

NBER WORKING PAPER SERIES

FOOD STAMPS AND AMERICA'S POOREST

Dean Jolliffe
Juan Margitic
Martin Ravallion

Working Paper 26025
<http://www.nber.org/papers/w26025>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2019

No financial support was received beyond salaries of the researchers. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2019 by Dean Jolliffe, Juan Margitic, and Martin Ravallion. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Food Stamps and America's Poorest
Dean Jolliffe, Juan Margitic, and Martin Ravallion
NBER Working Paper No. 26025
June 2019
JEL No. I32,I38

ABSTRACT

The paper provides the first assessment of: (i) America's progress in lifting the lower bound—the floor—of the distribution of real income; (ii) whether the country's largest antipoverty program, SNAP ("food stamps"), helped do so. An operational method of estimating the floor is implemented on micro survey data spanning 30 years, with various robustness and significance tests. SNAP partially compensated the poorest, and helped stabilize the floor. Nonetheless, the floor has been sinking over the last 30 years. The efficiency of SNAP in lifting the floor has declined over time. Full coverage of the poorest would lift the floor appreciable.

Dean Jolliffe
Development Data Group
World Bank
Washington, DC
djolliffe@worldbank.org

Juan Margitic
Department of Economics
Georgetown University
Washington, DC
jfm114@georgetown.edu

Martin Ravallion
Department of Economics
Georgetown University
ICC 580
Washington, DC 20057
and NBER
mr1185@georgetown.edu

1. Introduction

While it is well-known that top incomes in the US have risen appreciably over the last 30 years, it is unclear if this has come with gains to the country's poorest. One might look for evidence in the official poverty measures. In 1988, 13% of the US population were deemed poor based on official thresholds (U.S. Census Bureau, 1991). In 2016, almost 30 years later, the poverty rate was still about 13% (U.S. Census Bureau, 2017a).¹ However, such comparisons tell us nothing about how the worst-off families are doing. By definition, the poorest live at the floor—the lower bound to the distribution of real income.² As commonly measured, the poverty rate (or other “higher-order” measures) could show no change while the floor has been lifted, or has even fallen.

It is an open question as to whether the type of economic growth we have seen in the US in recent decades has helped the poorest. Given the initial inequalities in human capital, growth stemming from external trade expansion or technological change could well have adverse effects on less skilled wages, putting further downward pressure on already low incomes.³ Rising downside risk over time could also be a greater threat to poor families, especially the poorest.⁴ From what we know it remains unclear whether rising top incomes in the US have lifted the floor.

Concerns that the poorest might be left behind have prompted social-policy responses in the US as elsewhere. There are continuing public debates as to whether such programs are having much impact on poverty. The lack of longer term progress in reducing the country's poverty rate has led some observers to argue that America's antipoverty programs have failed, which is seen to strengthen the case against those programs.⁵ Of course, the observed measures

¹ The official poverty rate was 13.0% in 1988 and 12.7% in 2016 based on official poverty thresholds which are kept constant in real terms. This difference is not statistically significant—the test of equality has a p-value of 0.25. The official measures do not include the value of non-cash programs such as the Supplemental Nutrition Assistance Program. We return to this issue.

² This should not be confused with the biological minimum for survival; the floor is hopefully above that.

³ For example, Ebenstein et al. (2014) find evidence that wages for relatively unskilled occupations in the US fell in response to greater trade openness and outsourcing over the period 1984-2002. They attribute this to a shift in employment out of higher-wage manufacturing sectors to less remunerative occupations. Autor et al. (2003) and Goos et al. (2014) also argue that technological change in many rich countries (including the US) has displaced labor in more routine tasks, also putting downward pressure on wages for less skilled workers.

⁴ Hardy and Ziliak (2014) provide evidence of rising income volatility at both the bottom and top of the US income distribution.

⁵ Various reasons are heard including a lack of political will (possibly due to high and rising inequality), weak administrative capacity for policy implementation (especially in poor places), lack of awareness among poor people of their rights, incentive effects (discouraging work or savings), and social stigma associated with targeted programs.

do not tell us about the counterfactual of what would have happened in the absence of the programs. The bulk of the academic literature using some form of counterfactual analysis suggests that America's antipoverty programs have typically reduced the incidence of poverty, though there has been debate on the extent of this impact.⁶ Yet the gains to the poorest remain unclear. Furthermore, rising high-end inequality of income and wealth can come with greater lobbying against welfare programs. Even if the programs are deemed effective—in the specific, albeit narrow, sense that participants tend to be poor—political pressure from rich donors and their lobbyists can entail weak coverage, which limits the impact on the poor, including the poorest.

This paper aims to assess the gains to America's poorest from the country's largest antipoverty program, the Supplemental Nutrition Assistance Program (SNAP), colloquially referred to as "food stamps."⁷ Over the last 20 years or so, food stamps have become one of the most important elements of America's social safety net (Ziliak, 2016). While the paper's focus on the poorest is a novel feature in the context of the literature on poverty in America, that focus has deep roots in theories of distributive justice whereby a society's progress is judged in part by its ability to enhance the economic welfare of the least advantaged group. Famously, this is one of the principles of justice proposed by Rawls (1971).

Assessing the impact of any social program on the floor poses methodological challenges, given that conventional surveys are not designed for this purpose. Even in high quality surveys, the tails of the distribution are not likely to be measured accurately. Recorded incomes in surveys can be very low for some household in a survey response period, but this can be deceptive about living standards.⁸ Our method recognizes that there are both measurement errors and smoothable transient income effects in the observed survey data. Some averaging is called for. The idea is to measure the floor as a weighted mean of those deemed to be poor, with higher weight on observationally poorer people, following Ravallion (2016).

We implement an operational approach to measuring the floor in the US using cross-sectional national surveys spanning 30 years, and we use it to study the impact of SNAP. We estimate the level of the floor before and after recorded receipts from SNAP at the household

⁶ See, for example, Meyer and Sullivan (2012) and the comment by Hoynes (2012).

⁷ Spending on SNAP overtook the next largest program, the Earned Income Tax Credit, in 2010 (Hoynes, 2012).

⁸ See, for example, the analysis by Meyer et al. (2019) of the claims made about the numbers of people in the US living below \$2 a day (Edin and Shaefer, 2016).

level. This requires some potentially strong assumptions, so we provide various robustness tests, including tests of alternative weighting schemes, possible behavioral responses to SNAP among the poorest and the sensitivity of our results to including non-positive incomes. We also provide confidence intervals for the floor, taking account of survey design.

The paper finds that America's poorest gained from SNAP. Their mean gain exceeded mean spending on SNAP—implying that the program has historically done better than a universal basic income (UBI) with the same budget (and ignoring any differences in administrative cost, which could well be lower for a UBI). Alarming, we find that America's floor has been on a long-term decline, although SNAP helped prevent further decline in some periods, notably during the 2008-11 financial crisis. However, the efficiency of SNAP in raising the floor has also fallen over time, such that the per capita amount received by the poorest in 2016 is about the same as mean spending per capita.

2. Background on SNAP

SNAP is a Federal program (administered by the US Department of Agriculture) that aims to help targeted poor families purchase food. Prior to around 2000, paper coupons (“food stamps”) were provided but the program switched over time to Electronic Benefit Transfer (EBT).⁹ In (fiscal) 2016, SNAP covered about 44 million Americans (14% of the population) at a cost of \$71 billion, representing \$125 per person per month for food stamp beneficiaries. The program aims to reach families with gross income below 130% of the poverty line (with net income, allowing for certain admissible deductions, less than the poverty line), and to provide larger benefits to poorer families.¹⁰ (There is some variation in eligibility criteria across states.)

Much has been written about SNAP. While it is unclear how effective the program is in reaching the poorest, there is ample evidence that the beneficiaries are largely poor (Bartfield et al. 2016).¹¹ However, there appears to be a reasonably high rate of nonparticipation among those eligible for the program (Gundersen and Ziliak, 2018). The take-up rate of the benefits is

⁹ EBT entailed that SNAP recipients paid for food using a “debit card.” (If the pin code is verified and the account balance is adequate then payment is accepted.)

¹⁰ For further information and analysis on SNAP see Bartfield et al. (2016), Hoynes and Schanzenbach (2016) and Gundersen and Ziliak (2018). On the benefits to children from poor families see Jolliffe et al. (2005). For a broader overview of antipoverty policies in the US, including SNAP, see Ben-Shalom et al. (2012).

¹¹ A positive correlation has been found between SNAP participation and food insecurity; the causal interpretation is unclear, but the correlation is consistent with the view that families that face an income shock, creating food insecurity, turn to the program for help (Gundersen and Oliveira, 2001; Wilde and Nord, 2005).

difficult to estimate in national surveys largely due to the complexity of the eligibility criteria.¹² That same complexity generates transaction costs for applicants associated with establishing and maintaining eligibility, and naturally those costs tend to lower participation in the program among those eligible.¹³ Falk et al. (2015) estimate that about 30% of people who are eligible for SNAP are not participating, which is close to earlier estimates reported in Ways and Means Committee (2000, p.884). The existence of exclusion errors has also been noted in the media.¹⁴ In addition to the high nonparticipation rate in general, there is some evidence that poorer families among those eligible have fallen off the rolls of SNAP with cuts to this and other programs.¹⁵ And there is evidence that those in need often find it most difficult to register for the program (Currie and Gahvari, 2008).

Two specific SNAP policy changes are worth noting. First, a series of reforms in 1996-98 put emphasis on reducing perceived “leakage” to those not considered eligible, including by imposing work requirements, which can reduce participation by eligible participants.¹⁶ Time limits and recertification became stricter. Legal immigrants were variously eligible, then ineligible, then eligible again, but growing concerns about status likely reduced their participation. Able-bodied adults without dependents found it harder to access SNAP.

Second, spending on SNAP surged in the aftermath of the 2008 financial crisis and the subsequent rise in unemployment and poverty. As a part of the American Recovery and Reinvestment Act (ARRA), SNAP benefits increased by 14% in April 2009.¹⁷ This “SNAP stimulus” echoes a longer-term pattern of (countercyclical) co-movement between SNAP spending and the unemployment rate (Hoynes and Schanzenbach, 2016; Ganong and Liebman,

¹² See Zedlewski and Giannarelli (2015) for an example of modelling SNAP eligibility with the TRIM3 model, and a discussion of the various data inputs required to estimate eligibility.

¹³ As an example of transaction costs, Ponza et al. (1999) estimate that the average time to apply for the program is approximately five hours and two trips to a program office, and recertification of eligibility takes on average 2.5 hours and at least one trip. Currie (2003) synthesizes the literature on nonparticipation of those eligible, including, for example, a discussion of the potential stigma associated with program participation (Haider et al., 2003).

¹⁴ See, for example the discussion in *The Economist* (2011).

¹⁵ Many SNAP participants are automatically eligible with enrollment in other social assistance programs. The reduction of other assistance programs had adverse spillover effects on enrollment in SNAP.

¹⁶ The first reform was the Personal Responsibility and Work Opportunities Reconciliation Act of 1996 (with most provisions effective from mid-1997), followed by the Balanced Budget Act of 1997 and the Agricultural Research, Extension, and Education Reform Act of 1998 (USDA, 2017). For further discussion of these reforms to SNAP and their implications see Currie and Grogger (2001).

¹⁷ For example, the maximum SNAP benefit for a household of three rose from \$463 to \$526 per month. Under ARRA, states could suspend time limits for unemployed able-bodied adults. Subsequent legislation imposed an expiration date of November 2013 for the 13.6% SNAP benefit increase.

2018). Anderson et al. (2015) present evidence that SNAP participation and benefit levels rose among America's poor during the recession starting in 2008.

3. Measuring the floor and the impact of SNAP

We need a measure that can be implemented with repeated cross-sectional surveys, while recognizing the uncertainty as to whether the lowest observed consumption or income in any one survey is in fact the floor. Following Ravallion (2016) we postulate that any observed income level within a stratum of poor people has some (non-zero) probability of being the true floor, once the transient effects and measurement errors are eliminated. One might consider simply taking the ordinary (equally-weighted) mean income of a subset of people that is considered highly likely to include the poorest, such as those living below the poverty line. However, it is not plausible that all incomes below some point (above which the probability is zero) are equally likely to emerge as the lowest level of living if one could remove the transient effects and errors. Of course, the relevant probabilities are not data, and assumptions will be required in lieu of the missing data (as in all areas of economic statistics). The key assumption in Ravallion (2016) is that the probability of being the poorest household declines as the observed measure of income rises. While we are uncertain as to whether the lowest observed value is the floor, it is assumed that this value has the highest probability of being the floor—that our data are sufficiently good to believe that the probability is highest for the person who appears to be the worst off, though the probability may be well below unity. Beyond some income level there is no chance of finding the true floor.

A potentially important qualifier in this context is that we are measuring the floor using data on incomes not consumption. (We describe our data more fully below.) Incomes in a given year can be zero (or negative) due to an income shock in that year, although this need not be indicative of current living standards given the scope for drawing on savings or credit. This is an instance of the transient effects one would like to average out. In this respect, consumption is preferable.¹⁸ For developing countries over a 30-year time period, Ravallion (2018) tests the sensitivity of estimates of the floor to using consumption instead of income, and finds a slightly

¹⁸ There has been a debate on the choice between consumption and income as measures of economic welfare, including in the US (Slesnick, 2001; Meyer and Sullivan, 2012). We do not take a position on this issue here, as we have little choice for the present purpose given both the data available and the fact that (unlike consumption) income is additive with respect to SNAP spending.

higher floor using consumption, but with a very similar evolution over time. Nonetheless, given the possible concerns about the non-positive incomes, we test the effect on our estimates of dropping them. In other words, we assume instead that the lowest observed positive income has the highest probability of being the poorest, and the probability declines as observed income rises above that point.

Derivation of the floor: Let the observed distribution of household income be $y_i, i=1, \dots, n$. We postulate a corresponding (unobserved) distribution y_i^* , after eliminating the ignorable transient effects and measurement errors. Let the floor of the y_i^* distribution be denoted $y_{min}^* = \min(y_i^*, i = 1, \dots, n)$. We treat y_{min}^* as a random variable, with a probability distribution given the data such that our estimate of y_{min}^* is $\sum_{i=1}^n \phi_\alpha(y_i) y_i$ where $\phi_\alpha(y_i) = Prob(y_i = y_{min}^*)$ is the probability that person i , with y_i , is the worst off person. Following Ravallion (2016), we assume the following functional form satisfying these assumptions:

$$\phi_\alpha(y_i) = \frac{I_i \left(1 - \frac{y_i}{z}\right)^\alpha}{\sum_{y_j \leq z} \left(1 - \frac{y_j}{z}\right)^\alpha} \text{ for } \alpha > 0 \quad (1)$$

Here $I_i = 1$ if $y_i \leq z$ and $I_i = 0$ otherwise. For $\alpha > 0$, $\phi_\alpha(y_i)$ attains its maximum value for $y_{min} = \min(y_i, i = 1, \dots, n)$ and then falls monotonically with y_i , until it reaches zero at some value z , above which there is no chance of someone with that income being the poorest. Under this assumption, it can be readily shown that:

$$E(y_{min}^* | y_i, i = 1, \dots, n) = z(1 - P_{\alpha+1}/P_\alpha) \quad (2)$$

where $P_\alpha \equiv \frac{1}{n} \sum_{y_i \leq z} \left(1 - \frac{y_i}{z}\right)^\alpha$ is the Foster-Greer-Thorbecke (FGT) (1984) class of poverty measures. However, notice that the parameter α has a different interpretation to the FGT index; here α is the key curvature parameter for the probability function used for weights in estimating the floor, instead of being an ethical inequality-aversion parameter as in the FGT index.

Empirical implementation: As our benchmark case for the empirical implementation, we set $\alpha = 1$. Then the estimate of the floor is $z(1 - SPG/PG)$ where SPG is the squared poverty gap index (P_2) and PG is the poverty gap index (P_1). Since this is the expected value of the lower bound, after averaging out transient effects and measurement errors, we will observe incomes below this number in any survey. For the floor to rise over time, SPG must fall faster (in proportionate terms) than PG .

We assume that the poorest household has an observed income below the official US poverty threshold, which is fixed in real terms over time. However, the official poverty thresholds vary by family size and composition.¹⁹ It is thus simpler to express the floor as a proportion of the threshold, though to aid interpretation we provide some illustrative calculations for a family of four with two children. We refer to the ratio of the floor to the poverty line as the floor ratio.

We consider two variants of this approach. In a first variation, we test robustness to using $\alpha = 2$ (so that the probability declines as a quadratic function as income rises until z is reached) instead of the linear probability function ($\alpha = 1$). In the second, we set z at the population-weighted 20th percentile, which we denote as $q(0.2)$.²⁰ The difference here is that we fix the proportion of the population who define the reference group for calculating the floor, rather than fixing this group according to the official poverty line, whereby the size of the reference group changes.²¹

Benefit incidence: We make the standard assumption in benefit-incidence analysis of estimating the post-transfer distribution by adding transfers received at the household level. The official poverty measures for the US do not reflect the impact of SNAP because such income sources are omitted from the income aggregates used for the official measures.²² To obtain our “post-SNAP” income distribution, we add in the face value of the food stamps or in later years, the credit value on the EBT card. Thus, we can estimate the impact of SNAP receipts on the floor by calculating the *SPG* and *PG* measures before and after those receipts. Note that this is not just for SNAP recipients. We are automatically factoring in incomplete coverage of the poorest.

While it is standard practice in the literature on benefit incidence to assume that the nominal cash transfers reflect the real income gains, we acknowledge that this practice ignores behavioral responses such as through savings, labor supply or private transfers. It might be argued that strong behavioral responses are unlikely among the poorest, who presumably have a

¹⁹ The thresholds can be found [here](#).

²⁰ Note that $y = q(p)$ is the quantile function, obtained by inverting the cumulative distribution function. So the median is $q(0.5)$.

²¹ For computational convenience, the quantile for $p=0.2$ is fixed prior to adding SNAP receipts.

²² The literature has pointed to a number of limitations of the official poverty measures, related to both the income concept and the poverty lines; for further discussion see Citro and Michael (1995), Blank (2008) and Meyer and Sullivan (2012). The Census Bureau has introduced a “supplemental” measure that addresses some of these concerns (Short, 2011). Here we only address the problem related to the exclusion of SNAP from the official income aggregates based on the CPS.

strong incentive to maintain their (low) income. However, behavioral responses cannot be ruled out even for the poorest. For example, there is evidence of responses to SNAP through labor supply among single female-headed households (Hoynes and Schanzenbach, 2012).²³

In response to this concern, we provide a partial test of our assumption that the post-SNAP distribution is obtained by adding SNAP receipts to income at the household level. As usual, the test requires identifying assumptions. To explain the test, let τ_{min} denote the value of the food stamps received by the poorest, while y_{min}^{*pre} and y_{min}^{*post} are the true pre-SNAP and post-SNAP levels of the floor respectively. By definition:

$$y_{min}^{*post} = y_{min}^{*pre} + \tau_{min} - b \quad (3)$$

where $b \geq 0$ denotes the behavioral response. Let \hat{y}_{min}^{*pre} denote the estimate of y_{min}^{*pre} that is obtained by assuming that $b = 0$, i.e., $\hat{y}_{min}^{*pre} \equiv y_{min}^{*post} - \tau_{min}$. If there are behavioral responses by the poorest ($b > 0$) then clearly \hat{y}_{min}^{*pre} will underestimate the true value, y_{min}^{*pre} , i.e., $b = y_{min}^{*pre} - \hat{y}_{min}^{*pre}$ is the error due to ignoring behavioral responses. Our test assumes that: (i) y_{min}^{*pre} is a continuous function of a vector of observed covariates x , namely $\vartheta(x)$ (the function can include an innovation error term); (ii) x does not include mean SNAP spending per capita τ , i.e., the true level of the floor in the absence of SNAP does not depend on aggregate SNAP spending; and (iii) the behavioral effect b is a function (presumably non-decreasing) of τ . Thus:

$$\hat{y}_{min}^{*pre} = \vartheta(x) - b(\tau) \quad (4)$$

The test is then to see if there is a partial correlation between the estimated pre-transfer floor and mean spending at a given x . This can be thought of as a consistency check on our empirical analysis. Note that a spurious correlation could arise if SNAP spending is correlated with excluded determinants of the pre-transfer floor, so the test depends crucially on the inclusion of suitable controls for the pre-SNAP floor.

We also measure what we term Floor Transfer Efficiency (*FTE*), defined as the increment to the floor due to SNAP as a share of mean spending on SNAP for the population, i.e.,

$$FTE = (\hat{y}_{min}^{*post} - \hat{y}_{min}^{*pre})/\tau \quad (5)$$

²³ Using quasi-experimental methods and exploiting cross-county variation, Hoynes and Schanzenbach (2012) find evidence of a reduction in labor supply by single female-headed households due to SNAP, though no significant effect is evident for their sample as a whole.

As a benchmark, under a budget-neutral UBI, $FTE = 1$.

Data: To implement our measure of the floor and various tests, we use the micro data from the Annual Social and Economic (ASEC) Supplement to the Current Population Survey (CPS).²⁴ We use 29 years of CPS-ASEC data from 1989 to 2017, which allows us to estimate the floor and SNAP benefit levels from 1988 to 2016. The CPS is administered by the Census Bureau for the Bureau of Labor Statistics and collects data from a nationally representative sample of households on employment, unemployment, earnings, occupation, and hours of work.

The CPS is the data source for the official poverty estimates in the US and therefore particularly well suited to our task. However, there are two caveats to note in our use of these data. First, the CPS sample is primarily people living in single-family houses, apartments, and non-institutional “group quarters,” but it excludes any homeless people who are not living in shelters (U.S. Census Bureau, 2017a). The homeless are likely to be living at levels somewhere around our estimated floor (Burt et al., 1999). Their under-representation in the sample probably biases our estimated floor upwards.

A second concern is item non-response for the SNAP participation and benefits questions. The documented under-reporting of SNAP benefits in the CPS data suggests that our estimate of the effectiveness of SNAP in lifting the floor is biased downwards, though there are differing assessments of the level of under-reporting (compare, for example, Meyer et al., 2015, 2019, with Kang and Moffitt, 2019, who find a reasonably good match between survey responses and administrative records on SNAP). Given the paucity of data to address these concerns, we do not attempt to estimate their effect on our estimates, but simply note these caveats.

The measure of family income we use is the same as that for the US official poverty estimates. This includes money income before taxes from several sources (such as wages, salary, net-income from self-employment, social security payments, pensions, interest, dividends, alimony, other forms of periodic monetary income), but excludes capital gains and non-cash benefits such as fringe benefits or noncash social assistance programs.

The proportion of the population (summing CPS individual expansion factors) with non-positive incomes recorded in the CPS has been on a rising trend (Figure 1). Note also that a population share for non-positives of around 2% in recent years represents about 15% of the poor, as judged by the official thresholds (Figure 1). Most of the non-positives are zeros, and we

²⁴ Further information on the CPS can be found in U.S. Census Bureau (2017b).

have coded any negatives as zeros.²⁵ We expect that most of these non-positive observations reflect real hardships, but not all. Mechanically, the rising share of zeros puts downward pressure on the estimated floor.

The CPS public-release measure of income imposes top-codes on income components, and the methodology for top-coding changed over our time period in a manner that affects a small number of observations in our analytical sample. Because top-coding is done by income components and not total income, and because it has not (typically) been based on winsorizing but rather using a hot-deck method, it is possible for poor families to have public-release income estimates that are greater than the poverty threshold. We implement a correction using other information on these households available in the CPS public-release data.²⁶

4. The estimated floor and the impact of SNAP

Table 1 provides summary statistics and our estimates of the floor over time. We begin by focusing on SNAP spending. Figure 2a plots spending per capita of the US population over time while Figure 2b gives spending per recipient and the participation rate.²⁷ We see that spending per capita fluctuated over time, due mainly to participation rates, which closely tracked spending (comparing Figures 2a and 2b). There was a marked increase in spending per capita in the 2000-11 period; spending doubled between 2006 and 2012, but has tended to fall in recent years. Spending per recipient grew over time, with a growth rate of 1.34% per annum (s.e.=0.14%). Mean spending per recipient family rose from \$2.89 per person per day in 1988 to \$4.15 in 2016.

²⁵ This only affects a small share of the data; on average, only 0.09% of the population (and 0.09% of the sample) have negative incomes. This average ranges from 0.01% to 0.16% of the population (and 0.01% to 0.18% of the sample) over the 1989 to 2017 CPS data files.

²⁶ We refer to the variable *povll* in the CPS public-release data, which creates a series of bins based on the ratio of family income (as measured by the confidential data) to the family's poverty threshold. For example, in the 2008 CPS data there are nine sample observations (from a total of more than 206,000) where income as reported in the public-release files, is greater than the poverty threshold, but these are poor families as identified by *povll* (which is based on the confidential income data). In the 2008 CPS files, all nine of these observations had income that was less than the poverty line, but greater than 75 percent of this line. For these nine observations, we re-assign their income to be the mid-point of their bin. This re-assignment occurs in nine of our years. The number of re-assignments made in any given year is less than 24, with a total count of 90 re-assignments overall years from the more than four million total observations we examine.

²⁷ The CPS gives the value of food stamps received last year, and the number of months covered for each family. We have divided by the number of months, and then normalized by the number of people in the family covered by food stamps and the CPI. The "per recipient" series is just the "per capita" series averaged over all positives.

As noted in the introduction, the literature has often pointed to the existence of exclusion errors for SNAP, meaning that not all those with reported income below the poverty threshold are receiving SNAP benefits. Figure 3 provides our calculations of the proportion of those who are found in various intervals of the threshold who are receiving SNAP according to the CPS. (As elsewhere, the calculations are population weighted.) Three points are notable. First, SNAP coverage tends to be higher for poorer families. Second, coverage of the poor declined sharply from the mid-1990s, and did not start to recover until 10 years later. Third, a large share of the poor (40-60% depending on the time period) are not receiving the program.

Recall that consistency with our assumption of no behavioral responses for the poorest implies that the estimated pre-transfer floor (\hat{y}_{min}^{*pre}) should be uncorrelated with the level of mean SNAP transfers (τ) once one controls for the determinants of the true pre-transfer floor. In testing for this, we allow for two features of our estimated time series in Table 1, namely that the floor has a negative trend and that it is serially correlated. On including a time trend and the lagged dependent variable to accommodate these features of the data, we see in Table 2 that the test passes comfortably for various controls (the remainder of the vector x in equation 4). The lack of a partial correlation with SNAP spending evident in Table 2 is consistent with our assumption that those responses are negligible at the floor, as required by our method of estimating the post-transfer floor. (This also holds using the positives only to measure the floor.)

We find that the program raised the floor (as a proportion of the poverty thresholds) by about 3% points on average. For the benchmark case, the post-SNAP floor ratio for 2015 is almost exactly 30% of the official threshold, which was \$24,036 a year in that year for a family of four (two adults and two children) or \$16.46 per person per day or \$15.15 in 2010 prices. So the post-SNAP floor in 2015 was \$4.93, while the pre-transfer value was \$4.55. The quadratic probability function ($\alpha = 2$) naturally gives a lower estimate of the floor (since it attaches higher relative weight to lower observed incomes). For 2015, the post-SNAP floor was \$2.97, while it was \$2.71 pre-SNAP.

The floor for the US is about the level of the highest floors in developing countries when converted at Purchasing Power Parity and evaluated assuming a linear probability function.²⁸

²⁸ Based on the estimates by Margitic and Ravallion (2019) of the post-transfer floor in developing countries, the mean of the 10 highest values is \$5.60 a day.

Changes over time in the floor and confidence intervals: Figure 4 plots the floor ratio before and after food stamps using the official threshold (panels (a) and (b)) and our estimates using the poorest 20% as the reference group (panel (c)).²⁹ To reduce cluttering in the graphs, we only give the 95% confidence intervals (CI's) in panel (b) for the benchmark case ($\alpha = 1$ and the official thresholds), and for this case we re-scale the graph to enhance visibility. The CI's are based on variance estimates that correct for non-independence using a sandwich estimator (Wolter, 2007).³⁰ (The statistical addendum provides standard errors for other specifications.) While the floor is lower for $\alpha = 2$, the trajectories are similar, and $r = 0.998$ for the official threshold and 0.997 for the poorest 20%. We continue to focus on $\alpha = 1$ as the benchmark.

For all years except 2000, we found no overlap between the post-SNAP CI and pre-SNAP CI implying that the gain in the floor due to SNAP is statistically significant; the overlap for 2000 is very small (0.001). While food stamps lifted the floor, there has been a trend decline over the period as a whole in both the pre-transfer and the post-transfer floors. In the benchmark case, the growth rate of the pre-SNAP floor over the period is -1.28% per annum (s.e.=0.06), while it is -1.32% (s.e.=0.11) for the post-SNAP floor. Using the poorest 20% instead, the annual rates of decline are slightly lower but still significantly negative (-0.83% and -0.81% respectively, with s.e. = 0.04% and 0.03%).

The decline in the floor is statistically significant over the period as a whole in that the 95% CI's do not overlap either pre- or post-SNAP. For 1988, the 95% CI for the pre-SNAP floor ratio is (0.368, 0.386), while it is (0.261, 0.276) for 2016; for the post-SNAP floor, the CI's are (0.409, 0.430) and (0.284, 0.300) for 1988 and 2016 respectively.

Of course, overlaps in CI's are found within sub-periods (though that does not preclude significant differences). The upper panel of Figure 4(b) gives the number of years before CI's cease to overlap. For example, for the pre-SNAP floor, it is nine years from the beginning of the series in 1988 before the CI's cease to overlap, though for the post-SNAP floor it is six years. By the mid-1990s, after which the floor falls quickly, the CI's cease to overlap within just a few years. This changes in 2001 after which declines continue but CI's overlap substantially for longer periods. (For example, the 2001 post-SNAP CI overlaps for all years up to 2014.)

²⁹ As an indication of the \$ value in 2010 prices, one can simply multiply by 15 (recalling that the poverty line for a family of four is \$15.15 per person per day).

³⁰ Standard errors are estimated by passing synthetic strata and clusters as described in Jolliffe (2003) to Stata's "svy: ratio estimator," which can account for covariance of y_i and $\phi_\alpha(y_i)$.

Significant declines in both the pre-SNAP and post-SNAP floor then re-emerge over the last four years of the series.

About half (53% using the official threshold) of the overall decline in the level of the floor was in the period 1996-2001, coinciding with the aforementioned policy reforms. However, the sharp decline in the floor 1996-2001 cannot be attributed to the SNAP policy changes alone since we also see a similar decline in the pre-SNAP floor, though less than we find post-SNAP. There are signs that the post-SNAP floor stabilized from the early 2000s.

There is no significant trend in the post-SNAP floor from around 2003, though the fall in the pre-SNAP floor continued, albeit at a slower pace (Figure 4). The pre-SNAP floor fell during the crisis years, 2008-11, but the post-SNAP floor did not. The difference-in-difference (DD) is a little less than 1% of the poverty line, i.e., if the post-SNAP floor had maintained the same downward trajectory as the pre-SNAP floor during 2008-11 then the former would have been about 1% of the poverty line lower.³¹ The last few years of the series have seen a resumption in the floor's decline (Figure 4).

Recall that the proportion of non-positive incomes recorded in the CPS has been rising over time (Figure 1). As noted, the non-zeros are presumably transient effects, including some households who are not normally poor. There is no sound basis for excluding the non-positives, but it is of interest to see if the downward trend in the floor is robust to doing so. Figure 5 shows the pre-SNAP floor over time excluding the non-positives, for both linear and quadratic probability weights.³² At the end of the period, the pre-SNAP floor ratio rises from 0.27 to 0.32 for $\alpha = 1$ and from 0.16 to 0.19 for $\alpha = 2$. The downward trend is less pronounced, but still evident if one drops the zeros; on only using the positive incomes, the growth rate (for $\alpha = 1$) is -0.80% per annum (s.e.=0.04) for the pre-SNAP floor and -0.85 (0.09) for the post-SNAP floor. Our finding that the floor has been falling cannot be attributed to a rising share of non-positive incomes in the CPS.

Comparisons with other distributional statistics: Standard poverty measures are not highly correlated with the changes in the floor over time. As noted in the introduction, the poverty rate has remained roughly stationary over the period as a whole, though rising in some periods (notably in the early 1990s and during the financial crisis starting in 2008) and falling in

³¹ Using 2008 as the base year and 2011 as the final year, DD=0.0085; using 2012 instead one obtains DD=0.0090.

³² This is the prior floor estimate deflated by 1 minus the proportion of poor households with non-positive incomes.

others (the rest of the 1990s and in recent years) (Table 1). Figure 6 gives three standard measures. The periods 1993-2000 and 2012-16 saw declining poverty measures but a sinking floor (Figure 4). And the sharp rise in poverty measures in the crisis period (2008-10) came with a relatively stable floor post-SNAP, as noted. The proportionate changes over time in the floor are roughly orthogonal to those for the (post-SNAP) headcount index ($r = 0.041$) and not highly correlated for the two poverty gap indices ($r = -0.268$ for changes in *PG* and $r = -0.492$ for *SPG*). Tracking standard poverty measures alone is clearly not very informative about what is happening to the floor.

The real growth rate in mean income is 0.88% per annum (s.e.=0.16), but that is appreciable higher than for the median which had an annual growth rate of 0.53% (s.e.=0.12).³³ $q(0.2)$ shows essentially zero growth while the median has a small positive growth rate. The median increased with the mean, but the ratio of the median to the mean has been falling over time, from around 0.78 to 0.71 (Table 1). In other words, the median has tended to rise with the mean, but with an elasticity less than unity.³⁴ This decline in the median relative to the mean as the latter increases is another aspect of the inequitable growth process of the US. The Gini index of inequality in family income rose from 0.40 to 0.48 over this period (Table 1).

There is an evident disconnect between the evolution of the floor and the upper quantiles of the US income distribution. Figure 7 plots the overall mean and various quantiles—the median ($q(0.5)$) and top income quantiles, $q(0.9)$, $q(0.95)$ and $q(0.99)$. While the floor has been on a downward trend, the incomes of the top quantiles show a strong positive trend, though with fluctuations. The growth rate of $q(0.99)$ is 1.63% per annum (s.e.=0.32). For a family of four people, the floor fell from about 9.2% of $q(0.99)$ in 1988 to 3.9% in 2016.

So we see a marked divergence between the top incomes and the mean, median and (especially) the floor. Note also that these are quantiles based on the CPS. There is a likely bias in the CPS at the high end, associated with under-reporting and selective compliance (Korinek et

³³ Growth rates are estimated here by regressing the log of that variable on time.

All standard errors are robust to residual autocorrelation and heteroscedasticity, using the method in Newey and West (1987). We used the automatic lag specification with the degrees of freedom adjustment.

³⁴ The OLS regression coefficient of the log median on the log mean is 0.611 with a standard error of 0.037. One can reject the null that the coefficient is unity with $t = 10.64$.

al., 2006). The bias is unlikely to be confined to the top 1%, or even 10%, so the quantiles will be affected, implying that correcting for the bias would show even greater divergence.³⁵

The effect of SNAP on the floor: When the government spends more on SNAP, the floor tends to rise. This can be seen in Table 3, which gives regressions of the log of the gain in the floor ($\ln(\hat{y}_{min}^{*post} - \hat{y}_{min}^{*pre})$) on log spending per capita, again allowing for both serial dependence in the floor and a time trend. The elasticity of the floor to SNAP spending is around 0.5.

To explore this further, note that mean spending on SNAP is the product of the program participation rate and spending per recipient. By relaxing the assumption that the logs in these two variables have the same effect on the floor, we can assess which has been more important to the extent to which SNAP has raised the floor. Figure 8 plots the gain in the floor over time and both the SNAP participation rate and SNAP spending per recipient; all variables are in logs and the graph gives the deviations from mean. We see a much stronger co-movement between the extent of the gain in the floor and the participation rate (especially so from the early 1990s) than we see for spending per recipient. In the levels, the correlation coefficient between $\ln(\hat{y}_{min}^{*post} - \hat{y}_{min}^{*pre})$ and the log participation rate is 0.605, while it is -0.463 for the log spending per recipient. In the latter case, the (negatively-correlated) trends account for the correlation; the correlations between the first differences over time are 0.637 and 0.134 respectively. It is SNAP's participation rate that matters to its impact on the floor rather than its spending per recipient.

Table 3 also provides regressions of the log of the gain in the floor due to SNAP on the log of the participation rate and the log of spending per recipient, controlling for the dynamics of the floor impact and its time trend. It is also clear from these partial effects that the more important factor in explaining the variation over time has been the participation rate. Indeed, spending more per recipient has done very little toward raising the floor at a given SNAP participation rate. Separately to this, we find a downward trend over time in the contribution of SNAP to lifting the floor at given levels of spending.

Consistently with the last finding, our estimates reveal that there has been a decline in FTE—the ratio of the gain in the floor that we attribute to SNAP to spending per capita on the program (Figure 9). The mean gain in the floor exceeds mean spending on food stamps; the

³⁵ See, for example, the estimates combining CPS with income tax and national accounts data in Piketty et al. (2018) and [Worldwide Inequality Database](#). Also see the state-level estimates by Sommeiller and Price (2018). Of course, such adjustments to survey data require a great many assumptions.

difference between the two is \$0.12 a day (in 2010 prices) and this is significantly different from zero (s.e.=0.04). The program is reaching the poorest more than a UBI with the same budget. However, the program’s efficiency in raising the floor has declined appreciably over time. In the late 1980s, the gain in the floor was about 2.5 times mean spending, but by the last five years of the series it had fallen to about the same level, i.e., an FTE of unity.

Thus, in recent years, SNAP is not having any larger impact on the floor than one would expect from a budget-neutral UBI. In the past, SNAP was much more effective in reaching the poorest, although the reform period in the mid-1990s came with a slower rate of decline in FTE (Figure 9). Note also that the decline in FTE largely preceded the expansion in SNAP under ARRA, but the “SNAP stimulus” did not come with better performance in reaching the poorest.

Exclusions errors and the poorest: The calculations so far have not been conditional on SNAP participation; for example, Figure 4 gives means over the relevant population whether receiving SNAP or not. Our results indicate that an important reason why SNAP has not raised the floor more is the extent of the exclusion errors among the poorest. As a quantitative indication of just how much this matters, Figure 10 plots the floor that is implied if all the poorest received the mean SNAP spending per SNAP recipient (so simply adding the date-specific mean SNAP per recipient to the pre-SNAP floor).³⁶ We call this “full coverage.” The floor has been calculated for a family of four, with two adults (giving the aforementioned poverty line of \$15.15 per person per day in 2010 prices).

Two things are notable from Figure 10: First, with full coverage the floor would have risen to about \$8 per day, and (second) the floor would have been roughly stable over the period—almost fully negating the decline in the pre-SNAP floor that we find. The increment to the post-SNAP floor associated with full coverage rises from \$1.63 in 1988 to \$3.26 in 2016.

The size of these gains to the floor from full coverage reflect a high elasticity of \hat{y}_{min}^{*post} — \hat{y}_{min}^{*pre} to SNAP coverage of those living below (say) half the threshold. The regression coefficient of $\ln(\hat{y}_{min}^{*post} - \hat{y}_{min}^{*pre})$ on the log of the share of SNAP recipients among those living at less than $0.5z$ is 1.57 (s.e.=0.13). As one would expect, we also find that the extent to which SNAP lifts the floor is far more responsive to coverage of those living below $0.5z$ than it is to coverage among other eligible groups. In particular, if we add the (logs) of the three other coverage rates in Figure 3 (while controlling for the coverage rate of those living below $0.5z$), we

³⁶ In making this calculation we use the number of months of participation recorded by each participant.

find that their coefficients are small and statistically insignificant, while the elasticity w.r.t. the coverage rate for those under $0.5z$ remains high (1.94; s.e.=0.20).

It should be noted, however, that expanding coverage to all of those living at or near the floor would have required either a higher aggregate disbursement on the program, or a change in the allocation away from those living above the floor, many of whom are still poor. Lifting the floor this way will almost certainly come with trade-offs.

5. Conclusions

The rise in “top incomes” in the US has received much recent attention. We have focused here on what has been happening at the other tail of the distribution, and how effective the country’s largest antipoverty program, SNAP (popularly known as “food stamps”), has been in reaching the poorest. We do not argue that policymakers should only focus on the poorest, but we contend that efficacy in reaching them is an important, but neglected, metric of progress.

We have estimated the lower bound to the distribution of income—the “floor”—as a weighted mean of observed survey-based incomes below the poverty line, with higher weight on lower observed incomes. In estimating the income gains from SNAP we have ignored any behavioural responses by the poorest, though we have provided a test for such responses that is consistent with our assumption that such responses are negligible. We acknowledge that our measures might be improved with better data (notably on the living standards of the poorest), but our main qualitative results have stood up well to various tests of our assumptions.

We find nothing to suggest that the rise in top incomes has lifted the floor. Indeed, our results indicate that the floor was higher 30 years ago than today. The floor fell markedly in the 1990s, then it began to stabilize in the 2000s, but it appears now to be back on its prior downward trajectory. In 2016, the floor was about 30% of the official poverty line, or about \$5 per person per day. The decline in the floor is accountable in part to a rising share of non-positive incomes recorded in the data, but a downward trend is still evident if one only uses the positives. The rising gap between top incomes and the floor is another instance of the pattern of rising absolute inequality that has been observed globally (Ravallion, 2018).

Food stamps raised the floor, with a gain to the poorest that is significantly greater than mean spending on SNAP, though substantially less than mean SNAP receipts per recipient. The financial crisis emerging in 2008 put downward pressure on the pre-SNAP floor, but the

expansion of the program in the wake of the crisis was able to prevent a fall in the floor despite the underlying inequality-increasing growth process. Thus, SNAP partly compensated for the downward pressure on the pre-transfer floor. The program helped assure that the poorest could at least maintain their (low) living standards during a period of inequitable growth. The changes over time in the program's efficacy in raising the floor are accountable to changes in the overall participation rate rather than spending per recipient.

However, we find a marked longer-term decline in the extent to which SNAP has raised the floor, associated with declining efficacy of food stamps in reaching America's poorest since the mid-1990s. This could have been almost entirely avoided if the poorest had fully participated in SNAP, even at the mean transfer receipts per participant. The extent to which SNAP lifts the floor responds elastically to the program's coverage rate among those living well below the official poverty lines. Our calculations indicate that if all of those living at or near the floor had received even the mean SNAP payment per person the post-SNAP floor would have risen to about \$8 per person per day, and stayed fairly stable over the period 1988-2016, in contrast to the sinking floor that we actually observe.

References

- Anderson, Patricia, Kristin Butcher, and Diane Whitmore Schanzenbach, 2015, “Changes in Safety Net Use During the Great Recession,” *American Economic Review Papers and Proceedings* 105(5): 161-165.
- Autor, David H., Frank Levy and Richard J. Murnane, 2003, “The Skill Content of Recent Technological Change: An Empirical Exploration,” *Quarterly Journal of Economics* 118(4): 1279-1333.
- Bartfeld, Judi, Craig Gundersen, Timothy M. Smeeding, and James Patrick Ziliak, eds., 2016, *SNAP Matters: How Food Stamps Affect Health and Wellbeing*. Studies in Social Inequality. Stanford, California: Stanford University Press.
- Ben-Shalom, Yonatan, Robert Moffitt and John Karl Scholz, 2012, “An Assessment of the Effectiveness of Antipoverty Programs in the United States,” in Philip Jefferson (ed.) *The Oxford Handbook of the Economics of Poverty*, Oxford: Oxford University Press.
- Blank, Rebecca M., 2008, “How to Improve Poverty Measurement in the United States,” *Journal of Policy Analysis and Management* 27(2): 233-254.
- Burt, Martha R., Laudan Y. Aron, Toby Douglas, Jesse Valente, Edgar Lee, and Britta Iwen. 1999. “Homelessness: Programs and the People They Serve: Findings of the National Survey of Homeless Assistance Providers and Clients.” Washington, DC: Urban Institute. <https://www.urban.org/research/publication/homelessness-programs-and-people-they-serve-findings-national-survey-homeless-assistance-providers-and-clients>.
- Citro, Constance, and Robert Michael, 1995, (eds.) *Measuring Poverty: A New Approach*. Washington, DC: National Academy Press.
- Committee on Ways and Means, U.S. House of Representatives. 2000. 2000 Green Book: Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means. 106th Congress, Second Session. Washington, D.C.: U.S. Government Printing Office. <https://www.govinfo.gov/content/pkg/GPO-CPRT-106WPRT61710/pdf/GPO-CPRT-106WPRT61710-1.pdf>.
- Currie, Janet. 2003. “U.S. Food and Nutrition Programs.” In *Means-Tested Transfer Programs in the United States*, edited by Moffitt, Robert. Chicago: University of Chicago Press.
- Currie, Janet, and Firouz Gahvari, 2008, “Transfers in Cash and In-Kind: Theory Meets the Data.” *Journal of Economic Literature* 46 (2): 333–83.

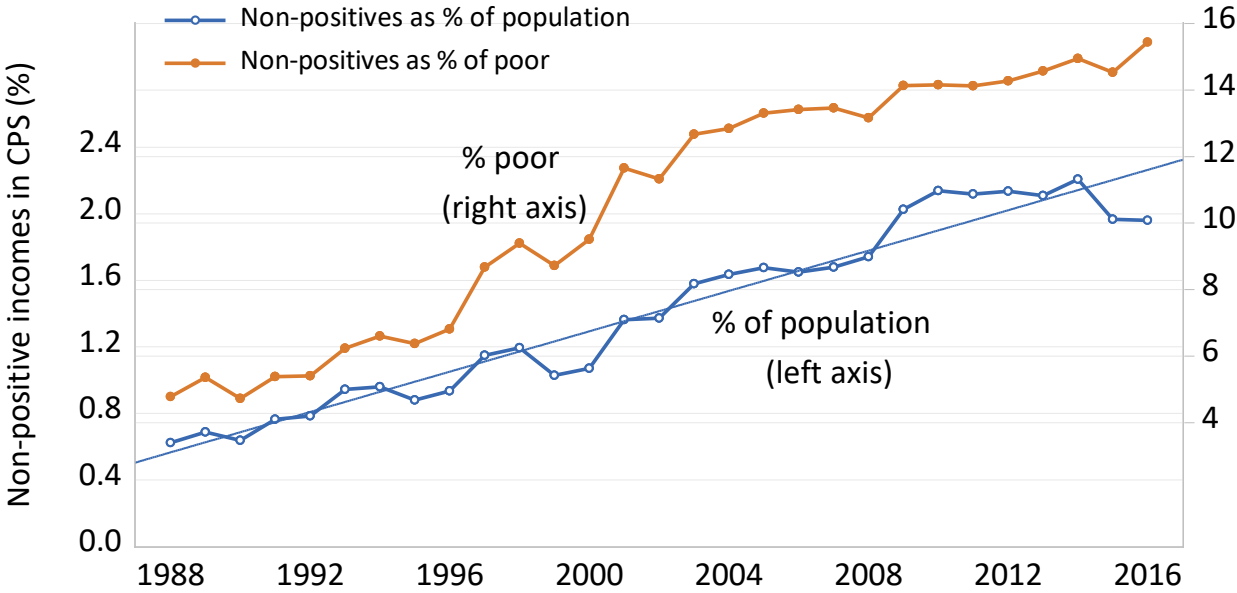
- Currie, Janet, and Jeffrey Grogger, 2001, “Explaining Recent Declines in Food Stamp Program Participation.” *Brookings-Wharton Papers on Urban Affairs*: 203–44.
- Daponte, Beth Osborne, Seth Sanders, and Lowell Taylor. 1999. “Why Do Low-Income Households Not Use Food Stamps? Evidence from an Experiment.” *Journal of Human Resources* 34 (3): 612–28. <https://doi.org/10.2307/146382>.
- Ebenstein, Avraham, Ann Harrison, Margaret McMillan and Shannon Phillips, 2014, “Estimating the Impact of Trade and Offshoring on American Workers using the Current Population Surveys,” *Review of Economics and Statistics* 96(4): 581-595.
- Economist, The, 2011, “[The Struggle to Eat](#),” *Economist Magazine*, July 14.
- _____, 2015, “[Just a Little Bit Richer](#),” *Economist Magazine*, April 4.
- Edin, Kathryn, and H. Luke Shaefer, 2016, *\$2.00 a Day: Living on Almost Nothing in America*, First Mariner.
- Falk, Gene, Alison Mitchell, Karen E. Lynch, Maggie McCarty, William R. Morton, Margot L. Crandall-Hollick. 2015. “Need-Tested Benefits: Estimated Eligibility and Benefit Receipt by Families and Individuals.” CRS report no. R44327. Washington, D.C.: Congressional Research Service. <https://crsreports.congress.gov/product/pdf/R/R44327>.
- Foster, James, J. Greer, and Erik Thorbecke, 1984, “A Class of Decomposable Poverty Measures,” *Econometrica* 52: 761-765.
- Ganong, Peter, and Jeffrey B. Liebman, 2018, “The Decline, Rebound, and Further Rise in SNAP Enrollment: Disentangling Business Cycle Fluctuations and Policy Changes,” *American Economic Journal: Economic Policy* 10 (4): 153-76.
- Goos, Maarten, Alan Manning, and Anna Salomons, 2014, “Explaining Job Polarization: Routine-Biased Technological Change and Offshoring,” *American Economic Review* 104(8): 2509-26.
- Gundersen, Craig, and Victor Oliveira, 2001, “The Food Stamp Program and Food Insufficiency,” *American Journal of Agricultural Economics* 83(4): 875- 887.
- Gundersen, Craig, and James Ziliak, 2018, “Food Insecurity Research in the United States: Where We Have Been and Where We Need to Go.” *Applied Economic Perspectives and Policy* 40 (1): 119–35.
- Haider, Steven J., Alison Jacknowitz, and Robert F. Schoeni. 2003. “Food Stamps and the Elderly: Why Is Participation so Low?” *Journal of Human Resources* 38: 1080.

- Hardy, Bradley, and James Ziliak, 2014, "Decomposing Trends in Income Volatility: The 'Wild Ride' at the Top and Bottom," *Economic Inquiry* 52(1): 459-476.
- Hoynes, Hilary, 2012, "Comment (on Meyer and Sullivan, 2012)," *Brookings Papers on Economic Activity*, Fall: 133-183.
- Hoynes, Hilary, and Diane Whitmore Schanzenbach, 2012. "Work incentives and the Food Stamp Program," *Journal of Public Economics* 96(1): 151-162.
- _____ and _____, 2016, "U.S. Food and Nutrition Programs", in *Economics of Means-Tested Transfer Programs in the U.S. Volume I*, edited by Robert Moffitt, University of Chicago Press.
- Jolliffe, Dean, 2003, "Estimating Sampling Variance from the Current Population Survey: A Synthetic Design Approach to Correcting Standard Errors." *Journal of Economic and Social Measurement* 28(4): 239–61.
- Jolliffe, Dean, Craig Gundersen, Laura Tiehen, and Joshua Winicki, 2005, "Food Stamp Benefits and Child Poverty," *American Journal of Agricultural Economics* 87(3): 569-581.
- Kakwani, Nanak, 1993, "Statistical Inference in the Measurement of Poverty," *Review of Economics and Statistics* 75(4): 632-639.
- Kang, Kyung Min and Robert Moffitt, 2019, "The Effect of SNAP and School Food Programs on Food Security, Diet Quality, and Food Spending: Sensitivity to Program Reporting Error," *Southern Economic Journal*, in press.
- Korinek, Anton, Johan Mistiaen and Martin Ravallion, 2006, "Survey Nonresponse and the Distribution of Income," *Journal of Economic Inequality* 4(2): 33–55.
- Margitic, Juan and Martin Ravallion, 2019, "Lifting the Floor? Economic Development, Social Protection and the Developing World's Poorest," *Journal of Development Economics*, in press.
- Meyer, Bruce, and James Sullivan, 2012, "Winning the War: Poverty from the Great Society to the Great Recession," *Brookings Papers on Economic Activity*, Fall: 133-183.
- Meyer, Bruce, Wallace K. C. Mok, and James X. Sullivan. 2015. "Household Surveys in Crisis." *Journal of Economic Perspectives* 29 (4): 199–226.
- Meyer, Bruce, Derek Wu, Victoria R. Mooers and Carla Medalia, 2019, "The Use and Misuse of Income Data and Extreme Poverty in the United States," NBER Working 25907.

- Newey, Whitney and Kenneth West, 1987, "A Simple, Positive Semidefinite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix," *Econometrica* 55: 703–708.
- Piketty, Thomas, Emmanuel Saez, and Gabriel Zucman, 2018, "Distributional National Accounts: Methods and Estimates for the United States," *Quarterly Journal of Economics* 133(2): 553-610.
- Ponza, Michael, James C. Ohls, Lorenzo Moreno, Amy Zambrowski, and Rhoda Cohen. 1999. "Customer Service in the Food Stamp Program." Mathematica Policy Research Report, No. 19806d533df943278178d9cf1. Washington, D.C.: Mathematica Policy Research. <https://ideas.repec.org/p/mpr/mprres/19806d533df943278178d9cf1d42f38f.html>.
- Ravallion, Martin, 2016, "Are the World's Poorest Being Left Behind?," *Journal of Economic Growth* 21(2): 139–164.
- _____, 2018, "What Might Explain Today's Conflicting Narratives on Global Inequality?" WIDER Working Paper 2018/141.
- Short, Kathleen, 2011, "The Research Supplemental Poverty Measure: 2010." Current Population Reports no. P60-241. Washington: U.S. Bureau of the Census.
- Slesnick, Daniel, 2001, *Consumption and Social Welfare*, Cambridge: Cambridge University Press.
- Sommeiller, Estelle, and Mark Price. 2018. [*The New Gilded Age: Income Inequality in the U.S. by state, metropolitan area, and county*](#). Economic Policy Institute, July 2018.
- United States, Bureau of Labor Statistics (BLS), undated, *Current Population Survey: Handbook of Methods*. United States, Bureau of Labor Statistics
- U.S. Census Bureau. 1991, [*Income and Poverty in the United States: 1988 and 1989*](#), Current Population Reports, P60-171. Washington DC: U.S. Government Printing Office.
- _____, 2017a, [*Income and Poverty in the United States: 2016*](#), Current Population Reports, P60-259. Washington DC: U.S. Government Printing Office.
- _____, 2017b. [*Current Population Survey, 2017 Annual Social and Economic \(ASEC\) Supplement*](#), Washington, DC: U.S. Census Bureau.
- United States Department of Agriculture (USDA), 2017, "[A Short History of SNAP](#)," United States Department of Agriculture.
- Wilde, Parke, and Mark Nord, 2005, "The Effect of Food Stamps on Food Security: A Panel Data Approach," *Review of Agricultural Economics* 27(3): 425–432.

- Wolter, Kirk. 2007. *Introduction to Variance Estimation*. 2nd edition. New York: Springer.
- Zedlewski, Sheila R., and Linda Giannarelli. 2015. "TRIM: A Tool for Social Policy Analysis." Washington, DC: Urban Institute. <https://www.urban.org/research/publication/trim-tool-social-policy-analysis>.
- Ziliak, James, 2016, "Why Are So Many Americans on Food Stamps," In: Bartfeld et al. (eds.) *SNAP Matters: How Food Stamps Affect Health and Wellbeing*. Studies in Social Inequality. Stanford, California: Stanford University Press.

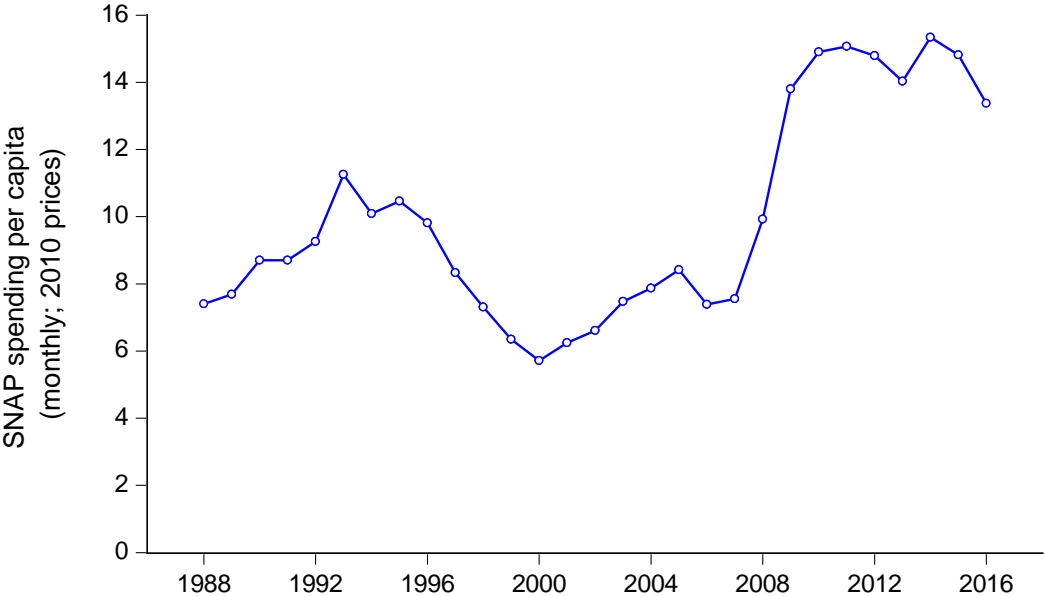
Figure 1: Non-positive values for incomes recorded in CPS



Note: “% of population” is based on the count of the US population for which poverty is estimated, i.e., sum of individual expansion factors in CPS unit record data.

Figure 2: Public spending on SNAP and participation rates

(a) Spending per capita of US population



(b) Spending per recipient and participation rates

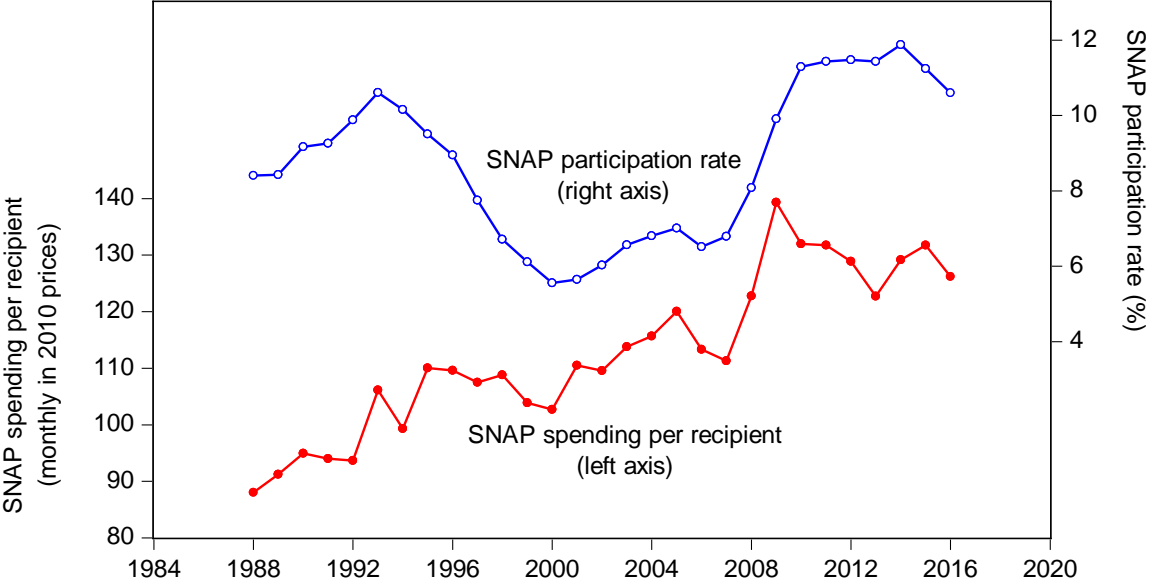


Figure 3: SNAP coverage of the poor

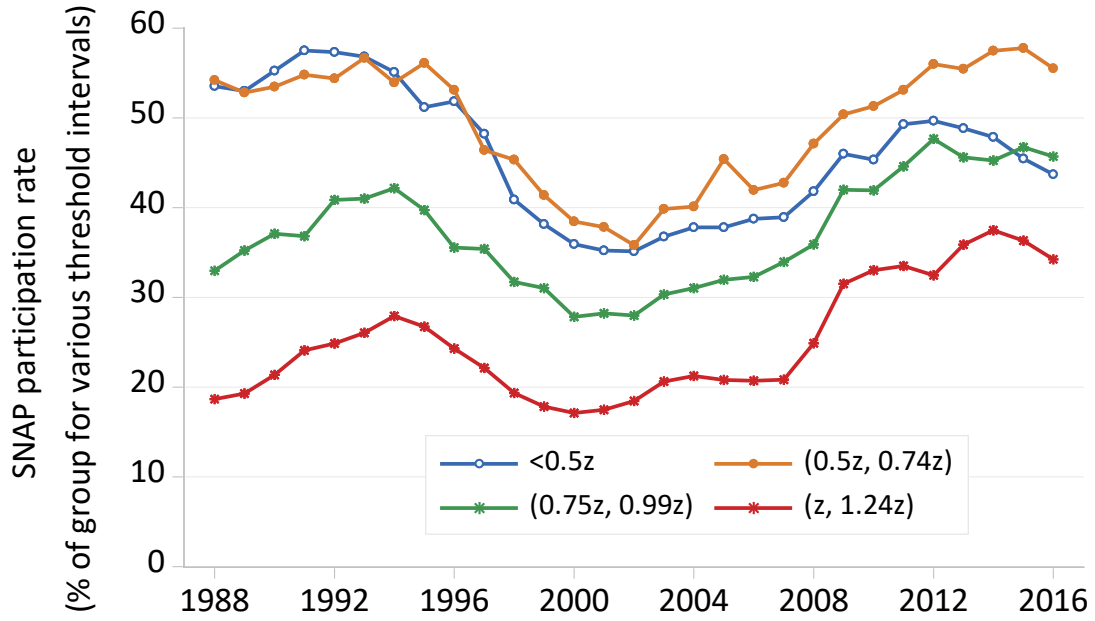
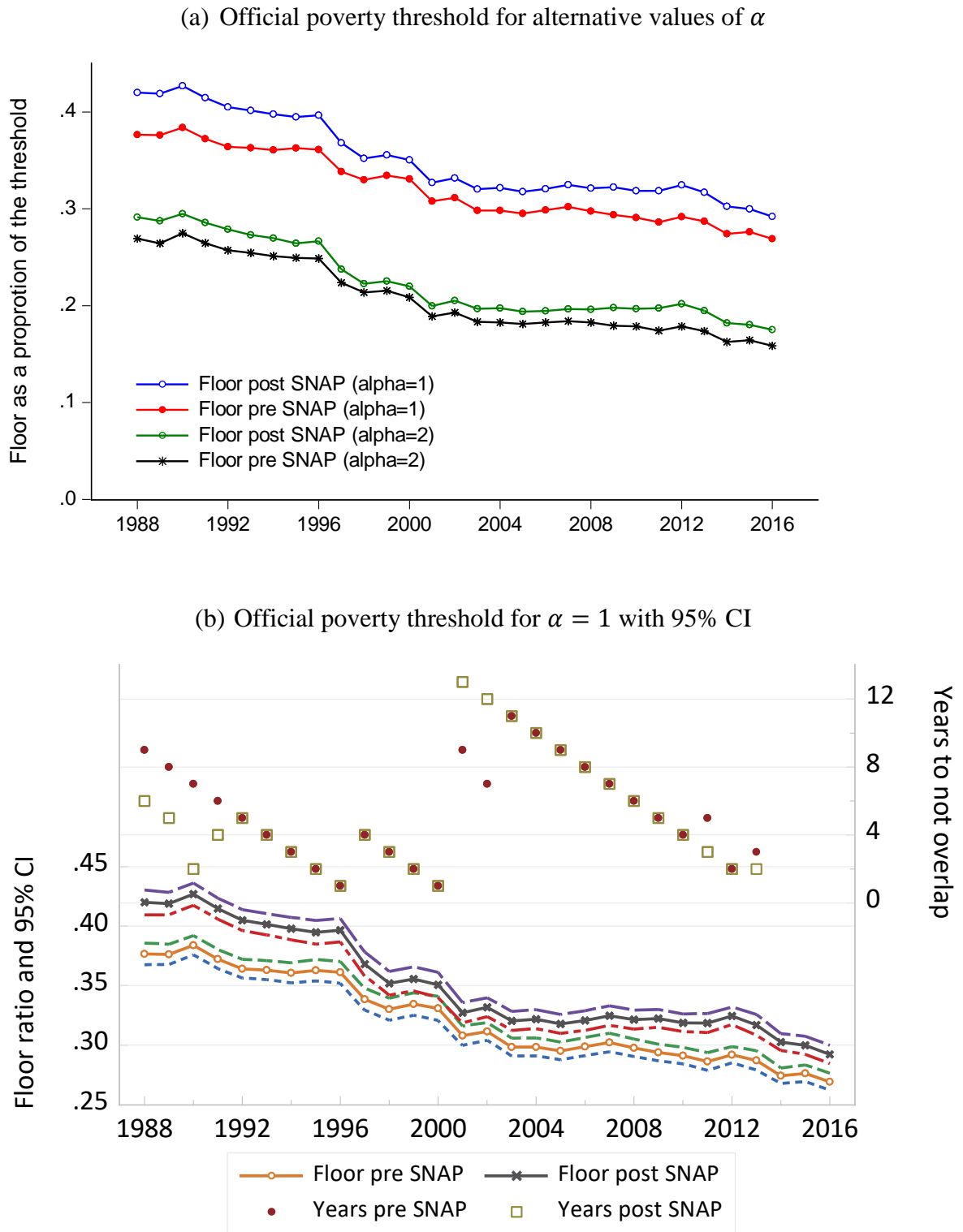


Figure 4: Floor in the United States 1988-2016 before and after food stamps



(c) Floor anchored to quantile of 20th percentile for alternative values of α

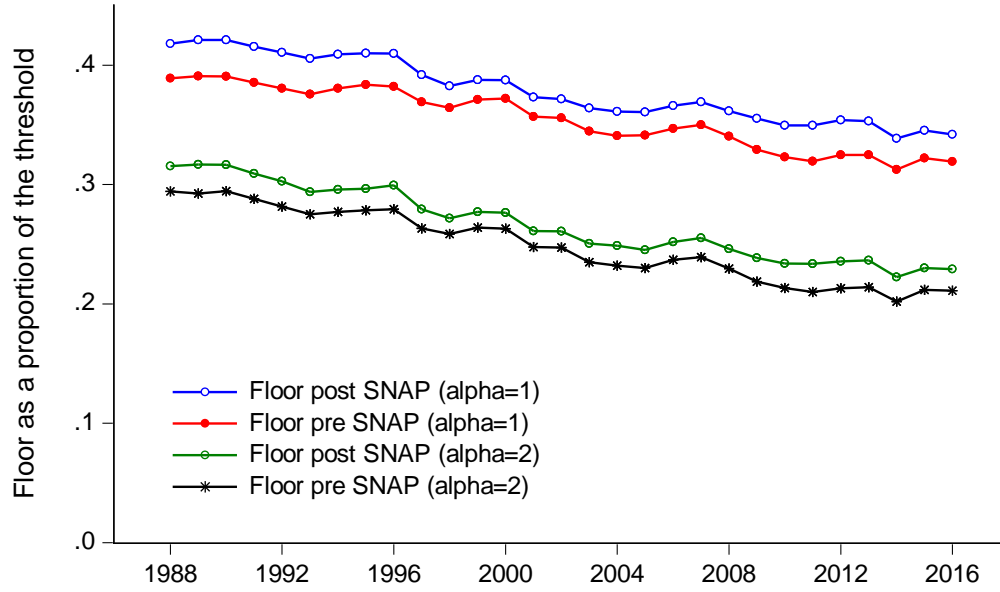


Figure 5: Effect of excluding non-positives on the floor

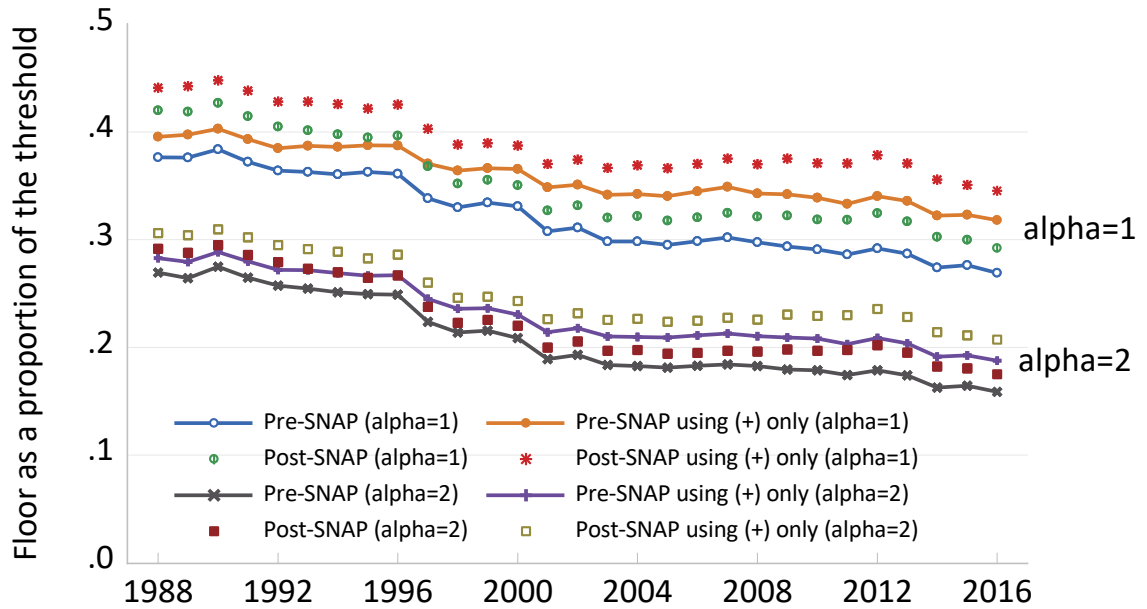
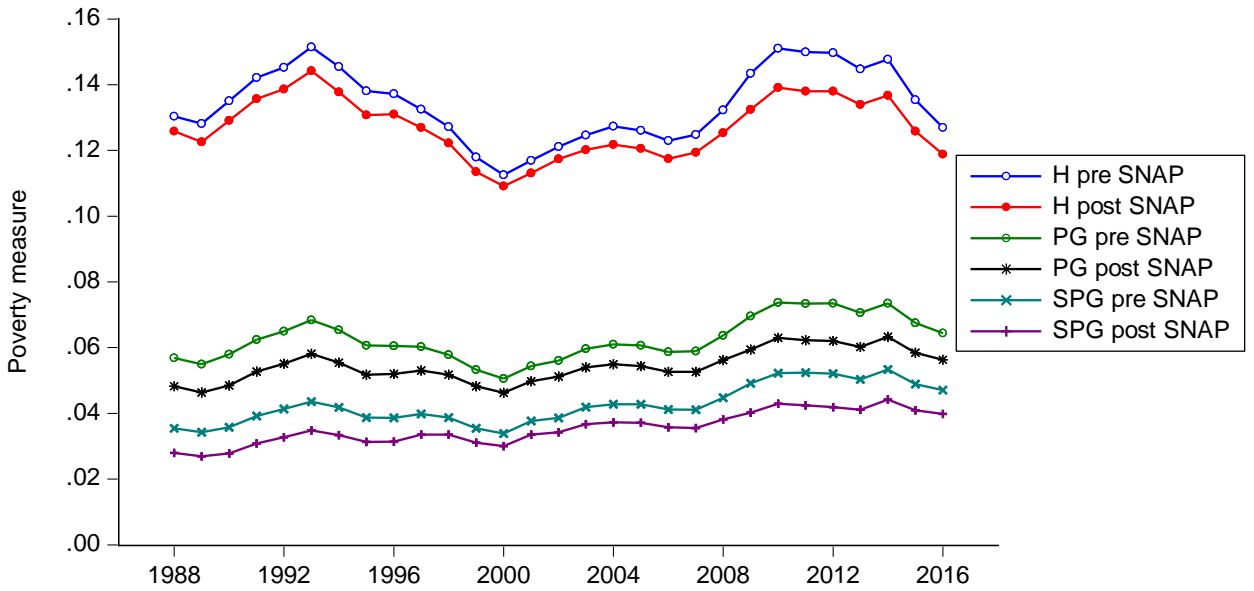


Figure 6: Poverty measures



Note: H: head count index; PG=poverty gap index; SPG=squared poverty gap index

Figure 7: Floor relative to overall mean and various quantiles

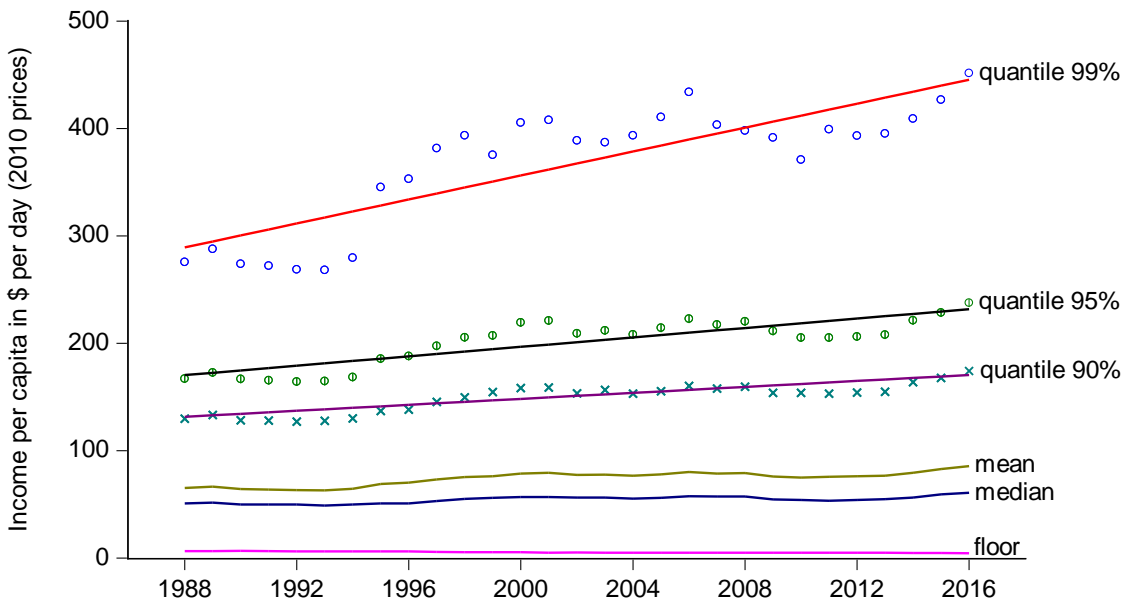


Figure 8: Gain in the floor due to SNAP compared to SNAP participation rate and spending per recipient

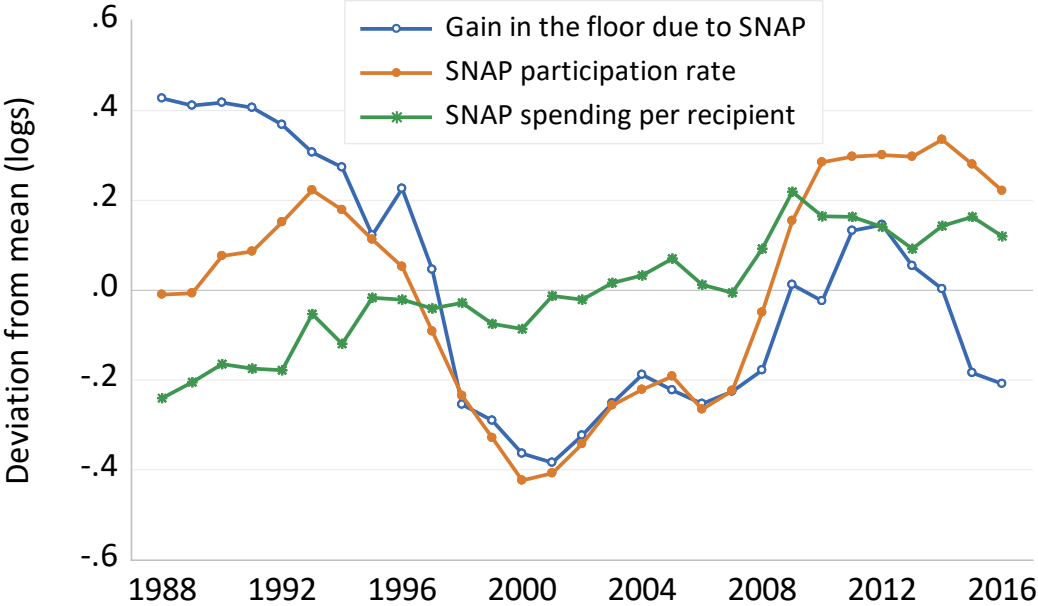
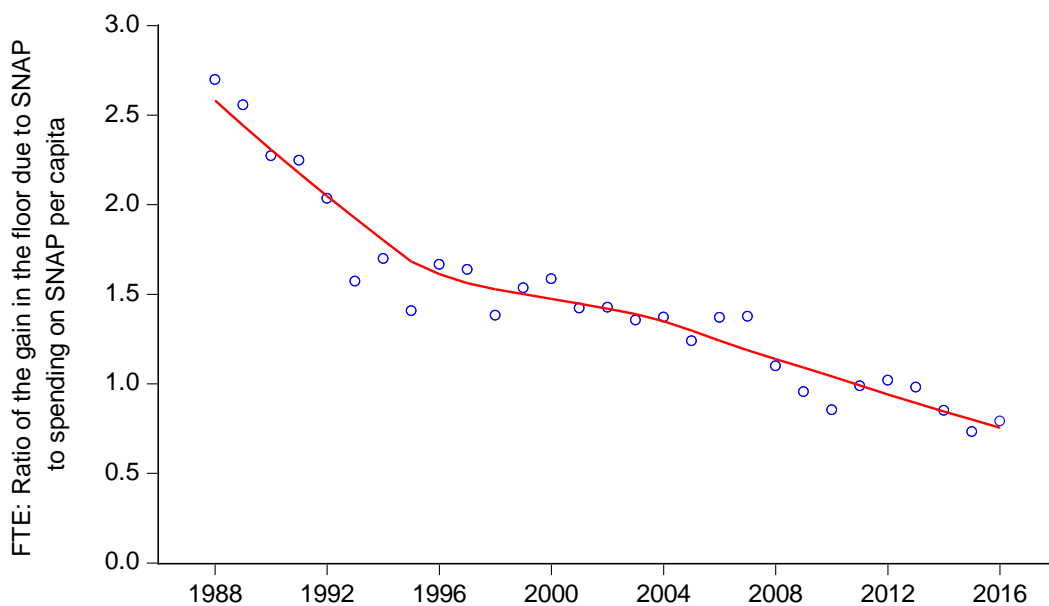


Figure 9: Floor transfer efficiency for SNAP



Note: FTE calculated for a family of four, two adults, two children, i.e., using a poverty threshold of \$15.15 a day in 2010 prices.

Figure 10: Hypothetical floor on eliminating exclusion errors among the poorest

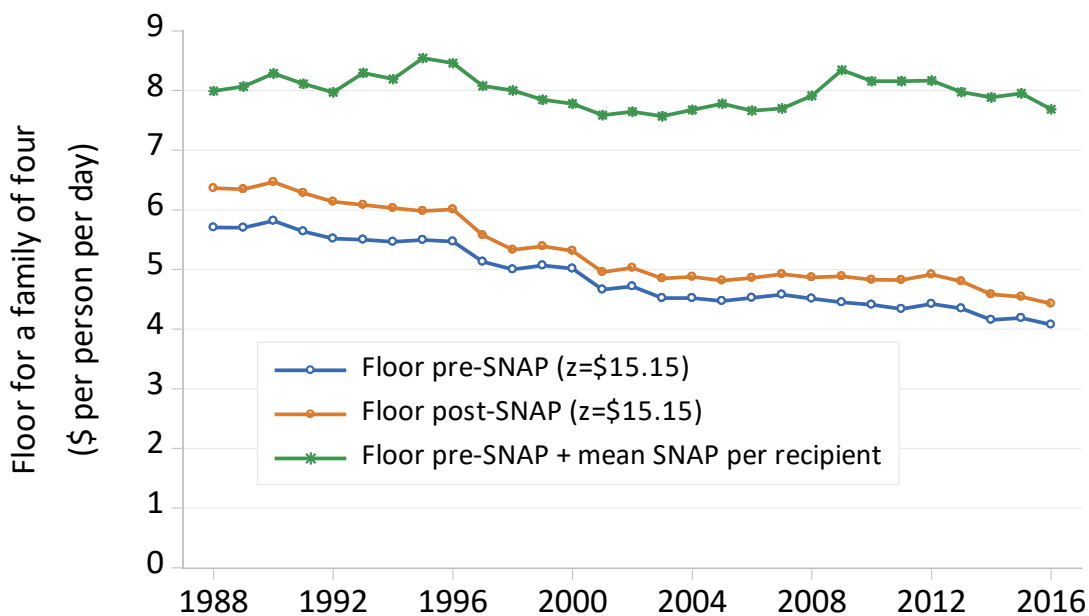


Table 1: Summary statistics and estimates of the floor ratio before and after SNAP

	Mean	Median	Quantile of 99 th percentile	Gini index	SNAP spending per capita	SNAP participa- tion (%)	Poverty rate (pre- SNAP; %)	Poverty rate (post- SNAP; %)	Floor ratio (pre-SNAP; $\alpha = 1$)	Floor ratio (post-SNAP; $\alpha = 1$)
1988	65.23	50.83	275.58	0.404	7.40	8.41	13.04	12.58	0.377	0.420
1989	66.44	51.44	287.74	0.406	7.68	8.43	12.82	12.25	0.376	0.419
1990	64.25	49.82	273.77	0.404	8.70	9.17	13.51	12.91	0.384	0.427
1991	63.63	49.84	272.16	0.407	8.71	9.26	14.22	13.57	0.372	0.415
1992	63.25	49.72	268.72	0.410	9.25	9.88	14.52	13.86	0.364	0.405
1993	62.98	48.87	268.17	0.417	11.26	10.61	15.14	14.42	0.363	0.401
1994	64.28	49.82	279.59	0.416	10.09	10.15	14.55	13.78	0.361	0.398
1995	68.84	50.78	345.17	0.444	10.47	9.51	13.81	13.07	0.363	0.395
1996	69.99	50.74	353.06	0.448	9.81	8.95	13.72	13.10	0.361	0.396
1997	73.00	52.91	381.59	0.450	8.33	7.75	13.25	12.69	0.338	0.368
1998	75.38	54.96	393.40	0.448	7.30	6.71	12.72	12.22	0.330	0.352
1999	75.98	55.99	375.40	0.438	6.34	6.11	11.79	11.35	0.334	0.356
2000	78.54	56.77	405.29	0.451	5.71	5.56	11.25	10.91	0.331	0.351
2001	79.18	56.68	407.80	0.457	6.24	5.65	11.69	11.31	0.308	0.327
2002	77.32	56.10	388.82	0.456	6.60	6.03	12.12	11.74	0.311	0.332
2003	77.53	56.21	386.92	0.458	7.48	6.57	12.46	12.02	0.298	0.320
2004	76.59	55.30	393.36	0.460	7.87	6.80	12.73	12.18	0.298	0.322
2005	77.90	55.94	410.52	0.463	8.42	7.01	12.60	12.05	0.295	0.318
2006	80.08	57.40	433.98	0.466	7.39	6.51	12.30	11.75	0.299	0.321
2007	78.52	57.18	403.24	0.457	7.56	6.79	12.48	11.94	0.302	0.325
2008	78.93	57.13	397.86	0.461	9.93	8.08	13.23	12.54	0.298	0.321
2009	75.88	54.50	391.44	0.468	13.80	9.91	14.34	13.24	0.294	0.322
2010	74.89	53.97	370.96	0.468	14.90	11.29	15.11	13.91	0.291	0.319
2011	75.50	53.22	399.13	0.476	15.06	11.43	14.99	13.80	0.286	0.318
2012	76.00	53.94	393.30	0.477	14.79	11.47	14.97	13.80	0.292	0.325
2013	76.55	54.64	395.07	0.475	14.03	11.43	14.48	13.39	0.287	0.317
2014	79.30	56.30	409.17	0.479	15.34	11.87	14.77	13.67	0.274	0.302
2015	82.80	59.27	426.71	0.476	14.81	11.24	13.54	12.58	0.276	0.300

2016	85.40	60.59	451.60	0.478	13.37	10.60	12.70	11.88	0.269	0.292
Mean	73.94	54.17	366.88	0.449	9.95	8.73	13.41	12.71	0.322	0.351
St. dev.	6.39	3.15	56.70	0.025	3.15	2.04	1.13	0.92	0.036	0.043

Note: Monetary values in real 2010 \$US per day except SNAP is per month. Gini index is for families (as usually calculated for the US). Poverty rate as % of population. Floor as a proportion of the threshold using $\alpha = 1$ and official poverty threshold. Authors' calculations based on CPS micro data.

Table 2: Test regressions for the pre-SNAP floor ratio

	Using official poverty line to define the reference group		Using quantile of the 20 th percentile to define the reference group	
SNAP spending per capita (log)	0.027 (0.042)	0.035 (0.040)	-0.004 (0.033)	0.002 (0.030)
Lagged dep. var.	0.388*** (0.140)	0.299* (0.171)	0.287 (0.172)	0.294* (0.171)
Year	-0.008** (0.004)	-0.006 (0.004)	-0.007** (0.003)	-0.005 (0.003)
Constant	14.995** (6.524)	10.580 (8.489)	12.388** (4.801)	9.304 (6.414)
Other controls	Log mean	Log mean, log median, log Gini, Global Financial Crisis	Log mean	Log mean, log median, log Gini, Global Financial Crisis
R ²	0.971	0.975	0.957	0.964

Note: Dependent var.=log pre-SNAP floor. N=28 (1988 lost due to lag). HAC standard errors in parentheses.
 ***: 1% significance, **: 5%; *10%. Floor ratio is used. Monetary variables are daily per capita. The control for
 the Global Financial Crisis is a dummy variable for 2009-11.

Table 3: Regressions for the log gain in the floor due to SNAP as a function of SNAP spending

	Using official poverty line to define the reference group			Using quantile of the 20 th percentile to define the reference group		
SNAP spending per capita (log)	0.473*** (0.155)	n.a.	n.a.	0.566*** (0.146)	n.a.	n.a.
SNAP participation rate (log)	n.a.	0.785*** (0.225)	0.912*** (0.085)	n.a.	0.821*** (0.195)	0.911*** (0.078)
SNAP spending per recipient (log)	n.a.	-0.233 (0.347)	-0.418 (0.309)	n.a.	0.034 (0.270)	-0.062 (0.238)
Lagged dep. var.	0.414** 0.157	0.126 (0.193)	n.a.	0.327** (0.144)	0.092 (0.163)	n.a.
Year	-0.019*** (0.006)	-0.017*** (0.005)	-0.019*** (0.005)	-0.015*** (0.004)	-0.011** (0.004)	-0.012*** (0.004)
Constant	35.994*** (10.578)	32.472** (10.494)	35.308*** (9.675)	27.447*** (7.174)	19.806** (7.836)	20.342 (7.772)
R ²	0.904	0.918	0.921	0.908	0.920	0.920

Note: Dependent var.=log (post SNAP floor - pre-SNAP floor). N=28 (1988 lost due to lag). HAC standard errors in parentheses. ***: 1% significance; **: 5%; *10%. Monetary variables are daily per capita.