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Featured Article

Criminal Incarceration, Statutory Bans on Food Assistance, and Food Security in Extremely Vulnerable Households: Findings from a Partnership with the North Texas Food Bank

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Abstract *Leveraging a unique partnership with the North Texas Food Bank, we are able to collect original survey data from food pantry clients in North Texas to investigate a question that has received little attention due to a lack of data. Specifically, we assess the relationship between criminal incarceration and food security. Our analysis suggests minimal impact of incarceration, broadly defined, on household-level food security. However, differentiating between drug- and non-drug-related incarceration, our analysis suggests a positive causal effect of* drug-related *incarceration on food security, particularly among U.S. born households. This is consistent with an important role being played by the Supplemental Nutrition Assistance Program (SNAP) since many states ban participation by former drug offenders. To this end, we document a positive causal effect of incarceration on SNAP participation only among non-drug related offenders. The results call into question the efficacy of statutory bans on program participation for those reintegrating into society.*

Key words: Food security, crime, incarceration, food stamps, SNAP.

JEL codes: I18, I38.

Household-level food insecurity, defined as the absence of sufficient, reliable access to food due to a lack of money and/or resources, is a major public health concern in the United States (Gundersen, Kreider, and Pepper 2011). Recent figures suggest that 17.5 million households (14.3%) are classified as food insecure, with 6.8 million of those households (5.6%) categorized as very low food secure. The figures for households with children are equally as startling. In particular, 3.8 million households with children (9.9%) were considered to be food insecure and another 360, 000 households with children (0.9%) were classified as very low food secure (Coleman-Jensen, Gregory, and Singh 2014).

The consequences of food insecurity are well documented and affect both children and adults alike. Among children, a lack of requisite nutrition is associated with an array of health and educational-related issues including anemia, increased levels of aggression, and decreased cognitive development (Gundersen 2013). With respect to adults, consequences include attenuated nutrient absorption, deficient dietary needs during pregnancy elevating the risk of birth defects, and general physical and mental health problems (Gundersen 2013).

Because of these complications, researchers have focused on trying to better understand the core determinants of food insecurity; widely accepted factors related to food insecurity include socioeconomic and demographic measures. Specifically, households classified as food insecure are generally those with incomes at or below the federal poverty line and headed by single parents, African-American and Hispanic individuals, and less-educated individuals. Food insecurity is also more prevalent in large cities and rural areas relative to suburban areas and regions on the outskirts of major metropolitan areas. While these factors do explain much of the incidence of food insecurity, there remains significant residual variation in need of explanation.

Here, we leverage a unique partnership with the North Texas Food Bank (NTFB) to collect original survey data from food pantry clients. By designing and administering our own survey, we are able to investigate a potential determinant of food insecurity that has received little attention: criminal incarceration.[1] There are several reasons to suspect a causal link between incarceration and current food insecurity, although the sign of the effect is theoretically ambiguous. First, incarceration may be beneficial if it achieves the intended effect of reforming individuals. This possibility is heightened when incarcerated individuals are able to acquire additional human capital while in prison, typically in the form of vocational training (Lockwood et al. 2012). Second, incarceration rates are the highest in the United States relative to the rest of the world (Walmsley 2016). This is particularly true for minority populations that experience higher rates of food insecurity (Cox 2012; Coleman-Jensen, Gregory, and Singh 2014). Accordingly, incarceration reduces household resources through legal fees and diminished labor market prospects (Finlay 2009; Cox and Wallace 2016; Agan and Starr 2017). As a consequence, the effects of criminal convictions tend to be long-lasting, limiting upward mobility in income (Kling 2006). Finally, and highlighted in this study, many individuals with criminal histories are subject to collateral consequences via statutory restrictions (Travis 2002). Specifically, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) limits individuals with drug-related, felony convictions from

[1] *Criminal incarceration in this study can mean prior incarceration of oneself or current and/or prior incarceration of another household member.*

accessing certain nutrition-related federal programs such as the Supplemental Nutrition Assistance Program (SNAP). Individual states can pass legislation opting out or modifying the ban. As recently as September 2013, thirteen states imposed lifetime bans on SNAP participation for individuals with prior drug-related, felony convictions (USDA-FNS 2015). As of October 2018, four states continue to impose lifetime bans with another 23 states imposing modified bans on individuals with prior drug-related, felony convictions (USDA-FNS 2018).

For these reasons we investigate the causal impact of criminal incarceration on household-level food security among food pantry clients in North Texas, as well as possible heterogeneous effects across drug- and non-drug-related incarcerations. This is important for policy discussions since SNAP participation is a likely mitigating factor for food insecurity while at the same time drug-related offenses often preclude individuals from participating in SNAP (Ratcliffe, McKernan, and Zhang 2011; Kreider et al. 2012; Mauer and McCalmont 2013).[2] This is especially relevant for the state of Texas where prior to September 1, 2015 individuals with drug-related, felony convictions faced lifetime bans from the program.[3] To proceed, we use original survey data collected among food pantry clients in North Texas in 2014—a period of time when this lifetime ban was in effect. We then utilize propensity score matching to estimate the Average Treatment Effect on the Treated (ATT) of prior incarceration, relative to no incarceration, on the likelihood of being classified as food insecure. Understanding the limitations and required assumptions of propensity score matching is warranted and caution should be exercised when interpreting the ATT as causal. As such, we assess the robustness of our results to violations of the conditional independence assumption (CIA) (Rosenbaum 1987, 2002; Nannicini 2007; Ichino, Mealli, and Nannicini 2008), and also explore the potential role of unobservables using recent methods proposed in Oster (2019).

Our analysis builds primarily on two previous studies, both of which utilize data from the Fragile Families and Child Well Being Study to assess the impact of *parental* and *paternal* incarceration on household-level food security among households with children using propensity score methods (Turney 2015; Cox and Wallace 2016). Turney (2015) finds that recent *paternal* incarceration is associated with an increased probability of food insecurity, but only for 5-year old children living with their biological fathers. Cox and Wallace (2016) find that *parental* incarceration increases the likelihood of food insecurity by four percentage points. Our study builds on these two previous studies by looking at households with and without children, as opposed to solely looking at households with children, as well as by focusing on the understudied and vulnerable pantry-going population.

We find little evidence of a causal impact of incarceration, broadly defined, on household-level food security. However, failing to account for the heterogeneous nature of incarceration can be misleading. In particular, when distinguishing between drug-related and non-drug-related incarcerations, we find fairly robust evidence that drug-related incarceration

[2] *Though not the focus of the current paper, it is worth noting that banning individuals from participating in SNAP has other public health and safety concerns. For example Tuttle (2019) shows that banning access to SNAP increases recidivism among drug traffickers as these individuals return to crime as a means of making up for the lost transfer income.*

[3] *See https://www.texastribune.org/2015/08/30/supporters-new-law-hopeful-it-will-reduce-repeat-o/. Accessed 23 May 2019.*

contributes to very low food security. Further, we find evidence suggestive of a positive causal effect of criminal incarceration on participation in SNAP, except for drug-related incarcerations. Together, these results point to the role of SNAP as a significant mediator of the possible adverse consequences of incarceration. Moreover, the results suggest that statutory restrictions on nutrition-related federal assistance programs may inhibit the successful rehabilitation and reintegration of previously incarcerated individuals. That said, caution should be used when generalizing our results beyond the North Texas population. Perhaps our experience partnering with the NTFB will serve as a roadmap for others to explore similar issues elsewhere in the United States.

Background

This project sprung from a partnership with the NTFB that begun in 2012. For the first time, NTFB invited research grant applications under the guidance of their director of research. The NTFB's objective was to improve their understanding of the clients served by their more than 170 pantries in the Dallas-Ft. Worth and surrounding areas. Specifically, the NTFB was interested in acquiring additional knowledge of the determinants and consequences of food insecurity in low-income households, as well as learning about any important urban-rural differences in the North Texas region.

While admirable, in our experience, it is uncommon to find a food bank willing to fund research. In conversations we have had with other food banks, the belief is often that funds are better spent on the primary activities of food banks and not on research. However, this partnership afforded us an opportunity to undertake a long-term project that, we believe, has benefited both parties. Specifically, we designed and administered our own survey to individuals from the population perhaps most at-risk of being food insecure: food pantry clients. In so doing, we were able to assemble data on a host of topics not typically found in a single survey. This allows us to examine determinants of food insecurity that are often bypassed due to data constraints. The results of the survey also provided the NTFB with a more detailed picture of their clientele.

The survey was administered at select NTFB pantries across North Texas in the summer of 2014. After the sample of pantries was settled, a pilot survey was conducted to test the survey instrument and train the survey staff. Ninety respondents were interviewed from five different pantries; the data are not included in the final database. The pilot survey allowed the staff to identify ambiguities with specific survey questions, flush out potential inconsistencies with regard to question comprehension, and also train the staff in proper etiquette, sensitivity, and overall protocol when conducting interviews. Once the pilot study was complete, the survey was redesigned accordingly and the study commenced. The survey was administered in either Spanish or English, depending on the preferences of the respondents.

Our partnership with the NTFB benefited the process in three main ways. First, the grant funding covered the costs associated with hiring individuals to administer the survey at 38 different pantries, as well as award a $20 Wal-Mart gift card to each survey respondent for their cooperation. Second, partnering with NTFB gave us access to the various pantries in the NTFB network. Third, NTFB staff was instrumental in the construction of the survey, lending expertise in terms of the subject at hand.

Our partnership benefited us, and Southern Methodist University (SMU) more broadly, in several ways. Most importantly, we were able to collect data on topics not typically found in survey data that also includes the USDA food security module. In Millimet, McDonough, and Fomby (2018) we analyzed the causal effect of financial literacy on food insecurity. Here, we are able to explore the relationship between criminal background, SNAP participation, and food insecurity. Additional data that has yet to be utilized include information on social networks, transportation, and more. Furthermore, despite the sample only including individuals visiting a food pantry, there remains substantial variation in food security status across households. Thus, we are able to assess the determinants of food security even within this very low-income population. Finally, many colleagues at SMU helped advance the project, while simultaneously using it as a learning opportunity. Specifically, we utilized a native Spanish speaker to translate the survey into Spanish. A statistics professor had her class determine how we should draw a stratified random sample as a project. Anthropology graduate students were employed to administer the survey, affording them a valuable opportunity to gain needed field experience.

Of course everything was not smooth sailing; four primary obstacles were confronted. First, despite the backing from the NTFB, not all food pantry directors allowed us access. Despite attempts to arrange our visits to administer the survey ahead of time, four (of 38) pantries refused our request. The worry was that we would deter clients from coming to the pantry. Second, the operation and organization of each pantry varied. This required real-time adjustments to accommodate each unique situation while maintaining the overall objectives of the study. Third, once on-site, individual pantry clients behaved in a manner that could have threatened the randomization process. Specifically, our intention was to interview every fifth client, assuming he or she was willing. The $20 gift card was more than sufficient as no one refused. In fact, it was the opposite. When individuals realized we were compensating them for their time, they sometimes called friends or relatives to tell them to come to the pantry. Thus, rather than our presence deterring individuals from frequenting a pantry, the opposite may have been true. The final obstacle we confronted was dealing with the usual delays in the research process, while the NTFB was not accustomed to the pace of academia. The NTFB was eager to justify the grant process by showing results. However, Institutional Review Board (IRB) approval delayed the roll out of the survey. Lastly, the publication process for the first study utilizing the data was of typical duration. This led to some anxiety on both sides.

Two primary lessons can be learned from our experience. First, projects involving low-income populations benefit enormously from leveraging the access and reputation gained by partnering with an agency already embedded in the community. However, the agency cannot be expected to fund the project. The opportunity cost of funding research is often viewed as too great. Nonetheless, it is our experience that agencies are willing to provide access, infrastructure, or other low-cost means of support if there exists the potential for research findings to be actionable. Second, the long timeline of academic research should be explained beforehand. Depending on the extent of involvement, agencies are eager for results. If projects are designed to be long-term, incorporating short-term objectives is advisable.

Empirics

Data

NTFB Survey. The NTFB survey includes 1,009 individuals at randomly selected food pantries. All surveys were collected over the time period of March–September 2014. As noted in the previous section, respondents were randomly chosen within pantries as well. For this study, it is noteworthy that at the time of data collection, individuals convicted of a drug-related felony faced a statutory ban on SNAP participation.

While food pantry clients are clearly non-random, our sample is representative of an important and large segment of the U.S. population. The NTFB services 13 counties in North Texas, including Dallas and surrounding areas, with a total population of roughly 4.5 million. In 2014 the NTFB provided food to roughly 440,000 unique individuals (Mills et al. 2014). Thus, NTFB's clients represent roughly 10% of the regional population. Nationally, the Feeding America network (of which NTFB is part) is the largest domestic hunger-relief organization in the United States, providing meals to roughly 46.5 million people per year (Weinfield et al. 2014). That means that *at least* 1 in 7 individuals is a food pantry client at least once during a year. As such, understanding food security within the population of pantry clients is of great importance. Millimet, McDonough, and Fomby (2018) verify that our sample looks similar in terms of observed characteristics to both the NTFB and Feeding America's client base.

The data contain individual- and household-level information including measures on basic demographics, household composition, economic resources, safety net utilization, self reported health status, and educational backgrounds. The 18-question USDA food security module (FSM) is included to measure food security over the preceding 12 months according to the USDA's official classification. Lastly, questions were asked to ascertain the criminal history associated with members of each household.[4]

Measurement of Food Insecurity and SNAP Participation. We use three outcomes in our study. The first is a binary indicator denoting whether the household is very low food secure. The second is a binary indicator denoting whether the household is food insecure, which includes both low and very low food secure households. Following the official USDA definitions, these classifications are based on the 18-question FSM, the first ten of which pertain to all households, while the final eight questions only pertain to households with children under the age of 18. Depending on the household composition and number of affirmative responses, households are classified as high food secure, marginally food secure, low food secure, or very low food secure. We analyze two food security-related outcomes: very low food secure and food insecure. The third outcome of interest measures whether or not anyone in the respondents' household received SNAP benefits in the previous 12 months.

In our sample, 5% of the respondents' households are classified as high food secure, 12% are classified as marginally food secure, 83% are classified as low or very low food secure, and 46% are classified as very low food secure. Thus, despite the fact that the sample includes only food pantry clients,

[4]*The survey was available in both English and Spanish. Details on the sample selection process, survey administration, sample representativeness, and a copy of the administered survey can be found in the supplemental appendix of Millimet et al. (2018).*

variation in food security status exists across households. Lastly, 52% of respondents' households received SNAP benefits in the preceding 12 months.

Measurement of Criminal History. In the survey we ask questions to ascertain whether anyone in the household, including the respondent, has *ever* served time in prison. We further ask questions to determine whether the reported incarcerations were a consequence of drug-related offenses (usage and/or distribution) or "other" types of criminal activity.[5] Additionally, we ask whether the respondentsns to determine whether the reported incarcerations were a consequWe found that 29% of the respondents live in a household where either they, or someone else, have served time in prison. Of that 29%, 18% of the imprisonments were a consequence of drug-related offenses. Further, 9% of respondents recall having either a parent or guardian serve prison time at some point during their childhood. Lastly, the control group in all cases is comprised of individuals with no reported record of criminal incarceration.

Controls. In addition to our covariate of interest capturing incarceration, control variables including age, gender, parental prison history, race, education, marital status, and household size are used in the analysis. We do not include measures of household income, debt, or assets, as doing so would likely introduce bias since these variables are not likely to be predetermined (e.g., Acharya, Blackwell, and Sen 2016). Descriptive statistics are provided for the full sample in table 1. To provide insight into how the distribution of observables vary across the subsamples used in the analysis, and to provide a helpful baseline for interpreting the various results presented below, descriptive statistics on the relevant subsamples can be found in supplemental appendix tables A1–A5.[6]

Estimation

To assess the impact of criminal incarceration on household-level food security, we begin by defining the following notation. Let T be a binary variable describing treatment status, where $T = 1$ for households with a current member incarcerated now or in the past, and zero otherwise. Let FI denote one of our food insecurity measures, where $FI = 1$ for food insecure households, and zero otherwise. The average treatment effect on the treated (ATT) is given by

$$\Delta^{ATT} = \Pr(FI_1 = 1|T = 1) - \Pr(FI_0 = 1|T = 1) \tag{1}$$

where FI_1 and FI_0 are potential outcomes associated with $T = 1$ and $T = 0$, respectively (Roy 1951; Rubin 1974). To circumvent the missing counterfactual problem, we use single nearest neighbor (SNN) propensity score matching. The propensity score is given by $\Pr(T = 1|x) = p(x)$, where x is a vector of controls. The estimator is obtained by

[5] *In the survey we asked individuals, conditional on serving or having served time in prison, jail, or another correctional facility, whether they were convicted and sentenced for drug-related crimes (usage, production, and/or distribution). We were not able to distinguish whether or not drugs were the primary reason for the convictions or secondary to some other, primary offense.*

[6] *Because not all respondents answered every question, we report the sample size along with each listed covariate in the various tables of descriptive statistics.*

Table 1 Descriptive Statistics: Full Sample

Variables	N	Mean	SD	Minimum	Maximum
Outcomes					
Food Insecure (1 = yes)	1, 000	0.826	0.379	0	1
Very Low Food Secure (1 = yes)	1, 000	0.461	0.499	0	1
SNAP Participation (1 = yes)	998	0.522	0.500	0	1
Sample Covariates					
HH Prison (1 = yes)	981	0.295	0.456	0	1
HH Prison - Drugs (1 = yes)	980	0.053	0.224	0	1
HH Prison - Non-Drugs (1 = yes)	981	0.242	0.428	0	1
Parent Prison (1 = yes)	991	0.090	0.286	0	1
Age (years)	985	47.82	14.27	16	86
Gender (1 = male)	1, 000	0.207	0.405	0	1
White (1 = yes)	1, 000	0.286	0.452	0	1
Black (1 = yes)	1, 000	0.381	0.486	0	1
Hispanic (1 = yes)	1, 000	0.275	0.447	0	1
Other Race (1 = yes)	1, 000	0.032	0.176	0	1
US Born (1 = yes)	999	0.776	0.417	0	1
Education (1 = No High School)	1, 000	0.103	0.304	0	1
Education (1 = Some High School)	1, 000	0.197	0.398	0	1
Education (1 = High School Degree)	1, 000	0.349	0.477	0	1
Education (1 = Some College)	1, 000	0.191	0.393	0	1
Education (1 = Post-Secondary, Non-degree Award or Associate's Degree)	1, 000	0.102	0.303	0	1
Education (1 = Bachelor's Degree or More)	1, 000	0.058	0.234	0	1
Married (1 = yes)	1, 000	0.319	0.466	0	1
Divorced (1 = yes)	1, 000	0.227	0.419	0	1
Separated (1 = yes)	1, 000	0.097	0.296	0	1
Widowed (1 = yes)	1, 000	0.092	0.289	0	1
Primary Language (1 = English)	1, 000	0.771	0.420	0	1
Total HH Size	984	3.163	2.029	1	17
Children in HH (1 = yes)	975	0.514	0.500	0	1
# of Children among HHs with Children	501	2.467	1.399	1	10

Note: Summary statistics for full sample unless otherwise noted. HH = household. SNAP = Supplemental Nutrition Assistance Program. Children are defined as any HH member 18 years old or younger.

$$\hat{\Delta}^{ATT} = \frac{1}{N_1} \sum_{i \in \{T_i = 1\}} \left(FI_{i1} - \frac{1}{\sum\limits_{l \in \{T_l = 0\}}} \omega_{il} \sum_{l \in \{T_l = 0\}} \omega_{il} FI_{l0} \right) \tag{2}$$

where N_1 is the number of treated households in the sample and ω_{il} denotes the weight given to control observation l by treated observation i. Under

SNN, the estimated missing counterfactual for household i in the treatment group is given by the outcome of a single household l with the closest propensity score in the control group. Thus, $\omega_{il} = 1$ if $d_{il} = |p(x_i) - p(x_l)|$ is less than $d_{il'}$ for all $l' \neq l$ and $T_l = T_{l'} = 0$, and zero otherwise. In practice, the propensity score is estimated using a probit model with controls discussed previously.

We estimate Δ^{ATT} for different outcomes, different treatments, and different samples. In terms of the outcome, we assess food insecurity (i.e., either low or very low food secure), very low food security, and SNAP participation. In terms of treatments, we allow for heterogeneous impacts of drug- and non-drug-related incarceration on food insecurity and SNAP participation. Finally, in terms of the sample, we explore heterogeneity by racial group and country of birth.

For $\hat{\Delta}^{ATT}$ to be unbiased, the conditional independence assumption (CIA) needs to hold (i.e., $FI_0 \perp T|x$).[7] The CIA is violated if treatment assignment and potential outcomes in the untreated state are both affected by unobserved factors. In the present context, unobserved factors affecting both household-level food insecurity and the propensity to be incarcerated—such as unobserved neighborhood attributes or individual mental health issues—are plausible. It is arguable that parental incarceration during the respondent's childhood may capture some of these factors. Nonetheless, we test the sensitivity of our estimates to violations of the CIA utilizing several different techniques. First, we obtain Rosenbaum Bounds on the significance level of our ATT estimates (Rosenbaum 1987, 2002). Second, we re-estimate the ATT after further conditioning on a simulated confounder using parental incarceration (Nannicini 2007; Ichino, Mealli ,and Nannicini 2008). Finally, we compare these results to those from a recently proposed method of assessing sensitivity to unobserved confounders in a linear regression framework (Oster 2019).

Results

Estimates under Conditional Independence

Baseline results are reported in panel A of table 2 and supplemental appendix tables A6–A8. In each table, columns 1 and 2 present the estimated ATT of any incarceration on food insecurity and very low food security, respectively. The various tables correspond to different estimation samples: the full sample or one of three sub-samples (U.S. born only, minorities, or whites).[8] Standard errors are based on Abadie and Imbens (2006). Lastly, the results presented here can be interpreted as being internally valid for

[7]*We also rely on two additional assumptions: (*a*) the sample is* iid *and (*b*) there is common support between control and treatment groups, that is, $P(T = 1|x) \in (0, 1)$. We check for common support in the full and various subsamples used in the estimation and find no overlap violations in the data.*

[8]*Covariate balance was assessed before and after matching by looking at the standardized differences in the raw and matched samples, respectively. Overall, matching achieved or improved balance according to the standardized differences in covariates; in absolute value the majority of differences are not "large." We follow Rosenbaum and Rubin (1985) and consider any absolute standardized difference greater than 0.20 to be large. One area where balance would ideally be better is when using drug-related incarcerations as the treatment. We explored other specifications of the propensity score in an attempt to achieve better covariate balance when using drug-related incarcerations as the treatment, but no alternative specification, when convergence was actually achieved, seemed to improve covariate balance in a meaningful way. With that being said, to the extent that any residual covariate imbalance matters in*

Table 2 Effect of Any Incarceration on Household Food Security and SNAP Participation (ATT) Relative to Never Incarcerated, U.S. Born

		Food insecure (1)	Very low food secure (2)	SNAP participation (3)
Panel (A)	ATT	0.066	0.045	0.160
		(0.047)	(0.060)	(0.066)
		[0.163]	[0.489]	[0.014]
Panel (B)	ATT	0.066	0.045	0.160
	RB: $\Gamma = 1.00$	0.026	0.166	0.000
	RB: $\Gamma = 1.25$	0.151	0.616	0.016
	RB: $\Gamma = 1.50$	0.383	0.908	0.127
	RB: $\Gamma = 1.75$	0.626	0.987	0.383
	RB: $\Gamma = 2.00$	0.804	0.999	0.667
Panel (C)	Sens ATT	0.048	−0.009	0.183
		(0.055)	(0.079)	(0.080)
		[0.384]	[0.907]	[0.022]
	N	726	726	725

Note: Panel (A) reports the ATT estimated using single nearest neighbor propensity score matching. ATT = Average Treatment Effect on the Treated. Abadie-Imbens standard errors in parentheses; p-values in brackets. Panel (B) reports upper Rosenbaum bounds (RB) on corresponding p-value. Γ = log odds of differential treatment assignment due to unobserved factors. Panel (C) reports the ATT after conditioning on a simulated confounder (Sens ATT), see text for more details. Standard errors are shown in parentheses using 250 reps; p-values appear in brackets. N = number of observations. Treatment is defined as being incarcerated for any reason and the control group consists of individuals having never been incarcerated.

food pantry clients in North Texas. However, caution should be applied when generalizing the results to the broader U.S. population of food pantry clients.[9]

Looking at the role of incarceration, the results generally indicate a positive but statistically insignificant association between incarceration and the two measures of food insecurity. To explore the possibility that SNAP participation acts as a mediator, ameliorating any negative effect of incarceration on food security, we next examine the association between incarceration and SNAP participation. If experiencing incarceration increases the likelihood of participating in SNAP, given the loss of income from imprisonment or the lack of employment opportunities upon release, then perhaps the increase in SNAP participation is mediating the direct negative effects of incarceration on food security, as the beneficial causal effect of SNAP on food security is (reasonably) well established (Ratcliffe, McKernan, and Zhang 2011; Kreider et al. 2012). As shown in column 3 of panel A in table 2 and supplemental appendix tables A6–A8, we find a positive and statistically significant association between incarceration and the likelihood of participating in SNAP. This is true not only for the full sample,

terms of biasing the estimator, this should be picked up by the sensitivity analysis testing the conditional independence assumption.

[9]*We were able to compare our sample to the national sample and Texas sample used in the Hunger in America (HIA) 2014 survey to assess its representativeness of the pantry-going population. Overall, our sample composition seems quite comparable to the HIA North Texas and national samples of food pantry clients. As noted earlier, more information on sample representativeness can be found in the supplemental appendix of Millimet et al. (2018).*

Table 3 Effect of Drug-Related Incarceration on Household Food Security and SNAP Participation (ATT) Relative to Never Incarcerated, U.S. Born

		Food insecure (1)	**Very low food secure (2)**	**SNAP participation (3)**
Panel (A)	ATT	0.279	0.326	0.000
		(0.099)	(0.122)	(0.133)
		[0.005]	[0.008]	[1.000]
Panel (B)	ATT	0.279	0.326	0.000
	RB: $\Gamma = 1.00$	0.001	0.002	0.500
	RB: $\Gamma = 1.25$	0.005	0.010	0.500
	RB: $\Gamma = 1.50$	0.012	0.028	0.500
	RB: $\Gamma = 1.75$	0.024	0.057	0.500
	RB: $\Gamma = 2.00$	0.039	0.097	0.500
Panel (C)	Sens ATT	0.205	0.296	0.051
		(0.127)	(0.157)	(0.168)
		[0.108]	[0.059]	[0.761]
	N	441	441	440

Note: Panel (A) reports the ATT estimated using single nearest neighbor propensity score matching. ATT = Average Treatment Effect on the Treated. Abadie-Imbens standard errors appear in parentheses; p-values are shown in brackets. Panel (B) reports upper Rosenbaum bounds (RB) on corresponding p-value. Γ = log odds of differential treatment assignment due to unobserved factors. Panel (C) reports the ATT after conditioning on a simulated confounder (Sens ATT), see text for more details. Standard errors are shown in parentheses using 250 reps; p-values appear in brackets. N = number of observations. Treatment is defined as being incarcerated for drug-related reasons and the control group consists of individuals having never been incarcerated.

but also the U.S. born and minority subsamples. Specifically, the impacts are 0.131 ($p = 0.015$), 0.160 ($p = 0.014$), and 0.172 ($p = 0.015$) for the full sample (table A6), U.S. born (table 2), and minority (table A7) subsamples, respectively. Since the SNAP participation rate is roughly 50% in our sample, this represents a substantial increase.

Next, we explore possible heterogeneity in the association between incarceration and food insecurity and SNAP participation depending on whether the incarceration was drug related or not. Results are shown in panel A of tables 3 and 4 and supplemental appendix tables A9 and A10.[10] Three results immediately stand out. First, we find a positive and economically meaningful association between drug-related incarcerations and the likelihood of being classified as food insecure or very low food secure. For the full sample (table A9), a drug-related arrest is associated with a 0.204 ($p = 0.025$) and 0.224 ($p = 0.053$) increase in the probability of being food insecure and very low food secure, respectively. The associated magnitudes are even larger when looking at U.S. born households (table 3). Specifically, the estimates increase to 0.279 ($p = 0.005$) and 0.326 ($p = 0.008$) for food insecure and very low food secure, respectively.

Second, we find no statistically meaningful association between non-drug-related incarceration and the likelihood of being classified as either food insecure or very low food secure. Finally, we find that the positive association between incarceration and SNAP participation reported for the

[10]*Due to issues associated with small sample sizes, we only estimate the ATT using the full sample and U.S. born sub-sample when looking across drug versus non-drug related incarceration.*

Table 4 Effect of Non-Drug-Related Incarceration on Household Food Security and SNAP Participation (ATT) Relative to Never Incarcerated, U.S. Born

		Food insecure (1)	Very low food secure (2)	SNAP participation (3)
Panel (A)	ATT	0.085	0.025	0.110
		(0.052)	(0.069)	(0.066)
		[0.101]	[0.719]	[0.094]
Panel (B)	ATT	0.085	0.025	0.110
	RB: $\Gamma = 1.00$	0.015	0.314	0.015
	RB: $\Gamma = 1.25$	0.094	0.749	0.144
	RB: $\Gamma = 1.50$	0.265	0.947	0.436
	RB: $\Gamma = 1.75$	0.481	0.992	0.725
	RB: $\Gamma = 2.00$	0.675	0.999	0.896
Panel (C)	Sens ATT	0.041	−0.065	0.183
		(0.057)	(0.083)	(0.079)
		[0.468]	[0.435]	[0.020]
	N	683	683	682

Note: Panel (A) reports the ATT estimated using single nearest neighbor propensity score matching. ATT = Average Treatment Effect on the Treated. Abadie-Imbens standard errors appear in parentheses; p-values are shown in brackets. Panel (B) reports upper Rosenbaum bounds (RB) on corresponding p-value. Γ = log odds of differential treatment assignment due to unobserved factors. Panel (C) reports the ATT after conditioning on a simulated confounder (Sens ATT), see text for more details. Standard errors appear in parentheses using 250 reps; p-values are shown in brackets. N = number of observations. Treatment is defined as being incarcerated for non-drug-related reasons and the control group consists of individuals having never been incarcerated.

full sample is driven by households with members incarcerated for non-drug-related offenses. Specifically, we estimate that non-drug-related incarcerations are associated with a 0.155 ($p = 0.021$) and 0.110 ($p = 0.094$) increase in the likelihood of SNAP participation for the full sample (table A10) and U.S. born subsample (table 4), respectively. We find no statistically meaningful association between drug-related incarceration and the likelihood of participating in SNAP. The lack of an association between *non-drug-related* offenses and food insecurity, but its positive association with SNAP participation, combined with the opposite results for *drug-related* offenses, is consistent with SNAP participation being an important mediator of the adverse consequences of incarceration. To explore whether these associations likely represent causal relationships, we turn to several sensitivity tests.

Allowing for Deviations from Conditional Independence

While the relationships reported in the previous section are very interesting, assessing whether the associations represent causal relationships is vital for policymaking. However, identification of the causal effect of criminal incarceration on food insecurity is quite challenging, especially in the absence of a valid exclusion restriction. Estimation of equation (2) assuming criminal incarceration to be exogenous is likely to yield a biased estimate $\hat{\Delta}^{ATT}$. To proceed in the absence of a valid exclusion restriction, we rely on several existing methods to assess the sign and magnitudes of the estimates presented in panel A of tables 2–4 and supplemental appendix tables A6–A10.

The first method is referred to as Rosenbaum Bounds (RB) on the significance level of our ATT estimates (Rosenbaum 1987, 2002). RBs provide lower bounds on the significance level of the matching estimates under various assumptions concerning the odds ratios of treatment assignment for observationally identical individuals. The ratio of odds between two observationally identical individuals, denoted by Γ, indicates how important unobserved factors can be in driving treatment assignment. For example, when $\Gamma = 1$, observationally identical individuals are assumed to be equally likely to be treated. As Γ increases beyond one, then the treatment probabilities of observationally identical individuals are allowed to diverge. If $\Gamma = 2$, then the treatment probabilities of observationally identical individuals can differ by a factor of two. If the estimated treatment effect, obtained under CIA is statistically significant (i.e., when $\Gamma = 1$) and remains statistically significant even when Γ is allowed to be as large as two, then the treatment effect is said to be free from hidden bias (Aakvik 2001; Rosenbaum 2002). Otherwise, the treatment effect obtained under CIA is said to be sensitive to hidden bias. If the estimated treatment effect obtained under CIA is statistically insignificant, then the RBs provide no additional insights.

The RB results are presented in panel B of tables 2–4 and supplemental appendix tables A6–A10. In each column, we repeated the estimated ATT obtained under CIA in the first row of the panel. Next, we report the lower bound on the estimated significance level, obtained using the Mantel and Haenszel (1959) statistics, as Γ varies from one to two (Aakvik 2001). In terms of the ATT of incarceration on food insecurity and very low food security, the only estimates that are statistically significant under CIA in panel A are found in table 3 and table A9 when the treatment is defined as drug-related incarceration. In table A9 (full sample), we find the effect of drug-related incarceration on food insecurity to remain marginally statistically significant (at the $p < 0.10$ significance level) until Γ nears two. The effect on very low food security becomes statistically insignificant at conventional levels when $\Gamma = 1.5$. Turning to table 3 (U.S. born subsample), we find robust evidence of a causal effect of drug-related incarceration on the probability of being food insecure and very low food secure; $p < 0.05$ an $p < 0.10$, respectively, even when $\Gamma = 2$. However, statistical significance of the estimated effect on being food insecure is not robust to the simulated confounder (discussed more below). Thus, overall we find some evidence of a causal effect of *drug-related* incarceration on food insecurity, particularly the likelihood of being very low food secure among U.S. born.

In terms of the effect of incarceration on SNAP participation, estimates presented in panel A fail to be robust to violations of the CIA. In all cases where the estimated ATT obtained under CIA is statistically significant, the estimates lose statistical significance when $\Gamma \geq 1.5$.

The next method of assessing sensitivity of the estimated to unobserved confounders is based on the approach devised in Ichino, Mealli, and Nannicini (2008). To proceed, we re-estimate the ATT after further conditioning on a simulated confounder. Specifically, an additional binary covariate is simulated and then added to the set of controls included in the propensity score model. The binary variable is simulated to match the joint distribution of the binary variable capturing the respondent's parental incarceration history, the respondent's treatment assignment, and the respondent's outcome. By assessing the distribution of the estimated ATT after augmenting the conditioning set to include the additional control we can learn how sensitive the estimated treatment effect obtained under CIA is to an unobserved variable resembling parental incarceration. Intuitively, the magnitude in the deviations in the estimated ATT relative to including and not including the simulated confounder provides insight to the robustness of the estimates.

The results can be found in panel C in tables 2–4 and supplemental appendix tables A6–A10 and are based on 250 simulations. In terms of the effect of incarceration on food insecurity and very low food security, in only one case is the sign and magnitude of the estimated effect moderately robust. This is the effect of *drug-related* incarceration on the likelihood of very low food security among the U.S. born (table 3). Here, the estimated effect is positive and large in magnitude; the point estimate is 0.296 ($p = 0.059$). This is the case where the RBs also provided evidence of a robust effect.

With respect to the effect of incarceration on SNAP participation, the Ichino, Mealli, and Nannicini (2008) approach provides much stronger evidence of a causal effect than the RBs. Specifically, table 2 and table A6 indicate a positive causal effect of incarceration using the U.S. born subsample and full sample on SNAP participation; estimates are 0.183 ($p = 0.022$) and 0.163 ($p = 0.027$), respectively. Table 4 and table A10 similarly indicate a positive causal effect of *non-drug-related* incarceration in the U.S. born subsample and full sample on SNAP participation; estimates are 0.183 ($p = 0.020$) and 0.159 ($p = 0.041$), respectively.

Finally, we implement the method proposed in Oster (2019), which builds on Altonji, Elder, and Taber (2005). The method differs from the preceding approaches as now the ATT is initially estimated via Ordinary Least Squares (OLS). Then we compute δ, the amount of selection on unobserved factors relative to the amount of selection on observed factors, which is necessary in order for the true ATT to be identically zero. Small values of δ (in absolute value) suggest that the estimated ATT is highly sensitive to selection on unobserved factors and should not be considered robust.

The results are shown in tables 5 and 6 and yield three findings. First, the OLS estimates of the ATT are qualitatively similar to the matching estimates presented above. Thus, the functional form assumptions imposed by the switch to a parametric regression framework do not appear consequential in this case. Second, the estimates of the ATT of incarceration on the two food insecurity outcomes are highly sensitive to even small amounts of selection on unobserved factors in most cases. The one exception to this is in table 6 when examining the impact of *drug-related* incarceration on food insecurity. The OLS estimate is statistically significant and economically large (coefficient $= 0.142$, *s.e.* $= 0.042$) and $\delta = 1.173$ implying that selection on unobserved factors would have to be more important than selection on observed factors in order to fully explain the OLS estimate. Given the controls used, including the respondent's parent's incarceration history, this seems unlikely. Finally, there is strong evidence of a robust, positive estimated effect of (any) incarceration on SNAP participation in the full sample and the U.S. born and minority subsamples; there is also a robust, positive estimated effect of *non-drug-related* incarceration in the U.S. born subsample.

Overall, the three methods of assessing sensitivity to unobserved confounders provide some evidence of a positive causal effect of *drug-related* incarceration on food insecurity and a positive causal effect of *non-drug-related* incarceration on SNAP participation. Given the lack of purely exogenous

Table 5 Sensitivity Analysis: Unobserved Factors Required to Explain the ATT (Any Incarceration)

	Food insecure (1)	**Very low food secure (2)**	**SNAP participation (3)**
ATT: Full Sample	0.074	0.056	0.111
	(0.028) [δ = 0.219]	(0.040) [δ = 0.056]	(0.039) [δ = 1.146]
ATT: U.S. Born	0.084	0.047	0.125
	(0.031) [δ = 0.139]	(0.044) [δ = 0.063]	(0.042) [δ = 7.390]
ATT: Minorities	0.079	0.116	0.093
	(0.033) [δ = 0.634]	(0.048) [δ = 0.222]	(0.047) [δ = 1.033]
ATT: Whites	0.051	−0.025	0.138
	(0.053) [δ = 0.093]	(0.074) [δ = -0.307]	(0.076) [δ = 0.657]
N Full Sample	928	928	926
N US Born	726	726	725
N Minority	653	653	651
N White	252	252	252

Note: ATT estimated via OLS. ATT = Average Treatment Effect on the Treated. *N* = number of observations Heteroskedasticity-robust standard errors appear in parentheses; δ in brackets, where δ is the amount of selection on unobserved factors relative to the amount of selection on observed factors in order for the estimated ATT to be zero. Treatment is defined as being incarcerated for any reason and the control group in all cases consists of individuals having never been incarcerated. See text for further details.

Table 6 Sensitivity Analysis: Unobserved Factors Required to Explain the ATT (Drug vs. Non-Drug)

	Food insecure (1)	**Very low food secure (2)**	**SNAP participation (3)**
ATT: Full Sample	0.142	0.256	0.113
(Drug)	(0.042) [δ = 1.173]	(0.071) [δ = 0.206]	(0.079) [δ = -0.328]
ATT: Full Sample	0.057	0.003	0.115
(Non-Drug)	(0.031) [δ = 0.168]	(0.043) [δ = 0.003]	(0.042) [δ = 0.832]
ATT: U.S. Born	0.154	0.306	0.061
(Drug)	(0.048) [δ = 0.408]	(0.076) [δ = 0.361]	(0.087) [δ = -0.240]
ATT: U.S. Born	0.067	−0.011	0.143
(Non-Drug)	(0.033) [δ = 0.106]	(0.047) [δ = -0.015]	(0.045) [δ = 4.174]
N Full Sample (Drug)	624	624	622
N Full Sample (Non-Drug)	879	879	877
N US Born (Drug)	441	441	440
N US Born (Non-Drug)	683	683	682

Notes: ATT estimated via OLS. ATT = Average Treatment Effect on the Treated. *N* = number of observations. Heteroskedasticity-robust standard errors appear in parentheses; δ in brackets, where δ is the amount of selection on unobserved factors relative to the amount of selection on observed factors in order for the estimated ATT to be zero. Treatment is defined as either being incarcerated for drug-related reasons or non-drug-related reasons, and the control group in all cases consists of individuals having never been incarcerated. See text for further details.

variation in incarceration, however, these findings should be interpreted with caution.[11] Nonetheless, these findings suggest that participation in government nutrition programs are an important mediator of the adverse effects of criminal incarceration on current food security and bear attention and additional future research. As previously noted, the external validity of these findings hinges on the comparability of food pantry clients in North Texas relative to the broader pantry-going population.

Conclusion

Food insecurity is among the most significant, nutrition-related public health issues facing the United States. While much prior research has investigated the determinants of food insecurity, there is limited research aimed at assessing the causal impact of criminal incarceration. There are important and timely policy reasons to better understand the linkages, if any, between criminal incarceration and food security. Beginning with the passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), individuals convicted of a drug-related felony are subject to a lifetime ban from SNAP, among others, *unless states decide to opt out*. As of October 2018, only four states continue a full lifetime ban on SNAP benefits (USDA-FNS 2018). Thus, improved understanding of the linkages between incarceration, food security, and safety net access is needed.

To this end, we use original survey data collected among food pantry clients in North Texas to assess the relationship between incarceration and household-level food security status. Our results are quite striking. First, we find a positive but statistically insignificant *association* between *any* criminal incarceration and food insecurity. Moreover, three distinct sensitivity analyses suggest that this positive association is unlikely to represent a causal relationship; the association can be explained by a small amount of selection on unobserved confounders. However, we do find robust evidence of a *causal* effect of *any* criminal incarceration and SNAP participation. Thus, the lack of an adverse effect of any criminal incarceration on food security may be attributable to access to safety net programs.

Second, we find evidence of a positive, statistically significant *association* between *drug-related* incarceration and food insecurity. Moreover, there is some evidence suggesting the relationship is indeed causal, particularly for U.S. born individuals. Consistent with this positive association with food insecurity, we find no evidence of a positive effect (association or causal) of *drug-related* incarceration on SNAP participation.

Finally, when assessing *non-drug-related* incarceration, we find similar results to those obtained when analyzing *any* incarceration. Specifically, we

[11] *To further address the potential for selection on unobserved factors to affect our estimates, we also estimate the ATT using propensity score matching and perform various sensitivity checks using* only *those who have been incarcerated. Now, we define the treatment group as those incarcerated for drug-related offenses and the control group as those incarcerated for non-drug-related reasons. A few results stand out. For the full sample, we find a positive and statistically significant association between drug-related incarceration, relative to non-drug-related incarceration, and the likelihood of being very low food secure (0.327; $p = 0.009$). For the U.S. born subsample, we find a positive and statistically significant association between drug-related incarceration, relative to non-drug-related incarceration, and the likelihood of being very low food secure (0.395; $p = 0.002$) and a negative and statistically significant association between drug-related incarceration, relative to non-drug-related incarceration, and SNAP participation (-0.372; $p = 0.004$). These results, with the exception of those related to SNAP participation, are robust to violations of the CIA. All results can be found in supplemental appendix tables A11 and A12.*

find a positive but statistically insignificant *association* with food insecurity that is unlikely to reflect a causal relationship. However, we do find some evidence of a *causal* effect on SNAP participation.

This pattern of results—no relationship between incarceration and food insecurity in cases where incarceration has a positive relationship with SNAP participation, but a positive relationship between incarceration and food insecurity when incarceration has no relationship with SNAP participation—suggests that SNAP is a potentially important mediator of any lasting impact of criminal incarceration. While further research is needed to better ascertain whether the relationships uncovered here should be interpreted in a causal manner, our analysis lends support to the idea that individuals released from prison should not face statutory bans from nutrition-related federal assistance programs while reintegrating into society.

Supplementary Material

Supplementary material are available at *Applied Economic Perspectives and Policy* online.

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