

Chapter 4

Related Research about SNAP and UI

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Several studies about the increase in Supplemental Nutrition Assistance Program (SNAP) receipt during the Great Recession have appeared, but most have not looked specifically at the interaction of SNAP and unemployment insurance (UI). The exceptions are papers by Finifter and Prell (2013) and Rothstein and Valletta (2014). Work by Mulligan (2012), Ganong and Liebman (2013), and Ziliak (2016) has addressed the role of policy change in SNAP caseload expansion. This work uses publicly available data to study the dynamics of SNAP-UI interaction during the Great Recession, but it also serves to identify opportunities to improve understanding among policymakers by developing new information, as the SNAP-UI project has done.

FINIFTER AND PRELL

David Finifter and Mark Prell (2013) use the Current Population Survey's Annual Social and Economic Supplement (CPS-ASEC) to study the overlap between SNAP and UI receipt among households before and during the Great Recession, specifically for calendar years 2005 through 2009. *Household* here refers to a household as defined by the U.S. Census Bureau (i.e., everyone living at an address). UI households are households that, at the time of the ASEC, report some income from UI in the previous calendar year. SNAP households are households that, at the time of the ASEC, report some receipt of SNAP benefits during the preceding year. The authors then define overlap

from SNAP and UI perspectives: they denote the share of SNAP households that are also UI households as the SNAP joint participation rate. Similarly, the share of UI households that are also SNAP households is the UI joint participation rate. Note that joint receipt need not be coincident within the calendar year. From both perspectives, the overlap increased as the Great Recession progressed: the SNAP joint participation rate rose from 7.8 percent in 2005 to 14.4 percent in 2009; the UI joint participation rate rose from 11.1 percent in 2005 to 13.4 percent in 2009.

These joint participation rates differ from the rates reported in Chapter 3, for at least three reasons:

First, the discussion of take-up in Chapter 3 concentrates on the subset of SNAP households that include adults aged 18–59. Had Finifter and Prell applied this restriction, their rates would have been even higher.

Second, the rates reported in this paper are for coincident receipt; Finifter and Prell count as overlap any receipt of both programs at any time during the year. A household that received UI from January to March and SNAP from June to October would be counted as a joint participant for Finifter and Prell, for example, but not in the quality-control-based point-in-time calculations presented in Chapter 3.

Third, the administrative data that underlie the quality-control calculations presented earlier avoid the CPS problems with underreporting.

Nevertheless, Finifter and Prell's longer, annual perspective is important, especially given the focus on annual income in most studies. Point-in-time assessment, the only thing that can be done with the quality-control data, will miss sequential interaction of UI exhaustion with SNAP take-up. This topic is studied extensively in the state chapters that follow.

Finifter and Prell find that among households receiving SNAP, those with householders having the lowest levels of education (i.e., less than high school) are less likely than others to be joint program participants. As might be anticipated, among households receiving

UI, the likelihood of SNAP participation is greatest for those with the lowest annual income from all sources.

ROTHSTEIN AND VALLETTA

Jesse Rothstein and Robert Valletta (2014) use the 2001 and 2008 panels of the Survey of Income and Program Participation to look at the experience of panel adults who receive UI payments during spells of unemployment around the time of the 2001 “Lesser Recession” and the Great Recession of 2007–2009.¹ The Lesser Recession panel covers the period from October 2000 through January 2004; the Great Recession panel covers May 2008 through April 2013. The authors first select all instances of reports of separation from jobs of at least three months’ duration that are followed by at least one week of unemployment. The separation period ends when the job loser subsequently reports at least four consecutive weeks of employment. Identified in this way, most such spells of unemployment (73 percent in the Lesser Recession sample; 70 percent in the Great Recession sample) do not involve UI. Of those that do, Rothstein and Valletta further restrict the sample to spells in which the unemployed person receives UI for at least four months. Within this subgroup, UI payments ceased before the end of unemployment in 19 percent of spells in the Lesser Recession panel and 18 percent of spells in the Great Recession panel. Rothstein and Valletta term this group “exhaustees.”

Table 4.1 reproduces important Rothstein and Valletta results. The first set of tabulations covers all separations identified across the several interview waves for each panel. The prevalence of SNAP receipt before and after the separation is tabulated, as well as a measure of poverty status. Job separations for both panels increase the prevalence of both SNAP receipt and poverty. As should be expected given the overall increase in SNAP take-up, job losers in the 2008 panel are significantly more likely to be in households receiving SNAP than is the case for their (approximate) counterparts in the 2001 panel. While

Table 4.1 SNAP Receipt and Poverty before and after Job Separation and UI Exhaustion

	2001 SIPP panel				2008 SIPP panel			
	Obs.	Pre	Post	Diff	Obs.	Pre	Post	Diff
Before and after job separation ^a	9,341				17,663			
UI receipt		0.036 (0.148)	0.780 (0.225)	0.743 (0.018)		0.090 (0.258)	0.825 (0.242)	0.735 (0.015)
SNAP receipt		0.076 (0.256)	0.125 (0.302)	0.049 (0.015)		0.130 (0.329)	0.175 (0.355)	0.045 (0.011)
In poverty		0.074 (0.224)	0.239 (0.357)	0.165 (0.022)		0.079 (0.248)	0.209 (0.362)	0.130 (0.015)
Before and after UI exhaustion ^b	504				1,098			
SNAP receipt		0.146 (0.353)	0.155 (0.362)	0.009 (0.019)		0.216 (0.412)	0.261 (0.439)	0.044 (0.012)
In poverty		0.253 (0.435)	0.418 (0.494)	0.165 (0.032)		0.216 (0.412)	0.377 (0.485)	0.160 (0.021)

NOTES: The “universe” for the first set of tabulations is all job separations reported for working adults over all waves of the indicated SIPP Panel. The sample is restricted to separations lasting at least 26 weeks. The second set of tabulations involves only the subset of job separations in which UI terminated before employment was regained. Proportions are unweighted; choice of appropriate weights, given the time frames, is ambiguous. Experiments with various weighting choices suggest general outcomes are not sensitive to weighting strategies. Differences that are statistically significant at the 5 percent level are bolded.

^a“Pre” columns report average values and standard deviations (in parentheses) over the three months prior to the month in which job separation occurred. “Post” columns report average values and standard deviations (in parentheses) over the period beginning the month after job separation and ending six months later or in the last month of the nonemployment spell, whichever comes first. “Diff” columns report the difference in means and the standard error (in parentheses) of this difference.

^b“Pre” columns report average values and standard deviations (in parentheses) over the three months prior to the last month in which UI income was received. “Post” columns report average values and standard deviations (in parentheses) over the period beginning the month after the last month of UI receipt and ending six months later or in the last month of the nonemployment spell, whichever comes first. “Diff” columns report the difference in means and the standard error (in parentheses) of this difference.

SOURCE: Transcribed from data in Tables 2 and 3 of Rothstein and Valletta (2014). Sample sizes are estimated from information in Table 1.

the poverty rate prior to job separation is not significantly different between groups, the poverty-rate increase following job loss is significantly smaller in the 2008 panel. It is tempting to view this difference as the product of higher SNAP receipt, but Rothstein and Valletta do not include SNAP benefits in the income measure used for assessing poverty status. Had they done so, the difference in SNAP receipt post-job separation for the two episodes would almost certainly have increased the difference in poverty rates.

The second set of tabulations in the table considers the subset of separations in which the subsequent period of joblessness extends beyond termination of UI benefits. These cases are assumed to be exhaustees. Here, “pre” and “post” are defined relative to exhaustion, not job loss. The outcome of exhaustion is a significant (and almost identical) increase in the poverty rate for both the Lesser Recession and Great Recession samples, but the postexhaustion increase in SNAP take-up is statistically significant only for the Great Recession. Here, too, it is likely that the difference in poverty impact is almost certainly understated because of failure to include SNAP benefits in income.

In sum, both Finifter and Prell (2013) and Rothstein and Valletta (2014) confirm a substantial overlap between receipt of UI and SNAP during the Great Recession. Both underscore the importance of intertemporal as well as contemporary interaction—a much higher proportion of households are counted as joint recipients if that designation means experiencing both UI and SNAP receipt within a year than is true for when the combination is counted only if it occurs within a single month. Rothstein and Valletta show that the overlap increased compared to the recession of 2001, consistent with the substantial increase in SNAP access between the two recessions. Neither study attempts to identify any differences that can be attributed to variation in state policy with respect either to SNAP or to UI.

MULLIGAN

Surely the most provocative study of interaction between UI and SNAP appears in Casey Mulligan's book *The Redistribution Recession*. As the title indicates, Mulligan (2012) essentially argues that the Great Recession was caused, or at least significantly worsened, by the labor market distortions created by the social safety net. For Mulligan, the major distorting programs were SNAP, UI, and programs of mortgage modification for persons who experienced substantial loss of home value because of the collapse of the housing bubble. He also considers other policy developments—including an increase in the minimum wage—to have played perverse roles.

There are micro- and macroeconomic components to Mulligan's argument. The microeconomic component involves estimation of the effect of changes in policy on benefits available to households at different income levels. Mulligan carefully reviews both UI extensions and changes in SNAP eligibility, especially the consequences of broad-based categorical eligibility and elimination of the able-bodied-adults-without-dependents (ABAWD) work test. Such changes, he argues, raised the probability of program take-up and reduced incentives for work by raising the marginal tax rate imposed on earnings. His numerical estimates of these effects suggest that observed reduction in employment between 2007 and 2009 is largely the product of incentive effects of enhancements to the safety net. Moreover, in Mulligan's judgment, the exceptional duration of the recession and the persistent reduction in employment rates in the recession's wake are also consequences of generous safety-net policy.

The macroeconomic side of the Mulligan story is a neoclassical growth model built around a simple (Cobb-Douglas) model of the aggregate economy. In this model, a reduction of labor supply due to expansion of the safety net raises the cost of labor and leads to substitution of nonlabor inputs for labor. In his model, even the prospect of an expansion in benefits can lead to contraction. This analysis

leads, he writes, to “an unconventional causal interpretation of the sharp drops in consumption, investment, and capital market values during 2008: the drops were, in significant part, a reaction to, and an anticipation of, labor market contractions created by the expanded social safety net. In this view, it is incorrect to attribute the labor market contraction to drops in investment and consumer spending” (Mulligan 2012, p. 121).

There has been little detailed evaluation of Mulligan’s arguments. In his review of *The Redistribution Recession* for the *Journal of Economic Literature*, Christopher Foote (2013) notes that “most economists will find it hard to accept that the labor market fallout from this calamity [the Great Recession] is mostly explained by an expanded safety net,” but he fails to say why. Robert Moffitt (2015) argues that Mulligan’s constructs for marginal tax rates exaggerate the actual impact of policy changes on incentives, and that many of his choices for labor supply estimates are too large. The heart of Moffitt’s argument is a series of regressions, using Current Population Survey (CPS) data on household income, of total transfers received on private income, allowing splines in income over four ranges of earnings defined as a proportion of the poverty standard: 0–50 percent, 50–100 percent, 100–150 percent, and above 150 percent. The estimates are repeated for various years before, during, and after the Great Recession. The slope of each regression combines the effects of policy changes on take-up of all programs and labor supply conditional on take-up. Moffitt writes that “[the marginal tax rates] even during the Great Recession were never more than 18 percent. Further, the increase in [the rates] from 2005 to 2010 was never greater than 8 percentage points, which implies a reduction in the net wage rate of about 10 percent. At any reasonable wage elasticity, this would generate only minor reductions in labor supply” (p. 461).

The macroeconomic source for economists’ reluctance to accept Mulligan’s (2012) arguments is classically Keynesian. If we suppose the safety net were taken away and all disincentive for work removed, then labor supply would increase and, in the Mulligan model, wages

would fall, leading to increased employment through two channels: one being the increased demand by firms for labor, given the lower price; the other being the positive effect on the real money supply of commodity price declines engendered by cheaper labor. Classically, Keynesians have questioned the flexibility of wages and have argued that in a recession the impact of monetary expansion is diminished because of hoarding and the zero-lower-limit of interest decline.

GANONG AND LIEBMAN

Peter Ganong and Jeffrey Liebman (2018) take a long view of Food Stamp/SNAP development and use both policy and enrollment history to provide perspective on the consequences of the Great Recession for SNAP.² Like Moffitt (2015), they challenge Mulligan's (2012) ascription of the surge in unemployment during the Great Recession to increased generosity of social assistance, especially SNAP and UI.

Ganong and Liebman divide recent SNAP policy history into three intervals, defined by trends in caseload and the Mathematica estimates of participation (Cunnyngham 2017).

The first, from 1992 through 2000, is the era of welfare reform and rapid economic growth. During this period the SNAP caseload declined, both because unemployment was low and because of welfare reform (first through state waiver-based experiments and then, after 1996, in the transition to TANF). SNAP take-up declined, the authors argue, because the contraction of TANF reduced categorical eligibility.

The second period extends from 2000 through 2007. During this period take-up grew, both as a "rebound" from the contraction engendered by welfare reform and because states adopted various policies to improve program access. These policies included not only altering restrictions on vehicle ownership but also the adoption, by some states, of some form of expanded categorical eligibility.

The third period, 2007–2011, is the Great Recession, marked by a 5 percentage point increase in national civilian unemployment (from 4.6 to 9.6 percent average monthly employment for the year) and a 73 percent increase in SNAP recipients. For this period, the question is, how much of the nationwide increase in SNAP enrollment is attributable to the increase in unemployment, and how much is the result of policy change? Ganong and Liebman's (2018) innovation is to approach this attribution problem from the bottom by first estimating a model of SNAP enrollment by county, based on estimates of county unemployment rates and an index of SNAP access, given state policies, including ECE. The national SNAP caseload is then the sum of county caseloads, and changes in SNAP enrollment nationwide occur as a result of a combination of state policies operating at the county level and demand generated as changes in the national economic trends are reflected in county unemployment. To address the well-known problems with measures of unemployment rates at the county level, they develop an instrument for county unemployment change in response to statewide economic development that is based on the composition of local employment.

Ganong and Liebman estimate their model for the period 1993–2015, then use it (by summing across county estimates) to predict the path of SNAP take-up during each subperiod. The estimated model implies that trends in unemployment account for most of the decline in SNAP take-up in the late 1990s, that state policy changes are an important contributor to growth in take-up during the early 2000s, and that unemployment explains about two-thirds of the caseload expansion during the Great Recession.

Ganong and Liebman's policy index is crude, constructed by calculating how many out of eight possible policies each state has adopted at each year and employing that ratio as a right-hand variable in each county's SNAP take-up equation. This means, for example, that adoption of broad-based categorical eligibility is treated as having the same incremental impact on the prevalence of SNAP receipt as substitution of phone interviews for in-person meetings for eli-

gibility redetermination. Moreover, identification in the model is achieved because of variation across counties in unemployment rates and across states in the nuances of SNAP policy. But some important Great Recession policies, most notably elimination of the work test for ABAWDS and the increase in SNAP benefits, were implemented nationally, so no intercounty variation exists. The upshot is that Ganong and Liebman’s regression-based estimates of policy impact are suspect.

To improve their estimate of some policy effects, Ganong and Liebman turn to the SNAP quality-control sample data (see Chapter 3) and attempt to estimate the impact of policy change by counting recipient households that in the absence of the policy would be ineligible. The results of this exercise on both the eve and the end of the Great Recession are reproduced in Table 4.2. Column 1 in the table, actual total enrollment for 2007, is the average monthly recipient count for the third quarter of the fiscal year (2007Q3) from the quality-control data.³ “Eligible under standard rules” is the Ganong

Table 4.2 Ganong-Liebman Estimates of SNAP Enrollment Effects of Eligibility Changes, 2007–2011

	Enrollment (Millions of recipients)			
	Actual		Policy- Counter- induced factual (2) – (3)	
	2007	2011	2011	2011
Total enrollment	26.04	45.14		
Eligible under standard rules	24.01	38.46		
Relaxed income and asset limits				
Income > standard threshold	0.42	1.68	0.67	1.01
Assets > standard threshold	0.09	0.71	0.15	0.56
Waiver of time limits for childless adults	1.52	4.30	2.43	1.87
Total enrollment change, 2007–2011			19.1	3.44
Share attributed to eligibility changes				0.18

SOURCE: Reproduced from Ganong and Liebman (2013), Table 4.

and Liebman estimate of what the number of recipients would have been in the third quarter of 2007 in the absence of expanded categorical eligibility and waiver of the time limits in some states for ABAWDs. Thus, the estimated impact of these policies at the prerecession baseline of 2007Q3 was to increase the recipient count by 8.5 percent—2.03 million people. The difference is allocated to relaxed income and asset limits or the nationwide suspension of ABAWD time limits. Since quality-control data do not include assets, the asset test estimate is derived from other sources.

Numbers in column 2 of the table are interpreted similarly. The counterfactual estimate includes expansion of the numbers of recipients eligible because of waivers or ECE provisions in 2007 at the same rate of growth as the numbers of recipients eligible under standard rules. It incorporates no growth from waiver expansion or adoption of ECE rules in other states. The difference reported in column 4 is the change in enrollment attributed to the expansion of broad-based eligibility from 13 to 41 states and the waiver of the ABAWD time limit everywhere. The result is that an estimated 3.4 million of the total 19.1 million increase in enrollment from 2007 to 2011—18 percent—is attributable to persons added to SNAP rolls as the result of policy changes in response to the Great Recession.

Ganong and Liebman compare their estimates of impact to those of Mulligan (2012), as replicated in Table 4.3. Interpretation of this table is aided by understanding its connection to Table 4.2. Note that

Table 4.3 Comparison of Ganong-Liebman and Mulligan Policy Impact Estimates

Policy	% enrollment due to policy changes	
	GL (2011)	CM (2010)
Relaxed vehicle policies	0.0	12.0
State BBCE adoption	3.5	5.7
ABAWD waivers	4.1	2.3
Total	7.6	20.0

NOTE: “GL” = Ganong-Liebman; “CM” = Casey Mulligan.

SOURCE: Ganong and Liebman (2013).

the reference point here is total enrollment on the reference date, not change in enrollment since some baseline. For Ganong and Liebman, this is 2011; Mulligan's calculations are for 2010. Ganong and Liebman's estimate of 7.6 percent (the "Total" line in Table 4.3) is calculated by dividing the estimated sum of "policy-induced" change in enrollment (3.44 million in Table 4.2) by total enrollment (45.14 million).⁴ Two things become clear. First, neither Ganong and Liebman nor Mulligan ascribes major responsibility for the level of SNAP enrollment in 2010–2011 to policy response. For Ganong and Liebman, the culprit is, of course, the recession-induced surge in unemployment; for Mulligan it is the behavioral response to increases in benefits access and the work disincentives embedded in programs like UI and SNAP. The second conclusion is that the major share of the difference in impact stems from different treatment of the consequence of eliminating or relaxing restrictions on vehicle equity value. For Mulligan, cars count. Ganong and Liebman assume no impact of vehicle policies, because most restrictions on automobile values were already in place by 2007.

The Ganong and Liebman analysis is rich and thoughtful, and it is now regularly cited (cf. Moffitt [2015], p. 463). Disaggregation of the SNAP-unemployment response to the county level appears to provide significant improvement in understanding the response of SNAP enrollment to economic distress. Ganong and Liebman's discovery of a post-welfare reform rebound effect is useful in understanding the sources of differences in state SNAP caseload growth from early 1999 through 2005. Their analysis of data from the Survey of Income and Program Participation (included only in the 2013 version of the paper) provides insight into the impact of duration of unemployment on SNAP take-up.

However, their analysis has significant shortcomings. One concerns functional form. The Ganong and Liebman enrollment model treats SNAP take-up as a function of current unemployment rates and the unemployment rate in the two preceding years; however, the estimated cumulative impact of a sustained increase in unemployment

substantially exceeds the short-term impact of a change. Ganong and Liebman then point out that their model implies that when recession abates and unemployment falls, enrollment decline will lag. But this is the product of the symmetry of functional form that is assumed: if there is a lag in response to the upturn, there must be a lag in response to the downturn. It may be true that what goes up must come down, but no reason is offered for assuming the same path is followed in both directions.

Similarly, Ganong and Liebman's (2018) model implies that when rules change, as in the adoption of broad-based categorical eligibility, the full impact on caseload is achieved in the year following adoption. As is discussed in Chapter 3 (and in Ganong and Liebman [2013]), caseload growth is the outcome of relative rates of change in case openings and case closings. Rule changes affect these flows in different ways. It seems unlikely that the time pattern of response would be the same, and near-instantaneous, for all.

A related issue concerns the way in which variation in eligibility standards affects take-up. Ganong and Liebman dismiss Mulligan's (2012) assumption that changes in vehicle valuation requirements influenced enrollment expansion after 2007, because by 2007 most states had relaxed these vehicle valuation requirements from federal requirements. Indeed, in 2007, no state applied the federal regulation (Food and Nutrition Service 2007). But Ganong and Liebman pay no attention to the characteristics of households that were at the margin of SNAP eligibility when the Great Recession hit. It seems likely that, given the unprecedented (in recent times) incidence of job loss, the recession reached further up the distribution of households as measured by previous income status and that, as a result, those losing income were more likely to own vehicles that had a value exceeding what would have been applicable maximums. Thus, the change in vehicle policy not only changed program take-up in Ganong and Liebman's second designated period, 2002–2007; it may also have facilitated access to SNAP for the families rendered newly needy by the combination of job loss and housing contraction.

As noted earlier, assessing the effect of broad-based categorical eligibility elimination of the SNAP assets test raises a larger issue concerning inhibition. Valuing assets is not always easy, and the timing of resource measurement can make a difference—for example, whether bank accounts are measured on direct-deposit payday or the week before. In assessing the impact of removing the assets restrictions, the approach taken by Mulligan as well as by Ganong and Liebman is to presume that the Food and Nutrition Service had good-enough data on assets to fully evaluate the impact of the restriction. But giving a census interviewer a sense of one’s checking account is one thing; signing a certification on penalty of law is another. Again, the point is that elimination of the assets test may have removed an important psychological barrier to application for working-class families made SNAP-eligible because of recession-related income loss.

ZILIAK

Like Finifter and Prell (2013), James Ziliak (2016) uses the CPS-ASEC annual data to study the reported incidence across households of SNAP receipt at any time during the year. However, Ziliak’s focus is on the determinants of take-up, not on the overlap of SNAP receipt with benefits such as UI. The core model is a linear probability function:

$$(4.1) \quad SNAP_{ijt} = \alpha + X_{ij}\gamma + Z_{jt}\delta + \pi_j + \varphi_t + u_{ijt}.$$

$SNAP_{ijt}$ is an indicator equal to 1 if any member of household i in state j reports receiving SNAP in year t . X_{ij} is a vector of demographic descriptors for the household, Z_{jt} is a vector of economic and policy variables, π_j is an indicator (fixed effect) for the household’s state of residence, φ_t is an indicator for the reference year, and u_{ijt} is a random error term. The coefficients are estimated by least squares, and

standard errors are adjusted for heteroskedasticity. The data cover 32 years, 1980–2011.

The demographic descriptors include various characteristics of the person designated by Census Bureau convention as household head, as well as measures of household composition. The economic descriptors include the state unemployment rate in the current as well as the two preceding years, median state income, and a measure of income dispersion. There are 20 variables measuring the state policy environment, including the level of the SNAP benefit schedule and the presence or absence of broad-based categorical eligibility. Because SNAP receipt may affect family income, family income is excluded from the model, but many of the demographic variables provide control for the expected economic status.

Among other things, Ziliak finds substantial positive effects of the state's unemployment rate (current and lagged) on the probability a household will report SNAP receipt, and various indicators of the level of SNAP benefits and ease of access. Notably, the presence of broad-based categorical eligibility is estimated to raise the prevalence of receipt by 0.6 percentage points in states that adopt the policy.

Ziliak assumes no interactions among the variables included in Equation (4.1). The advantage of this assumption is that effects are additive, and the contribution of groups of variables to change over some interval can be calculated by comparing the change with and without alteration of these measures from baseline values. Ziliak divides variables into four groups:

- 1) Measures of the state's economy (unemployment rates, income distribution)
- 2) Measures of nonfood policies (minimum wage, Earned Income Tax Credit, Aid to Families with Dependent Children [AFDC]/Temporary Assistance for Needy Families [TANF] details)
- 3) Measures of food policy (SNAP benefit, broad-based categorical eligibility, other state eligibility and procedural requirements)

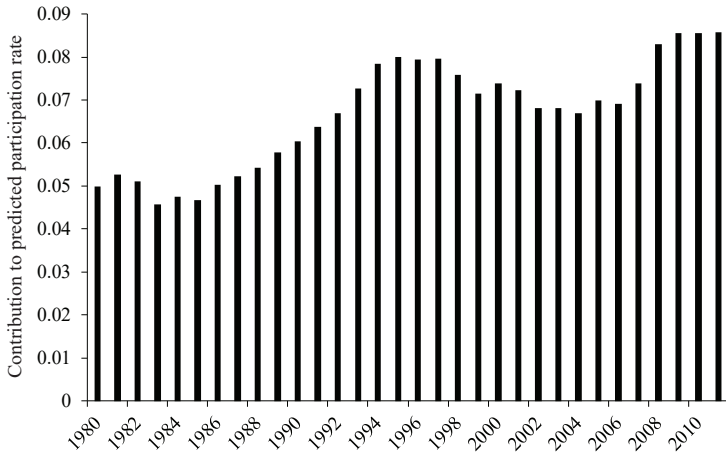
- 4) Demographics (size of household, characteristics of household head, and so forth)

He then calculates increase in the prevalence of SNAP receipt from a baseline year that would have been predicted to occur in the absence of change in the state's values for the variables in each group, allowing the other variables to change as recorded.

Ziliak performs these estimates for three periods, 2007–2011, 2000–2011, and 1980–2011. The results for 2007–2011 are illustrative: the baseline (2007) household participation rate was 6.5 percent; the rate in 2011 was 11.0 percent, 69 percent higher.⁵ Using regression estimates for Equation (4.1), Ziliak calculates that had the economy variables been held constant for all states at 2007 levels and all else allowed to change, the predicted increase in SNAP take-up would have been 35.8 percent. Hence the economy accounted for $(68.7 - 35.8) / 68.7 = 47.9$ percent of the change. Similar calculations attribute 1.6 percent of the increase to change in nonfood policies, 28.5 percent to change in food policies, and -3.7 percent to demographics (i.e., average household characteristics changed in ways that to a small extent offset the effects of other factors). The bottom line: the economy was twice as important in determining the SNAP case-load change between 2007 and 2011 as was change in food policy, including the expansion of broad-based categorical eligibility evident in Figure 3.1 from Chapter 3 of this book. The implication—indeed, the assumed structure of the model requires it—is that when the economy improves, should policy retreat, take-up will decline. Ziliak uses the regression to predict a decline of 12.2 percent following expiration at the end of Fiscal Year 2013 of the benefit increase created by ARRA (p. 33).

Note that the combination of estimated effects of the four variable groups for the change in the SNAP participation rate between 2007 and 2011 accounts for 74.2 percent of the total increase. The residual, over a quarter of the entire change, is accounted for by year fixed effects, the ϕ_t in Equation (4.1). It is instructive to look at the pattern of the fixed effects estimates. In Figure 4.1, the sum of the intercept

Figure 4.1 Intercept plus Year Fixed Effects, SNAP Participation Regression



SOURCE: Ziliak (2016).

and the year fixed effect is plotted for each year of the entire time span of the Ziliak sample. The change in bar height between dates is the amount of the increase (or reduction in the decrease) in the participation rate not attributed to alteration in values of other variables in the model. For 2007–2011, the change is 0.12. This “unexplained” component is slightly more than a quarter of the total take-up rate increase over the period.

Years ago, the “year fixed effects” would have been termed “dummy variables,” and caution is in order in their interpretation. The important message is that there is a substantial component of the SNAP take-up during the Great Recession that is greater than would have been predicted based on changes in the various components of Ziliak’s variable catalog. Moreover, the effect is constant over the three years 2009–2011. This unidentified component of change coincident to the Great Recession poses a significant problem for forecasting the future. One obvious next step would be to enrich the depic-

tion of policy (the Ziliak model includes no representation of state ABAWD policy and no reference to variation in other policies—notably UI—likely to affect SNAP take-up) and add years. The problem with extension is that the catalog of state policies developed by the USDA’s Economic Research Service and used by Ziliak has not at this writing been updated, and the data on timing and content of state policy collected by the Food and Nutrition Service are problematic. This is in part because of mysteries surrounding how TANF funds are used to confer categorical eligibility—in other words, the “base” in “broad-based categorical eligibility” is poorly defined.

CONCLUSIONS

The following conclusions emerge from this literature review:

- Liberalization of policy led to a steady increase in SNAP participation from 2001 on.
- The surge in SNAP participation as unemployment rose in the Great Recession was consistent with previous correlation evidence.
- Change in the ABAWD rules contributed significantly to the increase in SNAP receipt during the Great Recession.
- The impact of other policies associated with broad-based categorical eligibility is difficult to ascertain, in part because of uncertainty of timing and lack of attention to the time pattern of change in take-up in response to broad-based categorical eligibility implementation.
- It appears, from Rothstein and Valletta (2014), that SNAP played a greater role in income support for UI recipients during the Great Recession than was observed in the Lesser Recession, and that the importance of SNAP increased with UI exhaustion.

- Symmetry is an issue: must what went up (SNAP receipt) with the surge in joblessness come down with recovery, or did changes in SNAP policy produce a structural change in program take-up that will be sustained?

We end on this point: there is much to be learned from study at the state level, especially if better data can be obtained on the pattern of receipt of UI and SNAP benefits over time.

Notes

1. A revised version of their paper (Rothstein and Valletta 2017) was released as a National Bureau of Economic Research working paper in 2017. The revision, done for publication, combines analysis of UI recipient experience in the 2001 and 2007–2009 recessions because “reviewers generally felt that the differences in UI exhaustion effects between the 2001 and 2007–09 recessions were not substantial enough to consistently highlight them throughout the paper” (Rothstein and Valletta, e-mail to author). However, the difference in SNAP utilization is important to this chapter, and the general results from the Rothstein and Valletta analysis do not differ between versions.
2. The original version of the Ganong and Liebman paper includes important additional analyses. See Ganong and Liebman (2013).
3. The quality-control numbers are slightly lower than official recipient counts because the quality-control data set excludes cases judged in the quality-control audit to have been granted benefits in error.
4. Ganong and Liebman’s (2013) version of Table 4.3 includes a small inconsistency within the data they report in the original version of Table 4.2 for state broad-based categorical eligibility adoption. This is corrected here.
5. We thank James P. Ziliak for providing these data and the information on year fixed effects presented in Figure 4.1.

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**Supplemental Nutrition Assistance
and Unemployment Insurance**

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