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This dissertation focuses on aspects of behavior and public policy related to vulnerable populations. The first essay, coauthored with Christian Gregory and David C. Ribar, reviews recent theory and empirical evidence regarding the effect of Supplemental Nutrition Assistance Program (SNAP) participation on food insecurity and replicates the modelling strategies used in the empirical literature. We find that recent evidence suggesting the ameliorative effect of SNAP on food insecurity may not be robust to specification choice or data. Most specifications mirror the existing literature in finding a positive association of food insecurity with SNAP participation. Two-stage least squares and control function methods do show that SNAP reduces food insecurity, but effects are not consistent across sub-populations and are not always statistically significant.

In the second essay, I examine the relationship between SNAP participation and food insecurity using data from the 2001-2008 Current Population Survey (CPS-FSS). A behavioral Rasch selection model is proposed and estimated using four subsamples of low-income households: unmarried parent households, married parent households, all-elderly households, and other adult-only households. The behavioral Rasch selection model assumes responses to multiple food hardship questions may be modelled as indicators of a single underlying index of food hardships, and concurrently, controls for the endogeneity of program participation. Simultaneously modelling the outcomes this way leads to more efficient estimation. The models are identified using exogenous changes in state-level polices related to SNAP. The results suggest that SNAP has a

strong ameliorative effect on food insecurity for married parent households, all-elderly households, and other adult-only households, while SNAP continues to be associated with greater food hardships for unmarried parent households. Participating in SNAP reduces the probability of food insecurity by 22.4% for other adult-only households, 18% for all-elderly households, and 17% for married parent households.

The third and final essay examines the relationship between underage college drinking and the initial occupational choices of male college graduates using data from the National Longitudinal Survey of Youth 1997 (NLSY97). Focusing on recent college graduates and their initial occupational choices allows me to address important timing issues not considered by the existing literature. For the multivariate analyses, I estimate multinomial logistic models of occupational choice, where the occupational choice set is specified as employed full-time in white collar occupations, other occupations, enrolled in school, and neither in school nor employed full-time. In addition, I estimate multinomial logistic selection models that control for the potential endogeneity of underage drinking. The results suggest underage college drinking is not associated with young men's initial occupational choices, with the exception of the decision to be enrolled in school. Young men with any underage college days where they drank two or more drinks are 28.9% less likely to be enrolled in school after completing a bachelor's degree.

THREE ESSAYS IN HEALTH AND

NUTRITION ECONOMICS

by

Matthew P. Rabbitt

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Approved by

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Committee Chair

To my wife, Elizabeth.

For my grandfather, Thomas Sicking.

APPROVAL PAGE

This dissertation, written by Matthew P. Rabbitt, has been approved by the following committee of the Faculty of The Graduate School at The University of North Carolina at Greensboro.

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CHAPTER I

INTRODUCTION

The three essays in this dissertation focus on aspects of behavior and public policy related to vulnerable populations. The first essay, "The Supplemental Nutrition Assistance Program and Food Insecurity," reviews the recent theory, literature, and empirical evidence regarding the effect of Supplemental Nutrition Assistance Program (SNAP) participation on food insecurity and replicates the modeling strategies used in the empirical literature. Essay two, "Measuring the Effect of Supplemental Nutrition Assistance Program Participation on Food Insecurity Using a Behavioral Rasch Selection Model," estimates the effectiveness of SNAP participation in reducing food insecurity among low-income households. The third essay, "Underage College Drinking and the Occupational Choices of Recent College Graduates," examines the relationship between underage college drinking and the initial occupational choices of college graduates.

The Supplemental Nutrition Assistance Program (SNAP, formerly the Food Stamp Program) is intended to help low-income households obtain more nutritious food than they could otherwise afford. In doing so, the SNAP should—in both a normative and a positive sense—reduce households' food hardships. However, only recently has research begun to confirm this common sense association.

Since 1995, the United States has regularly measured food hardships nationally, using the Food Security Scale, a 10- to 18-item index that is intended to capture households' "access at all times to enough food for an active, healthy life" (Coleman-Jensen et al., 2012). The latest data indicate that 85% of U.S. households were food secure in 2011, while 15% (17.9 million households with 50.1 million people) were not. More often than not, researchers find that the receipt of SNAP benefits is associated with more, rather than fewer, food hardships. Are our common-sense predictions wrong, or are there statistical problems that confound the estimates? What are the methodological and, more importantly, the policy and well-being implications of the results? In the first essay, coauthored with Christian Gregory and David C. Ribar, we review the recent theory and empirical evidence regarding the effect of SNAP participation on food security. Using data from the Current Population Survey Food Security Supplement (CPS-FSS), we estimate most of the modelling strategies used in the empirical literature.

The main finding of this study is that recent results showing that food assistance reduces food insecurity may not be robust to specification choice or data. As in other research, most of our simple models suggest a higher conditional mean of food insecurity prevalence associated with SNAP. Moreover, the results for propensity score and longitudinal models mirror those in the empirical literature in showing, quite counterintuitively, that SNAP is associated with increases in food insecurity prevalence. Two-stage least squares (2SLS) results are a bit more consistent with recent findings, although the estimated sizes of the effects are statistically insignificant. Similarly, our findings using dummy endogenous approaches yield somewhat inconsistent results, with many of the statistically significant results being for married parent households with children. Most of the results using this method yield parameter estimates with the appropriate sign, even when they are not significant. The dose-response models are consistent with previous research in that they suggest larger amounts of SNAP benefits are associated with a reduction in the likelihood of food insecurity.

In the second essay, “Measuring the Effect of Supplemental Nutrition Assistance Program Participation on Food Insecurity Using a Behavioral Rasch Selection Model,” I examine the effectiveness of SNAP in reducing food insecurity among low-income households using data from the 2001-2008 CPS-FSS. Low-income households are disaggregated into four policy relevant subsamples: unmarried parent households, married parent households, all-elderly households, and other adult-only households.

Estimation of the relationship between SNAP participation and food insecurity is complicated by the fact that selection issues may be contributing to counterintuitive findings. Households participating in SNAP are likely to differ in both observable and unobservable ways from non-participating households. However, several studies that control for selection on unobservable characteristics generate inconclusive results. These studies rely on a single binary measure of food insecurity. While the interpretation of the food insecure versus food secure comparison is straightforward and easy to implement, considerable information is being suppressed. Information is lost when broad categories are created using responses to questions from the Food Security Scale. Therefore, the use of a single binary measure is likely contributing to the generation of insignificant results.

For the multivariate analyses of food insecurity, I estimate behavioral Rasch models. The Rasch model assumes responses to the Food Security Scale questions may be modeled as indicators of a single underlying index of food hardships, such as food insecurity. Simultaneously modeling the outcomes this way leads to more efficient estimation. I modify the standard Rasch model to incorporate a behavioral component and to account for selection on contemporaneous unobservables. The models are identified using exogenous changes in state-level policies and rules related to SNAP. Instrumental variables capture information on policies related to vehicle asset rules, outreach activities, recertification intervals, and immigrant eligibility.

Descriptive analyses of my data reproduce the findings of previous studies that SNAP receipt is associated with higher rates of food insecurity. Estimates from the multivariate models, which attempt to control for selection on observable characteristics, also yield the counterintuitive result of SNAP increasing food insecurity. However, after controlling for selection on unobservable characteristics, I find a highly significant and negative relationship between SNAP receipt and food insecurity for all household subsamples, with the exception of unmarried parent households. Participating in SNAP reduces the probability of food insecurity by 17% for married parent households, 18% for all-elderly households, and 22.4% for other adult-only households. When using a single binary measure of food insecurity instead of the Rasch specification, the results are inconclusive. My findings are robust to the use of alternative program participation indicators, the choice of instrumental variables, and sample restrictions based on household income.

My third dissertation essay considers the potential disadvantage of underage college drinking. A distinguishing feature of young adulthood is the number of choices made with potentially lifelong consequences. While in college young adults make choices about their drinking behaviors, which have implications for their health, schooling, and labor market outcomes. Upon completing the requirements for a bachelor's degree, many of these young adults transition from schooling to full-time permanent jobs for the first time. While searching for employment, young adults must make critical choices about their industry and occupation. In the third essay, "Underage College Drinking and the Occupational Choices of Recent College Graduates," I examine the relationship between underage college drinking and the initial occupational choices of college graduates using data on young men from the 1997 cohort of the National Longitudinal Survey of Youth (NLSY97). I depart from the previous literature by focusing on initial occupational choices which allows me to construct a sample of young men who are facing the same occupational choices and level of education. This allows me to address important timing issues that have received little attention by the previous literature.

For the multivariate analyses, I estimate multinomial logistic (MNL) models of occupational choice, where occupational choice is specified as employed full-time in white-collar occupations, other occupations, in school, and neither in school or employed full-time. These models control for young men's demographic and background characteristics, survey design, economic characteristics, and region and year fixed-effects. In addition, I also estimate MNL selection models (Terza, 2002; Terza & Vechnak, 2011) that control for potential unobserved heterogeneity related to underage drinking.

Based on specification tests and robustness checks, my preferred specification is a MNL model that controls for selection on young men's observable characteristics but that omits special controls for unobserved characteristics. The results suggest underage college drinking, regardless of how it is measured, is not associated with the probability of being employed full-time in white-collar occupations, other occupations, or neither in school nor employed full-time. In contrast, young men with any underage college days with two or more drinks are five percentage points (29.8%) less likely to be enrolled in school after completing a bachelor's degree. This result, while large, is consistent with the findings of Dee and Evans (2003). As a result, underage drinking has important implications for young men who are seeking professional occupations. For these students, any comparative advantages they possess cannot overcome the detrimental effects of excessive underage college drinking. Since professional occupations rely heavily on human capital, the mechanism that is occurring here is likely a reduction in study effort, which translates into poor grades. College administrators and public policy makers can use the evidence presented in this analysis to target polices at the drinking behaviors of students in pre-professional programs.

CHAPTER II

THE SUPPLEMENTAL NUTRITION ASSISTANCE PROGRAM AND FOOD INSECURITY

Co-authored with Christian Gregory and David C. Ribar[1]

Abstract

This chapter reviews recent theory and empirical evidence regarding the effect of SNAP on food insecurity and replicates the modelling strategies used in the empirical literature. The authors find that recent evidence suggesting an ameliorative effect of SNAP on food insecurity may not be robust to specification choice or data. Most specifications mirror the existing literature in finding a positive association of food insecurity with SNAP participation. Two-stage least squares and control function methods do show that SNAP reduces food insecurity, but effects are not consistent across sub-populations and are not always statistically significant.

Introduction

The Supplemental Nutrition Assistance Program (SNAP, formerly the Food Stamp Program) is intended to help low-income households obtain more nutritious food than they could otherwise afford. In so doing, the SNAP should—in both a normative

[1] Earlier versions of this paper were presented at the "Five Decades of Food Stamps" research conference, September 20, 2013 in Washington, DC and at the 16th Labour Econometrics Workshop, August 10, 2013 in Melbourne, Australia. The authors thank John Pepper, Steven Stillman, and conference participants for helpful comments. The views expressed in this paper are those of the authors and not necessarily those of the ERS or USDA.

and a positive sense—reduce households' food hardships. However, only recently has research begun to confirm this common sense association.

Since 1995, the United States has regularly measured food hardships nationally, using the Food Security Scale, a 10- to 18-item index that is intended to capture households' "access at all times to enough food for an active, healthy life" (Coleman-Jensen, Nord, Andrews, & Carlson, 2012). The latest data indicate that 85% of U.S. households were food secure in 2011, while 15% (17.9 million households with 50.1 million people) were not. More often than not, researchers find that the receipt of SNAP benefits is associated with more, rather than fewer, food hardships. For example, Coleman-Jensen et al. (2012) report that among households with incomes below 130% of the poverty line (households that meet the gross income test for SNAP receipt), 52% of SNAP participants reported being food insecure compared to 28% of non-participants.

Obviously, this example demonstrates simple association, rather than causation. But it hasn't been until quite recently that any methods have begun to get results consistent with the expectation that SNAP would reduce food insecurity. Are our common-sense predictions wrong, or are there statistical problems that confound the estimates? What are the methodological and, more importantly, the policy and well-being implications of the results? This chapter reviews and synthesizes previous research on these questions and conducts new analyses using several years of data from the Food Security Supplement of the Current Population Survey (CPS-FSS).

Measuring Food Insecurity and Other Food Hardships

The principal instrument for measuring food security in the U.S. is the Food Security Module of the CPS-FSS. The module asks 10 questions of all households and an additional eight questions of households with children, regarding progressively more severe hardships that range from anxiety over food running out to shortages of amounts and kinds of food to episodes of adults and children going without food for an entire day. All of the questions refer to the previous 12 months and are framed in terms of either shortages of money or affordability. The CPS-FSS also asks 30-day questions based on the same items. The items in the 12-month module are listed in Appendix A.

The Food Security Module was developed after extensive research that began with a conceptualization of food security and insecurity and proceeded to qualitative fieldwork to elicit themes for potential items, the development of candidate items, statistical and qualitative analyses of the items' validity and reliability, a selection of items, and a final scaling (see Hamilton et al., 1997). The testing included formal Item Response Theory modelling (specifically Rasch modelling) and indicated that the items were consistent with a unidimensional underlying, or latent, measure.

Household food security status is determined by summing the affirmed responses from the module. Households that affirm two or fewer items are classified as being "food secure," meaning that they have "consistent, dependable access to enough food for active, healthy living" (Coleman-Jensen et al., 2012, p. v). Households without children that affirm three to five items and households with children that affirm three to seven items are classified as experiencing "low food security," meaning that they "reported multiple indications of food access problems, but typically . . . reported few, if any, indications of reduced food intake" (Coleman-Jensen et al., 2012, p. 4). Households that affirm more items (six or more for households without children and eight or more for households with children) are classified as experiencing "very low food security," meaning that the "food intake of one or more members was reduced and eating patterns (were) disrupted because of insufficient money and other resources for food" (Coleman-Jensen et al., 2012, p. 4). The low and very low food security categories together constitute food insecurity.

The CPS-FSS Food Security Module has some limitations that should be kept in mind. In a careful review of the food security scale, the National Academy of Sciences (Wunderlich & Norwood, 2006) identified several problems, including that the module captures other relevant food hardships, such as problems with the supply, safety, or quality of food; that the unidimensional model for developing the scale might not be appropriate; and that the CPS-FSS is based on a household sampling frame that omits institutionalized and homeless people. Also, to lower the response burden on CPS subjects and to reduce the risks of false positive indications, the module is not asked of all households in the CPS-FSS but rather only of households that are at risk of insecurity because they have incomes below 185% of the poverty line, indicated that they are food insufficient, or indicated that they undertook actions to stretch their food budget. Although the food security measure is strongly associated with households' income-to-needs ratios (see, e.g., Coleman-Jensen et al. 2012), researchers have found that it has weak external validity in terms of some nutritional outcomes (Bhattacharya, Currie, &

Haider, 2004) and food expenditures (Gundersen & Ribar, 2011) and that items may have low reliability among parents and children (Fram et al., 2011).

In addition to the 12-month, 18-item food security scale, research on the SNAP has used other measures of food hardships. One of these, the food insufficiency measure, has already been mentioned. The food insufficiency question asks households if they have, "enough of the kinds of food (they) want to eat, enough but not always the kinds of food (they) want to eat, sometimes not enough to eat, or often not enough to eat?" The CPS-FSS also follows up affirmative responses to the 12-month food security questions with questions about whether the hardships were experienced in the last 30 days; the responses from these questions are used to construct a 30-day measure of food insecurity.

The 18-item food security module has been included in other U.S. surveys, such as the Panel Study of Income Dynamics and the National Health and Nutrition Examination Survey. However, due to time and budget constraints, some other surveys either ask the single-item food sufficiency question or a subset of the food security questions. For example, recent panels of the Survey of Income and Program Participation (SIPP) have asked six food security questions covering the previous four months; a food security scale has been developed from responses to five of these questions. The National Health Interview Survey currently fields the 10-item questionnaire. In general, measures derived from the full 18-item module, the food sufficiency question, and shorter modules are highly correlated.

Conceptual Analysis

To consider the ways in which SNAP might affect food hardships, we rely on Barrett's (2002) theoretical rational-choice model of how household food security is determined.[2] Barrett extended the household production framework of Becker (1965) and Gronau (1977) and the health production framework of Grossman (1972) to include household nutrition and food security. In Barrett's model, households choose purchases, savings or borrowing, and allocations of time to further the objective of maximizing their members' physical well-being and general consumption in the present, where they have full information about their circumstances, and in the future, where they have expectations about circumstances. Households pursue these objectives subject to production, health, budget, and time constraints. Specifically, each period's physical well-being depends on the level of well-being from the previous period; inputs of nutrition, other goods or services, and activities; and arbitrary shocks from illnesses and injuries. The nutritional inputs to physical well-being, in turn, are produced using inputs of food and other goods and of members' time. Each of these production functions is also conditioned by the household members' human capital. Also, households face subsistence constraints in the form of minimum amounts of nutrition to avoid hunger and minimum amounts of physical well-being to avoid impairment. With respect to the budget constraint, households' total per-period expenditures on food, other goods, and services must not exceed the sum of the members' earnings plus the return on their

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[2] Caswell and Yaktine (2013), Gundersen and Gruber (2001), Gundersen and Oliveira (2001), Huffman and Jensen (2003), Meyerhoefer and Yang (2011), and Ribar and Hamrick (2003) also provide conceptual models.

savings and other assets plus any borrowing and less any savings. The household members also have limits on the time available each period to work or participate in other activities.

From Barrett's framework, we can identify structural characteristics of households that increase the risk of food hardships. First, hardships are more likely to occur if household members have low labor productivity (through circumstances such as disability, a lack of education, or very young or old age) that reduce their ability to work in the home and the labor market. Second, households are at greater risk for hardships if they confront adverse terms of trade in the form of either low wages for the work they perform or high prices for the goods they purchase. Third, households are also at increased risk of hardships if they lack access to labor markets or goods markets. Fourth, risks are higher for households with low levels of savings and assets and for households with limited abilities to borrow and save. Fifth, risks increase if households have weak social or public support systems. Sixth, households face higher risks of food insecurity if their circumstances frequently leave them near the subsistence or food security thresholds, as this increases the chances that a given shock will knock them below the thresholds. Seventh, a general susceptibility to negative shocks, perhaps because of marginal health, residence in an area with a volatile economy, or work in a vulnerable industry, increases the risks of becoming food insecure.

We can also use Barrett's model to consider how the SNAP should affect households' food security. In principle, the program's EBT assistance should expand participating households' budget sets and relax their resource constraints. This should allow households to purchase more food and reduce the incidence of food hardships, including food insecurity. We would also anticipate complementary effects from the educational component of SNAP, which should increase household members' shopping, planning, and food preparation skills and thereby make them more effective at transforming budgetary and other resources into nutritional inputs and physical well-being outcomes.

At the same time, other elements of SNAP participation might work against these effects. First, means-testing of SNAP eligibility and benefits imposes an extra tax on market work, reducing poor people's incentives to work and earn (or possibly incentivizing them to work "off-the-books" in less stable informal jobs). These effects might be especially strong for households with children, where the receipt of SNAP confers categorical eligibility for free meals under the National School Lunch Program (NSLP) and School Breakfast Program (SBP) and adjunctive financial eligibility for the Special Supplemental Nutrition Program for Women, Infants and Children (WIC) program. Second, program participants are vulnerable to losses of benefits if they fail to comply with program rules regarding recertification and mandated work activities (Ribar, Edelhoch, & Liu, 2008, 2010). Ribar and Edelhoch (2008) found that recertification had especially detrimental participation effects for recipients who were marginally eligible financially and for recipients in very unstable circumstances. More generally, income volatility could both increase the risks of food insecurity (Gundersen & Gruber, 2001) and affect eligibility for food assistance (e.g., Jolliffe & Ziliak, 2008). Third, monthly cycles associated with SNAP issuance, spending, and benefit exhaustion could give rise to periodic shortages of food (Wilde & Ranney, 2000). Fourth, the increased time and preparation associated with SNAP-eligible food purchases as compared to other types of food purchases might negatively affect families. Although each of these issues might reduce the effectiveness of the SNAP, we would still expect the program's net effects to be positive.

Although theory predicts a positive effect of SNAP on food security, there are many reasons why results produced from an observational empirical analysis might differ. First and foremost, participation in the SNAP is endogenous. Food security and SNAP participation are each influenced by a host of characteristics, and failure to measure or account for these characteristics in an empirical analysis can give rise to spurious associations. For example, Joyce et al. (2012) document a host of hardships, including health problems, housing insecurity, and losses of utilities, that often accompany food hardships. There is also a possibility that food hardships may prompt SNAP participation and that the empirical association may be affected by simultaneity bias. Nord and Golla (2009) examined trajectories of food hardships prior to and after entering the SNAP; they found that food hardships rose in the months leading up to SNAP entry, suggesting that increased hardships motivated entry. As we discuss in the next section, the endogeneity of SNAP participation has been a predominant methodological concern in empirical research. Finally, mismeasurement and misreporting of food hardships and of SNAP participation may alter the observed relationships.

Previous Research

A vast number of studies have investigated the impacts of the SNAP on American's food outcomes. Comprehensive reviews by Barrett (2002), Currie (2003), and Fox, Hamilton, and Lin (2004) summarize the research as consistently indicating that the SNAP is associated with higher expenditures on food and greater food and nutrient availability within households. However, Currie (2003), Fox et al. (2004), and Wilde and Nord (2005) reach much different conclusions regarding the impact of SNAP on food insecurity and insufficiency and report that the results across studies are mixed and inconsistent. A more recent review by Caswell and Yaktine (2013) is more sanguine about the studies of SNAP and food hardships, although it also acknowledges many inconclusive and counter-intuitive results. Our review will focus on the statistical methodologies that studies have employed, summarize findings associated with those methodologies, and draw interpretations regarding potential biases.[3]

Comparison of SNAP Participants and Non-Participants

Most of the research on the potential effects of the SNAP on food hardships has been based on comparisons of outcomes for program participants and non-participants. The studies generally restrict their analyses to people with incomes that are below or near the gross-income eligibility limit for the SNAP.[4] The restrictions are intended to make the samples of participants and non-participants more comparable. For studies that use the CPS-FSS, the restrictions also ensure that everyone in the samples was asked the

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[3] In addition to these reviews, Meyerhoefer and Yang (2011) have summarized research on the association of SNAP with people's body weight and health.

[4] Borjas's (2004) multivariate analysis is a notable exception.

questions in the food security module and thus avoid an artificial sample selection issue that arises from the screening conditions for the module.

Descriptive results (comparisons of means) from each year's CPS-FSS are reported by the Economic Research Service in its *Household Food Security in the United States* series (e.g., Coleman-Jensen et al., 2012). Descriptive methods were also used in early research, such as Cohen et al. (1999). The descriptive comparisons indicate that food insecurity is substantially higher in SNAP households than in other households.

Multivariate statistical models include other observed measures, such as household size, race, and education of the household head, that are likely to be associated with both food hardships and SNAP participation and that may be sources of spurious associations. Several researchers, including Alaimo, Briefel, Frongillo, and Olson (1998) and Bhattacharya and Currie (2001) estimated standard binary or continuous regression models of food hardships, and Ribar and Hamrick (2003) estimated binary event-history models of entry into and exit from these conditions. Although the use of observed controls reduced the associations of SNAP participation and food hardships in these studies, substantial positive conditional associations remained.

A few standard-regression studies have generated different findings using narrower analysis samples and alternative participation comparisons in attempts to mitigate selection issues. Kabbani and Kmeid (2006) found that SNAP participation was negatively associated with 30-day food insecurity among a low-income sample of CPS-FSS households that were food insecure according to the 12-month measure. Rather than considering general comparisons of SNAP participants and non-participants, Gundersen and Gruber (2001) and Mykerezi and Mills (2010) focused on households that had lost benefits and found that such losses raised households' risks of food insufficiency and insecurity. Mabli, Ohls, Dragoset, Castner, and Santos (2013) compared food security outcomes for SNAP households at the starts of their participation spells and six months into those spells and found that food hardships decreased with households' SNAP tenures.

Matching techniques offer a more general and robust approach to addressing selection based on observable characteristics. Gibson-Davis and Foster (2006) employed propensity-score matching (PSM; Rosenbaum & Rubin, 1983) to compare SNAP participants and non-participants. They found that matching led to lower associations between SNAP and the incidence of food insecurity than standard logistic binary regressions but that many of the associations remained significantly positive. In a few specifications, that jointly (a) considered the food insecurity Rasch score, (b) were restricted to households that affirmed at least one food security item, and (c) were limited to a narrow range of propensity scores, Gibson-Davis and Foster found the expected negative associations.

Standard regression models and matching techniques address selection based on observable variables. If we assume that the theoretical model is indeed correct, the preponderance of counter-intuitive findings from the regression and matching studies indicates that selection must be coming from unobservable characteristics or simultaneity. When longitudinal data are available, multivariate fixed-effects methods can be used to account for time-invariant unobserved characteristics that might be confounded with both SNAP participation and food hardships. Wilde and Nord (2005) estimated household-level fixed effects models using the two-year panels that can be constructed from the CPS-FSS, and Greenhalgh-Stanley and Fitzpatrick (2013) estimated fixed effects models using data on households with elderly people from the Health and Retirement Survey. Both studies found that SNAP participation continued to be positively associated with food insecurity, even after fixed-effects controls were applied. The findings suggest that time-varying unobserved influences or simultaneity are a source of bias.

Instrumental variables methods, including two-stage least squares (2SLS), endogenous latent variable models, and dummy endogenous variable models, can address these other sources of bias. 2SLS and endogenous latent variable models rely on variable exclusions for identification. For these exclusions to be valid, the excluded variables—the instruments—must be strongly predictive of SNAP participation and must only affect food hardships through their effects on SNAP participation (i.e., must not independently predict food hardships). Dummy endogenous variable models, such as bivariate probit, can be formally identified through the functional forms in the model if there is sufficient variation in the explanatory variables (Wilde 2000). In practice, however, this source of identification can be weak, and researchers typically bolster identification through variable exclusions. A challenge for endogenous variable studies has been to uncover appropriate instruments.

Results based on two-stage and latent endogenous variable methods have been inconclusive. Borjas (2004) examined the effects of public assistance (including but not limited to SNAP receipt) on food insecurity, using citizenship and years since migration as instruments. Borjas found the anticipated negative associations, but most of his estimates were only marginally significant. Gundersen and Oliveira (2001) and Huffman and Jensen (2003) applied endogenous latent variable methods but obtained imprecise and statistically insignificant results. Greenhalgh-Stanley and Fitzpatrick (2013) estimated 2SLS models for elderly households from the Health and Retirement Survey in specifications that also included household-specific fixed effects. They generated estimates that were imprecise and statistically insignificant. Shaefer and Gutierrez (2012) also estimated 2SLS models using data from three panels of the SIPP and obtained statistically insignificant results.

In contrast, researchers who have applied dummy endogenous variable models have estimated strong negative associations. Yen, Andrews, Chen, and Eastwood (2008) found that SNAP participation was negatively associated with households' 30-day food insecurity Rasch scores; however, the researchers used a choice-based sample (the 1996-7 National Food Stamp Program Survey) with an over-representation of SNAP participants.[5] Mykerezi and Mills (2010) estimated a negative association between households' SNAP participation and food insecurity using data from the Panel Study of Income Dynamics. Ratcliffe, McKernan, and Zhang (2011) and Shaefer and Gutierrez (2012) obtained similar findings with data from the Survey of Income and Program Participation. Shaefer and Gutierrez estimated dummy endogenous variable models with and without variable exclusion restrictions with little change in their results, which

[5] The researchers used sampling weights to address this issue.

suggested that identification for this entire group of studies may have been obtained mainly from functional form.

The preceding statistical approaches all make strong assumptions in order to identify an effect of SNAP on food hardships. Additionally, these methods differ in what they measure. For example, propensity score matching models identify the average effect of the treatment on the treated (ATET), while 2SLS methods isolate the local average treatment effect (LATE)—that is, the effect of SNAP participation for those whose decision to participate is altered by the value of instruments or excluded variables. The dummy endogenous variables models mentioned here are aimed at identifying the average treatment effect (ATE) of SNAP—that is, the expected outcome if SNAP were given to a randomly assigned person in the population of interest. While the ATE might also be identified by longitudinal models, such models rely on the additional assumption that endogenous unobservables are time-invariant; as noted, this assumption seems to be at odds with current evidence.[6]

An alternative approach to introducing model assumptions *a priori* is to bound the possible impacts first using logical probability restrictions and then introducing relatively weak assumptions (see Manski, 1995 as general reference). While this approach reduces the reliance on strong assumptions, it tends to produce a wide range of plausible effects. Gundersen and Kreider (2008) have used the bounds approach to show that the same data that generate counter-intuitive differences in participants' and non-participants' food

[6] A fuller discussion of these issues in relation to food assistance programs can be found in Meyerhoefer and Yang (2011).

hardships are also consistent with underlying negative impacts when the possible influence of measurement error is accounted for.

Dose-Response Relationships

Another branch of the research literature has considered how food hardships change with more generous SNAP benefits or more intense participation (i.e., with a higher "dose" of the SNAP "treatment"). For example, in the most recent *Household Food Security in the United States* report, Coleman-Jensen et al. (2012) estimate that the rate of food insecurity was 56.0% among households that received SNAP benefits for 1 to 11 months during the preceding year but only 49.1% among households that receive SNAP benefits for all 12 months. Similarly, Mabli et al. (2013) found that food security prevalence decreased significantly for households that participated in SNAP for six months.

Studies with multivariate designs find similar evidence. Rose, Gunderson, and Oliveira (1998) estimated logit models of food insufficiency and found that higher levels of SNAP benefits were significantly negatively associated with food insufficiency. DePolt, Moffitt, and Ribar (2009) obtained similar results, estimating longitudinal multiple-indicator, multiple cause models of food insecurity. Van Hook and Balistreri (2006) used predicted measures of unmet program need in the form of reduced probabilities of SNAP participation and reduced SNAP allotments and found that these were positivity associated with hardships. Watson et al. (2012) found a strong dose-response effect of SNAP in reducing children's food insecurity.

Indirect Analyses

All of the preceding studies examined how an individual household's receipt or use of SNAP benefits was associated with its own food hardships. Several studies have investigated how measures of characteristics that are associated with the general availability of SNAP are associated with hardships. For example, Borjas (2004) showed how food insecurity for non-citizen immigrants jumped relative to food insecurity for native and naturalized citizens following the enactment of the Personal Responsibility and Work Opportunities Act of 1996. Nord and Prell (2011) compared 30-day food insecurity before and after SNAP benefits were increased as part of the American Recovery and Reinvestment Act of 2009; they found that food insecurity fell for households that were income-eligible for the SNAP but not for near-eligible households, suggesting that the higher benefits reduced hardships. Other studies, however, have found weaker associations or no associations. Using data from the CPS-FSS, Bartfeld and Dunifon (2006) found that state-level SNAP participation was associated with food security for above-poverty, low-income households but not for below-poverty households. Using data from Oregon, Bernell, Weber, and Edwards (2006) found that county-level SNAP participation was not associated with food insecurity.

Replication Analysis

Although there are many consistent results and patterns across the empirical studies of SNAP and food hardships, there are also considerable differences. Besides differing in their statistical methodologies, previous studies have differed in their measures of food hardships, measures of SNAP receipt, choice of surveys and time periods, and selection of analysis samples within those surveys. In this section, we attempt to replicate previous findings by employing most of the statistical methodologies to a single dataset—the 2009-2011 waves of the CPS-FSS.[7]

For each of these years of the CPS-FSS, we select households with annual incomes at or below 130% of the federal poverty line. Besides being the income cut-off used to examine SNAP in the annual *Household Food Security in the United States* reports, this threshold also leads to a sample that meets the gross-income test for SNAP and that satisfies the screen for answering the Food Security Module. We additionally restrict our analysis sample to households that responded to the FSS, that provided sufficient information to determine their food security status, and that provided information for other FSS measures that we use as explanatory variables.

For our analyses, we consider a sample that combines all households that meet the preceding criteria, but we also consider four mutually exclusive subsets of households: unmarried parent households with children under age 18, married parent households with children under age 18, households consisting entirely of members who are age 60 or older, and other adult-only households. These types of low-income households differ in their susceptibility to food hardships, are subject to different rules under the SNAP, and are differently eligible for other types of public assistance. Disaggregating this way increases the comparability of households within groups; it also helps us to ascertain the

[7] We focus on 2009-2011 because it is the most recent period available with consistent federal policies. The period includes the 15% benefit increase and other provisions from the American Recovery and Reinvestment Act of 2009. Extending the analysis further back would entail accounting for these policy changes.

robustness of our findings and the findings of previous studies that have adopted different analysis groups.

The outcome variable in most of our analyses is a binary indicator for the household being food insecure, which is constructed from the 12-month, 18-item Food Security Module. Our principal explanatory variables are indicators for the receipt of any SNAP benefits and a continuous measure of SNAP benefits. In some of our analyses, we use an indicator for the receipt of SNAP benefits any time during the preceding year. This is the first SNAP question that is asked in the CPS-FSS, and its reference period corresponds with the reference period for the Food Security Module items. In other analyses, we use an indicator for the receipt of SNAP benefits in the month preceding the interview. Although this question is asked conditional on the annual measure, it may be more reliably reported. We also consider this measure because of its use in previous research and because preliminary analyses showed that it led to a distinct result pattern. For our final analyses, we use a continuous measure of annual SNAP benefits which allows us to examine the dose-response of households to SNAP.

For our multivariate analyses, we incorporate numerous additional controls that are available in the CPS-FSS; most of these are standard and have been used in previous research. The controls include the household head's gender, age, race, ethnicity, nativity, marital status, education, and employment status; numbers of adults, children, and disabled members in the household; age of youngest member (households with children); an indicator for elderly members; residence in urban area; the state unemployment rate; household income; home ownership; food needs; receipt of SBP, NSLP, and WIC benefits (households with children); the use of food banks and soup kitchens; and state and year fixed effects. Means and standard deviations for our explanatory variables, calculated separately for SNAP participants and non-participants, for in each of our four analysis subsamples are in Appendix B.

We start our replication analysis by estimating linear probability models (LPMs) of households' food insecurity status. Estimated coefficients and standard errors for the SNAP receipt explanatory variable from alternative specifications and analysis samples are listed in Table 1. All of the regressions in Table 1 incorporate sampling weights provided with the CPS-FSS that adjust for the CPS sampling design and for differential response in the FSS. Estimates for the entire combined sample of households are reported in the first column of the table. The subsequent columns report estimates separately for the mutually exclusive subsamples of unmarried parent households, married parent households, households composed entirely of elderly members, and other adult-only households. The top panel lists estimates from models that include measures of any SNAP receipt in the previous year, while the bottom panel lists results from models of SNAP receipt in the previous month.

The first row in each panel of Table 1 reports coefficients from simple univariate LPMs of food insecurity regressed on SNAP receipt. The estimates, which represent unconditional differences in average food insecurity between SNAP participants and non-participants, are all strongly positive and consistent with estimates from previous descriptive analyses, such as Coleman-Jensen et al. (2012).

Table 1

Coefficients on SNAP Receipt from Linear Probability Models

	All households	HHs with children and unmarried parents	HHs with children and married parents	Households with all elderly members	Other adult-only households
		Received SNAP in last year			
LPM with no other controls	0.288*** (0.007)	0.188*** (0.015)	0.237*** (0.017)	0.290*** (0.018)	0.314*** (0.012)
LPM with standard controls [a]	0.226*** (0.008)	0.184*** (0.016)	0.231*** (0.018)	0.229*** (0.019)	0.256*** (0.014)
LPM with standard and economic controls [b]	0.207*** (0.008)	0.164*** (0.016)	0.209*** (0.019)	0.215*** (0.019)	0.234*** (0.014)
LPM with standard, economic, and other assistance controls [c]	0.136*** (0.008)	0.088*** (0.017)	0.116*** (0.019)	0.174*** (0.020)	0.161*** (0.014)
		Received SNAP in last month			
LPM with no other controls	0.256*** (0.007)	0.140*** (0.015)	0.198*** (0.018)	0.272*** (0.019)	0.293*** (0.013)
LPM with standard controls [a]	0.187*** (0.008)	0.131*** (0.016)	0.188*** (0.019)	0.206*** (0.020)	0.227*** (0.014)
LPM with standard and economic controls [b]	0.166*** (0.008)	0.108*** (0.016)	0.162*** (0.019)	0.192*** (0.020)	0.204*** (0.015)
LPM with standard, economic, and other assistance controls [c]	0.095*** (0.009)	0.032* (0.016)	0.066*** (0.020)	0.152*** (0.020)	0.132*** (0.014)

Note. LPMs estimated using weighted household data from the 2009-11 CPS-FSS. Robust standard errors appear in parentheses.

[a] Control for household head's gender, age, age squared, race, ethnicity, nativity, marital status, and education; numbers of adults, children, and disabled members in household; age of youngest member (households with children); elderly members; residence in urban area; state unemployment rate; and state and year fixed effects. LPMs for all households also control for household type.

[b] Control for head's employment status, log of household income, home ownership, log of food needs, and indicator for missing food needs.

[c] Control for participation in SBP, NSLP and WIC (households with children) and use of food pantries or soup kitchens.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

The differences are largest for the two groups of adult-only households and smallest for single-parent households. Also consistent with previous analyses, the differences in food insecurity are appreciably larger when SNAP receipt is measured on a previous-year basis rather than a previous-month basis.

The second rows in the panels list coefficients from LPMs that add controls for demographic characteristics of the households and their heads, geographic attributes, and state and time fixed effects. Adding these controls substantially reduces the estimated associations between SNAP receipt and food insecurity for the two groups of adult-only households but only slightly reduces the associations for the two groups of households with children.

The third rows report coefficients from specifications that also add controls for employment status, household income, home ownership, and subjectively-assessed food needs, and the use of these controls attenuates the associations between SNAP receipt and food insecurity more. Finally, the last rows in the panels add controls for SBP, NSLP, and WIC program participation for the households with children and food bank and soup kitchen use for all households. Although these controls further reduce the estimated coefficients, the conditional associations between SNAP receipt and food insecurity remain positive and statistically distinguishable from zero. The patterns of results are consistent with previous research findings that observed controls attenuate but do not eliminate the counter-intuitive positive associations between SNAP participation and food insecurity.

We next consider matching estimates as a more general way to mitigate confounding influences from observable characteristics. Results from this analysis are reported in Table 2, which follows the organization from Table 1 with estimates arranged by analysis groups in columns, by the periodicity of SNAP receipt in top and bottom panels, and by the type or specification of the estimator in rows within panels. Because of questions regarding the interpretation of sample weights in matching analyses, we report results computed with unweighted data. For purposes of comparison with our previous estimates, we report unconditional differences in food insecurity between SNAP participants and non-participants in the first rows of the panels and report coefficients from LPMs with our standard and economic controls (the same parameterizations as the third rows from Table 1) in the second rows. The estimates in the first two rows indicate that weighting has no substantive impact on the estimates for households with children but modest impacts for the two groups of adult-only households.

The third rows of the panels in Table 2 list the differences between the average rates of food insecurity between our participant samples and matched non-participant samples. The samples were matched using predicted probabilities from logit models of SNAP participation that included our standard and economic controls. For the matching itself, we selected nearest match neighbors with replacement and restricted the matches to the common support of the predicted probabilities (virtually the entire range of probabilities). Analyses (not shown) confirm that the matched samples were balanced in terms of the observed control variables.

Table 2

Coefficients on SNAP Receipt from Simple, LPM, and PSM Comparisons

	All households	HHs with children and unmarried parents	HHs with children and married parents	Households with all elderly members	Other adult-only households
		Received SNAP in last year			
Bivariate comparison	0.281*** (0.006)	0.186*** (0.013)	0.234*** (0.015)	0.274*** (0.013)	0.300*** (0.010)
LPM	0.197*** (0.007)	0.165*** (0.014)	0.208*** (0.016)	0.218*** (0.013)	0.227*** (0.011)
PSM comparison	0.184*** (0.011)	0.174*** (0.022)	0.207*** (0.023)	0.229*** (0.022)	0.230*** (0.018)
		Received SNAP in last month			
Bivariate comparison	0.252*** (0.006)	0.144*** (0.013)	0.199*** (0.016)	0.258*** (0.013)	0.278*** (0.011)
LPM	0.159*** (0.007)	0.114*** (0.014)	0.166*** (0.017)	0.200*** (0.014)	0.197*** (0.012)
PSM comparison	0.135*** (0.011)	0.111*** (0.021)	0.156*** (0.024)	0.165*** (0.023)	0.220*** (0.019)

Note. Estimates from unweighted household data from the 2009-11 CPS-FSS. LP and PSM models control for household head's gender, age, age squared, race, ethnicity, nativity, marital status, education, and employment status; numbers of adults, children, and disabled members in household; age of youngest member (households with children); elderly members; residence in urban area; state unemployment rate; log of household income; home ownership; log of food needs; missing food needs; and state and year fixed effects. Models for all households also control for household type. PSM comparisons use nearest-neighbor matching with replacement. Robust standard errors appear in parentheses.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

Turning to the results in the table, differences in food insecurity in the matched samples are mostly smaller than the unconditional differences and the regression-based conditional differences. Despite the general attenuation in the estimated differences, all of them remain significantly and substantively positive, mirroring the results reported by Gibson-Davis and Foster (2006) for the incidence of food insecurity.

We next consider longitudinal estimators. The design of the CPS, in which rotation groups of households are interviewed for four consecutive months, left alone for eight months, and then re-interviewed for four more consecutive months, allows the construction of short, two-year panels from adjoining years of the CPS-FSS. As with Wilde and Nord (2005), we take advantage of this feature to produce longitudinal analysis datasets and to estimate panel data models. The longitudinal data from the CPS-FSS have some limitations beyond their short lengths. Most importantly, the units that the CPS follows are physical addresses, not individuals or households. Thus, people who move between surveys cannot be longitudinally linked and effectively attrit from the panels. Also, the CPS does not produce sampling weights for longitudinally-linked CPS-FSS households, so we conduct our statistical analyses using unweighted data.

Results from our longitudinal analyses are reported in Table 3. For purposes of comparison, we estimate LPMs with our standard and economic controls but using the unweighted longitudinal sample. Estimates from these specifications in the first rows of the panels are all very similar to the LPMs for the full sample. The results reassure us that there is little, if any, selection bias associated with CPS-FSS longitudinal sample attrition.

Estimates from panel-data random- and fixed-effect LPMs are reported in the second and third rows of the top and bottom panels of Table 3. Comparisons of these estimates reveal that accounting for unobserved time-invariant characteristics through the use of fixed effects reduces the estimated associations between SNAP receipt and food insecurity. However, large positive and statistically significant associations remain for all groups except for unmarried parent households when SNAP is measured on the basis of the previous month. Formal specification tests are reported below the random-

(Breusch-Pagan) and fixed-effect (Hausman-Wu) LPM estimates in the top and bottom panels of Table 3. The LPMs are strongly rejected by the Breusch-Pagan test in favor of the random effect LPMs for all household types, regardless of how SNAP is measured. Hausman-Wu tests fail to reject the null that the random effect LPMs are consistent for unmarried parent households. For all other groups, the random effect LPM is rejected in favor of the fixed effect LPM. This result strengthens when SNAP is measured on the basis of the previous month.

Table 3

Coefficients and Marginal Effects for SNAP Receipt from Longitudinal Models

	All households	HHs with children and unmarried parents	HHs with children and married parents	Households with all elderly members	Other adult-only households
	Received SNAP in last year				
LPM	0.188*** (0.011)	0.149*** (0.024)	0.184*** (0.025)	0.193*** (0.023)	0.214*** (0.019)
Random effects LPM	0.176*** (0.010)	0.135*** (0.024)	0.178*** (0.023)	0.190*** (0.018)	0.193*** (0.018)
Breusch-Pagan Test	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Fixed effects LPM	0.114*** (0.016)	0.090*** (0.034)	0.126*** (0.039)	0.168*** (0.031)	0.098*** (0.029)
Hausman-Wu Test	[0.000]	[0.175]	[0.150]	[0.000]	[0.000]
Logit	0.169*** (0.011)	0.147*** (0.024)	0.175*** (0.024)	0.163*** (0.022)	0.197*** (0.019)
Fixed effects logit	0.085 (0.073)	0.049 (0.090)	0.196 (0.136)	0.045 (0.119)	0.102 (0.078)
	Received SNAP in last month				
LPM	0.159*** (0.011)	0.098*** (0.024)	0.166*** (0.026)	0.183*** (0.024)	0.182*** (0.020)
Random effects LPM	0.146*** (0.010)	0.085*** (0.023)	0.154*** (0.024)	0.179*** (0.019)	0.163*** (0.019)
Breusch-Pagan Test	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]

Table 3

(Cont.)

	All households	HHs with children and unmarried parents	HHs with children and married parents	Households with all elderly members	Other adult-only households
	Received SNAP in last month (cont.)				
Fixed effects LPM	0.082***	0.046	0.085**	0.155***	0.072**
	(0.016)	(0.033)	(0.038)	(0.031)	(0.029)
Hausman-Wu Test	[0.000]	[0.197]	[0.044]	[0.000]	[0.000]
Logit	0.140***	0.095***	0.155***	0.156***	0.165***
	(0.011)	(0.023)	(0.025)	(0.021)	(0.020)
Fixed effects logit	0.051	0.024	0.091	0.025	0.078
	(0.048)	(0.048)	(0.151)	(0.076)	(0.066)

Note: Models estimated using unweighted longitudinally-linked household data from the 2009-11 CPS-FSS and control for household head's gender, age, age squared, race, ethnicity, nativity, marital status, education, and employment status; numbers of adults, children, and disabled members in household; age of youngest member (households with children); elderly members; residence in urban area; state unemployment rate; log of household income; home ownership; log of food needs; missing food needs; and state and year fixed effects. Models for all households also control for household type. Robust standard errors appear in parentheses .P values are in brackets.
* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

To investigate the possible sensitivity of these findings to the use of LPMs rather than more specialized binary outcome models, we re-estimated the standard and fixed-effects models using standard and conditional, fixed-effect logit specifications, respectively. Average marginal effects were calculated for these models to facilitate comparison with the LPMs. Marginal effects from the logit models are qualitatively similar to the coefficients from the LPMs in most cases, though the marginal effects from the fixed-effect logit models are all statistically insignificant.

Next, we investigate evidence from 2SLS and dummy endogenous variable models. Asymptotic standard errors for the average marginal effects generated by the dummy endogenous variable models are estimated using the delta method.[8] For each type of model, we consider two potential instruments: an indicator for the household head being a non-citizen and an estimate derived from the SNAP Quality Control files of the median certification interval from SNAP cases in the household's state of residence. Non-citizen status is a consistently significant explanatory variable in models of SNAP participation for our samples. However, its use as an instrument is controversial because cultural and assimilation differences between non-citizens and other U.S. residents could contribute directly to experiences and reporting of food hardships. Certification intervals have a stronger theoretical basis for serving as instruments, but they are only modestly predictive in our samples.[9] To test the sensitivity of our 2SLS and dummy endogenous variable results, we estimate models first using both instruments and then using just the certification interval instrument. Estimates from our specifications are reported in Table 4.

For convenience, we reproduce the LPM estimates from our specifications with standard and economic explanatory variables in the first rows of the panels of Table 4. The second rows list estimates and a Hausman-Wu test from 2SLS models that are identified from exclusions on non-citizenship status and certification intervals. The coefficient estimates for all households and for households with children are large and negative, while the coefficient estimates for households with all elderly members are

[8] We also estimate asymptotic standard errors following Terza (2012); however, we report the delta method standard errors to increase the replicability of our analysis.

[9] In preliminary analyses, we also experimented with state-level measures of broad-based categorical eligibility policies and standard utility allowance provisions (two policies that are the focus of debate as the U.S. Congress considers the re-authorization of the SNAP). However, neither of these policy variables was predictive of SNAP receipt in our samples.

large and positive. However, all of the coefficients are wildly imprecise and unable to discriminate between large positive or large negative effects.

Table 4

Coefficients and Marginal Effects for SNAP Receipt from LPM, 2SLS, Probit, & Bivariate Probit Models

	All households	HHs with children and unmarried parents	HHs with children and married parents	Households with all elderly members	Other adult-only households
	Received SNAP in last year				
LPM (exogenous)	0.207*** (0.008)	0.164*** (0.016)	0.209*** (0.019)	0.215*** (0.019)	0.234*** (0.014)
2SLS—citizenship & cert. interval instr.	-0.162 (0.193)	-0.123 (0.568)	-0.362 (0.809)	0.549 (0.373)	-0.087 (0.218)
Hausman-Wu Test	[0.044]	[0.603]	[0.442]	[0.372]	[0.603]
2SLS—certification interval instrument	-0.086 (0.437)	-0.100 (0.682)	-0.585 (0.906)	-0.063 (5.128)	0.286 (0.611)
Hausman-Wu Test	[0.490]	[0.694]	[0.299]	[0.956]	[0.933]
Probit (exogenous)	0.199*** (0.008)	0.164*** (0.016)	0.207*** (0.018)	0.194*** (0.019)	0.227*** (0.014)
Biprobit—citizenship & cert. interval instr.	-0.141*** (0.055)	0.366 (0.259)	-0.109 (0.210)	-0.032 (0.071)	-0.174** (0.074)
Biprobit—certification interval instrument	-0.165*** (0.061)	0.382* (0.233)	-0.127 (0.197)	-0.069 (0.072)	-0.212*** (0.080)
Biprobit—no instruments	-0.178*** (0.061)	0.423** (0.182)	-0.142 (0.224)	-0.066 (0.071)	-0.225*** (0.074)
	Received SNAP in last month				
LPM (exogenous)	0.166*** (0.008)	0.108*** (0.016)	0.162*** (0.019)	0.192*** (0.020)	0.204*** (0.015)
2SLS—citizenship & cert. interval instr.	-0.166 (0.198)	-0.087 (0.395)	-0.321 (0.761)	0.772 (0.572)	-0.098 (0.242)
Hausman Test	[0.081]	[0.619]	[0.510]	[0.280]	[0.201]
2SLS—certification interval instrument	-0.081 (0.411)	-0.075 (0.506)	-0.578 (0.856)	-0.034 (2.776)	0.320 (0.690)
Hausman-Wu Test	[0.538]	[0.716]	[0.333]	[0.933]	[0.866]

Table 4

(Cont.)

	All households	HHs with children and unmarried parents	HHs with children and married parents	Households with all elderly members	Other adult-only households
			Received SNAP in last month (cont.)		
Probit (exogenous)	0.158*** (0.008)	0.108*** (0.016)	0.160*** (0.019)	0.172*** (0.019)	0.196*** (0.015)
Biprobit—citizenship & cert. interval instr.	-0.206*** (0.040)	0.135 (0.306)	-0.207 (0.164)	-0.019 (0.070)	-0.151** (0.070)
Biprobit—certification interval instrument	-0.228*** (0.039)	0.175 (0.297)	-0.227 (0.145)	-0.034 (0.070)	-0.166** (0.077)
Biprobit—no instruments	-0.239*** (0.037)	0.253 (0.264)	-0.239 (0.156)	-0.031 (0.070)	-0.176** (0.074)

Note. Models estimated using weighted household data from the 2009-11 CPS-FSS and control for household head's gender, age, age squared, race, ethnicity, nativity, marital status, education, and employment status; numbers of adults, children, and disabled members in household; age of youngest member (households with children); elderly members; residence in urban area; state unemployment rate; log of household income; home ownership; log of food needs; missing food needs; and state and year fixed effects. Models for all households also control for household type. Robust standard errors appear in parentheses. *P* values are in brackets.
* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

The Hausman-Wu test for all households provides evidence that SNAP is endogenous at the 5% level; however, this result weakens when SNAP is measured on the basis of the previous month. For the groups separately, we find no evidence of SNAP being endogenous. In the third row, we list results from 2SLS models that rely entirely on certification intervals for identification. These estimates are even less precise than the preceding estimates. In contrast to the previous 2SLS model, the Hausman-Wu tests do not indicate SNAP is endogenous for any specifications.

In the next four rows, we list results from probit specifications. The first row lists average marginal effects from standard probit specifications, and these generate estimates that are qualitatively and quantitatively similar to the LPM estimates. The next row lists estimates from a bivariate probit model that imposes exclusion restrictions on non-citizenship status and certification intervals. The marginal effects for the combined and married-parent samples are significantly negative. While these particular results are potentially encouraging for the theoretical model, they appear to stem entirely from functional form restrictions in the bivariate probit model. In the final rows of Table 4, where we report results from bivariate probit models without any variable exclusion restrictions, the marginal effect estimates are nearly identical in sign, magnitude, and precision to the preceding estimates. Thus, the results from the bottom four rows of Table 4 seem to bear out the findings of Greenhalgh-Stanley and Fitzpatrick (2013) and Shaefer and Gutierrez (2012).

Finally, we investigate the dose response of SNAP on food insecurity using cross sectional and longitudinal models. For each model, we consider two measures of SNAP; an indicator for receipt of SNAP benefits within the past 12 months and the inflation adjusted annual SNAP benefit amount. Including an indicator for the receipt of SNAP benefits allows us to assess the extent of selection bias in the dose-response literature, while the annual measure of SNAP benefits facilitates replication of the existing literature. We begin our dose response analysis by estimating LPMs, followed by random- and fixed-effect LPMs. Estimated coefficients and standard errors for the SNAP receipt and annual SNAP benefit variables from alternative specifications are listed in Table 5.

Table 5

Coefficients on SNAP Receipt and Annual Benefit Amount from Cross Sectional and Longitudinal Models

	All Households		HHs with children and unmarried parents		HHs with children and married parents		Households with all elderly members		Other adult-only households	
	SNAP Indicator	Annual SNAP Benefit	SNAP Indicator	Annual SNAP Benefit	SNAP Indicator	Annual SNAP Benefit	SNAP Indicator	Annual SNAP Benefit	SNAP Indicator	Annual SNAP Benefit
Cross Sectional Models										
LPM with no other controls	0.351*** (0.011)	-0.024*** (0.004)	0.283*** (0.023)	-0.029*** (0.006)	0.331*** (0.029)	-0.029*** (0.008)	0.345*** (0.029)	-0.045** (0.020)	0.391*** (0.019)	-0.043*** (0.009)
LPM with standard controls [a]	0.299*** (0.011)	-0.031*** (0.004)	0.276*** (0.023)	-0.030*** (0.006)	0.319*** (0.030)	-0.027*** (0.008)	0.292*** (0.029)	-0.056*** (0.021)	0.327*** (0.020)	-0.041*** (0.009)
LPM with standard and economic controls [b]	0.286*** (0.011)	-0.035*** (0.004)	0.264*** (0.023)	-0.035*** (0.006)	0.309*** (0.030)	-0.032*** (0.008)	0.283*** (0.029)	-0.061*** (0.021)	0.312*** (0.020)	-0.045*** (0.009)
LPM with standard, economic, and other assistance controls [c]	0.222*** (0.011)	-0.041*** (0.004)	0.194*** (0.023)	-0.039*** (0.006)	0.227*** (0.030)	-0.038*** (0.008)	0.235*** (0.029)	-0.052*** (0.020)	0.234*** (0.020)	-0.044*** (0.009)

Table 5

(Cont.)

	All Households		HHs with children and unmarried parents		HHs with children and married parents		Households with all elderly members		Other adult-only households	
	SNAP Indicator	Annual SNAP Benefit	SNAP Indicator	Annual SNAP Benefit	SNAP Indicator	Annual SNAP Benefit	SNAP Indicator	Annual SNAP Benefit	SNAP Indicator	Annual SNAP Benefit
					Longitudinal Models					
LPM	0.261*** (0.016)	-0.031*** (0.005)	0.245*** (0.035)	-0.029*** (0.009)	0.270*** (0.041)	-0.025** (0.011)	0.251*** (0.038)	-0.054* (0.028)	0.284*** (0.029)	-0.039*** (0.013)
Random effects LPM	0.238*** (0.014)	-0.025*** (0.005)	0.212*** (0.034)	-0.023*** (0.008)	0.249*** (0.037)	-0.020** (0.010)	0.250*** (0.027)	-0.053*** (0.019)	0.247*** (0.026)	-0.028** (0.012)
Breusch-Pagan Test	[0.000]		[0.000]		[0.000]		[0.000]		[0.000]	
Fixed effects LPM	0.152*** (0.021)	-0.012* (0.007)	0.128*** (0.046)	-0.009 (0.011)	0.173*** (0.052)	-0.012 (0.014)	0.238*** (0.044)	-0.053* (0.030)	0.115*** (0.039)	-0.003 (0.017)
Hausman-Wu Test	[0.000]		[0.108]		[0.074]		[0.144]		[0.000]	

Note. Models estimated using weighted household data from the 2009-11 CPS-FSS and control for household head's gender, age, age squared, race, ethnicity, nativity, marital status, education, and employment status; numbers of adults, children, and disabled members in household; age of youngest member (households with children); elderly members; residence in urban area; state unemployment rate; log of household income; home ownership; log of food needs; missing food needs; and state and year fixed effects. Models for all households also control for household type. Robust standard errors appear in parentheses. *P* values are in brackets.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

The SNAP receipt coefficients are generally consistent with the discussion presented above, so we will limit our discussion here to the annual SNAP benefit coefficients. While including observable controls and household fixed-effects reduces the association between SNAP receipt and food insecurity, a strong and highly significant relationship remains. The top panel lists estimates from cross sectional models, while the bottom panel lists results from longitudinal models.

The first rows of the top panel list coefficients from simple univariate LPMs of food insecurity, SNAP receipt, and the annual benefit amount estimated with the cross sectional sample. The coefficient on annual SNAP benefits is negative and significant for all groups of households. These patterns continue in the second, third, and fourth rows when increasing sets of observed controls are added.

The bottom panel of Table 5 considers longitudinal models. For the purposes of comparison, we estimate LPMs with standard and economic controls. The first rows report coefficients for LPMs. The associations between food insecurity and annual SNAP benefits are smaller for all groups with the exception of married parent households when compared to LPMs estimated using the cross sectional sample.

Estimates from panel-data random- and fixed-effect LPMs are reported in the second and third rows of the bottom panel of Table 5. Comparisons of these estimates reveal that accounting for unobserved time-invariant household characteristics through the use of fixed effects reduces the estimated associations between annual SNAP benefits and food insecurity. The coefficients on annual SNAP benefits are negative and insignificant for all household groups except households with all elderly members.

Breusch-Pagan and Hausman-Wu tests are reported below the random- and fixed-effect LPMs, respectively. The LPMs are strongly rejected for all household groups by the Breusch-Pagan test in favor of the random-effect LPMs models. In contrast to the participant/non-participant analyses, the Hausman-Wu tests fail to reject the random-effect LPM for all-elderly households. For unmarried parent households, the Hausman-Wu test still fails to reject random-effect LPM; however, the p-value is very close to the 10% confidence level. The random-effect LPM is rejected for all other groups.

Sensitivity Analyses

The replication analysis is based on a sample of households with incomes at or below 130% of the federal poverty line. However several previous studies estimate models with larger income cut-offs. A concern in these studies is that marginally eligible households will adjust their labor supply to ensure program eligibility, potentially affecting the observed relationship between SNAP and food insecurity. We examined the sensitivity of our findings to the choice of income limits by estimating models with a sample that restricted household income to 185% of the federal poverty line. We used the 185% of the federal poverty line threshold because it is the income screen used by the CPS-FSS for the food security questions. Models estimated using the 185% of the federal poverty line threshold (results not shown) were very similar to those using our primary (130%) sample.

Another potential concern is the use of a single binary measure of food insecurity. For the replication analysis we concentrate on a binary measure of food insecurity, which is consistent with most of the previous studies. As DePolt et al. (2009), Gundersen, Kreider, and Pepper (2011) and others have pointed out, these comparisons cast aside a considerable amount of information. To examine how the findings are affected by the choice of the food insecurity measure we re-estimated models using the count of affirmed food security questions, which under the assumptions of the measurement model used to determine food security status should be a sufficient statistic of the underlying food security scale. Estimating models with the count of affirmed food security questions generated results that were consistent with our reported findings using the binary food insecurity measure.

The replication analysis uses an annual measure of SNAP benefits to examine the dose response of SNAP on food insecurity. An alternative to the dollar amount of SNAP benefits is the number months of program receipt. We tested the sensitivity of our dose response findings to the choice of dose variable by estimating models with the count of months of SNAP receipt. A comparison of the estimates suggests our findings are robust to the choice of dose variable. All of our sensitivity analyses are available upon request.

Conclusion

It would be hard to overstate the importance of SNAP in the food assistance landscape. It is the largest food assistance program administered by the U.S. Department of Agriculture in terms of expenditures and participation. However, despite recent research that suggests SNAP reduces food insecurity, the evidence taken as a whole is somewhat inconsistent. In an effort to understand the empirical results that have grown up around the question of SNAP's effectiveness on food insecurity, we have examined theory, literature, and empirical evidence that looks at this question and have replicated methods used in previous research.

The main finding of this study is that recent results showing that food assistance reduces food insecurity may not be robust to specification choice or data. As in other research, most of our simple models suggest a higher conditional mean of food security prevalence associated with SNAP. Moreover, our results for propensity score and longitudinal models mirror those in the empirical literature in showing, quite counterintuitively, that SNAP is associated with increases in food security prevalence. Our 2SLS results are a bit more consistent with recent findings, although the estimated sizes of the effects are statistically insignificant. Similarly, our findings using dummy endogenous approaches yield somewhat inconsistent results, with many of the statistically significant results being for two-parent households with children. We note that most of the results using this method yield parameter estimates with the appropriate sign, even when they are not significant. Our dose-response models are consistent with previous research in that they suggest larger amounts of SNAP benefits are associated with a reduction in the likelihood of food insecurity. Finally, while we did not try to replicate the methods of Gundersen and Kreider (2008) or Kreider, Pepper, Gundersen, and Joliffe (2012), which involve using data and logical assumptions to identify plausible bounds for the effect of food assistance on food insecurity, our results are, broadly speaking within the bounds for their least restrictive models. This is true for models that do take account of measurement error and those that do not.

Taken together, these results suggest some directions for future research. For example, some models that have most consistently found that SNAP reduces food insecurity share an assumption about the functional form of the residuals in selection and outcome processes—bivariate normal. A next step could be to examine similar models while relaxing the bivariate normal assumption, perhaps by use of maximum simulated likelihood methods and factor structures—both discrete and continuous. Additionally, such a consideration should take into account that a full switching regression framework—in which the outcome is estimated separately for each treatment state, but simultaneously with treatment—may yield different results.[10] In addition, given that the results of our dose-response models are consistent with both the literature and with economic intuition about the effect of SNAP, further exploration into the uses of these methods and the design of surveys to exploit these relationships should be a priority. Nord and Prell (2011) offer a recent example of this kind of work. Finally, to the degree possible, studies using indirect methods and natural experiments should also be encouraged.

[10] This has recently been found by Gregory and Coleman-Jensen (forthcoming), who find that the ATE for SNAP participation is positive in a switching regression framework with bivariate normal errors, but negative in a simple bivariate probit.

CHAPTER III

MEASURING THE EFFECT OF SUPPLEMENTAL NUTRITION ASSISTANCE PROGRAM PARTICIPATION ON FOOD INSECURITY USING A BEHAVIORAL RASCH SELECTION MODEL

Abstract

This paper examines the relationship between Supplemental Nutrition Assistance Program (SNAP) participation and food insecurity using data from the 2001-2008 Current Population Survey Food Security Supplement (CPS-FSS). A behavioral Rasch selection model is proposed and estimated using four subsamples of low-income households: unmarried parent households, married parent households, all-elderly households, and other adult-only households. The model is identified using exogenous changes in state-level polices related to SNAP. The results indicate that SNAP has a strong ameliorative effect on food insecurity for married parent, all-elderly, and other adult-only households, while SNAP continues to be associated with greater food hardships for unmarried parent households. Participating in SNAP reduces the probability of food insecurity by 22.4% for other adult-only households, 18% for all-elderly households, and 17% for married parent households.

Introduction

While the majority of U.S. households have consistent, dependable access to enough food for an active healthy life (food secure; Andersen, 1990), a minority of households experience food insecurity. These households have limited access to adequate foods because of a lack of money or other resources. In 2011, nearly 15% (50.1 million people living in 17.9 million households) of all U.S. households were food insecure, with a third of these households experiencing a more severe level of food insecurity known as very low food security (Coleman-Jensen, Nord, Andrews, & Carlson, 2012). The consequences of food insecurity are far-reaching and occur for children and adults of all ages (Gundersen et al., 2011). Households employ a variety of methods to meet their basic food needs. Some rely on help from emergency food providers while others participate in one or more of the federal food and nutrition assistance programs.

The Supplemental Nutrition Assistance Program (SNAP, formerly the Food Stamp Program) is the largest food and nutrition assistance program funded by the U.S. Department of Agriculture (USDA), accounting for 73% ($75 billion) of federal food and nutrition assistance spending in fiscal year 2011 (Oliveira, 2012). The goal of SNAP is to help low-income households obtain access to food, a healthful diet, and nutrition education. By improving nutrition and diet, the program is also intended to advance other goals, such as improving food security. The effectiveness of SNAP in reducing food insecurity is an important policy issue for program administrators and policymakers.

Estimation of the relationship between SNAP participation and food insecurity is complicated by the fact that selection issues may be contributing to counterintuitive findings. Households participating in SNAP are likely to differ in both observable and unobservable ways from non-participating households. In particular, households with greater food needs and fewer resources are more likely to participate in SNAP. Failure to account for these differences could be confounding household characteristics with participation behavior, resulting in biased estimates.

Several studies control for selection on unobservable characteristics and generate inconclusive results. These studies rely on a single binary measure of food insecurity. While interpretation of the food insecure versus food secure comparison is straightforward and easy to implement, considerable information is being suppressed. The USDA measures food insecurity using a set of 18 questions from the Household Food Security Module (HFSSM), which is fielded by the Current Population Survey Food Security Supplement (CPS-FSS). Information is lost when broad categories are created using responses to questions from the HFSSM. Therefore, the use of a single binary measure is likely contributing to the generation of insignificant results.

This study examines the effectiveness of SNAP in reducing food insecurity among low-income households using data from the 2001-2008 CPS-FSS. Low-income households are disaggregated into four policy relevant subsamples: unmarried parent households, married parent households, all-elderly households, and other adult-only households.

For the multivariate analyses of food insecurity, I estimate behavioral Rasch models. The Rasch model assumes responses to the HFSSM questions may be modeled as indicators of a single underlying index of food hardship, such as food insecurity. Simultaneously modeling the outcomes this way leads to more efficient estimation. I modify the standard Rasch model to incorporate a behavioral component and to account for selection on contemporaneous unobservables. The models are identified using exogenous changes in state-level policies and rules related to SNAP. Instrumental variables capture information on policies related to vehicle asset rules, outreach activities, recertification intervals, and immigrant eligibility.

Descriptive analyses of my data reproduce the findings of previous studies that SNAP receipt is associated with higher rates of food insecurity. Estimates from the multivariate models, which attempt to control for selection on observable characteristics, yield the counter intuitive result of SNAP increasing food insecurity. After controlling for selection on unobservable characteristics, I find a highly significant and negative relationship between SNAP receipt and food insecurity for all household subsamples, with the exception of unmarried parent households. When using a single binary measure of food insecurity, the results are inconclusive. My findings are robust to the use of alternative program participation indicators, the choice of instrumental variables, and sample restrictions based on household income.

Conceptual Model

To motivate the empirical analyses, I begin with a discussion of Barrett's (2002) theoretical model.[11] For a more detailed discussion of Barrett's model, see Gregory, Rabbitt, and Ribar (2013) and Ribar (2013). Barrett's model extends the household production framework of Becker (1965) and the health production framework of Grossman (1972) to include food insecurity. Food insecurity falls out of the model as an indicator of risk exposure. The model assumes utility in each period is a function of physical well-being and consumption. Physical well-being in each period depends on

[11] Alternative conceptual models have been proposed by Gundersen and Gruber (2001), Gundersen and Oliveira (2001), Huffman and Jensen (2003), and Ribar and Hamrick (2003).

physical well-being in the previous period and nutritional intake, activity level, non-food consumption, and stochastic shocks in the current period. Nutritional inputs are determined by food consumption, the nutrient content of food, and stochastic shocks. All production functions are conditioned on the household's information set, which includes household members' human capital. The household chooses levels of consumption, physical well-being, savings, and activity levels so that utility is maximized subject to budget, time, and production constraints.

Barrett's model identifies structural characteristics of households that are associated with an increased risk of food hardships. First, food hardships are more likely to occur if household members have low labor productivity, which reduces their ability to work at home and in the labor market. Second, households that face adverse terms of trade in the form of higher food prices and lower labor market wages are at an increased risk of food hardships. Third, households with limited access to labor markets or goods markets are more likely to experience food hardships. Urban households have better access to these markets than rural households. Fourth, households with low savings and assets may find it difficult to smooth consumption over time, increasing the likelihood of food hardships. Fifth, households with a general susceptibility to shocks, because of poor health or residence in an area with a volatile economy are at an increased risk of food hardships. Sixth, households are more likely to face food hardships if they have unreliable social safety nets.

The largest public food assistance safety net in the U.S. is the SNAP. Program benefits are federally funded but administered at the state level. The SNAP is means-tested, and eligibility is usually determined using financial eligibility thresholds. Households without an elderly (age 60 or older) or disabled member must satisfy gross-income, net-income, and asset tests, while households with elderly or disabled members need only satisfy the net income test. Under the gross income test, basic monthly income must fall below 130% of the federal poverty line. The net income test restricts countable monthly income (gross monthly income less deductions[12]) to less than 100% of the federal poverty line. Prior to benefit calculation, households must also satisfy the asset test, which permits households without elderly or disabled members to have up to $2,000 in countable assets or $3,250 in countable assets if at least one member is elderly or disabled. Notable examples of countable assets include cash on hand, checking and savings accounts, savings certificates, stocks and bonds, and vehicles.

Alternatively, households are categorically eligible for SNAP if they receive benefits funded by the Temporary Assistance for Needy Families (TANF) block grant, Supplemental Security Income (SSI) cash assistance, or state General Assistance (GA). Households that receive benefits from these programs bypass the gross income and, more importantly, asset tests.

Barrett's model can also be used to consider how SNAP should affect households' food security. The receipt of SNAP benefits should expand the household's budget set and relax resource constraints. Program benefits allow households to purchase more food and should reduce the incidence of food hardships. I also anticipate complementary

[12] SNAP deductions include a standard deduction, earned income deduction, child support deduction, dependent care deduction, excess shelter deduction, and any out-of-pocket medical expenses (households with elderly or disabled members).

effects from the educational component of SNAP, which increases household member's meal planning, shopping, and preparation skills, making them more effective at transforming resources into nutritional inputs and physical well-being.

Meanwhile, other components of SNAP may be working against these effects. As with all means-tested programs, eligibility requirements effectively tax labor market activities, reducing household members' incentives to work. Depending on how strong these incentive effects are, SNAP receipt may increase food hardships. Participating households are also vulnerable to losses of benefits if they fail to comply with recertification and work requirements. Wilde and Ranney (2000) note the monthly cycle associated with benefit issuance, spending, and benefit exhaustion may give rise to food shortages. SNAP eligible food items, when compared to other types of food purchases, may potentially negatively affect households, as they require additional time and preparation when compared to other types of food items. While these factors might reduce the effectiveness of SNAP, the program's net effect is expected to be positive.

Although theory predicts SNAP participation decreases food hardships, several empirical studies have generated contradictory results. SNAP participation is clearly endogenous. The household's participation decision and determination of food insecurity are influenced by a host of characteristics, observable and unobservable. Failure to account for these characteristics will result in a spurious correlation. Barrett's model identifies several characteristics that are unobservable to the researcher such as household member's health status, food consumption patterns, and preferences. Joyce et al. (2012) also suggests that housing security and utility losses are often associated with food hardships. Mismeasurement and misreporting of food insecurity and SNAP may also be affecting the observed relationship.

Previous Literature

Empirical studies examining the relationship between SNAP and food insecurity have generated equivocal results. Studies employ a variety of methods and find a mixture of results.[13] Some studies use unconditional associations between SNAP receipt and food insecurity and find a positive relationship (Cohen et al., 1999). For example, the latest national food insecurity report (Coleman-Jensen et al., 2012) estimates that, among households with income less than 130% of the federal poverty line, 52% of SNAP participant households reported being food insecure, while only 28% of non-participant households reported this condition. Several other studies use multivariate analyses that attempt to control for observable differences between participant and non-participant households, yet find positive associations (Alaimo, Briefel, Frongillo, & Olson, 1998; Ribar & Hamrick, 2003; Gibson-Davis & Foster, 2006).[14] In contrast, studies that use narrower and alternative participation comparisons generate negative associations (Kabbani & Kmeid, 2006; Gundersen & Gruber, 2001; Mykerezi & Mills, 2010).

Assuming Barrett's (2002) theoretical model is correct, the preponderance of counterintuitive findings indicates that selection must be coming from unobservable

[13] See Barrett (2002), Currie (2003), Fox, Hamilton, and Lin (2004), and Gregory et al. (2013) for comprehensive reviews of the literature.

[14] A notable exception is Bhattacharya and Currie (2001). They use data from the third National Health and Nutrition Examination Survey (NHANESIII) to estimates OLS models and find that SNAP participation is negatively associated with food insecurity. Unfortunately all of the authors' models simultaneously control for participation in the National School Breakfast (SBP) and National School Lunch Programs (NSLP).

household characteristics. Studies using longitudinal methods to account for time-invariant unobservable differences between participants and non-participants generate the same results as studies that control for selection on observables. Wilde and Nord (2005) use the longitudinal structure of the CPS-FSS to construct a two-year panel and estimate a household-level fixed-effect model. Greenhalgh-Stanley and Fitzpatrick (2013) also estimate fixed-effects models, using data on elderly households from the Health and Retirement Survey (HRS). Both studies find SNAP participation continues to be positively associated with food insecurity after controlling for household-level fixed effects. Wilde and Nord (2005) suggest time-varying unobservable household characteristics may be an additional source of bias.

Several studies use instrumental variables methods and endogenous latent variable models to control for contemporaneous unobservable characteristics, but generate inconclusive results. Borjas (2004) examines the relationship between public assistance (including SNAP) and food insecurity using the natural policy experiment that occurred when immigrant eligibility for public assistance programs was restricted by the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996. Borjas's findings are consistent with the anticipated negative association; however, the majority of his estimates are only marginally significant. Gundersen and Oliveira (2001) and Huffman and Jensen (2003) estimate endogenous latent variable models, but obtain imprecise and statistically insignificant results. Greenhalgh-Stanley and Fitzpatrick (2013) use data on elderly households from the HRS to estimate 2SLS models with household-level fixed effects. Their models generate estimates that are imprecise and insignificant. Schaefer and Gutierrez (2012) also estimate 2SLS models using data from three panels of the Survey of Income and Program Participation (SIPP) and obtain statistically insignificant results.

While the majority of studies find inconclusive results, studies using dummy endogenous variable models estimate strong negative associations. Yen, Andrews, Chen, and Eastwood (2008) uses state-level policy and stigma variables as exclusion restrictions to estimate a dummy endogenous Tobit model and finds SNAP participation is negatively associated with households' 30-day food insecurity Rasch scores; however, their analysis uses data from the 1996-1997 National Food Stamp Program Survey (NFSPS), which is a choice-based sample with an overrepresentation of SNAP participants.[15] Mykerezi and Mills (2010) also find a negative association between SNAP and food insecurity using data from the Panel Survey of Income Dynamics (PSID). They use state-level error rates in benefit payments as instrumental variables, but do not include state-level controls. This opens up the possibility their instruments may be capturing state-level characteristics other than error rates. Ratcliffe, McKernan, and Zhang (2011) and Schaefer and Gutierrez (2012) both use data from the SIPP to estimate bivariate probit models and obtain similar results. Schaefer and Gutierrez (2012) estimate models with and without exclusion restrictions with little change in their results, suggesting identification for this entire group of models may be obtained from functional form.

A shortcoming of many of the previous studies may be the measures of food insecurity. Nearly all of the studies rely on a single binary measure of food insecurity

[15] The authors address this issue by using sampling weights.

(food insecure vs. food secure). However, the USDA measures food security using a set of 18 questions from the HFSSM. These questions capture information about a variety of conditions, experiences, and behaviors related to food hardships. Information is lost when broad categories are created using responses to questions from the HFSSM. For example, consider two households: the first household affirms 3 questions, while the second household affirms all 18. Both households are considered food insecure, yet the latter household has a greater level of food insecurity. Discarding this information may be contributing to the preponderance of inconclusive results.

Data

Data for the empirical analyses come from the 2001-2008 December Current Population Survey Food Security Supplements (CPS-FSS). The CPS is the official source of government statistics on employment status and poverty. Approximately 60,000 households are interviewed each month with data collected on labor force participation status, income, household demographics, and state identifiers. After weighting, CPS households are representative at the state and national levels of the civilian, noninstitutionalized population.

The Food Security Supplement is conducted as a supplement to the CPS for the Economic Research Service (ERS) of the USDA. The purpose of the CPS-FSS is to estimate the prevalence of food insecurity in the U.S. Each year, ERS estimates national prevalence rates of food insecurity in its *Household Food Security in the United States* series.[16] It was first fielded in April 1995 and has since been administered every

[16] The most recent report in this series is Coleman-Jensen et al. (2012).

subsequent year.[17] After 2000, the CPS-FSS was administered in December. Consistently fielding the CPS-FSS in December increases the comparability of food insecurity estimates because the reporting of food hardship will likely vary by month. The 2009-2012 CPS-FSS waves are not used because of changes to SNAP under the American Recovery and Reinvestment Act (ARRA) of 2008, which made the program less comparable to previous years. Households are asked questions about their food expenditures and basic food needs. To reduce respondent burden, households with income above 185% of the federal poverty line and households that show no signs of food stress[18] are not asked the food security questions. This screen is also applied to the food assistance program participation questions.

Dependent Variables

The empirical analyses examine responses to a series of food hardship questions taken from the HFSSM as dependent variables. The HFSSM asks all households 10 questions and asks households with children an additional eight questions. These questions elicit information to determine whether or not household members experienced difficulty meeting basic food needs. The severity of hardships experienced by the household ranges from anxiety over food running out to shortages of the amounts and

[17] The CPS-FSS was fielded in April 1995, September 1996, April 1997, August 1998, April 1999, September 2000, April 2001, and December 2001-present.

[18] The following preliminary screening questions are asked to determine if a household shows signs of food-access problems:

1. People do different things when they are running out of money for food in order to make their food or their food money go further. In the past 12 months, since December of last year, did you ever run short of money and try to make your food or your food money go further?
2. Which of these statements best describes the food eaten by your household-enough of the kinds of food we want to eat, enough but not always the kinds of food we want to eat, sometimes not enough, or often not enough to eat?

kinds of foods to episodes of adults and children going without food for an entire day. All of the questions refer to the previous 12 months and are framed in terms of either shortages of money or affordability. A complete listing of the questions and the methods used to convert them into binary indicators is in Appendix A.

The descriptive analysis and some alternative specifications use the household's food security status as a dependent variable. A household's food security status is determined by summing the affirmed responses from the HFSSM. Households are classified as food insecure if they affirm three or more items. Food insecure households may be further classified as having either low food security or very low food security. Households that affirm two or fewer items are classified as food secure. Childless households that affirm three to five items and households with children that affirm three to seven items are classified as experiencing low food security. These households report multiple indications of food access problems; however, there is little, if any, indication of reduced food intake. Households that affirm additional items (six or more for households without children and eight or more for households with children) are classified as experiencing very low food security. Common experiences for households with very low food security include reduced food intake for one or more members and disrupted eating patterns.

Explanatory Variables

The principal explanatory variable is an indicator of receipt of SNAP benefits within the past 12 months, where the indicator equals one if anyone in the household participated in SNAP, zero otherwise. In alternative specifications, the annual SNAP measure is replaced with an indicator of SNAP receipt within the past 30 days. The 30-day SNAP measure is useful because it facilitates comparison with previous studies that use this measure as their primary explanatory variable. Some specifications also include measures of participation in the National School Breakfast Program (SBP), National School Lunch Program (NSLP), Special Supplemental Program for Women, Infants, and Children (WIC), local food pantries, and soup kitchens. These variables describe additional resources that are available to households and provide additional controls for household behaviors not addressed by other household characteristics. Including information on participation in multiple food assistance programs may control for additional heterogeneity, such as community food resources, preferences for private versus public food assistance, and stigma. However, caution should be exercised when interpreting these coefficients because households self-select into these food assistance programs.

One potential weakness of the CPS-FSS is the underreporting of SNAP participation. Estimates suggest the CPS underreports SNAP participation by up to 50% (Parker, 2011; Wheaton, 2008); however, these estimates are based on the March CPS Supplement and not the CPS-FSS. The direction of bias will depend on the nature of the misreporting. If households with greater (less) food hardships are less (more) likely to report participation for reasons unobservable to the researcher, then the effect of SNAP participation on food insecurity will be biased downwards (upwards).

Another potential weakness of the CPS-FSS is that total household income is reported categorically and not continuously. Categorical income is converted into a continuous measure using the midpoints of the income ranges and adjusted for inflation using the Consumer Price Index for Urban Consumers (CPI-U). Household income is directly useful as a measure of short-term household resources and indirectly valuable as a potential control for SNAP eligibility.

In addition to weak income measures, the CPS-FSS also provides very little information on household assets. CPS-FSS respondents were asked if the household's current living quarters was owned or being bought by a household member. I use their responses to construct a homeownership status indicator. This measure describes the long-term economic resources of a household.

Additional controls include other demographic, geographic, and economic characteristics. These measures include the respondent's gender, age (and age-squared), race, ethnicity, nativity, marital status, employment status, and educational attainment. The analysis also includes measures of the number of adults, children, and disabled members; age of the youngest member (households with children); an indicator for the presence of an elderly member (age 60 or older); residence in an urban area; the state unemployment rate; and state and year fixed-effects. Means and standard deviations for the explanatory variables are in Appendix C.

Instrumental Variables

Estimation of the effectiveness of SNAP in reducing food insecurity is complicated by the endogeneity of SNAP. This problem is addressed by estimating a behavioral Rasch selection model. Changes in state-level policies related to SNAP are used to identify the model. Information on state-level policies is obtained by linking the food stamp rules database with the CPS-FSS using state identifiers and year. The food stamp rules database was first compiled by the Urban Institute (Finegold, Margrabe, & Ratcliffe, 2006) and updated by researchers at the ERS. The database provides a rich set of information on state-level policies and rules related to SNAP and other public assistance programs. From this database, I identified four instrumental variables for the empirical analyses related to vehicle asset rules, outreach spending, and recertification periods. I also constructed a measure that captures immigrant eligibility rules using household head's citizenship status from the CPS-FSS.

The first instrumental variable is a measure of vehicle asset rules for SNAP. Beginning in 2001, states were given flexibility with respect to how vehicles are treated in the asset test. States now have the option of using the SNAP vehicle deduction,[19] exempting some vehicles, or exempting all vehicles from the asset test. States with more generous vehicle asset rules are expected to have higher SNAP participation rates because they are effectively removing the asset test by excluding the value of vehicles. Similarly, categorical eligibility removes the asset test for eligibility purposes.[20] To capture the generosity of state SNAP vehicle asset rules, I constructed a binary measure

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[19] The SNAP vehicle deduction excludes the first $4,650 of a vehicle's fair market value while any excess is applied to the asset test.

[20] I tested specifications that included broad based categorical eligibility (BBCE), but it was found to be a weak predictor of the SNAP participation decision.

that equals one if a state exempts all vehicles from the asset test, zero otherwise.[21] This captures the most generous form of vehicle asset rules.

The second instrumental variable is a measure of outreach activities. A priori, higher levels of outreach are expected to increase SNAP participation rates. Outreach activities are intended to provide information to persons who may not be aware they are eligible for SNAP benefits and help current participants maintain their participation. Outreach activities include public service announcements, informational brochures, and projects designed to increase retention rates and simplify the application process. I constructed a measure of outreach spending per capita that is adjusted for inflation and lagged 12 months.

The third instrument is a measure of the state's recertification period for SNAP households with earnings. Periodically, households must show they continue to meet requirements for program eligibility. The recertification period varies by state, typically ranging from three months to one year. Studies have shown the length of the recertification period has a significant effect on SNAP participation rates (Ratcliffe, McKernan, & Finegold, 2008; Ribar, Edelhoch, & Liu, 2008, 2010). Shorter recertification rates increase the transaction costs of SNAP (e.g., travelling to a program office, filling out paperwork, etc.), reducing the likelihood of participation. I constructed a measure of the state's recertification period using the median recertification period.[22]

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[21] Alternative specifications were tested using the following measures of vehicle asset rules: any vehicle exemptions, one vehicle exempt per SNAP unit, one vehicle exempt per adult in SNAP unit, and all vehicles exempt. Combinations were considered were appropriate.

[22] In alternative specifications, I examined the percentage of caseloads with a 1-3 month recertification period.

The final instrument is a measure of immigrant eligibility. With the passage of the Personal Responsibility and Work Opportunity (PROWA) Act of 1996, immigrants were effectively ineligible for SNAP. Therefore, noncitizen immigrants are much less likely to participate in SNAP. I constructed a measure of immigrant eligibility using the household head's citizenship status. Specifically, I constructed an indicator that equals one if the head is not a U.S. citizen, zero otherwise. While this measure has proven strong in previous studies (Borjas, 2004; Ratcliffe et al., 2011), it may be questionable on theoretical grounds, as cultural differences and assimilation into a new culture could alter how respondents report food hardships.

Analysis Sample

The paper considers households with incomes below 130% of the federal poverty line. Restricting the sample using this income threshold approximates the gross income test for SNAP eligibility. At the same time, it increases the comparability of SNAP participants and non-participants. The descriptive and empirical analyses are adjusted for nonresponse and the complex survey design of the CPS-FSS using weights. The initial sample consists of 59,247 households. Households residing in Alaska and Hawaii are dropped from the sample because market prices and program benefits differ significantly from households located in the contiguous U.S. This reduces the sample to 58,001 households. An additional 1,706 households are dropped because they were asked experimental food security questions in 2007, reducing the sample to 56,295 households. Households failing to provide usable responses to one or more of the questions used to form the explanatory variable are excluded, leaving a final analysis sample of 54,298 households.

Households are disaggregated into four mutually exclusive subsamples: unmarried parent households with children under age 18, married parent households with children under age 18, households consisting entirely of members who are age 60 or older, and other adult-only households. These household types differ in their susceptibility to food hardships, are subject to different eligibility requirements under SNAP, and are differently eligible for other types of public assistance. Disaggregating households this way increases the comparability of households within groups.

Table 6 lists the proportions of low-income households experiencing food insecurity and those experiencing very low food security, calculated separately for SNAP participants and non-participants in each of the four analysis subsamples. Among the subsamples of low-income households, food insecurity is the highest among unmarried parent households (46%), followed by married parent households (37.8%), other adult-only households (33.4), and all-elderly households (18.1%). These findings are consistent with the most recent national food security report (Coleman-Jensen et al., 2012). Contrary to the food insecurity results, the prevalence of very low food insecurity is highest among other adult-only households (17.3%), followed by unmarried parent households (14.4%), married parent households (9.6%), and all-elderly households (7.2%).

Table 6

Food Hardships and SNAP Participation for Households with Income Less Than 130% of the Federal Poverty Threshold

	All Households	SNAP Participants	SNAP Non-Participants
	Unmarried Parents		
Food Insecure v. Low Food Security	46.0 (%) 14.4	53.5*** (%) 17.3***	37.7 (%) 11.2
N	12,918	6,850	6,068
	Married Parents		
Food Insecure v. Low Food Security	37.8 9.6	54.4*** 15.8***	31.5 7.2
N	9,317	2,591	6,726
	All Elderly		
Food Insecure v. Low Food Security	18.1 7.2	37.9*** 16.8***	14.3 5.3
N	13,417	2,187	11,230
	Other Adults		
Food Insecure	33.4	56.2***	27.3
V. Low Food Security	17.3	32.2***	13.3
N	18,646	4,214	14,432

Note. Means estimated using weighted household data from the 2001-2008 CPS-FSS. Differences in means were tested using *t*-tests.
* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

Comparisons of the proportion of households experiencing food insecurity for SNAP participants and non-participants within subsamples reveal higher rates of food insecurity among SNAP participants. For example, 53.5% of unmarried parent households participating in SNAP are food insecure, while only 37.7% of non-participating unmarried parent households experience this condition. Similarly, the proportion of households experiencing very low food security is highest among SNAP participants for all four subsamples. These results are consistent with the pervious literature when examining bivariate associations.

SNAP participation is the highest among unmarried parent households (53%) and lowest among all-elderly households (16%). Approximately one quarter of married parent households (28%) and other adult-only households (23%) participate in SNAP. Previous studies have consistently found SNAP participation to be the highest among households with children and the lowest among elderly households.

Econometric Specification

The USDA uses a Rasch (1960) measurement model to relate responses to HFSSM questions to a single underlying latent trait, food security. The Rasch model is a psychometric model from the field of Item Response Theory (IRT). The central idea behind the Rasch model is that multiple outcomes that can be observed (i.e., reports of food hardships) all derive from a single underlying variable, such as food security. Let household i's underlying continuous index of food hardship be denoted by θ_i, with the property that higher values of the index correspond to greater levels of hardship. While the researcher is unable to observe θ_i directly, suppose he has j continuous indicators, Y_{ij}^*, that are related to θ_i such that each depends on the index and some random measurement error, ν_j. The relationship between the observable indicators and the underlying latent index can be expressed as

$$Y_{ij}^* = \theta_i + \nu_j. \tag{1}$$

Equation (1) describes a factor analytic relationship in which the hardship index, θ_i, is the underlying factor and the factor loadings (discrimination parameters) are constrained to be equal across all items and normalized to one. While this may appear to be a strong assumption, Hamilton et al. (1997) found that most items in the HFSSM had similar factor loadings when they are allowed to vary, suggesting this assumption will only alter the scaling of model parameters.

Up to this point, the model has been expressed in terms of a set of continuous indicators of food hardships (the Y_{ij}^* variables); however, the observed indicators are discrete variables. The Rasch model assumes the continuous indicators, Y_{ij}^*, are related to the binary responses as follows:

$$Y_{ij} = \begin{cases} 1 & \text{if} \quad Y_{ij}^* > \delta_j \\ 0 & \text{if} \quad Y_{ij}^* \leq \delta_j \end{cases} \tag{2}$$

where δ_j is the threshold (calibration) parameter. The above specification of the relationship between the latent continuous indicators and the observed categorical responses is the same used in standard probit and logistic models. The thresholds (δ_j) are estimated as part of the multivariate model and take on different values for each type of food hardship. Higher values of the thresholds indicate items that capture greater severity of food hardships. Given equations (1)-(2) and the assumption that the random measurement error (ν_j) is distributed logistically, the probability that household i's respondent answers affirmatively to the jth food hardship item is

$$P\left(Y_{ij}=1\,|\,\theta_i,\delta_j\right)=\frac{\exp\left(\theta_i-\delta_j\right)}{1+\exp\left(\theta_i-\delta_j\right)}\ , \tag{3}$$

where exp ( ) is the exponential function.

The Rasch model assumes the errors in the responses are conditionally independent. This implies that the probability of an affirmative response to a given hardship question for a given value of θ_i (the latent trait) does not depend on the response to another question. Of the 18 items that constitute the food security scale, 3 are follow-ups of two-part items. Opsomer, Jensen, Nusser, Drignei, and Amemiya (2002) point out items with follow-ups often violate the assumption of conditional independence. Nord (2012) estimates Rasch models that directly model the structure of the follow-up questions using CPS-FSS data and finds that differences are negligible.

The conditional independence assumption implies the conditional probability of a given response vector is the product of the probabilities for each item. By stacking the households' responses (Y_{ij}) into a vector Y_i, the probability of observing a given response pattern is given by

$$P\left(Y_i=y_i\,|\,\theta_i,\delta_j\right)=\prod_{j=1}^{J}\left[\frac{\exp\left(\theta_i-\delta_j\right)}{1+\exp\left(\theta_i-\delta_j\right)}\right]^{Y_{ij}}\left[1-\frac{\exp\left(\theta_i-\delta_j\right)}{1+\exp\left(\theta_i-\delta_j\right)}\right]^{\left(1-Y_{ij}\right)} \tag{4}$$

where j runs over the 10 adult items for households without children and over all 18 items for households with children.

A useful property of equation (4) is that the conditional probability of a given response pattern can be factored using the count of affirmative responses (raw score).[23] This suggests that a household's food security status can be ranked and compared using simple counts of the affirmed items. Assuming households' responses to the food hardship items follow the Rasch model and households are asked and answer all J food hardship items, there are J+1 (0,1,…,J) potential values for θ_i that can be identified. Under the assumptions of the Rasch model, the count of affirmative items is a minimal sufficient statistic for θ_i.

Another attractive property of the Rasch model is its ability to compare food security scores among households who are asked different subsets of questions. For example, the food security scale consists of 18 items for households with children, but only 10 items for households without children. The 19 food security scores for households with children and 11 food security scores for households without children can be estimated and compared to determine a household's food security status. An additional benefit of this property is the ability to account for missing data on the food security items. As long as households provide a valid response to at least one of the food security items, their food security score can be computed and compared to other households.

[23] See the technical appendix to Wilde (2007) for a complete derivation of this factorization.

Behavioral Rasch Model

For my empirical analyses, household characteristics are incorporated into the Rasch model using a Generalized Linear Mixture Model (GLMM). Specifically, I re-express the hardship index as

$$\theta_i = \beta_s S_i + \beta_X{}' X_i + e_i, \tag{5}$$

where S_i is a SNAP participation indicator, X_i is a vector of observable control variables related to food insecurity, β_S is a scalar coefficient, β_X is a matrix of coefficients, and e_i is a random variable that is normally distributed with mean zero and unknown variance σ^2. At first glance, the error-component distributional assumption may appear to be an overly restrictive; however, this distribution represents the remainder of household heterogeneity after controlling for observable household characteristics and state and year fixed effects, not food insecurity itself (Opsomer, Jensen, & Pan, 2003).

Combining equations (1)-(3), (5), and the conditional independence assumption, the probability of observing a given response pattern may be re-expressed as

$$P\left(Y_i = y_i \mid S_i, X_i, u_i, e^*{}_i\right) = \int_{-\infty}^{\infty} \prod_{j=1}^{J} \frac{\exp\left(q_{ij}\left(\beta_s S_i + \beta_X{}' X_i + e_i - \delta_j\right)\right)}{1 + \exp\left(q_{ij}\left(\beta_s S_i + \beta_X{}' X_i + e_i - \delta_j\right)\right)} \frac{1}{\sigma} \varphi\left(\frac{e_i^*}{\sigma}\right) de_i^*, \tag{6}$$

where j runs over the 10 adult items for households without children and over all 18 items for households with children, $q_{ij} = 2Y_{ij} - 1$, and $\varphi(\ )$ is the standard normal probability density function (pdf). Assuming observations are independent, the likelihood function is the product of the probabilities of observing a given response pattern for all observations. This model will serve as the baseline specification for the empirical analyses.

Endogenous Behavioral Rasch Model

Estimation of the causal effect of SNAP benefit receipt on food insecurity is complicated by the endogeneity of the SNAP participation decision. Baseline behavioral Rasch models attempt to control for selection on observables, but fail to account for selection on unobservables. The household's SNAP participation decision is modeled as

$$S_i = \begin{cases} 1 \text{ if } \alpha_X{}' X_i + \alpha_Z{}' Z_i + u_i > 0 \\ 0 \text{ otherwise} \end{cases} \tag{7}$$

where S_i is defined above, X_i represents observable confounders, Z_i is a set of instrumental variables, α_x and α_z are coefficient matrices, and u_i is a stochastic error-component that is standard normally distributed. The resulting model is consistent with a probit model for the decision to participate in SNAP.

Following Terza (2009), I assume the error-component specified in equation (5) can be decomposed into u_i and e^* such that $e_i = \lambda u_i + e_i^*$. As a result, the food hardship index is now

$$\theta_i^* = \beta_s S_i + \beta_X{}' X_i + \lambda u_i + e_i^*, \tag{8}$$

where λ is an unknown parameter to be estimated, and e_i^* represents the new random-effect after controlling for observed and unobserved heterogeneity. The random-effect, e_i^*, is normally distributed with mean zero and unknown variance η^2. The error-component, u_i, generates correlation between the participation variable (S_i) and the food hardship indicators through λ. If λ is nonzero, u_i influences the household's selection into SNAP and the likelihood of affirming food hardship conditions, rendering the baseline model inconsistent.

Equations (6)-(8) imply the following likelihood function for a sample of size N:

$$\begin{aligned} L\left(\alpha,\beta,\delta,\eta^2,\lambda\right) = \prod_{i=1}^{N} \Bigg\{ & S_i \int_{-\alpha_X' X_i - \alpha_Z' Z_i}^{\infty} \int_{-\infty}^{\infty} \prod_{j=1}^{J} \left[\frac{\exp\left(q_{ij}\left(\theta_i^* - \delta_j\right)\right)}{1+\exp\left(q_{ij}\left(\theta_i^* - \delta_j\right)\right)} \right] \frac{1}{\eta} \varphi\left(\frac{e_i^*}{\eta}\right) de_i^* \varphi\left(u_i\right) du_i \\ & + \left(1-S_i\right) \int_{-\infty}^{-\alpha_X' X_i - \alpha_Z' Z_i} \int_{-\infty}^{\infty} \prod_{j=1}^{J} \left[\frac{\exp\left(q_{ij}\left(\theta_i^* - \delta_j\right)\right)}{1+\exp\left(q_{ij}\left(\theta_i^* - \delta_j\right)\right)} \right] \frac{1}{\eta} \varphi\left(\frac{e_i^*}{\eta}\right) de_i^* \varphi\left(u_i\right) du_i \Bigg\} \end{aligned} \tag{9}$$

where the 'i' subscript denotes the i^{th} sample household (i = 1, …, N). All of the model parameters are estimated simultaneously. The parameter estimate for λ is a factor loading parameter with the property that its nullity is a sufficient statistic for the exogeneity of the program participation variable. For estimation purposes, I conduct a line search with respect to the most troublesome parameter (λ in the present model) from -5 to 5 by increments of 1. All parameters are estimated conditional on the value of λ. The value of λ that yields the best fit (in terms of the log-likelihood) is used as its starting value in the unrestricted model. This ensures the results represent a global maximum.

The estimated SNAP coefficient describes how participation affects the food hardship index. However, the current study is interested in SNAP's effectiveness in reducing food insecurity. Causal inferences of SNAP's effect on food insecurity are based on the average treatment effect (marginal effect):

$$\text{ATE} = \frac{1}{N}\sum_{i=1}^{N}\int_{-\infty}^{\infty}\int_{-\infty}^{\infty}\left[P\left(Y_i \geq 3 \mid S_i = 1, X_i, u_i, e^*_i\right) - P\left(Y_i \geq 3 \mid S_i = 0, X_i, u_i, e^*_i\right)\right]\frac{1}{\eta}\varphi\left(\frac{e_i^*}{\eta}\right)de_i^*\varphi\left(u_i\right)du_i \quad (10)$$

where $P(Y_i \geq 3|S_i = 1, X_i, u_i, e_i^*)$ denotes food insecurity if anyone in the household participates in SNAP, and $P(Y_i \geq 3|S_i = 0, X_i, u_i, e_i^*)$ denotes food insecurity if no one in the household participates in SNAP. Thus, the average treatment effect describes how the probability of food insecurity would differ if all households participate in SNAP versus the probability if none of the households participate. The average treatment effect is calculated using simulation methods.

The principal advantage of Terza's (2009) framework is flexibility to account for contemporaneous unobservable confounders in the behavioral Rasch model. Previous studies that estimate dummy endogenous variable models rely on the assumption that the error-components are bivariate normally distributed (Mykerezi & Mills, 2010; Ratcliffe et al., 2011; Shaefer & Gutierrez, 2012; Yen et al., 2008). Terza's framework does not require the bivariate normality assumption, relying instead on separate error-component assumptions for the outcome and switching equations.

A disadvantage of the current approach is that it makes strong and potentially incorrect assumptions about functional form. If the data are not generated according to the probit and logistic models specified, then the analysis will suffer from misspecification bias. An additional disadvantage is the model's reliance on instrumental variables for identification. Therefore, interpretation of the results is conditional on the validity of the instrumental variables. I test the sensitivity of my results to the instrumental variables by estimating models with and without specific instrumental variables.

Results

Estimation results from alternative specifications of the behavioral Rasch model are listed in Table 7. The top panel contains results from the baseline specification of the behavioral Rasch model that attempts to control for selection on observables. The bottom panel lists results from a behavioral Rasch model that corrects for selection on observables and unobservables. The columns report coefficient estimates, standard errors, and average marginal effects for the four subsamples: unmarried parent households, married parent households, all-elderly households, and other adult-only households.

The first rows of the top panel report estimates from behavioral Rasch models with no controls. This is the Rasch model equivalent of unconditional differences in average food insecurity between SNAP participants and non-participants. All of the marginal effects are strongly positive and consistent with the descriptive analysis findings and previous studies such as Coleman-Jensen et al. (2012). The marginal effects are largest for other adult-only households and smallest for unmarried parent households.

Table 7

Estimates of the Effect of SNAP Participation on Food Insecurity for Households with Income Less Than 130% of the Federal Poverty Threshold

	Unmarried Parent	Married Parent	All Elderly	Other Adults
Behavioral Rasch Models				
No Controls	1.373***	2.084***	3.755***	3.610***
	(0.064)	(0.087)	(0.121)	(0.090)
	0.160	*0.226*	*0.224*	*0.285*
Standard Controls[a]	1.314***	1.883***	2.539***	2.587***
	(0.067)	(0.090)	(0.114)	(0.094)
	0.160	*0.210*	*0.166*	*0.209*
Standard and Economic Controls[b]	1.190***	1.742***	2.472***	2.421***
	(0.071)	(0.094)	(0.119)	(0.096)
	0.141	*0.196*	*0.147*	*0.193*
Standard, Economic, and Other Assistance Controls[c]	0.689***	0.931***	2.072***	1.598***
	(0.069)	(0.092)	(0.121)	(0.093)
	0.077	*0.101*	*0.119*	*0.126*
Corrected Behavioral Rasch Models				
Standard and Economic Controls[d]	4.170***	-1.108***	-1.993***	-2.725***
	(0.278)	(0.322)	(0.293)	(0.337)
	0.420	*-0.111*	*-0.083*	*-0.162*

Note. Models estimated using weighted household data from the 2001-2008 CPS-FSS.

[a] Standard controls include households head's gender, age, age-squared, race, ethnicity, nativity, marital status (HHs without children), education; number of adults, children, and disabled members in household; age of the youngest member (HHs with children); elderly members, residence in an urban area; state unemployment rate, and state and year fixed effects.

[b] Economic controls include household head's employment status, log of household income, and home ownership.

[c] Other assistance controls include participation in SBP, NSLP, and WIC (households with children) and use of food pantries and soup kitchens.

[d] Instrumental variables include annual outreach per non-SNAP participant, state median recertification period, vehicle asset rules, and head's citizenship status. Standard errors are in parentheses and average marginal effects are in italics.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

The second rows report estimates from specifications that add controls for the household respondent's gender, age, age-squared, race, ethnicity, nativity, marital status (households without children), education, number of adults, number of children, and the number of disabled members; age of the youngest member (households with children); presence of an elderly member; residence in an urban area; state unemployment rate; and state and year fixed-effects. Adding these controls reduces substantially the marginal effects for households without children, but only slightly for households with children. Yet the marginal effects remain positive and highly significant, indicating SNAP receipt is associated with greater food hardships, including food insecurity.

The third rows report estimates from specifications that also add controls the respondent's employment status, the natural log of household income, and home ownership status. The use of these controls continues to attenuate the association between SNAP receipt and food insecurity. The marginal effects, while smaller with the inclusion of additional controls, continue to indicate that SNAP is associated with food hardships. Finally, the last row of the top panel adds controls for SBP, NSLP, and WIC for households with children and food bank and soup kitchen use for all households. Even after controlling for alternative food assistance programs, the association between SNAP receipt and food insecurity remains positive and significant. When compared to specifications with no controls, adding the full set of controls reduces the marginal effects by approximately 50% for all subsamples; however, they remain positive. These findings are consistent with the findings of Alaimo et al. (1998), Ribar and Hamrick (2003), and Gregory et al. (2013).

The bottom panel of Table 7 lists estimates from behavioral Rasch models that control for selection on observables and unobservables. After controlling for unobserved heterogeneity, the marginal effects change direction from positive to negative for married parent, all-elderly, and other adult-only households, while the marginal effect remains positive for unmarried parent households. All of the coefficients are highly significant (at the 1% level or better). For unmarried parent households, adjusting for sample selection bias has made the SNAP coefficient and marginal effect more positive. After controlling for unobserved heterogeneity, SNAP receipt is associated with a 42 percentage point increase in the probability of food insecurity among unmarried parent households, while the baseline specification estimates a 7.7 percentage point increase. This unexpected result is likely being generated by additional unobserved heterogeneity for unmarried parent households. These households likely have a complex structure that is not being captured by the model, as the group includes single parent households, cohabiting parent (unmarried) households, and households with other family members. However, a second explanation may be that the instruments are not valid for this group.

In addition, the Barrett model may also provide some theoretical reasons for why I am finding this counterintuitive result. Unlike other types of households, unmarried parent households do not have multiple incomes, which translates into fewer resources to purchase food. Households with children also have greater food requirements. Fewer resources and greater food needs increase food hardships. When unmarried parent households do work, they are more likely to require childcare, which reduces the effectiveness of their labor market resources. Unmarried parent households may also be more vulnerable to losses of benefits because they have less time to comply with recertification requirements. For example, married parent households have at least two adults who can work, care for children, and comply with program requirements, while unmarried parent households have a single adult with the same responsibilities.

All other subsamples' marginal effects are consistent with the expected result that SNAP receipt reduces food insecurity after controlling for unobserved heterogeneity. The ameliorative effect of SNAP is the largest for other adult-only households and smallest for all-elderly households. For other adult-only households, SNAP receipt reduces the probability of food insecurity by 16.2 percentage points. Approximately one-quarter of other adult-only households receive SNAP benefits, suggesting SNAP has a substantial effect on low-income other adult-only households. This result can be placed into context using the estimated marginal effect and descriptive statistics to convert the percentage point decline into a percentage decline in food insecurity. Recall 56.2% of other-adult only households participating in SNAP are food insecure (Table 6). The corrected behavioral Rasch model results suggest that 72.4% of other adult-only households would be food insecure without SNAP benefits, suggesting SNAP reduces food insecurity among these households by 22.4%. While other studies do not specifically examine food insecurity among other adult-only households, the results are similar to previous studies that examine adult-only households.

After controlling for unobserved heterogeneity, SNAP receipt reduces the probability of food insecurity among married parent and all-elderly households by 8 to 11 percentage points. Nearly one-third of married parent households and one-fifth of all-elderly households participate in SNAP. For married parent households that participate in SNAP, 54.4% are food insecure. Based on the corrected behavioral Rasch model results, food insecurity among married parent households would be 11.1 percentage points higher (65.5%) if no SNAP benefits were available. This suggests that SNAP receipt reduces food insecurity among married parent households by 17%. The corrected behavioral Rasch model results also suggest that 46.2% of all-elderly households would be food insecure without SNAP, which corresponds to an 18 percentage point decline in food insecurity among all-elderly households. These results are consistent with previous studies, such as Ratcliffe et al. (2011) and Shaefer and Gutierrz (2012). Table 8 lists additional coefficients and standard errors for the SNAP participation and food insecurity equations from the corrected behavioral Rasch model. This is the same model listed in the bottom panel of Table 7. The columns report estimates separately for the four subsamples described above. SNAP receipt coefficients are listed fist, followed by the instrumental variables, respondent and household demographic characteristics, economic characteristics, calibration parameters, and the error-components.

The second panel of Table 8 lists coefficients for the instrumental variables. The first instrumental variable is a measure of the state outreach activities per capita, lagged 12 months. All of the coefficients are of the expected direction (positive) with the exception of married parent households; however, it is imprecisely estimated. Outreach activity is a significant predictor of program participation for other adult-only households (at the 5% level), which is surprising because outreach activities are also specifically targeted at elderly households. Perhaps, other adult-only households are more likely to be influenced by the information provided by outreach activities. The second instrument is all vehicles are exempt from the asset test. It is positive and significant (at the 10% level) for married parent households. For all other subsamples, it is imprecisely estimated. The predictive power of the vehicle asset rules may be limited by the decision to restrict the sample to households with income less than 130% of the federal poverty line.

The third instrumental variable is the state median recertification rate. Recertification period coefficients are positive and significant (at the 5% level) for unmarried parent and all-elderly households, suggesting these households are more sensitive to the costs associated with recertification of SNAP eligibility. Potential reasons for this increased sensitivity to recertification may be limited mobility or higher opportunity costs. The fourth and final instrument is an indicator for the household head being a non-citizen immigrant. The immigrant coefficients are negative and significant for all subsamples with the exception of all-elderly households.

To determine the overall predictive power of the set of instruments for the SNAP participation decision, I conducted a Wald test of their joint significance. The *p*-values for these tests are listed at the bottom of Table 8. The tests indicate that the set of instruments are highly jointly significant (at the 1% level or better) for unmarried and married parent households, and other adult-only households. For all-elderly households, the Wald test indicates that the set of instruments are only marginally jointly significant (at the 10% level).

Table 8

Corrected Behavioral Rasch Model Coefficient Estimates for Households with Income Less Than 130% of the Federal Poverty Threshold

	Unmarried Parent		Married Parent		All Elderly		Other Adult	
	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity
SNAP Participation, Past 12 Months		4.170*** (0.278)		-1.108*** (0.322)		-1.993*** (0.293)		-2.725*** (0.337)
Instrumental Variables								
Real Annual Outreach Per Cap, L1	0.003 (0.030)		-0.019 (0.044)		0.027 (0.034)		0.065** (0.027)	
All Vehicles Exempt	-0.021 (0.056)		0.135* (0.077)		0.061 (0.068)		-0.020 (0.052)	
State Median Recertification Period	0.015** (0.008)		0.015 (0.010)		0.022** (0.009)		0.000 (0.007)	
Head is a Non-Citizen, Immigrant	-0.240*** (0.057)		-0.167*** (0.054)		-0.052 (0.079)		-0.318*** (0.060)	
Head Demographic/HH Characteristics								
Female	0.215*** (0.035)	-0.118 (0.099)			0.147*** (0.033)	0.017 (0.117)	0.224*** (0.023)	0.557*** (0.087)

Table 8

(Cont.)

	Unmarried Parent		Married Parent		All Elderly		Other Adult	
	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity
Head Demographic/HH Characteristics (cont.)								
Age	-0.003 (0.007)	0.155*** (0.020)	-0.039*** (0.010)	-0.000 (0.027)	0.343*** (0.058)	0.804*** (0.221)	0.077*** (0.005)	0.325*** (0.020)
Age Squared	-0.000 (0.000)	-0.002*** (0.000)	0.000*** (0.000)	-0.000 (0.000)	-0.003*** (0.000)	-0.007*** (0.002)	-0.001*** (0.000)	-0.004*** (0.000)
Black	0.198*** (0.030)	-0.205** (0.085)	-0.027 (0.050)	0.290** (0.135)	0.164*** (0.039)	1.699*** (0.145)	0.269*** (0.029)	1.058*** (0.114)
Other	0.080 (0.058)	-0.085 (0.163)	0.028 (0.061)	0.171 (0.164)	0.159** (0.080)	0.422 (0.285)	0.159*** (0.052)	0.212 (0.179)
Hispanic	0.010 (0.041)	0.064 (0.120)	-0.064 (0.049)	0.411*** (0.138)	0.163*** (0.059)	0.601*** (0.209)	0.229*** (0.044)	0.664*** (0.153)
Married, Spouse Present					-0.188** (0.088)	0.079 (0.338)	-0.085** (0.036)	-0.252** (0.128)
Some College	-0.160*** (0.028)	-0.122 (0.079)	-0.188*** (0.035)	-0.190** (0.096)	-0.269*** (0.032)	-1.040*** (0.119)	-0.167*** (0.027)	-0.711*** (0.102)

Table 8

(Cont.)

	Unmarried Parent		Married Parent		All Elderly		Other Adult	
	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity
Head Demographic/HH Characteristics (cont.)								
College Graduate	-0.372*** (0.068)	-0.240 (0.197)	-0.483*** (0.071)	-1.613*** (0.193)	-0.161** (0.067)	-1.870*** (0.259)	-0.577*** (0.048)	-2.663*** (0.166)
Immigrant	-0.236*** (0.054)	0.040 (0.123)	-0.284*** (0.058)	-0.662*** (0.124)	0.213*** (0.058)	-0.246 (0.196)	-0.154*** (0.051)	-0.920*** (0.144)
Number of Adults	-0.056*** (0.016)	0.044 (0.046)	-0.006 (0.022)	0.109** (0.055)	-0.103 (0.078)	-0.965*** (0.311)	-0.008 (0.017)	-0.184*** (0.057)
Number of Children	0.213*** (0.013)	-0.100** (0.039)	0.185*** (0.013)	0.239*** (0.042)				
Number of Disabled	0.344*** (0.032)	0.150* (0.090)	0.443*** (0.035)	1.092*** (0.103)	0.417*** (0.033)	1.821*** (0.135)	0.449*** (0.021)	1.440*** (0.100)
Age Youngest HH	-0.025*** (0.003)	0.076*** (0.009)	-0.017*** (0.004)	0.009 (0.011)				
Elderly HH Member	-0.069 (0.103)	-0.295 (0.292)	-0.142 (0.126)	-0.176 (0.334)			0.074 (0.065)	-0.118 (0.242)
Urban HH	-0.044 (0.033)	0.190** (0.092)	-0.003 (0.041)	0.505*** (0.112)	-0.072** (0.035)	0.355*** (0.129)	-0.149*** (0.030)	-0.067 (0.109)

Table 8

(Cont.)

	Unmarried Parent		Married Parent		All Elderly		Other Adult	
	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity
Economic Characteristics								
State Unemployment Rate	0.027 (0.026)	0.080 (0.070)	0.029 (0.032)	0.227** (0.090)	-0.087*** (0.031)	0.260** (0.115)	0.024 (0.024)	0.274*** (0.085)
Head Employed	-0.346*** (0.027)	0.064 (0.086)	-0.263*** (0.032)	-0.623*** (0.095)	-0.614*** (0.069)	-1.502*** (0.219)	-0.443*** (0.028)	-1.016*** (0.106)
LN Real Total HH Income ($10,000)	-0.418*** (0.019)	0.376*** (0.061)	-0.357*** (0.025)	-0.385*** (0.089)	-0.301*** (0.031)	-0.534*** (0.119)	-0.152*** (0.019)	-0.356*** (0.069)
Own Home	-0.418*** (0.031)	-0.184** (0.093)	-0.495*** (0.034)	-0.984*** (0.109)	-0.630*** (0.031)	-1.497*** (0.129)	-0.471*** (0.028)	-1.597*** (0.119)
Calibration Parameters								
δ_1		-10.270*** (0.157)		-10.739*** (0.270)		-8.071*** (0.130)		-7.124*** (0.070)
δ_2		-9.164*** (0.156)		-9.695*** (0.269)		-7.299*** (0.128)		-6.372*** (0.068)
δ_3		-8.003*** (0.155)		-8.887*** (0.269)		-7.386*** (0.128)		-6.165*** (0.068)

Table 8

(Cont.)

	Unmarried Parent		Married Parent		All Elderly		Other Adult	
	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity
		Calibration Parameters (cont.)						
δ_4		-6.579*** (0.155)		-6.875*** (0.268)		-4.607*** (0.123)		-4.261*** (0.065)
δ_5		-6.594*** (0.155)		-6.958*** (0.268)		-4.717*** (0.123)		-4.193*** (0.065)
δ_6		-5.816*** (0.155)		-6.067*** (0.269)		-4.059*** (0.122)		-3.576*** (0.064)
δ_7		-4.938*** (0.155)		-5.302*** (0.269)		-2.469*** (0.122)		-2.487*** (0.063)
δ_8		-3.875*** (0.157)		-4.297*** (0.271)		-1.787*** (0.124)		-1.536*** (0.064)
δ_9		-3.475*** (0.158)		-3.556*** (0.274)		-0.408*** (0.134)		-0.714*** (0.066)
δ_{10}		-2.885*** (0.160)		-2.965*** (0.277)				
δ_{11}		-8.728*** (0.155)		-9.324*** (0.269)				

Table 8

(Cont.)

	Unmarried Parent		Married Parent		All Elderly		Other Adult	
	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity
				Calibration Parameters (cont.)				
δ_{12}		-6.979*** (0.155)		-7.745*** (0.268)				
δ_{13}		-5.482*** (0.155)		-6.107*** (0.269)				
δ_{14}		-3.329*** (0.158)		-3.650*** (0.273)				
δ_{15}		-2.818*** (0.160)		-3.301*** (0.275)				
δ_{16}		-1.948*** (0.166)		-2.462*** (0.282)				
δ_{17}		-1.498*** (0.171)		-1.933*** (0.289)				
				Error Components				
$LN(\sigma^2)$		2.159*** (0.037)		2.255*** (0.042)		2.758*** (0.040)		2.698*** (0.043)

Table 8

(Cont.)

	Unmarried Parent		Married Parent		All Elderly		Other Adult	
	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity	SNAP	Food Insecurity
Error Components (cont.)								
λ		-1.804*** (0.166)		1.725*** (0.191)		2.670*** (0.178)		3.014*** (0.203)
Log-Likelihood	-63,484.20		-38,892.90		-27,997.08		-56,730.70	
N								
NJ	232,206		167,518		133,985		186,142	
Wald Test, Joint Sig. IVs P-Value	[0.000]		[0.003]		[0.080]		[0.000]	

Note. Models estimated using weighted household data from the 2001-2008 CPS-FSS and control for households head's gender, age, age-squared, race, ethnicity, nativity, marital status (HHs without children), education, employment status; number of adults, children, and disabled members in household; age of the youngest member (HHs with children); elderly members, residence in an urban area; state unemployment rate, log of household income, home ownership, and state and year fixed effects. Standard errors are in parentheses.

* Significant at 0.10 level.

** Significant at 0.05 level.

*** Significant at 0.01 level

The remainder of Table 8 lists estimates for observable household and economic characteristics. Respondent and household characteristics are generally of the expected direction. For example, increasing the household head's level of education is associated with a decrease in the probability of SNAP participation and food insecurity. Households headed by females are more likely to participate in SNAP and experience food hardships.

Increasing the number of adults in a household reduces the probability of participation and food insecurity, while increasing the number of children has the opposite effect. Increasing the state unemployment rate is associated with higher SNAP participation (except all-elderly households) and increased food hardships. Households with an employed head that own their own home, or have higher incomes are less likely to participate in SNAP or experience food insecurity (except for unmarried parent households).

Finally, the bottom of Table 8 lists item calibration parameters and error-components. All of the calibration parameters for the Rasch model are highly significant and are consistent with the severity ordering determined by the USDA (Bickel et al., 2000). The error-components are also highly significant. The lambda parameter is positive and highly significant for all household groups except unmarried parent households, where it is negative and highly significant. This indicates that unobservables in the SNAP participation equation are positively correlated with those in the food insecurity equation for married parent households, all-elderly households, and other adult-only households, which is consistent with Yen et al. (2008), Mykerezi and Mills

(2010), Ratcliffe et al. (2011), and Schaefer and Gutierrez (2012). The negatively signed lambda for unmarried parent households is unexpected and requires further analysis.

Sensitivity Analyses

The corrected behavioral Rasch model results differ markedly from the descriptive analysis and the majority of previous studies by indicating that SNAP participation has a negative and highly significant relationship with food insecurity. An important question is: what specification and model assumptions lead to this result? Previous studies have typically used a binary food hardship indicator to examine the relationship between SNAP participation and food hardships with results depending on specific methodology. Studies that use instrumental variables methods typically find an insignificant relationship. I demonstrate the efficiency gains from the corrected behavioral Rasch model by estimating alternative models that use the more restrictive binary measure of food hardships.

Table 9 reports coefficient estimates, standard errors, and marginal effects for a logistic model that corrects for selection bias and a bivariate probit model. Separate results are presented in the columns for the four household subsamples. The logistic model closely parallels the behavioral Rasch model without making some of the more restrictive assumptions, such as conditional independence, but uses a less efficient measure of food hardships. The bivariate probit model has been employed in previous studies (Ratcliffe et al., 2011; Shaefer & Gutierrez, 2012) and produced some of the stronger evidence to date on the ameliorative effects of SNAP. For convenience, I reproduced the results from the preferred specification, the corrected behavioral Rasch model. The discussion focuses on marginal effects because of scaling differences between the three models.

Table 9

SNAP Coefficients and Marginal Effects from Corrected Logistic and Bivariate Probit Models

	Unmarried Parent	Married Parent	All Elderly	Other Adults
Corrected Behavioral Rasch Model	4.170***	-1.108***	-1.993***	-2.725***
	(0.278)	(0.322)	(0.293)	(0.337)
	0.420	*-0.111*	*-0.083*	*-0.162*
Corrected Logistic Model	0.517	-0.018	-0.968*	-0.103
	(0.364)	(0.447)	(0.494)	(0.259)
	0.123	*-0.004*	*-0.090*	*-0.019*
Bivariate Probit Model	0.318	-0.004	-0.185	0.009
	(0.230)	(0.259)	(0.195)	(0.144)
	0.122	*-0.001*	*-0.041*	*0.003*

Note: Models estimated using weighted household data from the 2001-2008 CPS-FSS and control for households head's gender, age, age-squared, race, ethnicity, nativity, marital status (HHs without children), education, employment status; number of adults, children, and disabled members in household; age of the youngest member (HHs with children); elderly members, residence in an urban area; state unemployment rate, log of household income, home ownership, and state and year fixed effects. Instrumental variables include annual outreach per non-SNAP participant, state median recertification period, vehicle asset rules, and head's citizenship status. Standard errors are in parentheses and average marginal effects are in italics.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

The second rows of Table 9 list estimates from a logistic model that corrects for unobservable heterogeneity. A comparison of these results with results from the corrected behavioral Rasch model demonstrates that the SNAP coefficients are less precisely estimated in the logistic model specification. Corrected logistic model standard errors are larger than the corrected behavioral Rasch model for all subgroups. Only all-elderly households have a significant coefficient for SNAP participation. When compared to the marginal effects for the preferred specification, marginal effects for the corrected logistic model are very small (with the exception of all-elderly households).

The bivariate probit model generates results that are insignificant for all household subsamples. The marginal effects are closely related for the bivariate probit and correct logistic model for all subsamples except elderly households. The more precisely estimated coefficients and larger marginal effects of the corrected behavioral Rasch model are most likely due to the use of a more efficient measure of food hardships (i.e., the set of food security questions versus a single food insecurity indicator).

For my next robustness check, I examined how sensitive results from the corrected behavioral Rasch model are to the choice of instrumental variables. As with all instrumental variables analyses, the results are contingent on the validity of the instruments. To test this, I estimated specifications without the measure of immigrant eligibility rules, an indicator that is equal to one if the household head is a noncitizen immigrant, zero otherwise. This is weakest instrument on theoretical grounds.

Table 10 lists estimates from alternative specifications of the corrected behavioral Rasch model. The columns report coefficient estimates, standard errors, and marginal effects for the four subsamples. The top panel contains models that use an annual measure of SNAP participation while the bottom uses a measure of SNAP receipt in the past 30 days. Estimates from the preferred specification are reproduced in the first row of the top panel for convenience.

Table 10

SNAP Coefficients and Marginal Effects from Alternative Corrected Behavioral Rasch Model Specifications

	Unmarried Parent	Married Parent	All Elderly	Other Adults
SNAP Participation, Past 12 Months				
IVs—Outreach, Vehicle Asset Rules, Recertification Period, and Citizenship	4.170***	-1.108***	-1.993***	-2.725***
	(0.278)	(0.322)	(0.293)	(0.337)
	0.420	*-0.111*	*-0.083*	*-0.162*
IVs—Outreach, Vehicle Asset Rules, and Recertification Period	4.289***	-1.015***	-1.985***	-2.736***
	(0.273)	(0.272)	(0.291)	(0.374)
	0.426	*-0.103*	*-0.083*	*-0.163*
HH's income less than 185% FPL	3.921***	-3.145***	-2.139***	-2.566***
	(0.284)	(0.266)	(0.297)	(0.328)
	0.398	*-0.242*	*-0.086*	*-0.152*
SNAP Participation, Past 30 Days				
IVs—Outreach, Vehicle Asset Rules, Recertification Period, and Citizenship Status	3.280***	-3.939***	-2.004***	-2.489***
	(0.398)	(0.313)	(0.311)	(0.344)
	0.345	*-0.290*	*-0.084*	*-0.152*

Note. Models estimated using weighted household data from the 2001-2008 CPS-FSS and control for households head's gender, age, age-squared, race, ethnicity, nativity, marital status (HHs without children), education, employment status; number of adults, children, and disabled members in household; age of the youngest member (HHs with children); elderly members, residence in an urban area; state unemployment rate, log of household income, home ownership, and state and year fixed effects. Instrumental variables include annual outreach per non-SNAP participant, state median recertification period, vehicle asset rules, and head's citizenship status. Standard errors are in parentheses and average marginal effects are in italics.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

The second row of Table 10 lists estimates from specifications that use real annual outreach per capita (lagged 12 months), vehicle asset rules, and the median recertification period as instrumental variables. The immigrant eligibility measure is excluded from the specification to determine if the results are sensitive to this instrument.

A comparison of the marginal effects for rows 1 and 2 of the top panel suggests that the results are not sensitive to the inclusion of the immigrant eligibility instrument. Marginal effects either do not change or change very little if this instrument is excluded. The largest change occurs for married parent households, as the marginal effect decreases by 0.008 percentage points.

As an additional robustness check, I examine how sensitive the results are to income restrictions placed on the sample. Previous studies show that households who are marginally eligible for SNAP may adjust their labor supply to ensure eligibility. The current analysis is restricted to households with income less than 130% of the poverty line; however, this is not a perfect measure of gross income eligibility because the CPS only provides a measure of annual income, not monthly income (which is used to calculate benefit eligibility), and no information on assets. By restricting the analysis to 130% of the poverty line, I may be missing households that are gross income eligible on a monthly basis, but not on an annual basis. To test the sensitivity of the results to this assumption, I increased the income cutoff to 185% of the federal poverty line. For all subsamples except unmarried parent households and other adult-only households, increasing the income threshold for the sample has increased the marginal effect in absolute magnitude. The marginal effect has doubled for married parent households (-0.111 vs. -0.242). The increase in strength of the SNAP effect with a more generous income threshold is consistent with previous studies findings. The opposite is true for unmarried parent and other adult-only households.

As a final robustness check, I used an alternative version of the SNAP participation variable. I estimated alternative specifications with the annual SNAP measure replaced by a measure of SNAP participation within the past 30 days. Households that report receiving SNAP benefits within the past 30 days are more likely to participate in SNAP for the full year. The 30-day SNAP measure is also utilized extensively in the previous literature. For married parent and all-elderly households, the 30-day SNAP measure generates larger marginal effects, while the opposite is true for unmarried parent and other adult-only households. To put this into perspective, when the 30-day SNAP measure is used, SNAP decreases food insecurity among married parent households by 34.8% (17% with annual measure).

Conclusion

This paper uses nationally representative data from the 2001-2008 CPS-FSS to estimate a behavioral Rasch model that corrects for selection on observables and unobservable household characteristics. Based on the preferred model specifications, I find strong evidence of the ameliorative effects of SNAP participation on food insecurity for low-income married parent, all-elderly, and other adult-only households. Results from the preferred specification suggest that SNAP participation reduces the probability of food insecurity by 16.2 percentage points (22.4%) for other adult-only households, 11.1 percentage points (17%) for married parent households, and 8.3 (18%) percentage points for all-elderly households. For unmarried parent households, the association between participation in SNAP and food insecurity remains positive after correcting for selection on contemporaneous unobservables.

In alternative specifications, I estimated models that used a binary food hardship measure (food insecure), rather than the full set of food security questions utilized by the corrected behavioral Rasch model. Both a logistic model that corrects for selection bias and a bivariate probit model were estimated using the same data. When compared to the corrected behavioral Rasch model, it is clear that the use of a binary food hardship indicator is less efficient. The majority of SNAP coefficients are insignificant, with one exception, in which case it is marginally significant. This reinforces the hypothesis that previous studies have found no association, or a weak association, between SNAP participation and food insecurity because they use inefficient measures of food hardships.

The results presented in this paper are robust to various assumptions; however, there are potential weaknesses. First, the Rasch model assumptions may not be realistic for the food security questions. While the model appears to fit the data well, the follow-up food security questions violate the conditional independence assumption. Future work should relax this assumption while maintaining the efficiency gains from a Rasch analysis. Second, I use instrumental variables to identify the models. As such, interpretation of the results is subject to the validity of the instruments. To test this, I excluded the weakest instrument, head’s citizenship status, and found that the results did not change. Finally, the behavioral Rasch model, as it is formulated, does not account for the ordinal nature of some of the food security questions. Future work should address this by directly modeling these responses as ordinal and not discrete.

While the corrected behavioral Rasch model formulated in this paper is directly useful in the SNAP participation and food insecurity literature, it is also indirectly useful in other areas of research. The corrected behavioral Rasch model is equivalent to a random-effects logistic model with item fixed effects and controls for contemporaneous unobservable heterogeneity. The model can be applied in any situation where a random-effects logistic model is warranted and there is concern about contemporaneous unobservables. The model can also be applied to discrete-time event history model to estimate the causal effect of a binary variable on the hazard.

CHAPTER IV

UNDERAGE DRINKING AND THE OCCUPATIONAL CHOICES OF RECENT COLLEGE GRADUATES

Abstract

This analysis examines the relationship between underage college drinking and the initial occupational choices of recent male college graduates using data from the National Longitudinal Survey of Youth 1997 (NLSY97). We exploit the longitudinal structure of the NLSY97 to identify the year in which young men transitioned from college to work. Focusing on recent college graduates and their initial occupational choices allows us to address important timing issues not considered by previous studies. For the multivariate analyses, we estimate multinomial logistic models of occupational choice, where the occupational choice set is specified as employed full-time in white-collar occupations, other occupations, enrolled in school, and not in school nor employed full-time. In addition, we estimate multinomial logistic selection models to control for any potential unobserved heterogeneity between drinkers and abstainers. The results suggest that underage college drinking is not associated with young men's initial occupational choices, with the exception of the decision to be enrolled in school. Young men with any underage college days where they drank two or more drinks are 28.9% less likely to be enrolled in school after completing a bachelor's degree.

Introduction

A distinguishing feature of young adulthood is the number of choices made with potentially lifelong consequences. Schooling allows young adults to invest in themselves, acquiring occupational skills for the labor market. According to the National Center for Education Statistics (NCES), 42% of young adults were enrolled in college in 2011. During the 2013-2014 academic year, colleges and universities awarded 1.8 million bachelor's degrees to young adults. Upon completing the requirements for a bachelor's degree, many of these young adults transition from schooling to full-time, permanent jobs for the first time. While searching for employment, young adults compare jobs based on their wages, fringe benefits, and the potential for career advancement. The search also involves critical choices about their initial industry and occupation. A considerable amount of evidence suggests early labor market history, including initial occupational choices, influence job mobility and income trajectories (Light, 2005; Oreopoulos & von Wachter, 2012). In addition, early occupational choices have an effect on health behaviors (Kelly et al., 2011).

Young adults must also make choices about their drinking behaviors, which have implications for their health, schooling, and labor market outcomes. For better or for worse, drinking during college has become part of the higher-education experience for most young adults. Approximately four out of five college students drink alcohol and half of those who drink report drinking in excess (National Institute for Alcohol Abuse and Alcoholism [NIAAA], 2014). According to the NIAAA, 25% of college students report academic consequences of their drinking such as missing class, falling behind, and poor performance on exams and papers. Research suggests drinking is associated with a reduction in the quality (Anderson et al., 1993; Carrell et al., 2011; Williams et al., 2003) and quantity of skills, lower employment status (Johansson et al., 2007; Mullahy & Sindelar, 1996; Terza, 2002), and reduced income (Mullahy & Sindelar, 1991).

Several studies examine the relationship between drinking and occupational choice. These studies typically focus on the occupational choices of males between the ages of 25 and 59. While truncating the sample at these ages removes individuals who are still in school or close to retirement, considerable heterogeneity remains because of differences in educational attainment, labor market experience, health, and drinking history. The timing of occupational choices for these individuals is also very different. Younger individuals are making initial occupation choices, while mid-career individuals may be switching occupations and older individuals are moving from career-type employment to periods of "bridge employment" as they progress towards retirement. However, for many of these individuals, their occupational choice was made prior to their inclusion in the sample. Studies also tend to focus on contemporaneous drinking behaviors, which are subject to concerns about reverse causality. For these studies, it is unclear if drinking behaviors are causing men to choose a specific occupation or if their occupations are causing them to drink.

This analysis examines the relationship between underage college drinking and the initial occupational choices of college graduates using data from the 1997 cohort of the National Longitudinal Survey of Youth (NLSY97). We exploit the longitudinal structure of the NLSY97 to identify the year in which a young adult transitioned from college to work after completing a bachelor's degree. We extend the previous literature by focusing on initial occupational choices, which allows us to construct a sample of young adults who are facing the same occupational choices and level of education. This allows us to address important timing issues that have received little attention by the previous literature. Focusing on underage drinking also allows us to identify drinking measures that are better aligned with the occupational choices of young adults.

For the multivariate analyses, we estimate multinomial logistic (MNL) models of occupational choice, where occupational choice is specified as employed full-time in white-collar occupations, other occupations, in school, and neither in school nor employed full-time. These models control for young adults' demographic and background characteristics, survey design characteristics, economic characteristics, and region and year fixed-effects. In addition, we estimate MNL selection models (Terza, 2002; Terza & Vechnak, 2011) that control for potential unobserved heterogeneity between drinkers and abstainers.

Background

Previous studies suggest the principal mechanism that drives the relationship between underage college drinking and occupational choice is the acquisition of occupational skills. While attending college, young adults acquire occupational skills. Upon graduation and entry into the labor market, employers use these skills to differentiate between higher and lower quality employees. Empirical evidence suggests drinking adversely affects the acquisition of occupational skills by affecting the cognitive abilities needed to learn. Reduced cognitive ability leads to lower productivity as a student (diminished capacity to acquire occupational skills), which generates lower quality (less skilled) employees (Anderson et al., 1993; Carrell et al., 2011; Williams et al., 2003).

A closely related mechanism that links drinking to occupational choice is health status. Light or moderate drinking may generate health benefits (Hamilton & Hamilton, 1997; Heien, 1996). For example, drinking can benefit health by reducing stress and tension levels and lowering the incidence of illness. College students are particularly susceptible to high levels of stress and tension. Improved health leads to reduced absenteeism from classes and increased productivity, which generates higher quality (more skilled) future employees. Conversely, excessive drinking can result in negative consequences for health that translate into increased absenteeism from classes and decreased productivity.

There are other mechanisms that might link drinking and occupational choice. Individuals' college experiences often involve social events where students interact with peers, faculty, and alumni. Drinking can have a "socializing" effect if part of the drinking is associated with time spent with peers. Young adults can use this time to develop social skills. Peters and Stringham (2006) examine the effect of drinking on productivity (measured by earnings) and find that social drinking increases productivity. Spending time with faculty and alumni while drinking may also be associated with a "networking" effect (Hutcheson et al., 1995). During this time young adults may obtain information about potential employment opportunities, reducing information asymmetries.

These mechanisms provide important insights for the present analysis. Specific mechanisms are more likely to affect certain occupations because of the skills required for young men to be productive workers. White-collar occupations and enrollment in school represent the highest-skilled occupational choices. For young men to be productive in these occupations they must have acquired skills in critical thinking, speaking, comprehension, and active learning while in college. The skills needed for these occupations are particularly susceptible to the effects of drinking during acquisition when these skills are developed. Drinking may decrease the cognitive skills needed to learn new skills, which will reduce the likelihood of young adult's being employed in higher-skilled occupations. Other occupations typically rely on physical skills. For example, construction occupations require coordination, monitoring, and strength skills for young adults to be productive workers. Drinking may potentially improve or decrease health which affects young adult's motor skills and strength. Drinking associated with "networking" is anticipated to increase the number of job opportunities for young adults by reducing information asymmetries, while "social" drinking will increase the likelihood of employment in occupations that place a high value on communication skills.

In addition to the multiple mechanisms outlined above, there are also empirical considerations that might link underage college drinking and occupational choice. Failure to account for unobserved individual characteristics (unobserved heterogeneity) that are correlated with occupational choice and underage college drinking could bias the results. Potential omitted variables include young adult's tastes and preferences, non-wage job attributes, and innate ability. For example, empirical evidence suggests drinkers have a high marginal utility of leisure, which is known by the individual and unobservable by the researcher. The theory of rational addiction suggests reverse causality might also affect the empirical relationship (Becker & Murphy, 1988; Kenkel & Wang, 1999). In the present context, reverse causality is a concern if individuals are more likely to become drinkers because of their occupation (and its attributes). Mismeasurement and misreporting of drinking behaviors may also affect the observed relationship.

Early studies of problem drinking (alcohol abuse or dependence) and occupational choice focus on the life-cycle effects of drinking. Mullahy and Sindelar (1989) estimate MNL models of occupational choice using data from the first wave of the Epidemiologic Catchment Area (ECA) survey for males between the ages of 25 and 59. Their models specify the occupational choice set as employed full-time in white-collar, blue-collar, and service-sector occupations (the base outcome is not employed full-time). The construction of their sample suggests males are making different occupational choices based on their labor market histories. For example, younger males are likely making initial occupational choices while older males are switching from career-type employment to "bridge employment" as they prepare for retirement. Their findings suggest the early onset of problem drinking (between the ages of 19 and 22) decreases the likelihood of being employed in a white-collar occupation and increases the likelihood of being employed in a blue-collar occupation. Yet, these effects are only marginally statistically significant.

Anderson et al. (1993) also use the ECA to estimate MNL models of occupational choice for males age 25 to 55, where the occupational choice set is specified as being employed in low- and high-skilled white-collar and blue-collar occupations, and unemployed (base category); however, they focus on substance abuse generally (problem drinking and drug abuse) rather than problem drinking. Disaggregating the occupational choice set this way increases the likelihood of their models detecting drinking effects related to skills acquisition; however, their sample consists of males at different stages in their careers. The authors estimate a generalized method of movements (GMM) variant of the MNL model to control for potential unobserved heterogeneity caused by differences in educational attainment. Model specification tests fail to reject the MNL models that control for selection on observables. The results suggest substance abuse has no effect on occupational choice with the exception of low-skilled white-collar occupations and educational attainment; however, these results are marginally significant. According to their results, substance abuse has a strong negative effect on educational attainment.

Other studies focus on contemporaneous problem drinking and occupational choice. Kenkel and Wang (1999) use data from the 1989 wave of the 1979 cohort of the National Longitudinal Survey of Youth (NSLY79) to estimate conditional means and probit models of occupational choice for males between the ages of 24 and 31. While Kenkel and Wang's sample is less heterogeneous than those of previous studies, it is still subject to concerns about the timing of occupational choices. They define the occupational choice set using three binary variables for employment in a white-collar, blue-collar, or service-sector occupation. Descriptive analyses using comparisons of the proportions of drinkers and abstainers in each occupational category suggest contemporaneous problem drinking reduces the likelihood of a young man being employed in a white-collar occupation, but increase the likelihood of being employed in a blue-collar occupation. Unfortunately, they did not test the statistical significance of these differences. Probit models for white-collar occupations reveal a similar relationship after controlling for male's observable characteristics; however, the result is marginally significant. Additionally, these models do not control for selection or potential reverse causality.

Terza and Vechnak (2011) estimate MNL selection models of occupational choice that control for unobserved heterogeneity related to substance abuse (problem drinking and drug abuse). The authors use data from the 1992 National Longitudinal Epidemiological Survey (NLAES) and focus on males and females between the ages of 24 and 59. Like other studies, Terza and Vechnak do not address timing issues related to occupational choice. Additionally, they do not estimate separate models for males and females, which increases the heterogeneity of their sample. Their models specify the occupation choice set as employed full-time in white-collar, blue-collar, and service-sector occupations; unemployed; and out of the labor force (base category). The MNL selection models were identified using information on parent's problem drinking, and alcohol and cigarette excise taxes. The results suggest current substance abuse is endogenous and reduces the probabilities of being employed full-time in a white-collar occupation and employed part-time.

A shortcoming of many of the previous studies is that they do not account for timing issues related to occupational choices. Many of the previous studies estimate models of occupational choice using heterogeneous samples. These samples include individuals (typically males) who are at various stages in their work lives. Some individuals are transitioning from school to work and making initial occupational choices, while others are switching careers or moving from career-type employment to “bridge employment” as they prepare for retirement. These occupational choices are very different. In addition, studies that focus on contemporaneous drinking are also subject to reverse causality, where it is possible that the person’s job is causing his or her drinking behaviors.

We extend the previous literature by focusing on initial occupational choices for recent college graduates. This allows us to address the timing of occupational choices because all of the young adults in our sample face the same choices. Restricting our sample to recent college graduates also reduces the heterogeneity in our sample because all of the young adults have bachelor’s degrees and lessens concerns about reverse causality because drinking is being measured and observed prior to those initial occupational choices.

Data

Estimation of the relationship between underage college drinking and subsequent occupational choices requires a data set that includes schooling, labor market, and drinking behaviors at the relevant points in time. The 1997 cohort of the National Longitudinal Survey of Youth (NLSY97) is perhaps the best publicly available, nationally representative data set that meets these requirements. The NLSY97 is designed to document transitions from school to work and into adulthood. The first wave of the NSLY97 was conducted in 1997 and included interviews with youths who were between the ages of 12 and 16. The NLSY97 has followed these youths each year with more than 82% of the sample still involved in 2012.

The NSLY97 consists of a sample of 8,984 youths and oversamples Hispanics and blacks. The present analysis considers male youths who completed a bachelor's degree. The analysis is limited to males because our focus is on underage college drinking and previous studies have shown males and females differ in their labor market behaviors, schooling experiences, and alcohol use patterns (Mullahy & Sindelar, 1992, 1996). Additional focus is placed on recent college graduates because college students are more susceptible to underage drinking (Wechsler et al., 2002) and they are an important policy group.

Of the 4,599 male youths interviewed in the first wave of the NSLY97, 841 received bachelor's degrees between 2001 and 2012. Graduates who reported being currently employed in farming or military occupations are excluded from the sample to enhance the comparability of young men across occupational categories. The labor market behavior of young men in farming and military occupations is likely to be very different from those in other occupations; also, these were infrequent transitions in the sample. Dropping these transitions results in an initial sample of 834 college graduates. An additional 123 young men are excluded from the analysis because of missing information for the dependent and principal explanatory variables. All men who completed a bachelor's degree in 2012 are excluded from the analysis sample because of missing information on beer excise taxes used to construct the instrumental variables (15 men). Young men failing to provide valid responses to one or more of the questions used to form the explanatory variables are also excluded, leaving a final analysis sample of 680 young men.

Dependent Variables

The empirical analyses examine responses to questions about the young men's schooling and labor market behaviors immediately following graduation as dependent variables. In each wave young men were asked to provide information about their current school enrollment and employment status. If young men reported working for an employer, then additional information was collected on each young man's employment history, number of jobs held, weeks worked, and hours worked per week; employer characteristics; and industry and occupation. In some instances, young men reported working for multiple employers. Because the focus of our analysis is on the initial occupational choices of young men, it is important to identify each young man's main job. When young men enter the labor market for the first time, it is common for some to experience periods of "bridge employment" prior to accepting a career-type position. We address this issue by defining each young man's main job as the job at which he reports working the most hours per week at the time of the survey.

Young men's responses to the schooling and labor market behaviors are used to construct a categorical measure of occupational choice with four categories. The first occupational category denotes men who are employed full-time in a white-collar occupation, where full-time is defined as working at least 35 hours per week at a job when the interview was conducted. The second occupational category describes males who are employed full-time in other occupations. These occupational categories are aggregations from the 2002 Census Bureau Occupational Classification System Codes. While these are rather broad occupational groupings, it is necessary for the tractability of the analysis and increases the comparability with previous studies. However, aggregating occupations into broad groups also imposes the assumption that drinking effects are homogeneous across finer categories. The white-collar and other occupation categories capture young men who are successful at finding career-type jobs.

The third occupational category captures young men who continued their schooling and were pursuing professional or graduate degrees. Young men are considered full-time students if they were enrolled in school and not associated with an employer at the time of the interview. Expanding the occupational choice set to include the schooling decision allows us address the endogeneity of young men's skills acquisition and capture men who will accept career-type jobs as professionals. The fourth and final category denotes men who were neither enrolled in school nor employed full-time. Men who fall under this category failed to find career-type employment at the time of the interview. Because the NLSY97 labor market behavior questions are tied to a specific employer, we cannot differentiate between young men who are unemployed and those who are not currently employed by a specific employer. This reduces the comparability of the analysis to previous studies where the occupational choice set is based on the young man's labor force participation status.[24] Additionally, the occupational choice set does not include young men who are unemployed and not searching for work (discouraged workers) or those who are out of the labor force and engaged in household production. While these omissions restrict the occupational choice set, they are mitigated by the analysis's focus on recent male college graduates because they are less likely to be discouraged workers or participate in household production.

The categorical occupational choice variable is similar to the specification used in Mullahy and Sindelar (1989). Anderson et al. (1993) disaggregates white-collar and other occupations into high- and low-skilled occupations. While this analysis does not group occupations in this fashion, their results are comparable with our white-collar and other occupation categories. Terza and Vechnak (2011) include an occupational alternative for those who are out of the labor force (in school and discouraged workers). Sample restrictions and data limitations reduce the comparability of our results with this particular category because we do not include discouraged workers; however, the remaining occupational categories are similar in spirit to those used here.

Explanatory Variables

The principal explanatory variables concerning underage alcohol consumption are constructed using responses to the drinking questions collected by the NSLY97. An advantage of the NLSY97 for this analysis is that respondents are asked in each wave about the number of days they consumed alcohol, drinks per day, and days they typically had five or more drinks within the past 30 days. Other data sets only ask questions about

[24] The NLSY97 collected information on each young man's labor force participation status using questions from the Current Population Survey (CPS) in waves 1, 4, and 10.

drinking behaviors in some years or rely on the respondent's ability to recall previous alcohol consumption, which is subject to recall bias. A limitation of the NLSY97 is that it does not collect the information necessary to construct clinical measures of alcohol abuse or dependence. In addition, young men's reports of drinking may be subject to misreporting. However, studies assessing the measurement of drinking in household surveys find that requests for detailed information about drinking yield reliable estimates (Poikolainen & Karkkainen, 1985; Williams et al., 1985).

We used information on young men's drinking behaviors to construct three binary measures of underage college drinking. We measure underage college drinking using binary variables to facilitate comparison with existing drinking policies for social, binge, and heavy drinking. The first drinking measure is a binary variable set to one if young men ever reported any drinking while underage and enrolled in college, zero otherwise. Measuring drinking as "any underage college drinking" likely captures moderate or "social" drinking along with heavier drinking and is comparable to previous studies that define drinking as "any consumption" (Dee et al., 2003; Mullahy & Sindelar, 1992). The second drinking measure is a binary variable set to one if young men ever reported drinking five or more drinks on one or more days while underage and enrolled in college, zero otherwise. This drinking measure is consistent with binge drinking (CDC, 2014; Jennison, 2004; NIAAA, 2004; Wechsler & Nelson, 2001). The third drinking measure is a binary variable that is set to one if young men ever reported drinking two or more drinks per day while underage and enrolled in college, zero otherwise. The "two or more drinks per day" measure captures drinking behaviors that are consistent with heavy drinking (Baer et al., 2001; Dee & Evans, 2003).

Our multivariate empirical analyses additionally control for demographic, geographic, and economic characteristics. The demographic measures include each young man's age, race, ethnicity, marital status, and subjective health status. The analysis also includes a measure of each young man's innate ability, which is measured by their Armed Services Vocational Battery (CAT-ASVAB) percentile score.[25] State-level economic controls include per-capita beer consumption and real income (in $1,000); and the percentage of the state population age 25 and older with at least a bachelor's degree. Information on state-level per-capita beer consumption and real income was obtained from the National Institute for Alcohol Abuse and Alcoholism (NIAAA) and the Bureau of Economic Analysis, respectively. The U.S. Census Bureau provided information on the percentage of the population age 25 and older with at least a bachelor's degree. Other controls include residence in an urban area; months since college graduation; an indicator for missing information for at least one wave; membership in the NLSY97 oversample; county-level unemployment rate (from the NSLY97 geocode files); and region and time fixed-effects. Means and standard deviations for the explanatory variables are in Appendix D.

Exclusion Restrictions

We estimate MNL models that do and do not account for possible selection in the underage drinking variable. Estimation of the MNL model with selection controls

[25] Missing information on the young man's ASVAB score and residence in an urban area was imputed using the sample means. Indicators for missing data were included in all models.

requires exclusion restrictions that predict drinking but do not predict occupational choice. We use state-level policies related to drinking and lagged delinquency and drinking behaviors as exclusion restrictions. State-level drinking policies are enacted to alter drinking behaviors and are not intended to affect occupational choices. These policies may be linked to occupational choices for empirical reasons if they are capturing state-level economic conditions. We address this by including state-level economic controls (described above). Lagged delinquency and drinking behaviors are far removed from young men's occupational choices, but strongly predictive of other drinking behaviors.

Information on cigarette (a complementary good) and beer excise taxes was obtained from the Tobacco Tax Council (2012) and the Beer Institute's Brewer's Almanac (2013), respectively. Data on state-level drinking policies was obtained from the Alcohol Policy Information System (APIS). The Tobacco Tax Council and APIS provide information on the exact date a policy was introduced, while the Brewer's Almanac provides information for the year. Information used to construct the exclusion restrictions was merged with the NLSY97 data using state identifiers provided by the NSLY97 geocode files.

The first set of exclusion restrictions measure cigarette and beer excise taxes for each state. The beer excise tax provides an indicator for interstate differences in alcohol beverage prices (Cook et al., 1994). Researchers also often use beer excise taxes to proxy for alcohol prices (Ruhm et al., 2012). Beer excise taxes are expected to affect young men's drinking behaviors by altering the price of alcohol. Higher taxes (prices) are anticipated to reduce drinking behaviors. Terza (2002) and Terza and Vechnak (2011) use measures of beer and cigarette excise taxes as instrumental variables. We construct measures of state beer and cigarette excise taxes for each young man over the previous four years using the state where he resided at the time of the survey.[26]

The second set of exclusion restrictions measure state drinking polices for blood alcohol content (BAC), social hosting, and Sunday sales of alcohol. BAC laws limit the amount alcohol allowed in an individual's bloodstream while operating a motor vehicle. Low BAC limits are consistent with stricter policies on drinking behaviors, reducing the likelihood of a young man drinking. Social host laws are targeted at reducing underage drinking by imposing liability on adults who host parties, and Sunday sale laws restrict the sale of alcohol on Sundays (Dills, 2009). Yoruk (2013) finds that states who repealed their laws restricting Sunday alcohol sales experienced significant increases in per-capita drinking. BAC and Sunday sale laws are expected to increase the opportunity costs of drinking. We construct measures of BAC, social host, and Sunday sale laws by taking the average of the proportion of time these laws were in effect over the previous four years, using the state where the young man resided at the time of the survey.[27]

The third set of exclusion restrictions measures lagged behaviors for young men prior to college from the NSLY97. The first measure uses the delinquency index created by Child Trends, Inc. for the NLSY97. The index was constructed using responses to questions in the first wave (1997) of the NSLY97. The questions elicited information

[26] We also tested alternative specifications for beer and cigarette excise taxes using the current year and yearly lags.

[27] We also tested alternative specifications for drinking policies using the current year and yearly lags. In addition, we examined state vertical ID laws and retailer scanner provisions for IDs.

about various delinquent activities young men participated in during the previous year. For a complete listing of the delinquency index questions, see Appendix E. Higher values of the index indicate more severe levels of delinquency. Greater delinquency is expected to increase the likelihood of young men exhibiting other behavioral problems, such as underage drinking. The second instrument is a measure of previous drinking: drinking during high school. Mullahy and Sindelar (1989) find that prior drinking experiences increase the likelihood of an individual reporting future drinking. We use this information to construct measures of delinquency and high school drinking.

Econometric Specification

The multivariate analysis uses a multinomial logistic model (MNL; Nerlove & Press, 1973) to describe how young men make occupational choices. These choices can be motivated by a random utility model. We assume each young man's utility is a function of potential lifetime productivity, demographic and background characteristics, and unobserved taste shifters. Suppose the i^{th} individual is faced with four mutually exclusive and exhaustive choices (Y_i): full-time employment in white-collar occupations, full-time employment in other occupations, in school, and neither in school nor employed full-time. Further, assume the utility of choice j is

$$V_{ij} = A_i\beta_{A_j} + X_i\beta_{X_j} + \varepsilon_{ij}, \tag{11}$$

where A_i (an indicator for underage college drinking) and X_i (a matrix of control variables) are reduced form determinants of the individual's lifetime utility for choice j; and ε_{ij} is stochastic error-component that is distributed according to a type 1 -extreme value (Gumbel) distribution. We assume young men will choose the occupational alternative that provides the maximum lifetime utility among the J choices.

Given equation (1) and the assumption that the stochastic error component (ε_{ij}) is distributed according to a type 1 extreme value distribution, the probability choice j is made is

$$P\left(Y_i = j \mid A_i, X_i\right) = \frac{\exp\left(A_i\beta_{Aj} + X_i\beta_{Xj}'\right)}{1 + \sum_{k-1}^{J}\exp\left(A_i\beta_{Ak} + X_i\beta_{Xk}'\right)}, j = 0,1,\ldots,J; k \neq j, \tag{12}$$

where exp( ) is the exponential function. Equation (2) and the independence of observations lead to a likelihood-function that is the product of the probabilities for the J choices for all individuals. The model as it is formulated above normalizes one of the occupational choice set’s parameters to be zero for identification purposes. This alternative will serve as the base category. Therefore, all coefficients must be interpreted relative to the base category. For the current analysis, full-time employment in white-collar occupations will serve as the base category.

An alternative approach to the MNL model is to model each young man’s choice set using a series of J binary logistic models; however, simultaneously modeling the choice set leads to more efficient estimation. Yet, this approach relies on the assumption of independence of irrelevant alternatives (IIA), which follows from the initial assumption that the stochastic error-components for the J choices are independent and homoscedastic. While this property is convenient for estimation purposes, it is not an appealing restriction to place on young men’s behavior. The IIA assumption implies that a series of pairwise comparisons are unaffected by the characteristics of alternatives other than the pair under consideration. Thus, the conditional probability of a choice does not depend on the alternatives. We will test this property using the Hausman test (Hausman & McFadden, 1984).

Endogenous Multinomial Logistic Model

Additionally, we estimate MNL models that control for the potential endogeneity/selectivity of underage college drinking. Selectivity may arise because of unobserved heterogeneity between drinkers and abstainers. We address this issue by assuming the young man's decision to drink while underage is

$$A_i = I\left(\alpha_X{}' X_i + \alpha_Z{}' Z_i + u_i > 0\right) \tag{13}$$

where I( ) is an indicator function that equals one if the condition is true, zero otherwise, X_i is a matrix of the young man's observable characteristics that are common to the occupational decision, Z_i is a set of variables that are only associated with the underage drinking decision (the excluded variables described in the previous section) and u_i is a stochastic error-component that is assumed to be identically and independently distributed according to a normal distribution. The resulting model is consistent with a standard probit model for the decision to drink while underage and in college.

Following Terza (2009), we assume the error-component specified in equation (1) can be decomposed into u_i and e_{ij}^* such that $\varepsilon_{ij} = \lambda_j u_i + e_{ij}^*$. As a result, the utility for choice j is now

$$V_{ij}^{*} = A_i\beta_{Aj} + X_i\beta_{Xj}^{'} + C_{ij}\beta_{Cj}^{'} + \lambda_j u_i + e_{ij}^{*} \tag{14}$$

where λ_j is an unknown parameter to be estimated, and e_{ij}^{*} represents the new stochastic error-component of utility after controlling for observed and unobserved heterogeneity. The error-component, u_i, generates correlation between the underage drinking variable (A_i) and the occupational choice variable (Y_j) through λ_j. If λ_j is nonzero, u_i influences the young man's underage drinking decision and the likelihood of choosing a specific occupational category, rendering the baseline model inconsistent.

Equations (2)-(4) imply the following likelihood function for a sample of size N, which is the same model as that used by Terza (2002) and Terza and Vechnak (2011):

$$\begin{aligned} L(\alpha,\beta_j,\lambda_j) = \prod_{i=1}^{N} \Bigg\{ & A_i \int_{-\alpha_X{}'X_i-\alpha_Z{}'Z_i}^{\infty} \prod_{j=1}^{J} \left[\frac{\exp\left(A_i\beta_{Aj} + X_i\beta_{Xj}^{'} + \lambda_j u_i\right)}{1+\sum_{k-1}^{J}\exp\left(A_i\beta_{Ak} + X_i\beta_{Xk}^{'} + \lambda_k u_i\right)} \right]^{Y_{ij}} \phi(u_i)\,du_i \\ & (1-A_i) \int_{-\infty}^{-\alpha_X{}'X_i-\alpha_Z{}'Z_i} \prod_{j=1}^{J} \left[\frac{\exp\left(A_i\beta_{Aj} + X_i\beta_{Xj}^{'} + \lambda_j u_i\right)}{1+\sum_{k-1}^{J}\exp\left(A_i\beta_{Ak} + X_i\beta_{Xk}^{'} + \lambda_k u_i\right)} \right]^{Y_{ij}} \phi(u_i)\,du_i \Bigg\} \end{aligned} \tag{15}$$

where φ( ) is the standard normal probability density function. All model parameters are estimated simultaneously. The estimates for the λ_j's are factor loading parameters with the property that their nullity is a sufficient statistic for the exogeneity of the underage drinking variable (Terza, 2009). For estimation purposes, we conduct a line search with respect to the most troublesome parameters (the λ_j's in the present model) from -3 to 3 by increments of 0.25. All parameters are estimated conditional on the value of λ_j. The value of λ_j that yields the best fit (in terms of the log-likelihood function) is used as its starting value in the unrestricted model. This ensures the results represent a global maximum.

The principal advantage of Terza's (2009) framework is its flexibility to account for unobservable characteristics in the MNL model. Terza's framework does not require the bivariate normality assumption, relying instead on separate error-component assumptions for the outcome and switching equations. Further, Terza's framework is particularly useful with respect to the MNL model. The stochastic error-components re-parameterization has the potential to alleviate concerns about IIA. Specifying the error-component according to the Terza framework allows the researcher to account for any unobserved heterogeneity that is correlated with the occupational categories and underage drinking behaviors, relaxing the independence of stochastic error-components across alternatives. By accounting for the unobserved heterogeneity, this approach reduces the likelihood that the occupational outcomes are correlated because of omitted variables, reducing the likelihood of IIA issues manifesting.

A disadvantage of the current approach is that it makes strong and potentially incorrect assumptions about functional form. If the data are not generated according to the probit and logistic models specified, then the analysis will suffer from misspecification bias. An additional disadvantage is the model's reliance on instrumental variables for identification. Therefore, interpretation of the results is conditional on the validity of the instrumental variables. We test the sensitivity of our results to the instrumental variables by estimating models with and without specific instrumental variables.

The estimated underage college drinking coefficients describe how underage college drinking affects men's valuations of occupational choices, relative to their valuations of the base outcome. To facilitate comparisons with previous studies, we estimate average marginal effects for the probabilities of making the choices. The average marginal effect of underage college drinking on the probability of choosing occupation category j is

$$AME = \frac{1}{N}\sum_{i=1}^{N}\int_{-\infty}^{\infty}\left[P\left(Y_i = j \mid A_i = 1, X_i\right) - P\left(Y_i = j \mid A_i = 0, X_i\right)\right]\phi\left(e_i^*\right)de_i^* \qquad (16)$$

where $P(Y_i = j \mid A_i = 1, X_i)$ denotes the probability of choosing occupational category j if an individual consumes alcohol while underage and $P(Y_i = j \mid A_i = 0, X_i)$ denotes the probability of choosing occupational category j if each individual abstains from underage drinking. Thus, the average marginal effect describes how the probability of choosing occupational category j would differ if all individuals drink while underage versus all individuals abstaining. The average marginal effect is calculated using Gauss-Hermite quadrature.

Results

Table 11 lists the proportions of recent college graduates by occupational category, calculated separately for underage college drinkers and abstainers. The top panel contains estimates using any underage college drinking to measure young men's drinking behaviors. Estimates from the middle panel were produced using five or more underage college drinks on one or more days to describe drinking. The bottom panel measures drinking using any underage college days with two or more drinks. Comparisons of the proportions of young men in white-collar and other occupations for drinkers and abstainers reveal an increased likelihood of finding career-type jobs for drinkers, regardless of how drinking is measured.

For example, 30.3% of young men with any underage college drinking are employed full-time in other occupations, while only 24% of abstainers are employed in other occupations. This result is consistent with the descriptive analysis presented in Kenkel and Wang (1999). Conversely, the proportions of young men enrolled in school and neither in school nor employed full-time are the highest for abstainers, suggesting drinking decreases the likelihood of not finding a career-type job; however, it appears to reduce the probability of accepting a professional career (through schooling). Yet, the differences in proportions are only statistically significant for other occupations when drinking is measured by any underage college days with two or more drinks.

We begin the multivariate analyses by estimating MNL models and MNL selection models. Estimated coefficients, standard errors, and marginal effects for young men's drinking behaviors from alternative specifications are listed in Table 12. Estimates for employed full-time in white-collar occupations are reported in the first column of the table. The subsequent columns report estimates separately for other occupations, in school, and neither in school nor employed full-time. The top panel lists estimates from models that include any underage college drinking, while the middle and bottom panels list results that use five or more drinks on one or more days and any underage college days with two or more drinks to describe drinking, respectively.

Table 11

Occupational Choice and Drinking Behaviors for Male College Graduates

	Drinkers	Abstainers
Any Underage College Drinking		
Full-time, White-collar Occupations	0.365 (%)	0.363 (%)
Full-time, Other Occupations[a]	0.303	0.240
In School	0.110	0.144
Not In School Nor Employed Full-Time	0.221	0.253
N	534	146
5 or More Underage College Drinks on One or More Days		
Full-time, White-collar Occupations	0.365	0.364
Full-time, Other Occupations[a]	0.310	0.262
In School	0.112	0.126
Not In School Nor Employed Full-Time	0.213	0.248
N	394	286
Any Underage College Days with Two or More Drinks		
Full-time, White-collar Occupations	0.370	0.356
Full-time, Other Occupations[a]	0.311*	0.251
In School	0.104	0.142
Not In School Nor Employed Full-Time	0.215	0.251
N	441	239

Note. Means estimated using unweighted data from the 1997-2011 NLSY97. Differences in means were tested using t-test. Farming and military occupations are excluded from the sample.
[a] Other occupations is defined as full-time blue-collar/service occupations.
* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

Table 12

Multinomial Logistic Models of Occupational Choice for Male College Graduates

	Full-time, White-Collar Occupation	Full-time, Other[a] Occupation	In School	Not In School Nor Emp. Full-Time
		Any Underage College Drinking		
No Controls		0.230	-0.270	-0.143
		(0.243)	(0.298)	(0.244)
	0.002	*0.064*	*-0.033*	*-0.033*
Standard Controls[b]		0.288	-0.284	-0.095
		(0.261)	(0.344)	(0.263)
	-0.009	*0.070*	*-0.035*	*-0.026*
Corrected MNL		0.353	0.289	0.283
		(0.621)	(0.945)	(0.808)
	-0.069	*0.042*	*0.008*	*0.019*
		5 or More Underage College Drinks on One or More Days		
No Controls		0.161	-0.125	-0.157
		(0.195)	(0.259)	(0.206)
	0.002	*0.047*	*-0.014*	*-0.035*
Standard Controls[b]		0.187	-0.230	-0.144
		(0.217)	(0.300)	(0.228)
	0.001	*0.053*	*-0.024*	*-0.030*
Corrected MNL		0.419	0.085	0.290
		(0.457)	(0.668)	(0.569)
	-0.068	*0.061*	*-0.013*	*0.020*
		Any Underage College Days with Two or More Drinks		
No Controls		0.175	-0.349	-0.192
		(0.205)	(0.263)	(0.213)
	0.014	*0.060*	*-0.038*	*-0.036*
Standard Controls[b]		0.170	-0.507*	-0.192
		(0.223)	(0.301)	(0.229)
	0.018	*0.062*	*-0.050*	*-0.029*

Table 12

(Cont.)

	Full-time, White-Collar Occupation	Full-time, Other[a] Occupation	In School	Not In School Nor Emp. Full-Time
Any Underage College Days with Two or More Drinks (cont.)				
Corrected MNL		0.426	0.308	0.147
		(0.501)	(0.732)	(0.590)
	-0.065	*0.064*	*0.011*	*-0.010*

Note: Models estimated using unweighted data from the 1997-2011 NLSY97. Robust standard errors are in parenthesis and marginal effects are in italics.
[a] Other occupations is defined as full-time blue-collar/service occupations.
[b] Standard controls include age, race, ethnicity, marital status, number of children, subjective health status, ASVAB score, missing ASVAB score, residence in an urban area, missing residence in an urban area, months since college graduation, non-response in at least one wave, NLSY97 oversample, county unemployment rate, state per-capita beer consumption, state percent of population with college education or higher, state real income per-capita, and region and year fixed effects.
[*] Significant at 0.10 level. [**] Significant at 0.05 level. [***] Significant at 0.01 level.

The first row of each panel of Table 12 reports estimates from MNL models of occupational choice that control for drinking behaviors. The marginal effects are positive and statistically insignificant for white-collar and other occupations. Additionally, the magnitude of the marginal effects for white-collar occupations is effectively zero. In contrast, the marginal effects suggest underage college drinking, regardless of how it is measured, is negatively associated with the school enrollment and neither in school nor employed full-time occupational alternatives. These results are consistent with the descriptive analysis.

The second row of each panel of Table 12 reports results from specifications that add controls for each young man’s age, race, ethnicity, marital status, number of children, subjective health status, CAT-ASVAB percentile score (and missing CAT-ASVAB percentile score), residence in an urban area (and missing residence in an urban area), months since college graduation, non-response to at least one wave of the NLYS97, membership in the NSLY97 oversample; local unemployment rate, state per-capita beer consumption and real income, the percentage of the state population 25 and older with at least a bachelor's degree, and region and year fixed-effects. Adding these controls increases the marginal effects (in absolute magnitude) for the school enrollment, white-collar (except when drinking is measured by any underage college drinking), and other occupation alternatives. Conversely, adding the controls decreases the marginal effects for the neither in school nor employed full-time occupational outcome. When compared to the MNL models with no additional controls, adding the full set of controls reduces the marginal effects by approximately 5–10%. Adding the controls also results in a marginally significant (at the 10% level) coefficient estimate for any underage college days with two or more drinks in the school enrollment equation. All of the other drinking coefficients are imprecisely estimated.

After controlling for selection on young men's observable characteristics, the marginal effect for any underage college days with two or more drinks is associated with a five percentage point reduction in the likelihood of young men enrolling in school after completing a bachelor's degree. This result can be placed into context using the marginal effect and the proportion of young men who are enrolled in school to convert the percentage point decline into a percentage decline. Recall, 11.8% of young men are enrolled in school. The baseline MNL model (controls for selection on young men's observable characteristics) results suggest 16.8% of young men would be enrolled in school if they did not have any underage college days in which they drank two or more drinks, suggesting underage drinking reduces the likelihood of being enrolled in school by 29.8%. This suggests "heavy" underage college drinking may have a strong effect on the graduate schooling decisions of young men, which will impact the likelihood of them obtaining career-type jobs in professional occupations in the future. Dee and Evans (2003) are similar in their findings, which suggest teen heavy drinking reduces the likelihood of young men and women entering college by 8.8 percentage points.

The third row of each panel of Table 12 reports estimates from MNL models that control for selection on young men's observable and unobservable characteristics. After controlling for unobserved heterogeneity, the marginal effects change sign from negative to positive for the schooling enrollment decision and neither in school nor employed full-time occupational choices. The marginal effects remain negative for white-collar occupations when drinking is measured by any underage college drinking and change from negative from positive when drinking is specified as five or more underage college drinks on one or more days or any college days with two or more drinks. However, all of the underage college drinking coefficients for the MNL selection model are imprecisely estimated.

Tables 13–15 list additional coefficients and standard errors for the occupational choice equations from MNL models and selection models. These are the same models listed in Table 12. The columns report estimates separately for the baseline MNL models (control for selection on observables) and MNL selection models (control for selection on observables and unobservables). Underage college drinking behavior coefficients are listed first, followed by demographic characteristics, survey design characteristics, economic characteristics, and the error-components.

The second panels of Tables 13–15 list coefficient estimates for young men's demographic characteristics. The number of children and subjective health status of young men are the only statistically significant predictors of occupational choice. Increasing the number of children is associated with a reduction in the likelihood of young men being enrolled in school or neither in school nor employed full-time relative to being employed full-time in white-collar occupations, regardless of how drinking is measured or if the model controls for selection on unobserved heterogeneity. Better health also appears to increase the likelihood of being enrolled in school (relative to being employed full-time in white-collar occupations).

The second panels of Tables 13–15 list estimates for the survey design characteristics and variables created using the longitudinal structure of the NSLY97. The only statistically significant variable for this group is the number of months since college graduation. The coefficient estimates suggest young men are less likely to be enrolled in school or employed full-time in other occupations the further they are removed from their college graduation. This result is similar for all models regardless of how drinking behaviors are measured and consistent with prior evidence that increasing the length of time of one's exit from formal schooling reduces the likelihood of returning. For the employed full-time in other occupations result, this may be suggestive of early-career "bridge employment."

Table 13

Selected Coefficients from Multinomial Logistic Models of Occupational Choice and Any Underage College Drinking for Male College Graduates

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
Drinking Behavior						
Any Underage College Drinking	0.288	0.353	-0.284	0.289	-0.095	0.283
	(0.261)	(0.621)	(0.344)	(0.945)	(0.263)	(0.808)
Demographic Characteristics						
Age	0.131	0.131	0.104	0.115	0.053	0.060
	(0.091)	(0.091)	(0.122)	(0.126)	(0.105)	(0.107)
Black	-0.027	-0.010	0.067	0.231	0.156	0.261
	(0.408)	(0.439)	(0.501)	(0.590)	(0.415)	(0.471)
Other	0.092	0.094	0.630	0.697	0.304	0.336
	(0.349)	(0.353)	(0.453)	(0.497)	(0.390)	(0.398)
Hispanic	0.029	0.027	0.611	0.620	-0.727	-0.725
	(0.366)	(0.366)	(0.489)	(0.497)	(0.485)	(0.486)

Table 13

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
			<u>Demographic Characteristics (cont.)</u>			
Married	-0.178	-0.170	0.183	0.230	0.264	0.303
	(0.344)	(0.347)	(0.458)	(0.478)	(0.352)	(0.362)
Number of Children	-0.347	-0.352	-0.892*	-0.917*	-0.955**	-0.982**
	(0.277)	(0.282)	(0.514)	(0.509)	(0.414)	(0.420)
Subjective Health Status	0.148	0.146	0.332**	0.311*	0.139	0.127
	(0.128)	(0.129)	(0.162)	(0.165)	(0.144)	(0.149)
ASVAB Percentile Score in 1999	-0.789	-0.794	0.892	0.876	0.369	0.345
	(0.500)	(0.500)	(0.821)	(0.828)	(0.545)	(0.549)
Missing ASVAB Score	0.057	0.057	0.192	0.202	-0.275	-0.275
	(0.291)	(0.291)	(0.394)	(0.399)	(0.346)	(0.348)
Urban Residence	0.009	0.013	0.143	0.187	-0.253	-0.224
	(0.293)	(0.296)	(0.427)	(0.435)	(0.314)	(0.324)
Missing Urban Residence	-0.287	-0.293	0.454	0.356	-0.295	-0.346
	(0.493)	(0.497)	(0.676)	(0.725)	(0.544)	(0.559)

Table 13

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
			Survey Characteristics			
Months Since College Graduation	-0.028*	-0.028*	-0.238***	-0.240***	-0.041	-0.042
	(0.015)	(0.015)	(0.049)	(0.049)	(0.037)	(0.037)
Missing Information At Least 1 Wave	0.109	0.109	-0.063	-0.062	-0.020	-0.021
	(0.306)	(0.306)	(0.436)	(0.438)	(0.326)	(0.330)
NLSY Oversample	0.287	0.290	-0.051	-0.088	0.437	0.421
	(0.407)	(0.408)	(0.542)	(0.543)	(0.461)	(0.466)
			Economic Characteristics			
County Unemployment Rate	0.568	0.569	1.222	1.227	-0.378	-0.381
	(0.694)	(0.694)	(0.939)	(0.944)	(0.725)	(0.732)
State Per-Capital Beer Consumption	-0.411	-0.432	-2.114**	-2.251**	-0.628	-0.719
	(0.690)	(0.692)	(0.949)	(0.985)	(0.792)	(0.827)

Table 13

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
			Economic Characteristics (cont.)			
State Percent of Population with College Educ. or Higher	8.683**	8.769**	14.216***	14.698***	7.030	7.375
	(4.326)	(4.328)	(5.341)	(5.481)	(4.820)	(4.887)
State Real Income Per Capita ($1,000)	-0.109***	-0.110***	-0.168***	-0.171***	-0.064	-0.066
	(0.039)	(0.038)	(0.050)	(0.051)	(0.042)	(0.042)
			Error-Components			
λ		-0.043		-0.395		-0.261
		(0.396)		(0.609)		(0.513)
Log-Likelihood	-832.21	-1,101.11	-832.21	-1,101.11	-832.21	-1,101.11
N	680	680	680	680	680	680
Wald Test, Joint Sig. λs P-Value		[0.850]		[0.850]		[0.850]

Note. Models estimated using unweighted data from the 1997-2011 NLSY97. Robust standard errors are in parenthesis and p-values are in brackets. Models also include region and time fixed effects.

[a] Other occupations is defined as full-time blue-collar/service occupations.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

Table 14

Selected Coefficients from Multinomial Logistic Models of Occupational Choice and 5 or More Underage College Drinks for Male College Graduates

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
Drinking Behavior						
Five or More Underage College Drinks on One or More Days	0.187	0.419	-0.230	0.085	-0.144	0.290
	(0.217)	(0.457)	(0.300)	(0.668)	(0.228)	(0.569)
Demographic Characteristics						
Age	0.129	0.133	0.108	0.113	0.051	0.058
	(0.090)	(0.091)	(0.121)	(0.123)	(0.104)	(0.106)
Black	-0.013	0.080	0.060	0.189	0.135	0.318
	(0.414)	(0.452)	(0.514)	(0.579)	(0.417)	(0.486)
Other	0.098	0.133	0.626	0.678	0.292	0.363
	(0.351)	(0.356)	(0.456)	(0.486)	(0.392)	(0.405)
Hispanic	0.026	0.022	0.603	0.598	-0.721	-0.727
	(0.370)	(0.371)	(0.489)	(0.490)	(0.483)	(0.486)

Table 14

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
	<u>Demographic Characteristics (cont.)</u>					
Married	-0.163	-0.106	0.144	0.215	0.244	0.347
	(0.344)	(0.359)	(0.470)	(0.482)	(0.354)	(0.377)
Number of Children	-0.320	-0.324	-0.928*	-0.926*	-0.966**	-0.961**
	(0.273)	(0.275)	(0.530)	(0.532)	(0.413)	(0.418)
Subjective Health Status	0.151	0.145	0.329**	0.320**	0.139	0.126
	(0.127)	(0.128)	(0.161)	(0.161)	(0.143)	(0.147)
ASVAB Percentile Score in 1999	-0.751	-0.732	0.866	0.896	0.347	0.378
	(0.499)	(0.500)	(0.818)	(0.835)	(0.545)	(0.546)
Missing ASVAB Score	0.050	0.044	0.195	0.190	-0.270	-0.282
	(0.289)	(0.290)	(0.395)	(0.397)	(0.346)	(0.347)
Urban Residence	-0.010	-0.017	0.166	0.161	-0.244	-0.254
	(0.294)	(0.297)	(0.428)	(0.428)	(0.313)	(0.319)
Missing Urban Residence	-0.271	-0.296	0.453	0.411	-0.294	-0.343
	(0.491)	(0.497)	(0.665)	(0.691)	(0.543)	(0.555)

Table 14

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
Survey Characteristics						
Months Since College Graduation	-0.028*	-0.029*	-0.241***	-0.243***	-0.042	-0.044
	(0.014)	(0.015)	(0.049)	(0.049)	(0.038)	(0.038)
Missing Information At Least 1 Wave	0.108	0.103	-0.065	-0.069	-0.015	-0.022
	(0.304)	(0.304)	(0.430)	(0.433)	(0.327)	(0.331)
NLSY Oversample	0.286	0.283	-0.071	-0.080	0.429	0.414
	(0.408)	(0.410)	(0.540)	(0.539)	(0.460)	(0.465)
Economic Characteristics						
County Unemployment Rate	0.586	0.604	1.188	1.213	-0.388	-0.358
	(0.683)	(0.688)	(0.951)	(0.948)	(0.729)	(0.735)
State Per-Capital Beer Consumption	-0.411	-0.475	-2.135**	-2.221**	-0.615	-0.729
	(0.689)	(0.689)	(0.931)	(0.950)	(0.793)	(0.825)
State Percent of Population with College Educ. or Higher	8.647**	8.788**	14.194***	14.423***	7.026	7.298
	(4.317)	(4.324)	(5.407)	(5.438)	(4.811)	(4.879)

Table 14

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
			Economic Characteristics (cont.)			
State Real Income Per Capita ($1,000)	-0.111*** (0.039)	-0.112*** (0.039)	-0.169*** (0.051)	-0.172*** (0.051)	-0.064 (0.042)	-0.067 (0.043)
			Error-Components			
λ		-0.171 (0.305)		-0.233 (0.421)		-0.320 (0.362)
Log-Likelihood	-832.46	-1,176.51	-832.46	-1,176.51	-832.46	-1,176.51
N	680	680	680	680	680	680
Wald Test, Joint Sig. λs P-Value		[0.784]		[0.784]		[0.784]

Note. Models estimated using unweighted data from the 1997-2011 NLSY97. Robust standard errors are in parenthesis and p-values are in brackets. Models also include region and time fixed effects.

[a] Other occupations is defined as full-time blue-collar/service occupations.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

Table 15

Selected Coefficients from Multinomial Logistic Models of Occupational Choice and Any Underage College Days with Two or More Drinks for Male College Graduates

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
Drinking Behavior						
Any Underage College Days with Two or More Drinks	0.170	0.426	-0.507*	0.308	-0.192	0.147
	(0.223)	(0.501)	(0.301)	(0.732)	(0.229)	(0.590)
Demographic Characteristics						
Age	0.130	0.137	0.095	0.117	0.050	0.058
	(0.091)	(0.092)	(0.123)	(0.128)	(0.104)	(0.106)
Black	-0.023	0.081	-0.054	0.264	0.115	0.253
	(0.410)	(0.454)	(0.512)	(0.594)	(0.416)	(0.489)
Other	0.091	0.120	0.596	0.723	0.292	0.333
	(0.350)	(0.354)	(0.460)	(0.503)	(0.391)	(0.402)
Hispanic	0.036	0.049	0.584	0.627	-0.733	-0.718
	(0.370)	(0.374)	(0.493)	(0.509)	(0.485)	(0.489)

Table 15

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
			Demographic Characteristics (cont.)			
Married	-0.180	-0.137	0.127	0.220	0.243	0.298
	(0.344)	(0.352)	(0.464)	(0.487)	(0.351)	(0.364)
Number of Children	-0.325	-0.332	-0.930*	-0.928*	-0.958**	-0.959**
	(0.274)	(0.277)	(0.524)	(0.539)	(0.415)	(0.413)
Subjective Health Status	0.153	0.150	0.337**	0.324**	0.138	0.133
	(0.127)	(0.128)	(0.160)	(0.164)	(0.143)	(0.145)
ASVAB Percentile Score in 1999	-0.749	-0.717	0.817	0.943	0.340	0.378
	(0.499)	(0.502)	(0.818)	(0.866)	(0.545)	(0.548)
Missing ASVAB Score	0.041	0.027	0.236	0.193	-0.262	-0.283
	(0.290)	(0.293)	(0.392)	(0.407)	(0.346)	(0.346)
Urban Residence	-0.013	-0.023	0.167	0.160	-0.238	-0.249
	(0.294)	(0.297)	(0.432)	(0.440)	(0.313)	(0.318)
Missing Urban Residence	-0.256	-0.258	0.400	0.352	-0.305	-0.309
	(0.493)	(0.500)	(0.664)	(0.706)	(0.545)	(0.547)

Table 15

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
<u>Survey Characteristics</u>						
Months Since College Graduation	-0.027*	-0.028*	-0.244***	-0.250***	-0.042	-0.043
	(0.014)	(0.015)	(0.049)	(0.051)	(0.038)	(0.039)
Missing Information At Least One Wave	0.115	0.115	-0.069	-0.073	-0.016	-0.013
	(0.306)	(0.307)	(0.432)	(0.445)	(0.324)	(0.329)
NLSY Oversample	0.275	0.251	-0.006	-0.105	0.448	0.414
	(0.409)	(0.416)	(0.544)	(0.553)	(0.460)	(0.473)
<u>Economic Characteristics</u>						
County Unemployment Rate	0.551	0.534	1.282	1.215	-0.356	-0.381
	(0.689)	(0.691)	(0.942)	(0.966)	(0.728)	(0.737)
State Per-Capital Beer Consumption	-0.408	-0.481	-2.037**	-2.256**	-0.616	-0.711
	(0.693)	(0.693)	(0.930)	(0.978)	(0.790)	(0.828)
State Percent of Population with College Educ. or Higher	8.524**	8.574**	14.364***	14.627***	7.105	7.167
	(4.319)	(4.330)	(5.376)	(5.601)	(4.799)	(4.846)

Table 15

(Cont.)

	MNL	Corrected MNL	MNL	Corrected MNL	MNL	Corrected MNL
	Full-time, Other[a] Occupations	Full-time, Other[a] Occupations	In School	In School	Not In School Nor Employed Full-Time	Not In School Nor Employed Full-Time
<u>Economic Characteristics (cont.)</u>						
State Real Income Per Capita ($1,000)	-0.110***	-0.111***	-0.167***	-0.171***	-0.064	-0.066
	(0.039)	(0.039)	(0.050)	(0.052)	(0.042)	(0.042)
<u>Error Components</u>						
λ		-0.184		-0.599		-0.244
		(0.331)		(0.480)		(0.379)
Log-Likelihood	-831.08	-1,171.16	-831.08	-1,171.16	-831.08	-1,171.16
N	680	680	680	680	680	680
Wald Test, Joint Sig. λs P-Value		[0.583]		[0.583]		[0.583]

Note. Models estimated using unweighted data from the 1997-2011 NLSY97. Robust standard errors are in parenthesis and p-values are in brackets. Models also include region and time fixed effects.

[a] Other occupations is defined as full-time blue-collar/service occupations.

* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

The third and fourth panels of Tables 13–15 list estimates for economic characteristics and the error-components. The estimated coefficients suggest per capita beer consumption, real income, and the percentage of the state population 25 and older with at least a bachelor's degree are strong predictors of occupational choice. State per-capita beer consumption and real income are associated with a reduction in the likelihood of young men being enrolled in school or employed full-time in other occupations, relative to being employed full-time in white-collar occupations. Conversely, increasing the percentage of the state population 25 and older with at least a bachelor's degree is linked to an increase in the likelihood of young men being enrolled in school or employed full-time in other occupations (related to being employed full-time in white-collar occupations). All of the MNL selection models have error-components that are imprecisely estimated (the λ_j's). A formal test of their joint significance using a Wald test fails to reject the null hypothesis that they are jointly equal to zero (which is the baseline model). Therefore, we fail to reject the baseline MNL model which controls for selection on young men's observable characteristics.

The failure to reject the baseline MNL model that controls for selection on observables suggests underage college drinking may be exogenous. Because this result runs counter to Terza and Vechnak (2011) and other studies, we explore potential reasons for why the results may differ. First, it is possible that the MNL selection model is poorly identified because of weak exclusion restrictions. We test for the presence of weak instruments by conducting a Wald test of the joint significance of the exclusion restrictions. *P*-values for these tests are displayed at the bottom of Table 16 and suggest we can reject the null hypothesis at the 1% level or better, providing some evidence that the exclusion restrictions are strong predictors of drinking. An additional concern is that the exclusion restrictions are predictive of occupational choice.

We informally tested this by including the exclusion restrictions as explanatory variables in the occupational choice equations. The results indicate exclusion restrictions are poor predictors (jointly and independently) of occupational choice. A second possibility is that we have sufficiently controlled for young men's unobserved heterogeneity with the restrictions placed on the sample. All of the young men included in our analysis are entering the labor market for the first time and have the same levels of education. A third possibility is that there is insufficient power because of a limited sample size; however, simulation results at similar sample size contradict this hypothesis.

Table 16

Probit Model Coefficients for Underage College Drinking Behaviors

	Any Underage College Drinking	Five or More Underage College Drinks on One or More Days	Any Underage College Days with 2 or More Days
Demographic Characteristics			
Age	-0.058	-0.042	-0.078
	(0.059)	(0.053)	(0.053)
Black	-0.659***	-0.908***	-0.860***
	(0.218)	(0.225)	(0.214)
Other	-0.345*	-0.443**	-0.369**
	(0.209)	(0.181)	(0.186)
Hispanic	-0.092	-0.057	-0.182
	(0.238)	(0.204)	(0.203)
Married	-0.184	-0.389**	-0.200
	(0.201)	(0.188)	(0.183)

Table 16

(Cont.)

	Any Underage College Drinking	Five or More Underage College Drinks on One or More Days	Any Underage College Days with 2 or More Days
Number of Children	0.242	-0.144	-0.044
	(0.177)	(0.203)	(0.166)
Subjective Health Status	0.067	0.004	-0.045
	(0.082)	(0.074)	(0.072)
ASVAB Percentile Score in 1999	0.508*	0.034	-0.208
	(0.306)	(0.281)	(0.278)
Missing ASVAB Score	-0.084	0.040	0.230
	(0.189)	(0.162)	(0.169)
Urban Residence	-0.489**	-0.122	-0.059
	(0.198)	(0.174)	(0.173)
Missing Urban Residence	0.348	0.132	-0.192
	(0.466)	(0.267)	(0.284)
Survey Characteristics			
Months Since College Graduation	0.008	0.006	0.001
	(0.009)	(0.009)	(0.009)
Missing Information At Least 1 Wave	0.071	0.080	-0.004
	(0.195)	(0.167)	(0.174)
NLSY Oversample	0.106	0.139	0.375*
	(0.228)	(0.224)	(0.222)
Economic Characteristics			
County Unemployment Rate	-0.057	-0.205	0.377
	(0.422)	(0.374)	(0.375)
State Per-Capital Beer Consumption	1.274***	0.596	0.730*
	(0.476)	(0.394)	(0.412)
State Percent of Population with College Education or Higher	-3.536	-2.607	-0.589
	(2.866)	(2.388)	(2.529)
State Real Income Per Capita ($1,000)	0.033	0.028	0.006
	(0.026)	(0.021)	(0.022)

Table 16

(Cont.)

	Any Underage College Drinking	Five or More Underage College Drinks on One or More Days	Any Underage College Days with 2 or More Days
Instrumental Variables			
Mean State Real Cigarette Tax previous 4 years ($)	0.074	0.355	0.424
	(0.390)	(0.350)	(0.349)
Mean State Real Cigarette Tax previous 4 years ($) squared	0.064	-0.118	-0.044
	(0.155)	(0.134)	(0.132)
Mean State Beer Tax previous 4 years ($)	1.645	0.011	-0.753
	(1.171)	(0.897)	(0.916)
Mean State Beer Tax previous 4 years ($) Squared	-0.641	-0.129	0.270
	(1.150)	(0.751)	(0.792)
Mean State Blood Alcohol Content Law Previous 4 years	-0.236	-0.241	-0.239
	(0.304)	(0.263)	(0.266)
Mean State Social Host Law previous 4 years	-0.016	0.093	0.157
	(0.167)	(0.138)	(0.143)
Mean State Sunday Sale Ban Law previous 4 years	0.017	-0.041	0.192
	(0.186)	(0.156)	(0.158)
Delinquency Index Score in 1997	0.193***	0.190***	0.162***
	(0.053)	(0.045)	(0.046)
High School Drinking	1.091***	1.147***	1.096***
	(0.141)	(0.120)	(0.121)
Wald Test, Joint Sig. IVs P-Value	[0.000]	[0.000]	[0.000]
N	680	680	680

Note. Models estimated using unweighted data from the 1997-2011 NLSY97. Robust standard errors are in parenthesis and p-values are in brackets. Models also include region and time fixed effects.
* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

Sensitivity Analyses

We test the sensitivity of this analysis to different model assumptions by estimating alternative specifications. An important question is: what specification and model assumptions are driving the results? Table 17 reports coefficients, standard errors, marginal effects, and p-values for tests of the endogeneity of underage college drinking behaviors for various specifications. The top panel lists estimates from models where drinking is measured as any underage college drinking. The middle and bottom panels list results from models that specify drinking as five or more underage college drinks on one or more days and any underage college days with two or more drinks, respectively.

The first row of each panel of Table 17 reports estimates from the MNL models that control for selection on young men's observable characteristics, which are reproduced for convenience. The second row list estimates from MNL models that control for unobservable heterogeneity using the two-stage residual inclusion (2SRI) method developed by Terza et al. (2008). These models provide an alternative method for estimating the relationship between underage college drinking and occupational choice without making the strong parametric assumptions necessary to estimate the MNL selection models. The MNL 2SRI estimates fail to produce any significant coefficient estimates and a joint test of the endogeneity parameters fails to reject the MNL model that controls for selection on observables (the *p*-value is 0.574).

The third row of each panel of Table 17 lists results from MNL models that control for unobservable heterogeneity using the two-stage predictor substitution (2SPS) method. The estimated coefficients are all imprecisely estimated except for the any underage college days with two or more drinks coefficient for the enrollment in school alternative, which is statistically significant at the 5% level. The marginal effects suggest that young men with any underage college days with two or more drinks are 6.9 percentage points less likely to be currently enrolled in school after completing a bachelor's degree; however, a joint test of the endogeneity parameters fails to reject the MNL model that controls for selection on observables (the *p*-value is 0.441).

Table 17

Alternative Models of Occupational Choice and Drinking Behaviors for Male College Graduates

	Full-time, White-Collar Occupation	Full-time, Other[a] Occupation	In School	Not In School Nor Employed Full-Time
Any Underage College Drinking				
Standard Controls[b]	-	0.288	-0.284	-0.095
	(-)	(0.261)	(0.344)	(0.263)
	-0.009	*0.070*	*-0.035*	*-0.026*
Two Stage Residual Inclusion[c]	-	0.338	0.556	0.484
	(-)	(0.619)	(0.939)	(0.653)
	-0.093	*0.021*	*0.027*	*0.045*
Wald Test, Joint Sig. λs P-Value	[0.574]	[-]	[-]	[-]
Two Stage Predictor Substitution[c]	-	0.282	-0.437	-0.205
	(-)	(0.298)	(0.377)	(0.282)
	0.008	*0.080*	*-0.048*	*-0.040*
Wald Test, Joint Sig. λs P-Value	[0.574]	[-]	[-]	[-]
Corrected Logistic Model[c]	-0.372	0.204	0.183	0.154
	(0.524)	(0.609)	(0.926)	(0.726)
	-0.081	*0.039*	*0.016*	*0.025*
Wald Test λ	[0.490]	[0.747]	[0.537]	[0.654]
Two State Least Squares[c]	-0.117	0.023	0.042	0.053
	(0.122)	(0.120)	(0.090)	(0.107)
Hausman-Wu Test	[0.199]	[0.617]	[0.243]	[0.296]

Table 17

(Cont.)

	Full-time, White-Collar Occupation	Full-time, Other[a] Occupation	In School	Not In School Nor Employed Full-Time
Five or More Underage College Drinks on One or More Days				
Standard Controls[b]	-	0.187	-0.230	-0.144
	(-)	(0.217)	(0.300)	(0.228)
	0.001	*0.053*	*-0.024*	*-0.030*
Two Stage Residual Inclusion[c]	-	0.334	0.301	0.245
	(-)	(0.469)	(0.678)	(0.504)
	-0.064	*0.041*	*0.011*	*0.013*
Wald Test, Joint Sig. λs P-Value	[0.794]	[-]	[-]	[-]
Two Stage Predictor Substitution[c]	-	0.149	-0.368	-0.244
	(-)	(0.254)	(0.335)	(0.250)
	0.018	*0.057*	*-0.034*	*-0.041*
Wald Test, Joint Sig. λs P-Value	[0.794]	[-]	[-]	[-]
Corrected Logistic Model[c]	-0.323	0.322	-0.092	0.122
	(0.400)	(0.414)	(0.635)	(0.516)
	-0.069	*0.063*	*-0.009*	*0.020*
Wald Test λ	[0.344]	[0.869]	[0.771]	[0.500]
Two Stage Least Squares[c]	-0.055	0.034	0.002	0.019
	(0.087)	(0.084)	(0.058)	(0.076)
Hausman-Wu Test	[0.373]	[0.829]	[0.573]	[0.413]
Any Underage College Days with 2 or More Drinks				
Standard Controls[b]	-	0.170	-0.507*	-0.192
	(-)	(0.223)	(0.301)	(0.229)
	0.018	*0.062*	*-0.050*	*-0.029*
Two Stage Residual Inclusion[c]	-	0.262	0.466	0.286
	(-)	(0.507)	(0.766)	(0.530)
	-0.066	*0.020*	*0.027*	*0.020*

Table 17

(Cont.)

	Full-time, White-Collar Occupation	Full-time, Other[a] Occupation	In School	Not In School Nor Employed Full-Time
<u>Any Underage College Days with 2 or More Drinks (cont.)</u>				
Wald Test, Joint Sig. λs P-Value	[0.441]	[-]	[-]	[-]
Two Stage Predictor Substitution[c]	-	0.151	-0.727**	-0.299
	(-)	(0.254)	(0.333)	(0.253)
	0.036	*0.072*	*-0.069*	*-0.040*
Wald Test, Joint Sig. λs P-Value	[0.441]	[-]	[-]	[-]
Corrected Logistic Model[c]	-0.346	0.341	0.171	0.007
	(0.424)	(0.466)	(0.708)	(0.534)
	-0.074	*0.066*	*0.015*	*0.001*
Wald Test λ	[0.254]	[0.944]	[0.285]	[0.703]
Two Stage Least Squares[c]	-0.051	0.006	0.006	0.038
	(0.094)	(0.092)	(0.066)	(0.082)
Hausman-Wu Test	[0.377]	[0.486]	[0.308]	[0.315]

Note. *N* = 680. Models estimated using unweighted data from the 1997-2011 NLSY97. Robust standard errors are in parentheses, marginal effects are in italics, and p-values are in brackets.
[a] Other occupations is defined as full-time blue-collar/service occupations.
[b] Standard controls include age, race, ethnicity, marital status, number of children, subjective health status, ASVAB score, missing ASVAB score, residence in an urban area, missing residence in an urban area; months since college graduation, non-response at least one wave, NLSY97 oversample, county unemployment rate, state per-capita beer consumption, state percent of population with college education or higher, state real income per-capita, and region and year fixed effects.
[c] Instrumental variables include the mean state real cigarette and beer excise tax for the previous four years; mean state blood alcohol content, social host, and Sunday sale laws for the previous four years; the young man's delinquency index score in 1997 and an indicator of high school drinking.
* Significant at 0.10 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

The fourth row of each panel of Table 17 lists estimates from binary logistic models that correct for unobserved heterogeneity (MNL selection model). Each of these models specify occupational choice as a binary variable that is set to one if occupational choice j is selected, zero otherwise. These models examine how the results are affected by simultaneously modeling occupational choices using the MNL model framework. An advantage of the logistic model is that it makes similar assumptions to the MNL model, but it is not subject to concerns about IIA. The logistic model results appear to be very similar with the exception that the standard errors are larger. However, this was anticipated because simultaneously modeling the young man's choice set using a MNL model is more efficient. Each logistic model has an endogeneity parameter (λ) that allows us to test for the presence of unobservable heterogeneity. Wald tests of these parameters fail to reject the null hypothesis, suggesting underage college drinking behaviors may be exogenous.

The fifth row of each panel of Table 17 lists results from two-stage least squares (2SLS) models of occupational choice that make the fewest assumptions about functional form. Like the logistic models, the 2SLS models assume occupational choice can be specified using a series of binary variables. The 2SLS estimates are directly comparable to the marginal effects calculated for the non-linear models, and a comparison suggests they produce similar results. We test for the presence of endogeneity in the 2SLS occupational choice models using Hausman-Wu tests and fail to reject the null for all occupational outcomes.

Because the analysis results strongly suggest we cannot reject the MNL models that control for selection on young men's observable characteristics, we test for the presence of IIA using a Hausman test (Hausman & McFadden, 1984). The central idea behind the test is that, if IIA is not present, then the MNL model coefficients will be unaffected by the removal of an occupational alternative. The full model and restricted model are compared, and a chi-squared statistic is generated that can be used to test for the presence of IIA. We conducted the Hausman test by removing the school enrollment occupational choice and comparing the MNL models coefficients. The calculated Hausman test statistics are 16.94, 16.71, and 17.42 for the MNL models that specify drinking as any underage college drinking, five or more underage college drinks on one or more days, and any underage college days with two or more drinks, respectively. The corresponding critical value for these statistics is 73.81 with 52 degrees of freedom at the 5% level. Therefore, we fail to reject the null hypothesis, which suggests IIA is not present. However, it should be noted that the test may have limited power in the present application. Very few controls are statistically significant in the analysis. Therefore, removing an occupational alternative may not have any effect on the other coefficients.

In addition, an examination of the line search performed to determine starting values also suggests that the log-likelihood function is concave and strongly peaked at zero for each λ_j, reinforcing the conclusion that underage drinking behaviors may be exogenous. See Appendix F for the line search results.

Conclusion

This paper uses nationally representative data from the 1997-2011 waves of the NLYS97 to estimate models of occupational choice for recent male college graduates. The analysis addresses important timing issues left unaddressed by the previous literature by exploiting the longitudinal structure of the NLSY97. This allows us to examine the initial occupational choices of young men making similar decisions at the same point in their lives. Focusing on underage college drinking also allows us to closely align our measures of underage drinking with the occupational choices being made by young men. The results fail to reject the hypothesis that underage college drinking is exogenous, providing some evidence that alcohol consumption may be exogenous in young men's occupational choices. This result is consistent with the findings of Anderson et al. (1993). However, the inability to reject the null hypothesis that underage college drinking is exogenous may be due to weak instruments or an artifact of our sample construction. By focusing on recent college graduates, we may have already controlled for all of the unobserved heterogeneity.

Based on specification tests and robustness checks, our preferred specification is a MNL model of occupational choice that controls for selection on young men's observable characteristics. The models include controls for each young man's demographic and background characteristics, economic characteristics, survey design, and region and year fixed-effects. The results suggest underage college drinking, regardless of how it is measured, is not associated with the probability of being employed full-time in white-collar occupations, other occupations, or neither in school nor employed full-time. In contrast, young men with any underage college days with two or more drinks are five percentage points (29.8%) less likely to be enrolled in school after completing a bachelor's degree. This result, while large, is consistent with the findings of Dee and Evans (2003). As a result, underage drinking has important implications for young men who wish to pursue further education.

While the findings of this study suggest underage college drinking has no effect on the occupational choices of young men, the results should be interpreted with caution. All of the results from this study were generated using a highly selective sample of college graduates. Underage college drinking may be affecting occupational choice through male's acquisition of skills. Therefore, excluding college dropouts may be biasing the results downwards because only the highest skilled individuals remain in the sample. The study also does not account for differences in college quality, which may have an important impact on young men's drinking behaviors and occupational choices. In addition, the findings of the study are limited by the small sample size. A larger sample may generate results that are statistically significant.

Our results indicate underage college drinking is not associated with the initial occupational choices of young men that have recently completed a bachelor's degree. However, we find evidence that more severe forms of underage college drinking have large effects on the decision to continue one's education. These results are particularly applicable for young men who intend to become professionals, especially since approximately 80% of college students drink and half of those who report drinking drink in excess (NIAAA, 2014). For these students, any comparative advantages they possess cannot overcome the detrimental effects of excessive underage college drinking. Since professional occupations rely heavily on human capital, the mechanism that is occurring here is likely a reduction in study effort, which translates into poor grades. College administrators and public policy makers can use the evidence presented in this analysis to target polices at the drinking behaviors of students in pre-professional programs.

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APPENDIX A

QUESTIONS IN THE FOOD SECURITY MODULE

Questions asked of all households:

1. "We worried whether our food would run out before we got money to buy more." Was that **often, sometimes**, or never true for you in the last 12 months?

2. "The food that we bought just didn't last and we didn't have money to get more." Was that **often, sometimes**, or never true for you in the last 12 months?

3. "We couldn't afford to eat balanced meals." Was that **often, sometimes**, or never true for you in the last 12 months?

4. In the last 12 months, did you or other adults in the household ever cut the size of your meals or skip meals because there wasn't enough money for food? (**Yes**/No)

5. (If yes to question 4) How often did this happen—**almost every month, some months but not every month**, or in only 1 or 2 months?

6. In the last 12 months, did you ever eat less than you felt you should because there wasn't enough money for food? (**Yes**/No)

7. In the last 12 months, were you ever hungry, but didn't eat, because there wasn't enough money for food? (**Yes**/No)

8. In the last 12 months, did you lose weight because there wasn't enough money for food? (**Yes**/No)

9. In the last 12 months did you or other adults in your household ever not eat for a whole day because there wasn't enough money for food? (**Yes**/No)

10. (If yes to question 9) How often did this happen—**almost every month, some months but not every month**, or in only 1 or 2 months?

Questions asked only of households with children under 18 years of age:

11. "We relied on only a few kinds of low-cost food to feed our children because we were running out of money to buy food." Was that **often, sometimes**, or never true for you in the last 12 months?

12. “We couldn’t feed our children a balanced meal, because we couldn’t afford that.” Was that **often, sometimes**, or never true for you in the last 12 months?

13. “The children were not eating enough because we just couldn’t afford enough food.” Was that **often, sometimes**, or never true for you in the last 12 months?

14. In the last 12 months, did you ever cut the size of any of the children’s meals because there wasn’t enough money for food? (**Yes**/No)

15. In the last 12 months, were the children ever hungry but you just couldn’t afford more food? (**Yes**/No)

16. In the last 12 months, did any of the children ever skip a meal because there wasn’t enough money for food? (**Yes**/No)

17. (If yes to question 16) How often did this happen—**almost every month, some months but not every month**, or in only 1 or 2 months?

18. In the last 12 months did any of the children ever not eat for a whole day because there wasn’t enough money for food? (**Yes**/No)

Note: “Affirmative” responses indicated in **bold**.

Definitions of food security status for households with and without children

Food security status	Households with children	Households without children
Food secure	0-2 affirmative responses	0-2 affirmative responses
Low food security	3-7 affirmative responses	3-5 affirmative responses
Very low food security	8-18 affirmative responses	6-10 affirmative responses

APPENDIX B

CHARACTERISTICS OF ANALYSIS HOUSEHOLDS

	Households with children and unmarried parents			Households with children and married parents			Households with all elderly members			Other adult-only households		
	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Receive d SNAP last month
Food Insecure	0.347	0.534	0.525	0.287	0.524	0.511	0.135	0.425	0.415	0.270	0.584	0.579
	(0.476)	(0.499)	(0.499)	(0.453)	(0.500)	(0.500)	(0.342)	(0.495)	(0.493)	(0.444)	(0.493)	(0.494)
Real SNAP	0.000	3.153	3.376	0.000	3.020	3.297	0.000	1.119	1.170	0.000	1.593	1.708
Ben. ($000)	(0.000)	(1.753)	(1.670)	(0.000)	(1.841)	(1.767)	(0.000)	(0.880)	(0.880)	(0.000)	(1.183)	(1.181)
Standard explanatory variables												
Female head	0.750	0.858	0.863	0.435	0.469	0.464	0.643	0.706	0.709	0.453	0.567	0.574
	(0.433)	(0.349)	(0.344)	(0.496)	(0.499)	(0.499)	(0.479)	(0.456)	(0.454)	(0.498)	(0.496)	(0.495)
Age	38.597	35.336	35.323	40.767	36.831	37.001	73.407	70.468	70.580	42.668	47.443	47.790
	(12.940)	(11.567)	(11.530)	(11.107)	(10.174)	(10.284)	(8.140)	(7.756)	(7.821)	(15.743)	(13.173)	(12.973)
White (reference)	0.663	0.587	0.585	0.796	0.797	0.796	0.815	0.729	0.734	0.735	0.639	0.647
	(0.473)	(0.492)	(0.493)	(0.403)	(0.403)	(0.403)	(0.388)	(0.445)	(0.442)	(0.441)	(0.480)	(0.478)
Black	0.268	0.359	0.362	0.111	0.127	0.123	0.139	0.225	0.222	0.184	0.304	0.298
	(0.443)	(0.480)	(0.481)	(0.314)	(0.333)	(0.329)	(0.346)	(0.418)	(0.416)	(0.388)	(0.460)	(0.457)
Other	0.069	0.053	0.053	0.093	0.076	0.080	0.046	0.046	0.044	0.081	0.057	0.055
	(0.254)	(0.225)	(0.224)	(0.290)	(0.266)	(0.271)	(0.209)	(0.210)	(0.204)	(0.272)	(0.231)	(0.229)
Hispanic	0.309	0.240	0.236	0.403	0.360	0.344	0.088	0.162	0.157	0.157	0.137	0.141
	(0.462)	(0.427)	(0.425)	(0.491)	(0.480)	(0.475)	(0.283)	(0.368)	(0.364)	(0.363)	(0.344)	(0.348)

	Households with children and unmarried parents			Households with children and married parents			Households with all elderly members			Other adult-only households		
	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Receive d SNAP last month
					Standard explanatory variables (cont.)							
Married, spouse	0.000	0.000	0.000	1.000	1.000	1.000	0.238	0.099	0.098	0.207	0.139	0.134
present							(0.426)	(0.298)	(0.298)	(0.405)	(0.346)	(0.340)
< high school	0.265	0.302	0.306	0.316	0.352	0.354	0.352	0.460	0.460	0.192	0.325	0.337
(reference)	(0.441)	(0.459)	(0.461)	(0.465)	(0.478)	(0.479)	(0.478)	(0.499)	(0.499)	(0.394)	(0.468)	(0.473)
Some college	0.651	0.661	0.661	0.562	0.592	0.590	0.555	0.475	0.481	0.642	0.620	0.609
	(0.477)	(0.474)	(0.474)	(0.496)	(0.492)	(0.492)	(0.497)	(0.500)	(0.500)	(0.479)	(0.485)	(0.488)
College graduate	0.084	0.038	0.033	0.122	0.056	0.055	0.093	0.064	0.059	0.166	0.055	0.055
	(0.278)	(0.190)	(0.179)	(0.327)	(0.231)	(0.229)	(0.290)	(0.246)	(0.236)	(0.372)	(0.227)	(0.227)
Immigrant	0.271	0.156	0.146	0.465	0.366	0.355	0.138	0.204	0.196	0.189	0.103	0.106
	(0.445)	(0.363)	(0.354)	(0.499)	(0.482)	(0.479)	(0.345)	(0.403)	(0.397)	(0.391)	(0.303)	(0.308)
No. of adults in	1.882	1.639	1.617	2.535	2.432	2.426	1.282	1.160	1.161	1.813	1.688	1.678
household	(1.087)	(0.890)	(0.870)	(0.918)	(0.838)	(0.842)	(0.467)	(0.410)	(0.415)	(0.974)	(0.881)	(0.875)
Number of children	1.845	2.125	2.133	2.241	2.602	2.637	0.000	0.000	0.000	0.000	0.000	0.000
in household	(1.034)	(1.149)	(1.146)	(1.230)	(1.319)	(1.326)						
Number of disabled	0.124	0.200	0.208	0.127	0.245	0.261	0.138	0.416	0.429	0.235	0.654	0.678
in household	(0.389)	(0.466)	(0.475)	(0.402)	(0.565)	(0.587)	(0.374)	(0.562)	(0.570)	(0.514)	(0.684)	(0.679)
Age youngest	7.250	5.713	5.705	6.208	4.691	4.706						
in household	(5.334)	(4.916)	(4.899)	(4.929)	(4.264)	(4.273)						
Any elderly in	0.071	0.041	0.041	0.065	0.034	0.038	1.000	1.000	1.000	0.107	0.104	0.104
household	(0.257)	(0.199)	(0.199)	(0.246)	(0.181)	(0.191)				(0.310)	(0.305)	(0.306)
Urban residence	0.828	0.812	0.807	0.821	0.771	0.768	0.739	0.765	0.760	0.817	0.777	0.771
	(0.377)	(0.391)	(0.395)	(0.384)	(0.420)	(0.422)	(0.439)	(0.424)	(0.427)	(0.387)	(0.416)	(0.420)

	Households with children and unmarried parents			Households with children and married parents			Households with all elderly members			Other adult-only households		
	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Receive d SNAP last month
					Standard explanatory variables (cont.)							
State unemp. rate	9.427	9.274	9.246	9.583	9.459	9.440	9.278	9.014	8.989	9.364	9.177	9.163
	(1.750)	(1.696)	(1.697)	(1.796)	(1.698)	(1.715)	(1.727)	(1.519)	(1.521)	(1.757)	(1.622)	(1.618)
					Economic explanatory variables							
Head employed	0.588	0.421	0.406	0.604	0.445	0.427	0.100	0.043	0.038	0.489	0.212	0.187
	(0.492)	(0.494)	(0.491)	(0.489)	(0.497)	(0.495)	(0.301)	(0.204)	(0.192)	(0.500)	(0.409)	(0.390)
Real total HH inc.	1.611	1.215	1.187	2.107	1.782	1.761	0.997	0.896	0.905	1.022	0.909	0.896
($0000)	(0.869)	(0.810)	(0.805)	(0.965)	(0.931)	(0.936)	(0.373)	(0.329)	(0.322)	(0.584)	(0.519)	(0.514)
Own home	0.341	0.188	0.187	0.533	0.343	0.341	0.624	0.300	0.306	0.365	0.251	0.246
	(0.474)	(0.391)	(0.390)	(0.499)	(0.475)	(0.474)	(0.484)	(0.458)	(0.461)	(0.481)	(0.434)	(0.431)
Real subjective food	116.430	139.683	141.283	134.392	152.152	153.790	53.291	62.534	60.721	77.360	90.319	90.299
needs	(88.146)	(104.475)	(104.762)	(92.598)	(102.766)	(104.168)	(48.859)	(53.454)	(51.295)	(63.508)	(75.075)	(75.285)
Missing food needs	0.085	0.054	0.053	0.069	0.040	0.038	0.167	0.097	0.099	0.094	0.070	0.065
	(0.279)	(0.227)	(0.224)	(0.253)	(0.196)	(0.190)	(0.373)	(0.296)	(0.299)	(0.291)	(0.256)	(0.247)
					Other assistance							
SBP last month	0.314	0.589	0.598	0.307	0.597	0.612						
	(0.464)	(0.492)	(0.490)	(0.461)	(0.491)	(0.488)						
NSLP last month	0.396	0.698	0.709	0.399	0.723	0.733						
	(0.489)	(0.459)	(0.454)	(0.490)	(0.448)	(0.443)						
WIC last month	0.135	0.303	0.307	0.157	0.366	0.380						
	(0.342)	(0.460)	(0.461)	(0.364)	(0.482)	(0.486)						

	Households with children and unmarried parents			Households with children and married parents			Households with all elderly members			Other adult-only households		
	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Received SNAP last month	No SNAP last year	Received SNAP last year	Receive d SNAP last month
						Other assistance (cont.)						
Food bank last month	0.098	0.264	0.274	0.086	0.250	0.261	0.054	0.265	0.262	0.097	0.369	0.376
	(0.297)	(0.441)	(0.446)	(0.281)	(0.433)	(0.439)	(0.226)	(0.441)	(0.440)	(0.295)	(0.483)	(0.484)
Soup kitchen last	0.004	0.023	0.025	0.004	0.013	0.014	0.007	0.022	0.021	0.016	0.078	0.079
month	(0.062)	(0.150)	(0.157)	(0.063)	(0.113)	(0.117)	(0.086)	(0.146)	(0.143)	(0.126)	(0.268)	(0.270)
						Instruments						
Non-citizen	0.185	0.104	0.095	0.305	0.274	0.266	0.038	0.036	0.037	0.113	0.037	0.037
	(0.388)	(0.306)	(0.294)	(0.461)	(0.446)	(0.442)	(0.191)	(0.185)	(0.190)	(0.316)	(0.188)	(0.190)
Median state	9.121	9.170	9.233	9.114	9.021	9.094	9.220	9.268	9.256	9.128	9.227	9.292
cert. interval	(2.987)	(2.979)	(2.975)	(2.982)	(2.984)	(2.982)	(2.980)	(2.981)	(2.985)	(2.987)	(2.977)	(2.971)
Observations	2266	3529	3130	2655	1591	1344	4391	1090	1004	6523	2824	2489

Note. Means and standard deviations (in parentheses) estimated using weighted household data from the 2009-11 CPS-FSS.

APPENDIX C

CHARACTERISTICS OF ANALYSIS HOUSEHOLDS

Table C.1

Means of the Analysis Variables for Households with Income Less Than 130% of the Federal Poverty Threshold

	All Households	Unmarried Parents	Married Parents	All Elderly	Other Adults
Head Demographic Characteristics					
Female	0.614	0.857		0.673	0.500
	(0.487)	(0.350)		(0.469)	(0.500)
Age	47.807	36.243	38.039	72.696	42.789
	(19.076)	(12.080)	(11.099)	(6.761)	(15.902)
White (Reference)	0.719	0.592	0.797	0.797	0.711
	(0.450)	(0.491)	(0.402)	(0.402)	(0.453)
Black	0.223	0.361	0.122	0.163	0.222
	(0.416)	(0.480)	(0.328)	(0.369)	(0.415)
Other	0.058	0.047	0.080	0.040	0.067
	(0.234)	(0.212)	(0.272)	(0.197)	(0.251)
Hispanic	0.203	0.236	0.413	0.112	0.140
	(0.402)	(0.424)	(0.492)	(0.316)	(0.347)
Married, Spouse Present	0.276			0.189	0.169
	(0.447)			(0.391)	(0.375)
< High School Grad (Reference)	0.354	0.316	0.383	0.487	0.271
	(0.478)	(0.465)	(0.486)	(0.500)	(0.444)
Some College	0.568	0.649	0.545	0.454	0.605
	(0.495)	(0.477)	(0.498)	(0.498)	(0.489)
College Graduate	0.078	0.036	0.072	0.059	0.124
	(0.268)	(0.185)	(0.259)	(0.235)	(0.330)
Immigrant	0.209	0.184	0.424	0.145	0.165
	(0.407)	(0.388)	(0.494)	(0.352)	(0.371)
Household Characteristics					
Number of Adults	1.673	1.573	2.402	1.224	1.703
	(0.861)	(0.856)	(0.795)	(0.431)	(0.885)
Number of Children	0.901	2.047	2.410		
	(1.323)	(1.123)	(1.250)		

	All Households	Unmarried Parents	Married Parents	All Elderly	Other Adults
Household Characteristics (cont.)					
Number of Disabled	0.243	0.175	0.176	0.189	0.362
	(0.503)	(0.437)	(0.464)	(0.426)	(0.587)
Age Youngest HH	32.874	6.435	5.448		
	(27.703)	(5.142)	(4.811)		
Elderly HH Member	0.306	0.052	0.051		0.110
	(0.461)	(0.221)	(0.219)		(0.313)
Urban HH	0.763	0.794	0.771	0.702	0.782
	(0.425)	(0.404)	(0.420)	(0.458)	(0.413)
Economic Characteristics					
Head Employed	0.400	0.543	0.587	0.082	0.437
	(0.490)	(0.498)	(0.492)	(0.275)	(0.496)
Real Total HH Income ($10,000)	1.028	1.110	1.684	0.808	0.803
	(0.637)	(0.670)	(0.782)	(0.293)	(0.446)
Own Home	0.387	0.244	0.477	0.560	0.316
	(0.487)	(0.430)	(0.500)	(0.496)	(0.465)
State Unemployment Rate	5.368	5.395	5.451	5.319	5.343
	(0.981)	(0.978)	(0.964)	(0.972)	(0.996)
Instrumental Variables					
Real Annual Outreach Per Cap, Lag 1 Year	0.366	0.357	0.322	0.383	0.381
	(1.117)	(1.095)	(1.013)	(1.143)	(1.162)
All Vehicles Exempt	0.377	0.392	0.312	0.387	0.393
	(0.473)	(0.476)	(0.452)	(0.475)	(0.476)
State Median Recertification Period	8.019	7.957	8.089	7.976	8.057
	(3.156)	(3.148)	(3.223)	(3.127)	(3.148)
Head is a Non-Citizen, Immigrant	0.127	0.123	0.309	0.036	0.103
	(0.332)	(0.328)	(0.462)	(0.186)	(0.304)
N	54,298	12,918	9,317	13,417	18,646

Note. Means and standard deviations (in parentheses) estimated using weighted household data from the 2001-2008 CPS-FSS.

APPENDIX D

CHARACTERISTICS OF ANALYSIS RESPONDENTS

		Any Underage College Drinking		Five or More Underage College Drinks on One or More Days		Any Underage College Days with 2 or More Drinks	
	All	Drinkers	Abstainers	Drinkers	Abstainers	Drinkers	Abstainers
Occupational Choice Variables							
Full-time, White-collar Occupations	0.365	0.365	0.363	0.365	0.364	0.370	0.356
	(0.482)	(0.482)	(0.483)	(0.482)	(0.482)	(0.483)	(0.480)
Full-time, Other Occupations[a]	0.290	0.303	0.240	0.310	0.262	0.311	0.251
	(0.454)	(0.460)	(0.428)	(0.463)	(0.441)	(0.463)	(0.435)
In School	0.118	0.110	0.144	0.112	0.126	0.104	0.142
	(0.322)	(0.314)	(0.352)	(0.315)	(0.332)	(0.306)	(0.350)
Not In School Nor Employed Full-Time	0.228	0.221	0.253	0.213	0.248	0.215	0.251
	(0.420)	(0.415)	(0.436)	(0.410)	(0.433)	(0.412)	(0.435)
Standard Explanatory Variables							
Age	23.585	23.470	24.007	23.355	23.902	23.420	23.891
	(1.700)	(1.619)	(1.917)	(1.519)	(1.880)	(1.533)	(1.939)
White	0.747	0.790	0.589	0.838	0.622	0.814	0.623
	(0.435)	(0.408)	(0.494)	(0.369)	(0.486)	(0.390)	(0.486)
Black	0.137	0.099	0.274	0.061	0.241	0.077	0.247
	(0.344)	(0.299)	(0.448)	(0.239)	(0.429)	(0.267)	(0.432)

	All	Any Underage College Drinking		Five or More Underage College Drinks on One or More Days		Any Underage College Days with 2 or More Drinks	
		Drinkers	Abstainers	Drinkers	Abstainers	Drinkers	Abstainers
<u>Standard Explanatory Variables (cont.)</u>							
Other	0.116	0.110	0.137	0.102	0.136	0.109	0.130
	(0.321)	(0.314)	(0.345)	(0.302)	(0.344)	(0.312)	(0.337)
Hispanic	0.115	0.114	0.116	0.112	0.119	0.116	0.113
	(0.319)	(0.318)	(0.322)	(0.315)	(0.324)	(0.320)	(0.317)
Married	0.129	0.122	0.158	0.089	0.185	0.104	0.176
	(0.336)	(0.327)	(0.366)	(0.285)	(0.389)	(0.306)	(0.381)
Number of Children	0.088	0.084	0.103	0.048	0.143	0.063	0.134
	(0.377)	(0.360)	(0.435)	(0.248)	(0.500)	(0.287)	(0.501)
Subjective Health Status	1.779	1.794	1.726	1.799	1.752	1.789	1.762
	(0.789)	(0.772)	(0.851)	(0.770)	(0.815)	(0.768)	(0.829)
ASVAB Percentile Score in 1999	0.725	0.738	0.677	0.739	0.705	0.732	0.711
	(0.211)	(0.203)	(0.230)	(0.204)	(0.218)	(0.204)	(0.221)
Missing ASVAB Score	0.137	0.131	0.158	0.129	0.147	0.138	0.134
	(0.344)	(0.338)	(0.366)	(0.336)	(0.355)	(0.346)	(0.341)
Urban Residence	0.853	0.839	0.902	0.853	0.852	0.855	0.847
	(0.347)	(0.359)	(0.295)	(0.345)	(0.350)	(0.344)	(0.353)
Missing Urban Residence	0.043	0.051	0.014	0.051	0.031	0.043	0.042
	(0.202)	(0.219)	(0.117)	(0.220)	(0.175)	(0.203)	(0.201)
Months Since College Graduation	6.847	6.779	7.096	6.754	6.976	6.689	7.138
	(6.457)	(5.643)	(8.838)	(6.037)	(7.002)	(5.824)	(7.491)
Missing Information At Least 1 Wave	0.144	0.139	0.164	0.137	0.154	0.138	0.155
	(0.351)	(0.346)	(0.372)	(0.344)	(0.361)	(0.346)	(0.362)

	All	Any Underage College Drinking		Five or More Underage College Drinks on One or More Days		Any Underage College Days with 2 or More Drinks	
		Drinkers	Abstainers	Drinkers	Abstainers	Drinkers	Abstainers
Standard Explanatory Variables (cont.)							
NLSY Oversample	0.119	0.105	0.171	0.084	0.168	0.102	0.151
	(0.324)	(0.307)	(0.378)	(0.277)	(0.374)	(0.303)	(0.358)
Northeast Region	0.190	0.195	0.171	0.195	0.182	0.195	0.180
	(0.392)	(0.396)	(0.378)	(0.397)	(0.386)	(0.397)	(0.385)
North Central Region	0.249	0.245	0.260	0.266	0.224	0.254	0.238
	(0.432)	(0.431)	(0.440)	(0.443)	(0.418)	(0.436)	(0.427)
South Region	0.347	0.335	0.390	0.305	0.406	0.311	0.414
	(0.476)	(0.473)	(0.490)	(0.461)	(0.492)	(0.463)	(0.494)
West Region	0.215	0.225	0.178	0.234	0.189	0.240	0.167
	(0.411)	(0.418)	(0.384)	(0.424)	(0.392)	(0.428)	(0.374)
County Unemployment Rate	0.574	0.566	0.603	0.552	0.603	0.564	0.591
	(0.219)	(0.204)	(0.264)	(0.198)	(0.242)	(0.207)	(0.238)
State Per-Capital Beer	1.188	1.195	1.164	1.196	1.178	1.193	1.179
Consumption	(0.176)	(0.176)	(0.175)	(0.175)	(0.177)	(0.175)	(0.178)
State Percent of Population with	0.285	0.284	0.288	0.286	0.284	0.286	0.283
College Educ. or Higher	(0.047)	(0.045)	(0.053)	(0.044)	(0.052)	(0.045)	(0.051)
State Real Income Per Capita	43.185	43.079	43.572	43.296	43.031	43.274	43.020
($1,000)	(5.972)	(5.840)	(6.438)	(5.575)	(6.485)	(5.674)	(6.494)
2001-2004	0.249	0.257	0.219	0.251	0.245	0.243	0.259
	(0.432)	(0.437)	(0.415)	(0.434)	(0.431)	(0.429)	(0.439)
2005	0.162	0.167	0.144	0.173	0.147	0.166	0.155
	(0.369)	(0.373)	(0.352)	(0.378)	(0.355)	(0.372)	(0.362)

	All	Any Underage College Drinking		Five or More Underage College Drinks on One or More Days		Any Underage College Days with 2 or More Drinks	
		Drinkers	Abstainers	Drinkers	Abstainers	Drinkers	Abstainers
			Standard Explanatory Variables (cont.)				
2006	0.191	0.210	0.123	0.231	0.136	0.227	0.126
	(0.394)	(0.408)	(0.330)	(0.422)	(0.344)	(0.419)	(0.332)
2007-2008	0.268	0.251	0.329	0.254	0.287	0.261	0.280
	(0.443)	(0.434)	(0.471)	(0.436)	(0.453)	(0.440)	(0.450)
2009-2011	0.131	0.116	0.185	0.091	0.185	0.104	0.180
	(0.338)	(0.321)	(0.390)	(0.289)	(0.389)	(0.306)	(0.385)
			Instrumental Variables				
Mean State Real Cigarette Tax previous 4 years ($)	1.026	1.038	0.980	1.071	0.942	1.054	0.986
	(0.614)	(0.613)	(0.617)	(0.602)	(0.627)	(0.591)	(0.642)
Mean State Beer Tax previous 4 years ($)	0.270	0.275	0.252	0.259	0.289	0.260	0.283
	(0.203)	(0.208)	(0.186)	(0.196)	(0.215)	(0.194)	(0.215)
Mean State Blood Alcohol Content Law Previous 4 years	0.856	0.847	0.890	0.845	0.877	0.839	0.880
	(0.266)	(0.276)	(0.224)	(0.280)	(0.237)	(0.287)	(0.233)
Mean State Social Host Law previous 4 years	0.422	0.426	0.407	0.445	0.381	0.440	0.397
	(0.461)	(0.461)	(0.463)	(0.464)	(0.454)	(0.460)	(0.463)
Mean State Sunday Sale Ban Law previous 4 years	0.273	0.266	0.299	0.267	0.285	0.259	0.293
	(0.410)	(0.406)	(0.423)	(0.404)	(0.421)	(0.401)	(0.421)
Delinquency Index Score in 1997	1.016	1.137	0.575	1.197	0.682	1.259	0.682
	(1.404)	(1.490)	(0.908)	(1.525)	(1.073)	(1.580)	(1.029)

	All	Any Underage College Drinking		Five or More Underage College Drinks on One or More Days		Any Underage College Days with 2 or More Drinks	
		Drinkers	Abstainers	Drinkers	Abstainers	Drinkers	Abstainers
Instrumental Variables (cont.)							
High School Drinking	0.526	0.620	0.185	0.678	0.247	0.716	0.266
	(0.500)	(0.486)	(0.390)	(0.468)	(0.432)	(0.452)	(0.442)
N	680	534	146	394	286	441	239

Note. Means estimated using unweighted data from the 1997-2011 NLSY97. Farming and military occupations are excluded from the sample.

[a] Other occupations is defined as full-time blue-collar/service occupations.

APPENDIX E

DELINQUENCY INDEX QUESTIONS

1. “Have you ever run away, that is, left home and stayed away at least overnight without your parent’s prior knowledge or permission?” (Yes/No)

2. “Have you ever carried a hand gun? When we say hand gun, we mean any firearm other than a rifle or shotgun.” (Yes/No)

3. “Have you ever belonged to a gang?” (Yes/No)

4. “Have you ever purposely damaged or destroyed property that did not belong to you?” (Yes/No)

5. “Have you ever stolen something from a store or something that did not belong to you worth less than 50 dollars?” (Yes/No)

6. “Have you ever stolen something from a store, person or house, or something that did not belong to you worth 50 dollars or more including stealing a car?” (Yes/No)

7. “Have you ever committed other property crimes such as fencing, receiving, possessing or selling stolen property, or cheated someone by selling them something that was worthless or worth much less than what you said it was?” (Yes/No)

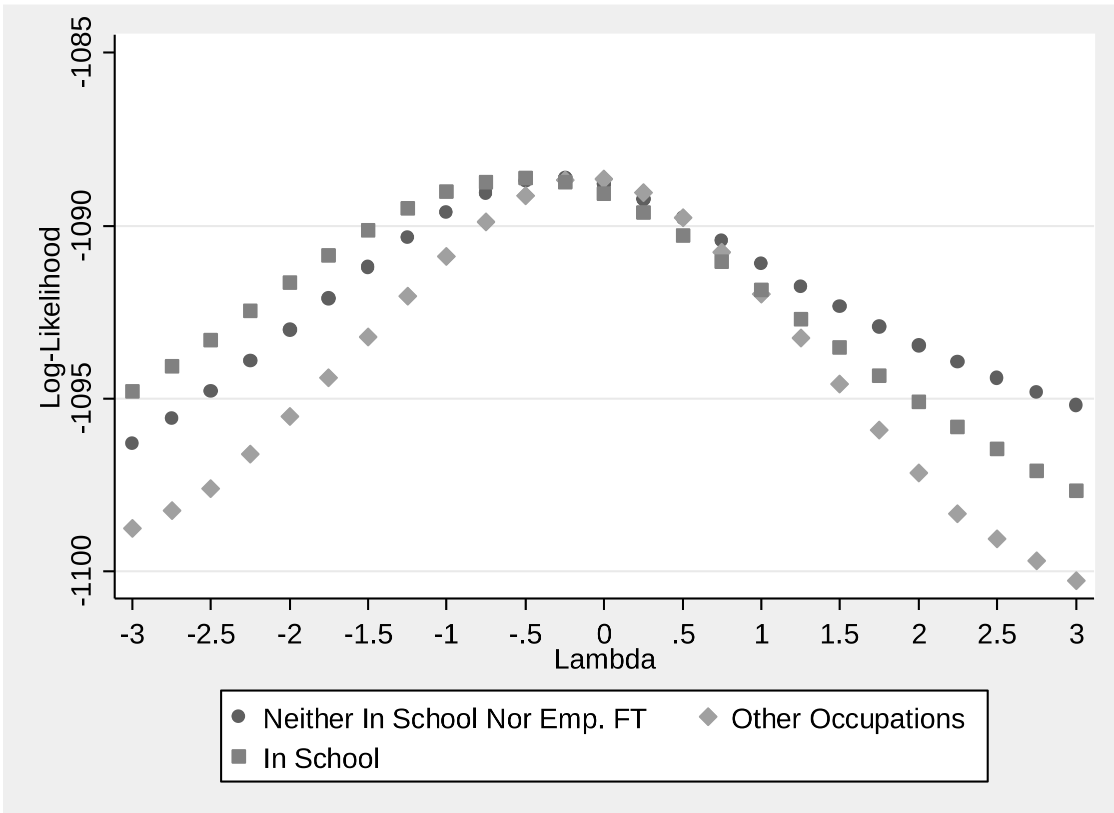
8. “Have you ever attacked someone with the idea of seriously hurting them or have a situation end up in a serious fight or assault of some kind?” (Yes/No)

9. “Have you ever sold or helped sell marijuana (pot, grass), hashish (hash) or other hard drugs such as heroin, cocaine or LSD?” (Yes/No)

10. “Have you ever been arrested by the police or taken into custody for an illegal or delinquent offense (do not include arrests for minor traffic violations)?” (Yes/No)

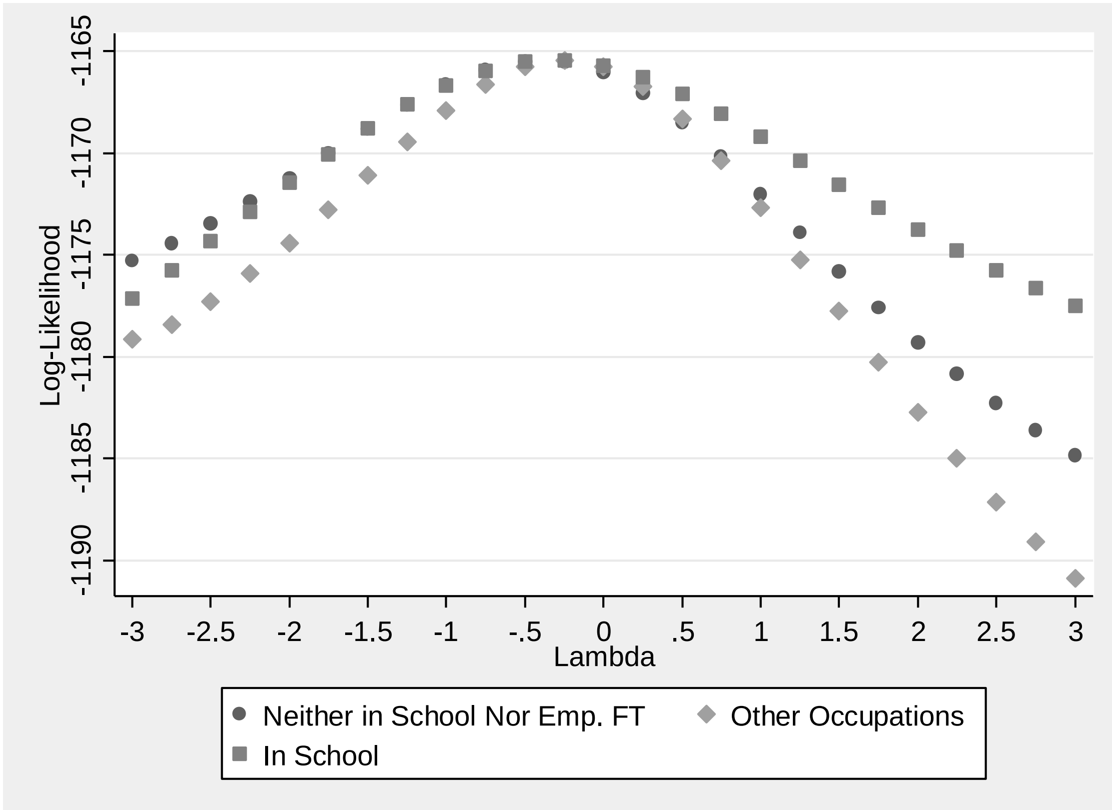
APPENDIX F

LINE SEARCH RESULTS FOR MULTINOMIAL LOGISTIC SELECTION MODELS



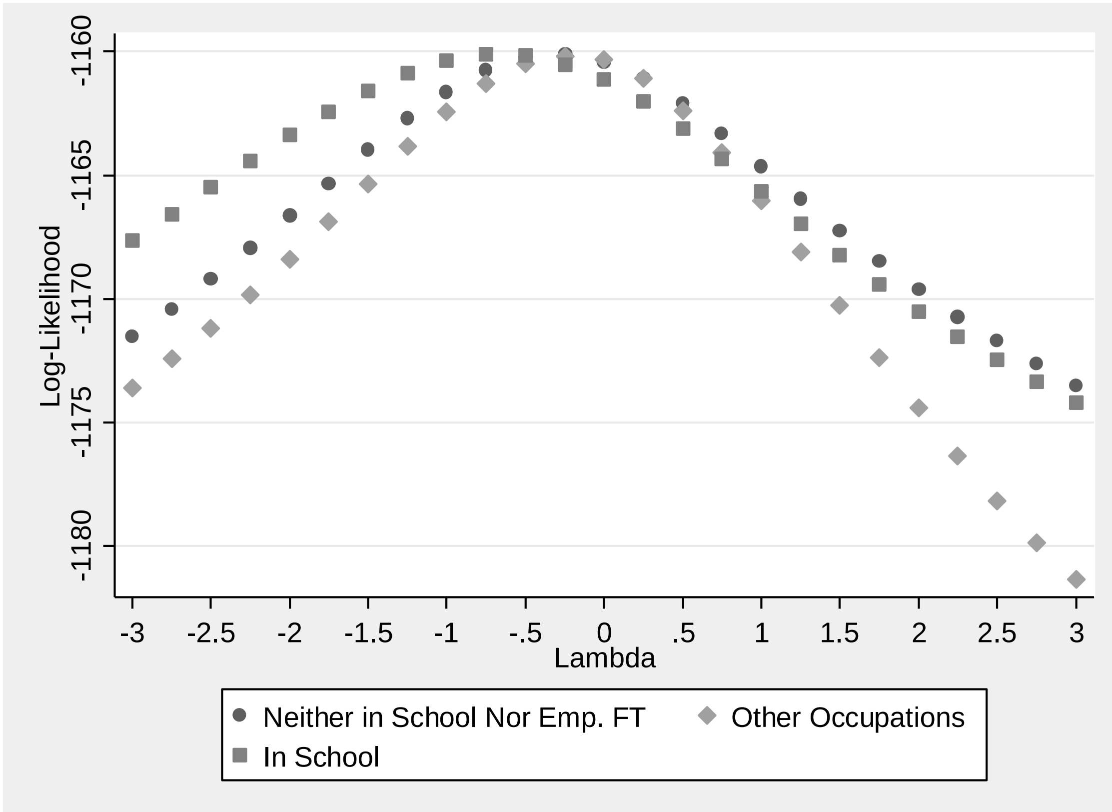
Note: Models estimated using unweighted data from the 1997-2011 waves of the NLSY97. Base outcome is employed full-time in white-collar occupations.

Figure C.1. Line Search Results for Multinomial Logistic Selection Models of Occupational Choice and Any Underage College Drinking.



Note: Models estimated using unweighted data from the 1997-2011 waves of the NLSY97. Base outcome is employed full-time in white-collar occupations.

Figure C.2. Line Search Results for Multinomial Logistic Selection Model of Occupational Choice and Five or More Underage College Drinks on One or More Days.



Note. Models estimated using unweighted data from the 1997-2011 waves of the NLSY97. Base outcome is employed full-time in white-collar occupations.

Figure C.3. Line Search Results for Multinomial Logistic Selection Model of Occupational Choice and Any Underage College Days with Two or More Drinks.