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Unequal we stand: An empirical analysis of economic inequality in the United States, 1967–2006 [☆]

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ABSTRACT

We conduct a systematic empirical study of cross-sectional inequality in the United States, integrating data from the Current Population Survey, the Panel Study of Income Dynamics, the Consumer Expenditure Survey, and the Survey of Consumer Finances. In order to understand how different dimensions of inequality are related via choices, markets, and institutions, we follow the mapping suggested by the household budget constraint from individual wages to individual earnings, to household earnings, to disposable income, and, ultimately, to consumption and wealth. We document a continuous and sizable increase in wage inequality over the sample period. Changes in the distribution of hours worked sharpen the rise in earnings inequality before 1982, but mitigate its increase thereafter. Taxes and transfers compress the level of income inequality, especially at the bottom of the distribution, but have little effect on the overall trend. Finally, access to financial markets has limited both the level and growth of consumption inequality.

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1. Introduction

The evolution of economic inequality in the United States has been extensively studied. One branch of the literature has focused on the wages of full-time men, using data from the March Current Population Survey (CPS). This work aims to describe the evolution of dispersion in productivity and skills, and to trace its macroeconomic sources to changes in technology, trade, or institutions (see Katz and Autor, 1999, for a survey). Another branch of the literature has focused on labor supply, studying, for example, how changes in female participation affect measures of economic inequality (see Cancian and Reed, 1998). Other authors have emphasized that the extent to which increasing dispersion is permanent or transitory in nature has important implications for policy and welfare, and have investigated income dynamics using the longitudinal dimension of the Panel Study of Income Dynamics (PSID) (e.g., Gottschalk and Moffitt, 1994). This shift from studying the sources of rising inequality toward exploring its welfare implications continues with papers investigating the dynamics of inequality in household consumption, a more direct measure of well-being, using the Consumer Expenditure Survey (CEX) (e.g., Cutler and Katz, 1991).

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Although much has been learned from these studies, the literature lacks a systematic analysis of US cross-sectional inequality that jointly examines all the key measures of economic inequality: wages, hours, income, consumption, and wealth. In this paper, we try to fill this gap, using comparable samples from the most widely used household-level data sets. Our key organizing device is the household budget constraint, which provides a natural framework for understanding how different dimensions of inequality are related via endogenous choices, markets, and institutions. We begin with changes in the structure of individual wages as our most primitive measure of inequality, and from there take a series of steps to contrast inequality in individual wages to that in individual earnings, household earnings, pre-government income, disposable income, and, ultimately, consumption and wealth. Along the way, we evaluate the impact on measured inequality of individual labor supply, household income pooling, private transfers and asset income, government redistribution, and household net saving.

Our empirical analysis of inequality for the United States should serve as a useful input to the quantitative macroeconomic research aimed at understanding how individual-level risk affects the distribution of economic outcomes. With a sharper characterization of the facts, structural models can be more confidently applied to exploring the cross-sectional relationship between risk and outcomes—already the subject of a large literature in quantitative macroeconomics (e.g., Imrohorglu, 1989; Huggett, 1993; Aiyagari, 1994; Ríos-Rull, 1995; Castañeda et al., 2003; Storesletten et al., 2004a).

We now briefly summarize our key substantive findings.

Inequality in individual wages rises steadily from the early 1970s for men and from the early 1980s for women. However, dispersion in hourly wages increases mostly at the bottom of the wage distribution in the 1970s, throughout the distribution in the 1980s, and at the top after 1990.

Shifting the focus from wage to earnings inequality, we detect a strikingly important role for labor supply. First, the cross-sectional variance of log male earnings increases much more rapidly than the variance of log male wages until the mid-1980s. The reason is that relative hours worked for low-skilled men declined in the 1970s as unemployment rose sharply, exacerbating earnings inequality at the bottom. Second, the age profile for wage inequality is concave, while that for earnings inequality is convex. This difference reflects the fact that hours dispersion is high for young workers (due to high unemployment), declines for prime-age workers, and then rises again for old workers (due to early retirement).

Going from individual to household earnings, we find that household earnings inequality increases less than earnings inequality for the main earner at the top of the distribution, but not at the bottom. Moving from earnings to disposable income, taxes and public transfers compress inequality dramatically. They are also an important buffer against rising earnings inequality in the 1970s.

The final step in tracing out the household budget constraint is from disposable income to consumption. The gap between the two is informative about the smoothing role of borrowing and saving. We examine this key relationship from three different viewpoints. First, in the time series, we find that cross-sectional inequality in non-durable consumption increases by less than half as much as inequality in disposable income. Second, we find an analogous result in the life-cycle dimension: only a fraction of the age increase in within-cohort dispersion in income translates into dispersion in consumption. Third, by exploiting the longitudinal dimension of the PSID, we can distinguish the relative importance of permanent and transitory shocks: the former are more likely to pass through to consumption, the latter are more easily insurable. Here, we focus on the volatility of “residual” wages, which most closely reflect idiosyncratic and unforeseen labor market fluctuations. We detect a rise in the permanent variance in the decade 1975–1985, precisely the period when cross-sectional consumption inequality rises the most.

We also investigate directly the dynamics of wealth inequality in the Survey of Consumer Finances (SCF) and show that the Gini coefficient for net worth increases by 5 points from 1983 to 2007.

Finally, when we focus on the dynamics of inequality at higher frequencies, we find that cyclical fluctuations in CPS per-capita income are twice as large as in National Income and Produce Accounts (NIPA) personal income. Thus, viewed through the lens of micro data, business cycles are more dramatic events. Household earnings at lower percentiles of the income distribution decline very rapidly in recessions, such that recessions are times when earnings inequality widens sharply. Since we do not find similar dynamics for individual wages, we conclude that the root of such large fluctuations in earnings cyclicalities is labor supply—especially unemployment.

Our paper makes three contributions that are more methodological in nature.

First, we check whether the CPS, CEX, and PSID tell a consistent story with respect to various measures of cross-sectional dispersion. We find that, with the exception of two discrepancies that we discuss in this paper, they align closely with respect to wages, hours, earnings, and disposable income. This finding is reassuring, since it means that researchers can estimate individual income dynamics from the PSID, or measure consumption inequality in the CEX, and safely make comparisons to cross-sectional moments from the much larger CPS sample.

Second, we show that combining income or consumption data from the CPS, PSID, or CEX with wealth data from the SCF can be misleading, since the SCF contains more high-wealth and high-income households. We find that dropping the top 1.47% of the wealth distribution in the SCF yields a sample that is comparable to the other three surveys. This adjustment reduces the ratio of mean wealth to mean pre-tax income—a common calibration target for heterogeneous-agent macro models—from 4.5 to 3.3.

Finally, we demonstrate that a standard permanent-transitory model for individual wage dynamics appears misspecified, since it cannot jointly replicate cross-sectional moments for wages in levels, and corresponding moments for wages in first-

differences. Domeij and Flodén (2010, this issue) and Fuchs-Schündeln et al. (2010, this issue) report a similar finding for Sweden and Germany.

The rest of the paper is organized as follows. Section 2 describes our three primary data sources: the CPS, the PSID, and the CEX. Section 3 compares measures of per-capita income and consumption in the NIPA to those constructed from the surveys. Section 4 describes the trends of US cross-sectional inequality over time. Section 5 focuses on the life-cycle dimension. Section 6 provides a detailed comparison of several measures of inequality across the three data sets. Section 7 exploits the panel dimension of the PSID to estimate the transitory and permanent components of individual wage dynamics. Section 8 explores wealth data from the SCF. Section 9 concludes. Many details of the empirical analysis are omitted from the main text and collected in Appendices A–D.

2. Three data sets

In this section, we describe our three main data sets: the CPS, the PSID, and the CEX. Appendices A–D contain more detail on each survey and a discussion of comparability across data sets. A brief description of the SCF is contained in Section 8.

2.1. CPS

The CPS is the source of official US government statistics on employment and unemployment, and is designed to be representative of the civilian non-institutional population. The Annual Social and Economic Supplement (ASEC) applies to the sample surveyed in March, and extends the set of demographic and labor force questions asked in all months to include detailed questions on income. For the ASEC supplement, the basic CPS monthly sample of around 60,000 households is extended to include an additional 4500 Hispanic households (since 1976), and an additional 34,500 households (since 2002) as part of an effort to improve estimates of children's health insurance coverage: this is the "SCHIP" sample.

The basic unit of observation is a housing unit, so we report CPS statistics on inequality at the level of the household (rather than at the level of the family).¹ The March CPS contains detailed demographic data for each household member and labor force and income information for each household member age 15 or older. Labor force and income information correspond to the previous year. We use the March supplement weights to produce our estimates.

2.2. PSID

The PSID is a longitudinal study of a sample of US individuals (men, women, and children) and the family units in which they reside. The PSID was originally designed to study the dynamics of income and poverty. For this purpose, the original 1968 sample was drawn from two independent sub-samples: an over-sample of roughly 2000 poor families selected from the Survey of Economic Opportunities (SEO), and a nationally representative sample of roughly 3000 families designed by the Survey Research Center (SRC) at the University of Michigan.

Since 1968, the PSID has interviewed individuals from families in the initial samples. Adults have been followed as they have grown older, and children have been observed as they have advanced into adulthood, forming family units of their own (the "split-offs"). Survey waves are annual from 1968 to 1997, and biennial since then. The PSID is the longest-running representative household panel for the United States.

The PSID data files provide a wide variety of information about both families and individuals, with substantial detail on income sources and amounts, employment status and history, family composition changes, and residential location. Although some information is collected about all individuals in the family unit, the greatest level of detail is ascertained for the primary adults in the family unit, i.e., the head (the husband in a married couple) and the spouse, when present.

We base our empirical analysis on the SRC sample. We use all the yearly surveys (1967–1996) and the biennial surveys for 1999, 2001, and 2003. Since the SRC sample was initially representative of the US population, the PSID does not provide weights for this sample. The primary concern about the representativeness of this sample is that it does not capture the post-1968 inflow of immigrants to the United States. We return to this point in Section 6.

2.3. CEX

The CEX consists of two separate surveys, the quarterly Interview Survey and the Diary Survey, both collected for the Bureau of Labor Statistics by the Census Bureau. It is the only US data set that provides detailed information about household consumption expenditures. The Diary Survey focuses only on expenditures on small, frequently purchased items (such as food, beverages, and personal care items), whereas the Interview Survey aims at providing information on up to 95% of the typical household's consumption expenditures. In this study, we will focus only on the Interview Survey (see Attanasio et al., 2007, for a study that uses both the Diary and the Interview surveys).

¹ A "household" is defined as all persons, related or unrelated, living together in a dwelling unit. The "family unit" is defined as all persons living together who are usually related by blood, marriage, or adoption. For example, a household can be composed of more than one family.

Table 1
Sample selection in the PSID, CPS, and CEX.

	PSID (1967–1996, 1998, 2000, 2002)		CPS (1967–2005)		CEX (1980–2006)	
	Dropped	Remaining	Dropped	Remaining	Dropped	Remaining
Initial Sample		123,788		2,217,997		638,237
Missing/miscoded household info ^a	1516	122,272	8993	2,209,004	104,302	533,935
Implausible consumption ^b					2723	531,212
Pos. labor inc. and zero hours	299	121,973	17,599	2,191,405	179	531,033
Wage < 0.5 × minimum wage	4298	117,675	121,367	2,070,038	47,046	483,987
Sample A		117,675		2,070,038		483,987
Head age 25–60	32,322	85,353	524,609	1,545,429	137,356	346,631
Sample B		85,353		1,545,429		346,631
Total individuals age 25–60 in sample B ^c		147,540		2,578,035		552,683
Individuals age 25–60 with hours >260	30,164	117,376	599,544	1,978,491	97,574	455,109
Sample C		117,376		1,978,491		455,109

^a In the CEX this category includes households classified as incomplete income respondents. In the PSID it includes households with missing labor income and positive hours worked.

^b In the CEX this category includes households which report non-positive total consumption expenditure (67), households which report non-positive expenditures on non-food consumption (118), and households which report quarterly expenditures on food of less than \$100 in 2000 \$ (2538).

^c In the PSID individuals are only either heads or wives.

The CEX Interview Survey is a rotating panel of households that are selected to be representative of the US population. It started in 1960, but continuous data are available only from the first quarter of 1980, which is the start of our sample. Each quarter the survey reports, for the cross section of households interviewed, detailed demographic characteristics for all household members, detailed information on consumption expenditures for the three-month period preceding the interview, and information on income, hours worked, and taxes paid over a yearly period.² Each household is interviewed for a maximum of four consecutive quarters.

2.4. Sample selection

In each of our three data sets, we construct three different samples, which we label Samples A, B, and C. Table 1 shows the number of records in each data set that are lost at each stage of the selection process.

Sample A is the most inclusive and is essentially a cleaned version of the raw data. We drop records only if 1) there is no information on age for either the head or spouse, 2) either the head or spouse has positive labor income but zero annual hours (zero weeks worked in the CPS), or 3) either the head or spouse has an hourly wage less than half of the corresponding federal minimum wage in that year. In the CEX, we also drop households reporting implausible consumption expenditures.³ In order to reduce measurement error in income and hours, we also exclude CEX households flagged as “incomplete income reporters” (see Nelson, 1994) and PSID households if labor income is missing but hours worked are positive. Sample A is designed to be representative of the entire US population and is used for Figs. 1 and 3, where we compare per capita means from micro data to NIPA aggregates.

Sample B is further restricted by dropping a household from Sample A if no household member is of working age, which we define as between the ages of 25 and 60 (in the PSID we drop households if neither the head nor the spouse, when present, falls in this age range). The household head is the oldest working age male, as long as there is at least one working-age male in the household; otherwise the head is the oldest working-age female. Sample B is our household-level sample and is used for Figs. 2 and 8–14.

Sample C instead is an individual-level sample. To construct it, we first select all individuals age 25–60 who belong to households in Sample B. From this group we then select those who work at least 260 hours during the year. Sample C is used for Figs. 4–7 and 15–18.

Table 2 reports statistics on some key demographic characteristics for Sample B. The table indicates broad agreement across data sets, both in terms of levels and with respect to demographic trends over time. One exception is that the fraction of white males is declining over time in the CPS and the CEX but is stable in the PSID. This reflects higher attrition for non-whites in the PSID, coupled with the fact that the PSID misses disproportionately-non-white recent immigrants. In addition, a significantly larger fraction of households (families) in the PSID contain married couples, suggesting that the PSID under-samples non-traditional households.

² See Appendix C for more details on the issue that income and consumption measures refer to periods that are never of the same length and that are, in some cases, non-overlapping.

³ Specifically, when quarterly equivalized food consumption is below \$100 in 2000 dollars. In the PSID, we categorize records as implausible when either (i) equivalized food consumption is below \$400 per year, (ii) food stamps exceed \$50,000, or (iii) food expenditures exceed ten times disposable income. In such cases, we drop households, but only when computing moments involving food consumption.

Table 2
Selected demographic characteristics of Sample B in the PSID, CPS, and CEX.

	1967–1979		1980–1989			1990–1999			≥2000		
	PSID	CPS	PSID	CPS	CEX	PSID	CPS	CEX	PSID	CPS	CEX
Avg. household size	3.32	3.33	2.98	2.98	2.99	2.89	2.89	2.88	2.77	2.82	2.86
% households with spouse	78.1	72.0	73.1	62.2	62.5	68.9	57.4	58.2	67.3	54.9	56.7
Avg. male age	41.2	41.0	39.7	39.7	39.4	40.9	40.3	40.3	42.1	41.7	41.9
Avg. female age	39.1	41.3	37.9	40.0	39.6	39.3	40.6	40.2	40.7	42.0	41.9
% white male	88.2	89.3	89.6	86.8	87.5	89.2	84.5	86.8	87.9	82.5	84.3
% male ≥16 years edu	23.6	19.4	30.3	26.0	27.8	33.5	28.0	31.3	33.9	30.2	31.0
% female ≥16 years edu	14.4	11.9	22.2	19.1	19.1	27.3	24.6	26.1	30.1	29.8	29.2

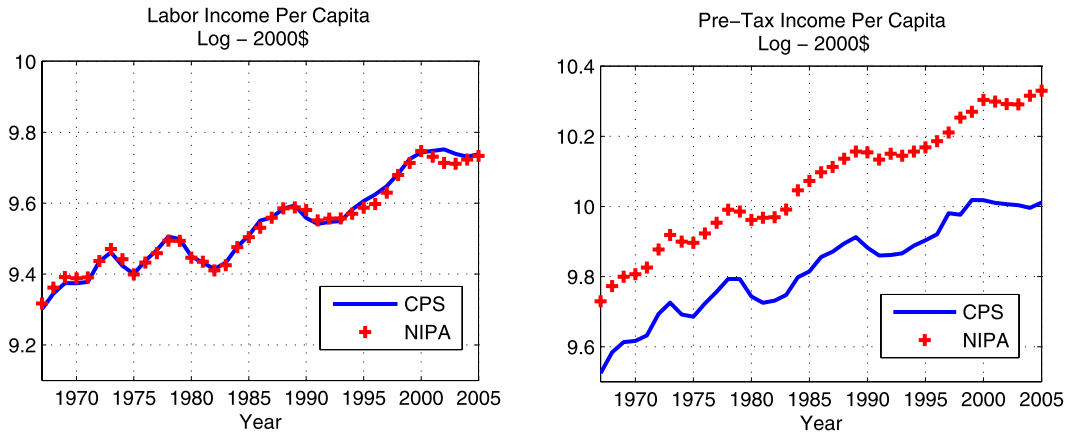


Fig. 1. Comparison between averages in CPS and NIPA: labor and pre-tax income.

Throughout the paper, we express all income and expenditure variables in year 2000 dollars. The price deflator used is the Bureau of Labor Statistics CPI-U series, all items.

In all data sets, we forecast mean values for top-coded observations by extrapolating a Pareto density fitted to the non-top-coded upper end of the observed distribution. We apply this procedure separately to each component of income in each year. Top-coding affects very few observations in the PSID but is a more serious concern in the CPS and the CEX.

3. Means

We begin by comparing the evolution of average household earnings, income, and consumption in our micro data to the official Bureau of Economic Analysis (BEA) NIPA, over the period 1967–2005.

Labor income. The income definition that is conceptually most similar across the CPS and the NIPA is labor income (wage and salary income, excluding self-employment income).⁴

The left panel of Fig. 1 compares labor income in the CPS to the NIPA. Both series are per capita, real and logged.⁵ Labor income aligns remarkably well, in terms of levels, trends, and business cycle fluctuations. On average across the 1967–2005 period, the CPS statistic exceeds its NIPA counterpart by 0.27%. The average absolute discrepancy is 1.1%. In the early 1990s, CPS labor income rises somewhat more rapidly than in the NIPA, a finding previously noted by Roemer (2002). Conversely, in the early 2000s the decline in CPS labor income is less evident than in the NIPA.⁶

Pre-tax income. The CPS measure of pre-tax income includes labor income, self-employment income, net financial income, and private and public transfers. This is our version of the “money income” concept constructed by the Census. Labor income

⁴ The NIPA labor income measure is “wage and salary disbursements” (NIPA Table 2.1, line 3). Two minor differences between the CPS and NIPA measures are worth noting (Ruser et al., 2004). The first is that the BEA classifies as dividends all S corporation profits distributed to shareholders, whereas the Census treats these profits as wage and salary income if the recipients are shareholder-employees. The second is that the BEA (but not the CPS) makes an upward adjustment for wage and salary income earned in the underground economy from legal but “off the books” activities.

⁵ The US population estimate is from NIPA Table 7.1, line 16.

⁶ The reliability of CPS labor income reporting is confirmed by Roemer (2002), who matches individuals in the March CPS to detailed earnings records from the Social Security Administration (DER). He finds that part-year, part-time workers have underestimated March CPS wages (CPS/DER ratio around 90%), but that for all other groups, wages align very closely.

alone accounts for fully three-quarters of total CPS pre-tax income. The corresponding NIPA income measure is “personal income” (NIPA Table 2.1, line 1). The two measures are reported in the right panel of Fig. 1.

Even though the long-run trends in these two measures line up well, on average across the sample period, CPS income falls 21% short of NIPA income. In light of the previous discussion, this gap must be attributed to income other than labor income. The NIPA-CPS gap widens over time, by around 10 percentage points of NIPA income. There are several reasons for this gap.

First, there is a downward bias in the CPS income series arising from internal censoring of high income values: our treatment of externally top-coded observations described in Appendices A–C should largely correct for this problem.⁷

Second, there is an important conceptual difference between survey-based income measures and NIPA income. The surveys record cash income received directly by individuals, whereas the NIPA records cash and in-kind income collected on behalf of individuals.⁸ The “by” versus “on behalf of” distinction means that dividends, interest, and rents received on behalf of individuals by pension plans, nonprofits, and fiduciaries is in NIPA income but not survey income. The “cash” versus “cash and in-kind” distinction means that employer contributions for employee pension and health insurance funds are in NIPA income but not survey income. Employer contributions of this type rose from 4.3% of NIPA personal income in 1967 to 9.0% in 2005, explaining a large part of the widening NIPA-CPS gap.⁹

In addition to these conceptual differences, an additional gap between the NIPA and survey-based estimates arises because survey respondents tend to under-report a range of types of income, while the BEA attempts in a variety of ways to make upward adjustments for components of income that are self-reported.¹⁰

Cyclical fluctuations. The CPS mirrors the business cycle fluctuations evident in the NIPA income series. However, cyclical fluctuations appear larger in the CPS than in the NIPA. From peak to trough, percentage real income declines in the CPS (NIPA) for the recessions in the mid-1970s, early 1980s, early 1990s and early 2000s are 3.9 (2.2), 6.6 (2.9), 5.1 (2.3), and 2.2 (1.3). Even though recession declines in per-capita pre-tax income are roughly twice as large in the CPS, declines in wages and salary income are very similar in magnitude. Thus, the difference in business cycle dynamics must be attributed to unearned components of income. Future work should more precisely characterize the reason for this discrepancy. Once the reasons are understood, it will become clear whether any specific macroeconomic model should target, or use as an input, the NIPA-based or the survey-based measure of average income. In the meantime, it is important to be aware that macro and micro data paint different pictures for the size of cyclical fluctuations.¹¹

Wages and hours. Fig. 2 plots average wages and hours over the sample period.¹² Wages are computed as annual earnings divided by annual hours, where earnings includes labor income plus two-thirds of self-employment income.¹³

The average real wage for women rises by 36% over the period. In contrast, the corresponding increase for men is only 14%, with real wage declines in the 1970s and 1980s recouped in the 1990s. Business cycle fluctuations are evident in both average wage series.

Average male hours decline in the 1970s and are broadly stable thereafter.¹⁴ In contrast, female market hours increase dramatically in the 1970s and 1980s, as female wages rise relative to male wages. This growth in female participation slows in the 1990s, at the same time that male wage growth resumes.

The growing importance of women in the labor market is central to reconciling stagnant real hourly wages and hours worked for male workers (Fig. 2) with rising per capita labor income (Fig. 1). Over the sample period, two-thirds of the growth in labor income per capita is attributable to growth in female labor income per capita. Rising female labor income, in turn, reflects both rising average hours for women and rising average labor income per hour. Of the two, the former is

⁷ At the start of the sample period our CPS estimate for per-capita income exceeds the official Census series by over 7%. This gap narrows to less than 1% towards the end of the period as the Census increased internal censoring points. For example between 1992 and 1993, when the censoring point for earnings on the primary job rose from \$300,000 to \$1m, the gap narrows from 5.3% to 2.5%.

⁸ Table 1 in Ruser et al. (2004) provides a careful and detailed account of the differences. They find that in 2001, 64% of the \$2.23 trillion gap between aggregate NIPA personal income and aggregate CPS money income can be accounted for by differences in income concepts.

⁹ Similarly the NIPA includes (but the surveys exclude) the imputed rental value of owner-occupied housing and in-kind transfers such as Medicare, Medicaid, and food stamps. In the other direction, the surveys include but the NIPA excludes personal contributions for social insurance, income from private pension and annuities plans, income from government employee retirement plans, and income from interpersonal transfers, such as child support.

¹⁰ For example, the proprietors' income adjustment is based on evidence that proprietors' actual income in 1999 was more than double levels reported on tax returns. Ruser et al. (2004) note that it is likely that respondents who under-report to the IRS also under-report in voluntary surveys. Comparing various components of income across the CPS and other independent estimates, Ruser et al. note that under-reporting in the CPS seems to be important for private and government retirement income, interest and dividend income, and social security income.

¹¹ The higher cyclicality of mean income in survey data relative to National Accounts is not a unique feature of the United States. It is also present, for example, in German and UK data. See Fuchs-Schündeln et al. (2010) and Blundell and Etheridge (2010) in this issue.

¹² The estimates of average hours in Fig. 2 are based on all 25–60 year-old individuals in Sample B, including those working zero hours. Average wages apply to Sample C, which excludes individuals working less than 260 hours in the year.

¹³ Prior to income year 1975, CPS information on hours—and thus wages—is not ideal because the question about weekly hours refers to hours worked last week (rather than usual weekly hours). Moreover, information about weeks worked in the previous year is available only in intervals prior to 1975. We have used information for years in which both measures of hours are available to splice together estimates for the 1967–1974 period and those for the later period.

¹⁴ Our CPS estimates align very closely by year and age group with the decennial Census-based estimates of McGrattan and Rogerson (2004, Table 3).

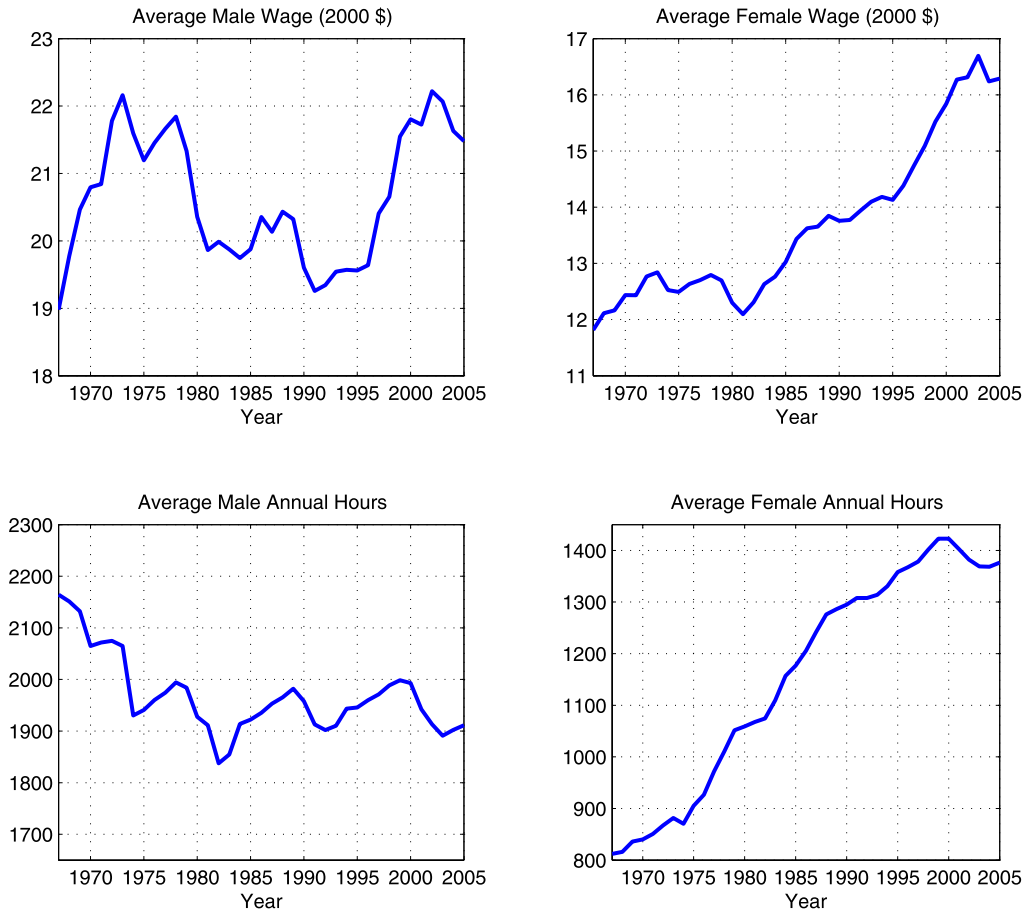


Fig. 2. Average wages and hours worked for men and women (CPS).

more important: hours worked per woman increase by 92% over the sample period; real female labor income per hour rises by 30%. Most of the increase in female hours is on the extensive margin.¹⁵

Consumption. Fig. 3 reports various measures of per-capita consumption for the CEX and the PSID, and contrasts them with comparable aggregates for personal consumption expenditures from the NIPA. The top left panel reports aggregate expenditure on food (including alcoholic beverages and food away from home). The plot confirms that food expenditures in the CEX and the PSID track each other fairly closely, especially in the earlier part of the sample (see Blundell et al., 2008, for a similar finding). However, the survey-based estimates are lower than NIPA food expenditure, and, more disturbingly, the gap between the two series is increasing over time. This growing discrepancy—from 20 to 60%—is even more marked for a broader definition of non-durable consumption (the top-right panel).¹⁶ The bottom two panels show that a widening gap is also evident, though less pronounced, for expenditures on durables and housing services.¹⁷

Some recent research investigates the large and growing gap between CEX and NIPA aggregate consumption (Slesnick, 2001; Garner et al., 2006). Conceptual differences between the CEX and the NIPA can account for some of the discrepancy. For example, among medical care expenditures, a rapidly growing item in NIPA consumption, the BEA includes expenditures by Medicare, Medicaid, and private insurers, whereas the CEX reports only out-of-pocket expenses. However, as Fig. 3 makes clear, the growing gap between the CEX and the NIPA applies across a broad range of consumption categories, suggesting that specific definitional differences are only part of the explanation.¹⁸

¹⁵ Hours are computed using hours worked last week, which is available throughout the sample period.

¹⁶ The definition (in both NIPA and CEX) includes the following categories of non-durables and services: food, clothing, gasoline, household operation, transportation, medical care, recreation, tobacco, and education.

¹⁷ Durable consumption includes expenditures on vehicles and furniture. Expenditures on housing services include imputed rent on owner-occupied housing plus rent paid by renters.

¹⁸ For example, Garner et al. (2006) show that the ratio between CEX and NIPA expenditures for the specific category “Pets, toys and playground equipment,” declines from 0.71 in 1984 to 0.48 in 2002.

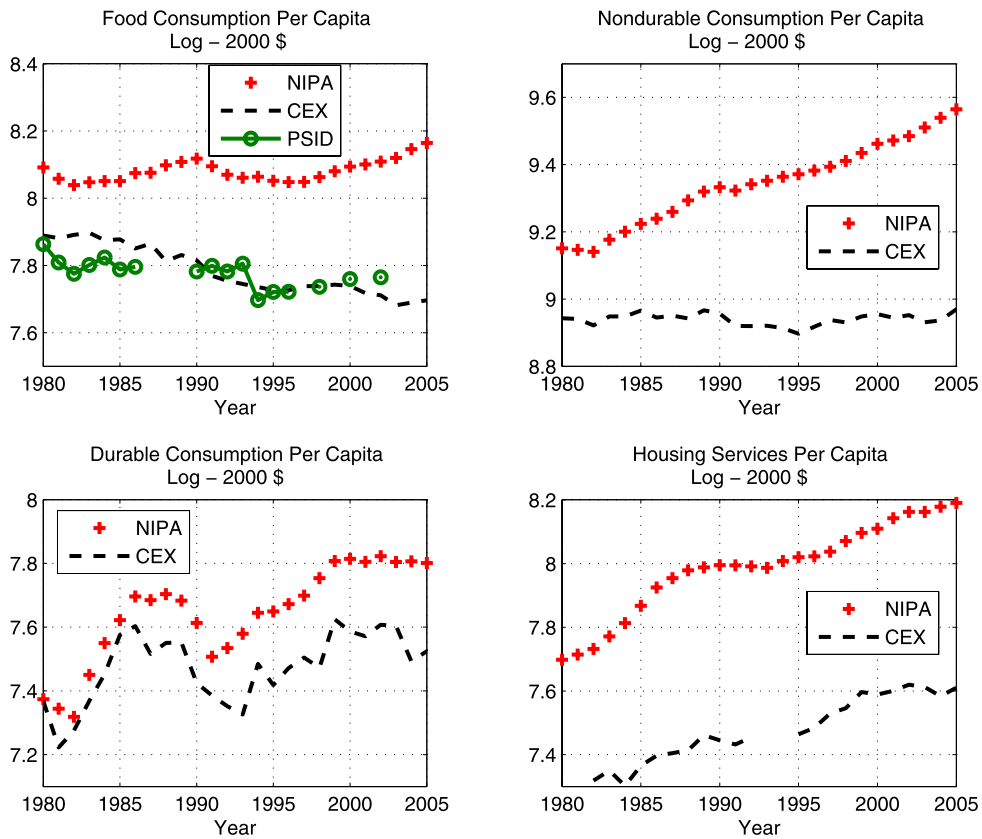


Fig. 3. Comparison between averages in CEX and in NIPA: consumption.

Another candidate explanation is that the CEX sample under-represents the upper tail of the income and consumption distributions, and that growth in aggregate consumption has been largely driven by these missing wealthy households. However, one would expect this type of sample bias to show up in income as well as in consumption, and it does not: CEX per-capita income tracks NIPA per-capita income well (see Section 6).

Interestingly, survey-based aggregate consumption also fails to keep up with survey-based income and with national accounts consumption in the UK (see Blundell and Etheridge, 2010, this issue), whereas the problem is absent in other countries, such as Canada and Germany (see Brzozowski et al., 2010; Fuchs-Schündeln et al., 2010, this issue). Understanding the reasons for this discrepancy remains an important open research question.

4. Inequality over time

This section is devoted to characterizing the evolution of cross-sectional inequality in the United States in the last 40 years. We find that making general statements about inequality over this period is difficult for two reasons. First, the specific metric for inequality matters, since measures of dispersion that emphasize the bottom of the distribution (such as the P50–P10 ratio or the variance of log) often evolve quite differently from measures that emphasize the top of the distribution (such as the P90–P50 ratio or the Gini coefficient). Second, and more importantly, wages, earnings, income, and consumption exhibit surprisingly different dynamics.

To understand why, we trace out the mapping suggested by the household budget constraint from dispersion in individual wages (reflecting inequality in human capital endowments) to dispersion in household consumption (reflecting inequality in welfare).¹⁹ The steps in this mapping are from individual wages to earnings, from individual earnings to household earnings, from household earnings to disposable income, and ultimately from disposable income to consumption.

To our knowledge, ours is the first paper documenting the joint evolution of all these variables in the United States using comparable samples from several surveys. The closest papers to ours are Burtless (1999) and Gottschalk and Danziger (2005), which explore the mapping from wages to pre-tax income in the CPS. However, they do not document trends in

¹⁹ Clearly, wages are only an imperfect proxy for skill endowments. But within the typical set of variables available in micro data, they are the closest. Similarly, household consumption is an imperfect proxy for household welfare. Leisure is another important determinant of welfare, but it is harder to measure. See Aguiar and Hurst (2007) for a study on trends in leisure inequality over the last four decades, based on time-use surveys.

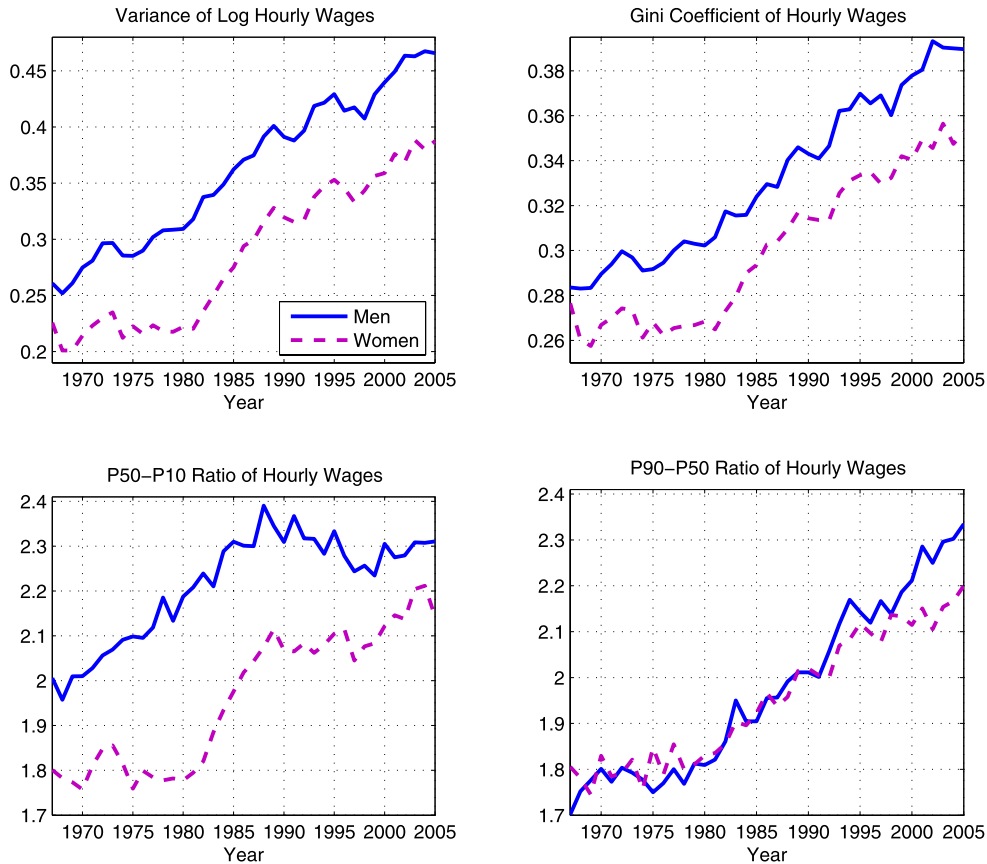


Fig. 4. Wage inequality for men and women (CPS).

disposable income, consumption, or wealth. For common variables and overlapping sample periods, our results line up well with theirs.

4.1. Individual-level inequality

Wages. We begin our discussion of individual-level inequality with wages. Fig. 4 displays four measures of dispersion in hourly wages by gender.²⁰

The variance of log hourly male wages increases throughout the period, while the variance of log female wages is relatively stable in the 1970s but increases rapidly in the 1980s. The Gini coefficient increases throughout the sample period, especially in the 1980s and 1990s. Quantitatively, the overall rise in wage inequality is substantial. The variance of male wages rises by around 21 log points, and the Gini by 11 points. The corresponding figures for women are 16 and 7 log points. Eckstein and Nagypal (2004, Fig. 3) report similar findings.

Turning to the percentile ratios, we uncover different trends in the top and bottom halves of the wage distribution.

The male 50th–10th percentile ratio (P50–P10) rises steadily until the late 1980s but is quite stable thereafter. The pattern for women is similar, except that almost all of the increase in the female P50–P10 is concentrated in the 1980s. Women are paid less than men on average, and are twice as likely to be paid at or below the federal minimum wage.²¹ Wage compression induced by the minimum wage floor may explain why the average level of the P50–P10 is lower for women. Interestingly, the 1980s, when the female P50–P10 wage ratio increases sharply, was a period when the US federal minimum wage was held constant in nominal terms (from January 1981), and declined dramatically in real terms.²²

²⁰ Recall that all the individual-level statistics are computed on Sample C, which includes individuals age 25–60 who work at least 260 hours per year, with wages at least half of the legal federal minimum wage.

²¹ About 4% of women paid hourly rates reported wages at or below the prevailing Federal minimum in 2002, compared to 2% of men. For more details on the characteristics of minimum wage workers, see <http://www.bls.gov/cps/minwage2002.htm>.

²² Lee (1999) and Card and DiNardo (2002) claim that the US federal minimum wage has a large impact in shaping the bottom of the wage distribution. The real minimum wage was stable at around \$8.50 (in 2008 dollars) between 1967 and 1979, then declined steadily to reach \$5.50 in 1990. If plotted together, the inverse of the real minimum wage and the P50–P10 ratio comove very closely, especially for women.

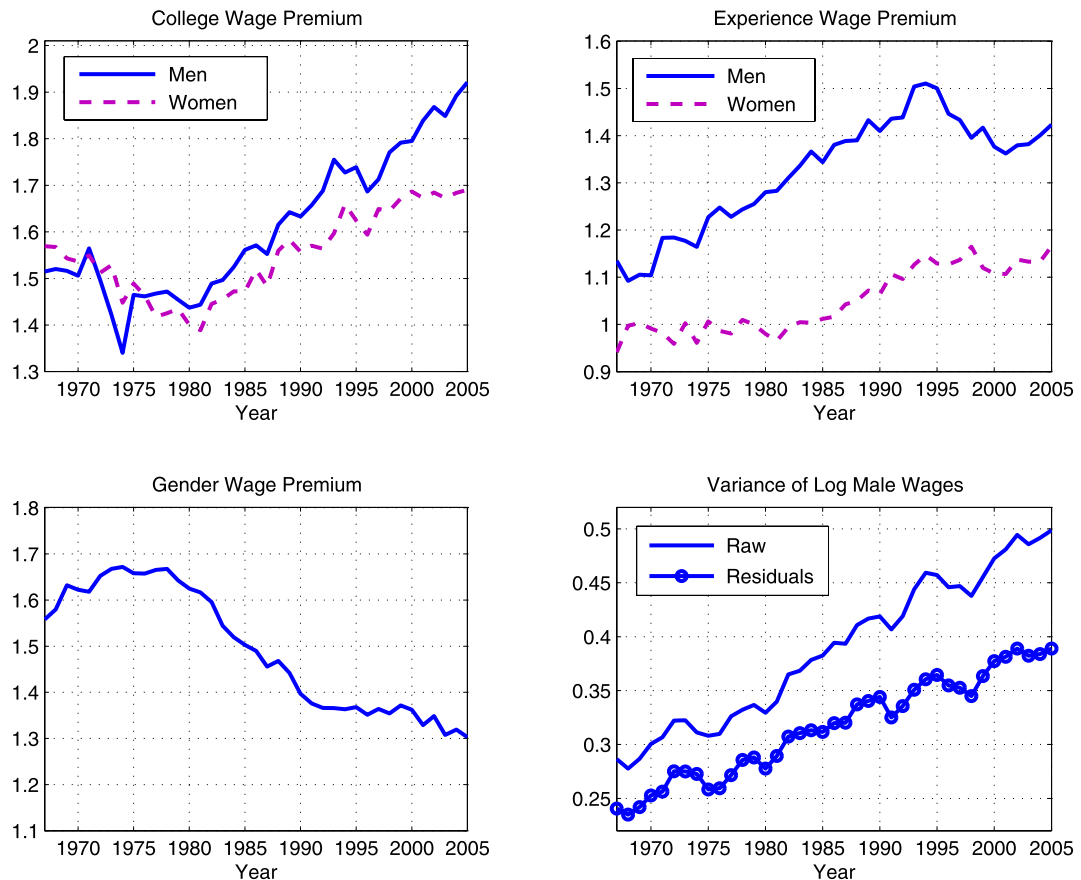


Fig. 5. Education, experience, gender wage premia, and residual wage inequality (CPS).

The level of inequality at the top of the wage distribution as measured by the 90th–50th percentile ratio (P90–P50) is similar for men and women. Inequality at the top increases throughout the sample period, and especially after 1980, with wages at the 90th percentile rising slightly more for men than for women, relative to the corresponding medians.

To summarize, the increases in US wage dispersion in (i) the 1970s, (ii) the 1980s, and (iii) the 1990s were concentrated, respectively, (i) within the bottom half of the wage distribution, (ii) throughout the wage distribution, and (iii) in the top half of the wage distribution.

A large empirical literature documents the evolution of cross-sectional wage inequality in the United States since the mid-1960s. The two most recent and comprehensive surveys are Katz and Autor (1999), and Eckstein and Nagypal (2004). A more up-to-date account is provided by Autor et al. (2008).²³ All of these papers are based on CPS data and focus only on full-time, full-year workers, i.e. individuals who work at least 35 hours per week and 40 plus weeks per year. Our analysis is based on a much broader sample, given our more inclusive criterion for hours worked. Nevertheless, the qualitative trends we uncover are very similar to these previous studies. A unique contribution of our study (see Section 6) will be to document that measured changes in the wage structure in the CEX and the PSID line up very well with those in the larger CPS sample.

Observables and residuals. In order to understand the sources of the rise in US wage inequality, it is important to distinguish the role of some key observable demographics such as education, age, and gender. We perform this decomposition in Fig. 5. We define the male education premium as the ratio between the average hourly wage of male workers with at least 16 years of schooling, and the average wage of male workers with less than 16 years of schooling. The pattern that emerges is the well-documented U-shape: the college wage premium declines until the late 1970s and then starts rising steadily. In 1975, US college graduates earned 40% more than high school graduates, while in 2005 they earned 90% more. A vast literature argues that upward trends in relative quantities and prices for college-educated labor reflect a skill-biased demand shift, associated with the technological shift toward information and com-

²³ Historically, the widening of the US wage structure during the 1980s was first documented by Davis and Haltiwanger (1991), Bound and Johnson (1992), Katz and Murphy (1992), Levy and Murnane (1992), Murphy and Welch (1992), and Juhn et al. (1993), among others.

munications technology (ICT), and to globalization (e.g., Katz and Murphy, 1992; Krusell et al., 2000; Acemoglu, 2002; Hornstein et al., 2005).²⁴

The experience (age) wage premium plotted in Fig. 5 is defined as the ratio between the average hourly wage of 45–55-year-olds and the hourly wage of 25–35-year-olds. The male experience premium more than doubles (from 20% to 40%) between 1975 and the end of the sample period. The increase for women is smaller and occurs somewhat later.²⁵ In the literature, the rise in the experience premium has received much less attention than the skill premium. One explanation emphasizes demographic change, i.e., the passage through the labor market of the baby boom generation, and growth in the number of working women, who tend to be younger than working men (Jeong et al., 2008). An alternative hypothesis is that recent technological change has favored more experienced workers, especially among less-educated groups (Weinberg, 2005).

The plot of the gender wage premium (the ratio of the average hourly wage of men to the average hourly wage of women) in Fig. 5 shows that, on average, men earned 65% more per hour than women in 1975, but only 30% more in 2005. This convergence was concentrated in the 1980s: from the early 1990s there has been little additional reduction in the raw gender gap.

The last panel of Fig. 5 displays residual wage inequality for males, measured as the variance of residuals from a regression of log wages on standard demographics.²⁶ Residual wage dispersion rises throughout the period. A comparison with the variance of “raw” wage inequality reveals that rising residual inequality accounts for essentially all of the increase in cross-sectional male wage dispersion in the 1970s but only about two-thirds of the rise since 1980. The other third is explained by changes in the relative “prices” of observable characteristics, particularly experience in the 1980s, and education in the 1980s and 1990s.

Labor supply. The bottom right panel of Fig. 6 plots the variance of log earnings for men and women. The variance of male earnings increases by 30 log points over the sample period, with two-thirds of this increase concentrated between 1967 and 1982.²⁷ Dispersion in female earnings, in sharp contrast, is essentially trendless. It is perhaps surprising that the pictures for dispersion in earnings looks so different from those for dispersion in wages in the top left panel, given that we measure wages as earnings per hour.

Mechanically, the variance of log earnings is equal to the variance of log wages plus the variance of log hours plus twice the covariance between log wages and log hours. With this in mind, the top right panel of Fig. 6 indicates that the variance of log female hours falls from 0.28 to 0.20, which partially offsets the impact of rising wage dispersion on female earnings inequality. This decline in female hours dispersion toward the level for men mirrors the convergence in female wages and hours (recall Fig. 2). Inequality in male hours is sharply counter-cyclical, but exhibits no obvious long-run trend.²⁸

The bottom left panel of Fig. 6 shows the correlation between log wages and log hours, and sheds light on the dramatic increase in the variance of male earnings. In particular, the correlation increases sharply in the first half of the sample period, precisely when earnings dispersion is rising most rapidly.

Earnings. Fig. 7 delves deeper into the evolution of inequality in male earnings. Here we rank men by earnings, and for each decile of the earnings distribution compute average hours and average wages. To focus on dynamics, we plot percentage changes for each variable relative to 1967.²⁹

The top left panel of the figure indicates that, relative to 1967, earnings of the bottom decile declined in real terms by 60% in the period up to 1982 before recovering somewhat in the 1990s. Earnings for the top decile rose steadily throughout the sample period. The top right and bottom left panels of the figure make a striking point: earnings dynamics at the top of the male earnings distribution are entirely driven by changes in wages, while changes in hours play a central role in shaping earnings dynamics at the bottom of the distribution. To see this, note that hours at the top of the male earnings distribution are stable and evolve very similarly to those at the median, while wages consistently grow more rapidly. Conversely, in the 1970s, when inequality at the bottom rises sharply, wage dynamics for the bottom decile of the earnings distribution are very similar to those for the median (more exactly, the P45–P55), showing a small decline. Instead, hours for these two groups evolve very differently: hours for the median are very stable, while hours for the bottom decile fluctuate dramatically as a virtual mirror image of the unemployment rate (the bottom right panel).³⁰

²⁴ Eckstein and Nagypal (2004) and, more recently, Lemieux (2006) document that the premium for postgraduate education increased even faster.

²⁵ Eckstein and Nagypal (2004, Fig. 15) plot the coefficient on experience from a standard Mincerian wage regression and find a pattern very similar to ours: the experience premium for women is much lower than for men, and for both sexes it rises in the 1970s and 1980s and stabilizes in the 1990s.

²⁶ See Appendix A for the exact regression specification.

²⁷ Kopczuk et al. (2009, Fig. 1) document a similar trend for male earnings inequality (fast rise in the 1970s and 1980s, slower rise in the 1990s) from Social Security Administration data.

²⁸ Recall that individuals are in the sample as long as they work at least 260 hours per year (one quarter of part-time employment). We have experimented with slightly higher and lower thresholds, and we found that the absence of a trend in hours inequality is robust.

²⁹ In every year both average wages and average hours increase monotonically across the bins ranked by earnings.

³⁰ Murphy and Topel (1987, Table 5) provide evidence supporting the view that the rise of unemployment was disproportionately borne by the low-wage workers. Between the periods 1971–1976 and 1980–1985, the unemployment rate of high school dropouts rose from 5.5% to 10.3%, that of high-school graduates from 4% to 7.5%, and that of college graduates from 1.7% to 2.2%.

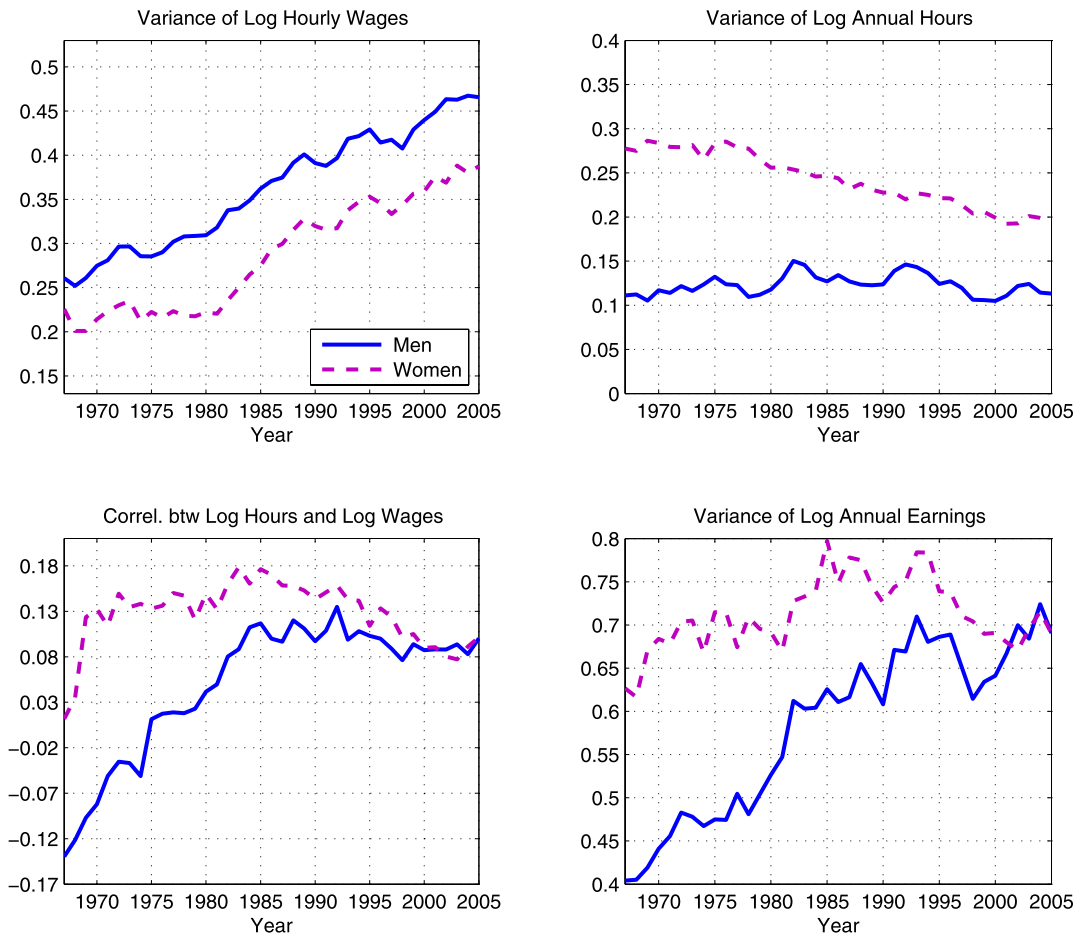


Fig. 6. Inequality in labor supply and earnings of men and women (CPS).

To recap, the key to understanding the evolution of the top of the male earnings distribution is to understand the evolution of the top of the male wage distribution, while the key to understanding the evolution of the bottom of the earnings distribution is to understand the evolution of hours worked and the unemployment rate. This evolution of wages and hours at different points in the distribution also explains the rise in the wage–hour correlation. Workers with low wages and low hours worked relative to the median saw hours decline, while high-wage, high-hours workers experienced wage gains.

Interpretation. In a broader macro context, trends in earnings inequality appear to be shaped by two forces: aggregate labor demand shifts and institutional constraints in the labor market (unions, the minimum wage). At the top of the earnings distribution—where wages drive earnings dynamics—institutional constraints are largely absent, and hence labor demand shifts in favor of skilled workers increase both wage and earnings inequality. Consistently with this interpretation, we note that the pattern for the college–high school premium (Fig. 5) is similar to that for the P90–P50 wage ratio (Fig. 4), suggesting that increasing demand for educated labor is a major factor widening inequality at the top.

For lower-skilled workers, unions and minimum wage laws deflect some of the impact of declining labor demand from prices (wages) to quantities (hours). In the 1970s, when these institutions were particularly strong, declining aggregate demand (the “TFP slowdown”) and declining relative demand for unskilled labor (skill-biased technical change) translated into a moderate fall in wages, and a sharp fall in employment for low-skilled men (Fig. 7).³¹ The combined effect was rapid growth in earnings inequality at the bottom. In the 1980s, unions weakened with the decline of the manufacturing sector, while the real value of the federal minimum wage was eroded by inflation. As these institutional constraints weakened, the impact of labor demand shocks at the bottom of the distribution shifted from quantities to prices: wages fell sharply in the 1980s, but hours worked partially recovered, slowing growth in earnings inequality. In the 1990s, the real minimum wage

³¹ Here we present the productivity slowdown and skill-biased demand shifts as two separate phenomena. However, economists have advanced a common interpretation for both based on learning effects associated to the introduction of ICT. See Hornstein et al. (2005) and the references therein.

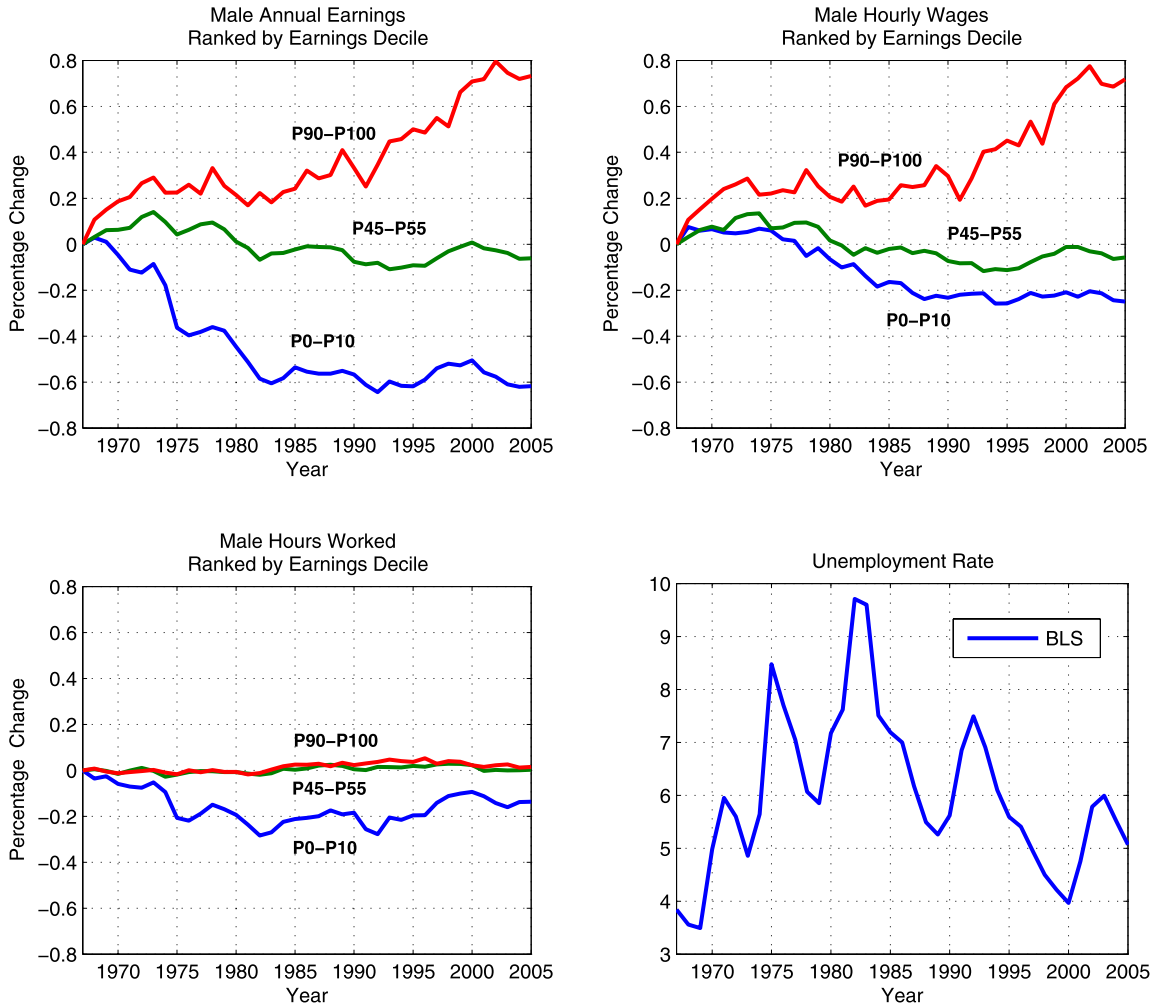


Fig. 7. Understanding male earnings inequality (CPS).

stabilized, while aggregate productivity growth recovered. The net effect was broad stability at the bottom of the wage and earnings distributions.

4.2. Household-level inequality

We compute all of our household-level measures of dispersion on Sample B, with the additional exclusion of households with non-positive household earnings. Also, since the variance of logs measure is very sensitive to the presence of realizations close to zero, we also exclude the bottom 0.5% of the remaining sample as ranked by income, reapplying this trim for each definition of income we study.

Equivalized household earnings. Fig. 8 plots four measures of dispersion in household earnings, where each household's income is first adjusted to a per-adult-equivalent basis using the OECD equivalence scale.³²

The top left and bottom left panels plot, respectively, the variance of log earnings and the P50–P10 ratio. These two series track each other extremely closely, reflecting the fact that the logarithmic function effectively amplifies small earnings values. The variance of household earnings rises rapidly in the 1970s and early 1980s before stabilizing. Qualitatively, the profile is similar to that for male earnings in Fig. 6.³³

³² The OECD scale assigns a weight of 1.0 to the first adult, 0.7 to each additional adult, and 0.5 to each child. In the PSID, a child is a family member age 17 or younger. In the CPS and the CEX, we define a child as age 16 or younger. The original OECD definition is age 13 or younger.

³³ The difference between the two series primarily reflects the fact that in Fig. 6 we plot the variance of male earnings for men working at least 260 hours, whereas in Fig. 8 there is no explicit selection on hours. Without this hours restriction, the variance of male earnings is essentially flat after the mid-1980s, just like the variance of equivalized household earnings.

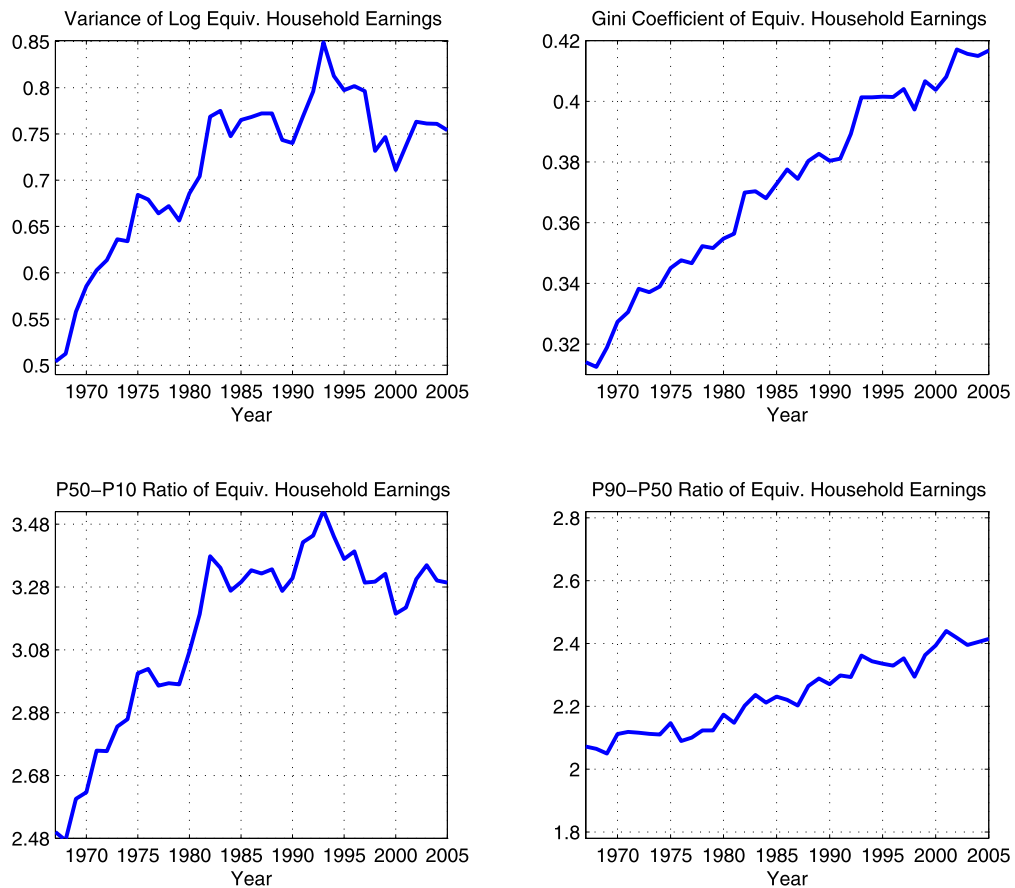


Fig. 8. Various measures of household earnings inequality (CPS).

The top right and bottom right panels plot the Gini coefficient for household earnings and the P90–P50 ratio. These two series also closely resemble each other, reflecting the sensitivity of the Gini coefficient to the shape of the upper portion of the earnings distribution. Inequality at the top of the household earnings distribution increases steadily across the entire sample period. However, when we compare the evolution of the P90–P50 and P50–P10 ratios, it is clear that while the growth in the former is more continuous, it is much smaller in overall magnitude.

Equalization and residual dispersion in household earnings. Equalization slightly reduces the level, but has no impact on the trend of the variance, which increases by roughly 30 log points until the early 1990s and then levels off. Thus, henceforth we will focus on dispersion in equalized income measures. Household demographic characteristics explain about 40% of the variance of household earnings. Consistently with what we observed for wages, growth in residual earnings dispersion accounts for most of the increase in the raw variance.

Cyclical dynamics of earnings inequality. Fig. 9 plots the trends in percentiles at different points in the distribution for household earnings (all normalized to zero in 1967), together with shaded areas denoting NBER-dated recessions. The panel shows clearly the fanning out of the distribution over time. Although the top 5% of the distribution have seen household earnings rise in real terms by around 60 log points over the sample period, those below the 10th percentile earned no more in 2005 than in 1970.

Earnings inequality tends to widen sharply in recessions, and then remains relatively stable during periods of expansion.³⁴ This reflects the fact that while household earnings are procyclical at each percentile, business cycle fluctuations are much more severe at the bottom of the distribution, with large percentage declines in earnings during recessions. Indeed, the 5th and 10th earnings percentiles closely mirror—inversely—the time path for the unemployment rate over the sample period.³⁵

³⁴ Barlevy and Tsiddon (2006) develop a theoretical model in which recessions amplify long-run trends in inequality.

³⁵ The troughs of the low-end percentiles in 1971, 1975, 1982, 1993, and 2004 correspond almost exactly to turning points in the unemployment rate in 1971, 1975, 1982, 1992, and 2003.

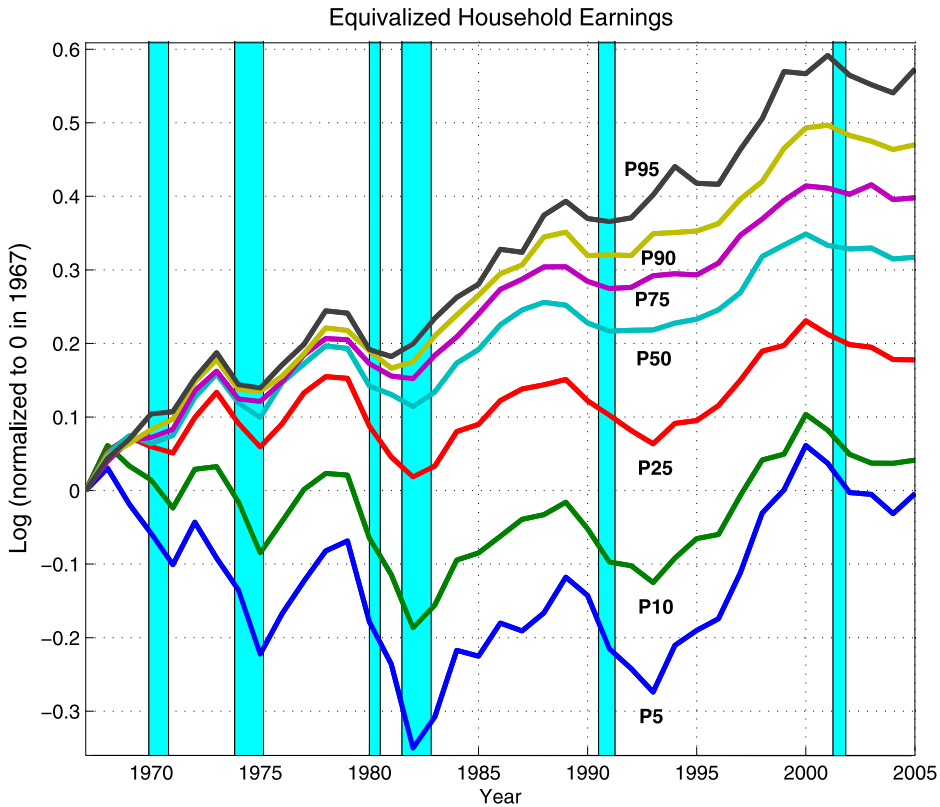


Fig. 9. Percentiles of the household earnings distribution (CPS). Shaded areas are NBER recessions.

Henceforth, we focus exclusively on the variance of log and the Gini coefficient as measures of dispersion, exploiting our finding from Fig. 8 that these capture, respectively, the dynamics of dispersion at the bottom and the top of the income distribution.

From individual to household inequality. The top two panels of Fig. 10 plot the evolution of inequality in labor earnings for the main earner and for total household earnings. A marked difference between the two measures of income is apparent when dispersion is measured by the Gini coefficient, but not in the corresponding series for the variance of log earnings.

The dynamics of the Gini coefficient illustrate that the family plays a role in providing insurance against individual risk—a role that has grown over time, thanks to the surge in female labor supply (see Section 3). The striking similarity between the variances of main-earner and household log earnings reflects the fact that households at the bottom of the earnings distribution tend to be either single households or married households with only one earner.

The remaining four panels of Fig. 10 further explore how the family shapes cross-sectional inequality.

Consider first the middle two panels. Among single households, earnings dispersion is larger than among married households, confirming that income pooling within married households reduces inequality (middle left panel). Although 80% of households in our sample were married in 1967, this share declines steadily over time to less than 60% in 2005 (middle right panel). This trend tends to increase overall cross-sectional dispersion, given that earnings are more unequally distributed within single households. At the same time, however, dispersion within single households is broadly stable over time, while dispersion within married households is generally rising. The net effect is that the variance of log household earnings for *all* households evolves very similarly for the corresponding series for *married* households.

The bottom two panels of the figure illustrate two key trends that determine how income pooling within *married* households translates into inequality in married household earnings. First, due to increasing female labor force participation, a rising fraction of married couples contain two earners (lower left panel), which reduces cross-household dispersion to the extent that earnings are imperfectly correlated across spouses. Second, among married two-earner households, the between-spouse correlation of earnings has almost doubled (lower right panel), which works in the opposite direction.³⁶

³⁶ Fitzgerald (2008) provides an analysis of income dynamics for all sorts of household types and also describes how the mix of different types has changed over time.

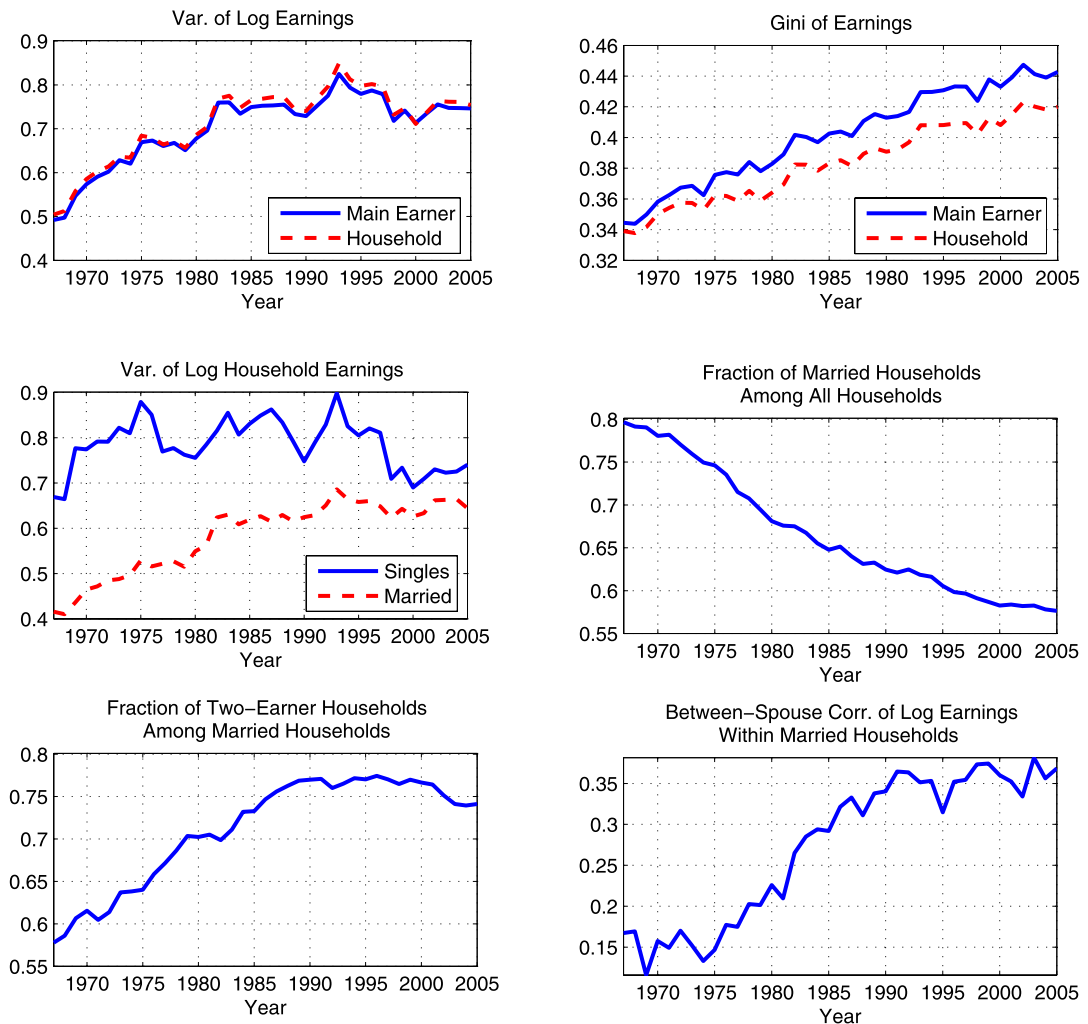


Fig. 10. Understanding the role of the family for earnings inequality (CPS).

Private transfers and asset income. In Fig. 11 we move beyond earnings to broader measures of income. It is important to keep two things in mind. First, our focus is on households containing at least one adult of working age. Thus, we exclude many older households, which rely primarily on unearned income. Second, most categories of unearned income suffer from serious under-reporting in the March CPS and in other household surveys (see Section 3).

With these important caveats in mind, we note that adding private transfers reduces income inequality mostly at the bottom. In part, this reflects the fact that households containing retirees tend to have lower earnings but higher private retirement income. Adding asset income has little impact on the variance of log income, except for increasing inequality slightly toward the end of the sample period. In contrast, including asset income markedly increases the Gini coefficient for income. This reflects the well-known fact that a large fraction of aggregate wealth is concentrated at the top of the wealth distribution, and that wealth and income are positively correlated in cross section.³⁷

Government redistribution. In Fig. 12 we analyze the role of transfers and taxes. Public transfers play a very important role in compressing inequality at the bottom of the income distribution, as is evident from the wide gap between the pre-government and pre-tax series for the variance of log income. Public transfers distributed through the unemployment insurance and welfare system also serve as a powerful stabilizing antidote to countercyclical surges in pre-government income inequality. This is evident from the fact that the variance of log household income is much smoother when benefits are included (top left panel).

³⁷ Our analysis of SCF data (see Section 8) based on a sample comparable to that of the CPS shows that in 2004 the Gini coefficient for net worth was 0.70, and the top quintile of the earnings distribution accounted for 52% of aggregate net worth. Budría Rodríguez et al. (2002) report a correlation between income and wealth of 0.6.

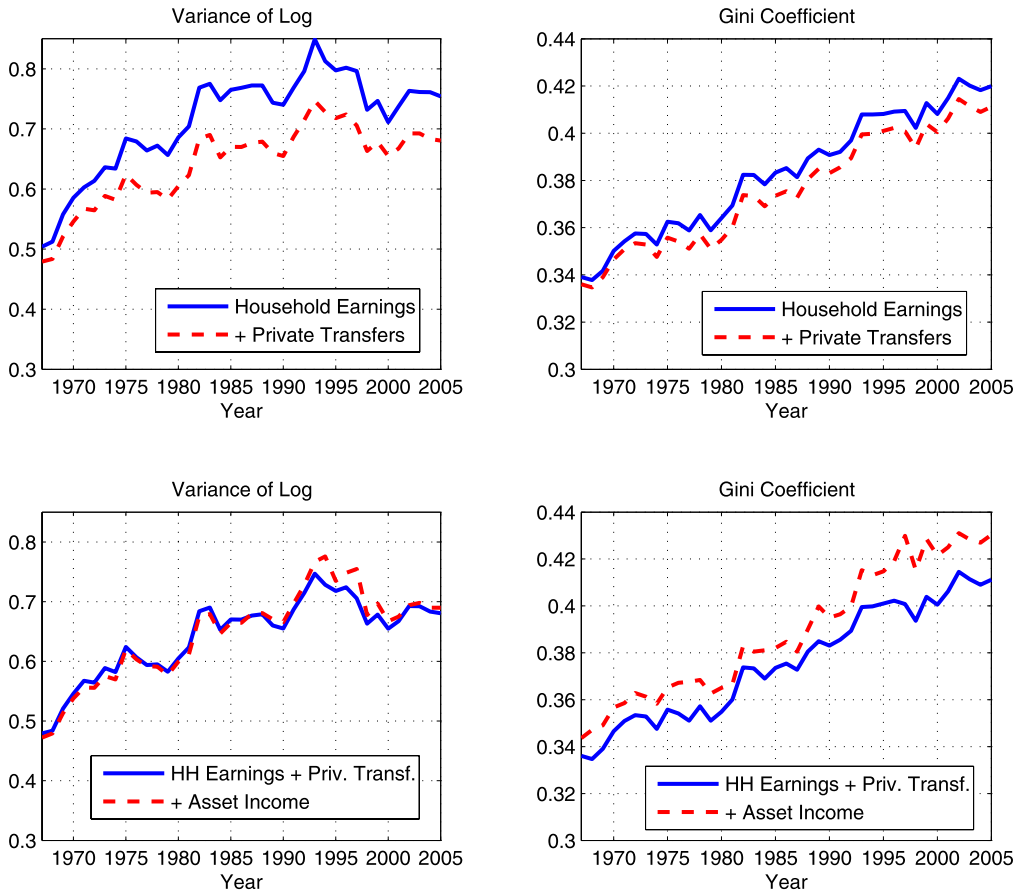


Fig. 11. From household earnings to pre-government income (CPS).

The tax code also appears to be quite progressive overall. Disposable income inequality is much smaller than pre-tax income inequality, for both measures of dispersion.

In the 1980s, pre-tax and post-tax income follow very similar trends. In the 1990s, by contrast, the gap between pre- and post-tax income inequality rises. These trends are consistent with the view that the taxes became less progressive under Reagan (1981–1989) and more progressive under Clinton (1993–2001). Piketty and Saez (2006) report that federal tax rates declined sharply at the top of the income distribution in the 1980s, and then increased somewhat in the 1990s (see their Table 2).³⁸

Finally, we should note that there are changes over time in the relative importance of transfers versus taxes in reducing income inequality. For example, in the mid-1990s there was a decline in the redistributive role of public transfers, following the PRWORA Act of 1996, which dramatically reduced cash assistance to the poor. At the same time, however, there was a tremendous expansion in assistance through taxes: the Earned Income Tax Credit more than tripled in the 1990s (see Hoynes, 2008). This shift in redistribution from transfers to taxes over the 1990s is visible in the top left and bottom left panels of Fig. 12.

From income to consumption inequality. Fig. 13 documents the evolution of inequality in equivalized disposable income and non-durable consumption expenditures across households in the United States.³⁹ Both series are computed from the CEX (Sample B). The comparison of these two series highlights the role of borrowing and lending as a device for consumption smoothing in the face of income fluctuations. Note that the variance of log disposable income displays a larger increase

³⁸ The effective tax rates reported in the Congressional Budget Office Study (2005) are also consistent with this view.

³⁹ We use this narrow definition of consumption expenditures (which excludes durables) for three reasons. First, it is consistent with the definition used in the other articles in this issue. Second, the construction of flow services from durables and owner-occupied housing is challenging. Third, adding services from housing to consumption would also require, for consistency, adding imputed rents to the income of home owners. But this would change our definition of income relative to the CPS and PSID, where imputed rents are not available. We obtained very similar findings using a broader definition of consumption including purchases of small durables (e.g., home durables, furniture, electronics), imputed services from vehicles, rents, and imputed rents for home owners. Results and details of the imputation procedure are available upon request.

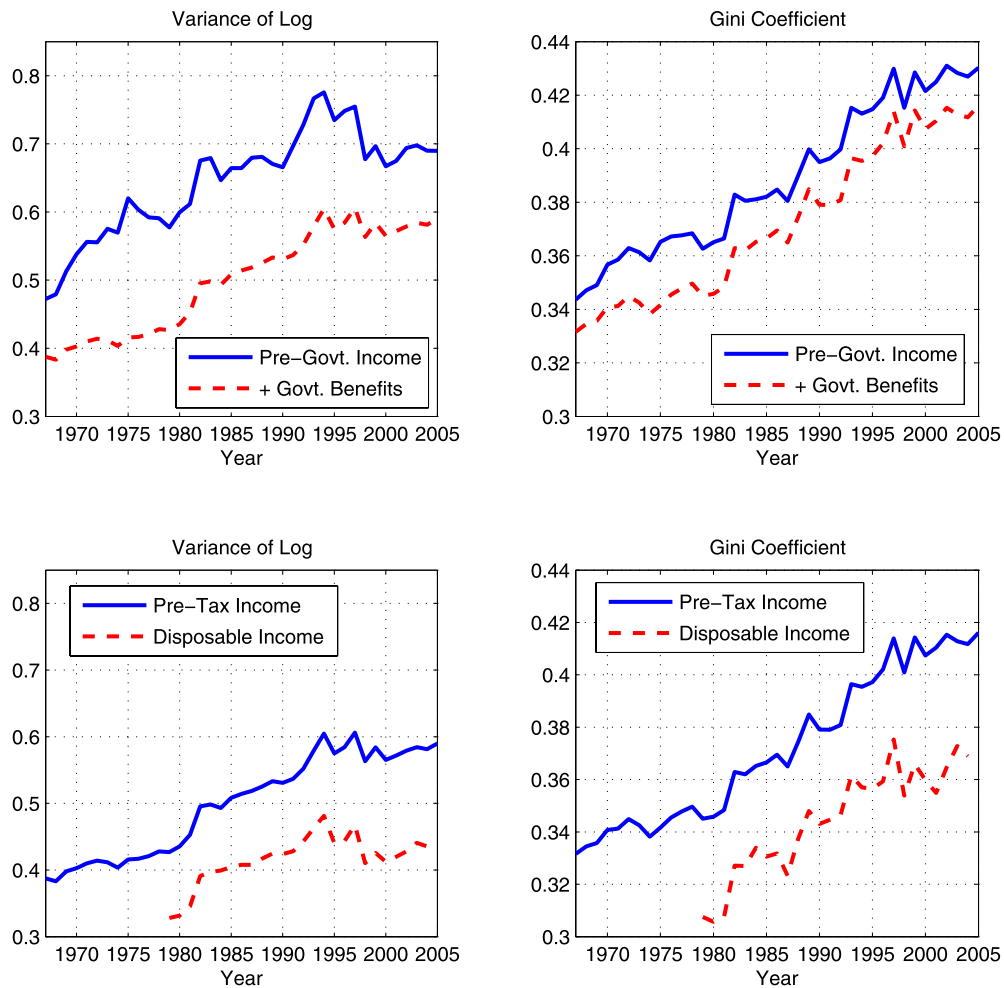


Fig. 12. From pre-government to disposable income (CPS).

over time in the CEX than in the CPS or the PSID. This discrepancy is due to differences across surveys in the way taxes are computed. See Section 6 for more on this topic.

The top two panels of Fig. 13 show two interesting facts about the relationship between disposable income and consumption inequality. First, consistent with standard economic theory, consumption inequality is substantially lower than income inequality.

Second, the rise in consumption inequality is much smaller than the rise in disposable income inequality. For example, the respective cumulative increases in the variance are 6 and 18 log points. This finding mirrors the conclusion of several recent papers including Slesnick (2001), Krueger and Perri (2006), and Attanasio et al. (2007), and suggests that some part of income inequality is effectively insurable and/or predictable in nature. Interestingly, in the last years of the sample period, consumption seem to track income more closely.

The bottom two panels of Fig. 13 indicate less transmission of income differentials into consumption inequality at the bottom of the income distribution than at the top. A possible explanation for this finding is that temporary shocks, which do not fully translate into consumption (e.g., short unemployment spells), are more likely to affect the distribution below the median.

5. Inequality over the life cycle

In the previous section, we argued that a sizable fraction of income differentials are essentially insurable, i.e., they do not translate into consumption. As originally emphasized by Deaton and Paxson (1994), the age profiles for inequality in earnings, income, and consumption contain information about the nature of risk and insurance when organized within life-cycle models with heterogeneous agents and incomplete markets (see also Storesletten et al., 2004a; Guvenen, 2007; Huggett et al., 2008; Kaplan, 2008; Kaplan and Violante, 2008; Heathcote et al., 2009).

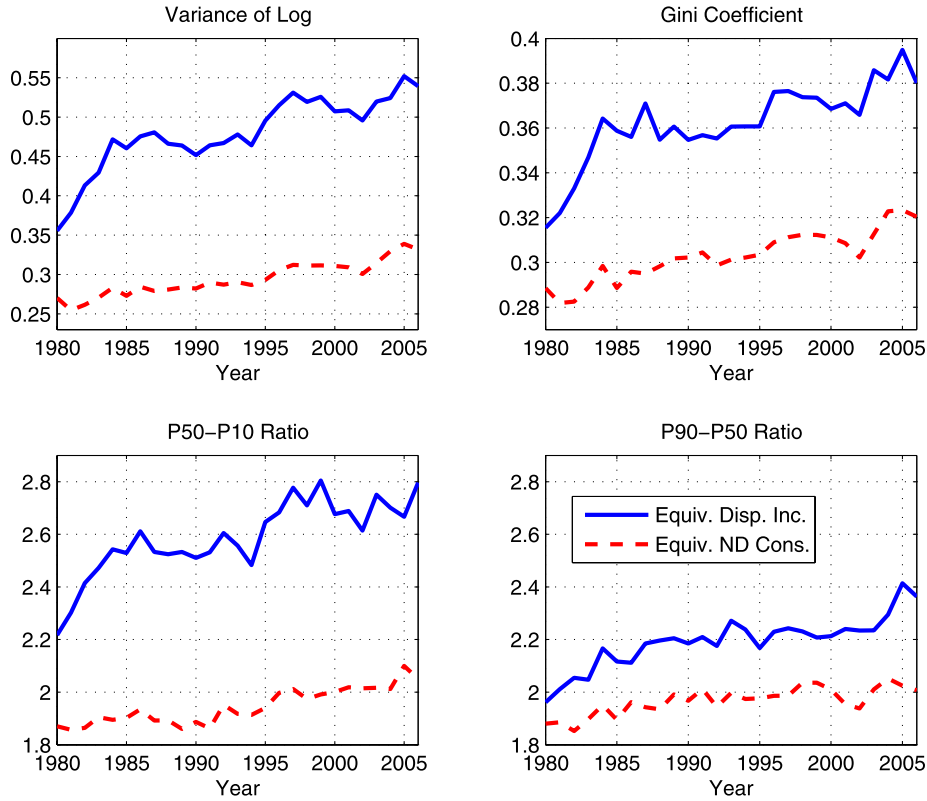


Fig. 13. From disposable income to consumption (CEX).

However, isolating a pure age profile from repeated cross sections in a non-stationary environment is challenging, because age, time, and cohort are linearly dependent (cohort is time minus age). Here, we do not attempt to argue whether the source of rising inequality in the United States is better characterized through time or cohort effects (for a discussion, see Heathcote et al., 2005; Pistaferri, 2009). We simply report two sets of estimates for the evolution of dispersion by age. The first set controls for time effects, the second set for cohort effects.

More specifically, let $m_{a,c,t}$ be a cross-sectional moment of interest (e.g., the variance of log earnings) for the group of households with head of age a belonging to cohort c (hence, observed at date $t = c + a$). To isolate the age profile, we run the two alternative regressions:

$$m_{a,c,t} = \beta'_a \mathbf{D}_a + \beta'_t \mathbf{D}_t + \varepsilon_{a,c,t}, \quad m_{a,c,t} = \beta'_a \mathbf{D}_a + \beta'_c \mathbf{D}_c + \nu_{a,c,t}, \tag{1}$$

where \mathbf{D}_t , \mathbf{D}_c , and \mathbf{D}_a are vectors with entries corresponding to a full set of year, cohort, and age dummies, respectively. The vectors β_t , β_c and β_a are the corresponding coefficients. The left panels in Fig. 14 plot the estimated values for β_a from the first regression, where we control for year effects, and the right panels plot the estimated values for β_a from the second regression, where we control for cohort effects.

Another important issue in documenting the evolution of household inequality over the life cycle is that the distribution over household size changes with age. We therefore report both inequality in raw household-level variables, without adjusting for size, and in equivalized household income, where we use the OECD equivalence scale to express earnings, income, and consumption in per-adult-equivalent units.⁴⁰

To allow for a straightforward comparison of how inequality in earnings, income, and consumption evolve with age, all the series plotted in Fig. 14 are based on Sample B from the CEX. Because the CEX sample is relatively small, rather than estimating a full set of age dummies, we group observations in 5-year age groups. The series are normalized so that each starts at zero at age 27, which is the midpoint of the first 5-year age group (25–29).

⁴⁰ An alternative way to equivalize is to regress household earnings (or income or consumption) on household characteristics (e.g., number of adults, number of children) and to use the predicted values for each household type as the scaling factors. Often, regression-based equivalence scales differ dramatically from the OECD scale we use. For example, the OECD treats additional children as enlarging the effective household size (reducing per-equivalent earnings), while according to the regression, additional children predict lower earnings and thus reduce effective family size. Aguiar and Hurst (2009) use the regression approach to estimate life-cycle growth in the variance of log non-durable consumption. When they control for cohort effects (their Fig. 1b), they find an increase of 12 points between ages 25 and 60, which is roughly twice as large as our increase (bottom right panel of Fig. 14).

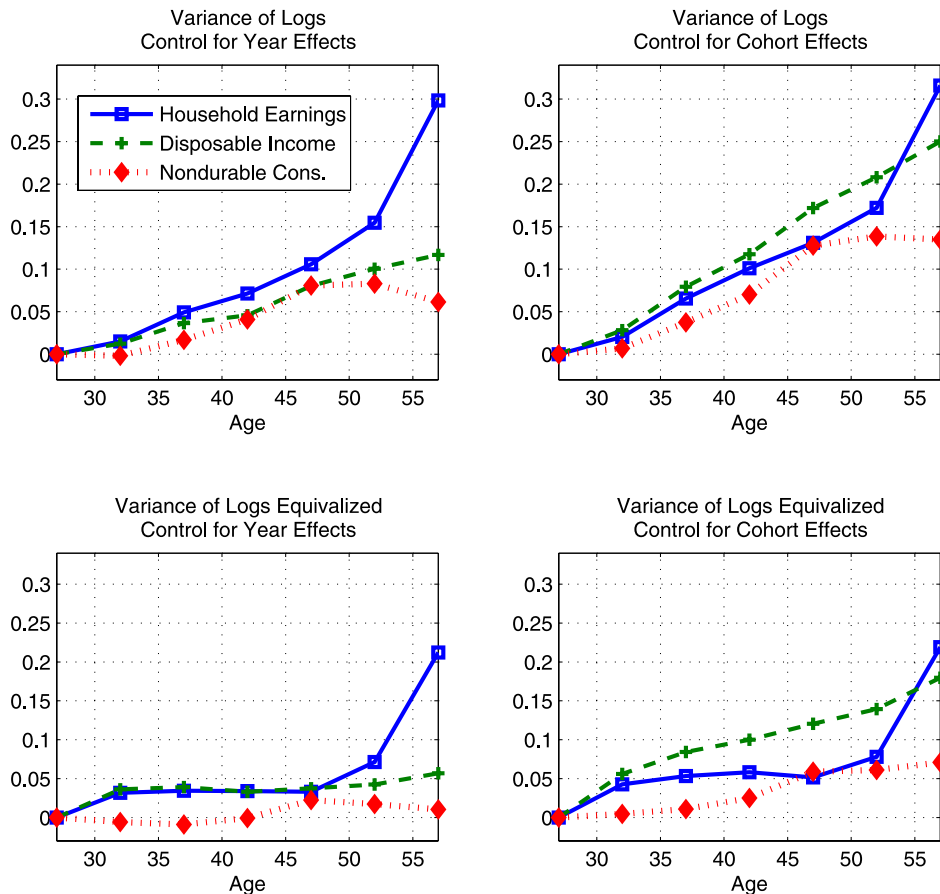


Fig. 14. Life-cycle inequality: controlling for time and cohort effects (CEX).

The figure shows that the variance of log household earnings rises over the life cycle by more than the variance of disposable income, which in turn rises by more than the variance of log consumption. The fact that dispersion in consumption grows less rapidly than dispersion in income indicates that households are able to effectively insure some fraction of persistent income fluctuations.

Cohort vs. time. The precise magnitudes of the life-cycle increases in inequality are sensitive to whether one controls for year or cohort effects. For example, the variance of log disposable income rises twice as fast under the cohort view (the right panels) than under the time view (the left panels).⁴¹

If one takes the pure cohort view, cross-sectional inequality can increase only if each successive cohort starts out with more unequal income. If one takes the pure time view, cross-sectional inequality can increase only if all cohorts see faster growth in within-cohort inequality. The right panels of Fig. 14 indicate that over this period, within-cohort disposable income inequality was rising rapidly, while the left panels attribute much of this rise to a general increase in income inequality over time.

Equalizing. Although equalizing had no impact on trends in our time-series analysis of inequality (see Section 4.2), the size of the life-cycle growth in dispersion is sensitive to whether or not one focuses on raw or equalized measures. Equalizing reduces the estimated life-cycle increases for inequality for all variables. For example, under the cohort view, the variance of log raw household consumption rises twice as fast as log equalized consumption. Equalizing reduces the overall growth in inequality primarily by compressing growth in inequality in the middle of the life cycle. In part, this is

⁴¹ Why is the life-cycle profile for income so sensitive to whether one adopts the time or cohort view, while the earnings and consumption profiles look more similar? Recall from Fig. 12 that the cross-sectional variances of log earnings and log consumption are relatively stable over time in the CEX, while the variance of disposable income shows a marked increase. Thus, whether non-stationarity is modeled through year or cohort effects should have relatively little impact on the implied age profiles for earnings or consumption inequality, whereas more is at stake in deciding whether to model rising income inequality through time or cohort effects.

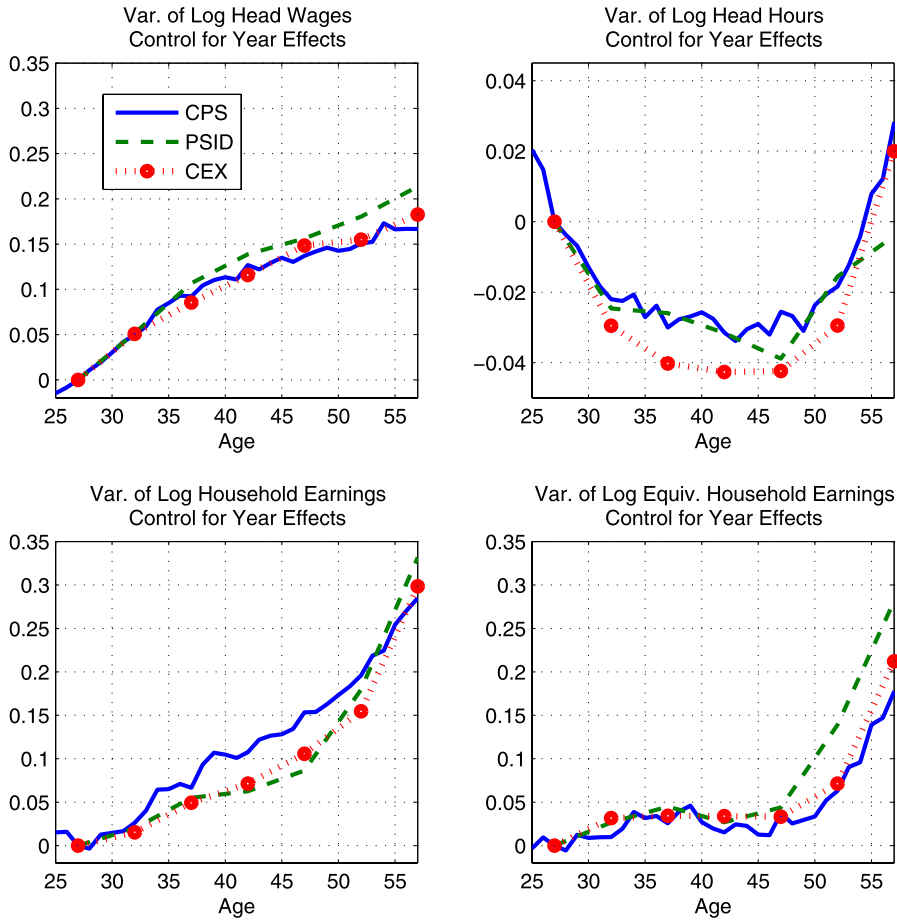


Fig. 15. Comparing life-cycle inequality across data sets.

because equalizing has the effect of amplifying income inequality for the youngest households in our sample, but has less impact on measured inequality for older households. This is consistent with the tendency of lower-income individuals to marry and have children at younger ages.

Curvature of the profiles. Finally, the profiles for income and consumption inequality over the life cycle exhibit differential curvature. The consumption profile is concave: inequality rises until roughly age 50 and is approximately flat thereafter. The earnings profile is convex, reflecting an acceleration in earnings inequality at older ages.

The concavity in consumption reflects the fact that as retirement approaches, the within-cohort distribution of permanent income stabilizes (see, for example, Storesletten et al., 2004a). Convexity of the earnings profile has been presented as evidence of “heterogeneous income profiles” (Lillard and Weiss, 1979; Baker, 1997; Guvenen, 2007), since an income process featuring only a unit root, or a persistent autoregressive component, would induce a linear or concave earnings profile.⁴² However, it is important to remember that the life-cycle profile for dispersion in earnings inherits the corresponding profiles for dispersion in wages and hours. Fig. 15, discussed in the next section, indicates that the life-cycle profile for the variance of log wages is actually slightly concave, and that the convexity of the earnings profile reflects increasing dispersion in hours worked at older ages, as individuals begin the transition to retirement.

To conclude, our study of life cycle inequality shows that the magnitude of growth in dispersion over the life-cycle is sensitive to two choices: (i) whether to control for non-stationarity via cohort or time effects, and (ii) the equivalence scale used to control for life-cycle changes in family size. More research should be devoted to disentangling cohort versus time effects, and to providing firmer theoretical foundations for the choice of household equivalence scale.

⁴² See Guvenen (2007) for a formal explanation of why the model with heterogeneous income profiles can generate convexity in the variance of earnings over the life cycle.

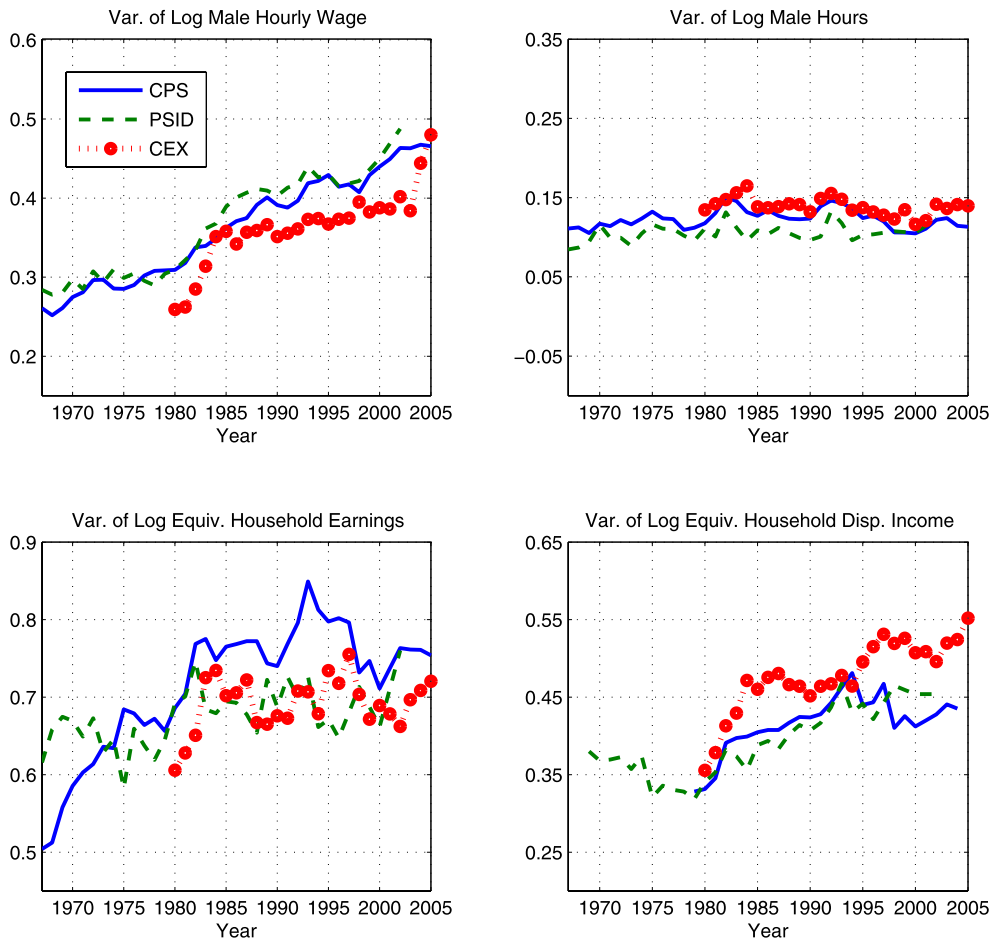


Fig. 16. Comparing the evolution of variances across data sets.

6. Comparison across data sets

In an ideal world, we could use a unique longitudinal household survey to track the joint dynamics of income, hours, consumption, net saving, and wealth. Unfortunately, no such data set exists for the United States.⁴³ The best one can do, with US data, is to combine different surveys. Therefore, it is paramount that different surveys yield similar patterns for the overlapping variables. In this section, we briefly assess the comparability of the CPS, the PSID, and the CEX.

Life cycle. Fig. 15 compares the evolution of inequality over the life cycle across our CPS, PSID and CEX samples. For all variables—head wage, head hours, raw household earnings, and OECD-equivalized household earnings—we find very close alignment across the three data sets. As discussed above, the life cycle profile for the variance of log wages is concave, but the dramatic U-shape in the variance of log hours translates into a convex profile for the variance of log household earnings. Fig. 15 plots age profiles controlling for year effects (see Section 5).⁴⁴ We also computed the same series under the cohort view, and once again found a remarkable degree of cohesion across data sets.

Time series for averages. With respect to per-capita averages, we have verified that both the levels and the trends of per-capita income in the CPS and the PSID are very similar. CEX per-capita income is roughly 15% lower on average, but it grows at a similar rate, except for the post-2000 period, when it grows somewhat faster.

Time series for inequality. Figs. 16 and 17 compare the evolution of inequality in male wages and hours, and in equivalized household earnings and disposable income across our three data sets. Fig. 16 shows inequality measured as variance of log, while Fig. 17 plots Gini coefficients.

⁴³ See Krueger and Perri (2009) and Jappelli and Pistaferri (2010, this issue) for studies along these lines using the Italian Survey of Household Income and Wealth.

⁴⁴ For the large CPS sample we estimated a full set of age dummies, rather than grouping observations in 5-year age groups.

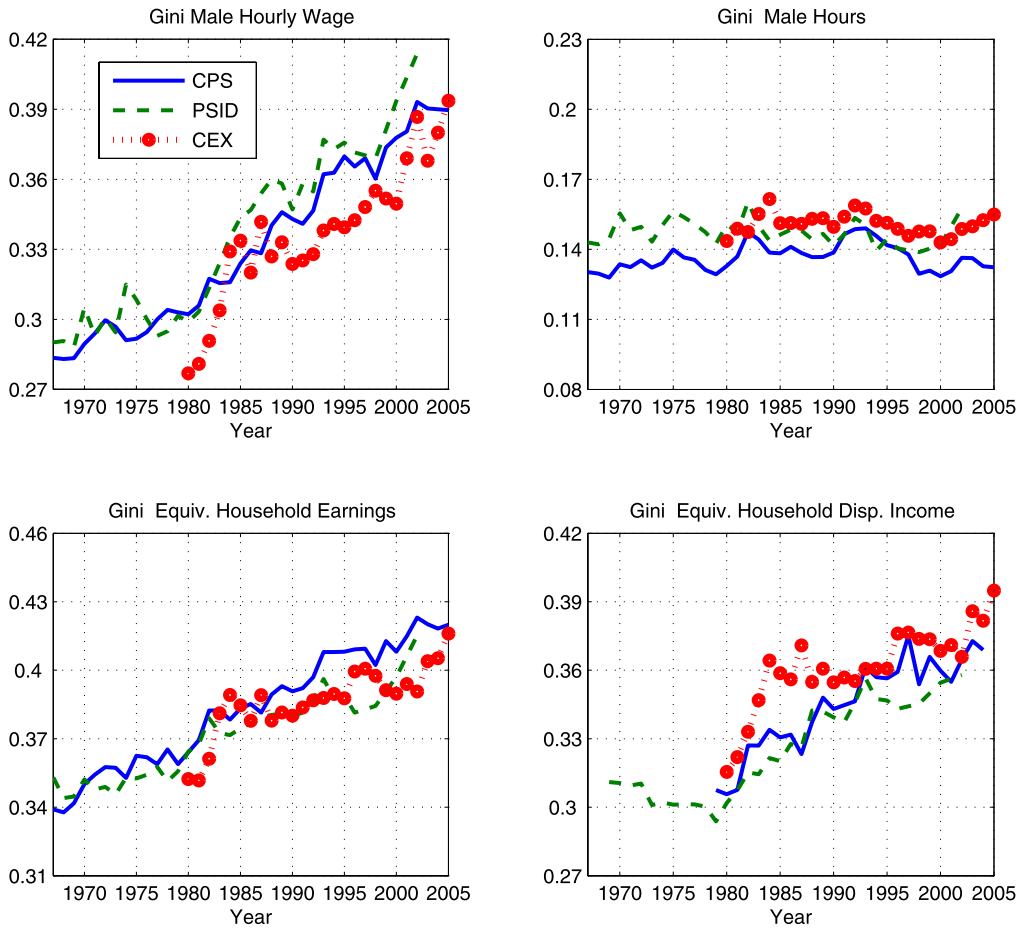


Fig. 17. Comparing the evolution of Ginis across data sets.

The top two panels of these two figures indicate broad agreement across data sets regarding inequality in wages and hours, at both ends of their respective distributions. The profiles for male wages in the CPS and the PSID align especially closely. The overall trends for male wages in the CEX are similar, but the CEX series is more volatile and indicates a more rapid increase in both the variance of log wages and the Gini coefficient for wages in the early 1980s. Compared to the CPS, the variance of log male hours is slightly lower in the PSID, and slightly higher in the CEX, though the cyclical fluctuations are remarkably similar in all three series.

A debate has developed recently on whether the rise in US wage inequality was mostly an episodic event of the 1980s, which plateaued by the end of the decade and never recurred (Card and DiNardo, 2002; Lemieux, 2006) or, rather, a long-term trend toward more wage inequality that started in the 1970s and is still ongoing (Autor et al., 2008). Interestingly, we find that both the PSID and the CEX give support to the “long-run” view.⁴⁵ Moreover, after 2000 one observes renewed growth in inequality at the bottom of the wage distribution in all three data sets. In line with our findings, Piketty and Saez (2003) use Internal Revenue Service (IRS) data to document that the top percentile wage shares started increasing in the early 1970s.

The bottom left panels of Figs. 16 and 17 plot our two measures of dispersion for equalized household earnings. Across the three data sets, the Gini coefficients track each other very closely through the entire sample period. The variance of log earnings in the CPS and CEX also line up closely over the 1980–2005 period, where both are available. However, the same panel shows a noticeable difference in the 1970s between the CPS and the PSID: the variance of household earnings in the CPS rises rapidly, while the corresponding series for the PSID is quite flat. We return to this issue below.

The series for dispersion in disposable income plotted in the bottom right panels of Figs. 16 and 17 show remarkable agreement in terms of levels and time trends for the CPS and the PSID. In contrast, inequality in disposable income in the

⁴⁵ The “episodic” interpretation of widening wage dispersion is based on the May Outgoing Rotation Group (ORG) CPS sample, which has point-in-time measures of usual hourly wages. Our measures of hourly wages in the PSID and the CEX are constructed as annual earnings divided by hours worked last year, as in the March CPS.

CEX increases more rapidly than in the CPS or the PSID, especially when measured in terms of the variance of log income. In the CPS and the PSID taxes are imputed: the CPS has an internal imputation procedure, while for the PSID we used TAXSIM (see Appendix B for more details). In the CEX, in contrast, taxes are self-reported. This differential treatment of taxes accounts for the discrepancy between the CEX and the other data sets: the variance of pre-tax income in the CEX (not plotted) closely tracks the corresponding series in the CPS and the PSID. Moreover, when we applied TAXSIM to the CEX to generate alternative estimates for taxes, we found that CEX disposable income aligns closely with the corresponding CPS and PSID series. Absent IRS data on actual taxes paid, it remains an open question whether imputed taxes or self-reported taxes are a more accurate measure of households' true tax burdens.⁴⁶

We conclude that comparing these data sets is a very useful exercise for students of inequality. The close alignment we find across the CPS and the CEX with respect to wages and earnings should give researchers more confidence when integrating CPS wage/earnings data and CEX consumption data. The close alignment we find with respect to wages across the CPS and the PSID should give researchers more confidence that models for wage dynamics estimated from the PSID panel data are consistent with the evolution of cross-sectional wage dispersion in the much larger CPS sample.

Variance of earnings: CPS versus PSID. The only striking discrepancy across data sets that we detect is the sharp increase in the variance of CPS household earnings in the 1970s, which is not apparent in the PSID. At a mechanical level, the difference can be attributed to the fact that over the period 1967–1982, the PSID reveals a much smaller drop in male and household earnings at the very bottom of their respective distributions (above the second decile of the distribution, household earnings in the CPS and PSID track each other closely). Male earnings decline by less in the PSID because male hours decline by less.

To understand why the bottom of the PSID earnings distribution evolves differently from the other data sets, recall that the PSID was designed to be representative of the US population in 1967, and in subsequent years has tracked the original families and their descendants. The PSID is likely to be imperfectly representative in later years for two reasons. First, the basic SRC sample under-represents recent immigrants, since by definition immigrants cannot be descendants of the original sample. Second, over the years there has been significant cumulative attrition from the original sample: over 50% by 1988. Fitzgerald et al. (1998) report that attritors are disproportionately non-white, older, and less educated. They are less likely to be married, and more likely to rent and to receive welfare. Attritors also work less and earn less, and have more volatile income.

Thus, it seems likely that the PSID is imperfectly representative of the bottom of the earnings and income distributions, and that this problem may have grown over time as the shares of non-white and non-married individuals in the CPS and CEX have grown more rapidly than in the PSID (see Table 2). Another reason to suspect that the PSID understates the declines in individual earnings in the lowest percentiles of the earnings distribution in the 1967–1982 period is that a decline in earnings (e.g., unemployment) increases the probability of attriting: thus attrition is particularly problematic during a period of rising unemployment and labor market instability.⁴⁷

7. Income dynamics

In labor economics, there is a long tradition of estimating structural models of income dynamics from panel data (starting from Lillard and Willis, 1978; Lillard and Weiss, 1979; MaCurdy, 1982). These models have recently been adopted by quantitative macroeconomists as a key ingredient in the calibration and estimation of heterogeneous-agent incomplete-markets models (e.g., Imrohorglu, 1989; Huggett, 1993; Aiyagari, 1994; Ríos-Rull, 1995).

In this section, we use the panel data from the PSID over the 1967–2002 period (Sample C) to estimate the dynamics of individual wages in the United States. We choose to focus on log hourly wage dynamics, since wages are the most primitive (i.e., closest to being exogenous) among the various income measures we analyze. We restrict attention to heads of households, since endogenous selection into work undermines the estimation of wage dynamics for the secondary earner.

As is common in the literature, we focus on “residual” dispersion, i.e., log wage residuals from a standard Mincerian regression with the same specification chosen for Fig. 5, run separately year by year. The variance of residual wage inequality grows by about 14 log points between 1967 and 2000 in the PSID, a rise very similar in size to that documented in Fig. 5 using CPS data. Given this upward trend, the statistical model is estimated non-parametrically to allow for non-stationarity, a standard approach in this literature since Gottschalk and Moffitt (1994).

Statistical model. Let $w_{i,c,t}$ be the residual log hourly wage for individual i of cohort c at date t . We estimate a permanent-transitory (PT) model of the form

$$w_{i,c,t} = z_{i,c,t} + \varepsilon_{i,c,t}, \quad z_{i,c,t} = z_{i,c,t-1} + \eta_{i,c,t}, \quad (2)$$

⁴⁶ In the context of our study, one reason to prefer the self-reported measure is that we have used self-reporting for virtually every other variable.

⁴⁷ Because the PSID under-represents those with low or zero earnings, it under-estimates poverty. The empirical literature on poverty consistently finds that poverty rates are lower in the PSID than in the March CPS (see, for example, Duncan and Rodgers, 1991, Fig. 1).

where $\varepsilon_{i,c,t}$ and $\eta_{i,c,t}$ are innovations that are uncorrelated over time, i.i.d. across individuals, and orthogonal to each other. Let $\sigma_{\varepsilon,t}$ and $\sigma_{\eta,t}$ denote the variances of the two shocks. As the notation suggests, these conditional variances are time-varying, but do not depend on cohort.⁴⁸

Methodology. The literature has followed two alternative approaches to estimating income processes. The first, common in labor economics (e.g., Abowd and Card, 1989; Meghir and Pistaferri, 2004; Blundell et al., 2008), uses moments based on income growth rates—or first-differences in log income. The second, more common in macroeconomic applications (e.g., Storesletten et al., 2004b; Guvenen, 2007; Heathcote et al., 2008), uses moments in log income levels. Although either approach can be used to estimate the permanent-transitory model described above, they differ with respect to the set of moments that identify the structural parameters $\{\sigma_{\varepsilon,t}, \sigma_{\eta,t}\}$.

In this section, we report estimates based on both methodologies. Exploiting PSID data until the most recent waves is challenging since after 1996 (survey year 1997), the data frequency goes from annual to biannual. Throughout, we use only moments that can be computed in biannual data. This makes estimates for the latter part of the sample, where wages are only observed at two-year intervals, entirely consistent with estimates from the first part of the sample, when workers were reinterviewed every year.

Let $\Delta^2 w_{i,c,t} = w_{i,c,t} - w_{i,c,t-2} = \eta_{i,c,t} + \eta_{i,c,t-1} + \varepsilon_{i,c,t} - \varepsilon_{i,c,t-2}$. In differences, the permanent-transitory model is estimated based on the following within-cohort covariances:

$$\text{cov}_c(\Delta^2 w_{i,c,t+2}, \Delta^2 w_{i,c,t}) = -\sigma_{\varepsilon,t}, \tag{3}$$

$$\text{var}_c(\Delta^2 w_{i,c,t}) = \sigma_{\eta,t} + \sigma_{\eta,t-1} + \sigma_{\varepsilon,t} + \sigma_{\varepsilon,t-2}. \tag{4}$$

The first set of moments (3) identifies $\sigma_{\varepsilon,t}$ for $t = 1969, \dots, 1994, 1996, 1998, 2000$. Then, given estimates for $\sigma_{\varepsilon,t}$, the second set of moments (4) identifies $\sigma_{\eta,t} + \sigma_{\eta,t-1}$ for $t = 1971, \dots, 1994, 1996, 1998, 2000$.

In levels, the same model is estimated based on the following within-cohort moment restrictions:

$$\text{var}_c(w_{i,c,t}) - \text{cov}_c(w_{i,c,t+2}, w_{i,c,t}) = \sigma_{\varepsilon,t}, \tag{5}$$

$$\text{var}_c(w_{i,c,t}) - \text{cov}_c(w_{i,c,t}, w_{i,c,t-2}) = \sigma_{\eta,t} + \sigma_{\eta,t-1} + \sigma_{\varepsilon,t}. \tag{6}$$

The first set of moments (5) identifies $\sigma_{\varepsilon,t}$ for $t = 1967, \dots, 1994, 1996, 1998, 2000$. Then, given estimates for $\sigma_{\varepsilon,t}$, the second set of moments (6) identifies $\sigma_{\eta,t} + \sigma_{\eta,t-1}$ for $t = 1969, \dots, 1994, 1996, 1998, 2000$.⁴⁹

Under the true model, none of the above moments (in levels or differences) depend on cohort, c . We therefore estimate variances at date t by averaging across all cohorts in the sample at t .⁵⁰ For example, to estimate $\sigma_{\varepsilon,t}$ using moments in levels, we use the moment

$$\sum_{c \in C_t} [\text{var}_c(w_{i,c,t}) - \text{cov}_c(w_{i,c,t+2}, w_{i,c,t})] = \sigma_{\varepsilon,t},$$

where the set C_t includes all cohorts between ages 25 and 60 in t and $t + 2$. Given the set of moments outlined above, once we pool across cohorts, all parameters are exactly and independently identified.

Findings. The parameter estimates for the permanent-transitory model for wages are plotted in Fig. 18.⁵¹ It is immediately obvious that the choice of whether to target moments in differences or in levels when estimating the model leads to diverging sets of parameter estimates. The variance of permanent shocks is three times as large when estimated in differences, while the variance of transitory shocks is larger when estimated in levels. We return to this point below.

The overall time trends in the permanent and transitory variances are somewhat more similar across estimation methods. For example, both sets of estimates suggest that the 1990s was a decade of high transitory uncertainty.⁵² Overall, both sets of estimates suggest that around half of the rise of residual wage inequality between 1967 and 2000 was transitory in

⁴⁸ In general, the model allows for cohort-specific variances of the initial condition z_0 . However, the estimation methods we implement below are based on moments that do not identify cohort-specific initial variances.

⁴⁹ By using more moments, one could identify more parameters in the early part of the sample. For example, one could use the level moment $\text{cov}_c(w_{i,c,t+2}, w_{i,c,t}) - \text{cov}_c(w_{i,c,t+1}, w_{i,c,t-1})$ or the difference moment $\text{cov}_c(\Delta w_{i,c,t+1}, \Delta w_{i,c,t})$ to estimate $\sigma_{\eta,t}$ year by year over the period when the PSID survey was administered annually. However, because one of our goals is to examine time trends in the variances of permanent and transitory shocks, we prefer to base our estimation on the same set of moments throughout the entire sample period, which dictates an identification scheme that can be applied to biannual data. In practice, we find that including moments of the type just described has a minimal impact on estimated levels or trends for permanent or transitory shocks.

⁵⁰ To increase the number of observations per cell, we define an individual as belonging to “cohort” k if her true cohort was $k - 1, k$, or $k + 1$. We discard cells/cohorts with less than 100 observations. Using all cells with positive observations and weighting by the number of observations yields very similar results for the estimation.

⁵¹ At each date t , the plotted variance of the permanent shock is simply $(\sigma_{\eta,t} + \sigma_{\eta,t-1})/2$.

⁵² In survey year 1993, the PSID shifted from manual to computer-assisted telephone interviewing. This change in methodology is often associated to a temporary increase in measurement error. Even though the values for the transitory variance in calendar year 1992–1993 may be artificially inflated, the fact that the transitory variance remains high suggests the rise in the 1990s is genuine. See Kim and Stafford (2000) for more details.

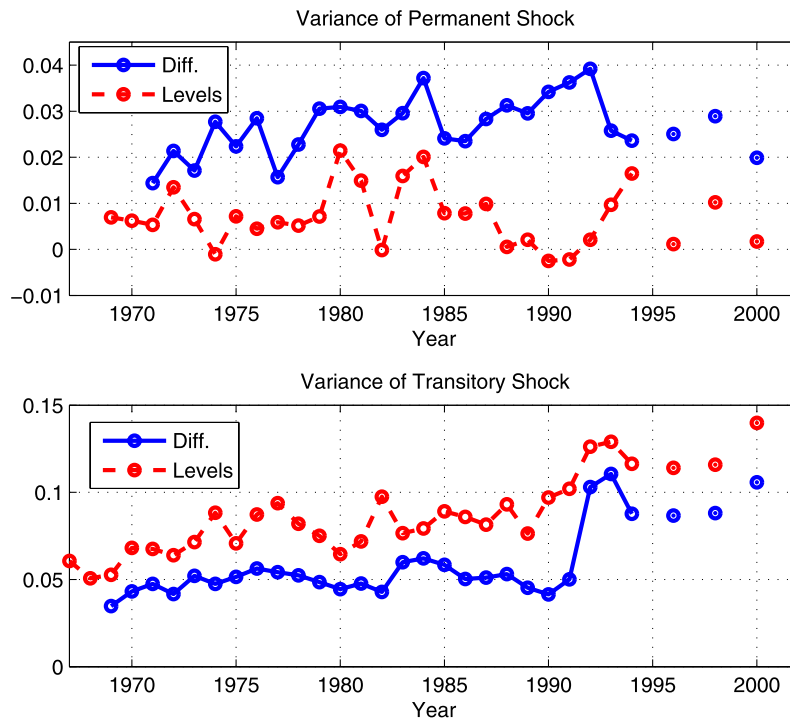


Fig. 18. Estimates of the variances of the transitory and permanent components (PSID).

nature. The finding that a significant fraction of the overall increase in wage inequality was transitory (and hence easily insurable) is consistent with the finding documented in Fig. 13 that inequality in consumption rose by less than inequality in income over this period.

We now return to the substantial divergence between the average transitory and permanent variances obtained when using the two sets of moments. This strong disagreement indicates that the permanent-transitory model is misspecified: this model, an “industry-standard,” cannot simultaneously replicate moments of the wage distribution in levels and moments in first differences. Interestingly, several papers in this issue that estimate the same model on longitudinal data for other countries reach the same conclusion (e.g., Domeij and Flodén, 2010, for Sweden; Fuchs-Schündeln et al., 2010, for Germany; Brzozowski et al., 2010, for Canada).

Misspecification is a serious concern because different estimation approaches give very different estimates for the variance of permanent wage shocks. A large quantitative theoretical literature indicates that precisely this variance is a key determinant of the welfare costs of incomplete insurance against idiosyncratic risk, and thus of the potential welfare gains from social insurance policies.

One sensible “reality check” for any variance estimate is what it implies for the growth of wage inequality over the life cycle. Fig. 14 suggests that the estimate for the permanent variance based on moments in first differences (0.027 on average) is implausibly high. This estimate implies a rise in the variance of log wages of 0.94 over the 35 years of working life from age 25 to 60, vis-à-vis the observed increase of 0.35 (0.20) when controlling for cohort (time) effects.⁵³ When estimated using moments in levels, instead, the average value for the variance of permanent shocks is 0.007, implying a realistic life cycle increase of 0.25 over 35 years.⁵⁴ However, in some years, the levels-based estimates for the variance of permanent shocks are negative—another indication of misspecification.

To sum up, the key challenge for future work is to develop a specification for the wage process that is both parsimonious enough to be used as an input to incomplete-markets models, and rich enough to account empirically for the covariance structure of wages in both levels and differences.

⁵³ When we trimmed the top and bottom 3% of the empirical distribution of log wage differences, we obtain a variance of permanent shocks of roughly the same size (and showing a similar trend) as the estimate in levels. However, the implied variance of transitory shocks is then less than one-third of its counterpart in levels. More generally, it is unclear whether trimming eliminates genuine wage variation or spurious outliers.

⁵⁴ An alternative strategy that is also able to replicate the growth of inequality over the life cycle is using moments based on log differences between dates t and $t + q + 1$, with q large enough (see Carroll, 1992). This strategy is also robust against misspecification caused by the presence of a MA(q) component in the true model. Carroll’s point estimate for the permanent variance of head labor earnings is 0.011 for the period 1967–1985, a value closer to our estimate in levels. For details, see Appendix A of his paper.

8. An exploratory look at wealth

In this section, we explore the dynamics of wealth inequality through the Survey of Consumer Finances (SCF), the best source of micro-level data on household-level assets and liabilities for the United States.

The SCF is a triennial survey of US households managed by the Board of Governors of the Federal Reserve System. The survey collects information on income (for the year preceding the survey) but focuses primarily on detailed information about household financial and non-financial assets, debts, and capital gains. The SCF survey has two parts: a standard random sample of US households, and a second sample that focuses on wealthy households, identified on the basis of tax returns. The SCF provides weights for combining the two samples. In the 2007 survey 4422 households were sampled. We exclude the 1986 survey, which was a condensed reinterview of respondents to the 1983 survey.⁵⁵

One difference between the SCF weighting scheme and the weights provided in the CPS and CEX is that the SCF weights are designed to correct for differential non-response rates by wealth. Because non-response is more common for wealthier households, the SCF tends to deliver higher estimates for average income relative to other surveys, in which the very wealthy are likely to be under-represented. To make the SCF sample more consistent with our other samples, we drop some of the highest net worth households in the SCF, choosing the fraction to drop so that mean pre-tax household income in the remaining 2007 SCF sample is equal to mean income in the 2007 CPS. This translates to dropping 1.47% of weighted observations in each year, which corresponds to 17.6% of unweighted observations in 2007 (because wealthy households are over-sampled, they are down-weighted in the SCF weighting scheme).

After this first step to align the raw SCF with the CPS, we apply the same basic sample selection criteria to the top-trimmed SCF as to the other data sets. In particular, we focus on households in which there is at least one adult of working age, and we drop households in which there are workers whose wage is below half the federal minimum. We then construct our preferred measure of earnings: wage and salary income plus two-thirds of business and farm income. Our measure of a household's net worth includes all its financial and non-financial assets (except the value of defined-benefit pension plans and claims to social security) minus all its debts. As with the other data sets, we equalize both household earnings and household net worth using the OECD scale, and then trim the bottom 0.5% of equalized observations before computing the measures of dispersion.

Dropping the top 1.47% of the net worth distribution has a significant impact on first and second moments of the distributions for earnings, pre-tax income, and net worth. In the trimmed sample, mean net worth is only 64% of mean net worth in the untrimmed sample (averaged across SCF surveys). The corresponding figures for earnings and pre-tax income are 91% and 88%. The average net worth to income ratio, a key input in the calibration in many macro models, is 4.5 in the untrimmed data, but only 3.3 in the trimmed data. Trimming also has a large impact on wealth inequality. The average Gini coefficient for net worth in the raw data is 0.77 but only 0.68 in the trimmed data. This is primarily attributable to a sharp decline in wealth concentration at the top: the share of net worth accounted for by the wealth-richest 1% falls from 33% to 14%. The conclusion we draw is that one should be very cautious when combining data on inequality in wages and earnings from the CPS or PSID, and data on inequality in net worth from the SCF. For example, one should not expect a model calibrated to wage or income dynamics from the PSID to replicate the extreme wealth inequality in the raw SCF.⁵⁶

Fig. 19 describes the key features of the trimmed SCF data. In the top two panels we compare inequality in equalized household earnings to the CPS. Both the variance of log and Gini coefficients indicate slightly more inequality in the SCF.⁵⁷ The SCF series are quite volatile, reflecting a relatively small sample size. To the extent that any trends are discernable in the noisy and intermittent measures of dispersion for the SCF, they are consistent with those in the CPS over the period when the data sets overlap: stability in the variance of log earnings, and an increase in the Gini coefficient.

The bottom two panels plot some statistics on wealth inequality in the trimmed sample. The Gini coefficient for equalized household net worth has risen by about 5 points since the mid-1990s, driven by increasing concentration at the top: the richest 10% of households increased their share of aggregate net worth from 51% to 59% between 1983 and 2007. At the bottom of the net worth distribution, the fraction of households with negative net worth and the aggregate net debts of the wealth-poorest 10% of households were lower in the 1980s and higher—but relatively stable—in the 1990s and 2000s.

Several candidate explanatory factors can account for the rise in wealth inequality since the mid-1990s. Rising transitory labor market risk (see Section 7) is one possible factor: in heterogeneous-agent incomplete-markets models, transitory income shocks transmit directly to wealth dispersion. However, other forces have also been at work. For example, since stock ownership is heavily concentrated among the rich, a booming stock market in the late 1990s amplified wealth inequality.⁵⁸

⁵⁵ For more details on the SCF, see Bucks et al. (2009) or Budría Rodríguez et al. (2002).

⁵⁶ See also Castañeda et al. (2003) for a discussion of this point.

⁵⁷ Trimming the top of the net worth distribution has very little effect on the level or trend for log equalized household earnings in the SCF but reduces the Gini coefficient by 4 points, on average.

⁵⁸ The impact of the boom in house prices between the mid-1990s and mid-2000s on overall wealth inequality is less clear-cut, given that housing is a relatively more important component of net worth for those in the middle of the net worth distribution than for households in either tail. However, the associated rise in the availability of housing-collateralized credit presumably allowed home owners to better smooth consumption in response to negative income shocks (see Favilukis et al., 2009).

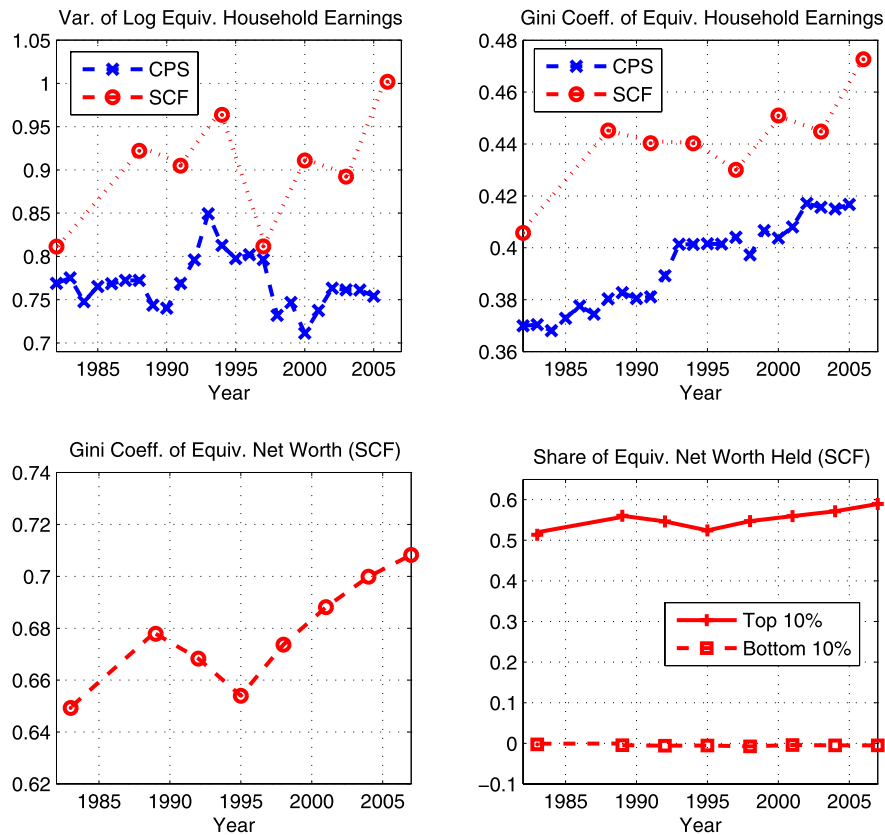


Fig. 19. Top panels: comparison between earnings inequality in SCF and CPS. Bottom panels: measures of wealth inequality (SCF).

9. Conclusions

This study sheds light on how a wide range of insurance and redistribution mechanisms operate at different points in the income distribution, and of how their respective roles have changed over the past 40 years. The importance of these mechanisms is reflected in our finding that both levels and trends in economic inequality depend crucially on the variable of analysis. Endogenous labor supply and government redistribution play especially important roles in shaping the dynamics of inequality. Future research based on structural models with heterogeneous agents and incomplete markets should therefore prioritize incorporating these features.

Substantively, we find a large and steady increase in wage inequality between 1967 and 2006. Changes in the distribution of hours worked sharpen the rise in earnings inequality in the first half of the sample, but mitigate rising inequality in the second half. Taxes and transfers compress the level of income inequality, especially at the bottom of the distribution, but have little overall effect on the trend. Consumption data suggest that access to financial markets has reduced both the level and growth of economic inequality since 1980.

Because borrowing and lending can more effectively smooth relatively transitory shocks to income, we estimated a standard permanent-transitory error component model for wage dynamics. We found that the estimated relative levels of the permanent and transitory variances are very sensitive to whether target cross-sectional moments are expressed in terms of levels or in growth rates, indicating model misspecification. Nonetheless, for two quite different identification schemes, our estimates suggest that a significant portion of the change in wage volatility was transitory in nature, and hence easily insurable through saving. The rise in wealth dispersion we uncover from SCF data is, potentially, consistent with this mechanism.

We have also identified several methodological issues that complicate comparisons across different data sets. First, comparing income or consumption data from the CPS, PSID, or CEX to wealth data from the SCF can be misleading because the SCF corrects for higher non-response rates among wealthier households, whereas the other data sets do not. Second, micro data and the aggregates from the national accounts do not line up well along two dimensions: per-capita consumption in the CEX displays almost none of the growth in aggregate consumption since 1980, and cyclical fluctuations in mean pre-tax income in the CPS are twice as large as those in the NIPA. The expansion of business-cycle analysis to richer models with heterogeneous agents is at the forefront of the research program in quantitative macroeconomics. Thus, understanding the sources of these divergences is a priority.

A final question is, what can be learned from our exercise about the likely distributional implications of the 2008–2009 recession? We have extended our analysis of the March CPS surveys to incorporate the survey years up to 2009, imposing the same sample restrictions used throughout the paper. Between income years 2002 and 2007, inequality in earnings and income is remarkably stable. Then, between 2007 and 2008, earnings inequality increases significantly with the onset of the recession, especially at the bottom of the distribution: the variance of equivalized household earnings increases by six log points. Inequality in pre-tax income (which includes government transfers and capital income) rises much less: the variance of log equivalized pre-tax income increases by two log points. Both the sharp rise in earnings inequality, and the smaller rise in dispersion for broader measures of income are typical features of past recessions in our sample (see Figs. 11 and 12).

History suggests two possible scenarios for the future. A benign prognosis would be something akin to the 1991 recession, in which economic growth quickly resumes and the increase in earnings dispersion is temporary. In this scenario, thanks to government assistance and temporary borrowing, increased inequality in earnings does not translate into inequality in disposable income and consumption. A perhaps more likely scenario is a repeat of the experience of the early 1980s, in which earnings declines at the bottom of the distribution are extremely persistent. In this case, as avenues for temporary smoothing are exhausted, a larger fraction of the growth in earnings dispersion will transmit to disposable income, consumption, and welfare. Moreover, a persistent increase in inequality now would come at a time when income disparities are already high by historical standards.

Irrespective of the precise pattern of post-recession recovery, dispersion in economic outcomes across American households will remain orders of magnitude larger than dispersion in aggregate outcomes over time, and inequality will continue to be a central focus for macroeconomists and policy-makers.

Appendix A. CPS

Survey description. Each household in the CPS is interviewed once a month for four consecutive months one year, and again for the corresponding time period a year later: a 4-8-4 rotating panel design. However, while it is sometimes possible to follow households from one year to the next, it is not always possible to match records across consecutive years. Thus we ignore the limited panel dimension to the CPS, and treat it as a pure cross section. Approximately 98,000 housing units were in sample for the 2007 ASEC (March CPS), of which 83,200 were determined to be eligible for interview, leading to about 76,100 interviews obtained.

There have been a succession of changes over time in the March CPS involving the sample construction, interview methods, data processing and imputation methods, weighting (reflecting new decennial Census population counts), and the structure and content of the questions themselves. More detailed questions about income were asked beginning with the 1976 survey, and the set of questions was expanded again in 1988.

For March 1988 two files are available: the regular and the rewrite file, which includes revised procedures for weighting and imputations (a previous change to the imputation procedure occurred in 1976). We use the rewrite file, which is recommended for comparison with future years. Two files are also available for 2001: including or excluding the SCHIP sample expansion. We use the smaller sample. The largest changes in the basic CPS survey methodology came in 1994, with the introduction of computer-assisted interviewing, and associated redesign of the questionnaire.

Weights. We use the March supplement weights to produce our estimates. We use individual weights for individual-level variables, and household weights for household-level variables.

Sample selection. Our basic sample selection strategy is outlined in the text: here we describe the details of how this applies to the CPS. To generate our Sample A, the cleaned version of the entire data set, we start by dropping households that do not have exactly one “reference person,” the person to whom the relationship of other household members is recorded (there are no such households from income year 1993 onwards). We then drop households in which there are household members with negative or zero weights (there are only a handful of such households from 1975 onwards). Next we drop households in which there are members with positive earnings but zero weeks worked (there are no such households from 1989 onwards). Next we drop households in which there is an individual whose hourly wage is less than half the legal minimum in that year. To apply a consistent sample selection rule across the whole sample period, we define the hourly wage here using the “hours worked last week variable,” which is available throughout the sample period (see below). There are no missing values for variables in the CPS, since missing values are imputed (see below). We do not exclude observations with imputed values, even if all income variables are imputed. This defines the basic “NIPA” sample, Sample A, used for comparison with BEA estimates of income in Fig. 1.

Sample B, the starting point for measuring inequality among the population of working age households, is Sample A less all households in which there are no individuals aged between 25 and 60, inclusive. Note that the CPS version of Sample B retains households as long as any household member falls in the 25–60 age range, even if both the CPS reference person and their spouse fall outside the range. The CPS estimates of average hours in Fig. 2 use all individuals in Sample B.

The estimates for measures of income inequality in Figs. 8–12, 15–17 and 19 are for a subset of Sample B. In each year, we drop households with zero household earnings. Then, for each different variable of interest (e.g. equivalized pre-tax household income) we trim the lowest 0.5% of observations. Thus, when we compare inequality across different measures

of income, the sample of households is the same for each income concept, except for variation in the identities of the 0.5% of households that are trimmed.

Hours. Recall that we compute an individual's wage as annual earnings divided by annual hours worked. To compute hours worked last year, we multiply weeks worked last year (*wkslyr*) by a measure of hours worked per week. Up to and including income year 1974 we are forced to use hours worked last week (*hours*), while from 1975 onwards a new variable (*hrslr*) becomes available which measures usual hours per week last year. One would expect this latter measure to produce a much more accurate estimate for an individual's annual hours, and thus for his annual wage.

We compute hours and wages both ways for the 1975–2005 period. For the *hrslr* version, we drop individuals in Sample A with wages (computed using that measure of hours) below half the minimum (recall that Sample A already includes a similar screen, but using the other measure of hours). Reassuringly, we find that trends in the variances of hours and wages are very similar across the two measures over this period, while there is some difference, unsurprisingly, in levels of inequality: there is less variance in wages using the better measure. We also find a very similar increase in the correlation between individual hours and individual wages using the two different approaches, though the level of the correlation is much lower using the hours-last-week question, indicating more severe division bias. For figures plotting time series moments involving wages and hours, we splice together the two series.

Prior to 1975 income year, there is a second reason why our measure of hours is of lesser quality, which is that the March CPS data files record weeks worked in intervals rather than as specific integers (even though the original questionnaires for the 1970–1975 survey years asked for integer responses). Based on the weeks-worked distributions in income years 1975 forwards, Unicon converts interval codes into estimates of cell means.

Imputation. The CPS is subject to two sources of non-response: non-interview households and item non-response. To compensate for the first data loss, the weights on non-interviewed households are distributed among interviewed households. Korinek et al. (2006) suggest an alternative procedure to deal with non-compliance based on the fact that average income and average non-response vary systematically across states. They find that it is high income households who disproportionately do not respond. While their adjustment raises the level of measured income inequality from the CPS (the Gini coefficient goes up by 4–5 points), trends are unaffected.

The second source is item non-response, meaning a respondent either does not know or refuses to provide the answer to a question. The Census Bureau imputes missing income data using a “hot deck” procedure which matches individuals with missing observations to others with similar demographic and economic information who did answer the questions. For example, the weekly earnings hot deck is defined by age, race, sex, usual hours, occupation and educational attainment. Before any edits are applied, the data are sorted geographically so that missing values are allocated from geographically close records.

We do not exclude households with imputed income because imputation is widely-used, especially for asset income categories. Thus dropping households with imputed values would drastically reduce the sample size, and call into question the appropriateness of the CPS-provided weights. Response rates for the CPS are high relative to other large household surveys, but have been declining over time. Moreover, non-response rates are higher for income than for other kinds of questions. Atrostic and Kalenkoski (2002) report response rates, defined as the percentage of all recipients (reported and imputed) who also reported an amount for the 1990 March CPS and the 2000 March CPS. Response rates for earnings from longest job (*incer1*) fell from 81.2% to 72.4%. Response rates for interest and dividend income fell from over 70% to below 50%. In terms of the share of income imputed, 26.8% of total wage and salary earnings, 43.8% of non-farm self-employment income, and 64.1% of interest and dividend income was imputed in 2000. For a significant fraction of households all income items are imputed.

Top-coding. Top-coding is an important issue to address in the CPS, both for computing means, and for measuring the evolution of inequality at the top of the income distribution. Public top-code thresholds vary widely across income categories, and across time. An additional problem is that the Census Bureau's internal data are also subject to censoring (to economize on computer tape, and to protect against gross errors). For example, the public use censoring point for the variable *incwag* (income from wages and salaries) was \$50,000 for the income years 1975–1980, \$75,000 for 1981–1983, and \$99,999 for 1984–1986. For the same variable, the internal CPS censoring points were \$99,999 for the period 1975–1984, and \$250,000 for 1985–1986.

We deal with top-coded observations by assuming the underlying distribution for each component of income is Pareto, and follow a suggestion from David Domeij by forecasting the mean value for top-coded observations by extrapolating a Pareto density fitted to the non-top-coded upper end of the observed distribution. This procedure automatically takes care of the internal censoring problem, since the internal threshold always exceeds the public use limit. It also has the advantage that in principle it adjusts appropriately to changes in top-code thresholds.

We apply this procedure at the most disaggregated decomposition of income possible. Thus, for example, for each year we divide the set of observations for the variable *incer1* (income from primary source) according to whether or not they are flagged as wage and salary or self-employment, and run separate regressions on the two sets of observations. This is important for two reasons. First, for any given individual, while one type of income may be top-coded others will not be. Second, there is more upper tail concentration in some types of income than others.

Beginning in income year 1995 the CPS started reporting cell means for top-coded observations, with cells identified by gender, race, and work experience. This allows us to assess the performance of the regression procedure. We find that the regression approach generally performs very well for most income categories. It leads us to slightly over-predict income from primary source flagged as wages and salary over the 1995–2005 income year period, and to slightly under-predict interest income.

Since our primary goal is to measure changes in inequality consistently over time, we use the regression approach for the primary income variable through the sample period, even when cell means are available. (Another reason to favor the regression approach is that there are likely errors in the reported cell means for earnings for the 2000 survey year (1999 income year): the replacement value for earnings (top-code value \$150,000) for male, non-black non-Hispanic full-year full-time workers falls from \$306,731 in 1999 to \$229,340 in 2000, and then rises to \$335,115 in 2001. Larrimore et al. (2008) were granted access to internal CPS data, and report a 2000 cell mean for this group of \$300,974.)

However, at the same time that the Census began reporting cell means, they drastically reduced public use censoring points for many income categories: the threshold for interest income declined from \$99,999 to \$35,000 between income years 1997 and 1998 and to \$25,000 in 2002, while the threshold for dividend income declined from \$99,999 to \$15,000. We found that when the distribution is truncated too far to the left, the Pareto-extrapolation procedure does not always perform well. Thus for income years 1998 to 2005 we use cell means for all income categories, except income from primary source. Unfortunately, switching from regression-based adjustment to cell means has the effect of reducing measured concentration at the top of the distribution of asset income. We therefore make an adjustment in Fig. 11 to our post-1998 estimates for Gini coefficients for income categories that include asset income. The adjustment factor is the ratio of the Gini coefficient in 1997 that emerges when top-coded observations are adjusted using the regression procedure, and the Gini coefficient for 1997 that emerges when we apply the 1998 top-code thresholds and reported cell means for asset income to the 1997 data.

Mechanically, the procedure we follow to compute top-coding adjustments is as follows. First, for a particular income variable, we identify the existence of top-coded observations. Then we sort observations in ascending order by income. The sample for our least-squares regression is the top (weighted) decile of non-zero, non-top-coded observations. For each individual i with income w_i we compute the fraction of households in our sample (including top-coded households) with income greater than w_i , which we denote v_i . We then regress $\log(v)$ on a constant and $\log(w)$, and set the adjustment factor to $\beta/(1 + \beta)$, where β is the estimated coefficient on income. For a given income type in a given year, all top-coded observations are assigned an income value equal to the top-code threshold times this adjustment factor.

Demographic variables. First we note that demographic variables (age, years of education, etc.) refer to the survey year, while questions about income refer to the previous year. We do not adjust for this timing discrepancy. Thus, for example, the CPS Sample B for income year 1980 corresponds to households who in March 1981 reported at least one household member age 25–60. *Head* If there are any 25–60 year-old males in the household, the oldest male is the head. If there are no such males, the oldest 25–60-year-old female is the head. Note that this definition of head makes no connection to the identity of the CPS reference person. *Education* We define an individual to be college educated if they have 16 years of schooling or more. *Race* We divide individuals into those identifying as “white” and those that do not, who we label “non-white.” Until 1988 the only non-white options were “black” or “other.” In 1988, American Indian and Asian were added as additional options. In 1996 the “other” option was dropped. In 2003 many new options were added.

Dispersion related to observables, and residual inequality. For the plot of residual dispersion in Fig. 5, the sample is those households in Sample B in which the head is male, and in which there are either one or two adults age 25–60. The independent variables for the two-adult households are: 3 race dummies (white–white, non-white–non-white, mixed–race), 2 sex dummies (male–female, male–male), 4 education dummies (college–college, college–non-coll, non-coll–college, non-coll–non-coll), average years of education for all adults, a quadratic in age (actual age minus 25) for the head, a quadratic in age for the non-head, number of household members below age 25, number of members above age 60. The independent variables for the one-adult households are analogous.

Income measures. Over our sample period there have been two important changes in the set of income questions asked in the March CPS, one beginning in the 1975 income year, and a second in the 1987 income year. However, these changes appear to have a negligible impact on either total income, or its division between different classes of income. The exception to this is for private transfers, which increases from 1.9% of pre-tax income in 1974 to 3.5% in 1975 (where these figures apply to Sample A).

Labor Income 1967–1986 = incwag, 1987–2005 = incer1 (if ernsrc = 1 (wage and salary)) + incwg1, where incwag = income from wage and salary; incer1 = earnings from longest job before deductions; incwg1 = income from other wage and salary.

Self Employment Income 1967–1986 = incse + incfrm, 1987–2005 = incer1 (if ernsrc = 2 or 3 (farm or non-farm self-employment)) + incse1 + incfr1, where incse = income from non-farm self-employment; incfrm = income from farm or non-incorporated self-employment; incse1 = income from other work – own business self-employment; incfr1 = income from other work – farm self-employment.

Earnings = labor income + 2/3 self-employment income.

Private Transfers 1967–1974 = incoth, 1975–1986 = incrt + incalc + incoth, 1987–2005 = incoth + incalm + inchld + incds1 + incds2 + incont + incrt1 + incrt2 + incsi1 + incsi2, where incoth = income from other sources; incrt = income from retirement funds; incalc = income from alimony and child support; incalm = income from alimony; inchld = income from child support; incds1 = income from disability income – primary source; incds2 = income from disability income – secondary source; incont = income from contributions, assistance from friends; incrt1 = income from retirement income – primary source; incrt2 = income from retirement income – secondary source; incsi1 = income from survivors income – primary source; incsi2 = income from survivors income – secondary source.

Earnings Plus = earnings + private transfers.

Net Asset Income 1967–1974 = incint, 1975–1986 = incint + incdiv, 1987–2005 = incint + incdv2 + incrnt, where incint = income from interest, dividends and net rentals; incdiv = income from dividends, rents and trusts; incdv2 = income from dividends; incrnt = income from rent.

Pre-Government Income = earnings plus + net asset income.

Public Transfers 1967–1974 = incpa + incomp + incss, 1975–1986 = incpa + incomp + incss + incsec, 1987–2005 = incpa + incss + incsec + inced + incvet + incwcp + incuc, where incpa = income from public assistance or welfare; incomp = income from unemplmt/workers comp/veterans payments/govt pensions; incss = income from social security or railroad retirement – from US govt; incsec = income from supplemental security; inced = income from educational assistance; incvet = income from veterans payments; incwcp = income from worker's compensation; incuc = income from unemployment compensation.

Pre-Tax Income = pre-government income + public transfers.

Taxes (imputed) 1979–2005 = fedtaxbc + statetaxbc + fica – eitcrd, where fedtaxbc = federal income tax liability, before credits; statetaxbc = state income tax liability, before credits; fica = social security retirement payroll deduction; eitcrd = earned income tax credit.

Disposable Income = pre-tax income – taxes.

Appendix B. PSID

Definition of “head.” The head of the family unit (FU) must be at least 16 years old, and the person with the most financial responsibility in the FU. If this person is female and she has a husband in the FU, then he is designated as head. If she has a boyfriend with whom she has been living for at least one year, then he is head. However, if she has 1) a husband or a boyfriend who is incapacitated and unable to fulfill the functions of head, 2) a boyfriend who has been living in the FU for less than a year, 3) no husband/boyfriend, then the FU will have a female head. A new head is selected if last year's head moved out of the household unit, died or became incapacitated, or if a single female head has gotten married. Also, if the family is a split-off family (hence a new family unit in the sample), then a new head is chosen.

Samples. In addition to the SRC sample, described in the main text, the second sample which belonged to the original 1968 survey is part of the Survey of Economic Opportunity (SEO) which was conducted by the Bureau of the Census for the Office of Economic Opportunity. The PSID selected about 2000 low-income families with heads under the age of sixty from SEO respondents. In 1997, the SEO sample was reduced by one-half.

In 1990, PSID added 2000 Latino households, including families originally from Mexico, Puerto Rico, and Cuba. While this sample (the so-called “Latino sample”) did represent three major groups of immigrants, it missed out on the full range of post-1968 immigrants, Asians in particular. Because of this crucial shortcoming, and a lack of sufficient funding, the Latino sample was dropped after 1995. A sample of 441 immigrant families, including Asians, was added in 1997 (the so-called “Immigrant sample”).⁵⁹

File structure of the PSID data. Information on family-level variables and on individual-level variables (for individuals in families belonging to the PSID sample) are split in two different sets of files. There are several family-level files, one for each year (*Single-year Family Files*), which contain one record for each family interviewed in the specified year. Individual income measures, and a large set of other individual-level variables (e.g., race, marital status) are contained in the *family files*. There is only one cross-year individual file with some individual-level data (e.g. education) collected from 1968 to the most recent interviewing wave (*Cross-year Individual File*). The file also contains the ID of the family with whom the person is associated in each year, which can be used to match individual-level data and family-level data.

The PSID contains many useful data supplements. The *Family Income-Plus Files, 1994–2001* contain various constructed income variables for household income and its components. The *Hours of Work and Wage Files, 1994–2001* contain constructed variables for total annual hours worked of heads and wives. The *Wealth Supplement File* includes detailed wealth information for 1984, 1989, 1994, 1999, 2001, 2003, and 2005. It can be linked to the rest of PSID data. Finally, a *Validation Study* was designed to assess the quality of economic data obtained in the PSID. The first wave of the Validation Study was

⁵⁹ The so-called PSID core sample combines the SRC, SEO, and Immigrant samples. If one plans to combine these three samples together, weights should be used.

conducted in 1983 and a second wave was conducted in 1987. For the Validation Study, the standard PSID questionnaire was administered to a sample drawn from a single large manufacturing firm. Questionnaire results were compared to company records to verify respondents' answers to questions such as earnings and hours worked. This source of data has been frequently used in the past to assess the size of measurement error in earnings and hours.

Data quality. Traditionally the PSID data has been released in two stages—an *early release* file with variables named ERxxxxx, and a *final release* file with variables named Vxxxx. The final release file contains data that has been subject to more stringent cleaning and checking processes and contains a number of constructed variables (e.g., total annual labor income of the head and wife). From 1994 on the final release files have not been made available. Instead, clean variables for labor income, annual hours and several other variables, are available in some of the supplementary data sets. These include the *Family Income-Plus Files* which contain various constructed income variables, the *Hours of Work and Wage Files*, which are used for data on annual hours worked.

Top-coding and bracketed variables. We deal with top-coded observations by assuming the underlying distribution for each component of income is Pareto, and by forecasting the mean value for top-coded observations by extrapolating a Pareto density fitted to the non-top-coded upper end of the observed distribution.

In some of the early waves, a number of income measures were bracketed. For these variables, we use the midpoint of each bracket, and $1.5\times$ the top-coded thresholds for observations in the top bracket.

Variable definitions. In the PSID all the questions are retrospective, i.e., variables in survey-year t refer to calendar year $t - 1$. The interview is usually conducted around March. When variables were not defined consistently across years (for example race was categorized differently in different years), the variables were recoded based on their original (and less detailed) coding, so as to be consistent across years.

A detailed definition of the key variables used in the study follows below:

Earnings. For heads and wives, annual earnings includes all income from wages, salaries, commissions, bonuses, overtime and the labor part of self-employment income. The PSID splits self-employment income into asset and labor components using a 50–50 rule.

Annual Hours of Work. For heads and wives, it is defined as the sum of annual hours worked on the main job, on extra jobs, plus annual hours of overtime. It is computed by the PSID using information on usual hours worked per week and the number of actual weeks worked in the last year.

Hourly Wage. It is defined as Earnings divided Annual Hours of Work.

Household Earnings. It is defined as the sum of head and wife Earnings.

Household Earnings Plus. It is defined as Household Earnings plus private transfers. Private transfers include alimony, child support, help from relatives, miscellaneous transfers, private retirement income, annuities and other retirement income.

Financial Asset Income. It includes income from interests, dividends, trust funds, and the asset part of self-employment income.

Total Asset Income. It includes Financial Asset Income plus rental income. We do not include an imputed rental value for owner-occupied housing in the definition of rental income.

Household Pre-Government Income. It is the sum of Household Earnings Plus and Total Asset Income.

Household Pre-Tax Income. It is the sum of Household Pre-Government Income plus public transfers. Public transfers include payments from the Aid to Families with Dependent Children (AFDC) program, Supplemental Security Income payments, other welfare receipts, plus social security benefits, unemployment benefits, worker's compensation and veterans' pensions. In the 1968 and 1969 interview years, many items are missing, so we start computing this measure from the 1970 survey (actual year 1969).

Taxes. An estimate of household federal income taxes, and state income taxes is computed based on the NBER's TAXSIM program. For around 400 PSID households we cannot compute income taxes since there is no information on state of residence.

Household Disposable Income. It is constructed as the sum of Household Pre-Government Income plus public transfers less federal and state taxes.

Food Consumption. It is defined as total expenditures on food eaten at home, on food eaten out of home, on food delivered, and on food purchased using food stamps. There is no food data available in the 1973, 1988 and 1989 interview years, except for food purchased using food stamps, so we omit those years in all calculations using this variable.

Appendix C. CEX

Our data come from the CEX Interview Surveys 1980 through 2006 provided by the Bureau of Labor Statistics (BLS). Consumption expenditure data are from the Family Characteristics and Income (FAMILY) files except for the years 1982 and

1983, for which the FAMILY files do not contain consumption information. For those years consumption data are from the Detailed Expenditures (MTAB) files. Consumption data for those years is fully consistent with consumption data for other years as consumption reported in the FAMILY files is just an aggregation of the information in the MTAB files. Income data are from the FAMILY files and hours worked by household members (also used to construct wages) are from the Member Characteristics and Income (MEMBER) files.

Sample size. The total sample size for the CEX is reported in Table 1 above. The sample size is not uniform across years as in 1999 there has been a major sample increase. Our basic sample (Sample A) has an average size of around 15,500 observations per year during the period 1980–1998, and its size increases to around 22,800 observations per year in the period 1999–2006.

Definition of “head.” We define the household head to be the oldest male between age 25 and 60. If there are no such males in the household we define the head as the oldest female age 25–60. If there is no such female the head is not defined (the household is not included in Sample B).

Non-durable consumption expenditures. The definition of non-durable consumption expenditures used in Figs. 12 and 13 includes the following categories: food and beverages (including food away from home and alcoholic beverages), tobacco, apparel and services, personal care, gasoline, public transportation, household operation, medical care, entertainment, reading material and education. Each observation is constructed by adding up household nominal expenditures in these categories during the three months period preceding the interview and then deflating the total using the CPI-U for that period. A change in survey methodology (see Battistin, 2003, for details) causes a sizeable (about 15%) systematic downward bias in reported food expenditures for all the observations in the years 1982–1987. In order to correct for this bias, we regress the log of food expenditures for all years on a quadratic time trend, on quadratics in income and total non-food consumption expenditures, on weeks worked, on a complete set of household characteristics (including age, education, region of residence, and family composition), on a dummy for the period 1982–1987, and on the interactions term of the dummy with all other independent variables. We then use the regression coefficients to scale up food expenditures for every observation in the period 1982–1987.

Wages, earnings and disposable income. Earnings of each household member are computed as the sum of wages and salaries plus two thirds of business and farm income earned by that member. Hours worked by each member are computed as number of weeks worked during the year times the number of hours per week usually worked by that member. Wages are computed as earnings divided by hours. Household earnings are the sum of earnings of each household member. Household disposable income includes the sum of wages, salaries, business and farm income earned by each member plus household financial income (including interest, dividends and rents) plus private transfers (including private pensions, alimony and child support) plus public transfers (including social security, unemployment compensation, welfare and food stamps) minus total taxes paid (including federal, state, local and social security contribution).

Imputation. Until 2004 the CEX did not use imputation methods to derive income for non-responses. For the years 2004 and 2005 income imputation is used and it is not always possible to select out only observations with non-imputed measures. In 2006 more information is provided in the survey and thus it is possible to select only observations with non-imputed measures. For consistency, when possible, we use only observations with non-imputed measures.

Top-coding. Only a very limited number of consumption categories are subject to top-coding. In particular within non-durable consumption expenditures only some categories of medical spending (such as hospital services) are subject to top-coding. We do not attempt to correct for it and we simply use the value reported by the CEX (the value is equal to the top-coding threshold before 1996 and equal to the mean of the top-coded observations after 1996). Top-coding in earnings is potentially more important as the fraction of top-coded earnings observations in some year can reach 2% of the sample. Also public top-coding thresholds vary across income categories, and across time. We deal with top-coded observations in the CEX following a procedure as close as possible to the one followed in CPS. We assume that the underlying distribution for each component of income is Pareto, and we forecast the mean value for top-coded observations by extrapolating a Pareto density fitted to the non-top-coded upper end of the observed distribution. This procedure automatically adjusts appropriately to changes in top-code thresholds.

We apply this procedure separately to the three components of individual earnings (salary, business income and farm income). Some components of disposable income (such dividends or interests) are also subject to top-coding but, since the fraction of top-coded observations never exceeds 0.1% of the sample, we simply use the value reported by the CEX (the value is equal to the top-coding threshold before 1996 and equal to the mean of the top-coded observation after 1996).

Time aggregation. We assign an observation to a given year if the interview is completed in that year.

Weighting. All annual aggregate consumption measures (Fig. 3) are computed using weighted data from annual cross sections. All annual consumption inequality measures are computed using unweighted data from annual cross sections. Inequality measures are basically not affected by weighting.

Non-overlapping income and consumption. As mentioned in the main text a given household is interviewed in the CEX a maximum number of 4 consecutive quarters. Each quarter the household members is asked to report consumption expenditures information but income questions are only asked to the households during the first and fourth interview. So income information reported for households in the 2nd and 3rd interview is the same as the one reported in the first interview. This implies that for roughly half of our CEX observations income and consumption do not refer to an overlapping period. See Gervais and Klein (2008) for a detailed analysis of this issue. In order to assess whether this issue affects the relation between income and consumption inequality (Figs. 12 and 13) we constructed a sample where we selected only households that are in the CEX for all 4 interviews and we constructed consumption as the sum of consumption over all 4 interviews and used income in the last interview. In this case the measures of income and consumption fully overlap. Results for the alternative sample are very similar to our basic sample (the only difference is that the alternative sample is more volatile over time as the sample size is significantly smaller).

Appendix D. Comparability

The unit of analysis in the CPS and the CEX is the household, while in the PSID it is the family unit. In addition, prior to 1975 and post 1994, labor income and hours worked are not reported in the PSID for household members who are not heads or spouses. Thus all our labor market statistics for the PSID refer only to heads and spouses, whereas in the CPS and the CEX we also include other adult household members.

Individual labor income is defined in all three surveys as the sum of all income from wages, salaries, commissions, bonuses, and overtime, and the labor part of self-employment income. The CPS imputes values for missing income data, while the PSID and the CEX do not. In CPS and CEX data we allocate 2/3 of self-employment income to labor and 1/3 to capital, while the reported PSID income data builds in a 50–50 split. Only in the CEX is it possible to impute rents from owner-occupied housing across the entire sample period, so for the sake of consistent measurement we exclude imputed rents throughout.

The calculation of taxes differs across data sets. The PSID includes a variable for household income taxes only up until 1991. Rather than using this variable, we use the NBER's TAXSIM program to calculate an estimate of household federal and state income taxes that is comparable across all years in the sample. The CPS contains imputed values for federal and state income taxes, social security payroll taxes, and the earned-income tax credit for the 1979–2004 income years. The CEX asks each household member in the second and fifth interview to report taxes paid (federal, state and local) in the previous year.

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