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Snapping Back: Food Stamp Bans and Criminal Recidivism†

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I estimate the effect of access to food stamps on criminal recidivism. In 1996, a federal welfare reform imposed a lifetime ban from food stamps on convicted drug felons. Florida modified this ban, restricting it to drug traffickers who commit their offense on or after August 23, 1996. I exploit this sharp cutoff in a regression discontinuity design and find that the ban increases recidivism among drug traffickers. The increase is driven by financially motivated crimes, suggesting that the cut in benefits causes ex-convicts to return to crime to make up for the lost transfer income. (*JEL* H75, I38, K42)

Since the late 1990s, state and federal prisons in America have released over half a million prisoners every year (Council of Economic Advisors (CEA) 2016). Upon release, these offenders face a myriad of obstacles that inhibit a successful transition into a new life as law-abiding citizens.[1] To start, offenders have trouble finding work—survey evidence suggests over half are unemployed even a year after release (Schmitt and Warner 2010). Job searchers with a felony conviction are subject to extra scrutiny in the hiring process. Recent audit studies suggest that a felony conviction cuts probability of being called back by an interviewer in half (Pager, Western, and Sugie 2009). In addition, some occupational licensing rules bar felons from ever entering an occupation (Bushway and Sweeten 2007). Furthermore, offenders do not meet the requirements of the Unemployment Insurance program upon release and are frequently denied public housing by local Public Housing Authorities (CEA 2016). Finally, as a consequence of the 1996 welfare reform, many offenders are now banned from receiving Supplemental Nutrition Assistance Program (SNAP, formerly named food stamps) and Temporary Assistance for Needy Families (TANF) benefits. With this in mind, it may not come as a surprise that half

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[1] I use the terms "offender," "ex-offender," "former offender," "prisoner," "inmate," "felon," "releasee," etc., frequently throughout this paper. These terms describe different groups. However, convicted and released drug traffickers (whom I also frequently refer to as simply "drug traffickers"), the focal group of this paper, belong to all of those groups or belonged to them at one point.

of releasees are back in prison within five years of their release and three-quarters are rearrested within five years (CEA 2016). Recidivism in America may be at least partly the consequence of these barriers to reentry.

In this paper, I focus on one of those barriers, the SNAP ban, and ask how it affects recidivism outcomes, defining recidivism as a return to prison after release. It is particularly critical that we understand the effect of the SNAP ban because it is currently in effect in 27 states, and because survey evidence suggests SNAP is an important resource for offenders post-release (Wolkomir 2018). Approximately 70 percent of the former inmates in the Boston Reentry Study report receiving SNAP benefits even just two months after release (Western et al. 2015).[2] Even more, SNAP benefits are an important component of income for recipients. Based on a representative sample of adult male recipients (not limited to offenders), SNAP benefits make up approximately 20 percent of their reported gross income (see Table 2). Finally, to the extent that SNAP availability has insurance value, it may also affect the decisions of non-recipients.

To study the effect of the SNAP ban on recidivism, I use a federal policy change (as it was implemented in Florida) that imposed a lifetime ban from SNAP receipt on offenders who committed drug trafficking on or after August 23, 1996.[3] I will often refer to this as "the cutoff date" in the remainder of the paper. Offenders committing drug trafficking on or after this date are also subject to a lifetime ban from TANF benefits. That said, over 85 percent of drug traffickers are male and less than 10 percent of TANF recipients are male—if TANF does play a role, it is likely to be small in comparison to SNAP, for which almost 40 percent of recipients are males aged 18–65 (Office of Family Assistance 2015). For this reason, I refer to the treatment only as "access to SNAP" or "the SNAP ban" in the remainder of the paper. To estimate the causal effect of the ban on recidivism, I employ a regression discontinuity design that compares outcomes for offenders who committed drug trafficking in a small window before the cutoff date to outcomes for offenders who committed it on or slightly after the cutoff. I find the SNAP ban has increased the probability of recidivism among drug traffickers.

Specifically, I find that drug traffickers subject to the ban are about 9 percentage points more likely to return to prison after release than drug traffickers who have access to SNAP. An increase of this size is large for drug traffickers in Florida. Among those offenders who commit their trafficking offense in the 240 days before the cutoff date, about 16 percent return to prison at some point post-release. This implies that the SNAP ban increased recidivism among drug traffickers by about

[2] Similar estimates of SNAP usage among households with an interaction with the criminal justice system can be found in the Fragile Families and Child Wellbeing Study (Sugie 2012) and in the Panel Study of Income Dynamics (1995–2013).

[3] I focus on Florida in this paper for a number of reasons, the foremost being that inmate-level data for all offenders released after October 1, 1997 is publicly available for download. Florida also has more people in prison or jail than all states but two (California and Texas) and has more people participating in SNAP than all states but two (again, California and Texas) (Kaeble and Cowhig 2018, Food and Nutrition Service (FNS) 2017). Finally, the discontinuity is well functioning in Florida—I find no evidence of sorting, manipulation, or endogenous responses near the cutoff. I explored a similar policy discontinuity in North Carolina, but found evidence of sorting near the cutoff—offenders on the other side of the cutoff were older, more risky, and received higher sentences. In addition, a McCrary density test suggested a drop in crime right after August 23, 1996 in North Carolina. This invalidates the current approach in the context of North Carolina, and hence I focus on Florida.

60 percent. However, this estimate is based on the small sample of about 1,000 drug traffickers committing an offense sufficiently close to the cutoff date. Although I am able to reject a null effect of the ban, the estimate is noisy and the confidence interval is large. The 90 percent confidence interval on the main estimate is 1.7 percentage points to 17 percentage points, which implies the SNAP ban increased recidivism among drug traffickers by about 10 percent to 105 percent. Unfortunately, I do not have the statistical power to produce a more precise range of possible effect sizes.

Furthermore, the increase in recidivism is primarily driven by an increase in recidivism for financially motivated crimes (such as property crime and selling drugs). This result has important implications for state SNAP bans and for reentry policy in general. In fact, it is consistent with recent work by Munyo and Rossi (2015) showing that a disproportionate amount of recidivism happens on the first day of release and that first-day recidivism can be almost completely stifled by giving releasees a sufficient monetary stipend. Their work suggests that financial support can ease reentry. I provide further support for this idea by showing that recidivism increases after we decrease financial support to offenders by banning them from SNAP.

More broadly, this paper contributes to a literature in public economics that studies labor supply responses to transfer programs. Economic theory predicts that denying offenders SNAP benefits will incentivize work, encouraging offenders to reenter the labor force and earn the money necessary to put food on the table. For a number of reasons, however, finding employment in the legal sector is a challenge for ex-convicts. As such, the work incentives could drive offenders back into the illegal sector. The evidence in this paper is consistent with a model in which removing SNAP benefits does increase the labor supply of drug traffickers.

This relates to work by Hoynes and Schanzenbach (2012) that finds reductions in employment and hours worked for female-headed households after food stamps is introduced in a county. In this paper, I emphasize the importance of considering the illegal labor margin when designing policies that will affect work incentives, especially when those policies will be applied to people who have high attachment to the illegal labor market or high difficulty entering the legal labor market, both of which are true in the case of drug traffickers.

Finally, a number of papers have documented a long list of benefits from SNAP and safety net programs in general. First and foremost, SNAP relieves families of food insecurity and reduces poverty (Mabli and Ohls 2015, Short 2015). In addition, recent research suggests that SNAP receipt leads to a wide range of other positive outcomes, including improved adult health, improved child health in the long run, better birth outcomes, and higher test scores for primary school students (Almond, Hoynes, and Schanzenbach 2011; Gassman-Pines and Bellows 2015; Gregory and Deb 2015; and Hoynes, Schanzenbach, and Almond 2016). I add another policy-relevant benefit to that list—access to SNAP decreases recidivism among drug traffickers.

Making a few crude but conservative assumptions about the cost of incarcerating an extra person and the social cost of crime, I can use the estimated effect of the ban on recidivism to calculate the societal cost of the SNAP ban in Florida. A more comprehensive cost-benefit analysis of the ban is beyond the scope of this paper, as it would require estimates of the effect on legal employment and the deterrence effect of the ban for would-be first-time traffickers. Rather, this cost estimate is intended to highlight the potential benefit of reducing recidivism by providing SNAP or other financial support post-release. I estimate the ban costs Florida about $3,700 per banned person. Given that Florida has approximately 19,000 people currently subject to the ban, this implies that the ban has cost the state over $70 million to date, a number that grows with each drug trafficker shut out from SNAP.

The remainder of the paper is organized as follows. Section I recounts a short history of the SNAP ban, and Section II reviews the related literature. I describe the data in Section III. Section IV presents the methodology, and Section V discusses the corresponding results. Section VI concludes.

I. The Federal SNAP Ban

The passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996 dramatically changed welfare programs in America. Along with other major changes to welfare policy, PRWORA imposed a lifetime ban from SNAP on felony drug offenders. The ban was introduced as an amendment to the act by Senator Phil Gramm and passed through Congress with little opposition. Upon introducing the amendment, Senator Gramm argued, "if we are serious about our drug laws, we ought not to give people welfare benefits who are violating the nation's drug laws." Based on remarks by Senator Connie Mack, it also appears that some believed that drug dealers should not receive benefits since, were their informal earnings counted, they would likely be ineligible (US Congress 1996, S8498).

Since the passage of PRWORA, many states have modified or repealed the SNAP ban. Currently, 46 states have opted out or modified the SNAP ban, up from only half of all states in 2002 (Gilna 2016, Wolkomir 2018). While some states have opted out entirely, many states have modified the ban to grant eligibility to people convicted of substance abuse crimes or to require enrollment in substance abuse treatment classes to become eligible (Wolkomir 2018). Florida quickly modified the ban such that it would only apply to people convicted of drug trafficking crimes committed on or after August 23, 1996.[4]

In Florida, drug trafficking constitutes the selling, manufacturing, or distributing of illegal drugs in large amounts. For example, a person is charged with "trafficking heroin" if they sell, manufacture, or distribute greater than 4 grams of heroin (FL Statute 893.135). Importantly, "selling, manufacturing, or distributing" (henceforth referred to as SMD) is a separate offense category that applies to people who sell,

[4] The application for SNAP in Florida has a section that requires applicants to report whether or not they have been convicted of a drug trafficking offense that was committed on or after August 23, 1996. While the Florida Department of Children and Families does not have an automated system to cross-check applications with the Florida Department of Corrections (FL DOC), offender information is easily searchable online. The Office of Public Benefits Integrity in Florida has also partnered with the FL DOC in the past to identify drug traffickers who were currently receiving or had received SNAP benefits. Florida estimates approximately $360,000 worth of SNAP and cash assistance benefits had been disbursed to ineligible individuals. Assuming those benefits were strictly SNAP benefits, that the average recipient stayed on SNAP for one year, and that the average benefit per month is $150, this implies only 200 drug traffickers were receiving benefits for which they were ineligible. Florida is home to approximately 19,000 drug traffickers who are subject to this ban, implying that only 1 percent of drug traffickers subverted the ban.

manufacture, or distribute illegal drugs in smaller amounts. People convicted of SMD or felony possession are eligible for SNAP benefits in Florida, regardless of when the offense was committed. I use these groups in placebo tests to emphasize that the increase in recidivism is specific to drug traffickers, the offenders who are banned from SNAP if they commit the offense after the cutoff date.

II. Related Literature

In this paper, I build on three literatures in economics and criminology by studying the effect of the SNAP ban on drug traffickers in Florida. To my knowledge, I provide one of the first empirical evaluations of a policy that currently affects former drug offenders in 27 states. This policy evaluation contributes broadly to the literature on prisoner reentry, specifically that which explores the effects of financial support for released offenders. Second, I contribute new evidence highlighting the relationship between financial need and criminal behavior. Finally, I add to an extensive literature in public economics that studies the effect of cash and in-kind transfers on labor supply.

For ex-offenders, finding legal work can be especially difficult. A large literature discusses the challenges that offenders face when looking for legal work, from occupational licensing restrictions to employer discrimination to the detrimental effects of incarceration itself. I provide a broad review of this literature and other work on prisoner reentry in online Appendix B. The immense difficulty of successfully reintegrating into life outside of prison has spurred an interest in programs that can ease the transition and prevent offenders from returning to crime. In this paper, I examine one reentry strategy: providing financial support to offenders via SNAP. This builds on a growing literature on the effect of giving offenders financial support upon release.

In concurrent work, Yang (2017b) and Luallen, Edgerton, and Rabideau (2018) study the effect of the SNAP and TANF bans on criminal recidivism. Both papers contribute further evidence to this important policy question. Luallen, Edgerton, and Rabideau (2018) uses data from the National Corrections Reporting Program (NCRP), which includes information about prison admissions and releases for several states. The authors also use the discontinuity in banned status at the cutoff date in addition to variation in state-level modifications of the SNAP ban. They find no effect on recidivism.

I depart from the analysis in Luallen, Edgerton, and Rabideau (2018) in two major ways. First, I focus on longer-run recidivism outcomes, while they study the effect on recidivism within three years. In this paper, I also find a small and statistically insignificant positive effect on recidivism within three years. Second, I use administrative data from Florida that includes the date each offense was committed. The NCRP data does not include the date the offense was committed, and thus, the authors must use conviction date (proxied by prison admission date) to identify treatment. Since the ban is actually determined by the date the offense was committed, the authors have a very noisy measure for treatment (convictions often take place months or years after the date the offense was committed). This measurement error will attenuate their results. In fact, I reestimate the main results from this paper using conviction date rather than offense date and also find a statistically insignificant effect on recidivism (results in Table A28 of online Appendix A).

Yang (2017b) exploits the extent to which states opt out of the federal ban and the differential timing of opting out. Yang uses state-by-time-by-crime variation in the application of the ban in a triple difference design. Using data from the NCRP, she finds that access to SNAP benefits decreases the probability of returning to prison within one year by about 2.2 percentage points or 13 percent from the mean. This result is consistent with my findings that access to SNAP decreases the probability of re-incarceration for drug traffickers.

My paper presents a more comprehensive analysis of the SNAP ban by examining long-run recidivism outcomes and the types of crimes offenders commit due to the ban. In addition, I focus on drug traffickers, a group of offenders who have ties to the illegal labor market and thus, may be most at risk to return to it. Also, several states that have partially opted out of the ban have, like Florida, maintained the ban for drug traffickers. Finally, the estimates from the triple difference design are biased if states enact policies that specifically affect drug felons in the same year that they opt out of the welfare ban. I approach the evaluation of this ban with a regression discontinuity design that is not subject to that concern.

There is an older literature in criminology and sociology that analyzes random experiments that allocate unemployment benefits to offenders and consistently finds that financial support decreases probability of rearrest for property crimes (Mallar and Thornton 1978; Berk, Lenihan, and Rossi 1980). Specifically, these studies find that financial aid for ex-offenders reduces their likelihood of rearrest for property crime by about 8–27 percent.[5] The effect of these programs on rearrest in general is less clear, but the largest effects are concentrated in rearrest for property crimes, which is both consistent with theory and with the results in this paper. Interestingly, Berk and Rauma (1983), in an early application of regression discontinuity design, also finds that giving unemployment benefits to offenders decreases the likelihood of recidivism (defined as re-incarceration, parole revocation, or parole violation) by about 13 percent. As Raphael (2011) points out, the cash assistance programs studied in the 70s and 80s typically had benefit reduction rates from formal earnings of 100 percent, and as a result, led to a substantial drop in formal labor supply that may have had an offsetting effect on recidivism.

Another compelling line of research documents an increase in crime two to three weeks after welfare disbursement, suggesting recipients are spending down the entire check and committing crimes until the next payment (Foley 2010). Similarly, Carr and Packham (2017) demonstrates that theft in grocery stores in Chicago fell dramatically after Illinois implemented a staggered disbursement schedule for SNAP. They leverage variation in benefit issuance based on first-letter of the recipient's last name and estimate similar effects from a shift in issuance dates in Indiana. This work further highlights the relationship between transfer programs and crime.[6]

[5] Berk, Lenihan, and Rossi do not find an effect of financial aid in their reduced form analysis of the experiment. They introduce a model that incorporates legal employment effects and report the results of that model.

[6] Studies of the effect of housing vouchers on crime tend to find a negligible or negative effect of voucher receipt on crime (Jacob, Kapustin, and Ludwig 2015; Carr and Koppa 2017). Carr and Koppa (2017) argues that vouchers

A more detailed review of the literature on financial need and criminal behavior is in online Appendix B. The results in this paper, that the SNAP ban increases recidivism among released drug traffickers, provide further evidence that financial need is an important factor in the decision to commit crime.

The work cited above ties into a distinct literature in public economics about the effect of transfer programs on labor supply. Both theory and empirical evidence suggests that transfer programs discourage work. For SNAP, in particular, Hoynes and Schanzenbach (2012) uses variation in county-level rollout of the food stamps program and find that the introduction of food stamps in a county decreases annual hours worked in those households most likely to be affected by the program (non-elderly, female-headed households).[7] Their paper provides valuable evidence about the labor supply response of female-headed households to food stamps, but evidence for the labor supply of males is necessarily limited, and there is no consideration of illegal labor supply. While I do not observe hours worked or wages, I do observe recidivism, which for many drug traffickers corresponds to participation in the illegal labor market.

In summary, public economic theory as well as empirical evidence suggests that decreasing transfer income may push workers back into the labor force. Yet other work highlights the difficulty offenders face in the legal labor market and the ease with which they can reenter the illegal labor market (see online Appendix B). A strong incentive to return to work coupled with the difficulty of finding legal work may drive offenders back to the illegal sector (see online Appendix C for a formal model of this phenomena). Existing research on the effect of financial support on recidivism typically focuses on short-run outcomes or considers financial support programs that differ markedly from SNAP in terms of benefit amount, potential length of receipt, and benefit reduction rate. The effect of the SNAP ban on recidivism speaks to labor supply responses to SNAP benefits, and even more, it directly relates to current prisoner reentry policy.

III. Data and Descriptive Statistics

A. *Offender Data*

Florida Department of Corrections (FL DOC 2017) makes data from its Offender Based Information System (OBIS) publicly available. These data include information about both active offenders and released offenders. I combine offense-level data, prison stay-level incarceration histories, and offender-level demographic data into a dataset where each observation is a unique prison stay. Using this data, I calculate recidivism for a given stay j as whether or not the offender ever has a prison stay occurring after stay j. Likewise, recidivism is recorded as "financially

free up financial resources to such an extent that they effectively subsidize spending on things that are complements to crime, like alcohol.

[7] For another example, Jacob and Ludwig (2012) exploits variation in housing voucher receipt from randomized placement on a waitlist in Chicago and find that voucher use decreases labor force participation by 6 percent. Also, Deshpande (2016) uses a policy discontinuity from PRWORA to demonstrate that children removed from SSI increase their labor supply but not by enough to offset the lost benefits.

motivated" if the offender was charged with a financially motivated crime for the prison stay occurring after stay j, and recidivism is recorded as "non-financially motivated" if the offender was not charged with a financially motivated crime for the prison stay after stay j.[8] In some analyses, I use a measure of time until recidivism—this is defined as the time between release from prison stay j and the earliest offense occurring after stay j.

I limit this data to offenses committed after October 1, 1995. First, Florida implemented a suite of criminal justice reforms that apply to offenders committing offenses on or after October 1, 1995. Most notably, offenders sentenced after October 1, 1995 are required to serve 85 percent or more of their sentence. Kuziemko (2013) shows that fixed-sentencing systems alter incentives for offenders while in prison, stifle the allocative efficiency of parole boards, and ultimately, increase recidivism. Restricting the sample to offenses committed after October 1, 1995 avoids including offenders that were sentenced under a drastically different system. Second, offenders are included in the publicly available OBIS data if they committed a felony, served time in a Florida prison for that felony, and were released after October 1, 1997. If an offender meets those three criteria, then all of their stays in Florida prisons are included in the data. Limiting the sample mitigates sample selection problems arising from that restriction imposed by FL DOC.[9] Further details on data construction are in online Appendix E.

For the main results, I also remove individuals who are identified as Hispanic in the data (less than 7 percent of my sample). PRWORA restricted access to SNAP for documented and undocumented immigrants regardless of criminal history. In addition, noncitizen immigrants often face deportation after committing drug trafficking since it is classified as an "aggravated felony" under the Immigration and Nationality Act. For these reasons, many noncitizen immigrants will lose access to SNAP regardless of the date their offense is committed, thus including them in the sample will attenuate the estimated effect. Unfortunately, I do not observe immigrant status in the data. In the 2000 census, about 41 percent of Hispanic individuals "institutionalized" in Florida are born outside of the United States and less than 5 percent of black or white individuals institutionalized in Florida report a birthplace outside the United States (Ruggles et al. 2015). I report the main results on recidivism with Hispanics included in online Appendix Table A4 to demonstrate that the results are qualitatively similar, but as expected, are attenuated slightly.

Summary statistics for offenders who committed offenses from October 1, 1995 to October 1, 1997 are reported in Table 1 for three groups: drug traffickers, all

[8] FL DOC categorizes most offenses here: http://www.dc.state.fl.us/AppCommon/offctgy.asp#PC. I define financially motivated crimes as: property crimes (excluding property damage crimes such as vandalism), selling/manufacturing/distributing drugs, drug trafficking, fraud, forgery, racketeering, prostitution, counterfeiting, and crimes containing a "$," "sale," or "sell" in the charge description. I define non-financially motivated crimes as all crimes that are not categorized as financially motivated.

[9] Only six drug trafficking offenders in the data from October 1, 1995 to October 1,1997 are released prior to October 1, 1997. The results are not affected by the inclusion of these six offenders. Also, on average, drug traffickers are sentenced to approximately 4.6 years, and over 90 percent of drug traffickers are sentenced to 2 years or more. Finally, selection bias from the FL DOC restriction will bias all results downward since offenders in the control group (those committing an offense prior to August 23, 1996) are more likely to be released prior to October 1, 1997 and thus only observed in the event of recidivism.

Table 1—Summary Statistics for Drug Traffickers and Other Offenders in Florida

	October 1, 1995–October 1, 1997			Full sample
	All nondrug offenders	SMD offenders	Drug trafficking offenders	Drug trafficking offenders
Any recidivism	0.399	0.564	0.178	0.112
	(0.490)	(0.496)	(0.382)	(0.224)
Financial recidivism	0.246	0.364	0.113	0.087
	(0.431)	(0.481)	(0.317)	(0.195)
Nonfinancial recidivism	0.153	0.200	0.065	0.024
	(0.360)	(0.400)	(0.246)	(0.103)
Days until recidivism	1,330.189	1,204.634	1,615.329	1,075.090
	(1,237.552)	(1,187.955)	(1,269.476)	(899.813)
Black	0.455	0.850	0.486	0.377
	(0.498)	(0.357)	(0.500)	(0.485)
Age at intake	30.952	31.031	33.181	33.910
	(10.114)	(9.155)	(10.226)	(10.164)
Time sentenced (in years)	4.438	3.006	5.163	4.116
	(4.040)	(2.649)	(3.563)	(5.159)
Observations	22,893	6,002	1,435	18,656

Notes: The first four rows present recidivism statistics: the fraction of offenders in each group who recidivate, recidivate with a financially motivated crime, recidivate with a non-financially motivated crime, and finally, the days until an offender recidivates (conditional on recidivating). Financially motivated crimes are: property crimes (excluding property damage crimes such as vandalism), selling/manufacturing/distributing drugs, drug trafficking, fraud, forgery, racketeering, prostitution, counterfeiting, and crimes containing a "$", "sale", or "sell" in the charge description. Non-financially motivated crimes are defined as all crimes that are not categorized as financially motivated. Financially motivated recidivism is thus defined as recidivism that involves a financially motivated crime, whereas non-financially motivated recidivism is defined as recidivism that does not involve any financially motivated crime. The last three rows show the fraction of offenders who are black, the average age at intake, and the average sentence handed down by the court. SMD offenders are those offenders convicted of selling/manufacturing/distributing drugs. SMD is a separate offense from drug trafficking and those offenders were not ultimately included in the SNAP ban in Florida. An offender is tagged as a drug trafficking offender if they are convicted of a drug trafficking offense. An offender is tagged as a nondrug offender if they are not convicted of a drug crime. An offender is tagged as an SMD offender if they are convicted of SMD, but are not convicted of a drug trafficking offense. In addition, when calculating the summary statistics for all drug trafficking offenders, I collapse to the offender ID level since some offenders will have more than one stay for drug trafficking in this time period.

nondrug offenders, and offenders convicted of selling, manufacturing, or distributing drugs (SMD offenders). I also report summary statistics for all drug traffickers released after October 1, 1997. Drug traffickers are quite different from offenders who commit other crimes. As Table 1 shows, recidivism is lower for drug traffickers than nondrug offenders or SMD offenders in Florida. When benchmarking the recidivism results I find in Section V, it is important to keep in mind the rates at which other criminals return to prison. A 9 percentage point increase in the recidivism rate of drug traffickers does not yield an unrealistic recidivism rate, rather, it yields a rate of recidivism that is still lower than the rates for nondrug offenders and other drug offenders.

B. *SNAP Quality Control Data*

Using the 1996–2014 SNAP Quality Control files provided by Mathematica Policy Research, I report summary statistics on the SNAP population in Florida

Table 2—Summary Statistics on Male SNAP Population in Florida

	No nationwide ABAWD waiver		Nationwide ABAWD waiver	
	Single male	Male with family	Single male	Male with family
Fraction black	0.310	0.168	0.292	0.141
	(0.463)	(0.374)	(0.455)	(0.348)
Age	45.280	39.932	43.077	40.994
	(11.502)	(11.969)	(12.693)	(11.644)
Fraction unemployed	0.916	0.602	0.914	0.617
	(0.278)	(0.490)	(0.281)	(0.486)
SNAP benefit (in 2010 dollars)	85.50	206.28	150.41	324.48
	(47.97)	(138.93)	(69.02)	(222.99)
Observations	1,587	1,656	1,962	1,188
Benefit, percent of income	15.703	25.818	18.124	29.326
	(16.700)	(21.135)	(16.720)	(22.723)
Observations	1,027	1,347	924	968

Notes: Summary statistics above are derived from the Mathematica Policy Research SNAP QC files from 1996–2014, which provide data on a sample of the SNAP population in each state. Mathematica Policy Research constructs the SNAP QC files to be representative at the state level. I limit the sample to males aged 18–65 and listed as the primary or secondary recipient of the SNAP benefits. In calculating the benefit as a percentage of gross income, I remove zeroes in gross income and benefit-income ratios above one. In columns 1 and 2, I provide statistics for all years from 1996–2014 without nationwide ABAWD work requirement waivers (1996–2000, 2004–2008). In columns 3 and 4, I provide statistics for all years from 1996–2014 with nationwide ABAWD work requirement waivers (2001–2003, 2009–2014). The ABAWD work requirement states that able-bodied adults without dependents are limited to only 3 months of SNAP receipt every 3 years unless they: work 20 or more hours per week, participate in an employment and training program, or participate in a workfare program (USDA 2016b).

in Table 2. I focus on male recipients aged 18–65 for this exercise since 89 percent of offenders are male. These statistics paint a picture of the male SNAP population in Florida and contain two key observations: the SNAP benefit is an important source of income, and recipients do not have to be employed to receive SNAP benefits, despite the well-known work requirements of post-PRWORA SNAP.

Notably, the SNAP benefit men receive in Florida is around 20 percent of the total gross income they report. SNAP transfers are a sizable portion of gross income for this population. This statistic gives us a rough estimate of the toll of the SNAP ban on offenders. Assuming SNAP transfers would make up the same share of drug traffickers' reported gross income, then the SNAP ban effectively denies offenders this stream of income upon release. In other words, offenders who commit drug trafficking on or after August 23, 1996 are banned from SNAP and thus take home 20 percent less in gross income than offenders who commit drug trafficking just before August 23, 1996. Again, this is an estimate based on the SNAP benefits of male recipients aged 18–65 in Florida. SNAP transfers may represent more or less than 20 percent of former drug traffickers' gross income. In this light, it makes sense that there are potentially large effects of the SNAP ban on recidivism, especially since SNAP take-up among former offenders is high.

Approximately 70 percent of the former inmates in the Boston Reentry Study report receiving SNAP benefits even just two months after release (Western et al. 2015). Sugie (2012) also finds that about 70 percent of families in the Fragile Families and

Child Wellbeing Study with a recent paternal incarceration report receiving SNAP in the past year. The Panel Study of Income Dynamics (1995–2013) asks respondents in 1995 if they have ever been in the corrections system (jail, prison, youth corrections). Almost 50 percent of respondents who answered yes to that question were in families that reported receiving SNAP at some point from 1995–2013. Unfortunately, I cannot identify the subsample of these people who have been to prison (given prison, jail, and youth corrections are three very different populations).

PRWORA also introduced more stringent work requirements for SNAP recipients. Perhaps the requirement most relevant to this study is the work requirement for able-bodied adults without dependents (ABAWDs) since many offenders may be considered ABAWDs. The ABAWD work requirement states that able-bodied adults without dependents are limited to only three months of SNAP receipt every three years unless they: work 20 or more hours per week, participate in an employment and training program, or participate in a workfare program (US Department of Agriculture (USDA) 2016b).

First, note that ABAWDs do not have to be employed to meet the requirement; they can meet the requirement by enrolling in employment and training programs, many of which are actually targeted at ex-offenders (USDA 2016a). In fact, Table 2 shows that only 10 percent of single males receiving SNAP are employed and only 40 percent of men with families are employed. Second, when states face tough economic times, they can request to waive this requirement. This requirement was waived nationally from 2001–2003 and 2009–2016. In addition, the requirement was waived prior to 2009 for Labor Surplus Areas (counties in Florida with especially high unemployment) and for counties where Florida chose to apply a special exemption that allows states to exempt 15 percent of the state's caseload from the work requirement (USDA 2016b).

I exploit this variation in the ABAWD requirement and find that the SNAP ban does have the largest effect on recidivism when the ABAWD requirement is waived in Florida. Table 2 shows statistics broken down by years with and without nationwide ABAWD work requirement waivers. SNAP benefits are higher in years with nationwide ABAWD waivers, and single males represent a greater portion of the male SNAP population in Florida during those years.

IV. Methodology

SNAP eligibility for drug traffickers is determined by a sharp cutoff date. Offenders who committed drug trafficking before August 23, 1996 are eligible for SNAP benefits, while offenders who committed drug trafficking on or after August 23, 1996 are permanently banned from SNAP. To estimate the effect of the SNAP ban on recidivism, I employ a regression discontinuity design that exploits this sharp policy rule. In general, the regression model is as follows:

$$(1) \qquad Recidivism_{it} = \alpha + \beta_1 After_{it} + g(DaysFromCutoff_{it}) + g(DaysFromCutoff_{it}) \times After_{it} + \omega_{it},$$

where $Recidivism_{it}$ is equal to one if the offender i at time t ever returns to prison after being released and equal to zero if the offender does not return to prison.[10] The variable $After_{it}$ is an indicator equal to one when the offense is committed on or after August 23, 1996 and equal to zero otherwise—this indicates whether the offender is subject to the SNAP ban or not; $g(DaysFromCutoff_{it})$ is a flexible function of offender i's offense date expressed as number of days from August 23, 1996 (centered at zero). The interaction term allows the relationship between the running variable (distance from August 23, 1996) and recidivism to vary before versus after the cutoff. No baseline covariates are included in this specification.[11]

My preferred specification for all results is the local linear regression discontinuity design with a rectangular kernel. I present the main results in this paper using two bandwidths. First, I show every result using the Imbens and Kalyanaraman (IK) (2012) optimal bandwidth chosen for that regression with polynomial of degree one and a rectangular kernel. This procedure yields different bandwidths for every dependent variable. For example, when examining the effect of the ban on any recidivism, the optimal bandwidth is ± 212 days from August 23, 1996, whereas the optimal bandwidth is ± 242 days when examining the effect of the ban on financially motivated recidivism. In addition, since I limit the data to offenses occurring after October 1, 1995, any bandwidth greater than ± 327 days will be asymmetric. For these reasons, I also include results based on a consistent bandwidth of ± 240 days.[12]

The choice to focus on the local linear design is motivated by Gelman and Imbens (2018) who suggest using lower-order polynomials. However, in a working paper, Card et. al (2014) argues that the optimal polynomial is dependent on the underlying data generating process, and in some cases, higher-order polynomials are indeed optimal. In addition, while I focus on the IK optimal bandwidth in this paper, other researchers have designed alternative algorithms for choosing a bandwidth (Ludwig and Miller 2007; Calonico, Cattaneo, and Titiunik 2014). I show that the main results are robust to higher-order polynomials, alternative kernels, and many alternative bandwidths.

The main identifying assumption with the regression discontinuity design is that all unobserved determinants of recidivism are continuous with respect to the offense date (Imbens and Lemieux 2008). This assumption, although inherently untestable, does yield testable implications. First, the observable characteristics of offenders should be continuous across the threshold. Second, the density of drug trafficking

[10] Throughout the paper, I introduce a variety of "recidivism" measures. For example, I also estimate equation (1) on "financially motivated recidivism" and "non-financially motivated recidivism." Financially-motivated recidivism is equal to one if the offender returns to prison with any crime that is financially motivated and is equal to zero if the offender returns to prison only with crimes that are not financially motivated or if the offender does not return to prison. Non-financially motivated recidivism is equal to one if the offender returns to prison only with crimes that are not financially motivated and is equal to zero if the offender returns to prison with any crime that is financially motivated or if the offender does not return to prison.

[11] If covariates are orthogonal to the treatment and explain recidivism, including them should tighten my standard errors without changing the magnitude of my coefficients. I introduce controls for offender characteristics and offense day-of-week fixed effects in online Appendix Table A5 and find that the results are similar but more precise.

[12] The bandwidth is convenient because it corresponds to an even number of months (8 months before and after the cutoff) and is the average of the three IK optimal bandwidths for any recidivism, financially motivated recidivism, and non-financially motivated recidivism rounded to the nearest ten.

offenses should also be continuous across the threshold. I test for discontinuous breaks in observed characteristics at the cutoff by estimating the following:

$$(2) \qquad Characteristic_{it}^{D} = \alpha + \beta_1 After_{it} + g(DaysFromCutoff_{it}) + g(DaysFromCutoff_{it}) \times After_{it} + \omega_{it},$$

where $Characteristic_{it}^{D}$ is an indicator for whether or not the offender i on day t is black, male, their age at intake, their total sentence length, the type of drug they are charged with trafficking, the number of prior offenses for which they have been convicted, and the number of concurrent offenses for which they were convicted. In addition, I test for a break in risk of recidivism. I calculate risk of recidivism using a logistic regression of recidivism on all characteristics and age-squared. I run this regression for those offenders not subject to the ban and not in the ± 212 day IK bandwidth (those committing drug trafficking from October 1, 1995 to January 24, 1996) and predict the "risk score" for offenders in my sample.

Results from the "risk score" test are presented in Figure 1, while online Appendix Table A2 and Figures A1a–A1h show the results for each characteristic separately. If the identifying assumption is violated, we would expect to see a significant difference in observable characteristics after August 23, 1996 ($\beta_1 \neq 0$). I find no evidence of sorting around the cutoff on observable characteristics. I also run a regression of the dummy variable indicating the offense was committed after the cutoff on total years sentenced, race, age, number of concurrent offenses, type of trafficking, sex, and number of prior offenses. A joint significance test on the covariates in this regression further suggests no sorting occurred near the cutoff (p-value $= 0.9504$). These results lend credence to the assumption that offenders, judges, police, and lawyers are not changing their behavior in response to the policy.[13]

In addition, I conduct a McCrary density test for excess mass in the number of drug trafficking crimes on either side of the discontinuity (McCrary 2008). A spike in the number of drug trafficking offenses after August 23, 1996 could suggest judges, police, or lawyers are manipulating the offense date or offense classification to subject more offenders to the SNAP ban. However, a significant drop in the number of drug trafficking offenses after August 23, 1996 could suggest offenders are decreasing drug trafficking activity once the policy goes into effect or that judges, police, or lawyers are manipulating offense date or offense classification to help offenders avoid the ban. In either case, this type of behavior would confound a causal estimate of the SNAP ban on recidivism. I do not find evidence that the number of drug trafficking offenses changes after August 23, 1996. These results, in online Appendix Figure A2, provide further evidence that the identifying assumption is satisfied.

[13] The break in probability an offender is black before versus after the cutoff is not significant, but it is large in the specification with the ± 240 day bandwidth. Including a control for race in the main regression yields similar results in size and significance. Without controlling for race, the coefficient is 0.095. When I control for race, the coefficient is 0.103. In addition, I am testing several different characteristics with several different bandwidths. Importantly, when I combine these characteristics into a composite risk score, I find no break at the cutoff, and when I do a joint significance test of all characteristics, I find no evidence of a change in the characteristics of offenders at the cutoff.

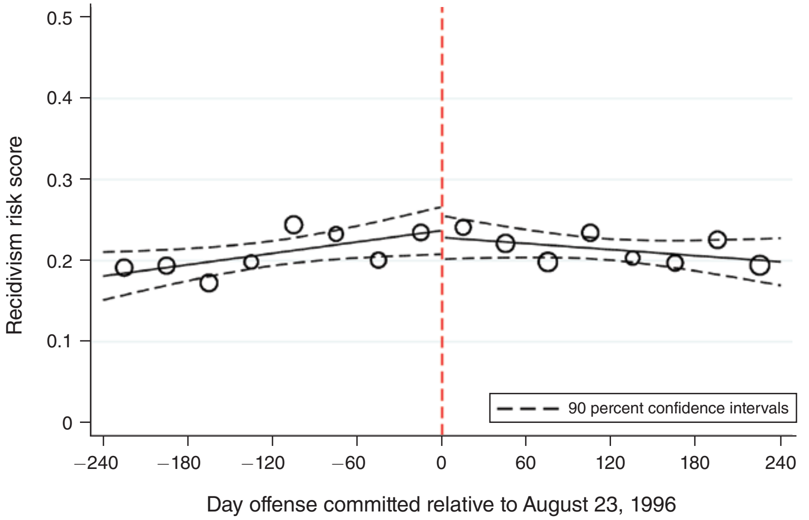

Figure 1. Smoothness Through Cutoff in Offender's Risk of Recidivism

Notes: Recidivism risk score is calculated by estimating the relationship between offender characteristics and recidivism using a sample of pre-ban drug traffickers who are not included in the Imbens-Kalyanaraman (IK) optimal bandwidth, and applying those estimates to drug traffickers in the sample. The characteristics used to create this measure of offender risk are: age, age-squared, total years sentenced, total number of prior offenses, total number of concurrent offenses, sex, race, and type of drug trafficked. The figure above (and the following RD plots more generally) displays the lines from two local linear regressions, estimated separately on each side of the cutoff using the offense-level micro data. I also overlay a scatter plot of 30-day bin averages of the dependent variable weighted by the number of offenses in each 30-day bin. The dependent variable in this figure is offender risk score, and the figure shows that offender risk of recidivism (an index of several offender characteristics) is smooth through the cutoff date. Finally, the running variable in this figure (and the following RD plots) is the number of days between the offender's offense date and August 23, 1996 (the cutoff date that determines the offender's ban status). The running variable is centered at zero such that offenders committing an offense before August 23, 1996 have a negative distance from the cutoff date and offenders committing an offense after August 23, 1996 have a positive distance from the cutoff date.

Although the tests reported in Figure 1, Table A2, and Figure A2 suggest no sorting is happening near the cutoff in Florida, it is worth discussing a few context-specific details that may further ease concerns about sorting. When PRWORA was introduced, it did not include the amendment that banned drug offenders from SNAP benefits—this amendment was introduced by Senator Phil Gramm on July 23, 1996, only a month before President Clinton signed the bill into law (US Congress 1996, S8498). This leaves a very short amount of time for information about the ban to disseminate to offenders, judges, police, prosecutors, or anyone else who could feasibly induce sorting. Even more, as the president had vetoed the previous two welfare reform bills, there was at least some uncertainty over whether or not the bill would become law (Haskins 2006). Finally, although PRWORA as a whole was widely covered by news outlets at the time, the ban on drug felons received little to no publicity.[14]

[14] Searches for the phrases "food stamps felon," "food stamps crime," and "welfare felon" in LexisNexis return zero news articles from August 22, 1995 to August 22, 1997. The phrases "food stamps ban" and "food stamps drug" turn up only two articles—one about the PRWORA work requirements and the ban on noncitizens and the

V. Results

A. *Main Results*

I begin by estimating the effect of the SNAP ban on any recidivism using the sharp cutoff date of the ban. Since I do not have access to SNAP administrative records, the effects estimated in this paper should not be interpreted as the average or local average treatment effect of SNAP receipt on recidivism. Rather, the results should be interpreted in one of two ways. First, as an intent-to-treat (ITT) effect, which can then be scaled up by the SNAP take-up rate among former offenders to estimate the local average treatment effect of SNAP receipt. Second, the ban itself may affect recidivism even apart from actual SNAP receipt. If the potential of receiving SNAP has insurance value, the ban may affect decision making even among offenders who would not receive SNAP. In this case, the results should be interpreted as the local average treatment effect of the SNAP ban on recidivism.

The main results are in Table 3 below. In panel A, I show results using the Imbens-Kalyanaraman (IK) optimal bandwidth and in panel B, I show results using a bandwidth of ± 240 days. I will discuss results in terms of panel B to make comparisons across analyses easy. Column 1 of panel B shows the effect of the SNAP ban on any recidivism (ever returning to a Florida state prison). I estimate that the SNAP ban increased any recidivism among drug traffickers by about 9.5 percentage points on average. The baseline recidivism rate for drug traffickers committing their crime in the 240 days prior to the cutoff date is about 16.4 percent. This implies that the SNAP ban increased recidivism among drug traffickers by about 58 percent.

Admittedly, an effect of this magnitude is large and at first blush, might seem unrealistic. First, note that the 9.5 percentage point estimate is only the point estimate. Because the sample size is small, the estimates are noisy and the confidence interval is large. For example, the 90 percent confidence interval for the estimate in column 5 of Table 3 is (0.017, 0.172), which implies the SNAP ban increased recidivism among drug traffickers by about 10 percent to 105 percent.[15] Second, even large estimates may be reasonable when we consider that the SNAP benefit is a substantial chunk (about 20 percent) of gross income for men receiving SNAP in Florida.

In addition, SNAP benefits are an important resource for ex-offenders. Recall that approximately 70 percent of the former inmates in the Boston Reentry Study report receiving SNAP benefits even just two months after release (Western et al. 2015). Sugie (2012) also finds that about 70 percent of families in the Fragile Families and

other detailing a case of food stamps fraud. In addition, a search of the Vanderbilt Television News Archive reveals 12 major news broadcasts over this period about "food stamps." All of these segments are under 4 minutes long and based on the descriptions, they are broad discussions of the 1996 welfare reform. It does not appear that the ban on felony drug offenders was a particularly salient piece of the welfare overhaul in 1996.

[15] A 10 percent increase in recidivism is reasonable and in line with other papers in this field. Yang (2017b) finds that SNAP bans increase one-year recidivism rates by about 13 percent. Yang (2017a) finds that a 5 percent increase in real wages due to local labor market opportunities decreases recidivism by about 2.3 percent—extrapolating this based on Table 2, a 25 percent increase in real wages due to SNAP receipt would decrease recidivism by 11.5 percent. Finally, several earlier papers found that giving unemployment assistance to released offenders decreased probability of rearrest by 8 to 27 percent.

Table 3—Main Results: Effect of the SNAP Ban on Recidivism

Outcome	Recidivism (1)	Financially motivated recidivism (2)	Non-financially motivated recidivism (3)
Panel A. Imbens-Kalyanaraman optimal bandwidth			
Offense committed after Aug. 23, 1996 (banned)	0.0803	0.1043	−0.0100
	(0.0493)	(0.0398)	(0.0280)
Control group mean	0.1587	0.0874	0.0764
Observations	791	936	980
Bandwidth (in days)	±212	±242	±254
Degree of polynomial in days from Aug. 23, 1996	1	1	1
Panel B. Consistent bandwidth of ±240 days			
Offense committed after Aug. 23, 1996 (banned)	0.0950	0.1003	−0.0053
	(0.0467)	(0.0404)	(0.0286)
Control group mean	0.1644	0.0880	0.0764
Observations	918	918	918
Bandwidth (in days)	±240	±240	±240
Degree of polynomial in days from Aug. 23, 1996	1	1	1

Notes: Standard errors clustered at the day of offense are in parentheses. Number of days the drug trafficking offense was committed before or after Aug. 23, 1996 is the running variable (centered at zero). Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender ever returns to a Florida prison after being released. Columns 2 and 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table 1 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. In panel A, the Imbens-Kalyanaraman optimal bandwidth is used with polynomial of degree one and a uniform kernel. In panel B, a bandwidth of ±240 days (or ±8 months) is used with polynomial of degree one and a uniform kernel. Results are robust to these choices (see online Appendix Tables A13–A15).

Child Wellbeing Study with a recent paternal incarceration report receiving SNAP in the past year. The Panel Study of Income Dynamics (1995–2013) asks respondents in 1995 if they have ever been in the corrections system (jail, prison, youth corrections). Almost 50 percent of respondents who answered yes to that question were in families that reported receiving SNAP at some point from 1995–2013.[16]

Finally, it is easy to assume that former drug traffickers are not reliant on SNAP because drug trafficking is potentially lucrative. However, when these offenders are released from prison, they do not automatically return to drug trafficking. The key idea in this paper is that former drug traffickers choose a number of hours to work in the illegal sector and that access to SNAP informs that choice. I argue that former drug traffickers who are banned from SNAP do choose to work more hours in the illegal sector, and thus, will be more likely to return to prison. In addition, it is not even clear that active drug traffickers earn a substantial income, on average. For example, a person is charged with trafficking heroin in Florida if they sell, manufacture, or distribute 4 grams of heroin. While 4 grams of heroin has a value of approximately $1,000 according to the Drug Enforcement Administration (2015), this does

[16] Also, a 58 percent increase in recidivism is not far from some others in the literature. Carr and Packham (2017) finds that the timing of SNAP receipt alone decreases grocery store theft in Chicago by 32 percent. Di Tella and Schargrodsky (2013) finds that electronic monitoring of inmates (relative to imprisonment) reduces rearrest by half of baseline. Hansen (2015) uses a discontinuity in driving under the influence (DUI) punishments and finds that being charged with an "aggravated DUI" reduces reoffending by 27 percent. Finally, Aizer and Doyle (2015) finds that incarceration as a juvenile increases likelihood of adult incarceration (by the age of 25) by about 70 percent.

not imply that the trafficker nets a profit of \$1,000. Work by Levitt and Venkatesh (2000) suggests that even "officers" (the position above "foot soldier" but below "gang leader") in a drug-selling gang earn approximately \$1,400 per month (in 2010 dollars). Foot soldiers earn even less at around \$200 per month (in 2010 dollars).[17]

In columns 2 and 3 of Table 3, I estimate the effect of the SNAP ban on probability of financially motivated recidivism and probability of non-financially motivated recidivism. I find the effect is completely driven by recidivism for financially motivated crimes. Column 2 of panel B suggests that the SNAP ban increases financially motivated recidivism by 10 percentage points while column 3 suggests the ban had no detectable effect on non-financially motivated recidivism. The total increase observed in column 1 was 9.5 percentage points. This implies that 100 percent of the increase in the probability of returning to prison comes from offenders committing crimes that have monetary compensation. Preexisting differences in the types of crimes drug traffickers returned to prison for cannot account for this result. Drug traffickers who committed their offense in the 240 days prior to the cutoff date were equally likely to return to prison for both financial and nonfinancial crimes. Finally, the increase in recidivism for financially motivated crimes is significantly different from the change in nonfinancial crimes at the 5 percent level (p-value $= 0.0427$).

Figures 2 and 3 present visual evidence of the results in Table 3. The figures show linear polynomials (fitted on the underlying data) overlaid on scatter plots of recidivism outcomes collapsed to 30-day bin averages. In online Appendix A, I include Figures A4a–A4f, which show both quadratic and kernel-weighted, smoothed polynomial versions of Figures 2 and 3. To further demonstrate the robustness of the main results to choice of bandwidth and polynomial, I show the results of local linear, quadratic, and cubic regressions for bandwidths of 30–1,080 days in online Appendix Figures A5a–A5c. In online Appendix Tables A6–A8, I report results from probit, logit, and Cox Hazard estimations, all of which are consistent with the main results in Table 3.

Since most drug traffickers in my sample never return to prison, the data used in the analyses discussed above include many zeroes. To address concerns about over-dispersion, I collapse the data to 15-day bin averages, and redo the main analysis using OLS on the binned data (weighted by the number of observations in each bin). In these regressions, the dependent variable is the average recidivism rate for all offenders in a given 15-day bin. Likewise, the running variable—distance from August 23, 1996—takes on the average value of distance for all offenders in a bin. Binning also facilitates analyzing the data as count data in a Poisson model and as time-series data. I also control for the number of Fridays in each bin. These results are reported in online Appendix Tables A9–A12 and Figure A6, and are also consistent with the findings in this paper. The evidence here and in the online

[17] Levitt and Venkatesh also discusses legal sector employment, noting that around 80 percent of foot soldiers are employed in the legal sector at some point in a given year. However, these are not stable jobs (only 40–50 percent of foot soldiers are employed at any given time) and the jobs tend to be low-wage service-sector work. Levitt and Venkatesh further stresses that both foot soldiers and officers report living with family because they cannot afford their own housing. Finally, to the extent that access to SNAP influences how much time (if any) to allocate to illegal work post-release, that decision should be reflected in the probability of recidivism.

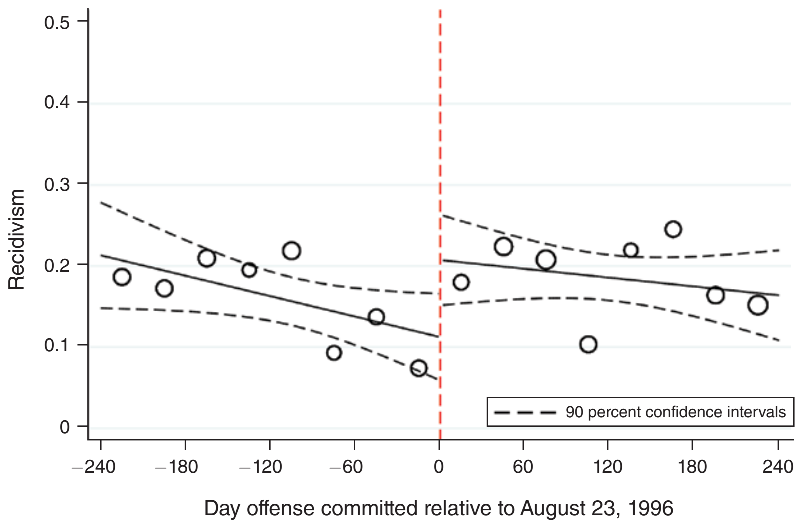

Figure 2. Effect of SNAP Ban on Any Recidivism

Notes: See Figure 1 notes for a general description of the creation of the RD plots, including information about bin size, estimation of the fit lines, and definition of the running variable. In this figure, the dependent variable is recidivism, and recidivism is defined as whether an offender ever returns to a Florida prison after release.

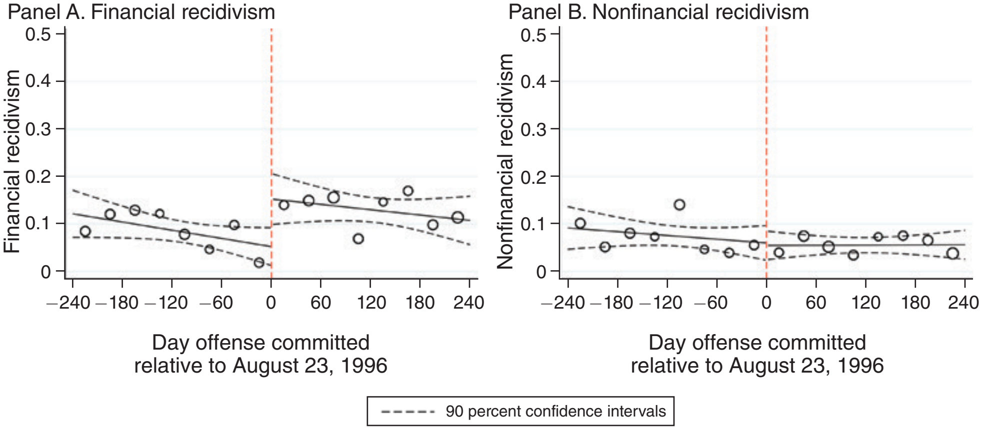

Figure 3. Effect of SNAP Ban on Financial and Nonfinancial Recidivism

Notes: See Figure 1 notes for a general description of the creation of the RD plots, including information about bin size, estimation of the fit lines, and definition of the running variable. In this figure, the dependent variables are financial recidivism and nonfinancial recidivism. See Table 1 for a definition of financially and non-financially motivated crimes and the associated recidivism measures.

Appendix suggests that the SNAP ban increased the probability of recidivism for drug traffickers.

B. *Heterogeneity Tests*

The effect of the SNAP ban may be exacerbated by certain factors. The model in online Appendix C predicts that when legal labor market opportunities are more scarce, the banned offenders will be more likely to turn to the illegal labor market. I test this in two ways. First, the effect of the SNAP ban should be smaller when ex-offenders face a tight labor market and increasing legal labor supply becomes more feasible. I interact the state-level unemployment rate at the month of the offender's release with all other variables in equation (1), and present the results in online Appendix Table A16 (Bureau of Labor Statistics (BLS) 1996–2016). The effect is not statistically different from zero, but the point estimates imply the ban increases recidivism more for offenders released in poor legal labor markets. Second, evidence suggests that ex-offenders who are black face heightened discrimination in the legal labor market. If the SNAP ban does affect recidivism via work incentives, we should see stronger effects for black offenders. These results are in online Appendix Table A17. Again, the estimates on the interaction between race and the cutoff are all positive, as expected, but they are not statistically different from zero.

I also investigate how the SNAP ban affects timing of re-incarceration. To do this, I estimate the effect of the ban on the probability the offender returns to prison in zero to five years and the effect of the ban on the probability the offender returns to prison in five to ten years. These results, presented in online Appendix Table A18, suggest that the effect of the ban is slightly focused in earlier years rather than later years. Also, in online Appendix Figure A7, I show the effect of the ban on recidivism within one-year windows. Again, these results show that the increase in recidivism due to the ban is occurring in both earlier years and later years, though more so in earlier years. It is difficult to interpret these results since time to re-incarceration is a function of both the time it takes for an ex-offender to reenter the illegal labor market and the time it takes for an ex-offender to be caught once they reenter. In addition, SNAP generosity and ABAWD waivers both vary over time.

Finally, I compare the effect of the SNAP ban on the probability an offender recidivates in a month (using month of offense) and county (using county of conviction) when the ABAWD work requirement is waived and the effect of the SNAP ban on the probability an offender recidivates in a month and county when the ABAWD work requirement is in effect (Florida Department of Children and Families (FL DCF) 1996–2016). When the ABAWD work requirement is waived, able-bodied adults without dependents who are not banned from SNAP can receive SNAP benefits even if they are unemployed and not enrolled in employment/training programs. Online Appendix Figure A9 displays the geographic variation in county-level ABAWD work requirement waivers for 1996, 1998, 2000, 2004, 2006, and 2008.[18] If the main results are due to SNAP receipt, then the increase in recidivism as a result of the ban should be driven by increased recidivism occurring in months and counties with ABAWD waivers. This is when the disparity in transfer income between the control group (not banned from SNAP) and the treatment group (banned from SNAP) is the greatest. In online Appendix Table A19, I show that the

[18] I do not show 2002 or years after 2008 because nationwide ABAWD waivers are in place. An animation showing the geographic variation in waivers from January 1996–December 2008 can be found here: https://www.dropbox.com/s/kufg1ieiwtjm0b6/Waivers%20by%20County-Month.gif?dl=0.

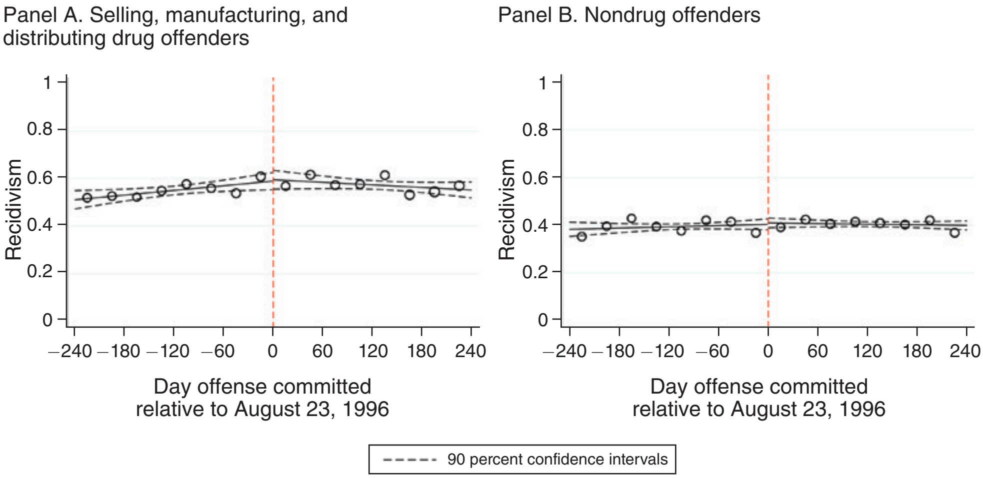

Figure 4. Effect of SNAP Ban on Any Recidivism for Placebo Offenses

Notes: See Figure 1 notes for a general description of the creation of the RD plots, including information about bin size, estimation of the fit lines, and definition of the running variable. In both figures, the dependent variable is recidivism, defined as whether the offender ever returns to a Florida prison or not. Figure 4, panel A displays this relationship for offenders convicted of committing the crime of selling/manufacturing/distributing (SMD) drugs. These offenders were exempted from the SNAP ban by the Florida legislature in May 1997. Thus, if the main results are driven by endogenous sorting around the cutoff, we should also observe an effect for SMD offenders. Figure 4, panel B displays this relationship for offenders convicted of committing any nondrug crime. These offenders were never subject to the SNAP ban, and thus, their likelihood of recidivism should be smooth through the cutoff date. Both placebo tests show no change in recidivism for offenders committing their offense after the cutoff date.

increase in recidivism is concentrated in months and counties when the ABAWD work requirements are waived.[19,20]

C. *Placebo Tests and Threats to Validity*

Florida modified the Federal SNAP ban to exempt offenders convicted of drug possession or selling, manufacturing, and distributing (SMD) drugs; however, state lawmakers did not pass legislation modifying the ban until May 1997 (US Government Accountability Office (GAO) 2005).[21] If the results in this paper are driven by endogenous sorting around the cutoff, we should also find effects for offenders committing SMD since all available information as of August 23, 1996 indicated that the ban would apply to those offenders. These results are in Figure 4a and online Appendix Table A22. I find no effect for SMD offenders, which further suggests that the effect for drug traffickers is not driven by endogenous sorting at the cutoff. I also estimate the effect of the SNAP ban with a regression discontinuity

[19] At a bandwidth of $\pm$ 240 days from the cutoff date, the effect of the ban on recidivism when the ABAWD requirement is waived is statistically different from the effect on recidivism when the ABAWD requirement is in effect at the 5 percent level (p-value $=$ 0.0461) in the local linear model.

[20] I present alternative versions of this test in online Appendix Tables A20 and A21.

[21] The sample of people who committed SMD or drug trafficking consists almost entirely of people who were incarcerated for over a year.

difference-in-differences design, using SMD offenders as a control group. Using the ± 240 day bandwidth, this strategy yields a coefficient estimate of about 9.5 percentage points.

Figure 4b and online Appendix Table A23 display another placebo test examining recidivism for all nondrug offenders around the cutoff date. These offenders were never banned from SNAP as part of the federal policy, and thus their behavior should also be unaffected by the cutoff date. I find no change in recidivism for these offenders. I conduct additional placebo tests using all offenders convicted of a DUI, drug possession, property crime, and violent crime in online Appendix Table A24 and Figures A11a–A11d. I find no evidence of increased recidivism after the cutoff date for these offenders.

One major concern with regression discontinuity designs that use time as the running variable is that the policy cutoff date coincides with a seasonal pattern. If the results in this paper are driven by a general seasonal trend in the relationship between recidivism and date of offense or a trend specific to 1996, the placebo tests in Figures 4a–4b, online Appendix Tables A22–A24, and Figures A11a–A11d should also recover positive estimates but that they do not. However, it is possible that there is spurious seasonality around August 23 that is specific to drug traffickers. To rule out this explanation, I run 16 placebo regressions, one for each August 23rd from 1997–2012.[22] For example, in the 1997 regression, I code the variables $After_{it}$ and $g(DaysFromCutoff_{it})$ as if the cutoff date is August 23, 1997. I do not include years after 2012 since offenders committing crimes in those years have little time to recidivate. I use a bandwidth of ± 180 days in each regression to avoid overlapping observations in the tests. The distribution of coefficients from these regressions is in Figure 5. Standard regression discontinuity plots for all years from 1997–2012 are included in Figure A12 of online Appendix A. In addition, I estimate a regression discontinuity difference-in-differences design using all August 23rds from 1996-2012. I exclude August 23, 1998 and August 23, 1999 from this test because two criminal justice policies affecting drug traffickers were introduced in Florida in those years.[23] The results in online Appendix Table A25 provide further evidence that seasonality in the relationship between offense date and recidivism cannot explain the findings in this paper.

Placebo tests using different crimes around August 23, 1996 rule out threats to validity that would affect multiple types of crime in 1996. Similarly, placebo tests using drug traffickers around other August 23rds rule out threats to validity that would affect drug traffickers in all years. Still, it is possible that some other event occurred near August 23, 1996 that affected only drug traffickers. While I cannot find any information about other potential treatments in Florida around this time, I

[22] Ganong and Jäger (forthcoming) suggests a similar exercise designed to test the significance of the estimated effect using randomization inference. Results from that test are plotted in online Appendix Figure A14.

[23] To determine which years to exclude I refer to the document covering years 1980–2002 here: http://www.dc.state.fl.us/pub/history/index.html. For years after 2002, I search the phrase "'Florida' 'committed on or after' 'YYYY'" where "YYYY" is the year in question. I examine the first page of search results, and if a policy that affects drug traffickers is mentioned, I exclude that year. Through this process, I exclude 1998 and 1999. In October 1998, Florida overhauled their criminal justice system with a new "punishment code" that lowered the requirements necessary to receive a prison sentence. In July 1999, Florida instituted mandatory minimums for drug trafficking offenses.

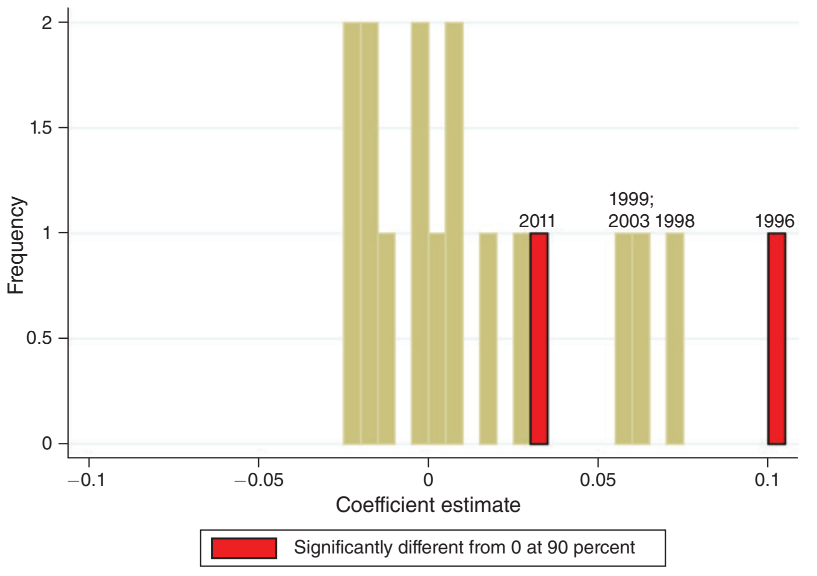

Figure 5. Distribution of Coefficients from Placebo Tests at August 23, 1997–2012

Notes: The figure above displays a histogram of the coefficient estimates from 16 placebo regressions (one at each August 23rd from 1997–2012) and the coefficient estimate from the main result (at August 23, 1996). The dependent variable in these placebo tests is recidivism—whether the offender ever returns to a Florida prison or not. In all regressions, I use a bandwidth of ±180 days to avoid overlapping observations across tests. Only one estimate from the 16 placebo regressions is statistically different from zero; it is from the year 2011, and it is much lower in magnitude than the main result. In addition, there are three estimates that are larger than the 2011 placebo estimate. These correspond to years 1998, 1999, and 2003. In October 1998, Florida overhauled their criminal justice system with a new "punishment code" that lowered the requirements necessary to receive a prison sentence. In July 1999, Florida instituted mandatory minimums for drug trafficking offenses.

also show results of a test designed to detect other significant breaks in my bandwidth. This test, designed by Card, Mas, and Rothstein (2008), detects August 29, 1996 as the true cutoff date. August 29, 1996 is only six days from the policy cutoff date. In fact, the 15 placebo dates with the highest R^2 are all within 9 days of August 23, 1996, and August 23, 1996 yields the fourth highest R^2. Dates near September 27, 1996 also return high R^2. I check again in Florida and at the federal level for other policies enacted around September 27, 1996—I do not find any. These placebo results provide further evidence that the SNAP ban causally affects the recidivism outcome of drug traffickers.

I interpret the increase in financially motivated crimes as an increase in the illegal labor supply of ex-offenders. However, a more subtle interpretation is that ex-offenders not subject to the SNAP ban face a bigger deterrent to committing drug trafficking than ex-offenders subject to the ban—those not subject to the ban initially will lose access to SNAP if they commit drug trafficking after they are released since the ban applies to anyone who commits drug trafficking after August 23, 1996. This is an important concern for my analysis since these two interpretations yield different policy implications. If the ban increases the recidivism of banned offenders by pushing them into illegal work, that is a negative consequence that should be factored into policy discussions. If the ban decreases the recidivism of non-banned

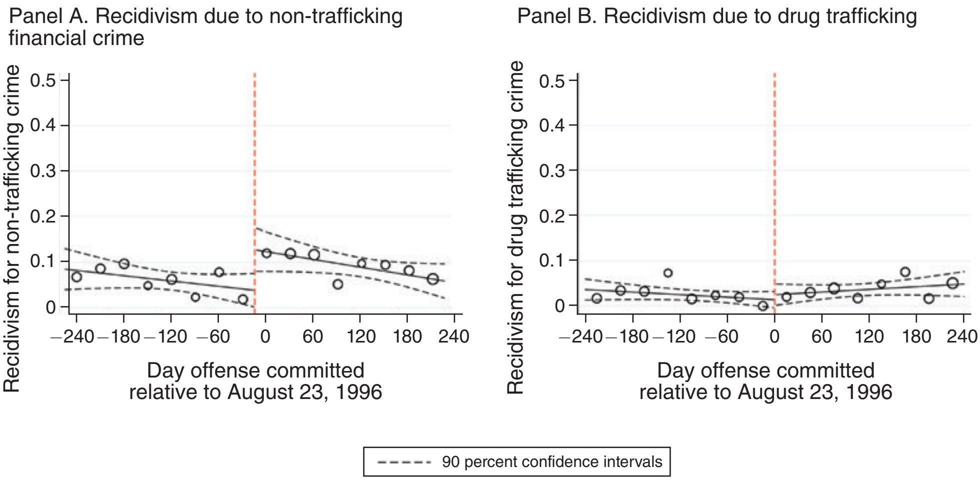

Figure 6. Effect of SNAP Ban on Recidivism Due to Trafficking and Non-trafficking Crimes

Notes: See Figure 1 notes for a general description of the creation of the RD plots, including information about bin size, estimation of the fit lines, and definition of the running variable. In Figure 5, panel A, the dependent variable is financial recidivism excluding recidivism for drug trafficking crimes. In Figure 5, panel B, the dependent variable is recidivism for drug trafficking crimes only. See Table 1 for a definition of financially motivated crime and the associated recidivism measure. If the SNAP ban causes an increase in recidivism by reducing the drug trafficking activity of non-banned offenders (deterred by the threat of the ban after they are released), then recidivism for drug trafficking should be higher for banned offenders than non-banned offenders. Instead, recidivism for drug trafficking is similar for both banned and non-banned offenders while recidivism for non-trafficking crimes is higher for banned offenders. These figures imply that the main results are driven by increased criminal activity of banned offenders.

offenders by deterring them from drug trafficking, that is a positive consequence that should be considered in policy discussions.[24]

Fortunately, the deterrence hypothesis yields a testable implication. If the increase in recidivism is driven by non-banned offenders deterred from future drug trafficking, then the increase should be concentrated in an increase in recidivism for drug trafficking crimes. I find no detectable increase in recidivism due to future drug trafficking. However, I do find statistically significant increases in recidivism for other financially motivated offenses. The results in Figure 6 and online Appendix Table A26 indicate that banned offenders are 8.9 percentage points more likely to return to prison due to a financial crime that is not drug trafficking and only 1.1 percentage points more likely to return with a drug trafficking offense. Recall that the total effect on financial recidivism is a 10 percentage point increase. This suggests that only 11 percent of the total effect can be explained by the deterrence hypothesis.

[24] A similar alternative explanation is that all offenders return to drug gangs upon release and that those gangs allocate their "banned" members to riskier crimes since they have less to lose if they are caught. Being assigned to carry out riskier crimes thus leads to increased recidivism for those subject to the SNAP ban. I also estimate the effect of the SNAP ban on recidivism for theft, a crime that I assume drug gangs are less likely to be in the business of committing (only 23 percent of offenders who have served time for selling, manufacturing, or distributing drugs in the data have also served time for a theft charge). I find that offenders subject to the SNAP ban are indeed more likely to return to prison for theft.

VI. Conclusion

SNAP provides valuable assistance to millions of low-income Americans. However, many ex-felons, a particularly needy and at-risk population, are excluded from SNAP. This paper provides evidence that denying drug offenders SNAP benefits has increased their likelihood of recidivism. Standard econometric tests for breaks in the data as well as institutional features of the policy change alleviate concerns about sorting threats to the regression discontinuity identification. Also, it does not appear that the ban was widely publicized in the year prior to August 23, 1996 or in the year following August 23, 1996. This main result speaks to an important policy discussion about state repeals of these bans.

Looking closely at the types of crimes that land these offenders back in prison, I find that the increase in recidivism is driven by crimes that have a monetary motive (property crimes, selling drugs, etc.) rather than crimes like drug possession or violent crimes. This result contributes to a literature on the labor supply effects of transfer programs, and highlights the importance of acknowledging the illegal labor margin when designing policies and programs that affect work incentives.

Using the estimate of the effect of the SNAP ban, I provide a back-of-the-envelope calculation of the cost associated with the increased recidivism. For every offender who recidivates because of the SNAP ban, Florida pays the cost to incarcerate that offender and the citizens of Florida suffer costs of victimization.[25] Using existing estimates of the marginal cost of incarceration and costs of victimization, I derive the cost of banning an extra drug offender. Cost per offender is defined as (Marginal Increase in Probability of Offending due to the Ban) $\times$ (Marginal Cost of Year of Incarceration) $\times$ (Mean Years Sentenced) $+$ (Marginal Increase in Probability of Offending due to the Ban) $\times$ (Victim Cost). More details on this calculation are shown in online Appendix D. Assuming the ban increases recidivism by about 9 percentage points (the point estimate from the main results), I find the societal cost of the ban in Florida is about $3,700 per banned offender. With approximately 19,000 banned offenders, this implies the ban has cost Florida over 70 million dollars to date, a number that grows with every new trafficker who resorts to crime to make up for the lost benefits.

Ultimately, analysis of the SNAP ban speaks to prisoner reentry policy in general as well as the work incentives associated with transfer programs. Even more, analysis of the ban contributes to an active policy discussion about the repeal of these bans. In April 2016, Georgia's Governor Nathan Deal signed a law modifying the SNAP ban, joining Texas and Alabama, the two other states that modified the ban in 2016 (Phillips 2016). The SNAP ban continues to affect the day-to-day life of

[25] The "marginal cost" of incarceration is a term used by the Department of Justice (OJG 2011) defined as "the direct care cost incurred […] to house an inmate […] includes the cost of feeding, clothing, and providing medical care for an inmate." This number is significantly lower than the "average cost" of incarceration, which takes into account fixed costs, and using it in the cost-benefit analysis leads to a more conservative estimate of the costs. Also, in calculating the societal cost of the ban, I ignore the cost of providing released drug traffickers SNAP benefits. However, if we ignore the private benefit of SNAP to drug traffickers, taxpayers in general do save money by denying SNAP benefits to all drug traffickers.

drug felons in 27 states, and it is certainly a relevant and important topic for future research.

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