for "Has Consumption Inequality Mirrored Income Inequality?" by Mark Aguiar and Mark Bils

In this appendix we describe construction of the variables in our data set and the impact of sample restrictions. All data are available from the authors' web page.

Construction of variables from CE

The income variables we examine are total household labor earnings, total household income before tax, and total household income after tax. These variables are principally based on responses in the last quarterly interview that cover income from the previous 12 months. Household labor income sums all household member earnings, before deductions, over the past 12 months. The before-tax income in the CE (FINCBTAX) includes labor earnings, business (including farm), and professional income, interest, dividend, rental, and royalty income, income from social security and railroad retirement benefits, income from pensions and annuities, scholarships or stipends, workers' compensation and veterans' benefits, and alimony and child support received. It also includes the following transfer payments: public assistance (welfare) payments including those related to job training, food stamps, supplemental security income, and unemployment benefits.

We adjust this measure of before-tax income in the following ways to be consistent with budget accounting. We add in food as pay and other money receipts. The latter includes lump-sum receipts of alimony and child support, lump-sum receipts from estates, selling household items, prizes or gambling winnings, and refunds of insurance payments, property taxes, or employer over withholding on social security taxes. We subtract alimony and child support payments, to be consistent with those receipts being treated as income. We also subtract expenditures that we do not treat as consumption. These include life insurance premiums, occupational expenses, fees for financial services, finance charges, legal fees, funeral expenses, moving expenses, and support for college students. We treat the implicit rental from owner-occupied housing both as a component of expenditures and a part of income. So we add home owner's estimate of rental equivalence to before-tax income. At the same time we subtract expenses of home ownership for mortgage interest, property taxes, expenditures for capital repairs and replacements, home insurance, security systems, pest control, and other maintenance expenses both from income and expenditures.

We subtract personal taxes from our measure of before-tax income to arrive at a measure of after-tax income. These taxes include federal, state and local income taxes. We also

subtract the income contributed to social security by all household members during the year, as well as contributions for government or railroad retirement programs. The CE measure of social security contributions is estimated by the BLS. Our measure of after-tax income differs from the CE measure (FINCATAX) due to all the adjustments listed above to before-tax income, and because we subtract contributions to social security, government, and railroad retirement programs. We consider an alternative measure of after-tax income by replacing self-reported federal income taxes with taxes calculated from the NBER's TAXSIM program. We do not adjust for state and local taxes, as we do not know the state of residence for many households in the CE. We also considered replacing social security taxes with TAXSIM values, but this has little effect on the results. This is not surprising, as the social security contributions in the CE are estimated by the BLS as well.

We aggregate CE expenditure items into 20 groups, as described in the text. Our definitions of expenditures by good closely follow definitions in the CE with a few exceptions, most notably for housing services. As in the CE, for renters we define housing by rent paid. But for home owners we use self-reported rental equivalence rather than out of pocket expenditures. This adjustment was described above in discussing adjustment to income. For the eight quarterly surveys conducted in 1980 and 1981 households were not asked about rental equivalence. We impute the rental equivalence for homeowners in these early waves as follows. We use the two years of surveys conducted in 1982 and 1983 and regress reported rental equivalence on total expenditures minus out of pocket housing expenditure, after-tax income, and a set of dummies for age, marital status, family size, and number of earners. We then fit this regression for the earlier waves that do not report a housing service measure. For vacation homes there is no measure of self-reported rental equivalence before 1999. So, for all years, we measure expenditures on vacation homes, like the CE, based on expenditures for mortgage interest, taxes, and maintenance.

We differ from the CE measure of expenditures on vehicles in that we subtract the value of used vehicles that are sold by a household, even when this is separate from any vehicle purchase. (Both our measure and the CE expenditure on vehicles, by using net payments for vehicle purchases, implicitly deducts the value of vehicles traded in as part of purchases.) We also adjust the reported expenditures on food at home in the CE for the 1982 to 1987 waves. Spending on food at home shows a distinct drop for these waves, apparently reflecting a difference in the questionnaire wording from other waves. To adjust for this drop, we increase food at home expenditure by 11% for these waves. This 11% adjustment is derived from a regression for surveys 1980 to 1989 of log food at home expenditures on log after-tax income, log total expenditure, quadratic time trends, and a zero/one dummy variable that

equals one for the waves from 1982 to 1987. This adjustment is similar to that in Krueger and Perri (2006).

Our measure of total expenditure will differ from the BLS measure of total expenditure in the CE (TOTEXP) due to these adjustments. It also differs because we treat a set of expenditures (e.g., alimony payments, life insurance, financial fees, social security contributions) as deductions from income, rather than as consumption expenditures. We also treat payments to private pensions as a component of savings, whereas the CE includes these as part of total expenditure.

We examine robustness of our results to excluding durable spending. We define durables following NIPA conventions.[1] Eighty-three percent of total expenditures for our sample remain after excluding durables. Eleven of our twenty categories are entirely nondurable. Several others retain the vast majority of their spending. (For instance, dropping durable equipment from health spending reduces that categorys spending by 4 percent.) The categories most affected are furniture and fixtures, reduced by 84 percent, and vehicle purchasing, leasing, and insurance, reduced by 75 percent. The reduction in importance for such categories is captured in our estimation, as we weight categories by expenditure shares.

The CE asks respondents a number of questions on active savings. The BLS employs these responses to publish statistics on net changes in assets and liabilities (see addenda to Current Expenditure Tables, www.bls.gov/cex/home.htm #tables). In each quarterly interview, households report the net change in savings accounts and purchases and sales of stocks and other financial assets. In addition, households report new loans undertaken, including mortgages and home equity loans, and reports equity payments against mortgages and other loans. Households also report purchases and sales of real assets including houses, businesses, home improvements, and vehicles. They report the net changes in money borrowed or loaned to other households. The CE records the total outstanding credit balances in the first and fourth interviews covering expenditures, which are 9 months apart. We estimate net payments of credit by subtracting the fourth interview's value from that in the first, and annualize by multiplying by 4/3. (Because all other responses for savings already reflect changes in assets or changes in liabilities, these do not require differencing across interviews.)

Our measure of net changes in assets and liabilities differs in a couple respects from the CE measures reflected in BLS published statistics. The primary difference is that we add payments into private pensions as a form of savings (not as a component of expenditures).

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[1]One exception is vehicle leases. Unlike NIPA, we treat these as durable as many of these expenditures in the CE are lump-sum payments. We follow NIPA in treating housing services (rental equivalence) as nondurable expenditure.

Secondly, we do not include net purchases of vehicles, as we treat these as a component of expenditures.

As discussed in the text, the data on new mortgages in the CE raise the question of whether the CE accurately records the net effect of refinancing on savings. We observe a number of reported new mortgages without a corresponding purchase of a house or a significant paying down of an existing mortgage. The CE data imply an average "cash out" percentage of 73 percent from new mortgages not associated with a house purchase, a rate not supported by studies of refinancing. For instance, Greenspan and Kennedy (2007), find that 13 percent of the value of new mortgages is taken in the form of cash, not used to pay off existing mortgages or to pay related fees. To address this potential measurement error, we construct an alternative measure of household savings that caps the amount of net borrowing (cash out) associated with new mortgages at one third the size of that mortgage. This reduces the average implied cash out ratio of refinanced mortgages to 14 percent, close to the number reported by Greenspan and Kennedy (2007).

Lastly, we create demographic variables for age of the reference person (identified by who owns or rents the residence), the number of household members, and number of household earners, with all variables based on responses in the households final quarterly interval. These variables are used to divide households within each of five income groups into cells, as described in the text.

The impact of sample restrictions

We impose a set of sample restrictions; the impact of these restrictions is reported Table A 1. We begin with 252,758 households for the 1980-2010 surveys. We aggregate expenditures for each household across the four interviews–so each household appears only once in the sample. There is considerable attrition across surveys. The BLS responds to attrition by introducing households with the second, or later, survey instrument, so as to keep a balanced panel across interview quarters. Focusing on households that begin with the first survey instrument reduces the potential sample of households to 186,716.

We make the following restrictions on the sample. The 1981 through 1983 surveys include only urban households. For consistency we restrict the samples to urban households for their entirety. This reduces the sample by 9 percent to 170,319. We restrict households to those with reference persons between the ages of 25 and 64, reducing the sample by 28 percent to 122,514. In order to contrast household expenditures with income, it is necessary to have

Table A 1: Sample Construction

Total Number of Households	252,758
Households who enter at "first" interview"	186,716
After Sample Restriction:	
Urban	170,319
Ages 25 to 64	122,514
Full-year of Interview Coverage	84,850
Complete Income Reporter	72,791
No Expenditure Outliers	69,702
Truncate Before-Tax Income: 5-95 pctile (Final Sample)	62,734

Note: This table reports the sample size after each restriction. The first row reports the original CE sample obtained from the BLS. Each sample restriction is discussed in the data appendix. The final row represents the sample used in the analysis.

measures of expenditures and income over comparable periods. In turn this requires that households participate in all four interviews in order to be present for the income variables in the final interview. This reduces the sample by 31 percent to 84.850. We require households to be "complete income reporters," which the BLS defines as respondents with values for some major source of income, such as wages, self-employment income, or Social Security income. (Even complete income reporters might not have provided full accounting for all household members.) This restriction reduces the sample by 14 percent to 72,791. We drop households that report implausibly large spending on smaller goods categories. More exactly, we require that households spend less than half of their after-tax income on any category, unless it is housing, food, or vehicle purchases. This restriction reduces the sample by 4 percent to 69,702. (Of those eliminated, 928 households showed negative or zero after-tax income.) Lastly, in order to eliminate outliers and to mitigate the impact of time-varying top-coding, we exclude households in the top and bottom five percent of the before-tax income distribution. (The fraction of households top coded on income fluctuates from about one to just over four percent across survey waves.) This results in a sample of 62,734 households.

References

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Homes," *Federal Reserve Board Finance and Economics Discussion Series 2007-20*, 2007.

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