those who have been there for some time. The two lines of reasoning lead to similar empirical expectations. Units that have just risen to a given measured income will tend to spend less on consumption than units that have remained at that measured income, and these, in turn, less than units that have fallen to this measured income. But the two lines of reasoning imply rather different hypotheses of consumption behavior and suggest different directions of research into the effect of change in income.

On the permanent income hypothesis, the effect of change in income is to be sought entirely in its implications for the meaning of measured income. A simple example will serve to show that our hypothesis has such implications, and that they lead to fairly precise inferences about the effect of change in income. Consider a group of consumer units which has the same average measured income in two years; suppose that transitory components of income average out to zero in each year, so that average permanent income is equal to average measured income and is also the same in the two years. Let

Age of Head of Unit	*Average Number of Persons Per Unit*	
	U.S.	*Great Britain (1953)*
18–24	1.68	1.24
25–34	3.14	2.80
35–44	3.67	3.52
45–54	2.86	2.67
55–64		2.09
65 and over	2.24	1.44
All ages	2.85	2.36

us classify these consumer units by the change in income from the earlier year to the later and for each such class determine the regression of consumption in the later year on measured income in the same year. What, if anything, can we say on our hypothesis about these regressions and their relation to one another and to a regression computed for the group as a whole?

For simplicity, suppose the permanent component to be the same in the two years for each unit separately as well as for the group as a whole.[58] Any changes in income are then attributable to changes in transitory components. But these transitory components are supposed uncorrelated with the permanent components. Hence groups classified by change in income will not differ systematically with respect to the distribution of permanent components of income; they will all tend to have the same mean permanent component and the same dispersion of the permanent component. But, on our hypothesis, consumption depends only on the permanent component of income, not on the transitory component. Consequently, our hypothesis implies that mean consumption is the same for all income-change classes.

Mean measured income is not, however, the same. The units that experienced a decline in income must have had a smaller transitory component in the second year than in the first; we have just seen that their mean permanent component is the same; accordingly, their mean measured income must be lower in the second year than in the first. Similarly, units experiencing a rise in income must have a larger transitory component in the second year and so a higher mean measured income. Given the same mean measured income for all groups combined in the two years, and symmetry between the two years, measured income in the second year is on the average lower for the units experiencing a decline in income than for those experiencing a rise. Since average consumption is the same, it follows that the regressions of consumption on measured income must differ in height for the different income-change classes, being lower, the larger the algebraic change in income.

What of the slope of the regression? The considerations just adduced make it clear that the members of an income-change group are more alike with respect to transitory components than is the group as a whole. For example, the group experiencing the largest increase in income can have hardly any members who have a large negative

[58] This is not required by the definition of the permanent component. For example, it is noted above that the permanent component may be systematically related to the age of the head of the unit. The inability to take account of such differences in age is one source of error in the empirical examples that follow.

transitory component in the second year. It follows that the variance of the transitory component is smaller for an income-change class than for the group as a whole; but the variance of the permanent component is the same; hence, P_y is greater for an income-change class than for the group as a whole. Our hypothesis therefore implies that the consumption-income regression tends to be steeper for a group of units that have experienced the same income change than for all units.

FIGURE 10

Hypothetical Regressions for Groups Classified by Change in Income (average change assumed zero for group as a whole)

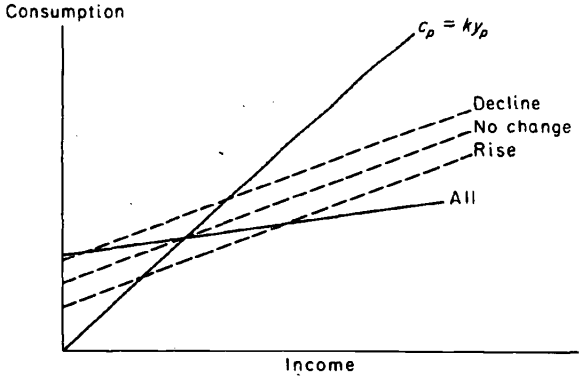


Figure 10 displays the relations among the regressions to which our hypothesis leads. This figure does not show the common average consumption for the different groups.

The preceding discussion has been deliberately phrased so as to apply to either the arithmetic or logarithmic variant of our hypothesis. For the arithmetic variant, the units are to be regarded as classified by the absolute amount of the change in income; for the logarithmic variant, by the percentage change in income.

The Appendix to this chapter presents a more detailed analysis of the implications of these two variants of our hypothesis. This analysis is, in one respect, more general than that given above: it allows for a change in the average permanent component between the two years, provided the change is of the same absolute amount (arithmetic variant) or the same percentage amount (logarithmic variant) for each consumer unit; and it does not assume transitory components to average out to zero. In another respect it is less general: it makes special assumptions about the distribution of the transitory components in the two years (that they are jointly normally distributed with the same variance in the two years). This analysis confirms and

extends the preceding conclusions. Perhaps the most interestir general extension is that the slope of the regression for a given chan in income is the same whatever may be the magnitude of the chan in income; i.e. that if regressions like the dashed lines in Figure were computed for a series of values of the change in income (or fc equally broad income-change classes) they would be parallel. This the justification for making them parallel in Figure 10. The analys leads also to specific formulas expressing the slope and intercept c the regression for a particular income-change class in terms of para meters for the group as a whole, the size of the income change, and characteristic of the income structure. We shall use these formulas i interpreting available data on the effect of change in income.

I have examined in some detail the consistency of our hypothes with two bodies of data: (1) Data for something over 600 farm familie which have been used by Ruth Mack to analyze the effect of chan in income.[59] These data were collected by the Farm Security Admini tration and are for the years 1940 to 1942.[60] (2) Data for nearly 5,00 spending units collected in the Survey of Consumer Finances an used by James N. Morgan to analyze the effect of change in income. These data are for 1948 and 1949.

In addition, I have examined rather more casually the consistenc of the hypothesis with data for nearly 400 farm families in Tennesse for 1943 and 1944. Like the other two bodies of data, these confor reasonably closely to the implications of the hypothesis, though the differ from it in one respect.[62]

[59] Ruth P. Mack, "The Direction of Change in Income and the Consumption Fun tion," *Review of Economics and Statistics*, XXX, No. 4 (November 1948), pp. 239–25

[60] See Willard W. Cochrane and Mary D. Grigg, *The Changing Composition of Fami Budgets for Selected Groups of Corn Belt Farmers*, Department of Agriculture, Burea of Agricultural Economics (Washington, October 1946).

[61] See Katona, Klein, Lansing, Morgan, *op. cit.*, pp. 153–154.

[62] See Jean L. Pennock and Elisabeth L. Speer, *Changes in Rural Family Income a Spending in Tennessee, 1943–1944*, Miscellaneous Publication No. 666, Department Agriculture (March 1949).

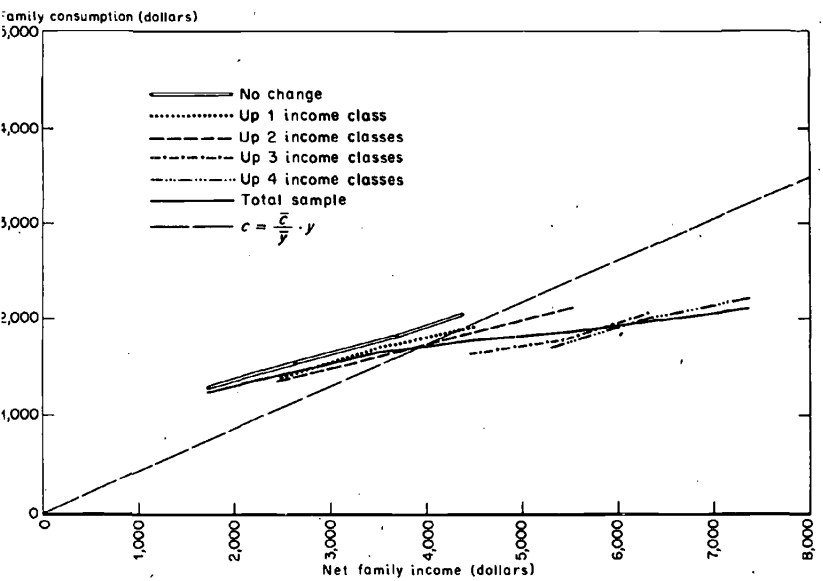
The chart on page 8 of this publication is like Figures 11 and 12 that follow except th it is for only three income-change groups (decreased, constant, increased), and does n contain any relation for all combined. The plotted values for the three groups wou clearly yield three very nearly parallel regressions, steeper than the regression for a groups and less steep than a relation between permanent components. The one respe in which there is a discrepancy from the predictions of our hypothesis is that t regression for the constant income group, instead of being between the other two, is the right of the regression of the increased income group. A possible explanation is th this discrepancy reflects the use of a definition of consumption that includes expenditur on durable consumer goods. Perhaps the higher expenditures at each measured incor level of the families that experienced an increase in income than of the families that h constant income simply reflect greater expenditure on consumer durables. Unfortu ately, the published data are not adequate to test this explanation, though the small b of relevant information in Table 7, page 9, are consistent with it.

1. THE FSA DATA

Figure 11 is reproduced from Ruth Mack's article, except that I have added a line through the mean of the sample and the origin to represent $c_p = ky_p$ or the relation between the permanent components.[63] It will be seen that Figure 11 corresponds closely with our hypothetical Figure 10, if allowance is made for the difference between them in the average change in income for which they are drawn. Figure 10 was drawn for a group that had the same average

FIGURE 11

Regression of Family Consumption on Income for Farm Families, Five Income-Change Groups, 1942 (income change from 1940 to 1942)



Source: Ruth P. Mack, "The Direction of Change in Income and the Consumption Function," *Review of Economic Statistics*, November 1948, p. 242.

income in the two years, so it is the "no change" line that goes through the intersection of the regression for all units and the relation between permanent components. Figure 11 is for a group that had an increase in average income of $1,618. The "up two income classes" subgroup comes closest to having the same average change—the average change for it is $1,910—so the regression for it corresponds to the "no change" line in Figure 10 and goes through the intersection of the two relations for the group as a whole. As our hypothesis predicts, the

[63] I am indebted to Ruth Mack for making her worksheets available to me.

regressions for individual income-change classes are roughly parallel, steeper than the regression for the group as a whole, and less steep than a line through the origin and the mean point of the group.

Table 10 gives a more exacting comparison between the observed data and predictions derived from our hypothesis. Because the units

TABLE 10

Comparison of Observed and Predicted Results for Income-Change Classes, Farm Security Administration Sample of Farm Families
(*dollar figures in thousands*)

	Slope of Regression		*Income at Intersection of Regression for Class and for All Families*		
Income-Change Class	*Observed*[a]	*Predicted*[b]	*Observed*[a]	*Predicted*[c]	*Average Consumption*[d]
No change	.29	.23	$1.9	$1.6	$1.5
Up 1 income class	.27	.23	2.7	2.8	1.6
Up 2 income classes	.25	.23	3.1	4.2	1.7
Up 3 income classes	.23	.23	5.9	5.8	1.7
Up 4 income classes	.24	.23	6.7	7.3	1.9
All classes	.15				1.6

[a] Computed from graphic fit to points plotted in Figure 11.

[b] Computed from equation (4.26) in the Appendix, with $r_{tt'}$ set equal to zero. The observed ratio of mean consumption to mean income was used for k, and the slope of the graphically fitted regression to all families for b.

[c] Computed from equation (4.27) in the Appendix, using the numerical values in the preceding note, together with values of $(d - \bar{d})$ computed from Willard W. Cochrane and Mary D. Grigg, *The Changing Composition of Family Budgets for Selected Groups of Corn Belt Farmers*, Department of Agriculture, Bureau of Agricultural Economics (Washington; October 1946).

[d] Computed from data in *ibid.*

Note: Regressions are for 1942 and income change is from 1940 to 1942. Income class is $1,000 in width, except for top open-end class.

are classified by the absolute change in income, we have had to use the arithmetic variant of our hypothesis in making predictions. Again the general agreement is excellent: our hypothesis predicts a slope of .23 for the separate income-change-class regressions; the actual slope varies from .23 to .29; there is a very close family resemblance between the observed and predicted incomes at the intersection of the separate and the over-all regressions.

Yet there are also some minor differences in detail. (1) The formula from which the predicted slope is computed is for a hypothetical class all members of which have precisely the same income change; the actual classes include a range of income changes approximately $1,000 wide; the effect should be to make the observed slope lower than the predicted slope; yet the observed slopes are uniformly higher. (2) The observed intersection incomes differ systematically from the predicted

being less widely dispersed. (3) Another reflection of the same phenomenon is the final column showing average consumption. If the arithmetic model as developed in the Appendix applied fully to these data, these averages would be identical; yet they display a decided upward trend.

Deviations (2) and (3) reflect a shortcoming in a supplementary assumption we found it necessary to incorporate in our model, rather than a real deviation from the basic hypothesis. The group as a whole experienced a substantial increase in income from 1940 to 1942—an increase of over $1,600 or nearly 75 per cent of the 1940 level. Our arithmetic model assumes that any change in permanent income is the same *absolute* amount for all units. This might not be a bad approximation for a small change in average income and so presumably also in permanent income. For a change as large as 75 per cent in average income, and presumably of a similar order of magnitude in permanent income, it would be preferable to regard the change in permanent income as the same *percentage* for all units—this is clearly so for the something like 20 per cent increase that must be attributed simply to a rise in prices, and seems reasonable for the rest as well. But on this interpretation, the absolute amount of income change is not uncorrelated with the size of the permanent income; the larger the absolute change in income, the larger on the average the absolute change in permanent income, and hence the larger the average permanent income. The rising average consumption in the final column of Table 10 presumably reflects this rising average permanent income; so also does the lesser dispersion of the observed points of intersection than of the predicted points, for the effect of the rising average permanent income is to bring the several regressions closer together.

One factor that may partly account for deviation (1) is that the mean transitory component of consumption is probably negative. One of the parameters used in computing the predicted slope is k, the ratio of mean permanent consumption to mean permanent income. I have used the observed ratio of average consumption to average income for the group as a whole as an estimate of k. It seems clear that this is too low. 1942 was a wartime year when consumption restrictions were beginning to be felt; the mean transitory component of consumption was almost certainly negative, and of income, almost certainly positive. In another connection, an estimate is made below (see Chapter VI, section 4) of the mean transitory component of consumption for this group. Use of the corresponding k raises the predicted slope, but from .23 only to .24, so this factor accounts for only part of deviation (1).

b. THE SURVEY OF CONSUMER FINANCES DATA

The Consumer Finances data are for a much larger sample—nearly 5,000 units, approximately half of whom reported for 1947, the rest for 1948. The data are mostly for urban spending units but presumably include some farm units.[64] Data for the two years are pooled in the analysis. For each year, the income-change class is determined by the reported income change from the preceding year. These data have one advantage over the FSA data in addition to the larger sample: the units are classified by percentage change in income, and so we can use the logarithmic variant of our hypothesis, the variant that in general we have found to fit the data rather better. But they also have one serious disadvantage: the basic data are nowhere published in anything like the same fullness. The analysis that follows is based mainly on some unpublished data kindly made available to me by James Morgan; unfortunately, even these are not extensive enough for our purposes, so I have had to resort to rather arbitrary measures to shape the data to the needs of our analysis.

Figure 12 is an adaptation of a chart published by Morgan.[65] As on his chart, points for the final income class, $7,500 and over, are omitted. The reason is that the values used for income are midpoints of class intervals (except for the class $5,000 to $7,500 for which $6,000 is used), computed averages apparently not being available, and the use of any single value for the upper open-end class would be arbitrary and subject to undue error. I have added to the chart the two lines for all classes combined: the line labeled *A*, which is the assumed relation between permanent components, the value of k being taken as the ratio of average consumption to average income for the sample as a whole; and the line labeled *B*, which is an estimated regression of the logarithm of consumption on the logarithm of income.[66] It will be seen that Figure 12, like Figure 11, corresponds

[64] Morgan does not specify the exact coverage of the sample or its relation to the samples he used earlier in the same chapter, but I infer from the numbers involved that it must include farm and business units.

[65] *Op. cit.*, chart IV, p. 154. Morgan devotes considerable attention to the effect of income change. But most of his analysis uses the residuals from consumption-income regressions, and so, as noted in section 2f above, is of little value. He does give some regressions between savings and income for income-change classes (pp. 155 and 160). But these are for subgroups classified by amount of liquid assets. The analysis of the Appendix clearly does not apply directly to such subgroups, and I have not been able to construct a plausible variant that would. This is why I do not discuss these results.

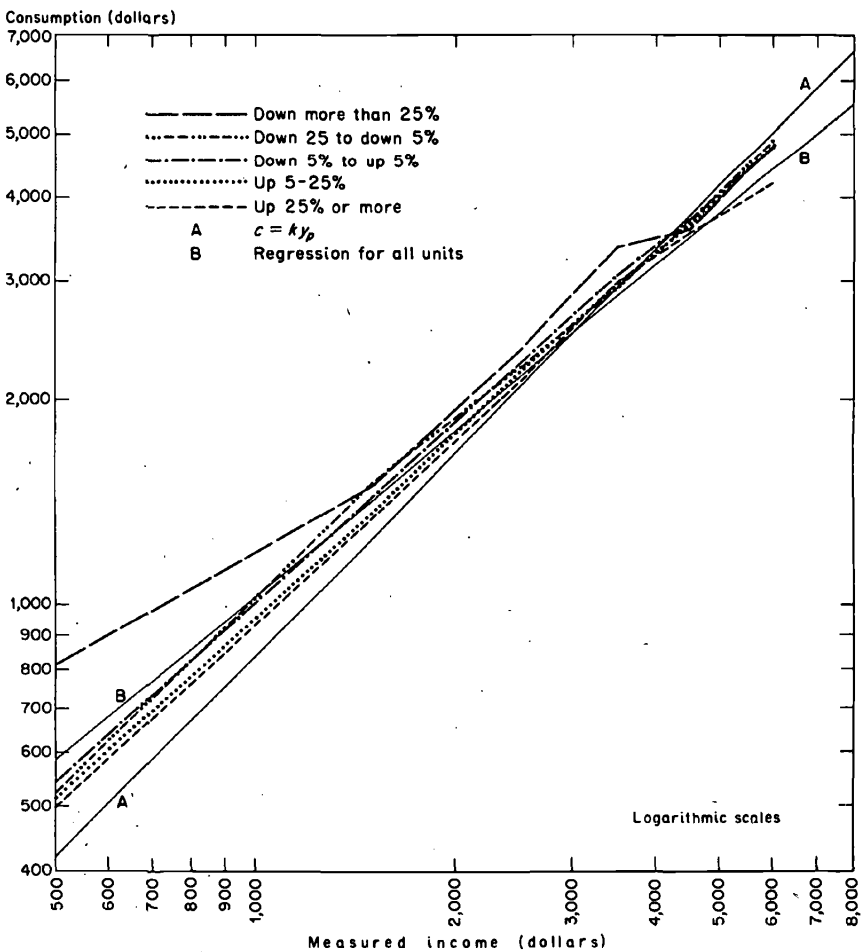
[66] Since I did not have data for all families on mean income and mean consumption for individual income classes, this regression was determined by a roundabout process. Among the data Morgan made available were the sums, sums of squares, and sums of cross-products of the arithmetic values of income and savings. From these was computed the least-squares straight line between the arithmetic values of income and consumption and the elasticity of this regression at the arithmetic mean income; this was used as the

very closely with our hypothetical Figure 10. The one exception is for the class whose income fell more than 25 per cent; the regression for this class is abnormally high and flat.

Table 11 presents a numerical comparison between observed and

FIGURE 12

Regressions of Consumption on Income for Five Income-Change Groups, Based on Survey of Consumer Finances Data, 1947 and 1948 (income change from preceding year)



Source: Adapted from James N. Morgan, in *Contributions of Survey Methods to Economics*, Columbia University Press, 1954, Chart IV, p. 154.

slope of *B*. The geometric mean of consumption and income was approximated from these data and the intercept of *B* determined so that the line *B* would pass through the corresponding point.

TABLE 11

Comparison of Observed and Predicted Results for Income-Change Classes, Based on Survey of Consumer Finances Data, 1947 and 1948
(dollar figures in thousands)

Income-Change Class (1)	*Measured Income Elasticity of Consumption*		*Income at Intersection of Regression for Class and for All Families*		*Arithmetic Average Consumption*[d] (6)	*Standard Deviation of*		
	Observed[a] (2)	*Predicted*[b] (3)	*Observed*[a] (4)	*Predicted*[c] (5)		*Measured Income*[e] (7)	*Permanent Component of Income*[f] (8)	*Transitory Component of Income*[g] (9)
Down more than 25%	.77	.89	$19.6	$ .1	$1.9	$1.42	$1.25	$.68
Down 25% to down 5%	.87	.89	1.6	.6	2.5	2.36	2.20	.85
Down 5% to up 5%	.88	.89	1.4	1.6	2.4	2.26	2.11	.80
Up 5% to up 25%	.89	.89	2.3	3.5	2.8	2.15	2.03	.71
Up 25% or more	.87	.89	5.3	8.9	2.5	2.55	2.37	.93
All classes	.82				2.5	2.28	2.06	.98

[a] For income-change groups, elasticity is slope of log-log straight line fitted graphically to points plotted in Figure 12; for all classes, elasticity at arithmetic mean income of arithmetic straight line fitted to original data by least squares. I am grateful to James Morgan, of the Michigan Survey Research Center, for making available the basic data for this computation and the others referred to in these notes. This elasticity is used because the data for a graphic log-log fit were not available to me. As a check, corresponding elasticities were computed for the several income-change classes; the results, in the order in which the classes are listed in the above table, were: .77, .88, .86, .81, .77. The regression for all classes used in computing the points of intersection and plotted in Figure 12 is a log-log straight line with slope equal to .82 and going through a point corresponding to the geometric mean income and consumption. These in turn were estimated from the corresponding arithmetic means and variances.

[b] Computed from formula (4.20′) in the Appendix to this chapter. $r_{YY'}$ was set equal to .828, the correlation between the logarithms of incomes in 1947 and 1948 for urban spending units of 2 or more persons in the Survey of Consumer Finances reinterview sample (see below, Table 18, item 8).

[c] Computed from formula (4.23′). The values of D were taken, rather arbitrarily, as the logarithms of .65, .85, 1.00, 1.15, and 1.35 for the several income-change classes, and of $\overline{D}$ as the weighted average of these values, the weights being the number of units in each class.

[d] Computed from the data made available by Morgan.

[e] Computed from sums and sums of squares of absolute incomes made available by Morgan.

[f] Square root of product of entry in column (2) and square of standard deviation of measured income.

[g] Square root of product of complement of entry in column (2) and square of standard deviation of measured income.

Note: Sample combines observations for 1947 and 1948, so regressions are for average of 1947 and 1948. Income change is from preceding year (1946 to 1947 or 1947 to 1948). Regressions are logarithmic.

predicted values. Except for the income-change class just referred to, observed elasticities are very close to the predicted, and even the deviations conform to expectation. The observed elasticity should be lower than the predicted because the predicted is for a single value of income change, the observed, for a class containing a range of values. For the same reason, the observed elasticity should fall below the predicted by a larger amount, the broader the income-change class, as, with one exception, it does.

The observed and predicted incomes at the intersection of the regressions for each class and for all classes combined agree much less well. Again, the only very large deviation is for the group whose income fell more than 25 per cent, but the remaining values are considerably more discrepant than the corresponding values for the FSA sample. I have not been able to construct any plausible explanation for this discrepancy.

The average consumption of the income-change classes, given in Column (6), conforms more closely to expectation than the corresponding column for the FSA sample. Except for the first class, which is a deviant throughout, average consumption is much the same for the various income-change classes and shows no systematic relation to income-change.

For these data, we can extend the test to the estimated dispersion of the components of income for the separate income-change classes, the estimates of absolute dispersion in columns (8) and (9) being derived from the observed elasticities in column (2) and the computed standard deviations of measured income in column (7). We have seen that, on the permanent income hypothesis, the dispersion of the permanent component of income should be roughly the same for the separate income-change classes, and the same for each as for the group as a whole; whereas the dispersion of the transitory component should be less for each income-change class than for the group as a whole. The data clearly conform closely to these predictions, except again for the deviant class, "down more than 25 per cent," which has an unusually low dispersion both in total and for each component separately.

Why should the "down more than 25 per cent" class be so consistently a deviant? I have no fully satisfactory answer. But, in part, this deviant behavior reflects a defect of the analysis enforced by the absence of data, namely, failure to take into account the age of the heads of the consumer units. For a group of the same age it is reasonable to suppose, as our analysis does, that the members of the group retain the same relative permanent income status from one year to the next. This is much less reasonable for a group differing in

age; those not yet at their peak income period are climbing the relative income status ladder, those past their peak are descending. Data are available for 1948 on the age distribution of the heads of units in the various income-change classes. These show a disproportionately large number of heads 65 years of age or older in two income-change classes: "down 5 per cent to up 5 per cent" and "down more than 25 per cent."[67] Presumably the former retired some time ago and in the survey year were receiving a stable retirement income; they did not experience any significant change in permanent income status and so cause no difficulty in the analysis. The older heads in the "down more than 25 per cent" class, on the other hand, must mostly have retired or partly retired during or just prior to the survey year; the decline in their income is in substantial part a decline in permanent income. The effect is to make the average permanent income for the "down more than 25 per cent" class lower than for the other classes, and so to make average consumption lower, as it is according to column (4). It seems unlikely, however, that this effect can account for the whole difference between average consumption for this class and the other classes. The difference is of the order of 20 per cent, which implies a difference of the same magnitude in average permanent income; yet units with heads of 65 or over account for only about 18 per cent of all units in this income-change class in 1948 and units with heads of 55 or over for only 35 per cent. Similarly, while this effect may help to explain why the measures of dispersion in columns (7), (8), and (9) are so low for this class, it seems unlikely that it can account for it in full.

Expressed as percentages of mean income, the standard deviations for all classes are 75 per cent, 68 per cent, and 32 per cent, for measured income, permanent component, and transitory component, respectively. These estimates are very close to those for all nonfarm or urban families in 1935–36 and 1941 in Table 4 above. I have not computed similar figures for the separate income-change classes because mean measured income for such a class cannot be regarded as an approximation to mean permanent income.

C. THE SIGNIFICANCE OF THE COMPARISONS

There is clearly a very close correspondence between the observed effect of classifying consumer units by change in income and the effect predicted by the permanent income hypothesis. This correspondence goes beyond general features and carries over to rather

[67] "1950 Survey of Consumer Finances, Part III," *Federal Reserve Bulletin* (August 1950), Table 3.

precise numerical characteristics. Two points need to be emphasized in judging the significance of these results.

(1) The correspondence, particularly in numerical characteristics, is in some ways the most striking bit of evidence for our hypothesis that has so far been adduced. It is so partly because the comparisons between observation and prediction are for fairly precise implications of the hypothesis that could readily have been contradicted; partly, because these implications have not heretofore been drawn from other hypotheses and some had not even been established as empirical generalizations. These comparisons therefore both demonstrate the fecundity of the hypothesis in generating implications and provide new fields on which to try out the hypothesis.

(2) The acceptance of our hypothesis in interpreting these data does not mean that change in income is not an important variable for consumption analysis. Our hypothesis explains why it should be an important variable and what effect it can be expected to have. The consistency of the data with our hypothesis means that the effect of change in income need not be regarded as evidence of any meaningful "lag" in the reaction of consumer units to changes in circumstances; on the contrary, change in income can be regarded as having an effect precisely because it cannot be taken as a valid change in circumstances, because a large change in income is—on the average—a sign that measured income is affected to an unusual extent by transitory factors.

Appendix to Section 4:
The Effect of Change in Income on the Regression of Consumption on Income

Consumption data classified by change in income generally take one of two forms: (1) like the FSA data, they are classified by the *absolute amount* of the change in income; (2) like the Survey of Consumer Finances data, they are classified by the *percentage* change in income. It turns out to be convenient to make slightly different assumptions in deriving the implications of our hypothesis for these two types of data: to assume for (1) that any change in permanent income between the years for which the income change is recorded is of the same absolute amount for every consumer unit in the group considered; to assume for (2) that it is of the same percentage.

1. PERMANENT INCOME CHANGE OF SAME ABSOLUTE AMOUNT

Let y = measured income in the year in question
y' = measured income in the earlier year from which the change in income is calculated

$d = y - y' =$ change in income
$c =$ measured consumption in year in question
$a =$ intercept of regression of c on y for all units
$b =$ slope of regression of c on y for all units

Let a subscript d to any of these symbols (other than d itself) represent the corresponding variable for a group of units for which d is the same, i.e. for an income-change class.

The problem is to determine the values of a_d and b_d.

From our earlier analysis,

$$b = kP_y \tag{4.1}$$

$$a = \bar{c} - b\bar{y} . \tag{4.2}$$

By strictly similar reasoning,

$$b_d = k_d P_{yd} \tag{4.3}$$

$$a_d = \bar{c}_d - b_d \bar{y}_d . \tag{4.4}$$

By assumption, the relation between permanent components is the same for every unit, so

$$k_d = k . \tag{4.5}$$

Given that the change in the permanent component of income is the same absolute amount for all units, any differences among units in the value of d are accounted for by differences in the transitory component of income. But, on our hypothesis, the transitory component of income is uncorrelated with the permanent component of income and hence of consumption and also with the transitory component of consumption. It follows that consumption is uncorrelated with the value of d, whence

$$\bar{c}_d = \bar{c} . \tag{4.6}$$

We can thus restrict attention to evaluating P_{yd}, and $\bar{y}_d$, or features of the income distribution. Now

$$P_y = \frac{\sigma_p^2}{\sigma_y^2} , \tag{4.7}$$

where σ_p^2 is the variance of the permanent component of income and σ_y^2 is the variance of measured income. Similarly

$$P_{yd} = \frac{\sigma_{pd}^2}{\sigma_{yd}^2} . \tag{4.8}$$

But under our assumptions,

$$\sigma_{pd}^2 = \sigma_p^2 , \tag{4.9}$$

since differences in d reflect only differences in transitory components, which are assumed uncorrelated with permanent components. The distribution of permanent components, like the distribution of consumption, is the same for a group of units with the same d for all values of d and the same as for all units together. It follows that

$$P_{yd} = \frac{\sigma_y^2}{\sigma_{yd}^2} \cdot P_y , \tag{4.10}$$

so our problem reduces to determine $\bar{y}_d$ and σ_{yd}^2.

For simplicity, assume that y and y' are jointly normally distributed. Then y and $d = y - y'$ are also jointly normally distributed. Hence, $\bar{y}_d$ is a linear function of d, say

$$\bar{y}_d = e + f(d - \bar{d}). \tag{4.11}$$

The parameters are given by

$$f = \frac{E(y - \bar{y})(d - \bar{d})}{E(d - \bar{d})^2} , \tag{4.12}$$

$$e = \bar{y} . \tag{4.13}$$

But since $(d - \bar{d}) = (y - \bar{y}) - (y' - \bar{y}')$,

$$f = \frac{\sigma_y^2 - r_{yy'}\sigma_{y'}\sigma_y}{\sigma_y^2 - 2r_{yy'}\sigma_y\sigma_{y'} + \sigma_{y'}^2} . \tag{4.14}$$

Assume that $\sigma_y = \sigma_{y'}$. Then

$$f = {}^1/_2 , \tag{4.15}$$

so

$$\bar{y}_d = \bar{y} + {}^1/_2(d - \bar{d}). \tag{4.16}$$

We now want to compute

(4.17)

$$\begin{aligned} \sigma_{yd}^2 &= E(y - \bar{y}_d)^2 = E[(y - \bar{y}) - {}^1/_2(d - \bar{d})]^2 \\ &= E[{}^1/_2(y - \bar{y}) + {}^1/_2(y' - \bar{y}')]^2 = {}^1/_4(\sigma_y^2 + 2r_{yy'}\sigma_y\sigma_{y'} + \sigma_{y'}^2) . \end{aligned}$$

Again assuming $\sigma_y = \sigma_{y'}$

$$\sigma_{yd}^2 = {}^1/_2\sigma_y^2(1 + r_{yy'}) . \tag{4.18}$$

Substituting in (4.10),

$$P_{yd} = \frac{2P_y}{1 + r_{yy'}} . \tag{4.19}$$

Substituting (4.5) and (4.19) in (4.3), and using (4.1),

$$b_d = \frac{2k \cdot P_y}{1 + r_{yy'}} = \frac{2b}{1 + r_{yy'}} . \tag{4.20}$$

Substituting (4.6) and (4.16) into (4.4) and using (4.2),

$$\begin{aligned} a_d &= \bar{c} - b_d\bar{y} - {}^1/_2 b_d(d - \bar{d}) \\ &= a + \bar{y}(b - b_d) - {}^1/_2 b_d(d - \bar{d}) . \end{aligned} \tag{4.21}$$

In application, a_d is likely to be subject to a very large sampling error, so it is better to use some index of height which is nearer the middle of the range of incomes observed. This could be consumption at the mean income which is simply

$$c(\text{at } \bar{y}) = \bar{c} - {}^1/_2 b_d(d - \bar{d}) . \tag{4.22}$$

Another alternative is to determine the value of y at which the regression for a given value of d intersects the regression for the group as a whole. This intersection value of y is given by

$$y = \frac{a_d - a}{b - b_d} = \bar{y} - \frac{1}{2}\left(\frac{b_d}{b - b_d}\right)(d - \bar{d}) . \tag{4.23}$$

We can convert (4.20) into a somewhat different form by getting a different expression for $r_{yy'}$. Let p' stand for the deviation of the permanent component in year 1 from its mean value, t' for the corresponding deviation of the transitory component, and p and t for the same variables in year 2. By our assumptions, $p = p'$. Then

$$r_{yy'} = \frac{E(p + t')(p + t)}{\sigma_y \sigma_{y'}} = \frac{\sigma_p^2 + r_{tt'}\sigma_t\sigma_{t'}}{\sigma_y \sigma_{y'}} , \tag{4.24}$$

taking into account the zero correlation between p and t and p and t'. Again, assume $\sigma_y = \sigma_{y'}$. Also assume $\sigma_t = \sigma_{t'}$. We then have

$$r_{yy'} = P_y + (1 - P_y)r_{tt'} . \tag{4.25}$$

Substitute (4.25) into (4.20) and also replace P_y by its equivalent b/k. The result is

$$b_d = \frac{2kb}{k + b + (k - b)r_{tt'}} . \tag{4.26}$$

Substitute (4.26) into (4.23). The resulting intersection value of y is

$$y = \bar{y} + \frac{k}{(k - b)(1 - r_{tt'})}(d - \bar{d}) . \tag{4.27}$$

Although t and t' are taken as uncorrelated with p, there is nothing

in our hypothesis that requires them to be uncorrelated with one another. Whether they are depends on the length of the horizon implicit in permanent income and on the lapse of time between the two years considered. If the horizon were two years in length, t and t' would be uncorrelated even if the years considered were adjacent; if it is three years, they would not be uncorrelated for adjacent years because the unit would regard some effects lasting more than one year as transitory, but they would be uncorrelated for years separated by one year. And so on.

For the FSA data covered by Figure 11, the change in income is between two years with one year intervening. Evidence is presented later which suggests that a horizon of three years is a good approximation. If this is so, for these data $r_{tt'}$ can be taken to equal zero, which puts (4.26) and (4.27) into particularly simple form.

One important implication of (4.20) or (4.26) which is independent of the precise numerical value it yields is that the slopes of the regressions for given values of d are independent of d—i.e. that the regressions are parallel. Another implication is that if $k > b$, then $k > b_d > b$, i.e. that the regression for a given value of d is steeper than for the group as a whole and less steep than the relation between the permanent components.

It should be emphasized that our analysis is for a given value of d. The effect of grouping units for which d is between two values will be to give a result intermediate between b and b_d, so that in general (4.20) or (4.26) may be expected to overestimate the observed slopes.

2. PERMANENT INCOME CHANGE OF SAME PERCENTAGE

The preceding case was constructed to correspond with the FSA data, which are classified by the absolute change in income. This case is constructed to correspond with the Consumer Finances data, which are classified by the percentage change in income.

Accordingly, let

$$y_p = my'_p . \tag{4.28}$$

Take logarithms of both sides, letting capital letters stand for logarithms of corresponding lower case letters:

$$Y_p = M + Y'_p . \tag{4.29}$$

Let

$$D = Y - Y' . \tag{4.30}$$

Consider now the logarithmic variant of our hypothesis, for which

$$B = P_Y \tag{4.1'}$$

$$A = \bar{C} - B\bar{Y} = K + \bar{C}_t - \bar{Y}_t P_Y + \bar{Y}_p(1 - P_Y) . \tag{4.2'}$$

Correspondingly

(4.3′) $$B_D = P_{YD}$$

(4.4′) $$A_D = \bar{C}_D - B_D \bar{Y}_D .$$

As before,

(4.6′) $$\bar{C}_D = \bar{C}.$$

If we suppose Y and Y' to be jointly normally distributed, this case reduces to the preceding case and we can write down directly:

(4.16′) $$\bar{Y}_D = \bar{Y} + {}^1/_2(D - \bar{D}),$$

(4.20′) $$B_D = P_{YD} = \frac{2P_Y}{1 + r_{YY'}} = \frac{2B}{1 + r_{YY'}},$$

(4.21′) $$A_D = A + \bar{Y}(B - B_D) - {}^1/_2 B_D (D - \bar{D}),$$

(4.22′) $$C(\text{at } \bar{Y}) = \bar{C} - {}^1/_2 B_D (D - \bar{D}).$$

The intersection value of Y is

(4.23′) $$Y = \bar{Y} - \frac{1}{2}\left(\frac{B_D}{B - B_D}\right)(D - \bar{D}).$$

Once again

(4.25′) $$r_{YY'} = P_Y + (1 - P_Y) r_{TT'},$$

so

(4.26′) $$B_D = \frac{2B}{1 + B + (1 - B) r_{TT'}}$$

and the intersection value of Y is

(4.27′) $$Y = \bar{Y} + \frac{1}{(1 - B)(1 - r_{TT'})}(D - \bar{D}).$$