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Local Labor Demand and Program Participation Dynamics: Evidence from New York SNAP Administrative Records

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Local Labor Demand and Program Participation Dynamics: Evidence from New York SNAP Administrative Records

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Abstract

This study estimates the effect of fluctuations in local labor conditions on the likelihood that existing participants are able to transition out of the Supplemental Nutrition Assistance Program (SNAP). Our primary data are SNAP administrative records from New York (2007–2012) linked to the 2010 Census at the person-level. We further augment these data by linking to industry-specific labor market indicators at the county-level. We find that local labor markets matter for the length of time individuals spend on SNAP, but there is substantial heterogeneity in estimated effects across local industries. While employment growth in industries with small shares of SNAP participants has no impact on SNAP exits, growth in local industries with high shares of SNAP participants, especially food service and retail, significantly increases the likelihood that recipients exit the program. We also observe corresponding increases in entries when these industries experience localized contractions. Notably, estimated industry effects vary across race groups and parental status, with Black Alone non-Hispanic, Hispanic, and mothers benefiting the least from improvements in local labor market conditions. Our models include county fixed effects and time-trends, and our results are identified by detrended within-county variation in local labor market conditions. We confirm that our results are not driven by endogenous inter-county mobility, New York City labor markets, or cohort composition effects associated with the Great Recession.

Keywords: Administrative Records, Duration Models, Local Labor Markets, Program Participation
JEL Codes: I32, I38, J23

1 Introduction

The link between labor market conditions and participation in US Department of Agriculture's (USDA) Supplemental Nutrition Assistance Program (SNAP) has long been of interest to policy makers and program administrators. Historically, SNAP caseloads

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have risen and fallen with the unemployment and poverty rates, suggesting that the economy is an important driver of program participation (Mabli and Ohls, 2012; Mabli et al., 2011a; Hanson and Oliveira, 2012). It was unsurprising, then, when SNAP caseloads reached historically high levels during the Great Recession, which was the worst economic downturn in the U.S. since the program's inception. During the recovery that followed, however, the caseload has been slower to decline. This persistently high caseload has led to concerns that participants are not responsive to economic opportunity and that SNAP receipt itself fosters long- term dependence.

Reflecting these concerns, measures have been put forward that seek to impose stricter work requirements for SNAP participants.[1] Yet there is relatively little evidence to inform the current debate on tighter and broader work requirements that would decouple SNAP eligibility determination from labor market conditions. Much of the evidence that does exist on the link between SNAP participation and the economy comes from caseload studies that measure labor market conditions at the state or national level (Currie and Grogger, 2001; Ratcliffe et al., 2008; Klerman and Danielson, 2011; Hanratty, 2006; Kornfeld, 2002; Kabbani and Wilde, 2003; Ziliak et al., 2003; Figlio et al., 2000; Ziliak, 2015; Mabli et al., 2009; Hanson and Oliveira, 2012; Hardy et al., 2015). These studies have generally found SNAP caseloads to be responsive to changes in economic conditions, although the estimated magnitudes have varied widely. Moreover, the responsiveness of the caseload to economic conditions may not be symmetric. The caseload has tended to be more responsive to economic downturns than to upswings. The same trend appears to hold during the most recent recovery: the SNAP caseload has remained high even as national labor market indicators began to show a gradual recovery underway.

Individuals' employment prospects, however, may depend on more local labor market conditions, which can differ substantially from conditions at even the state level. And since low-income employment tends to be concentrated in certain industries, capturing labor market conditions relevant to SNAP recipients (and those at-risk for SNAP receipt) may be further improved by disaggregating indicators by industry, as well as by geography. Capturing this latter type of heterogeneity may be particularly important during a recovery that has proceeded unevenly across sectors of the economy (Bitler and Hoynes, 2015). Lastly, even if employment in SNAP-relevant sectors has improved, and more recipients are finding jobs, wages in those jobs may still not be sufficient to lift their earnings above the income threshold for program eligibility. Indeed, recent work has shown that SNAP caseloads track poverty rates more closely than unemployment rates, particularly in recent years (Stone et al., 2015). The slower downward adjustment of the SNAP caseload during the economic recovery does not necessarily demonstrate therefore that SNAP recipients are unresponsive to labor market opportunities; it may be that indicators of the appropriate labor markets for SNAP recipients are not being considered.

This study uses SNAP administrative records from New York State linked to county-

[1]For example, a proposed (though ultimately unsuccessful) amendment to the 2012 Farm Bill would have eliminated state waivers of the federal time limits on the participation of able-bodied adults without dependents (ABAWDs) and would have expanded time limits to nearly all SNAP participants who do not work or participate in a training program, including parents with young children and individuals with disabilities.

level labor market indicators to obtain more accurate estimates of the effect of local labor market conditions on participants' decisions to transition off the program. Building on similar research by Hoynes (2000) and Herbst and Stevens (2009), who used administrative program records to examine effects of local labor market conditions on cash welfare receipt (i.e., Assistance for Families with Dependent Children (AFDC) and, later, Temporary Assistance for Needy Families (TANF)), we use discrete-time hazard models to estimate the probability of exit from SNAP in a given month. In addition, we investigate the relationship between local labor market conditions and SNAP entry rates. We focus on the influence of county-level employment, overall and by industry. Our models also control for individual and case characteristics, county fixed effects, year effects, and county-level time trends.

We find that local labor market conditions matter both for the length of time individuals spend on SNAP and also for the number of new entries to the program. In particular, employment growth in the local food service and retail industries—two important destination industries for SNAP participants—increases the likelihood of a recipient leaving the program in a given month and decreases the entry rate in that locality. These results are fairly robust to a variety of specification checks, and indicate that modest growth in food service and retail employment—controlling for the size of the overall county population and labor force—can significantly increase the hazard of exit from SNAP, and hence shorten SNAP spell durations, conditional on entry. For instance, raising county-level employment in retail by one percent leads to a more than threefold increase in the likelihood of SNAP recipient in that county leaving the program in that month. Similarly, a rise in county-level employment in the food service industry is associated with an over 50 percent increase in the hazard of program exit.

The results are also quite robust to unobserved differences that may exist between single-spell participants and multiple-spell participants, as our estimates are very similar for participants in their first spells and second spells. We also show that our main findings do not change when introducing lagged local labor market variables to account for potential general equilibrium effects, controlling for unobserved individual heterogeneity, or when excluding New York City residents from our models.

We also rule out concerns that residential mobility may bias our results. For example, if more motivated SNAP participants endogenously relocate within-state to counties with favorable labor market conditions, then our estimates of local labor market effects on the hazard of exiting SNAP would be biased upward.[2] We find, however, that our results are very similar when estimating our models over the subsample of individuals whose county of residence does not change during our observation period.[3]

This study extends the literature on SNAP and the economy in several ways. This is the

[2]Similarly, if households anticipating an extended time on the program relocate to more economically depressed areas, where the cost of living is lower, then that would also bias our estimates upward.

[3]Out-of-state mobility presents other hurdles. Since the data do not allow us to observe individuals when they are not receiving program benefits in New York State, we are unable to distinguish apparent program exits from migration to another state where SNAP receipt continues. Two factors mitigate this potential issues. One is that in some cases out- of-state moves are identified in the administrative records. The other mitigating factor is that estimates from the American Community Survey (ACS) indicate the incidence of out-of-state moves in a given year is quite low, on the order of one to two percent (U.S. Census Bureau, 2016a).

first study that, to the best of our knowledge, uses SNAP administrative records to analyze the relationship between local labor market conditions and SNAP participants' spell duration. Second, it evaluates the importance of employing more granular measures of labor market conditions when analyzing the impact of the economy on SNAP enrollment. Lastly, by using recent microdata on SNAP receipt from New York—a large and diverse state—this study provides improved, and more recent, estimates of SNAP recipients' behavioral responses to local labor market conditions. This last contribution is similar in nature to Hoynes (2000) and Herbst and Stevens (2009), who used administrative records to examine the AFDC/TANF programs in California and Maryland. Our study extends this previous work by accounting for utilization of other safety-net programs such as TANF and several state public assistance programs while estimating the labor market effect of participation in SNAP, an important step when considering program participation dynamics in the context of multiple program enrollment. Our study also differentiates itself by showing how antipoverty program participants respond to economic opportunity when programs offer in-kind benefits rather than direct cash transfers (as in AFDC/TANF). This is particularly relevant in light of large role in the U.S. safety-net of programs offering in-kind benefit transfers, such as SNAP, Housing Assistance, and the Special Supplemental Nutrition Program for Women, Infants and Children (WIC).

The remainder of the paper is organized as follows. Section 2 describes the literature on factors influencing individuals' entry into and exit from SNAP. Section 3 describes the data and sample construction, Section 4 details our estimation strategy, and Section 5 presents our results and sensitivity analysis. Section 6 offers concluding remarks.

2 Prior Research on SNAP Dynamics

A number of studies have investigated the dynamics of SNAP participation using household microdata. The USDA Food and Nutrition Service (FNS) has commissioned an ongoing series of reports, produced by Mathematica Policy Research (MPR), that uses the most recent panel of the Survey of Income and Program Participation (SIPP) to analyze the determinants of program entry and exit (Gleason et al., 1998; Cody et al., 2005, 2007; Mabli et al., 2011b,a). These reports have focused on state policy variables and household-level trigger events and have consistently identified adverse income shocks as the most common trigger for program entry. Along the same lines, Mabli and Ohls (2012) used SIPP data from 2001 to 2003 to focus on the relationship between SNAP dynamics and changes in employment status. Their results suggest that employment changes are more strongly associated with entry to (and exit from) SNAP for individuals with more stable employment histories.

These reports employ longitudinal data that follow respondents for a period of about 2-3 years. But this rather brief window of observation gives rise to two shortcomings. One is that many of the program spells observed in the data are left-censored and therefore the beginning of the spell, as well as other events contemporaneous with the start of the spell, cannot be identified. Omitting left-censored spells, which tend to be longer than average, results in a biased sample.[4] Atasoy et al. (2010) pursue a somewhat different

[4]Another shortcoming, which we are not able to fully address in this study, is that researchers often cannot determine if the spell observed in the data is in fact an individual's first, or subsequent, spell on the program.

approach to the study of SNAP receipt dynamics. Rather than estimating a duration model, they employ lagged-dependent variable models, which control for individual unobserved heterogeneity and estimate one-period state dependence in SNAP. Their sample is drawn from the Panel Study of Income Dynamics (PSID) which allows them to examine SNAP dynamics over a longer time frame than the studies using the SIPP. They found that welfare reform measures also had the effect of reducing long-term dependence (i.e., referred to as state dependence) in SNAP participation. Moreover, SNAP policies that discourage program entry, either through changes in benefit levels or certification requirements, also have the effect of reducing state dependence in SNAP participation.

Schroeder (2007), Cadena et al. (2008), and Ribar (2005) each study dynamics using administrative records from a single state. Ribar (2005), however, is the only one of these studies to model unobserved heterogeneity. Using the NLSY79, Baum (2008) examines the role of SNAP in transitions off cash welfare and into employment. He finds some evidence that SNAP may discourage employment and transitions off welfare. Although with Atasoy et al. (2010), this is one of the few studies to explicitly account for individual unobserved heterogeneity. In this case, discrete mass points are used to approximate the distribution of unobserved heterogeneity. However, this study does not directly model SNAP dynamics, but rather is interested in the dynamics of TANF and work (e.g., on welfare without work, off welfare with work, etc.).

Another strand of evidence on SNAP and labor market comes from studies using household survey data (Mabli et al., 2011a; Mabli and Ohls, 2012; Ziliak, 2015; Ganong and Liebman, 2013). These data sources also suffer from a number of shortcomings. Although household surveys provide rich information on individuals, they typically do not disclose sub-state geographic identifiers, so that labor markets smaller than the state cannot be identified. Moreover, sample sizes in the household panel data are generally too small to support analysis of labor market conditions at a level lower than the state. A final concern with survey data is the well-documented measurement error in SNAP participation itself (Meyer et al., 2009; Meyer and Goerge, 2011). SNAP participation is measured with substantial error in the cross-section, and survey measures of participation spells appear to suffer from even greater mismeasurement (Bollinger and David, 2005, 2010).

3 Data and Descriptive Statistics

3.1 Data Sources

We use administrative records from New York State linked to a number of other data sources that provide information on person and county characteristics. The use of administrative microdata address a number of shortcomings associated with other data sources. For example, many studies utilize administrative caseload data aggregated monthly in order to study patterns of program participation. However, aggregate caseload data for a given month combine existing spells and new entries, so researchers cannot separately identify the effects of spell length from changes in entry rates. Individual microdata, on the other hand, allow researchers to identify the effect of spell length *conditional on* *entry* (Hoynes, 2000).[5] Second, microdata permit anlysis by demographic subgroup, which is not possible using aggregated data.

The administrative microdata also confer several benefits over microdata from household surveys. One of the chief benefits is that SNAP administrative records allow us to overcome the misreporting of program receipt pervasive in household survey data (Meyer et al., 2009; Meyer and Goerge, 2011; Meyer et al., 2015). The administrative records also provide rich information on how individuals are grouped to form SNAP cases. This is important because the survey concept of a household often differs from the SNAP case unit, and a given survey household can contain multiple case units. The administrative data allow us to precisely measure characteristics of the case unit, such as unit size, the number of elderly and non-elderly individuals, presence of children, and monthly benefit transfer amounts. Finally, our data feature a long observation period (six years) relative to the Survey of Income and Program Participation (SIPP), which is commonly used to analyze dynamic aspects of SNAP participation. This long panel enables us to better observe spells in their entirety, obviating to a greater degree the difficulties that can arise from left-censored observations, but it also allows us to better identify individuals with multiple spells. Accounting for multiple spells may be important given over 30 percent of recipients in our six-year observation period experienced two or more SNAP spells.[6]

Another benefit of the administrative records is that they contain very granular geographic identifiers, down to the Census block and tract level. In this study, we demarcate the local labor market as the recipient's county of residence and use county-level identifiers to merge to two other sources of data: the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) and the Census Bureau Quarterly Census of Employment and Wages (QCEW). From the LAUS data we obtain monthly county-level unemployment rates. From the QCEW, we obtain county- level monthly employment counts, overall and by industry. The QCEW also provide quarterly wage data. In this study, we use the average weekly wage in a quarter, both across all industries within a county and disaggregated by industry. These data sources allow us to address potential measurement error that stems from defining the labor market too broadly and thereby masking potentially important heterogeneity in labor market conditions across counties within a state and across industries within a county (Lindo, 2013).

An important consideration for this study is how to delineate local labor markets. In this study, we define local labor markets as coterminous with counties. To be sure, counties do not always correspond to local economies: local labor markets can encompass several counties or, in some cases, a county can contain more than one local labor market. Other measures, such as commuting zones (CZs) and Labor Market Areas (LMAs), have been developed in an attempt to better capture economically integrated geographic areas, generally defined as areas in which individuals can reside and find employment within a reasonable distance. These alternative measures are also not without drawbacks. Commuting zones, developed by USDA Economic Research Service (ERS), have been not

[5]The caseload, measured at a given point in time, is a stock sample, and therefore subject to length-biased sampling. The administrative microdata, on the other hand, is a flow sample.

[6]We smooth over one month gaps in enrollment to avoid treating temporary lapses in certification as actual spell breaks.

been updated since the release of measures based on the 2000 Census, and are currently undergoing a revision of the underlying methodology. LMAs, also developed by USDA ERS, were last constructed from the 1990 decennial census and have since been discontinued. BLS defines its own Labor Market Areas, based primarily on metropolitan and micropolitan statistical areas (which in turn are based on Core- Based Statistical Areas (CBSAs)), as well as small LMAs which typically consist of one or more counties. Previous work on local labor markets has relied primarily on the county-based definitions (Hoynes, 2000; Herbst and Stevens, 2009; Lindo, 2013). While counties may not perfectly capture local labor markets, we believe they remain reasonable, and convenient, approximations to local labor markets. How different definitions of local labor markets affect the analysis presented here is a topic of future work.

The administrative records are a longitudinal file, composed of person-month records. We identify individuals over time based on a unique, protected, identifier. When a Social Security Number (SSN) is available in a data set like the SNAP administrative records, the identifier is assigned primarily based on SSN (in essence, the unique identifier is a "scrambled" SSN).[7] Files like the 2010 Census, which do not include an SSN, use personally identifiable information such as name, address, and date of birth are used in probabilistic matching to assign persons to their identifier. The fields used for matching are compared against the same fields in a master reference file that contains the unique identifier. In all cases, personal information is then removed from each data set before a researcher may link the data sets together and use them for research purposes. Only those observations that received the unique identifier are used in the analysis. For more information on the linking process, see Wagner and Layne (2014).

The unique protected identifier permits us to augment limited demographic information in the administrative records. We use the identifier to link individuals to their responses about race and Hispanic origin in the 2010 Census. These demographic characteristics allow us to control not only for overall differences in the likelihood of exiting SNAP across groups, but also for differential responses across groups to changes in local labor market conditions. A discussion of the data linkage, including match rates and limitations of this approach, follows in Section 3.3.

3.2 Sample Construction

The universe of SNAP participants in New York from 2007 to 2012 includes records for 185.2 million person-months, representing just under 8.6 million individuals and 11.1 million participation spells.[8] In general, the rate of protected identifier assignment in the administrative data is very high. Of the 185.2 million person- months, 99.2 percent of the records were assigned an identifier. Of the nearly 8.6 million person records, 96.8 percent were assigned an identifier.

To construct our analysis sample, we drop all individuals without an identifier. Next, in order to focus on the working-age population, we exclude children under 18 and

[7]The assignment is validated by also ensuring name and date of birth also match.

[8]As is typically done with survey data, we smooth over one month gaps in spells, as these are likely the result of administrative churning that reflects neither a true interruption in the spell or a change in the participant's economic circumstances.

individuals over 64 years of age.[9] Finally, we drop one-month spells.

Even with these restrictions imposed on the universe of SNAP administrative records, our full analysis sample is still quite large. In the six years of our analysis period, we observe nearly 84.5 million person-months, with 4.2 million entries and 3.6 million exits. We observe over 3.9 million unique individuals with over 5 million participation spells. These data are used in the first part of Section 3.3 to provide a complete picture of caseloads, entries, and exits.

The resulting administrative data are linked to the set of individuals assigned a protected identifier in the 2010 Census for New York state. Although the identifier assignment rate for the New York component of the Census is not as high as that of the administrative records, it is still quite high at 90.8 percent.[10] The result of the data linkage was that 74.2 percent of the person-months and 71.6 percent of the persons in the SNAP administrative data were matched to their records in the 2010 Census.

The final step in constructing our analysis sample is to take a two percent random flow sample of our data for our model estimation. We do this because estimation of non-linear hazard models over the full matched data set is computationally intensive, particularly when including county fixed effects. This is a relatively common approach. Hoynes (2000), for example, works with a 1 percent sample of California's Medicaid data.

It is important to be aware that whether or not a person is successfully assigned a protected identifier, particularly in the case of the Census data, is non-random. Young children, minorities, residents of group quarters, immigrants, recent movers, low-income individuals, and non- employed individuals are less likely to receive a PIK (Bond et al., 2014; Rastogi and O'Hara, 2012). Since we focus our analysis on working age adults, the low rate of identifier assignment for young children will not affect our results, although the low rates of identifier assignment for the other groups listed may introduce composition bias.

Appendix Table A1 provides a detailed analysis of the differences between the matched and non-matched samples. In general, those individuals whose records were not found in the Census are more likely to be male, live in smaller households, live with fewer children, and live with fewer elderly members. They are also more likely to live in metropolitan areas and are more likely to have moved during the observation period. While these differences are mostly statistically significant, many of them are practically small. Furthermore, there do not appear to be meaningful differences, in age, participation in other programs like TANF and state public assistance, or the year in which someone first appear in the data. In addition, estimating our main models over the entire sample, but without controlling for the demographic characteristics included in the matched sample, yields findings that are similar to our main results.

[9] Furthermore, members of these age groups are less likely to play a role in the decision to participate in the program. According to SNAP rules, children under 18 years of age cannot form their own SNAP unit if there is another "responsible" adult in the household. And as with children, some elderly individuals will also be more likely to be a dependent in a SNAP unit and thus may not be making the participation decision on their own.

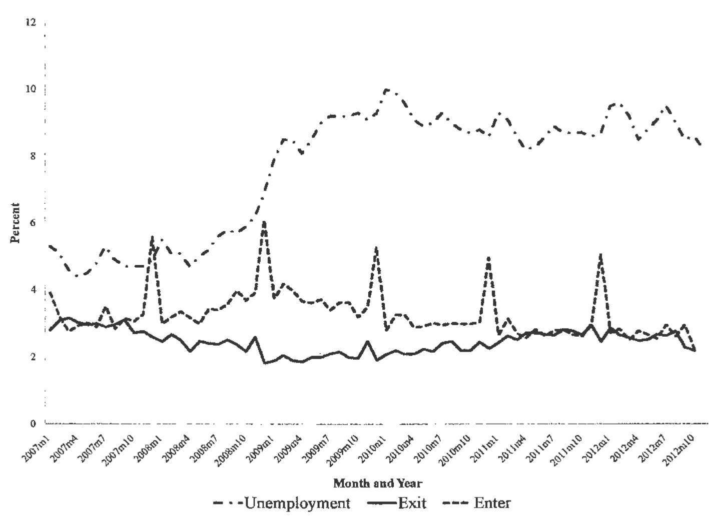
[10] The relatively high match rate in the administrative files is due to the requirement that individuals provide valid Social Security Numbers in order to qualify for SNAP benefits. The decennial census, on the other hand, does not collect respondents' SSNs.

3.3 Descriptive Statistics

We begin by discussing censoring, individual spell counts, and spell lengths using the full set of administrative records, subject to the restrictions described in the preceding section. We are able to observe roughly 60 percent of the spells in their entirety. Left-censored spells—i.e., spells in progress as of January 2007—account for about 16.5 percent of all spells. Of these, 22.8 percent are also right-censored. A further 25 percent of spells are right-censored only, so that right-censored spells account in total for nearly 28.7 percent of spells.[11] The high incidence of right-censored spells is a product of the large number of spells that started during the recession and recovery but did not end as of December 2012. Fortunately, right-censoring does not pose a problem for the hazard models we estimate below.

The majority of individuals in our data—75.1 percent—have only one spell between 2007 and 2012. Nearly 25 percent of the recipients during this period, however, have multiple spells of SNAP receipt. Specifically, 18.5 percent have two spells, 4.8 percent have 3 spells, and nearly 2 percent have 4 or more spells. Among all spells, the mean spell length is 16.9 months. Restricting attention to only complete (i.e., non-censored spells), the mean spell length is nearly 11 months. As expected, excluding censored spells biases the mean spell lengths downward. Given the degree of right-censoring, however, this bias is perhaps not as large as one might have expected. The longest possible time participating in our data is 72 months. The mean total number of months participating, however, is 21.6 months.

Figure 1
SNAP Entry and Exit Rates in New York



Source: 2007-2012 New York SNAP Administrative Records and BLS Local Area Unemployment Statistics

Figure 1 plots monthly SNAP entry and exit rates, along with the (seasonally unad-

[11] In some cases temporary out-of- state residency is recorded in the administrative records, typically the last month of an observed spell. We treat these spells as censored.

justed) unemployment rate in New York. The figure illustrates the divergence of entry and exit rates starting early in 2008, which drove the large increase in the SNAP caseload over this period. The figure also shows a convergence of the two rates early in 2011. Again, it is notable that the caseload changes were driven by substantial changes in both entry and exit rates, although entry rates appeared to adjust more quickly to their pre-recession level. This occurred despite a very modest decline in the state unemployment rate.

What is also striking about this figure are the pronounced spikes in the entry rate in January of each year. This is likely the result of a program in New York State under which Supplemental Security Income (SSI) recipients who live alone are automatically enrolled in SNAP under the New York State Nutrition Improvement Project (NYSNIP) at the beginning of the calendar year. These new case openings will be closed if the benefits are not used in a given amount of time, thus many of these case openings are in fact spurious spells.[12]

Figure 2
New York SNAP Caseload and State Unemployment Rate

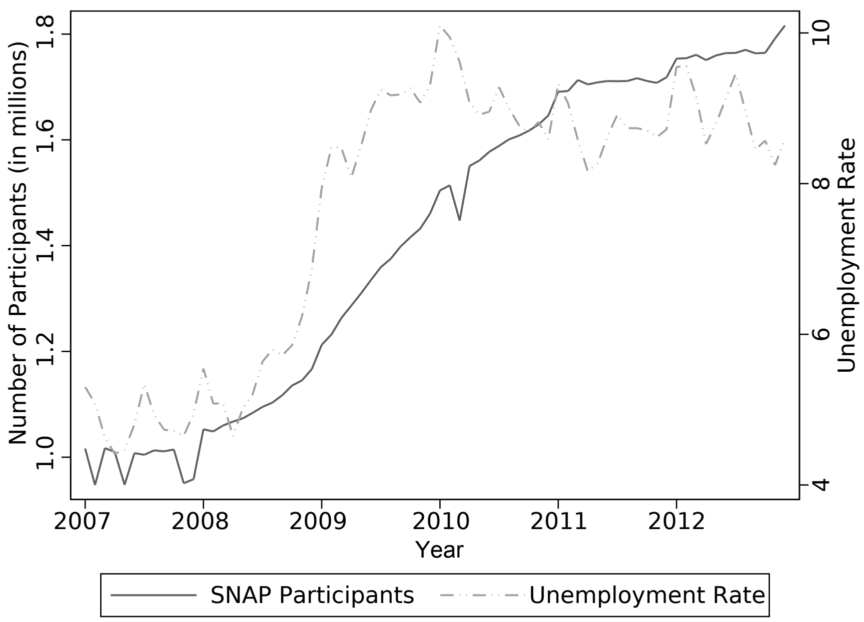


Figure 2 plots the time series of the SNAP caseload (measured in persons) and the (unadjusted) unemployment rate in New York.[13] Prior to the recession, there was an interesting association between the unadjusted unemployment rate and the SNAP caseload. We observe the rate of growth in the SNAP caseload leveling off starting in 2011, but the overall caseload continued to grow, which may indicate that the January "spikes" observed in Figure 1 may not entirely reflect spurious spells.

Table 1 displays descriptive statistics for the two percent estimation sample linked to the 2010 Census. The unit of analysis is the individual in the first month we observe that person in the administrative records. More than half of the sample is female (56 percent), and the average age is nearly 37 years old. About 40 percent of the sample

[12]More research will need to be done to try to identify these NYSNIP-related spells.

[13]Note that, due to our prior cuts to the data, these numbers represent the number of adults on the programs, also excluding those over age 64.

reported their race as non-Hispanic White alone in the 2010 Census, 25 percent reported non-Hispanic Black alone, and 28 percent reported Hispanic. This distribution differs from the overall population of 18 to 64 year olds in New York state who were assigned a PIK. Among those, 63 percent report their race as non- Hispanic White alone, 13 percent as non-Hispanic Black or African American alone, and 15 percent as Hispanic.[14] Thus, the SNAP participants in our data are less likely than the general population of 18 to 64 year olds to report non-Hispanic White alone, and more likely to report non-Hispanic Black alone or Hispanic.

The average case unit size is 2.5 members, with 25 percent of cases having a child four years old or younger and another 22 percent having child between the ages of five and 17. About 4 percent of case units have a member who is elderly. The average log monthly benefit amount for a case was 5.55, or $257.24, 2 percent of cases were on TANF at some point in the observation period, and 4 percent were on another form of state-run public assistance. Most participants in our data (76 percent) lived in metropolitan areas, and only 7 percent of the sample relocated from one county in New York State to another during the sample period.

4 Methodology

Our main methodological approach is to estimate hazard models to examine the determinants of exit from SNAP. Specifically, we estimate discrete-time hazard models of program exit using the complementary log-log link function. Individuals are considered to be at risk of exiting SNAP as soon as they are first observed to enter the program. Thus for individual i the probability of exiting a SNAP spell at time t in county c is given by:

$$\lambda_{ict} = Pr(T_i | T_i \geq t, x_{ict}) = F(\alpha_0 + \alpha_1(t)x_{ict} + \gamma_{it}), \tag{1}$$

where $F(\cdot)$ denotes the complementary log-log link function. This functional form approximates in discrete-time the proportional hazard model in continuous time. The vector x_{ict} contains the explanatory variables of the model and γ_{it} represents duration dependence, or the effect on the SNAP exit probability of time at risk. We choose a relatively flexible functional form for the baseline hazard: modeling it as a cubic function of time spent on SNAP.[15] The discrete-time hazard model estimates the probability of an individual exiting SNAP in a given month conditional on that individual not having left the program prior to that month.

The log likelihood function takes the following form:

$$log\, L = \sum_{i=1}^{N} \sum_{t=1}^{\bar{t}} [(1 - y_{it})log(1 - \lambda_{it}) + y_{it} log \lambda_{it}] \tag{2}$$

where $\bar{t}$ is the longest observed duration, N is the number of individuals in the sample, and y_{it} is equal to one if individual i is observed to exit SNAP in period t and is equal to zero otherwise. With the data arranged in person-period format, individuals who do not exit SNAP during the sample period will have a y_{it} sequence equal to zero for every

[14] Authors' calculations from internal 2010 Census files.

[15] An even more flexible form would model the baseline hazard as a monthly step function. With our large data set, and long panels, this functional form made it more difficult for our models to converge.

period, t. Individuals observed to exit SNAP during the period will have a y_{it} sequence equal to zero for every period except for the period in which they exit SNAP, their last period in the sample.

Following Hoynes (2000), our preferred specification includes county fixed effects and county time trends, so that x_{ict}'s in equation 1 becomes:

$$x_{ict} = Z'_{it}\beta + \phi Labor_{ct} + \alpha_0 County_c + \alpha_1 Time_t + \alpha_2 County_c \cdot Trend_t. \quad (3)$$

The variables of interest are those in the vector $Labor_{ct}$, which reflect alternative measures of time-varying, county-level, and industry-specific labor market conditions, conditional on log county labor force. We ensure that ϕ is identified by non-linearly trending within-county variation by including the vectors $County_c$, which represent county fixed effects (time-invariant unobserved county characteristics), and $County_c \cdot Trend_t$, which captures county time trends. The vector $Time_t$ includes year fixed effects.[16] In Appendix Table A4, we present results from base models that relax our identification assumptions; namely, we estimate models that do not include the county fixed effects or time trends as well as models that include county fixed effects but not time trends. Standard errors are clustered at the person-level.[17]

Our preferred specification estimates the effect of county- and industry-specific fluctuations in employment on exit hazard while controlling for monthly county-wide average wage levels.[18] In all our models, we focus on industries that are likely to be important for SNAP recipients: Retail, Food Service, Manufacturing, and Construction. The Retail and Food Services industries ranked first and third in terms of the shares of SNAP participants employed in New York during 2007, the first year of our data.[19] The Medical Services industry ranked second, but we chose not to include it because of its high mean income and wide income dispersion, signaling relatively high occupational heterogeneity within the industry. We observed similar characteristics in other industries employing high shares of SNAP participants, such as Professional Services and Education. The Manufacturing and Construction industries employed the sixth and ninth highest shares of SNAP participants.

Our parameters of interest, ϕ, are identified by within-county and across-industry variation in employment that differences out county-specific trends in the labor market. Figure 3 provides a sense of the degree of variation over the sample period in county- and industry-level labor market conditions. We plot QCEW employment levels for each of the four industries relevant to SNAP participants as well as three additional industries that employed small shares of SNAP participants: Information Services, Finance, and Utilities. We show plots for total employment over all of New York State in the top-left

[16]We also estimate specifications that include seasonal controls as a robustness check, however these controls did not influence our main results.

[17]We also estimate equation 3 clustering standard errors at the county-level, however our results were not meaningfully affected. Results are available from the authors upon request.

[18]In a second approach, we focus on the effects of county- and industry-specific fluctuations in *wages* while controlling for county employment levels for that month. Results from these "wage effect" specifications are similar to those of our preferred "employment effect" specifications, although the magnitudes of the point estimates are smaller and somewhat less precise. This may simply be due in part to wages displaying less variation than employment. Wages are measured quarterly (not monthly, like employment) and are generally slower to adjust than employment. Results are available from the authors upon request.

[19]Authors' calculations from the 2007 ACS linked to 2007 New York SNAP Administrative Records.

panel of Figure 3 and for three counties that exemplify the types of variation across the state. The top-right panel shows trends for Kings County, which includes Manhattan; the bottom-left shows Erie County, where Buffalo is located; and the bottom-right panel shows Yates County, which has the third-smallest population in the state.[20]

Figure 3
Employment Trends, Selected Industries and Counties

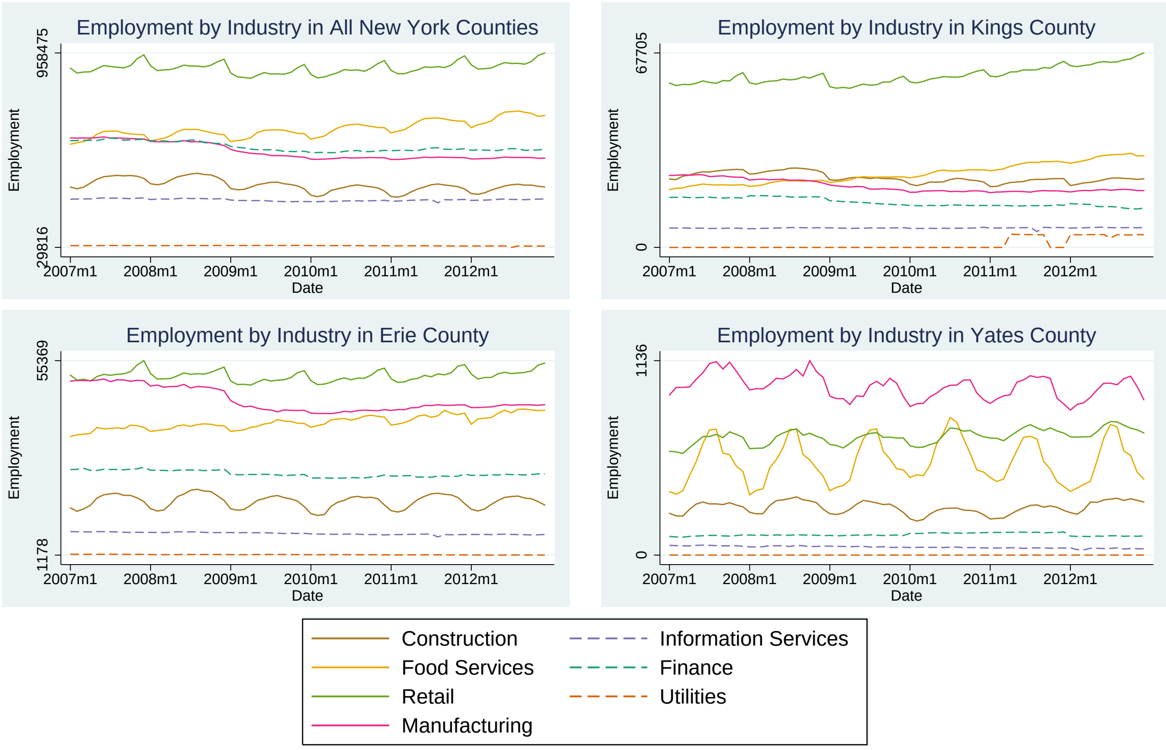


Figure 3 demonstrates that within a given county, and also at the state-level, meaningful differences in employment trends exist across industries. For example, employment levels in Manufacturing tend to be quite stable—probably owning to the high ratio of capital to labor—but the Retail, Food, and Construction industries each display strong seasonal patterns in employment. However, while Retail typically peaks in the fourth quarter, Construction and Food Services typically peak in the third quarter. And although Construction employment has a gradual increase and decline with a rounder peak, Food Services employment tends to feature rapid changes and sharp peaks. It is worth noting that there is also substantial within-industry variation in employment trends across counties for each of the four SNAP-intensive industries. On the other hand, the industries that employ smaller shares of SNAP participants—Finance, Information Services, and Utilities—all display remarkably stable employment levels, underscoring the economic volatility faced by many of the people who qualify for SNAP benefits throughout the year.

The vector Z includes covariates for recipient age, age squared, gender, race and Hispanic origin, several variables characterizing SNAP unit composition (presence of children under 5, presence of children 5–17, presence of elderly members, presence of non-elderly members, and presence of non- elderly adults), an indicator for monthly inter-county mobility, and an indicator for ever changing county of residence during our

[20]Note that the scales of the y-axes are not uniform.

observation period. In all models we control for the natural log of the monthly SNAP benefit amount, and receipt of TANF and state general assistance benefits by members of the unit.[21] To capture other time- varying environmental factors that may affect the SNAP exit probability, we also include year indicators, and we model the baseline hazard, γ_{it}, as the number of months on SNAP, plus its square and its cube. Finally, we control for the natural log of the county's population size and the urban-rural status of the county.

Without adequate pre-sample information on recipients, we are not able to address left-censoring by modeling initial conditions (Wooldridge, 2010). We therefore follow much of the literature and eliminate left-censored spells from the main analysis sample and analyze them separately. In doing so, we are likely disproportionately eliminating longer than average spells. And as previously noted, we also eliminate one-month spells.

A substantial proportion of our sample (about 25 percent) had more than one spell of SNAP participation during the period of observation. We follow much of the literature on hazard modeling and restrict our estimation sample to first spells of participation (Singer and Willett, 2003). We do, however, extend our analysis by estimating our main model over second spells and find that the relationship between local labor market conditions and hazard of program exit for those in their second spell is very similar to those in their first spell. Future work will seek to incorporate higher order spells of participation into the analysis, including models that allow for individual unobserved heterogeneity to be correlated across spells for a given recipient.[22]

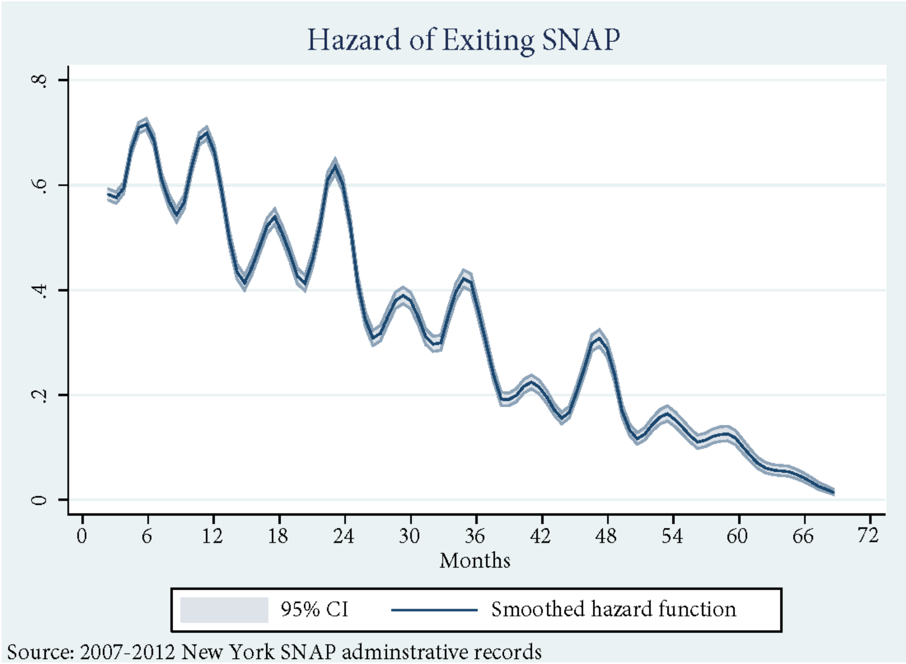
5 Results

In figure 4, we plot the unconditional hazard of SNAP exit. Two features should be noted in this figure. One is that the hazard of exit is decreasing with spell duration, suggesting negative duration dependence. Negative duration dependence implies that the longer someone is on SNAP, the less likely they are to exit. It is important to note, however, that we are not controlling for unobserved heterogeneity. And given the sparse individual-level characteristics in our model, neglecting this unobserved heterogeneity may well be important, despite not imposing any assumptions on the form of the duration dependence. By ignoring unobserved heterogeneity we may confounding the hazards of two (or more) very heterogeneous groups of SNAP participants: one group we might term "fast exiters," and another, "slow exiters." That is, we might have one group that is on SNAP for a short spell in the face of some negative shock. They contribute to the high exit rates we observed for early durations. At longer durations, however, as more of these fast exiters leave the program, only slow exiters will remain in the at-risk pool. Disability may be one source of unobserved heterogeneity that could lead to such a scenario. In the results that follow, we control for several person and case-level characteristics to attain estimates of conditional

[21] Note that our administrative records do not contain information on income. However, conditional on case unit size, benefit amount is largely a function of the unit's income (in some cases it is also a function of medical expenses, shelter costs and other deductions), so controlling for benefit amount provides a proxy for case unit income.

[22] Attempts to estimate single spell models that incorporated unobserved individual heterogeneity as a discrete mass point distribution (with two mass points) had difficulty converging. As noted in the text, it is often argued that flexibly controlling for the baseline hazard function ameliorates much of the bias that can arise from ignoring individual unobserved heterogeneity in discrete-time duration models. And in a similar study using AFDC/TANF administrative records from California, Hoynes (2000) reports that specifications that accounted for individual unobserved heterogeneity did not appreciably alter her results.

Figure 4
SNAP Exit Model: Unconditional Hazard



discrete time hazard models.

5.1 Main Results

Table 2 displays the results of the discrete time hazard model described by equation 3. The sample includes first (i.e., non-left-censored) spells only. We look first at the effect of overall county employment in column 1. Estimates from a fully-saturated model appear in column 2. Columns 3–6 focus in turn on employment in construction, the food industry, manufacturing, and retail. All regressions include the demographic controls (age, sex, race, and Hispanic origin) and case-composition controls listed in Section 4, year effects, duration controls (spell duration, its square, and its cube), county characteristics, county fixed effects, and county time trends. In addition, we include an indicator for changing county of residence during the observation month to reduce potential bias that may arise from individuals endogenously moving to areas with more favorable labor conditions. We also include an indicator for ever changing county of residence during the entire observation period in order to account for potential differences in unobserved characteristics of those who relocate relative to those who do not. We present estimates of our parameters of interest, ϕ, in Table 2; full regression results are provided in the appendix.

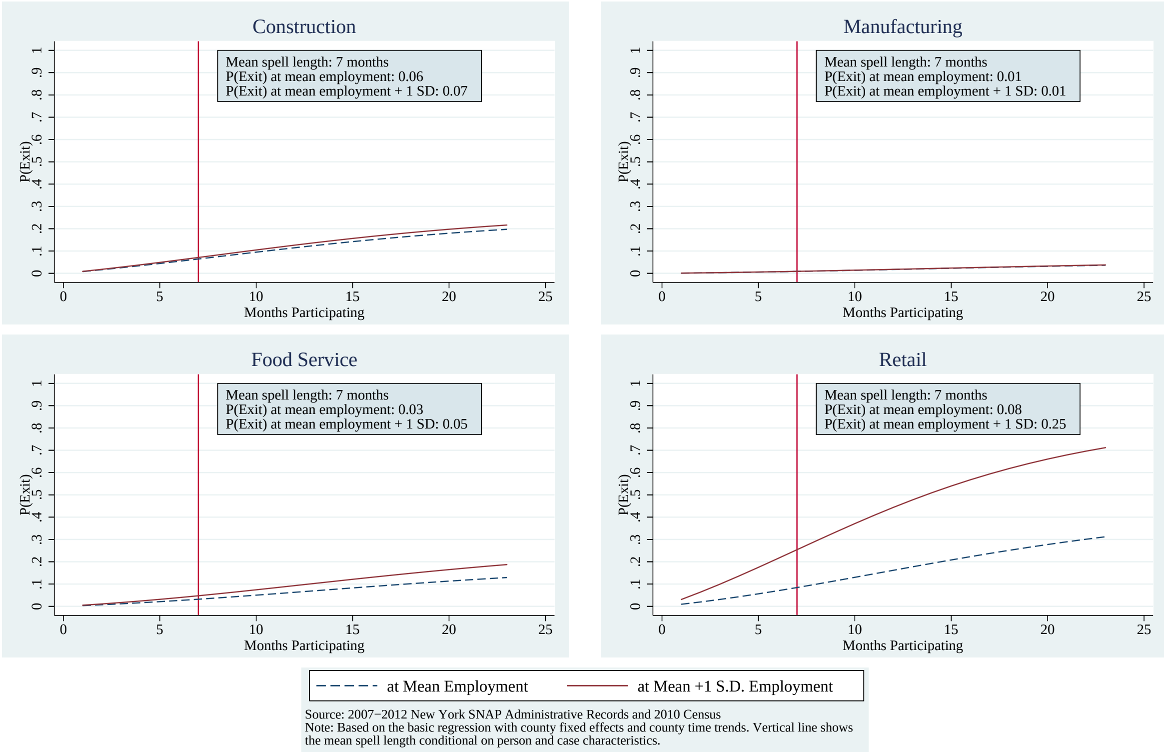
Table 2 shows positive and statistically significant relationships between the SNAP exit hazard and several of employment measures included in the regressions. The results in column 1 indicate that, at the mean, a one percent increase in a county's overall employment level is associated with a roughly fourfold increase in the likelihood of exiting SNAP. Columns 2–6, however, show that employment growth in a given county has substantially heterogeneous estimated effects across industries. The full model in column 2 simultaneously estimates four industry-specific effects. All the coefficients are, as expected, positive, but only the coefficients on construction and retail employment are statistically different from zero (recall that when interpreting hazard ratios, as we are here, the null effect is at unity rather than at zero). Employment in the manufacturing sector has essentially a null effect; a one percent increase in construction sector employment raises the probability of program exit seven percent, an effect that is marginally statistically significant. The coefficient on employment in the food service industry is moderately sized (1.29), but not precisely measured. Retail sector employment has the largest effect, with a hazard ratio of 2.82, and is significant at the one percent level.

Multicollinearity may be a concern when all four industry measures are included in the model. We therefore estimate alternative specifications in which employment in each industry enters the model separately in columns 3–6 of Table 2. Column 3 shows point estimates on employment in the construction industry that are very similar to those shown in column 2. The most notable result in columns 3–6 is the increase in the magnitude and significance of the coefficients on employment in food services and retail. For example, column 4 indicates that a 1 percent increase at the mean in food services employment is associated with a 52 percent increase in the hazard of exiting SNAP. As in the full model, the most pronounced effect is for retail employment. Column 6 suggests a one percent increase at the mean in retail employment in a person's local labor market more than triples the hazard of exiting SNAP.

In order to visualize these results, Figure 5 uses the estimates from Table 2, columns 3–6, to simulate how a given increase in industry employment would change the mean probability of SNAP exit. Specifically, we simulated, for each of the four industries, a one standard deviation increase in industry employment from the mean. The horizontal axis captures the number of months of SNAP participation (i.e., spell length), while the vertical axis measures the cumulative probability of exit in each month of the spell (i.e., the cumulative hazard function). The vertical line in each graph denotes the overall mean spell length in New York. The most striking feature of this figure is the rapid and wide divergence between the cumulative hazard functions in the retail sector. By the mean spell length of seven months, a one standard deviation increase from mean retail employment raises the exit hazard from eight to 25 percent. For construction and manufacturing, the divergence is negligible. In the food service sector, a one standard deviation increase in employment raises the cumulative hazard two percentage points, from three to five percent, by month seven of a SNAP spell.

As a whole, Table 2 and Figure 5 suggest that even in the presence of controls for county fixed effects and time trends, local labor market conditions—proxied by employment levels—are positively related to the likelihood that SNAP participants exit the program. SNAP exits were very responsive to changes in total county employment (controlling for the county population and county labor force), however looking only at total county employment masks substantial heterogeneity in estimated effects across industries. Among the industries considered in this study, SNAP exits appear to be most sensitive to changes in the strength of the retail sector, although the food service sector also shows strong (albeit somewhat imprecise) employment effects. Finally, we see some evidence that fluctuations in construction employment levels are modestly but precisely associated with exiting the program. In the follow section, we extend the results in Table 2 and also conduct several

Figure 5
Predicted Probability of SNAP Exit at Mean Industry Employment and Mean+1 S.D.



robustness checks.

5.2 Robustness checks

In Table 3, we estimate several alternative specifications of our basic hazard model in order to test the robustness of the results in Table 2. These robustness checks include accounting for unobserved individual heterogeneity, endogenous mobility, composition effects based on New York City residence, composition effects based on differences in cohort characteristics due to the Great Recession, potential endogeneity of labor market measures, and higher order spells. We also include a "placebo test" to verify that employment growth in industries with small shares of SNAP participants has no discernable effect on SNAP exit. Each coefficient in Table 3 comes from a different regression where industry employment variables appear one at a time, as in Table 2.

5.2.1 Unobserved Individual Heterogeneity

A shortcoming of the basic discrete-time hazard model represented in equation 3 is that it does not account for unobserved individual heterogeneity(Heckman and Singer, 1984). Unlike in linear models, ignoring individual heterogeneity in a (non-linear) hazard model can lead to biased estimates, even if the unobserved heterogeneity is uncorrelated with the independent variables in the model (Abbring and van den Berg, 2003; Gaure et al., 2007; Heckman and Singer, 1984; Meyer, 1991; Nicoletti and Rondinneli, 2010). Moreover, the potential bias introduced by ignoring unobserved heterogeneity is not limited to estimates of the duration dependence parameter. Monte Carlo evidence shows it can also substantially affect estimates of the other explanatory variables in the model(Nicoletti and

Rondinneli, 2010).[23] Despite these pitfalls, few studies of SNAP dynamics have considered the implications of different assumptions on unobserved heterogeneity. We incorporate unobserved individual heterogeneity into the basic model by introducing a random term, η:

$$x_{ict} = X'\beta + \phi Labor_{ct} + \alpha_0 County_c + \alpha_1 Time_t + \alpha_2 County_c \cdot Trend_t + \eta_i. \tag{4}$$

We assume η follows a Gaussian, or normal, distribution.[24] Underlying this approach is the strong assumption that the unobserved individual heterogeneity is not correlated with any of the other covariates in x_{ict}.

The results from this model appear in column 1 of Table 3. Although the estimated coefficients on food services and retail—the industries with the largest estimated effects in the base model—become even larger (and more precise) when Gaussian unobserved individual heterogeneity is introduced, the results are quite similar to those appearing in Table 2.

5.2.2 Endogenous mobility

As shown in Table 1, 7 percent of our sample relocated to a different county during the observation period, and all of the models up to this point include an indicator for changing county of residence in that month as well as an indicator for ever changing county of residence during our observation period. Nevertheless, one may still be concerned that SNAP recipients' relocation decisions are related to other unobservable characteristics that affect their labor market outcomes. This may occur, for instance, if more motivated or relatively high-skilled recipients are more apt to relocate to counties with favorable labor market conditions.[25] Alternatively, SNAP recipients who are not as employable may be more inclined to move to counties with a lower cost of living and potentially poorer labor market conditions. Under this scenario, our estimates of ϕ will combine the effect of local labor market conditions with the effect of endogenous relocation decisions.

To rule out the possibility that our main results in Table 2 are driven by this type of endogenous mobility, we estimate equation 3 over the subsample of individuals who are never observed to change counties. Column 2 of Table 3 shows the results of this exercise. The point estimates on the log employment variables are remarkably similar to those in Table 2. As in column 1, the magnitude of the coefficients on food service and retail employment increase slightly and become more precise. In general, these results suggest endogenous mobility is not a factor that drives our main findings.

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[23] Wooldridge (2010) and Meyer (1991) indicate that individual unobserved heterogeneity may be less of a concern in models that control flexibly for duration dependence. Although we do not control in a completely flexible manner for duration dependence (i.e., a monthly step function in spell length), our cubic function in spell length does avoid strong parametric assumptions).

[24] Other functional forms for η are also possible. Common choices include gamma distribution or a more flexible discrete mass point distribution. We attempted to estimate models with alternate functional forms of η, however, these models would not converge.

[25] This may be a particular concern since we are not able to control for typical measures of individual skill such educational attainment.

5.2.3 New York City

New York City comprised over 42 percent of the state population in 2010.[26] Indeed, Table 1 showed that 76 percent of our sample lived in a county that is part of a metropolitan area. To verify that our main results hold for residents of counties that are not part of New York City, we obtain estimates that exclude residents of Bronx, Kings, New York, Queens, and Richmond Counties from the sample. Column 3 of Table 3 shows the results of this exercise.

Despite the reduction in observations (person-months) from just over 431,700 in Table 2 to just over 191,760, our main findings do not change appreciably when analysis is restricted to counties outside of New York City. In fact, the point estimate on construction is essentially unchanged, and the point estimate on food service is larger and more precise when New York City residents are excluded from the sample. The point estimate on retail employment is somewhat smaller in magnitude and less precise, at the five percent significance level. We conclude that our main results in Table 2 hold throughout the state, and are not specific to the particular economic climate in New York City.

5.2.4 Cohort Effects

Individuals who enrolled in SNAP prior to the Great Recession likely differed on average from those who entered the program during or even after the recession.[27] The Great Recession brought many first-time recipients into the program who had stronger attachment to the labor force than the typical pre-recession recipient Moffitt (2013). Indeed, Appendix Table A2 shows a compositional shift over our observation period toward a greater share of urban-dwellers, non-Hispanic white individuals, males, and persons with more stable residential histories.[28] To address potential compositional differences between the pre-recession entry cohort and other entry cohorts we add an indicator for appearing in the data after the beginning of the recession and interact that indicator with the log employment measures for each of the industries. Column 4 of Table 3 shows the main estimated effects on the log employment measures; allowing log employment effects to vary by the pre- and post-recession entry cohorts has little impact on our point estimates interms of magnitude or precision. We conclude that, despite the measureable differences in the types of people whose first observable SNAP spells began after the onset of the Great Recession, the estimated relationship between localized employment growth in relevant industries and SNAP exit is not influenced by Recession-induced changes to the SNAP caseload composition.

5.2.5 Lagged labor market effects

As another sensitivity check, we re-estimate the full model in Table 2 but also include additional controls for one-period lagged measures of log employment for each of the industries. This is meant to address concerns over the potential endogeneity of the labor market variables as well as the timing of individuals' behavioral response to changes in

[26] Authors' calculation based on the 2010 Census Demographic Profile (U.S. Census Bureau, 2016c,b).

[27] At least on the national level, the job market was slow to recover even after the official end of the recession.

[28] The reduction in those for whom we observe inter-county mobility may due either to true changes in the types of people entering the program, or it may simply be a function of the fact that we observe later entrants for shorter periods of time.

local labor market conditions. It is possible, for example, that changes in the aggregate rate of exit from SNAP in a given month might well have an independent effect on county-level employment and wages in common destination industries for SNAP leavers. If there is substantial feedback from SNAP exit rates in a given month to employment in these industries, we would expect that the models in Table 2 would overstate the (positive) effect of industry-specific employment on the hazard of SNAP exit. Moreover, it may be that, for administrative and other reasons, labor demand conditions operate with a lag on the decision of individuals to exit SNAP, so that the behavioral response of SNAP recipients is better captured by a one-period lag of local labor market variables.

Including one-period lagged measures along with the contemporaneous labor market measures produces results broadly similar to our primary specification, and very much in line with the results of other specification checks in columns 1–3. The coefficients on construction and retail are attenuated relative to our main results, but the estimated effects of employment growth in food services and retail services remain relatively large and statistically significant.

5.2.6 Second spells

As we noted in Section 3, roughly 30 percent of adult recipients in New York experienced more than one SNAP spell between 2007 and 2012, even when smoothing over one-month gaps in participation. The models in Table 2, however, were estimated only for first spells observed during the sample period. Ignoring higher order spells may introduce bias to our estimates. To address this, we estimate separate models for each spell number, allowing all covariates in the model to vary by spell number and show results for second spells in column 6. Unsurprisingly, some precision is lost when estimating the model on the smaller sample. However, the point estimates remain remarkably stable, especially considering that this population of SNAP "recidivists" is likely to be quite different from population included in the analysis of first spells only.

5.2.7 "Placebo" Test

Lastly, we examine the possibility that employment trends in the industries we have selected are simply following overall county trends that could be captured just as well by other industries that are not major employers of SNAP recipients. We therefore run a "placebo test" in which we substitute the previous set of industries that have high shares of SNAP participants with three industries that have relatively low shares of SNAP recipients. These industries are finance, information, and utilities. Results, appearing in column 7 of Table 3, show small and statistically insignificant estimates for each of the industries considered. This suggests employment trends in at least some of the important destination industries for SNAP recipients are capturing variation that is different from industries that employ relatively few SNAP recipients.

5.3 Extensions

5.3.1 Race and Hispanic origin

Next, we interact race and Hispanic origin indicators with each of the county- and industry-specific measures of employment and wages in order to examine how the main results in Table 2 differ across demographic groups. The omitted category is non-Hispanic White alone, and we obtain estimates for non-Hispanic Black or African American alone, Hispanic, and all other non-Hispanic races. Results appear in Table 4. In general, we find that labor market effects on SNAP exit vary substantially across race and ethnic groups.

Table 4 shows that the estimated effects in Table 2 are largely driven by non-Hispanic White alone participants' responsiveness to their local labor market conditions. For example, we saw in Table 2 column 4 that a one percent increase in food service employment is associated with a 52 percent overall conditional increase in the likelihood of exiting SNAP. In Table 4 column 2, the main industry effect is 1.54 (significant at the 1 percent error level), implying non-Hispanic White alone participants (the omitted category) experience a 54 percent increase in exit hazard for a 1 percent increase in food service employment. The estimated coefficient for the interaction term between food service employment and SNAP participants who identify as Hispanic is 0.95 (significant at the 0.01 percent level). This means Hispanic SNAP particpants see an increased hazard of SNAP exit when local food service employment grows, but the increase in exit hazard is about 5 percent less than it is for non-Hispanic White alone.[29] Non-Hispanic Black alone participants exhibit no statistical difference relative to their non-Hispanic White counterparts in the relationship between local food service employment levels and the hazard of exiting the program. A similar pattern is observed for manufacturing employment growth. The estimated effects of growth in local retail employment on SNAP exit in column 4 is slightly different. Here, those who report non-Hispanic Black alone display an even larger increase in the likelihood of exiting SNAP, relative to those who report non-Hispanic White alone, when there is local growth in retail employment. We observe no statistical difference in the sensitivity of SNAP exit to fluctuations in retail employment, though, between Hispanic and non-Hispanic White alone SNAP participants.

5.3.2 Gender and the Presence of Children

It is well-established that labor market experiences and patterns of anti-poverty program participation differ along dimensions of gender and parenthood. In Table 5, we examine whether estimated industry employment effects vary by the gender of the SNAP recipient and by the presence of children in the SNAP unit. The positive and significant coefficients (save for manufacturing) on the main industry effects in columns 1–4 show that SNAP exit is more sensitive to local labor market fluctuations among males without children (the omitted category) than males with children as well as females with children. For example, females with children experience an increase in SNAP exit hazard that is 6 percent less than the reference group when employment grows in their local food service industry (significant at the 5 percent level).

5.4 SNAP Entries

Our focus thus far has been exclusively on the likelihood that existing SNAP recipients leave the program. It is important to verify that fluctuations in entry rates are also consistent with our findings that SNAP participants are sensitive to highly localized and

[29]When coefficients on interaction terms are expressed in odds (or hazard) ratios, they can be interpreted in multiplicative terms, relative to the main effect (Buis, 2010).

industry-specific fluctuations in their labor markets. Our approach is straightforward. We estimate OLS regressions of county log entry rates for a given month, in total and by demographic group, as a function log industry employment. We control for log population, log labor force, year effects, county effects, and county time trends. Standard errors are cluster at the county-level.

The results appear in Table 6 and suggest that food service employment, in particular, is associated with a decline in SNAP entries for the sample as a whole as well as for the female and non-Hispanic White alone subsamples. The non- Hispanic Black alone and Hispanic subsamples also exhibited negative coefficients but these were smaller and insignificant. An increase in retail employment is also associated with a significant decrease in SNAP entries for the sample as a whole and for the non-Hispanic White alone subpopulation. We conclude that the patterns of entry rates are broadly consistent with our findings on exits.

6 Conclusion

This study offers an investigation of an issue of central importance for SNAP policy and administration: how do labor market conditions affect the probability that recipients leave the program? The key contribution has been to focus on more granular measures of local labor market conditions than has previously been done in studies of SNAP dynamics, looking at both the county and the industry level. We find that, even when including county fixed effects and county time trends, increases in employment in certain industries important to SNAP recipients significantly increase the probability that SNAP recipients exit the program. In particular, the retail and food service industries showed robust, positive effects on the exit hazard for SNAP recipients. Manufacturing and, in some specifications, construction had more modest positive effects on the likelihood of exit.

Our main findings are conditional on several person, case unit, and local area controls, and they are robust to a series of sensitivity analyses. Although our main analysis is based on first spells and contemporaneous measures of county-level industry-specific labor market variables, we find that the results hold when looking at second spells and when using lagged measures of the local labor market variables. We also rule out the possibility that our estimates are driven by endogenous mobility or unobserved characteristics specific to the residents and labor markets of New York City. Finally, we leverage the demographic information afforded by linking the administrative data to the 2010 Census to show that employment and wage fluctuations in participants' local industries have differential effects across demographic groups. Non-Hispanic White alone participants, especially, benefited from employment growth across the local industries highlighted in this study. Growth in retail employment, however, significantly increased program exit probabilities for non-Hispanic Black alone participants.

Our findings point to the important role of local and industry-specific labor demand factors on the duration of SNAP participation. It is notable that we found strong effects for these variables without controlling for many of those individual, or household, characteristics, such as health issues and disability, that would likely signal a need for long-term SNAP receipt irrespective of local labor demand. Looking too broadly at labor demand—at the national or even state level—may provide a misleading picture about the strength of the link between labor demand and SNAP, especially in time of uneven economic recovery. Policies that ignore this local link and seek to reduce the regulatory flexibility currently given to states to extend eligibility in areas where labor markets are slower to recover (or faster to decline) may have undesirable consequences.

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Table 1
Sample Descriptive Statistics

	Mean	SD	Min	Max
Female	0.56	0.50	0.00	1.00
Age	36.83	12.41	18.00	64.00
White Alone non-Hispanic	0.40	0.49	0.00	1.00
Black Alone non-Hispanic	0.25	0.43	0.00	1.00
Other non-Hispanic	0.08	0.27	0.00	1.00
Hispanic	0.28	0.45	0.00	1.00
Number of members in case	2.50	1.80	1.00	23.00
Presence of children under 5	0.25	0.43	0.00	1.00
Presence of children 5–17	0.22	0.42	0.00	1.00
Presence of elderly members	0.04	0.20	0.00	1.00
Log benefit amount	5.55	0.83	0.69	8.63
TANF receipt	0.02	0.15	0.00	1.00
Other public assistance	0.04	0.18	0.00	1.00
Metro area	0.76	0.43	0.00	1.00
Ever changed county	0.07	0.26	0.00	1.00
Individuals		22,304		

Source: 2007–2012 New York SNAP Administrative Records linked to 2010 Census, linked to BLS Local Area Unemployment Statistics and Census Quarterly Census of Employment and Wages (QCEW). 2 percent sample.

Table 2
Exponentiated Discrete-Time Hazard Ratios of SNAP Exit, First Spells

	(1)	(2)	(3)	(4)	(5)	(6)
Log County Employment	4.17**					
	(1.86)					
Log Industry Employment:						
Construction		1.07*	1.08**			
		(0.03)	(0.03)			
Food		1.29		1.52**		
		(0.19)		(0.21)		
Manufacturing		1.02			1.02	
		(0.01)			(0.01)	
Retail		2.82**				3.47***
		(0.91)				(1.05)
Log-Likelihood	-59,059.23	-59,049.78	-59,059.99	-59,060.04	-59,063.28	-59,056.22
Observations	431,704	431,704	431,704	431,704	431,704	431,704

Source: 2007–2012 New York SNAP Administrative Records, 2 percent sample, linked to BLS Local Area Unemployment Statistics and Census Quarterly Census of Employment and Wages (QCEW). First spells only.
Notes: Mean wages and mean industry wages are measured weekly and in $100s. Exponentiated coefficients are shown. Standard errors are clustered at the person-level and in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3
Robustness of Main Estimates

	(1) Unobserved Heterogeneity	(2) Endogenous Mobility	(3) NYC Effect	(4) Cohort Effect	(5) Lagged Employment	(6) Second Spells	(7) "Placebo Test"
Log Industry Employment:							
Construction	1.07**	1.07**	1.07*	1.17***	1.03	1.04	
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.06)	
Food	1.53**	1.57**	2.05***	1.61***	1.51**	1.43	
	(0.22)	(0.23)	(0.38)	(0.23)	(0.21)	(0.31)	
Manufacturing	1.01**	1.02	1.50	1.04	1.01	1.04**	
	(0.01)	(0.01)	(0.57)	(0.02)	(0.01)	(0.02)	
Retail	4.24***	3.35***	3.11*	3.30***	2.89***	1.77	
	(1.34)	(1.06)	(1.45)	(1.01)	(0.88)	(0.91)	
Finance							1.24
							(0.24)
Information							1.05
							(0.11)
Utilities							1.00
							(0.01)
Observations	431,704	394,674	191,761	431,704	398,897	164,884	423,258

Source: 2007–2012 New York SNAP Administrative Records, 2 percent sample, linked to BLS Local Area Unemployment Statistics and Census Quarterly Census of Employment and Wages (QCEW). First spells only in Columns (1)-(4). Second spells only in Column (5).

Notes: **Each coefficient is from a different regression where industry employment variables appear one at a time as in Table 2, so each column represents four separate regressions.** Column (1) adds Gaussian random effects to the models in Table 2. Column (2) drops any indiviudals who changed counties during the observation period. Column (3) drops all residents of New York City. Column (4) adds an indicator for appearing in the data after the start of the Great Recession and interacts that indicator with each of the log employment variables (only the main effects are shown). Column (5) adds one-period lags of industry employment measures (only the contemporaneous measures are shown). Column (6) estimates exit hazard using individuals in their second spells rather than their first spells. Column (7) shows results for industries with small shares of SNAP participants. Exponentiated coefficients are shown. Standard errors are clustered at the person-level and in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A Additional Tables

Table 4
Hazard Model of SNAP Exit with County Fixed Effects and Time Trends, by Race, First Spells

	(1)	(2)	(3)	(4)
Log Industry Employment:				
Construction	1.08**			
	(0.03)			
× Black Alone	1.06*			
	(0.03)			
× Hispanic	0.98			
	(0.02)			
× Other	0.88***			
	(0.03)			
Food		1.54**		
		(0.22)		
× Black Alone		1.05		
		(0.03)		
× Hispanic		0.95*		
		(0.02)		
× Other		0.85***		
		(0.03)		
Manufacturing			1.04*	
			(0.02)	
× Black Alone			1.00	
			(0.01)	
× Hispanic			0.97*	
			(0.01)	
× Other			0.95**	
			(0.02)	
Retail				3.45***
				(1.04)
× Black Alone				1.10***
				(0.03)
× Hispanic				0.97
				(0.03)
× Other				0.84***
				(0.03)
County Mean Wages	0.99	0.99	0.99	0.98*
	(0.01)	(0.01)	(0.01)	(0.01)
Log County Labor Force	4.83*	1.78	5.79*	3.09
Log-Likelihood	-59043.40	-59038.93	-59056.98	-59033.28
Observations	431,704	431,704	431,704	431,704

Source: 2007–2012 New York SNAP Administrative Records, 2 percent sample, linked to BLS Local Area Unemployment Statistics and Census Quarterly Census of Employment and Wages (QCEW).
Notes: Mean wages are measured weekly and in \$100s. Exponentiated coefficients are shown. Standard errors are clustered at the person-level and in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5
Hazard Model of SNAP Exit with County Fixed Effects and Time Trends, by Sex and Presence of Children Under 5, First Spells

	(1)	(2)	(3)	(4)
Log Industry Employment:				
Construction	1.08**			
	(0.03)			
× Female	1.01			
	(0.02)			
× Children	0.98			
	(0.02)			
× Female × Children	0.98			
	(0.03)			
Food		1.52**		
		(0.21)		
× Female		1.02		
		(0.02)		
× Children		1.00		
		(0.03)		
× Female × Children		0.94*		
		(0.03)		
Manufacturing			1.02	
			(0.01)	
× Female			0.99	
			(0.01)	
× Children			1.01	
			(0.02)	
× Female × Children			0.99	
			(0.02)	
Retail				3.45***
				(1.04)
× Female				1.02
				(0.02)
× Children				0.99
				(0.03)
× Female × Children				0.95
				(0.03)
County Mean Wages	0.99	0.99	0.99	0.98*
	(0.01)	(0.01)	(0.01)	(0.01)
Log County Labor Force	4.76*	1.78	5.78*	3.05
	(3.60)	(1.52)	(4.36)	(2.35)
Log-Likelihood	-59052.49	-59050.60	-59057.34	-59047.53
Observations	431,704	431,704	431,704	431,704

Source: 2007–2012 New York SNAP Administrative Records, 2 percent sample, linked to BLS Local Area Unemployment Statistics and Census Quarterly Census of Employment and Wages (QCEW).
Notes: Mean wages are in \$100s. Exponentiated coefficients are shown. Standard errors are clustered at the person-level and in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6
Log Entries by Demographic Group (OLS)

	(1) Total	(2) Female	(3) White	(4) Black	(5) Hispanic
Log Industry Employment:					
Construction	-0.01	-0.00	-0.03	0.01***	-0.00
	(0.01)	(0.01)	(0.02)	(0.00)	(0.00)
Food	-0.35***	-0.19***	-0.40***	-0.17	-0.09
	(0.06)	(0.05)	(0.06)	(0.10)	(0.12)
Manufacturing	-0.01	-0.02	0.01	0.00	0.01*
	(0.02)	(0.03)	(0.01)	(0.01)	(0.00)
Retail	-0.35*	-0.14	-0.37*	0.05	-0.11
	(0.14)	(0.11)	(0.17)	(0.22)	(0.27)
Observations	4,337	4,333	4,336	3,727	3,917

Source: 2007–2012 New York SNAP Administrative Records, 100 percent sample, linked to 2010 Census, BLS Local Area Unemployment Statistics, and Census Quarterly Census of Employment and Wages (QCEW).
Notes: Unit of observation is the county-month. Each cell contains the estimated coefficient from a separate OLS regression. See text for additional control variables. Standard errors are clustered at the county-level and in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A1

Matched and Non-Matched Individuals in Administrative Records and the 2010 Census

	Not Matched to 2010 Census		Matched to 2010 Census		
	Mean	SD	Mean	SD	Difference
Female	0.47	0.5	0.57	0.49	-0.10***
Age	35.58	12.34	36.84	12.29	-1.26***
Number of case members	2.03	1.56	2.49	1.8	-0.46***
Presence of children under 4	0.2	0.4	0.25	0.43	-0.05***
Presence of children 5-17	0.15	0.36	0.23	0.42	-0.08***
Presence of nonelderly	0.78	0.42	0.71	0.45	0.06***
Presence of nonelderly males	0.44	0.5	0.33	0.47	0.11***
Presence of elderly	0.04	0.19	0.04	0.2	0
Ln(Benefit Amount)	5.46	0.78	5.55	0.83	-0.10***
TANF	0.02	0.15	0.03	0.16	0
Other Public Assistance	0.06	0.24	0.04	0.2	0.02***
Metro Area	0.82	0.38	0.76	0.43	0.06***
Ln(Population)	13.76	1.12	13.54	1.24	0.21***
Ln(Labor Force)	13.02	1.12	12.81	1.23	0.21***
Ever changed county of residence	0.12	0.33	0.08	0.27	0.04***
Year 2007	0.17	0.38	0.16	0.37	0.01
Year 2008	0.17	0.38	0.18	0.38	-0.01
Year 2009	0.2	0.4	0.21	0.41	-0.02***
Year 2010	0.18	0.38	0.19	0.39	-0.01*
Year 2011	0.16	0.36	0.15	0.35	0.01*
Year 2012	0.13	0.33	0.11	0.32	0.01***
Individuals	9,553		25,161		
Match Rate	72.50%				

Source: 2007–2012 New York SNAP Administrative Records, 2 percent sample, linked to 2010 Census, BLS Local Area Unemployment Statistics, and Census Quarterly Census of Employment and Wages (QCEW).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2
Descriptive Statistics by Entry Cohort

	2007		2008		2009		2010		2011		2012	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Female	0.60	0.49	0.59	0.49	0.56	0.50	0.54	0.50	0.55	0.50	0.54	0.50
Age	35.54	11.68	36.01	12.09	36.64	12.41	36.74	12.51	37.92	12.85	39.05	12.74
White Alone non-Hispanic	0.37	0.48	0.37	0.48	0.39	0.49	0.41	0.49	0.44	0.50	0.45	0.50
Black Alone non-Hispanic	0.28	0.45	0.26	0.44	0.24	0.43	0.24	0.43	0.22	0.42	0.21	0.41
Other non-Hispanic	0.06	0.23	0.07	0.25	0.08	0.27	0.09	0.28	0.08	0.28	0.08	0.28
Hispanic	0.28	0.45	0.30	0.46	0.29	0.45	0.26	0.44	0.26	0.44	0.26	0.44
Number of members in case	2.53	1.75	2.56	1.82	2.54	1.83	2.45	1.74	2.47	1.84	2.38	1.81
Presence of children under 5	0.27	0.44	0.28	0.45	0.25	0.43	0.24	0.43	0.24	0.43	0.22	0.42
Presence of children 5–17	0.24	0.43	0.23	0.42	0.23	0.42	0.22	0.42	0.21	0.41	0.21	0.41
Presence of non-elderly members	0.72	0.45	0.72	0.45	0.72	0.45	0.72	0.45	0.71	0.46	0.70	0.46
Presence of non-elderly males	0.32	0.47	0.32	0.47	0.35	0.48	0.36	0.48	0.34	0.48	0.34	0.48
Presence of elderly members	0.01	0.10	0.02	0.16	0.04	0.19	0.05	0.21	0.07	0.26	0.08	0.27
TANF receipt	0.04	0.21	0.02	0.15	0.02	0.13	0.02	0.12	0.02	0.12	0.02	0.14
Other public assistance	0.07	0.26	0.04	0.19	0.02	0.15	0.03	0.16	0.02	0.15	0.03	0.16
Metro area	0.73	0.44	0.75	0.43	0.77	0.42	0.76	0.43	0.75	0.43	0.77	0.42
Ever changed county	0.13	0.33	0.11	0.31	0.06	0.24	0.05	0.22	0.04	0.19	0.02	0.13
Individuals	3,779		3,750		4,731		4,212		3,302		2,530	

Source: 2007–2012 New York SNAP Administrative Records linked to 2010 Census, linked to BLS Local Area Unemployment Statistics and Census Quarterly Census of Employment and Wages (QCEW). 2 percent sample.
Notes: Entry cohorts are based on the year individuals begin their first observed SNAP spells.

Table A3
Complete Estimates from Table 2, Columns 2-6

	(1)	(2)	(3)	(4)	(5)
Log of Industry Employment:					
Construction	1.07*	1.08**			
	(0.03)	(0.03)			
Food	1.29		1.52**		
	(0.19)		(0.21)		
Manufacturing	1.02			1.02	
	(0.01)			(0.01)	
Retail	2.82**				3.47***
	(0.91)				(1.05)
Female	0.87***	0.87***	0.87***	0.87***	0.87***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Black Alone non-Hispanic	1.12***	1.11***	1.11***	1.11***	1.11***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Other non-Hispanic	0.82***	0.82***	0.82***	0.82***	0.82***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Hispanic	0.99	0.99	0.99	0.99	0.99
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Age	1.03***	1.03***	1.03***	1.03***	1.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Age squared	0.94***	0.94***	0.94***	0.94***	0.94***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Presence of children under 5	0.88***	0.88***	0.88***	0.88***	0.88***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Presence of children 5–17	1.06*	1.06*	1.06*	1.06*	1.06*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Presence of non-elderly	1.07*	1.07*	1.06*	1.07*	1.07*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Presence of non-elderly males	1.09**	1.09**	1.09**	1.09**	1.09**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Presence of elderly	0.86**	0.86**	0.86**	0.86**	0.86**
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Log benefit amount	0.86***	0.86***	0.86***	0.86***	0.86***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log percent change in benefit	0.96	0.96	0.96	0.96	0.96
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
State TANF	0.32***	0.32***	0.32***	0.32***	0.32***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Other state public assistance	0.61***	0.61***	0.61***	0.61***	0.61***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Ever changed county	0.82***	0.82***	0.82***	0.82***	0.82***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Changed county this month	4.35***	4.36***	4.36***	4.36***	4.36***
	(0.37)	(0.37)	(0.37)	(0.37)	(0.37)
Log population	0.03	0.49	0.18	0.23	0.02
	(0.15)	(2.38)	(0.86)	(1.13)	(0.12)
Log county labor force	1.49	4.85*	1.78	5.84*	3.05
	(1.27)	(3.67)	(1.52)	(4.40)	(2.35)
Average weekly wages	0.99	0.99	0.99	0.99	0.98*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
year=2007	2.96e+09	0.00	5610.50	0.03	60881580.13
	(1.55e+11)	(0.00)	(291055.54)	(1.66)	(3.17e+09)
year=2008	0.75	0.74	0.79	0.72	0.73
	(0.16)	(0.15)	(0.16)	(0.15)	(0.15)
year=2009	0.65**	0.61**	0.65**	0.60***	0.63**
	(0.10)	(0.09)	(0.10)	(0.09)	(0.10)
year=2010	0.69***	0.66***	0.69***	0.65***	0.67***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
year=2011	0.95	0.94	0.95	0.92	0.94
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
t	1.12***	1.12***	1.12***	1.12***	1.12***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
t squared	0.99***	0.99***	0.99***	0.99***	0.99***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
t cubed	1.00***	1.00***	1.00***	1.00***	1.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Trends	Yes	Yes	Yes	Yes	Yes
Log-Likelihood	-59049.78	-59059.99	-59060.04	-59063.28	-59056.22
Observations	431,704	431,704	431,704	431,704	431,704

Source: 2007–2012 New York SNAP Administrative Records, 2 percent sample, linked to BLS Local Area Unemployment Statistics and Census Quarterly Census of Employment and Wages (QCEW). First spells only.
Notes: Mean wages and mean industry wages are measured weekly and in $100s. Exponentiated coefficients are shown. Standard errors are clustered at the person-level and in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4
Exit Hazard With and Without County FE and Time Trends

	(1)	(2)	(3)
Construction	1.08***	1.07**	1.07*
	(0.02)	(0.03)	(0.03)
Food	1.23***	1.17	1.29
	(0.06)	(0.16)	(0.19)
Manufacturing	1.00	1.02	1.02
	(0.01)	(0.01)	(0.01)
Retail	0.71***	2.74***	2.82**
	(0.04)	(0.78)	(0.91)
ln(County Labor Force)	1.19	2.82	1.49
	(0.18)	(1.99)	(1.27)
Average Weekly Wages (100s)	0.99*	0.98*	0.99
	(0.00)	(0.01)	(0.01)
County Fixed Effects	No	Yes	Yes
County Time Trends	No	No	Yes
Log-Likelihood	-59202.42	-59088.95	-59049.78
Observations	431,704	431,704	431,704

Source: 2007–2012 New York SNAP Administrative Records, 2 percent sample, linked to BLS Local Area Unemployment Statistics and Census Quarterly Census of Employment and Wages (QCEW). First spells only.

Notes: Mean wages are measured weekly and in $100s. Exponentiated coefficients are shown. Standard errors are clustered at the person-level and in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$