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The Effect Of The Supplemental Nutrition Assistance Program On Mortality

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ABSTRACT The Supplemental Nutrition Assistance Program (SNAP) is the largest food assistance program in the United States. Although participation in it has been shown to reduce food insecurity, there is comparatively less clear causal evidence of positive health effects of participation, particularly among adults. We examined the relationship between SNAP participation and premature mortality using data for 1997–2009 from the National Health Interview Survey, linked to data for 1999–2011 from the National Death Index. Results from bivariate probit models found that participation in SNAP led to a populationwide reduction of 1–2 percentage points in mortality from all causes and a reduction in specific causes of death among people ages 40–64.

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The Supplemental Nutrition Assistance Program (SNAP) is the cornerstone of federal food assistance in the US fight against food insecurity. In fiscal year 2018 SNAP provided $61 billion in nutrition support to over forty million Americans living in twenty million households.[1] The means-tested program provides monthly benefits for food purchases for consumption in the home, accessed through an electronic debit card that is redeemable at about 250,000 certified outlets nationwide. To be eligible, the typical household must either have a gross income of less than 130 percent of the federal poverty level for the relevant family size (approximately $2,100 per month for a family of four) and a net income of less than 100 percent of poverty or be categorically eligible by participating in other government programs.[2] Although details of eligibility and program administration differ across states, the program's basic structure and payment amounts are set at the federal level.

Recent research suggests that SNAP is effective at alleviating hardship across a variety of domains.[3,4] For example, participation in SNAP lowers the risk of household food insecurity by 5–20 percent, depending on the sample and method of identification.[5,6] After households experience a drop in household income, the program has been shown to reduce food consumption losses by 30 percent.[7] And in the period 2000–11 SNAP benefits reduced the incidence of household poverty by 4.9 percent and of deep poverty by 14 percent on average, with even larger effects observed on child poverty.[8]

Despite the program's goal of assisting families with nutritional consumption, comparatively little is known about the causal relationship between SNAP and health outcomes beyond food insecurity. A major challenge for estimation is the fact that characteristics that make a household eligible for SNAP, such as low income, are associated with poor health outcomes, on average.[9] This means that correlational studies that do not control for possible reverse causation often find spuriously positive associations between SNAP participation and poor health.[9–11] There have been some causal studies that documented both the occurrence of trade-offs between food and health care needs among low-income households that lack the financial resources to cover basic needs and the fact that participation in SNAP substantially lowers out-

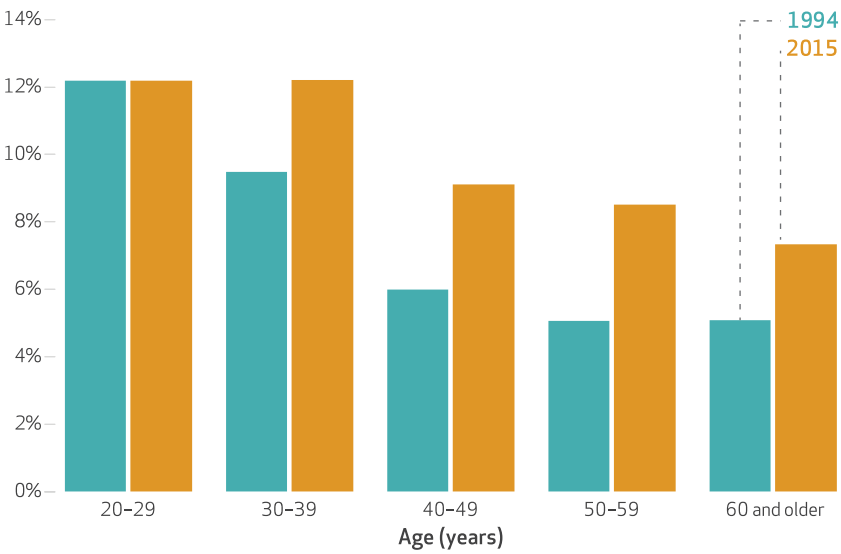
of-pocket health care spending by about $1,400 per year.[12–14] SNAP has also been found to have long-term health benefits in terms of reduced rates of metabolic syndrome among adults exposed to the program during childhood.[15] However, only one prior study directly examined the relationship between SNAP participation and mortality.[16] That study used data from the National Health Interview Survey (NHIS) for the period 1990–94, linked to 1997 data from the National Death Index of the Centers for Disease Control and Prevention, and it found mixed results. For the average person randomly drawn from the population, mortality risk was higher if they participated in SNAP than if they did not, even when unobserved differences related to selection into SNAP participation were controlled for. However, among actual participants in the program, the relationship was reversed, and SNAP participation was protective against mortality. These findings, now fifteen years old, no longer reflect the current policy environment.

At the time of that study, states had much less administrative freedom from federal rules to affect the level of SNAP participation among eligible people. Now, states have implemented a host of policies such as simplified reporting guidelines, longer recertification periods, and electronic benefit transfers, as well as online application systems and call centers that eliminate the need for in-person application in many locations—which collectively increased the share of the eligible population who participate. In addition, broad-based categorical eligibility guidelines in place in many states during our study period increased the income eligibility limit to well above the threshold of net income of 130 percent of poverty that was in place at the time of the previous study, which increased the size of the eligible population. These policy changes are among the leading factors that underlie the increase in SNAP participation among adults ages thirty and older from 1994 (before welfare reform) to 2015 (after welfare reform and the Great Recession)[17] (exhibit 1).

This article expands on the literature about the causal link between SNAP and health by investigating the causal relationship between SNAP participation and the probability of premature mortality in the period 1997–2011. Premature mortality has ascended in policy salience with the well-documented rise in mortality from alcohol poisoning, opioid overdose, and suicide among middle-aged Americans—along with the overall decrease in life expectancy that started in 2014 and the widening geographic inequality in life expectancy and mortality in the United States that started earlier.[18–22] In light of the evidence that SNAP reduces rates of food insecurity and metabolic syndrome in adulthood and reduces health care spending, we hypothesized that SNAP could reduce premature mortality. At the same time, there has been substantial churn into and out of the program, and these periods of transition have been associated with more severe emotional distress—which could contribute to early mortality for specific causes among adults ages 40–64.[23,24]

EXHIBIT 1

Participation in the Supplemental Nutrition Assistance Program, by age, 1994 and 2015



SOURCE Authors' calculations of data from the 1995 and 2016 Annual Social and Economic Supplements to the Current Population Survey, Bureau of the Census.

Study Data And Methods

DATA SOURCES We used individual-level data for 1997–2009 from the NHIS, a nationally representative epidemiologic surveillance survey, linked both to data for 1999–2011 from the National Death Index and to state-year economic and policy data from the National Welfare Data of the University of Kentucky Center for Poverty Research[25] and the SNAP Policy Database of the Economic Research Service, Department of Agriculture.[26] The NHIS contains detailed self-reported information on health status, income, and SNAP participation, as well as detailed demographic information. The National Death Index data contain Multiple Cause of Death information on NHIS respondents who died between the month of the NHIS interview and 2011. We used the restricted-use version of these files, which allowed us to identify cause-of-death codes from the *International Classification of Diseases,* Ninth Revision (ICD-9), and the *International Statistical Classification of Diseases and Re-*

It is possible that the existence of food assistance creates a protective buffer for health even if the assistance is not accessed directly.

lated Health Problems, Tenth Revision (ICD-10). The advantages of using the restricted-use files rather than the public use files were that we could identify specific causes of death beyond the ten broad categories found in the public use files after 2007, the data were not infused with noise to protect NHIS respondents' confidentiality, and we could identify respondents' state of residence at the time of the NHIS interview.[27] All data linkages were conducted by the National Center for Health Statistics.

RESEARCH METHODS We were interested in estimating the effect of SNAP participation on the probability of premature mortality, controlling for known demographic confounders of mortality (age, sex, race, education, income, and health status, among others) as well as unknown confounders. The dependent variable was dichotomous and had a value of 1 if a person had died by 2011 and a value of 0 otherwise. Our focal independent variable also was dichotomous, equaling 1 if the person participated in SNAP and 0 otherwise. After we controlled for demographic characteristics, if participation in SNAP was unrelated to unobserved factors that affect mortality and the model error term was normally distributed, then estimation could proceed from standard probit maximum likelihood. However, the evidence suggests that this exogeneity assumption does not hold and that there is endogenous self-selection into SNAP participation based on unobserved determinants of health status.[4,9,13,15]

Thus, to improve causal inference, we made the assumption that the model error terms governing mortality and SNAP were jointly normally distributed, which led us to use the bivariate probit model.[16,28] Estimation proceeded via full information maximum likelihood and was implemented using the extended probit (eprobit) command in Stata. The estimated coefficients provided only the direction of the effect of SNAP on mortality, not its magnitude. Thus, we used the estimated coefficients to construct two counterfactuals: what the predicted mortality would be if everybody participated in SNAP, and what the predicted mortality would be if nobody participated. For a random person in the population, the difference between these two predictions yields the average treatment effect, while among the subpopulation of people participating in SNAP, the difference yields the average treatment effect on the treated. The online appendix contains additional technical details of estimation.[29]

The demographic confounders of mortality and SNAP participation controlled for in the model included characteristics identified from the baseline NHIS interview. Specifically, indicators were included for age, sex, race, Hispanic ethnicity, educational attainment, marital status, residency within a metropolitan area, and number of people in the household. We checked the sensitivity of the models to the presence of two additional potentially endogenous regressors: self-reported health status (excellent, very good, good, fair, or poor) and income-to-needs ratio (the ratio of total household income divided by the federal poverty threshold for the household size). The arguments for why SNAP might be endogenous to mortality might also apply to self-reported health status and the income-to-needs ratio in this framework. Thus, we tested the sensitivity of the effect of SNAP on mortality without and with controls for self-reported health status and income at the time of the interview.

The bivariate probit model is distinct from a standard instrumental variables model, and, strictly, the nonlinear functional form of the bivariate probit model will provide identification of model parameters, including (in this case) the effect of SNAP on mortality. However, most applications aim to identify variation beyond nonlinearity via exclusion restrictions similar to those in an instrumental variables model, and this is the approach we followed.

To help identify the effect of SNAP, we used factors that determined participation in the program but not mortality, once we controlled for participation in SNAP. The demographic confounders did not satisfy this criterion and belonged in both the SNAP and mortality equations. However, we exploited the time variation of the state policy environment to assist in identifying the effect of SNAP participation on mortality, using the information in the National Welfare Data and the SNAP Policy Database. We included the maximum SNAP benefit according to family size, the share of total state SNAP

dollars issued via an electronic benefit transfer card as opposed to SNAP coupons, the inflation-adjusted value of SNAP outreach funding, and indicator variables for each of the following: whether the state exempted the value of vehicles from eligibility determination, required fingerprinting at the time of application for SNAP, used short recertification periods for SNAP eligibility, used broad-based categorical eligibility, extended eligibility to all legal noncitizens, and used simplified reporting. These variables have been shown to influence participation in SNAP, and we assumed that they did not affect mortality once we controlled for SNAP participation in the mortality model.[17,30]

Some state economic and political variables could affect both the risk of mortality and SNAP participation, so we included them in both equations. These were the state poverty and unemployment rates, an indicator variable for a Democratic governor, and the maximum of either the state or the federal minimum wage. Because other unmeasured state-specific and aggregate macroeconomic conditions could influence mortality and SNAP participation, we included state and year fixed effects in both the mortality and SNAP models.[31]

We began by examining all-cause mortality. The estimation sample size pooled across all years was 970,137 observations. The analysis sample was then limited in three ways. First, we focused on a subsample of households with an income-to-needs ratio below 200 percent of the federal poverty level, the primary target of SNAP policy ($n = 414{,}486$). Second, because mortality and SNAP participation vary substantially across young, middle-aged, and elderly respondents, the sample was restricted to respondents who would be younger than age sixty-five at the end of the study period ($n = 785{,}884$), so that we could investigate whether SNAP reduced premature death among the nonelderly. Third, we combined the age and low-income restrictions to examine mortality among respondents who would be younger than age sixty-five at the end of the survey period and who had household income-to-needs ratios below 200 percent of poverty ($n = 337{,}604$). Finally, we reestimated models for those four samples in which we restricted the definition of *mortality* to that associated with alcoholic liver disease or cirrhosis, poisoning, or suicide. This allowed us to investigate whether SNAP participation protected against mortality due to specific causes of death in an older population that has recently been singled out for greater risk of what have been termed deaths of despair.[18]

LIMITATIONS There were a number of limitations to this study. First, estimating the relationship between SNAP participation and mortality is difficult, given the differential selection into SNAP. We attempted to control for the selection process through our bivariate probit strategy, assisted by our use of the state-year changes in the policy environment to predict SNAP participation in the NHIS interview year.

Second, there were possible intervening factors between the observed period of SNAP participation and the actual date of death that might have changed the health trajectory of people in ways that we were unable to observe because we did not have longitudinal data. However, since it is just as likely that the trajectory could have improved as that it could have worsened, these unobserved factors attenuated our ability to identify an effect of SNAP participation but did not necessarily bias our findings.

Third, a common finding that plagues many social surveys is the underreporting of participation in social assistance programs, including SNAP.[32] While we were not aware of any validation studies on this issue for the NHIS, this form of misclassification error likely pertains to the NHIS. If the mismeasured regressor is continuously distributed, then the standard solution to correct for possible bias is to use an instrumental variables model. However, if the mismeasured regressor is discrete, as in our model, then instrumental variables could identify an upper bound of the treatment effect.[33] The latter result assumes that the misclassification error is unrelated to the model error—and, importantly, that the model's parameters are linear. But if it is related, or if the model has nonlinear parameters, as in our specification, then in general it is not possible to determine the direction of the bias, if it exists.[34] We attempted to control for what might be first-order endogeneity bias via the bivariate probit specification, but we acknowledge that the approach might not fully correct for possible misreporting of SNAP participation.

Finally, mortality and premature death are relatively infrequent events. This increased estimation noise, even with the benefit of a longer time horizon and larger samples than those in previous work. While this noise was reflected in standard errors, the qualitative pattern provided evidence of an overall reduction in mortality for SNAP participants.

Study Results

Exhibit 2 presents summary statistics on mortality and SNAP participation for each income and age sample. The base probability of mortality was 7.02 percent in the full sample but 8.40 percent in the all-age low-income group. Since the

EXHIBIT 2

All-cause mortality, specific-cause mortality, and participation in the Supplemental Nutrition Assistance Program (SNAP) among respondents to the National Health Interview Survey in 1997–2009, by age and income

	All ages		Younger than age 65	
	Full sample	Low-income subsample	Full sample	Low-income subsample
All-cause mortality				
All respondents	7.02%	8.40%	2.04%	2.39%
SNAP participants	7.85	7.77	3.40	3.35
Specific-cause mortality				
All respondents	0.26	0.30	0.24	0.27
SNAP participants	0.41	0.40	0.38	0.37
SNAP participation	5.75	13.44	6.16	14.03

SOURCE Authors' analysis of restricted-use data for 1997–2009 from the National Health Interview Survey, linked to data for 2011 from the National Death Index, Centers for Disease Control and Prevention. **NOTES** This exhibit presents summary statistics. The low-income subsample consists of respondents in households with an income-to-needs ratio below 200 percent of the federal poverty level. The subsample of people younger than age sixty-five consists of respondents who would not have reached age sixty-five by the end of the survey period. Specific-cause mortality consists of mortality caused by alcoholic liver disease or cirrhosis, poisoning, or suicide.

risk of mortality is much higher for the elderly, when we restricted the analysis to include only people who would be younger than age sixty-five by the end of the survey period, the baseline risk fell to 2.04 percent in the full sample and 2.39 percent in the low-income sample. The probability that mortality was associated with alcoholic liver disease or cirrhosis, poisoning, or suicide was relatively small in the entire population (0.26 percent), as well as among those younger than age sixty-five (0.24 percent). However, as highlighted in the literature, the specific causes of death identified here are significantly more likely to be identified as a cause of death among middle-aged adults, relative to people at other ages. When we restricted the sample to people who would be younger than age sixty-five at the end of the survey period, specific causes of death accounted for 11.8 percent of deaths for households of any income level and 11.3 percent of deaths in the low-income sample (data not shown). Finally, SNAP participation was 5.75 percent in the full sample, 13.44 percent in the low-income sample of people of all ages, 6.16 percent among people younger than age sixty-five, and 14.03 percent among the low-income sample of younger people. The lower rates of SNAP participation among the elderly reflect lower take-up rates among those eligible in that age group.[35] The full set of demographic characteristics for each of our four samples split by mortality status is in appendix tables 1–4.[29]

Exhibit 3 presents the estimated effects of SNAP participation on all-cause mortality from the bivariate probit models. As noted above, for each sample we present the predicted probability of mortality for the population under the assumption that nobody participated in SNAP and also under the assumption that everybody participated. We estimated similar probabilities for a random person from the SNAP population as well. We denoted whether the difference in predicted probabilities was significant, which in the former case represented the average treatment effect and in the latter case, the average treatment effect on the treated.[36,37] The complete set of parameter estimates (except state and year fixed effects) from the bivariate probit models is in the appendix.[29]

We predicted that the risk of mortality in the full sample would be 7.09 percent if nobody participated in SNAP, a share that would fall to 6.29 percent if everybody participated (exhibit 3). This yielded a significant average treatment effect of SNAP of 0.8 percentage points. A similar size for the average treatment effect on the treated held in the SNAP population, and both effects increased in magnitude when we controlled for self-rated health and income. There was no substantive or statistically significant difference in predicted mortality between SNAP participants and nonparticipants in the low-income sample.

Our analysis of the sample younger than age sixty-five provides evidence that SNAP significantly reduced predicted mortality and that the effect was larger for SNAP participants (average treatment effect on the treated) than for the population as a whole (average treatment effect) for both the full and the low-income samples. For example, we predicted that the risk of mortality for the population in the full sample younger than age sixty-five would be 2.33 percent if nobody participated in SNAP and 0.79 percent if

EXHIBIT 3

Estimated effects of participation in the Supplemental Nutrition Assistance Program (SNAP) on all-cause mortality among respondents to the National Health Interview Survey in 1997–2009, by age

	Full sample		Low-income subsample	
	Not controlled for health and income	Controlled for health and income[a]	Not controlled for health and income	Controlled for health and income[a]
ALL AGES				
Estimated mortality (%)				
All respondents				
Nobody on SNAP[b]	7.09	6.98	8.40	8.25
Everybody on SNAP[c]	6.29***	5.56***	8.55	7.91
SNAP participants[d]				
Nobody on SNAP[b]	8.85	9.74	7.71	8.21
Everybody on SNAP[c]	7.94***	7.95***	7.85	7.87
Observations	970,137	916,432	414,486	360,997
YOUNGER THAN AGE 65				
Estimated mortality (%)				
All respondents				
Nobody on SNAP[b]	2.33	2.21	2.78	2.67
Everybody on SNAP[c]	0.79***	1.04***	1.34***	1.63**
SNAP participants[d]				
Nobody on SNAP[b]	8.10	6.23	6.23	5.14
Everybody on SNAP[c]	3.33***	3.33***	3.30***	3.31*
Observations	785,884	746,939	337,604	298,789

SOURCE Authors' analysis of restricted-use data for 1997–2009 from the National Health Interview Survey, linked to data for 2011 from the National Death Index, Centers for Disease Control and Prevention. **NOTES** The exhibit shows results based on bivariate probit models. All models included state and year fixed effects and controlled for age, sex, race, ethnicity, education, marital status, urban residence, and household size. The full parameter estimates are in the appendix (see note 29 in text). The low-income subsample and the subsample of people younger than age sixty-five are explained in the notes to exhibit 2. [a]In addition to the controls listed, the models also controlled for self-rated health and the income-to-needs ratio. [b]Assumes that nobody in the relevant group participated in SNAP, as explained in the text. [c]Assumes that everybody in the relevant group participated, as explained in the text. [d]People who actually participated in SNAP. *$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

everybody participated, yielding a significant average treatment effect of approximately 1.55 percentage points. Our estimated average treatment effect on the treated—the difference in predicted mortality among the SNAP population if nobody is on SNAP and everybody is on SNAP—is even larger in both the full and low-income samples, ranging from 0.34 to 2.90 percentage points in models that controlled for income and health. The finding that SNAP reduced the risk of premature mortality among the low-income nonelderly but not among low-income people of all ages could be related to the sharp decline in SNAP participation among older people, as shown in exhibit 1. It is also likely related to the fact that people older than age sixty are not subject to the gross income test for SNAP eligibility, and thus the low-income sample removed eligible seniors who would benefit from the insurance value of SNAP (that is, the psychological value inherent in being able to access the benefit if it were needed).

Exhibit 4 is presented in the same format as exhibit 3, but instead of examining all-cause mortality, we narrow our focus to specific causes of death. We once again present estimated mortality with and without SNAP associated, with the difference in predicted probabilities yielding the associated average treatment effect and average treatment effect on the treated. The full parameter estimates are in the appendix.[29] Among people of all ages, we found no clear relationship between SNAP participation and specific causes of death. However, the full sample of people younger than age sixty-five showed evidence that SNAP significantly reduced the odds of premature mortality—although the estimates became less significant when we controlled for income and health status at the time of the NHIS interview. One reason for this finding may be the infrequency of these specific causes of death among the entire sample, which led to smaller observation sizes, small expected estimates, and relatively large standard errors. When we restricted the sample to people ages 40–64—the age group highlighted in the recent literature[18]—the rate of specific causes of death was 0.35 percent (data not shown), significantly higher than the rate of 0.26 percent found for the sample of all age groups (exhibit 2). In the sample of peo-

EXHIBIT 4

Estimated effects of participation in the Supplemental Nutrition Assistance Program (SNAP) on specific-cause mortality among respondents to the National Health Interview Survey in 1997–2009, by age

	Full sample		Low-income subsample	
	Not controlled for health and income	Controlled for health and income[a]	Not controlled for health and income	Controlled for health and income[a]
ALL AGES				
Estimated mortality (%)				
All respondents				
Nobody on SNAP[b]	0.27	0.26	0.27	0.29
Everybody on SNAP[c]	0.24	0.26	0.73	0.47
SNAP participants[d]				
Nobody on SNAP[b]	0.45	0.42	0.14	0.25
Everybody on SNAP[c]	0.41	0.41	0.40	0.40
Observations	970,137	916,432	414,486	360,997
YOUNGER THAN AGE 65				
Estimated mortality (%)				
All respondents				
Nobody on SNAP[b]	0.29	0.26	0.27	0.28
Everybody on SNAP[c]	0.08***	0.14*	0.32	0.27
SNAP participants[d]				
Nobody on SNAP[b]	1.13	0.68	0.31	0.38
Everybody on SNAP[c]	0.38**	0.38	0.37	0.37
Observations	785,884	746,939	337,604	298,789
AGES 40–64				
Estimated mortality (%)				
All respondents				
Nobody on SNAP[b]	0.45	0.39	0.50	0.50
Everybody on SNAP[c]	0.10***	0.18**	0.46	0.59
SNAP participants[d]				
Nobody on SNAP[b]	2.89	1.64	0.88	0.69
Everybody on SNAP[c]	0.82**	0.81*	0.81	0.80
Observations	369,183	350,442	126,566	107,911

SOURCE Authors' analysis of restricted-use data for 1997–2009 from the National Health Interview Survey, linked to data for 2011 from the National Death Index, Centers for Disease Control and Prevention. **NOTES** The exhibit shows results based on bivariate probit models. All models included state and year fixed effects and controlled for age, sex, race, ethnicity, education, marital status, urban residence, and household size. Specific-cause mortality consists of mortality caused by alcoholic liver disease or cirrhosis, poisoning, or suicide. The low-income subsample and the subsample of people younger than age sixty-five are explained in the notes to exhibit 2. The subsample of people ages 40–64 consists of respondents who could reach age 40 but would not have reached age 65 by the end of the survey period. The full parameter estimates are in the appendix (see note 29 in text). [a]In addition to the controls listed, the models also controlled for self-rated health and the income-to-needs ratio. [b]Assumes that nobody in the relevant group participated in SNAP, as explained in the text. [c]Assumes that everybody in the relevant group participated, as explained in the text. [d]People who actually participated in SNAP. *$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

ple ages 40–64 we found stronger evidence that SNAP participation reduced premature mortality due to these specific causes overall, but not among people with low incomes. This lack of effect among the low-income sample could stem from the fact that the gross income test is waived for people who are disabled, have any household member ages sixty and older, or live in a state that offered broad-based eligibility for SNAP, and thus our using an income threshold of 200 percent of poverty might have excluded vulnerable people still eligible for SNAP based on income.

Discussion

This article estimates the impact of SNAP participation on mortality, both all-cause and for specific causes, for US samples split by income and age. We exploited unique restricted-use data that combined nationally representative data from the NHIS with vital statistics data from the National Death Index. Using bivariate probit models of SNAP participation that controlled for the effect that unmeasured confounders of selection into SNAP have on all-cause mortality, we found average reductions in the risk of premature mortality in the range of 1–2 percentage points associated with SNAP participation. The protective effects of SNAP from specific causes of death

were concentrated among people in middle age.

Our main results present a more consistent relationship between SNAP participation and mortality than previous research did.[16] We found that SNAP participation had a protective effect on overall mortality, while the earlier study found the opposite effect in the population overall.[38] It is not clear why the previous study found that SNAP had an increased effect on overall mortality at the population level, but one conjecture is that our study used considerably stronger exclusion restrictions with more variation because of the changes in the SNAP policy environment. Given the higher transaction costs of participating in SNAP during the previous study's time period, it may be that only those people who derived the highest benefit participated. As a consequence, it is possible that the existence of food assistance, and the knowledge that there is a safety net one can access, creates a protective buffer for health even if the assistance is not accessed directly. Therefore, in the current policy environment, we found that the SNAP program was protective for health.

Both studies found that participation in SNAP reduced the probability of premature all-cause mortality among participants. We also believe that ours is the first study to discuss and analyze the effects of SNAP on mortality from specific causes associated with deaths of despair. We found that 11.3 percent of premature deaths were associated with these specific causes among respondents younger than age sixty-five in low-income households (data not shown). The estimates presented here for the effect of SNAP on specific causes of death were statistically inconclusive overall but protective among the population younger than age 65—and especially for the population ages 40–64 that has figured prominently in the recent literature.[18]

While our results suggest that SNAP participation reduces mortality, more work needs to be done to identify the pathways of this effect. Additionally, we focused here on just two mortality outcomes: all-cause mortality and mortality from specific causes associated with deaths of despair. There are many other potential mortality outcomes that are worthy of future research, such as deaths due to illnesses influenced by nutritional choices (hypoglycemia, hypertension, obesity, and so on). Mortality is also the most extreme health outcome. Relying on it reduces health measurement issues but suggests that SNAP is having effects on various health outcomes that may be more sensitive in the short run than mortality. Lastly, we examined mortality for a period shorter than fifteen years following respondents' health surveys. Conclusions cannot be made about how receiving SNAP benefits in early life or middle age affects life expectancy over longer periods.

Conclusion

SNAP participation is associated with a populationwide average decline of 1–2 percentage points in the risk of mortality. As US policy makers weigh the benefits and costs of food and nutrition programs, the evidence that SNAP participation reduces premature mortality, as well as SNAP's potential impact on other health outcomes, should be considered. ■

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NOTES

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2 These restrictions may be reduced for households with people ages sixty and older or for people with certified disabilities. Participants may also be categorically eligible (though they usually also must have a gross income below 185 percent of poverty) through participation in a state Temporary Assistance for Needy Families program or General Assistance program or in the federal Supplemental Security Income program.

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38 The samples did differ. Patrick Krueger and coauthors restricted their sample to people ages 18–59 with incomes below 130 percent of poverty (see note 16). SNAP reaches a much larger share of the population than their sample, and thus we had a more expansive sample than those authors did.