Volume Title: The Personal Distribution of Income and Wealth

Volume Author/Editor: James D. Smith, ed.

Volume Publisher: NBER

Volume ISBN: 0-870-14268-2

Volume URL: http://www.nber.org/books/smit75-1

Publication Date: 1975

Chapter Title: Introduction to "The Personal Distribution of Income and Wealth"

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Chapter URL: http://www.nber.org/chapters/c3748

Chapter pages in book: (p. 1 - 8)

Introduction

James D. Smith

This volume contains papers presented at the annual meeting of the Conference on Research in Income and Wealth held on the campus of Pennsylvania State University in the fall of 1972.

The meeting and the one to follow it in 1974 reflect a renewed interest in the distributions and determinants of income and wealth. The papers in this volume focus on income and wealth defined in traditional monetary terms. The 1974 follow-up conference will extend the scope of inquiry to include non-monetary dimensions.

In large measure, the resurgence of researcher interest in personal distributions of income and wealth has resulted from increased availability of microdata and the sustained methodological efforts by Orcutt and others demonstrating the superiority of microdata approaches to the estimation of many socially relevant or intellectually interesting models of behavior.

With respect to data, the decade of the sixties saw a rich harvest of microdata, reflecting the desire of policymakers to estimate in advance and measure in retrospect the consequences of social programs. Early in the sixties, the Board of Governors of the Federal Reserve System produced the Survey of Financial Characteristics of Consumers and the Survey of Changes in Financial Characteristics. By mid-decade, the Department of Labor was at work on the National Longitudinal Surveys, and the Office of Economic Opportunity had begun work on the Surveys of Economic Opportunity (1966 and 1967). The Office of Economic Opportunity, in conjunction with the Survey Research Center of the University of Michigan, also began collecting data from an ongoing panel of families in an effort known as the Panel Study of Income Dynamics. Microdata from all of these studies were made available to researchers. Near the end of the decade, Internal Revenue Service (IRS) tapes of income tax returns (without names or street addresses) became available to researchers who needed them. By the beginning of the decade of the 1970s, the IRS had made available microdata from estate tax returns, and the Census Bureau was creeping toward the release of a standard version of

the Current Population Survey which could be purchased by researchers who were not among the superrich.

The release of public data by government agencies has not been achieved easily, but the benefits, as evidenced by the papers in this volume, justify the effort. The push to make microdata available came from many researchers inside and outside of government. Raymond Bowman in his many years at the Bureau of the Budget contributed to this effort, as did Richard Ruggles, Guy Orcutt, Joseph Pechman, Benjamin Okner, Robert Levin, Harold Watts, and Ralph Nader, among others, outside of government.

Opposition to the release of microdata often has been argued in terms of confidentiality. The principle of confidentiality has both positive and negative aspects. We can easily agree that the protection of individuals is often well served by priviledged information, but when applied to the innocuous data of Census and tax records after names and street addresses have been removed, the argument rings false. Indeed, secrecy may harbor worse evils than those it is intended to prevent. One of the serious consequences of microdata privy only to the government agency collecting it is that the situation offers a tremendous incentive for "Watergating" errors which, understandably enough, occur in large-scale data-collection efforts. The social benefits of accessible microdata include an incentive to agencies to do first-rate work, many eyes to uncover elusive, but often serious, errors which can escape even very careful workers, and greater use of information that is usually collected at great cost. In any event, it is clear that research using microdata is on the ascendency, and that government agencies and private researchers are finding ways to reap the social benefits of microdata without injury to citizens who provide information.

The first chapter in this volume has very little to do with data per se but, rather, is concerned with economic methodology in the broadest sense—that is to say, with meta-economics. It is characteristic Boulding, piercing through the fine polish of technique to the important questions: What is it all about? What does it mean? The author forces reflection:

> Considering the enormous symbolic importance of the concept [equality], surprisingly little work has been done on it, philosophically, theoretically, or empirically. Perhaps the reason for this is that it is too painful, too contradictory, too confusing, and too important to be the object of anything but rhetoric.

And what of the complement of equality: inequality? Is there a meaningful quantitative interpretation of that? Boulding suggests that we have named the measure rather than measured the named. Gini coefficients, standard deviations, means of algebraic sums or absolute differences, ranges—bah, humbug! It is all a myth. The inequality of a distribution is a psychological phenomenon, a measurable but unmeasured perception of beholders. He suggests mental mapping may be the path to a better understanding. Social psychology in the Sherifian tradition may or may not provide the conceptual basis of quantitative measures, but, in any event, reading the essay is delightful medicine for cerebral sclerosis.

Part II of the volume contains three papers on redistributive mechanisms. The first paper (Chapter 2) by Robert Lampman, deals with measuring the redistributive impact of tax and transfer systems within the National Income Accounts framework. Starting with the basic proposition that every economy has within it mechanisms for redistributing its national product among consumers, he asks how the redistributive process can be more meaningfully monitored within the National Income Accounts than it currently is. In answer, he suggests that the household sector be subdivided to show insurance and pensions, philanthropic organizations, and families' subsectors, and that direct interfamily transfers be identified. These changes would permit the accounts to quantify the relative importance of transfers between years and between nations.

He urges government to make such changes in the official accounts, but suggests the profession should be prepared to move forward on its own.

In Chapter 3, Ben Okner imaginatively manipulates a synthetic population (the MERGE File) created from the 1966 personal income-tax model and the 1967 Survey of Economic Opportunity. Using an assortment of income concepts, Okner addresses a number of questions dealing with the distribution of tax burden a la Musgrave ("The Distribution of Tax Burden by Income Group: A Case Study for 1949," *National Tax Journal,* Vol. 4, March 1954), and with the combined redistributive impact of taxes and transfers.

Looking only at the tax burden, his results are similar to those of Musgrave, which appeared two decades ago: essentially, there is little progressivity in federal, state, and local tax systems taken as a group. Such progressivity as does exist is to be found far out in

the right tail. Thus, the combined personal tax structure tends to be proportional with respect to income and, consequently, does not per se contribute significantly to income redistribution.

Transfers, on the other hand, significantly improve the income position of persons in the lower tail of the pretransfer income distribution, but they also contribute importantly to the upper tail.

Using an income concept which comes close to being consumption minus taxes plus changes in net worth, he found a 13 percent reduction in the Gini coefficient due to the combined influence of taxes and transfers.

In his early work on tax burdens, Musgrave had been forced to work with aggregated data, which precluded estimation of the tax burden for subpopulations. Using the microdata developed from the tax and survey data mentioned above, Okner was able to demonstrate that there are substantial differences in the effect of tax-transfer systems on subpopulations. For instance, where he found a 13 percent reduction in the Gini coefficient for all families, a 30 percent reduction for families with a head age 65 or over was discovered.

In Chapter 4, Harold Watts and Jon Peck use the MERGE File to simulate the redistributive consequences of a number of variants of a tax function combining a constant marginal rate and a fixed credit. They compare the before and after well-being of a set of six "family" types. The family typology, obviously selected for its relevance to the problem of welfare reform, consisted of:

1. aged individuals;
2. families with aged heads;
3. families with female heads;
4. nonaged individuals;
5. families with nonaged male heads and 2 to 5 members; and
6. families with nonaged male heads and 6 or more members.

The authors examine the before and after tax, income status of these units, and more interestingly, the before and after tax distribution of the welfare ratio (income/poverty threshold).

They conclude that the extant tax structure is differentially beneficial to the aged and to female-headed units, and that

working poor families headed by males are significantly overtaxed or undertransferred. This is a finding of both economic and political significance.

Part III deals with accounting periods of lengths other than the traditional one-year period. It is noteworthy that two of the chapters in this part use large microdata sets assembled outside the federal government, and that the third uses microdata produced cooperatively by the government and Herbert Parnes.

Martin David and Roger Miller, relying on a decade of professional investment in the development of a file of Wisconsin tax returns which links families between years and individuals to their parental families, examine the importance of capital gains on the size distribution of income. They find little difference in long versus shorter accounting periods, which can be explained by the inclusion of capital gains. The reason adduced is that capital gains tend to be a recurring form of income for those who receive them.

Kohen, Parnes, and Shea (Chapter 6) examine income instability among two groups of men: those aged 14 to 24 in 1966 and a group aged 45 to 59 in the same year. The microdata used by them was collected in a joint effort by Parnes, the Department of Labor, and the Bureau of the Census and has become known as the National Longitudinal Surveys. They develop a measure of relative income instability (RIC) which is intended to measure the relative instability of an individual's income change vis-á-vis the mean change in income of his cohort.

They found that the instability of income rank was greater for blacks and for younger men generally than for whites and men in the 45 to 59 year age group.

Among whites, earnings were more stable than total family income. While the same relation was found among the older black group, the reverse was true for young blacks.

As has been found in other studies, a regression toward the mean was found when changes in income rank were compared to initial rank.

Jacob Benus and James Morgan use three sets of panel data collected by the Survey Research Center to study the influence of the unit of analysis, the accounting period, and the income concept on the size distribution of income. The first of these data sets was produced in a study of the response of consumers to the 1964 tax cut. Families were interviewed quarterly in that study and thus provided a basis for looking at short-term changes in

income. The second set of data were collected from about 1,400 respondents in four interviews spaced approximately a year apart. The third data set was the Panel Study of Income Dynamics, a panel study now in its fifth year and still going on. The authors find that the unit of analysis and the concept of income have greater influence on the size distribution than does the length of the accounting period. In terms of the influence of other factors on income stability, occupation, age, and race—in that order—were found to be important.

There were no formal discussants in the session from which these papers came, but each participant was asked to provide, within the time allotted to him, such observations upon the papers of the other participants as he felt would be useful. Martin David provided a set of comments reflecting on his own joint effort with Roger Miller and on the other two papers presented in this part. David's comments are included as Chapter 8.

The three chapters of Part IV are concerned with the distribution of wealth. Lee Soltow presents estimates of the distribution of wealth, income, and social class of men in large northern cities in 1860 (Chapter 9); A. B. Atkinson writes of the distribution of wealth in England in 1968; and James Smith presents a study of the distribution of wealth in Washington, D.C., in 1967.

The Soltow piece starts with the microdata of the 1860 Census and incorporates it into a model which permits one to predict the probability of escape from a state of propertylessness—a state which was not as uncommon as one might suspect. According to Soltow's data, slightly over half of the adult males were without assets; and according to his model, the probability of remaining in a propertyless condition in a given year was .96.

Soltow also presents a model which employs a Pareto-rectangular income distribution and a conventional savings function to determine the 1860 wealth distribution.

Atkinson (Chapter 10) provides the latest available statistics of the distribution of wealth in Britain. He uses the estate multiplier method of estimating the number of wealth holders and their asset holdings in 1968.

His paper is particularly valuable not simply because it extends a long series of wealth estimates for Britain which have been made using the estate multiplier method, but because it examines the sensitivity of the estimates to a methodology which has been

increasingly employed in the United States and in other parts of the world.

Atkinson presents evidence that the concentration of British wealth is not decreasing: the conventional wisdom that the richest 1 percent own one-third, and the richest 5 percent one-half, of the total wealth is more nearly correct than some recently published estimates which suggest that there has been a significant decrease in inequality of wealth in Britain. A discussion of Atkinson's paper by Kathleen Langley, herself one of the pioneers in British wealth estimates, is included at the end of Chapter 10.

Using the same technique as Atkinson, but concerning himself with Washington, D.C., Smith (Chapter 11) estimates that the total wealth of all residents of the District of Columbia was $5.5 billion in 1967. However, his focus is not upon the aggregate level of wealth or its size distribution among all residents, but upon the differences in wealth holdings of whites and blacks. He found blacks to have an average net worth of $1,000, while whites averaged $19,000. He also found that 96 percent of the black population had a net worth of under $5,000, about the same figure that Soltow's work suggests for the population as a whole in 1860.

A discussion of Smith's paper by Vito Natrella is included at the end of Chapter 11.

In Part V, two papers which served as the focal point of a session on the quality of data on income and wealth are presented. One of the papers deals with a retooled model of the former OBE income distribution series, which for many years presented family-income size distributions aligned to personal income aggregates in the National Income Accounts (NIA), but which was subjected to a major overhaul in 1962. The other paper is a test of the quality of transfer income data reported in the *Current Population Reports.* The two papers are related by the fact that they both have the Current Population Survey (CPS) as a focal point; the latter paper directly and the former indirectly, because the CPS is the basic source of income information in the redesigned OBE series.

In Chapter 12, Dorothy Projector and Judith Bretz present the results of tests of the reliability of three types of transfer reported in the CPS: (1) Social Security and railroad retirement; (2) public assistance; and (3) unemployment and workmen's compensation. They compared the incidence of reported incomes of each type of

the CPS against a simulated incidence based upon the characteristics of families and individuals in the CPS file. They also compared the CPS reported receipts of transfers to administrative tallies of the numbers of persons receiving such transfers and their value.

Projector and Bretz found the characteristics of CPS respondents reporting transfer incomes to be consistent—or at least not inconsistent—with eligibility criteria established for specific programs. They concluded, using personal income in the National Income Accounts as a standard, that all types of transfer payments were understated in the Current Population Survey. Comparing program aggregates, the CPS was found to understate the value of transfer payments by about 8 percent. The understatement was found to be greater for younger age groups than for those over 65.

In Chapter 13, Edward Budd and Daniel Radner provide a guided tour of the statistical labyrinth they negotiate in producing the BEA income size-distribution series. The BEA series is intended to produce income size-distributions which are conceptually consistent with personal income in the National Income Accounts and such that the sum of the distributed incomes is equal to the personal income aggregate in the NIA.

The old OBE (now BEA) income size-distribution series was discontinued in 1962 because benchmark data used to relate tax and survey data had become suspect, and because the art of income size-distribution estimating was trailing far behind the science of data processing.

Budd and Radner have spent several years incorporating microdata techniques into the BEA series. They have been hampered by lack of access to direct matches of tax return and survey data, but they have made imaginative use of statistical matching procedures to overcome this impediment.