Snapping Back: Food Stamp Bans and Criminal Recidivism*

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Abstract

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.

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I. Introduction

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 of releasees are back in prison within five years of their release and three-quarters are re-arrested 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.

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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.

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

[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 2016; 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,

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 (U.S. Department of Health and Human Services (HHS) 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 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

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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.

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 2010; Gassman-Pines and Bellows 2015; Gregory and Deb, 2015; 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 dollars 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.

II. 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 (U.S. 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, 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.

III. 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

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[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 Families and Children does not have an automated system to cross-check applications with the Florida Department of Corrections, offender information is easily searchable online. The Office of Public Benefits Integrity in Florida has also partnered with the Florida Department of Corrections 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.

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 (2017) 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 use 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 in two major ways. First, I focus on longer-run recidivism outcomes, while they study the effect on recidivism within 3 years. In this paper, I also find a small and statistically insignificant positive effect on recidivism within 3 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 opt-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 re-arrest 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 re-arrest for property crime by about 8-27 percent.[5] The effect of these programs on re-arrest in general is less clear, but the largest effects are concentrated in re-arrest 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 find 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) demonstrate 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] 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) use 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 (noneldery, 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

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[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) argue that vouchers 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) exploit 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, Desphande (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.

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.

IV. Data and Descriptive Statistics

A. Offender Data

Florida Department of Corrections (FL DOC) 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, that recidivism is recorded as "financially motivated" if the offender was charged with a financially motivated crime for the prison stay occurring after stay j and that 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

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[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.

offender meets those three criteria, then all of their stays in FL 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, non-citizen 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 non-citizen 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 US and less than 5 percent of Black or White individuals institutionalized in Florida report a birthplace outside the US. 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 non-drug 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 non-drug 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 non-drug 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 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: (1) the SNAP benefit is an important source of income and (2) recipients do not have to be employed to receive SNAP benefits, despite the well-known work requirements of post-PRWORA SNAP.

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[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.

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 & Child Wellbeing Study with a recent paternal incarceration report receiving SNAP in the past year. The Panel Study of Income Dynamics 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 3 months of SNAP receipt every 3 years unless they: (1) work 20 or more hours per week, (2) participate in an employment and training program, or (3) participate in a workfare program (U.S. 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. The table below 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.

V. 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:

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

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] $After_{it}$ 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

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[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.

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

$$Characteristic_{it}^{D} = \alpha + \beta_1 After_{it} + g(DaysFromCutoff_{it}) +$$

$$g(DaysFromCutoff_{it}) \times After_{it} + \omega_{it} \quad (2)$$

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 offense, 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-

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[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

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. On the other hand, 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.

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 (U.S. 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]

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[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.

[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 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.

VI. 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 & Child Wellbeing Study with a recent paternal incarceration report receiving SNAP in

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[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 1-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 re-arrest by 8 to 27 percent.

the past year. The Panel Study of Income Dynamics 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 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), 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. Pre-existing 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 non-financial crimes. Finally, the increase in recidivism for financially motivated crimes is significantly different from the change in non-financial crimes at the 5 percent level (p-value=0.0427).

Figure 2 and Figures 3a-3b 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

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[16]Also, a 58 percent increase in recidivism is not far from some others in the literature. Carr and Packham (2017) find that the timing of SNAP receipt alone decreases grocery store theft in Chicago by 32 percent. Di Tella and Schargrodsky (2013) find 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) find that incarceration as a juvenile increases likelihood of adult incarceration (by the age of 25) by about 70 percent.

[17]Levitt and Venkatesh also discuss 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 stress 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.

30-day bin averages. In online Appendix A, I include Figures A4a-A4f, which show both quadratic and kernel-weighted, smoothed polynomials versions of Figures 2-3b. 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 30-1080 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 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 0 to 5 years and the effect of the ban on the probability the offender returns to prison in 5 to 10 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 1-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 re-enter the illegal labor market and the time it takes for an ex-offender to be caught once they re-enter. 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 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 (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 difference-in-differences design, using

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[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

[19]At a bandwidth of plus-or-minus 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.

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 non-drug 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–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 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

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[22]Ganong and Jäger (2018) suggest 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.

August 29, 1996 as the true cutoff date. August 29, 1996 is only six days from the policy cutoff date. In fact, the fifteen placebo dates with the highest R-squared are all within 9 days of August 23, 1996, and August 23, 1996 yields the fourth highest R-squared. Dates near September 27, 1996 also return high R-squared. I check again in Florida and at the Federal-level for other polices 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 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 Figures 6a-6b and online Appendix Table A16 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.

VII. 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

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[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.

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)×(Marginal Cost of Year of Incarceration)×(Mean Years Sentenced)+(Marginal Increase in Probability of Offending due to the Ban)×(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 drug felons in 27 states, and it is certainly a relevant and important topic for future research.

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[25]The "marginal cost" of incarceration is a term used by the Department of Justice 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 estimates 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.

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Tables and Figures

Table 1. Summary Statistics for Drug Traffickers & Other Offenders in Florida

	October 1, 1995 - October 1, 1997			Full Sample
	All Non-Drug Offenders	Sell/Mfg/Dist 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)
Non-Financial 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

Note: 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 motivate 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. Sell/mfg/dist offenders are those offenders convicted of selling/manufacturing/distributing drugs. Sell/mfg/dist 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 non-drug 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.

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 $)	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 as % of Gross Income	15.703	25.818	18.124	29.326
	(16.700)	(21.135)	(16.720)	(22.723)
Observations	1,027	1,347	924	968
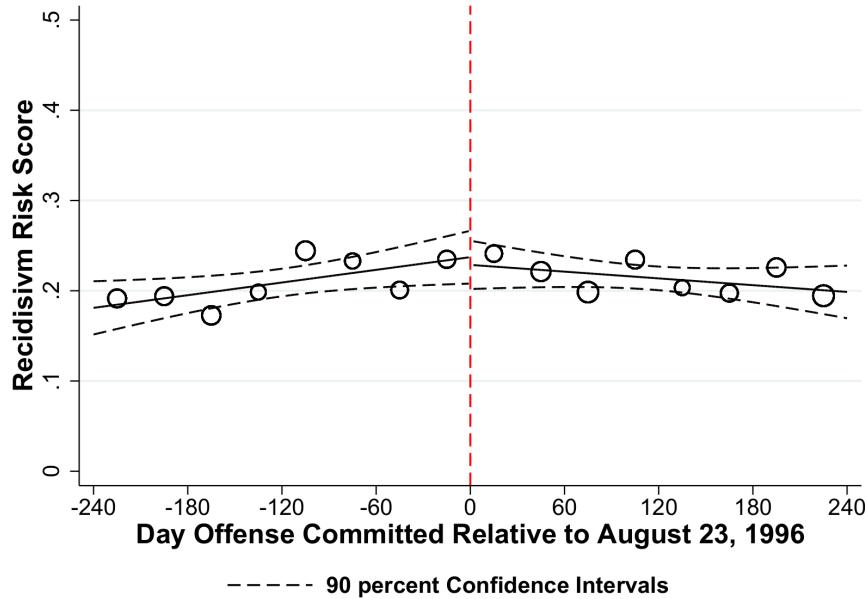
Note: Summary statistics above are derived from the Mathematica Policy Research SNAP QC file from 1996-2014, which provide data on a sample of the SNAP population in each state. Mathematica Policy Research constructs the SNAP QC files to be representative at the state-level. I limit the sample to males 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: (1) work 20 or more hours per week, (2) participate in an employment and training program, or (3) participate in a workfare program (USDA, 2016b).

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

| Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(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
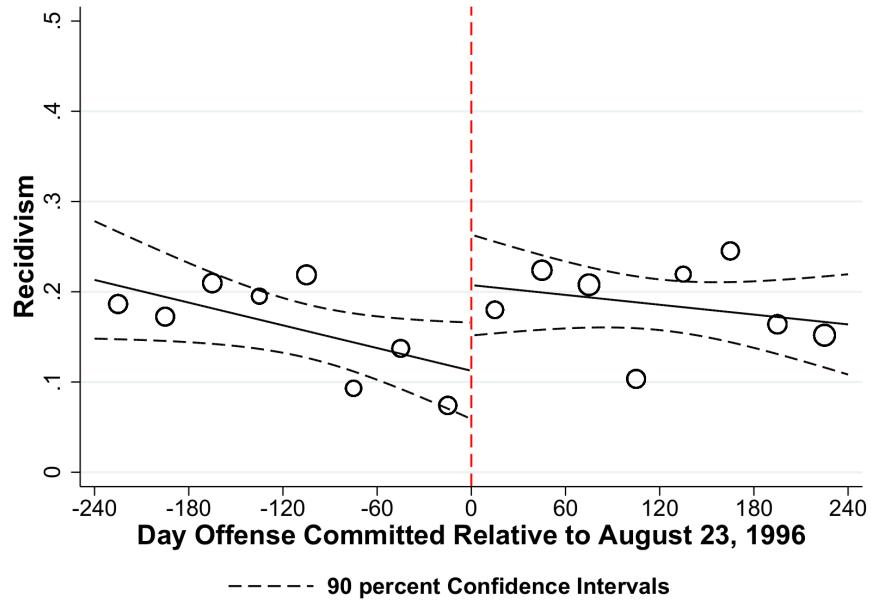
Notes: Standard errors clustered at the day of offense in parentheses. Number of days the drug trafficking offense was committed before or after Aug. 23, 1996 is the running variable (centered at zero). Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender ever returns to a Florida prison after being released. Column 2 and Column 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). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Figure 1: Smoothness Through Cutoff in Offender's Risk of Recidivism**
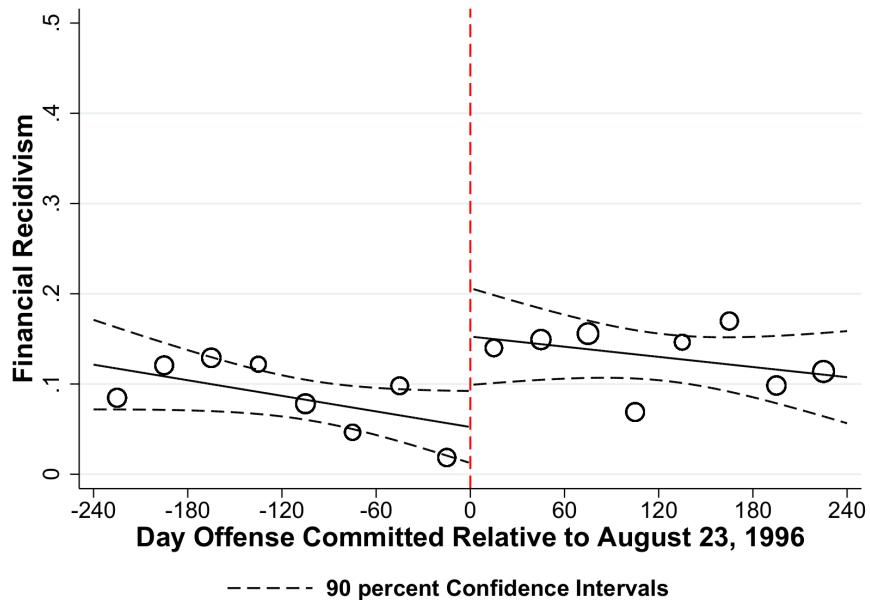
Notes: Recidivism risk score is calculated by: (1) estimating the relationship between offender characteristics and recidivism using a sample of pre-ban drug traffickers who are not included in the Imbens-Kalyanaraman (IK) optimal bandwidth and (2) applying those estimates to drug traffickers in the 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.

Figure 2. Effect of SNAP Ban on Any Recidivism
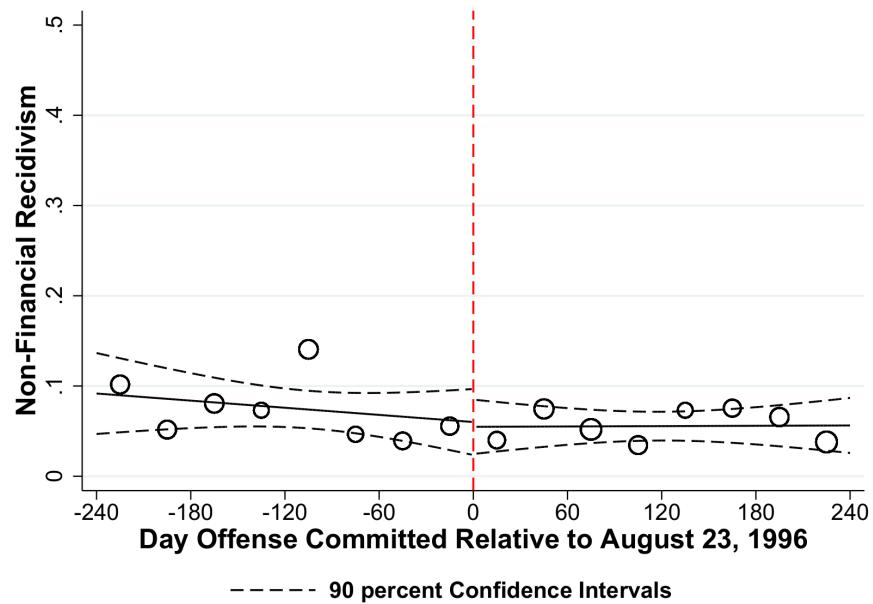
Notes: See Figure 1 notes for a general description of the creation of the RD plots, including information about bin size, estimation of the fit lines, and definition of the running variable. In this figure, the dependent variable is recidivism, defined as whether an offender ever returns to a Florida prison after release.

Figure 3a. Effect of SNAP Ban on Financial Recidivism
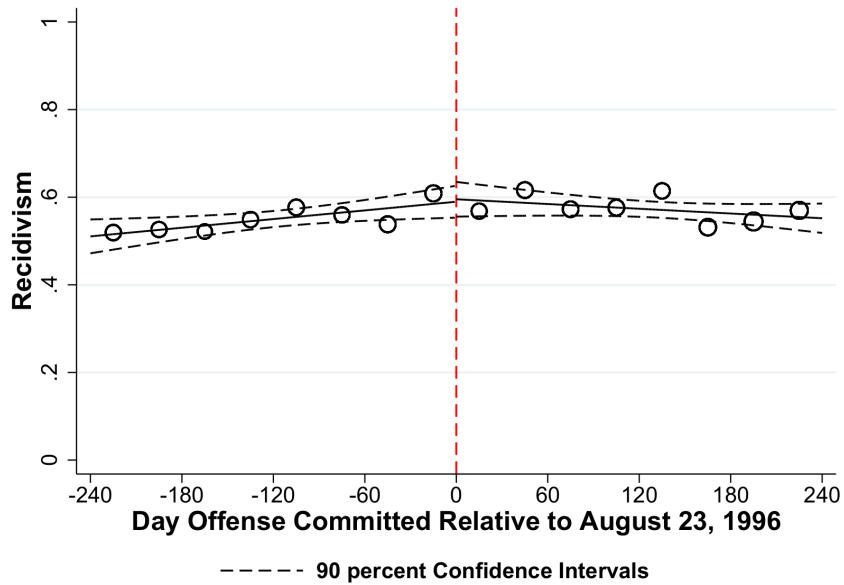

Notes: See Figure 1 notes for a general description of the creation of the RD plots, including information about bin size, estimation of the fit lines, and definition of the running variable. In this figure, the dependent variable is financial recidivism. See Table 1 for a definition of financially and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

Figure 3b. Effect of SNAP Ban on Non-Financial Recidivism
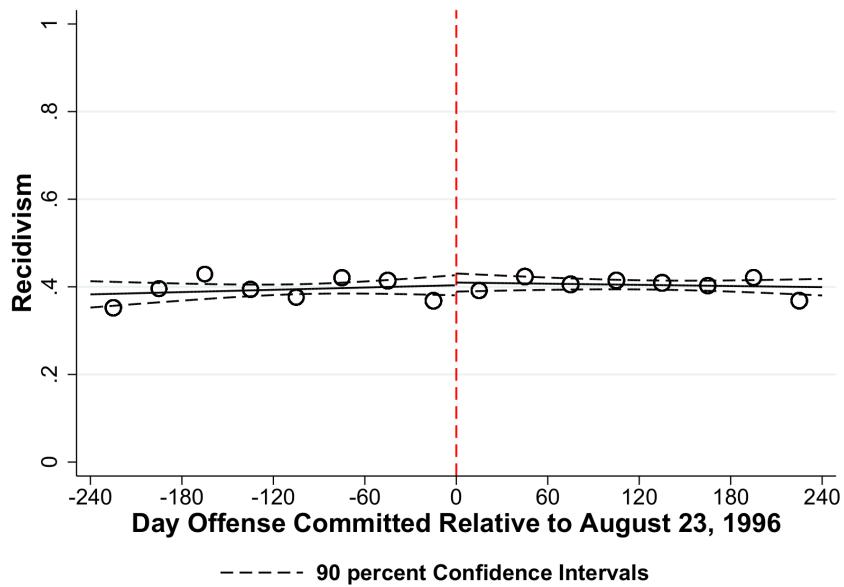

Notes: See Figure 1 notes for a general description of the creation of the RD plots, including information about bin size, estimation of the fit lines, and definition of the running variable. In this figure, the dependent variable is non-financial recidivism. See Table 1 for a definition of financially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

Figure 4a. Effect of SNAP Ban on Any Recidivism for Selling/Manufacturing/Distributing Drug Offenders

90 percent Confidence Intervals
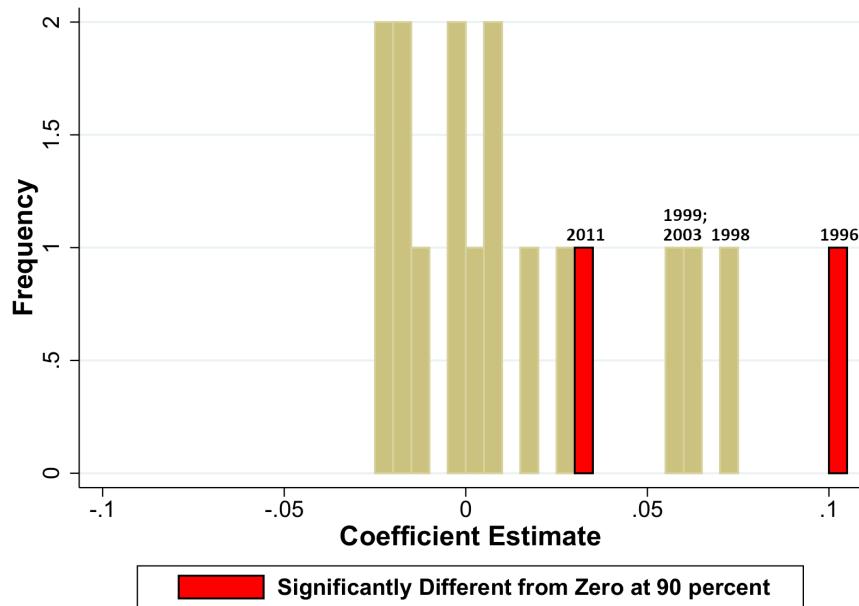
Figure 4b. Effect of SNAP Ban on Any Recidivism for Non-Drug Offenders
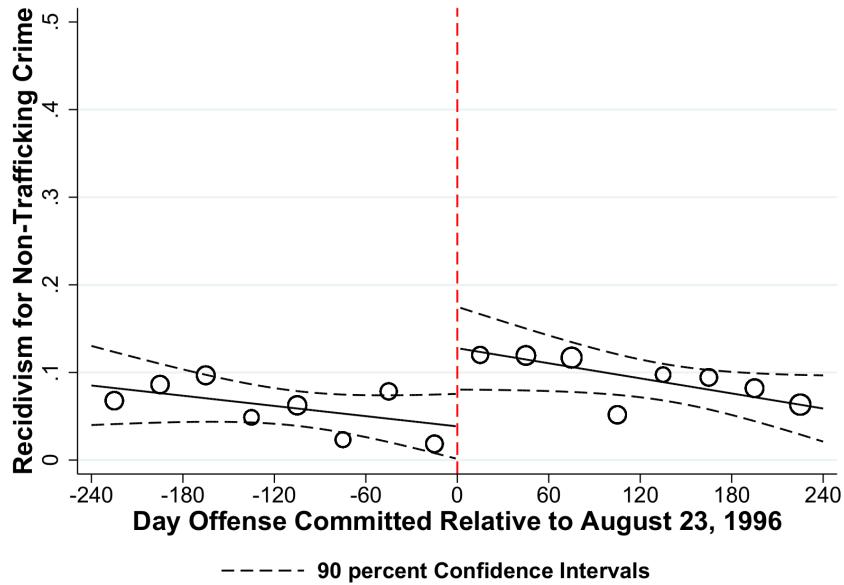

90 percent Confidence Intervals

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 4a 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 4b displays this relationship for offenders convicted of committing any non-drug 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.

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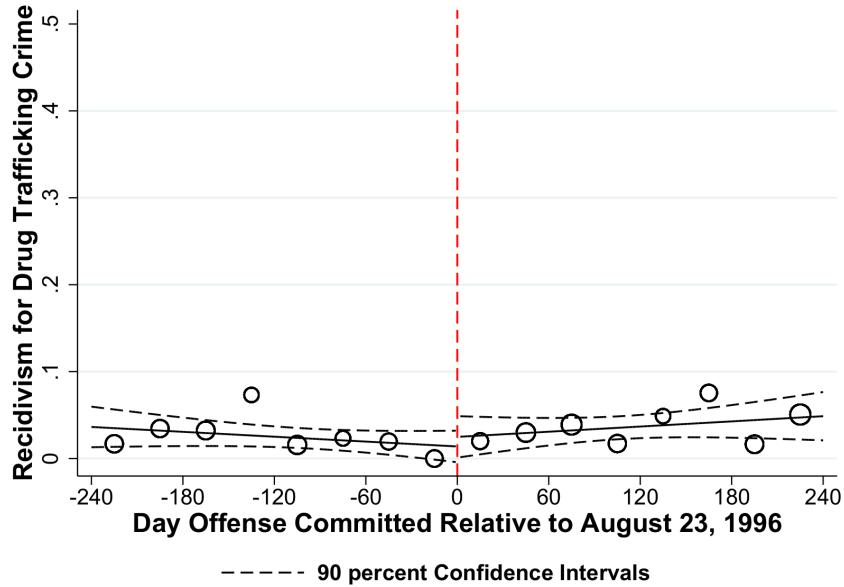

Notes: The figure above displays a histogram of the coefficient estimates from 16 placebo regressions (one at each August 23rd from 1997-2012) and the coefficient estimate from the main result (at August 23rd, 1996). The dependent variable in these placebo tests is recidivism, whether the offender ever 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.

Figure 6a. Effect of SNAP Ban on Recidivism due to Non-Trafficking Crimes

— — — — 90 percent Confidence Intervals

Figure 6b. Effect of SNAP Ban on Recidivism due to Drug Trafficking

— — — — 90 percent Confidence Intervals

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 5a, the dependent variable is financial recidivism excluding recidivism for drug trafficking crimes. In Figue 5b, 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.

Snapping Back: Food Stamp Bans and Criminal Recidivism

Cody Tuttle

Online Appendix

Appendix A. Supplementary Analyses

Table A1. Additional Summary Statistics for Offenders in Florida

	October 1, 1995 - October 1, 1997			Full Sample
	All Non-Drug Offenders	Sell/Mfg/Dist Offenders	Drug Trafficking Offenders	Drug Trafficking Offenders
Recidivism -	0.216	0.288	0.102	0.072
ABAWD Waiver	(0.412)	(0.453)	(0.303)	(0.178)
Recidivism -	0.183	0.276	0.075	0.039
No ABAWD Waiver	(0.386)	(0.447)	(0.264)	(0.131)
# of Recidivism	0.994	1.635	0.413	0.502
Offenses	(1.715)	(2.133)	(1.146)	(1.232)
Trafficking Cocaine	-	-	0.789	0.410
	-	-	(0.408)	(0.468)
# of Prior Offenses	0.578	1.007	0.228	0.298
	(1.052)	(1.291)	(0.586)	(0.663)
# of Concurrent	1.578	2.134	1.502	1.629
Offenses	(0.929)	(1.080)	(0.894)	(0.871)
Male	0.928	0.917	0.885	0.868
	(0.258)	(0.276)	(0.319)	(0.339)
Observations	22,893	6,002	1,435	18,656

Note: The first three rows present recidivism statistics: the fraction of offenders in each group who recidivate in a time and place (based on county of conviction) where ABAWD work requirements are waived, the fraction who recidivate in a time and place where the work requirements are not waived, and the number of offenses committed after prison stay j but before prison stay $j+1$ (coded as zero if there is no stay $j+1$ i.e. the offender does not recidivate). For the ABAWD recidivism measures, conviction county and date of earliest offense after stay j is used. The last four rows show: the fraction of offenders who were convicted of trafficking cocaine, the average number of prior offenses, the average number of concurrent offenses, and the fraction of offenders who are male.

Table A2. Evidence RD Identifying Assumption Holds: No Differences in Observable Characteristics

Characteristic:	# Other Offenses	Years Sentenced	Black	Age	# Prior Offenses	Male	Trafficking Cocaine	Risk Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Imbens Kalyanaraman Optimal Bandwidth								
Offense Committed After	0.0017	0.5552	-0.0563	-0.3157	-0.0988	0.0384	-0.0213	-0.0218
Aug. 23, 1996 (Banned)	(0.1276)	(0.3782)	(0.0692)	(1.3460)	(0.0764)	(0.0393)	(0.0527)	(0.0197)
Control Group Mean	1.5046	5.3285	0.4818	33.5553	0.2478	0.8631	0.8007	0.1952
Observations	944	1580	1281	1067	2290	1275	1317	1391
Bandwidth (in Days)	±246	±465	±338	±281	±802	±334	±349	±380
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1	1	1	1	1	1
Panel B. Consistent Bandwidth of ±240 Days								
Offense Committed After	-0.0108	0.3198	-0.1096	0.1294	-0.0072	0.0196	-0.0342	-0.0085
Aug. 23, 1996 (Banned)	(0.1305)	(0.4950)	(0.0830)	(1.4691)	(0.1046)	(0.0461)	(0.0626)	(0.0240)
Control Group Mean	1.5046	5.1615	0.4861	33.4352	0.2616	0.8611	0.8009	0.2083
Observations	918	918	918	918	918	918	918	918
Bandwidth (in Days)	±240	±240	±240	±240	±240	±240	±240	±240
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1	1	1	1	1	1

Notes: Standard errors clustered at the day of offense in parentheses. Number of days the drug trafficking offense was committed before or after Aug. 23, 1996 is the running variable (centered at zero). 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. Since the data begins with offenses committed on October 1, 1995, the bandwidth is asymmetric for analyses where the bandwidth exceeds ±327 days. Column (1) shows no break in the number of other offenses for which the offender is currently being charged. Column (2) shows no break in the total number of years sentenced. Column (3) shows no break in racial composition and column (4) shows no break in age composition. Column (5) shows no break in the number of prior offenses the offender has been incarcerated in FL prison for. Column (6) shows no break in sex composition. Column (7) shows no break in the probability of trafficking cocaine. Risk of recidivism in Column (8) is calculated from a logistic regression of recidivism on all variables in columns (1)-(7) and age-squared for drug traffickers not subject to the ban and not in the IK sample window. The risk score is then predicted by applying the coefficients from that regression to the sample of drug offenders in my analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3. Effect of the SNAP Ban on Time-Constrained Recidivism Rates

Outcome:	Recidivism within 10 Years	Financial Recidivism within 10 Years	Recidivism within 8 Years	Financial Recidivism within 8 Years	Recidivism within 5 Years	Financial Recidivism within 5 Years
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Imbens Kalyanaraman Optimal Bandwidth						
Offense Committed After	0.1099**	0.0965**	0.0950**	0.1026***	0.0436	0.0748**
Aug. 23, 1996 (Banned)	(0.0511)	(0.0403)	(0.0452)	(0.0336)	(0.0372)	(0.0301)
Control Group Mean	0.1652	0.0846	0.1393	0.0671	0.1046	0.0552
Observations	684	818	840	922	1028	972
Bandwidth (in Days)	±209	±242	±235	±256	±277	±259
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1	1	1	1
Panel B. Consistent Bandwidth of ±240 Days						
Offense Committed After	0.0894*	0.0914**	0.0909**	0.0998***	0.0581	0.0685**
Aug. 23, 1996 (Banned)	(0.0488)	(0.0411)	(0.0446)	(0.0357)	(0.0402)	(0.0316)
Control Group Mean	0.1649	0.0851	0.1386	0.0693	0.1071	0.0548
Observations	803	803	854	854	893	893
Bandwidth (in Days)	±240	±240	±240	±240	±240	±240
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1	1	1	1

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. Columns 1 and 2 estimate the effect of being banned from SNAP on whether or not the offender returns to prison within 10 years of being released and whether or not they return due to a financial crime within 10 years. Columns 3 and 4 estimate the effect on recidivism and financially motivated recidivism within 8 years of release. Finally, columns 5 and 6 estimate the effect on recidivism and financially motivated recidivism within 5 years of release. 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 motivate 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. Time until recidivism is defined as the difference between the offender's release date for prison stay j and the next offense date before prison stay $j+1$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4. Effect of the SNAP Ban on Recidivism Outcomes, Hispanic Individuals Included

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0787*	0.0922**	-0.0049
Aug. 23, 1996 (Banned)	(0.0456)	(0.0369)	(0.0258)
Control Group Mean	0.1525	0.0865	0.0704
Observations	867	1023	1067
Bandwidth (in Days)	±216	±248	±258
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.0873**	0.0882**	-0.0010
Aug. 23, 1996 (Banned)	(0.0435)	(0.0380)	(0.0266)
Control Group Mean	0.1591	0.0882	0.0710
Observations	987	987	987
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 in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. Hispanic offenders are included in the sample for this analysis. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Effect of the SNAP Ban on Recidivism Outcomes, Controls for Offender Characteristics & Day-of-Week Effects

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0922*	0.1064***	-0.0037
Aug. 23, 1996 (Banned)	(0.0492)	(0.0389)	(0.0289)
Control Group Mean	0.1587	0.0874	0.0764
Observations	791	936	980
Bandwidth (in Days)	±212	±243	±255
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.1053**	0.1043***	0.0010
Aug. 23, 1996 (Banned)	(0.0461)	(0.0395)	(0.0297)
Control Group Mean	0.1644	0.0880	0.0764
Observations	918	918	918
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. These analyses include controls for race, age, sex, type of trafficking, total years sentenced, number of prior offenses, number of concurrent offenses, and offense day-of-week fixed effects. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A6. Effect of the SNAP Ban on Recidivism Outcomes, Logit Model

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0776*	0.1022***	-0.0094
Aug. 23, 1996 (Banned)	(0.0460)	(0.0386)	(0.0261)
Control Group Mean	0.1587	0.0874	0.0764
Observations	791	936	980
Bandwidth (in Days)	±212	±243	±255
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.0924**	0.0975**	-0.0055
Aug. 23, 1996 (Banned)	(0.0443)	(0.0389)	(0.0269)
Control Group Mean	0.1644	0.0880	0.0764
Observations	918	918	918
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. This table shows the main specifications estimated with logistic regressions. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7. Effect of the SNAP Ban on Recidivism Outcomes, Probit Model

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0793*	0.1034***	-0.0092
Aug. 23, 1996 (Banned)	(0.0466)	(0.0388)	(0.0265)
Control Group Mean	0.1587	0.0874	0.0764
Observations	791	936	980
Bandwidth (in Days)	±212	±243	±255
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.0940**	0.0986**	-0.0052
Aug. 23, 1996 (Banned)	(0.0449)	(0.0389)	(0.0273)
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 in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. This table shows the main specifications estimated with probit regressions. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table A8. Effect of the SNAP Ban on
Recidivism Outcomes, Hazard Model**

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.5558	1.0959**	-0.1270
Aug. 23, 1996 (Banned)	(0.3415)	(0.4287)	(0.4681)
Control Group Mean	0.1587	0.0874	0.0764
Observations	791	936	980
Bandwidth (in Days)	±214	±271	±233
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.6419**	1.0710**	-0.0453
Aug. 23, 1996 (Banned)	(0.3200)	(0.4368)	(0.4804)
Control Group Mean	0.1644	0.0880	0.0764
Observations	918	918	918
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. This analysis employs a Cox survival model in which offenders enter the sample when they are released and exit when they return to prison. The coefficients are approximate semi-elasticities. For example, the coefficient in column (1) of Panel B indicates that the ban increased recidivism by approximately 60% from baseline. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A9. Results from Regression on 15-day Bin Averages of Recidivism

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0975*	0.1031**	-0.0092
Aug. 23, 1996 (Banned)	(0.0555)	(0.0463)	(0.0268)
Control Group Mean	0.1609	0.0880	0.0761
Observations	28	32	34
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	0.1000*	0.1031**	-0.0031
Aug. 23, 1996 (Banned)	(0.0491)	(0.0463)	(0.0272)
Control Group Mean	0.1644	0.0880	0.0764
Observations	32	32	32
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1

Notes: Standard errors clustered at each 15-day bin in parentheses. In this analysis, the outcome variable is the average recidivism rate within each 15-day bin. Also, the average number of days the drug trafficking offenses in a bin were committed before or after Aug. 23, 1996 is the running variable (centered at zero). All models also control for the number of Fridays in each bin. Also, each regression is weighted by the number of offenders in each bin. In Panel A, the Imbens-Kalyanaraman optimal bandwidth (chosen from the micro data pre-aggregation) 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. Column 1 estimates the effect of the SNAP ban on recidivism rates. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures.

Table A10. Results from Regression on 15-day Bin
Counts of Recidivism, Poisson Model

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.6126*	1.0435*	-0.1639
Aug. 23, 1996 (Banned)	(0.3538)	(0.5407)	(0.3821)
Observations	28	32	34
Bandwidth (in Days)	±212	±242	±254
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.6085**	1.0435*	-0.0713
Aug. 23, 1996 (Banned)	(0.3063)	(0.5407)	(0.3930)
Observations	32	32	32
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			

Notes: Standard errors clustered at each 15-day bin in parentheses. In this analysis, the outcome variable is the average recidivism rate within each 15-day bin. Also, the average number of days the drug trafficking offenses in a bin were committed before or after Aug. 23, 1996 is the running variable (centered at zero). All models also control for the number of Fridays in each bin. Also, each regression is weighted by the number of offenders in each bin. In Panel A, the Imbens-Kalyanaraman optimal bandwidth (chosen from the micro data pre-aggregation) 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. The coefficients are approximate semi-elasticities. For example, the coefficient in column (1) of Panel B indicates that the ban increased recidivism by approximately 60% from baseline. Column 1 estimates the effect of the SNAP ban on recidivism rates. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A11. Results from Time-Series Analysis of 15-day Bin Averages of Recidivism

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After Aug. 23, 1996 (Banned)	0.1131**	0.1191***	-0.0090
	(0.0504)	(0.0409)	(0.0273)
Control Group Mean	0.1609	0.0880	0.0761
Observations	28	32	34
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)	0.1133**	0.1191***	-0.0031
	(0.0441)	(0.0409)	(0.0276)
Control Group Mean	0.1644	0.0880	0.0764
Observations	32	32	32
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1

Notes: Standard errors clustered at each 15-day bin in parentheses. Each regression includes one lag of the dependent variable (number of lags chosen based on model with highest AIC). In this analysis, the outcome variable is the average recidivism rate within each 15-day bin. Also, the average number of days the drug trafficking offenses in a bin were committed before or after Aug. 23, 1996 is the running variable (centered at zero). All models also control for the number of Fridays in each bin. In Panel A, the Imbens-Kalyanaraman optimal bandwidth (chosen from the micro data pre-aggregation) 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. The coefficients are approximate semi-elasticities. For example, the coefficient in column (1) of Panel B indicates that the ban increased recidivism by approximately 60% from baseline. Column 1 estimates the effect of the SNAP ban on recidivism rates. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A12. Results from Time-Series Analysis of 15-day Bin Counts of Recidivism, Poisson Model

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.8792**	1.1973**	-0.1017
Aug. 23, 1996 (Banned)	(0.3898)	(0.4819)	(0.4455)
Observations	28	32	34
Bandwidth (in Days)	±212	±242	±254
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.7767**	1.1973**	0.0025
Aug. 23, 1996 (Banned)	(0.3237)	(0.4819)	(0.4623)
Observations	32	32	32
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			

Notes: Standard errors clustered at each 15-day bin in parentheses. Each regression includes one lag of the dependent variable (number of lags chosen based on model with highest AIC). The Stata command **arpois** is used to estimate this time-series Poisson model as illustrated in Schwartz et al. (1996). In this analysis, the outcome variable is the average recidivism rate within each 15-day bin. Also, the average number of days the drug trafficking offenses in a bin were committed before or after Aug. 23, 1996 is the running variable (centered at zero). All models also control for the number of Fridays in each bin. In Panel A, the Imbens-Kalyanaraman optimal bandwidth (chosen from the micro data pre-aggregation) 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. The coefficients are approximate semi-elasticities. For example, the coefficient in column (1) of Panel B indicates that the ban increased recidivism by approximately 60% from baseline. Column 1 estimates the effect of the SNAP ban on recidivism rates. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. As part of the time-series analysis, I conduct a Wald test for a known structural break at Aug. 23, 1996 and I reject the null that there is no break in the data. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A13. Effect of the SNAP Ban Robust to
Alternative Optimal Bandwidths

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Calonico, Cattaneo, Titiunik (CCT) Bandwidth			
Offense Committed After	0.1454**	0.1458***	0.0462
Aug. 23, 1996 (Banned)	(0.0604)	(0.0539)	(0.0422)
Control Group Mean	0.1477	0.0605	0.0802
Observations	520	471	423
Bandwidth (in Days)	±139	±126	±111
Panel B. Half the Imbens, Kalyanaraman (IK) Bandwidth			
Offense Committed After	0.1678**	0.1454***	0.0281
Aug. 23, 1996 (Banned)	(0.0694)	(0.0545)	(0.0377)
Control Group Mean	0.1348	0.0613	0.0783
Observations	405	465	475
Bandwidth (in Days)	±106	±121	±127
Panel C. Ludwig, Miller Cross-Validation (CV) Bandwidth			
Offense Committed After	0.0616	0.0813**	-0.0196
Aug. 23, 1996 (Banned)	(0.0407)	(0.0341)	(0.0256)
Control Group Mean	0.1617	0.0887	0.0730
Observations	1252	1252	1252
Bandwidth (in Days)	±325	±325	±325

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about the running variable. In Panel A, the CCT optimal bandwidth is used with polynomial of degree one and a uniform kernel. In Panel B, the IK optimal bandwidth multiplied by one-half is used with polynomial of degree one and a uniform kernel. In Panel C, the CV optimal bandwidth is used with a polynomial of degree one and uniform kernel. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A14. Effect of the SNAP Ban Robust to Alternative Polynomials

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Imbens Kalyanaraman Optimal Bandwidth						
Offense Committed After	0.1249***	0.1379***	0.0032	0.1523**	0.1141*	0.0022
Aug. 23, 1996 (Banned)	(0.0483)	(0.0459)	(0.0320)	(0.0681)	(0.0674)	(0.0488)
Control Group Mean	0.1612	0.0884	0.0728	0.1612	0.0884	0.0728
Observations	2549	1549	1509	1813	1280	1259
Bandwidth (in Days)	±938	±451	±433	±583	±336	±326
Degree of Polynomial in Days from Aug. 23, 1996	2	2	2	3	3	3
Panel B. Consistent Bandwidth of ±240 Days						
Offense Committed After	0.1344*	0.1420**	-0.0076	0.1461	0.0971	0.0490
Aug. 23, 1996 (Banned)	(0.0703)	(0.0617)	(0.0414)	(0.0896)	(0.0784)	(0.0610)
Control Group Mean	0.1644	0.0880	0.0764	0.1644	0.0880	0.0764
Observations	918	918	918	916	916	916
Bandwidth (in Days)	±240	±240	±240	±240	±240	±240
Degree of Polynomial in Days from Aug. 23, 1996	2	2	2	3	3	3

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about the running variable. In Panel A, the Imbens-Kalyanaraman optimal bandwidth is used with polynomials of degree two and three and a uniform kernel. In Panel B, a bandwidth of ±240 days (or ±8 months) is used with polynomials of degree two (columns 1-3) and three (columns 4-6) and a uniform kernel. Columns 1 & 4 estimate the effect of being banned from SNAP after release on whether or not the offender returns to prison after being released. Columns 2 & 5 and Columns 3 & 6 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A15. Effect of the SNAP Ban Robust to Alternative Kernels

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Imbens Kalyanaraman Optimal Bandwidth						
Offense Committed After	0.1061**	0.1069***	-0.0093	0.1046**	0.1077***	-0.0109
Aug. 23, 1996 (Banned)	(0.0483)	(0.0369)	(0.0285)	(0.0486)	(0.0372)	(0.0290)
Control Group Mean	0.1626	0.0904	0.0732	0.1614	0.0879	0.0733
Observations	1042	1201	1250	967	1109	1180
Bandwidth (in Days)	±270	±309	±324	±251	±287	±301
Kernel	Triangle	Triangle	Triangle	Epanechnikov	Epanechnikov	Epanechnikov
Panel B. Consistent Bandwidth of ±240 Days						
Offense Committed After	0.1108**	0.1164***	-0.0056	0.1064**	0.1156***	-0.0092
Aug. 23, 1996 (Banned)	(0.0513)	(0.0420)	(0.0324)	(0.0498)	(0.0408)	(0.0316)
Control Group Mean	0.1644	0.0880	0.0764	0.1644	0.0880	0.0764
Observations	918	918	918	918	918	918
Bandwidth (in Days)	±240	±240	±240	±240	±240	±240
Kernel	Triangle	Triangle	Triangle	Epanechnikov	Epanechnikov	Epanechnikov

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about the running variable. In Panel A, the Imbens-Kalyanaraman optimal bandwidth is used with polynomial of degree one and two kernels: (1) triangle (columns 1-3) and (2) Epanechnikov (columns 4-6). In Panel B, a bandwidth of ±240 days (or ±8 months) is used with polynomial of degree one and two kernels: (1) triangle and (2) Epanechnikov. Columns 1 & 4 estimate the effect of being banned from SNAP after release on whether or not the offender returns to prison after being released. Columns 2 & 5 and Columns 3 & 6 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A16. Effect of SNAP Ban on Offenders Released During High Unemployment Months

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0442	0.0413	0.0509
Aug. 23, 1996 (Banned)	(0.1100)	(0.0839)	(0.0626)
Unemployment Rate (UR)	-0.0189*	-0.0161**	0.0002
	(0.0113)	(0.0066)	(0.0084)
UR X Banned	0.0070	0.0135	-0.0132
	(0.0198)	(0.0149)	(0.0102)
Control Group Mean	0.1587	0.0874	0.0764
Observations	791	936	980
Bandwidth (in Days)	212	242	254
Panel B. Consistent Bandwidth of $\pm$240 Days			
Offense Committed After	0.0831	0.0346	0.0486
Aug. 23, 1996 (Banned)	(0.1019)	(0.0849)	(0.0612)
Unemployment Rate (UR)	-0.0188*	-0.0157**	-0.0031
	(0.0101)	(0.0066)	(0.0079)
UR X Banned	0.0026	0.0142	-0.0116
	(0.0179)	(0.0151)	(0.0099)
Control Group Mean	0.1644	0.0880	0.0764
Observations	918	918	918
Bandwidth (in Days)	$\pm$240	$\pm$240	$\pm$240

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. Column 1 estimates heterogeneity in the effect of being banned from SNAP on whether or not the offender returns to prison after being released by labor market conditions upon release. Column 2 and Column 3 estimate this heterogeneity in the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. Unemployment rate is the state-level unemployment rate in the month of the offender's release. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A17. Effect of SNAP Ban on Black Offenders

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0415	0.0694	-0.0299
Aug. 23, 1996 (Banned)	(0.0649)	(0.0474)	(0.0388)
Black	0.0604	0.0159	0.0327
	(0.0648)	(0.0408)	(0.0471)
Black X Banned	0.0987	0.0799	0.0460
	(0.1051)	(0.0797)	(0.0617)
Combined Effect:	0.1402	0.1492	0.0161
Banned+(Black X Banned)	0.0773	0.0640	0.0445
Control Group Mean	0.1587	0.0874	0.0764
Observations	791	936	980
Bandwidth (in Days)	212	242	254
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.0294	0.0646	-0.0351
Aug. 23, 1996 (Banned)	(0.0602)	(0.0487)	(0.0386)
Black	0.0313	0.0136	0.0177
	(0.0630)	(0.0414)	(0.0483)
Black X Banned	0.1497	0.0846	0.0651
	(0.0976)	(0.0813)	(0.0625)
Combined Effect:	0.1791	0.1491	0.0300
Banned+(Black X Banned)	0.0734	0.0647	0.0461
Control Group Mean	0.1644	0.0880	0.0764
Observations	918	918	918
Bandwidth (in Days)	±240	±240	±240

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. Column 1 estimates heterogeneity by race in the effect of being banned from SNAP on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate heterogeneity by race on the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. The row "Combined Effect: Banned+(Black X Banned)" is the linear combination of the coefficients on "Banned" and "Black X Banned" and represents the total effect of the ban on black offenders. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A18. Effect of SNAP Ban on Timing of Re-Incarceration

Outcome:	Recidivism within 0-5 Years	Financial Recidivism within 0-5 Years	Recidivism within 5-10 Years	Financial Recidivism within 5-10 Years
	(1)	(2)	(3)	(4)
Panel A. Imbens Kalyanaraman Optimal Bandwidth				
Offense Committed After	0.0438	0.0716**	0.0438	0.0308
Aug. 23, 1996 (Banned)	(0.0372)	(0.0301)	(0.0310)	(0.0227)
Control Group Mean	0.1046	0.0536	0.0508	0.0304
Observations	1029	964	721	1042
Bandwidth (in Days)	277	256	219	305
Degree of Polynomial in	1	1	1	1
Days from Aug. 23, 1996				
Panel B. Consistent Bandwidth of ±240 Days				
Offense Committed After	0.0581	0.0685**	0.0259	0.0184
Aug. 23, 1996 (Banned)	(0.0402)	(0.0316)	(0.0296)	(0.0264)
Control Group Mean	0.1071	0.0548	0.0497	0.0260
Observations	893	893	796	801
Bandwidth (in Days)	±240	±240	±240	±240
Degree of Polynomial in	1	1	1	1
Days from Aug. 23, 1996				

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison within 0-5 years of being released. Column 2 estimates the effect on financially motivated recidivism within 0-5 years of being released. Column 3 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison within 5-10 years of being released. Column 4 estimates the effect on financially motivated recidivism within 5-10 years of being released. See Table A3 for a definition of financially motivated crimes and the associated recidivism measure. Time until recidivism is defined as the difference between the offender's release date for prison stay j and the next offense date before prison stay $j + 1$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A19. Effect of Ban when SNAP is Most Generous for Non-Banned Offenders

Outcome:	Recidivism in Time/Place with ABAWD Work Waiver	Recidivism in Time/Place with No ABAWD Work Waiver
	(1)	(2)
Panel A. Imbens Kalyanaraman Optimal Bandwidth		
Offense Committed After	0.0996**	-0.0039
Aug. 23, 1996 (Banned)	(0.0415)	(0.0292)
Control Group Mean	0.0874	0.0761
Observations	936	990
Bandwidth (in Days)	±242	±256
Degree of Polynomial in	1	1
Days from Aug. 23, 1996		
Panel B. Consistent Bandwidth of ±240 Days		
Offense Committed After	0.1037**	-0.0087
Aug. 23, 1996 (Banned)	(0.0418)	(0.0306)
Control Group Mean	0.0880	0.0764
Observations	918	918
Bandwidth (in Days)	±240	±240
Degree of Polynomial in	1	1
Days from Aug. 23, 1996		

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison with a crime that was committed in a time (based on earliest offense date after release) and place (based on county of conviction) where ABAWD work requirements were waived. Column 2 estimates the effect on recidivism with a crime that was committed in a time and place where ABAWD work requirements were in effect. 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: (1) work 20 or more hours per week, (2) participate in an employment and training program, or (3) participate in a workfare program (USDA 2016). Thus, when these requirements are waived, SNAP is especially generous for ABAWDs not subject to the ban. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A20. Effect of Ban when SNAP is Most Generous for Non-Banned Offenders, Using Release Plan Residence

Outcome:	Recidivism in Time/Place with ABAWD Work Waiver	Recidivism in Time/Place with No ABAWD Work Waiver
	(1)	(2)
Panel A. Imbens Kalyanaraman Optimal Bandwidth		
Offense Committed After	0.0997**	0.0002
Aug. 23, 1996 (Banned)	(0.0420)	(0.0317)
Control Group Mean	0.0833	0.0797
Observations	918	997
Bandwidth (in Days)	±240	±258
Degree of Polynomial in	1	1
Days from Aug. 23, 1996		
Panel B. Consistent Bandwidth of ±240 Days		
Offense Committed After	0.0997**	-0.0048
Aug. 23, 1996 (Banned)	(0.0420)	(0.0335)
Control Group Mean	0.0833	0.0810
Observations	918	918
Bandwidth (in Days)	±240	±240
Degree of Polynomial in	1	1
Days from Aug. 23, 1996		

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison with a crime that was committed in a time (based on earliest offense date after release) and place (based on county of residence on release plan) where ABAWD work requirements were waived. Column 2 estimates the effect on recidivism with a crime that was committed in a time and place where ABAWD work requirements were in effect. See Table A19 for more information about the ABAWD work requirement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A21. Effect of SNAP Ban on Offenders When ABAWD Work Requirements Waived, Hazard Model with Year Effects

Outcome:	Recidivism	
	(1)	(2)
Panel A. Imbens Kalyanaraman Optimal Bandwidth		
Offense Committed After	0.5680*	-0.7222
Aug. 23, 1996 (Banned)	(0.3413)	(0.8467)
Banned X ABAWD Waiver		1.6465*
		(0.9752)
Combined Effect:		0.9243**
Banned + (Banned X Waiver)		0.4105
Observations	117441	117441
Bandwidth (in Days)	±212	±212
Panel B. Consistent Bandwidth of ±240 Days		
Offense Committed After	0.6499**	-0.7483
Aug. 23, 1996 (Banned)	(0.3184)	(0.7009)
Banned X ABAWD Waiver		1.8310**
		(0.8301)
Combined Effect:		1.0827***
Banned + (Banned X Waiver)		0.3909
Observations	135733	135733
Bandwidth (in Days)	±240	±240

Notes: Standard errors clustered at the day of offense in parentheses. This analysis uses a Cox survival model in which offenders enter when they are released from prison and exit when they return to prison. Since the analysis includes time-varying covariates, the data was transformed to a format where every row is an offender-month-year observation for the time that they are out of prison. All specifications include year fixed effects. 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 returns to prison. Column 2 estimates heterogeneity in the effect by whether or not the offender is living in a county where ABAWD work requirements are waived. In Panel A, the Imbens-Kalyanaraman optimal bandwidth (chosen from the micro data pre-transformation) 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. See Table A19 for more information about the ABAWD work requirement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

<div align="center">

Table A22. Placebo Test: Recidivism for
Sell/Mfg/Dist Drug Offenders (Not Banned)

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Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0109	-0.0235	0.0366
Aug. 23, 1996 (Banned)	(0.0294)	(0.0244)	(0.0255)
Control Group Mean	0.5534	0.3473	0.1934
Observations	4903	6103	3925
Bandwidth (in Days)	±302	±412	±239
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.0056	-0.0313	0.0369
Aug. 23, 1996 (Banned)	(0.0326)	(0.0304)	(0.0254)
Control Group Mean	0.5510	0.3577	0.1933
Observations	3934	3934	3934
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths. Number of days the selling/manufacturing/distributing drugs (SMD) offense was committed before or after Aug. 23, 1996 is the running variable (centered at zero). Column 1 estimates the effect of committing an SMD offense on or after Aug. 23, 1996 on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. SMD offenses are not subject to the SNAP ban, and thus, committing one before versus after the cutoff date should not affect an individual's recidivism. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A23. Placebo Test: Recidivism for
Non-Drug Offenders (Not Banned)

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0088	0.0065	-0.0002
Aug. 23, 1996 (Banned)	(0.0143)	(0.0126)	(0.0092)
Control Group Mean	0.3930	0.2425	0.1505
Observations	26375	29232	21928
Bandwidth (in Days)	±506	±595	±373
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.0062	0.0072	-0.0010
Aug. 23, 1996 (Banned)	(0.0187)	(0.0164)	(0.0109)
Control Group Mean	0.3933	0.2427	0.1506
Observations	15166	15166	15166
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 in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths. Number of days the non-drug offense was committed before or after Aug. 23, 1996 is the running variable (centered at zero). Column 1 estimates the effect of committing an SMD offense on or after Aug. 23, 1996 on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. Non-drug offenses are not subject to the SNAP ban, and thus, committing one before versus after the cutoff date should not affect an individual's recidivism. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A24. Additional Placebo Tests: Recidivism Outcomes for Other (Not Banned) Offenders

Outcome:	Recidivism				
Offender Type:	**All Non-Drug Offenders**	**DUI & Revoked License**	**Drug Possession**	**Property Crime**	**Violent Crime**
	(1)	(2)	(3)	(4)	(5)
Panel A. Imbens Kalyanaraman Optimal Bandwidth					
Offense Committed After	0.0088	-0.0669	0.0040	-0.0238	-0.0085
Aug. 23, 1996 (Banned)	(0.0143)	(0.0756)	(0.0278)	(0.0195)	(0.0259)
Control Group Mean	0.3930	0.4264	0.5613	0.4756	0.3418
Observations	26375	798	5254	10523	7906
Bandwidth (in Days)	505	177	249	234	238
Degree of Polynomial in	1	1	1	1	1
Days from Aug. 23, 1996					
Panel B. Consistent Bandwidth of ±240 Days					
Offense Committed After	0.0062	-0.0560	0.0082	-0.0225	-0.0085
Aug. 23, 1996 (Banned)	(0.0187)	(0.0648)	(0.0284)	(0.0191)	(0.0257)
Control Group Mean	0.3933	0.4077	0.5619	0.4756	0.3417
Observations	15166	1092	5103	10785	7965
Bandwidth (in Days)	±240	±240	±240	±240	±240
Degree of Polynomial in	1	1	1	1	1
Days from Aug. 23, 1996					

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths. Number of days the placebo offense was committed before or after Aug. 23, 1996 is the running variable (centered at zero). Column 1 estimates the effect of committing any non-drug offense after Aug 23, 1996 on recidivism. Column 2 estimates the effect of committing a DUI or driving with a revoked license after Aug 23, 1996. Column 3 estimates the effect of committing drug possession after Aug 23, 1996. Column 4 estimates the effect of committing a property crime after Aug. 23, 1996. Column 5 estimates the effect of committing a violent crime after Aug 23, 1996. None of these offenses are subject to the SNAP ban, and thus, committing one before versus after the cutoff date should not affect an individual's recidivism. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A25. Effect of the SNAP Ban on Recidivism with Seasonal Controls

Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Offense Committed After Aug. 23, 1996 (Banned)	0.0986* (0.0532)	0.1046** (0.0468)	-0.0060 (0.0316)
Offense Committed After Any Aug. 23 1996-2012	0.0040 (0.0094)	0.0042 (0.0086)	-0.0002 (0.0062)
Control Group Mean	0.1587	0.0825	0.0762
Observations	16519	16519	16519
Bandwidth (in Days)	±180	±180	±180
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1

Notes: Standard errors clustered at the day of offense in parentheses. Number of days the drug trafficking offense was committed before or after Aug. 23 in a given year is the running variable (centered at zero). Specifically, I estimate both a "seasonality effect" and a "true effect" of the ban, where the seasonality effect is the effect of committing a drug trafficking offense after Aug. 23 in general and the true effect is the effect of committing a drug trafficking offense after Aug. 23, 1996. In all specifications a bandwidth of ±180 days is used to avoid overlapping observations across years. Also, this estimation excludes the years 1998 and 1999 since those are two years in which Florida implemented criminal justice policies that would directly affect drug traffickers. Column 1 estimates the effect of being banned from SNAP on whether or not the offender ever returns to prison. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A26. Test of Deterrence Hypothesis: Effect of Ban on Type of Financially Motivated Recidivism

Outcome:	Recidivism for Drug Trafficking Crime	Recidivism for Non-Drug Trafficking Crime	Recidivism for Property Crime	Recidivism for Theft
	(1)	(2)	(3)	(4)
Panel A. Imbens Kalyanaraman Optimal Bandwidth				
Offense Committed After	0.0197	0.0919**	0.0415**	0.0549***
Aug. 23, 1996 (Banned)	(0.0161)	(0.0356)	(0.0192)	(0.0165)
Control Group Mean	0.0312	0.0621	0.0212	0.0123
Observations	1452	940	1232	1048
Bandwidth (in Days)	±411	±244	±317	±275
Degree of Polynomial in	1	1	1	1
Days from Aug. 23, 1996				
Panel B. Consistent Bandwidth of ±240 Days				
Offense Committed After	0.0110	0.0892**	0.0526**	0.0586***
Aug. 23, 1996 (Banned)	(0.0181)	(0.0363)	(0.0218)	(0.0174)
Control Group Mean	0.0255	0.0625	0.0208	0.0116
Observations	918	918	918	918
Bandwidth (in Days)	±240	±240	±240	±240
Degree of Polynomial in	1	1	1	1
Days from Aug. 23, 1996				

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths. 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 returns to prison due to a drug trafficking crime. Column 2 estimates whether or not the offender returns to prison due to a financially motivated crime that is **not** drug trafficking. Column 3 estimates the effect on recidivism due to a property crime, and column 4 estimates the effect on recidivism due to theft. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A27. Effect of SNAP Ban on Recidivism for Crimes in Offender's History, Not in Offender's History, and Total Crimes

Outcome:	Recidivism with Only Crimes Not Convicted of Previously	Recidivism with a Crime Convicted of Previously	Total # of Crimes After Trafficking Conviction
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	-0.0071	0.1168**	0.3195**
Aug. 23, 1996 (Banned)	(0.0064)	(0.0504)	(0.1522)
Control Group Mean	0.0018	0.1600	0.3943
Observations	1225	735	735
Bandwidth (in Days)	±314	±197	±197
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	-0.0118	0.1067**	0.2464*
Aug. 23, 1996 (Banned)	(0.0087)	(0.0467)	(0.1374)
Control Group Mean	0.0023	0.1620	0.3866
Observations	918	918	918
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1
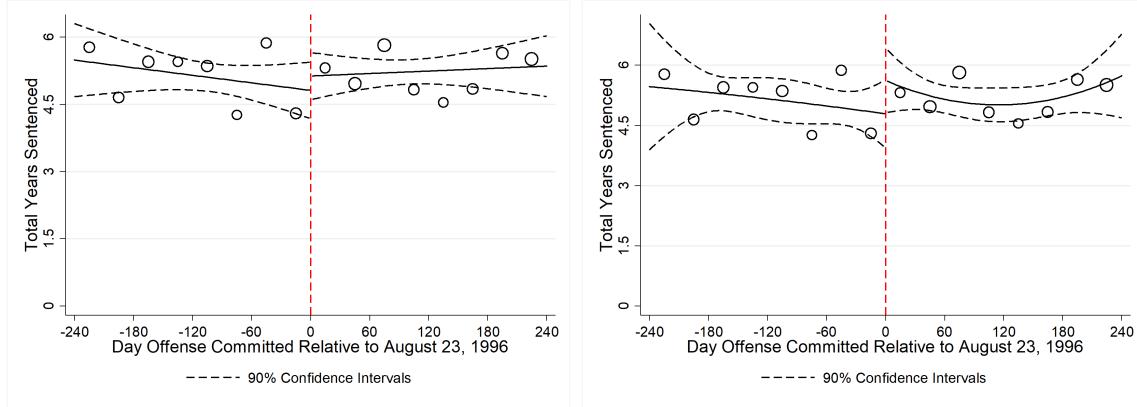
Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths and the running variable. Column 1 estimates the effect of being banned from SNAP after release on whether or not the offender returns to prison exclusively due to a crime that they have not committed before. Column 2 estimates the effect of being banned from SNAP on whether or not the offender returns to prison with a crime that they have committed before. Column 3 estimates the effect of being banned from SNAP on the total number of crimes the offender is convicted of in the future. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A28. Effect of SNAP Ban on Recidivism in Florida,
Mis-Measuring Treatment by Using Conviction Date
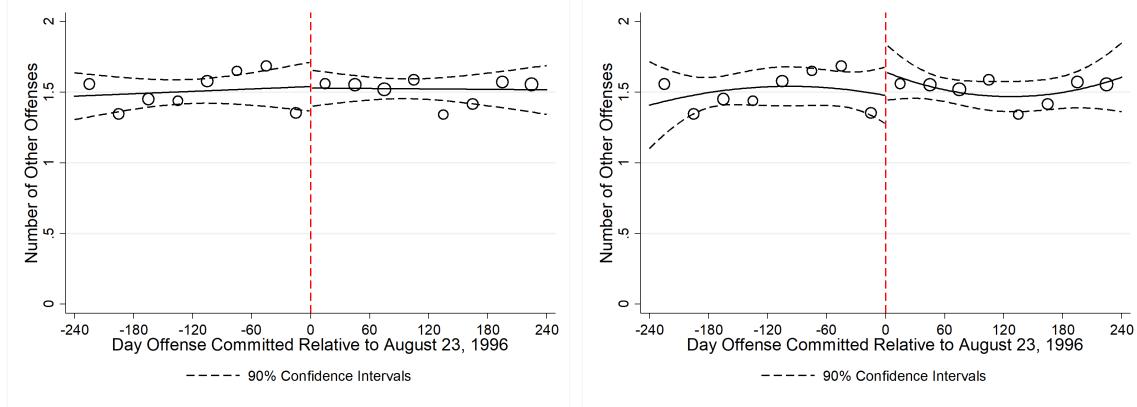
Outcome:	Recidivism	Financially Motivated Recidivism	Non-Financially Motivated Recidivism
	(1)	(2)	(3)
Panel A. Imbens Kalyanaraman Optimal Bandwidth			
Offense Committed After	0.0195	-0.0599	0.0876**
Aug. 23, 1996 (Banned)	(0.0651)	(0.0423)	(0.0409)
Control Group Mean	0.1545	0.1048	0.0488
Observations	733	1147	702
Bandwidth (in Days)	±452	±687	±433
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After	0.1136	-0.0223	0.1359**
Aug. 23, 1996 (Banned)	(0.0791)	(0.0609)	(0.0552)
Control Group Mean	0.1570	0.1074	0.0496
Observations	387	387	387
Bandwidth (in Days)	±240	±240	±240
Degree of Polynomial in	1	1	1
Days from Aug. 23, 1996			

Notes: Standard errors clustered at the day of offense in parentheses. See Table A2 for general notes about the RD estimation, including information about bandwidths. Number of days the drug trafficker was convicted before or after Aug. 23, 1996 is the running variable (centered at zero). Column 1 estimates the effect of being banned from SNAP on whether or not the offender returns to prison after being released. Column 2 and Column 3 estimate the effect on recidivism with financially motivated crimes and the effect on recidivism with non-financially motivated crimes, respectively. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures. Since the ban is determined based on the date the drug trafficking offense is committed, estimating the effect based on date of conviction introduces measurement error into the model. Conviction dates are often months or years after the offense date. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Visual Evidence that Regression Discontinuity Identifying Assumption Holds
**Figure A1a. No Sorting Near Cutoff in Total Years Sentenced**
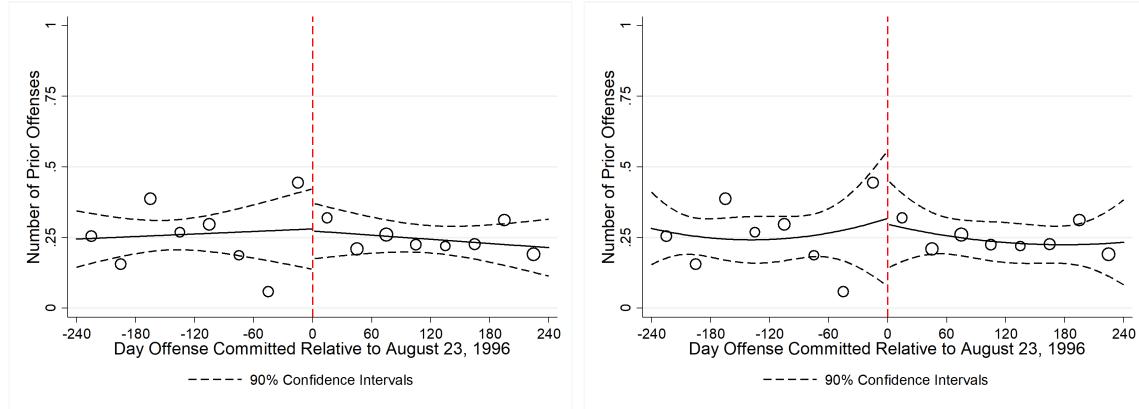

Figure A1b. No Sorting Near Cutoff in # of Concurrent Offenses
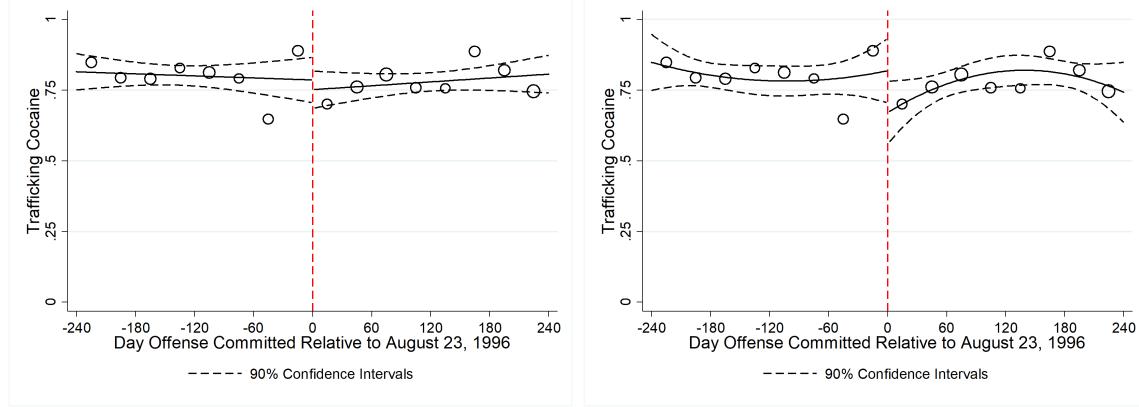
Notes: The figures in the first column display the lines from two local linear regressions, estimated separately on each side of the cutoff using the offense-level micro data. The figures in the second column display the lines from two local quadratic regressions, estimated separately on each side of 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 running variable in these figures (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. The dependent variables in these figures are offender characteristics: total years sentenced and number of concurrent offenses.

Visual Evidence that Regression Discontinuity Identifying Assumption Holds
**Figure A1c. No Sorting Near Cutoff in # of Prior Offenses**
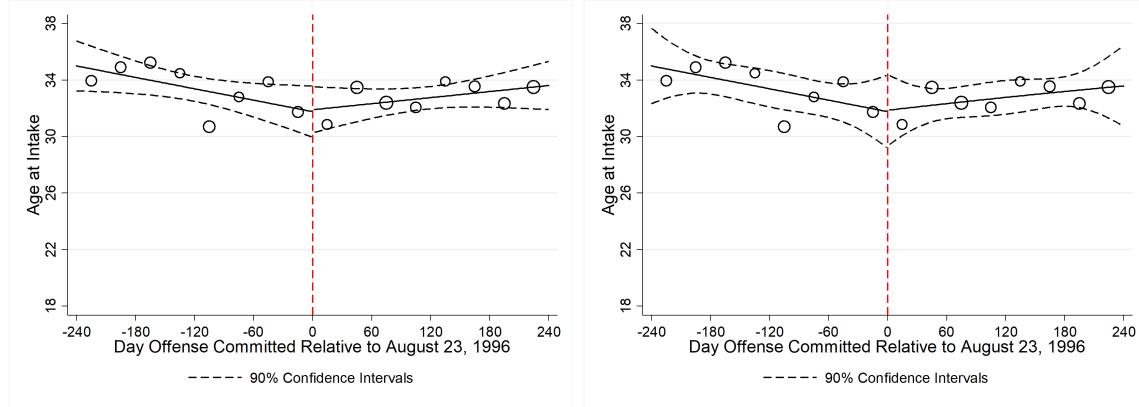
Figure A1d. No Sorting Near Cutoff in the Type of Trafficking Offense
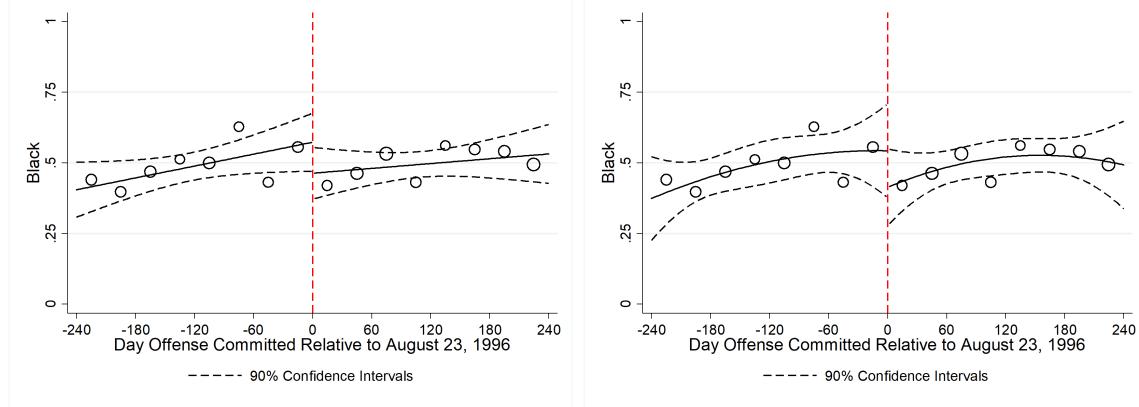
Notes: See notes from Figures A1a-A1b. The dependent variables in these figures are offender characteristics: number of prior offenses and type of trafficking.

Visual Evidence that Regression Discontinuity Identifying Assumption Holds
Figure A1e. No Sorting Near Cutoff in Offender Age at Intake
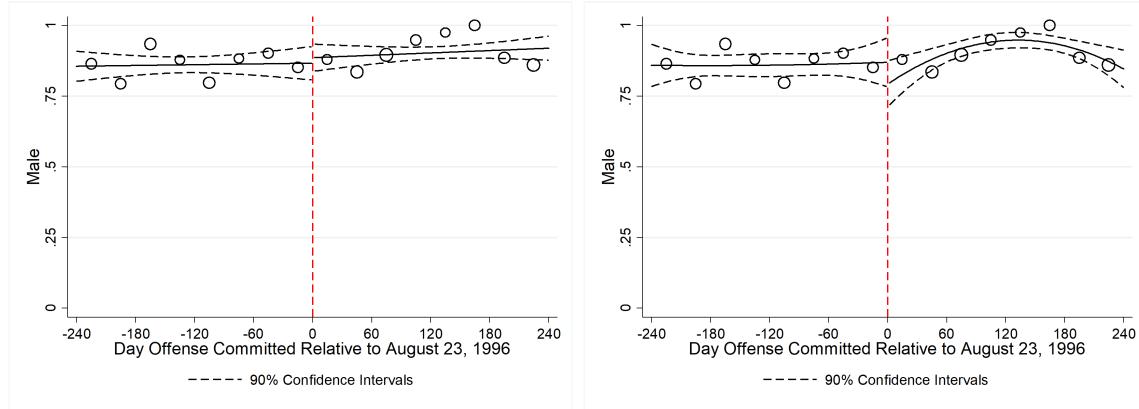
Figure A1f. No Sorting Near Cutoff in Offender's Race
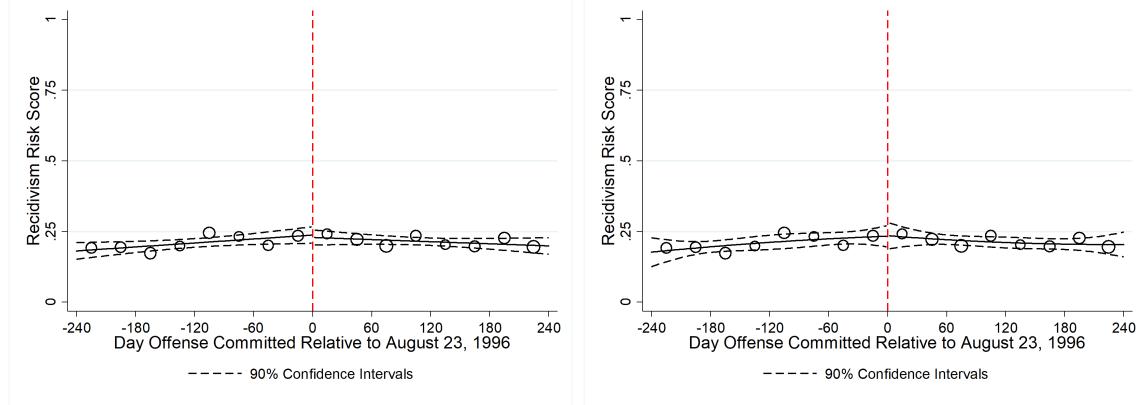
Notes: See notes from Figures A1a-A1b. The dependent variables in these figures are offender characteristics: age at intake and race, and risk of recidivism.

Visual Evidence that Regression Discontinuity Identifying Assumption Holds
Figure A1g. No Sorting Near Cutoff in Offender's Sex
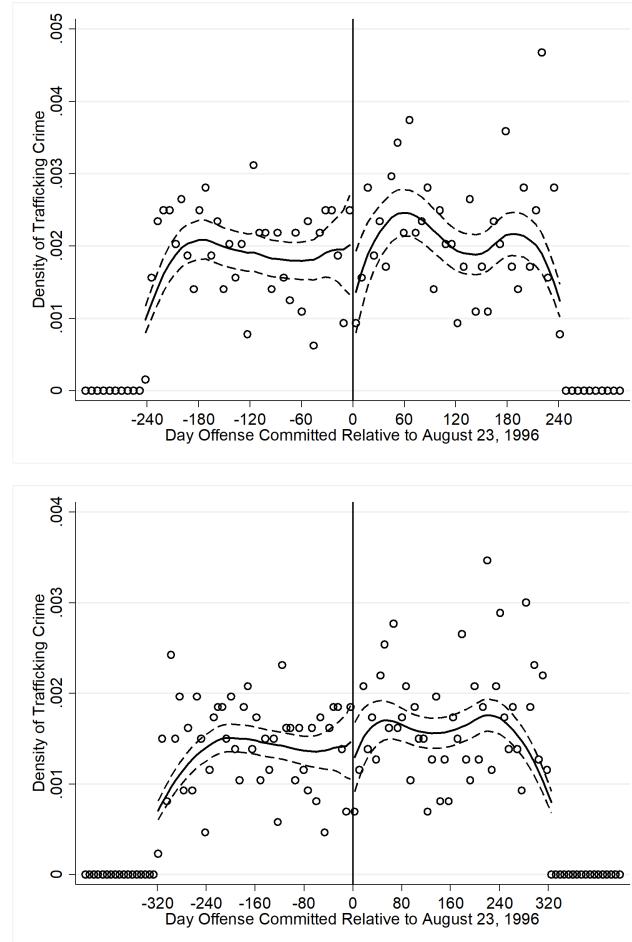
Figure A1h. No Sorting Near Cutoff in Offender's Risk of Recidivism
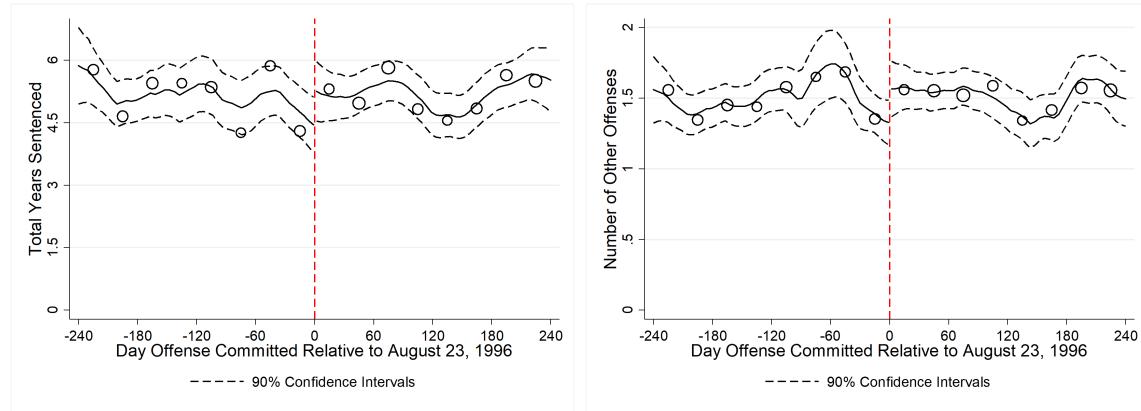
Notes: See notes from Figures A1a-A1b. The dependent variables in these figures are offender characteristics: sex and risk of recidivism. See Figure 1 or Table A2 for notes about the calculation of risk of recidivism.

Visual Evidence that Regression Discontinuity Identifying Assumption Holds
Figure A2. No Break in the Density of Drug Trafficking Crime Near August 23, 1996
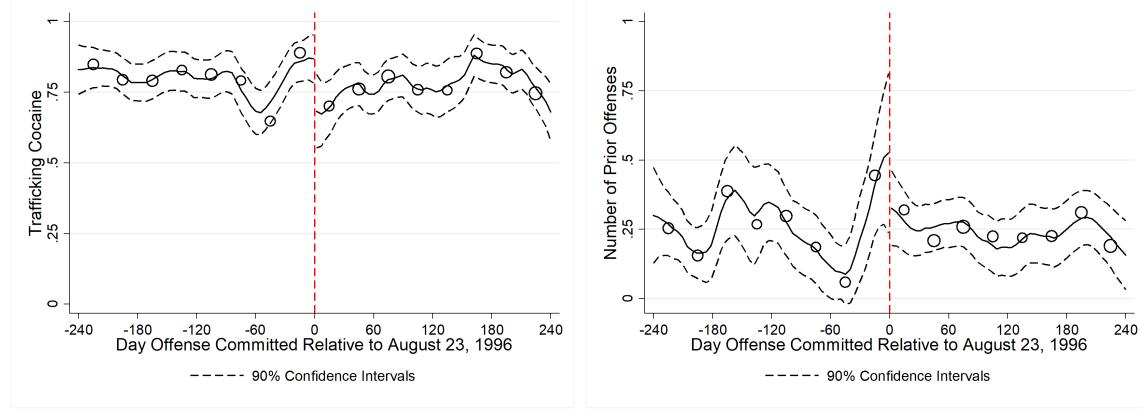
Notes: Both figures display the density of drug trafficking crime on each day in a narrow band around August 23, 1996. The figure the first row shows this for a bandwidth of 240 days before and after August 23, 1996 while the figure in the second row shows this for bandwidth of 320 days before and after August 23, 1996. Neither figure shows a statistical break in the density of drug trafficking crimes near the cutoff date—this is further evidence against endogenous sorting. I use the Stata program **DCDensity.ado** provided by Justin McCrary and Brian Kovak to conduct this test.

Non-parametric Visual Evidence that Regression Discontinuity Identifying Assumption Holds

Figure A3a. No Sorting in Years Sentenced

Figure A3b. No Sorting in # of Concurrent Offenses
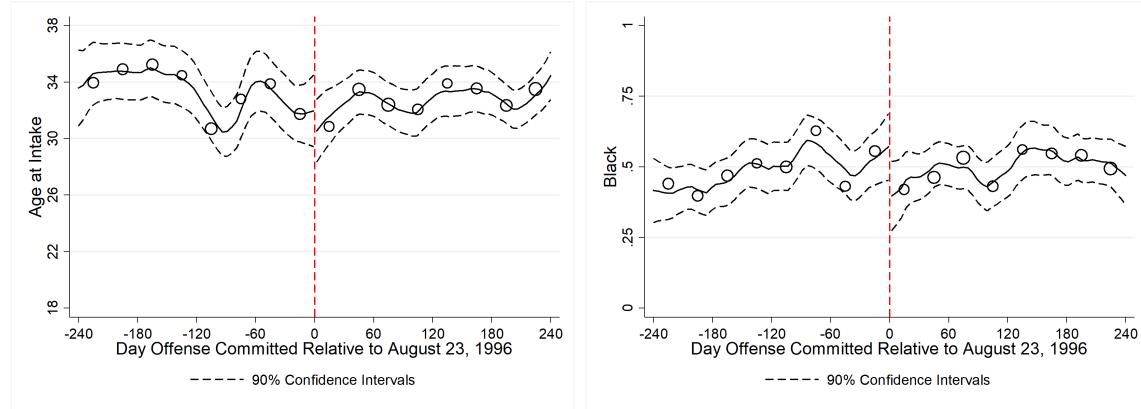
Figure A3c. No Sorting in Type of Trafficking

Figure A3d. No Sorting in # of Prior Offenses
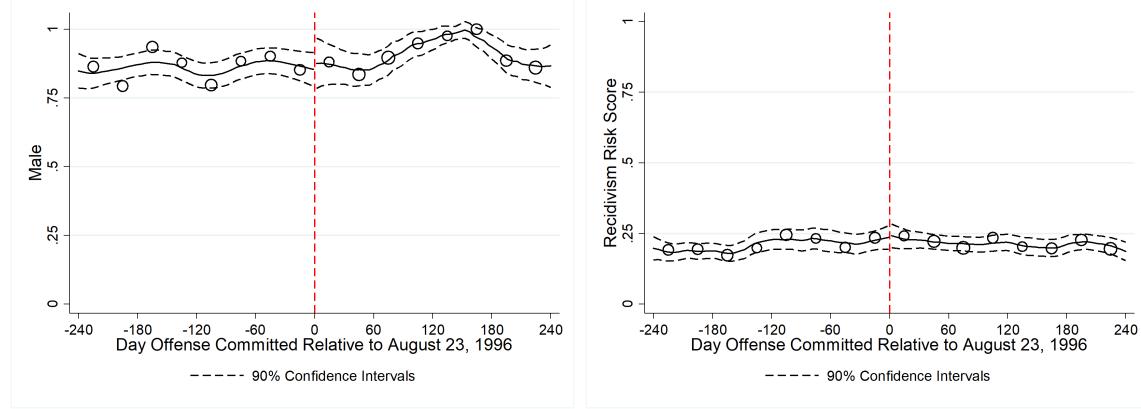
Notes: The figures above display the lines from two locally smoothed regressions, estimated separately on each side of the cutoff using the offense-level micro data. I also overlay a scatter plot of 30-day bin averages of the dependent variable weighted by the number of offenses in each 30-day bin. See Figures A1a-A1b for notes about the running variable. The dependent variables in these figures are offender characteristics: total years sentenced, number of concurrent offenses, number of prior offenses, and type of trafficking. All figures are made with Stata command **lpolyci** using the default settings.

Non-parametric Visual Evidence that Regression Discontinuity Identifying Assumption Holds

Figure A3e. No Sorting in Age at Intake

90% Confidence Intervals

Figure A3f. No Sorting in Race

90% Confidence Intervals
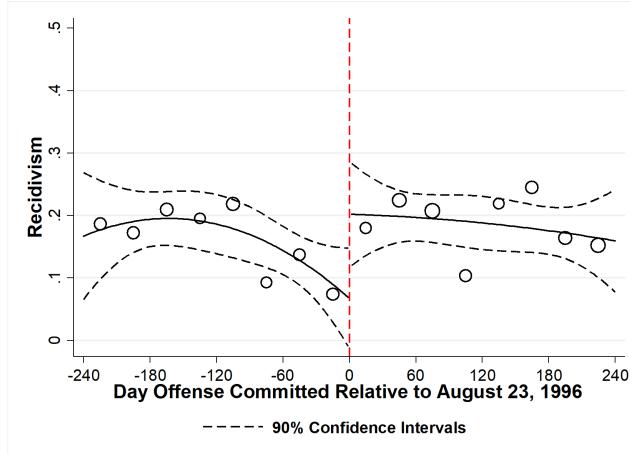
Figure A3g. No Sorting in Sex

90% Confidence Intervals

Figure A3h. No Sorting in Risk of Recidivism

90% Confidence Intervals
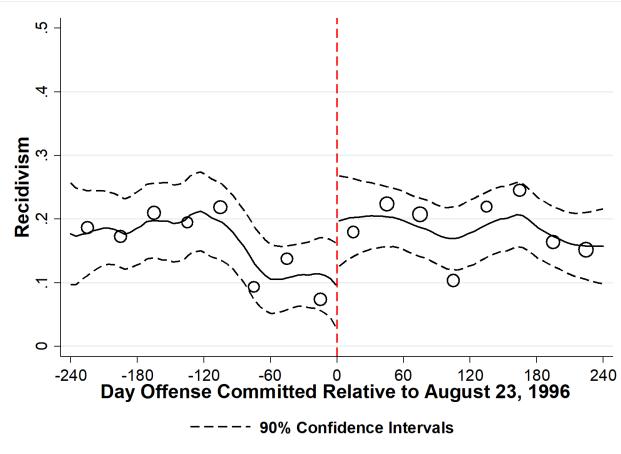
Notes: See the notes for Figures A3a-A3d. The dependent variables in these figures are offender characteristics: age at intake, race, sex, and risk of recidivism. See Figure 1 or Table A2 for notes on how risk of recidivism is calculated.

Visual Evidence of Main Result: Offenders Subject to SNAP Ban are More Likely to Recidivate
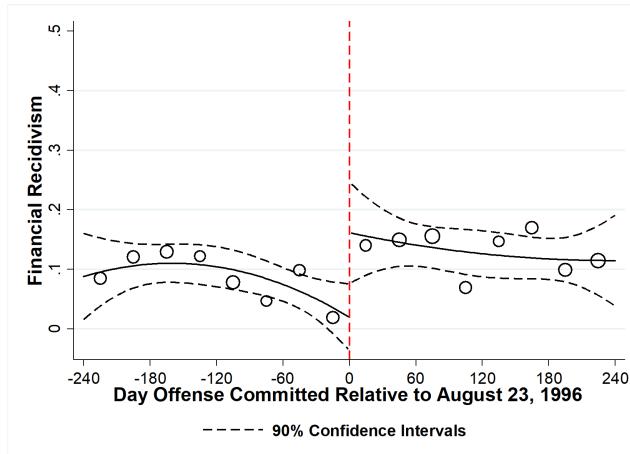
Figure A4a. Any Recidivism, Quadratic

90% Confidence Intervals
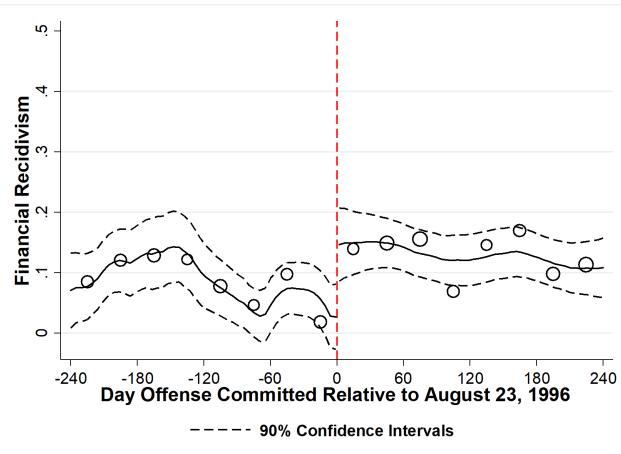
Figure A4b. Any Recidivism, Nonparametric

90% Confidence Intervals
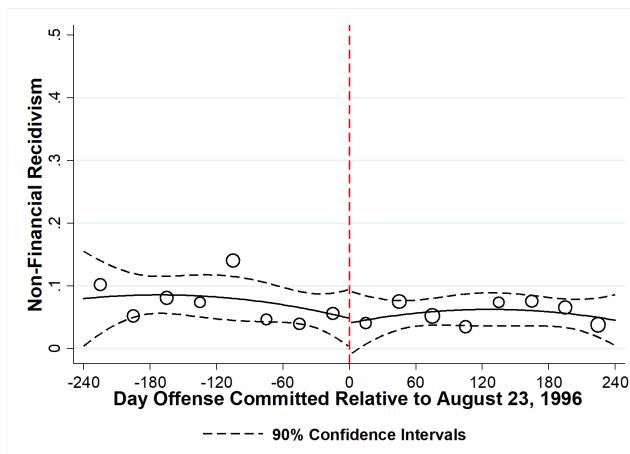
Figure A4c. Financial Recidivism, Quadratic

90% Confidence Intervals
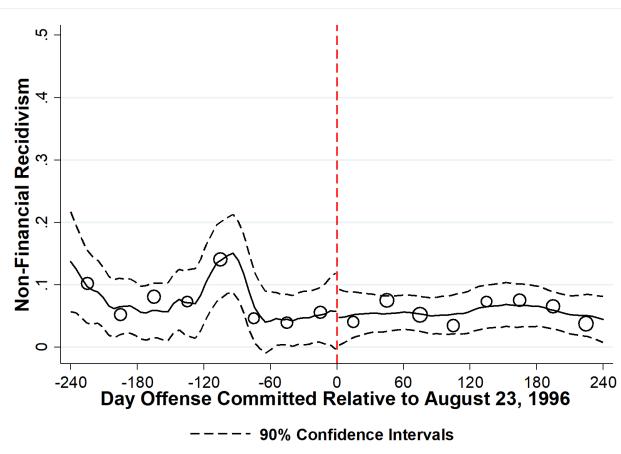
Figure A4d. Financial Recidivism, Nonparametric

90% Confidence Intervals

Figure A4e. Non-Financial Recidivism, Quadratic

90% Confidence Intervals
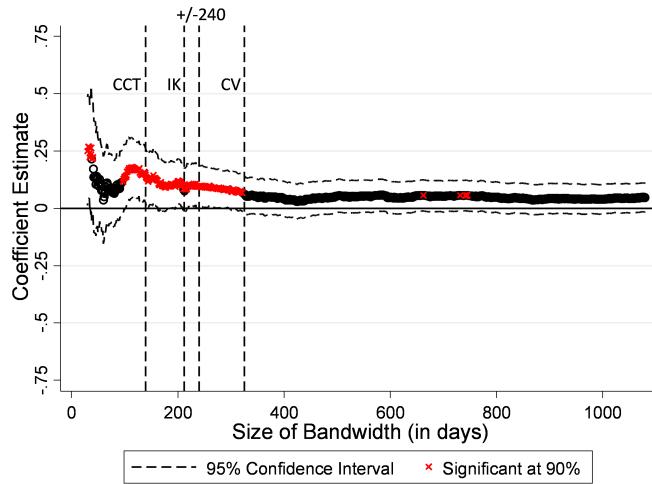
Figure A4f. Non-Financial Recidivism, Nonparametric
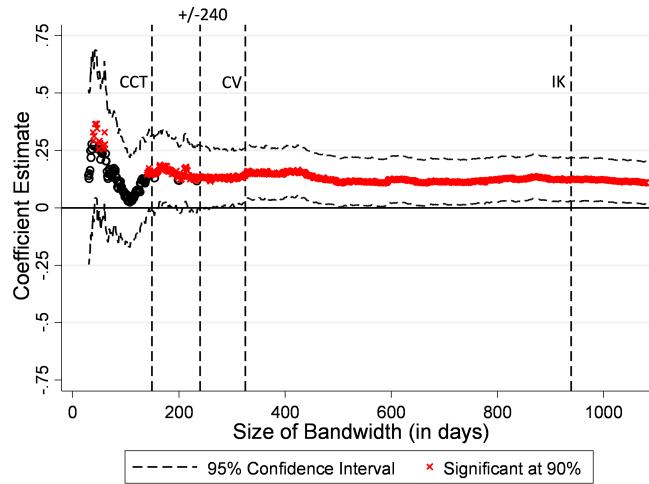
90% Confidence Intervals

Notes: The figures in the first column display the lines from two local quadratic regressions, estimated separately on each side of the cutoff using the offense-level micro data. The figures in the second column display the lines from two locally smoothed 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. See Figures A1a-A1b for notes about the running variable. The dependent variables in these figures are offender outcomes: recidivism, financial recidivism, and non-financial recidivism. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures.

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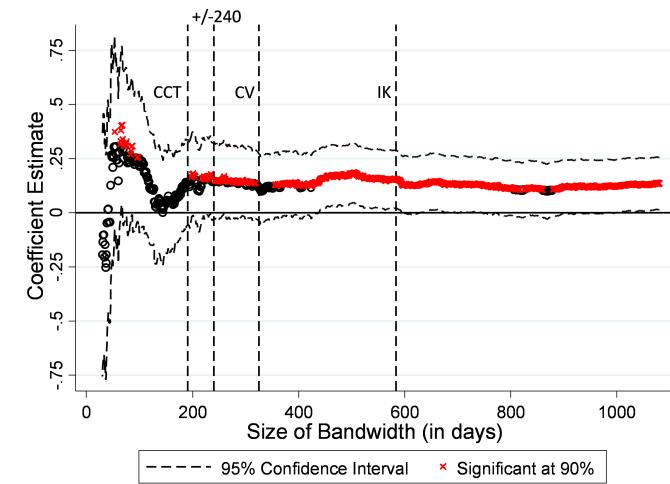
Figure A5a. Estimate of Effect over Many Bandwidths, Linear Polynomial

Figure A5b. Estimate of Effect over Many Bandwidths, Quadratic Polynomial
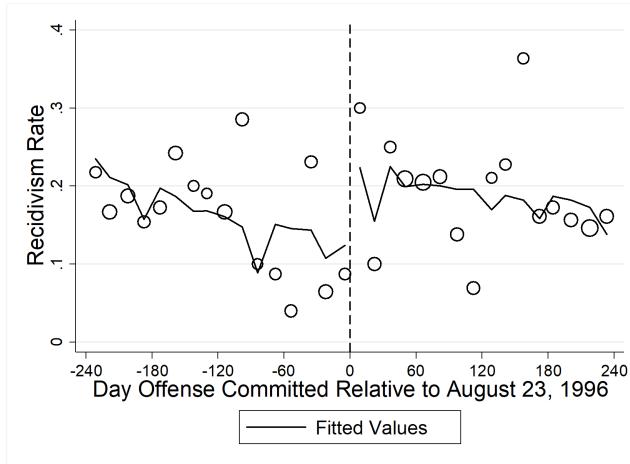
**Figure A5c. Estimate of Effect over Many Bandwidths, Cubic Polynomial**
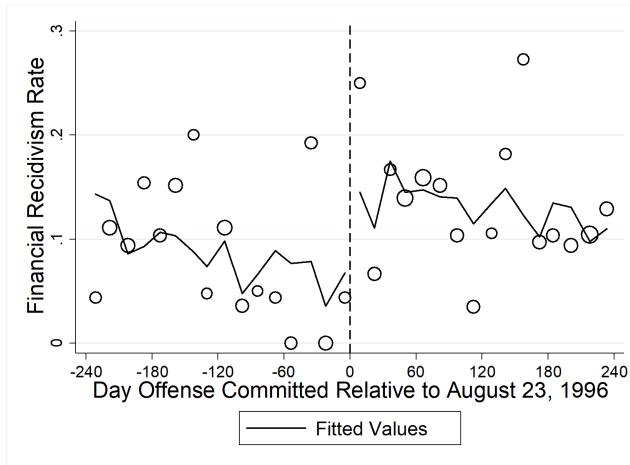
Notes: The figures above display the coefficient estimates from regressions with bandwidths ranging from ±30 days from August 23, 1996 to ±1080 days from August 23, 1996. The coefficient estimate is plotted on the y-axis and the corresponding bandwidth that yields that coefficient is plotted on the x-axis. Each figure includes four vertical lines denoting the Calonico, Cattaneo, Titiunik (CCT) optimal bandwidth, the Ludwig, Miller Cross-Validation (CV) optimal bandwidth, the Imbens, Kalyanaraman (IK) optimal bandwidth, and the consistent ±240 day bandwidth used throughout the paper. In Figure 2a, the regressions include a linear polynomial of the running variable. In Figure 2b, the regressions include a quadratic polynomial of the running variable. In Figure 2c, the regressions include a cubic polynomial of the running variable. 95% confidence intervals are plotted and coefficients are marked red when significant at the 90% level. Bandwidths greater than ±327 days are asymmetric since the data only includes offenses occurring after October 1, 1995.

Visual Evidence of Time-Series Result: Offenders Subject to SNAP Ban are More Likely to Recidivate
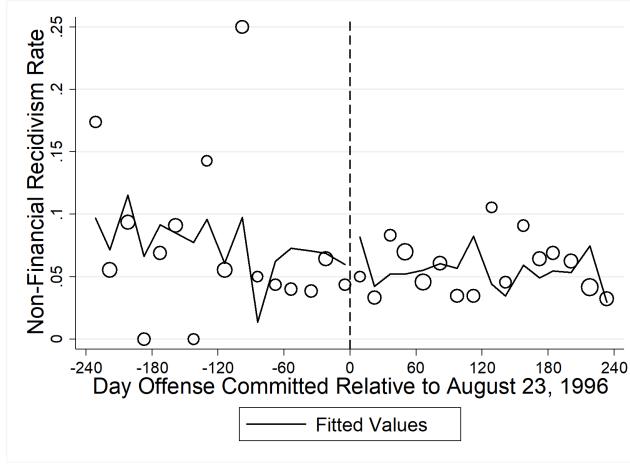
Figure A6a. Any Recidivism

**Figure A6b. Financial Recidivism**
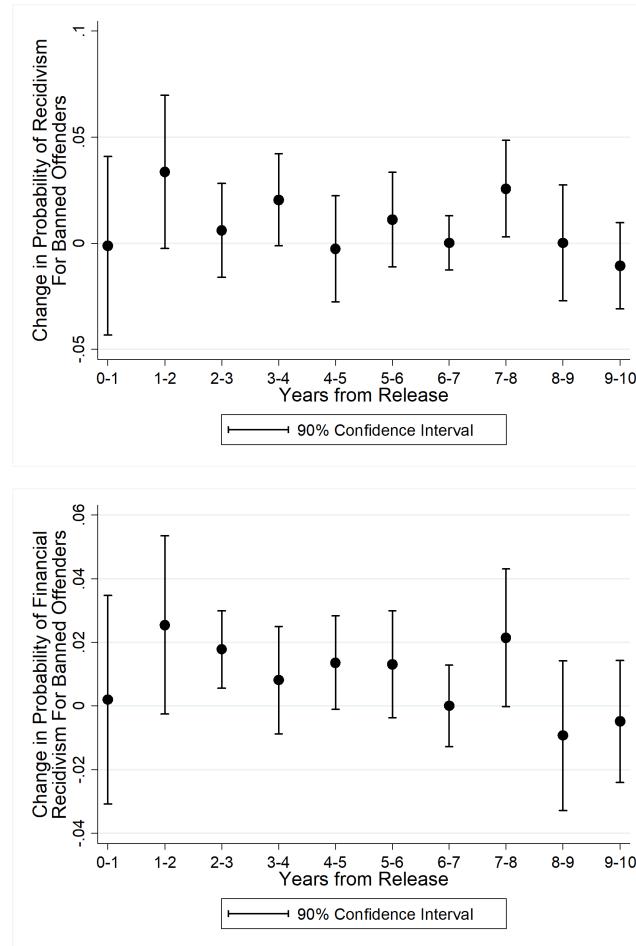

Figure A6c. Non-Financial Recidivism
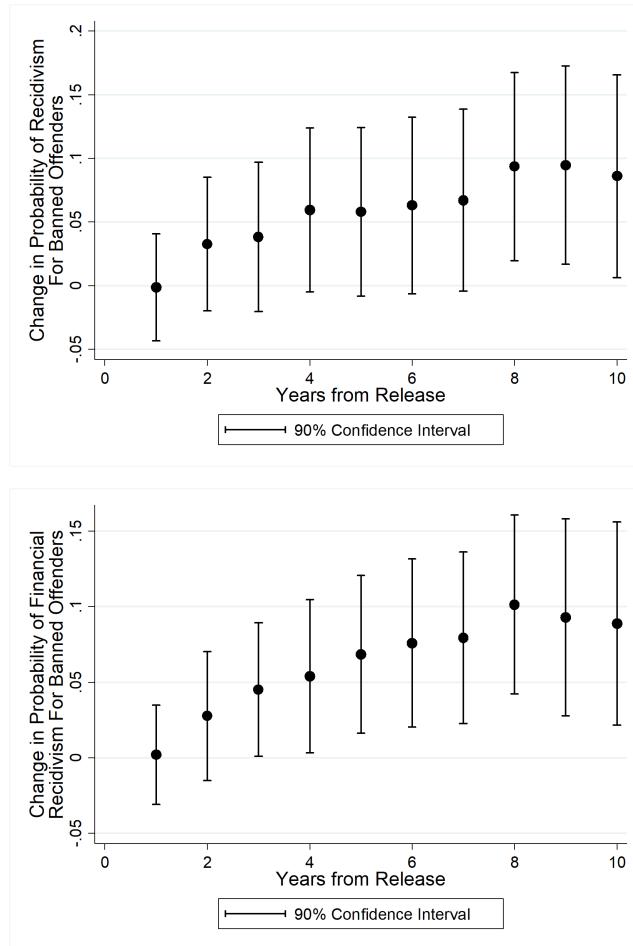
Notes: The figures above plot the lines of fitted values from time-series regressions modeling recidivism rates as an AR(1) process (number of lags chosen using the model with the highest AIC). All figures are overlaid with a scatter plot of the dependent variable averaged in 15-day bins. See Figures A1a-A1b for notes about the running variable. See Table A11 for notes about the time-series estimation. See Table A4 for a definition of financially and non-financially motivated crimes and the associated recidivism measures.

Figure A7. Effect of SNAP Ban on Timing of Re-incarceration
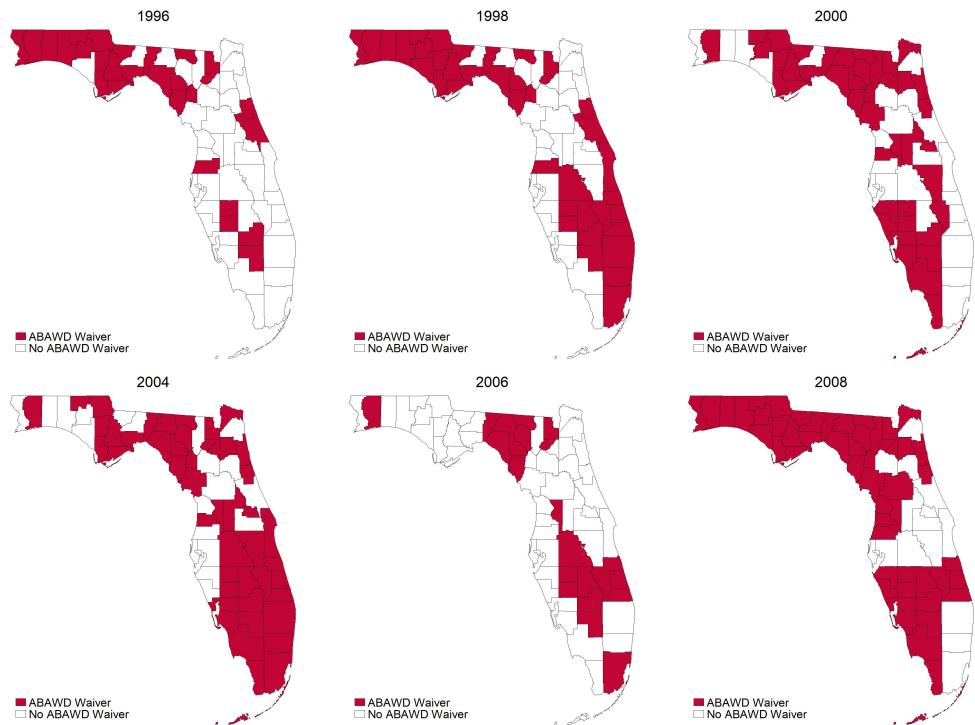
Notes: The first figure above displays the coefficient from ten separate regressions to illustrate how the SNAP ban affects timing of re-incarceration. For example, the coefficient plotted at "1-2" on the x-axis is the coefficient from a regression of whether or not the offender returns to prison within 1-2 years after release on whether or not the offender is banned from SNAP (committed a drug-trafficking offense on or after Aug 23, 1996). The second figure displays ten coefficients from similar regressions that use timing of financial recidivism as the dependent variable instead of timing of any recidivism. All regressions use a linear polynomial of the running variable, uniform kernel, and a bandwidth of ±240 days. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures.

**Figure A8. Effect of SNAP Ban on Timing of Re-incarceration, Cumulative**
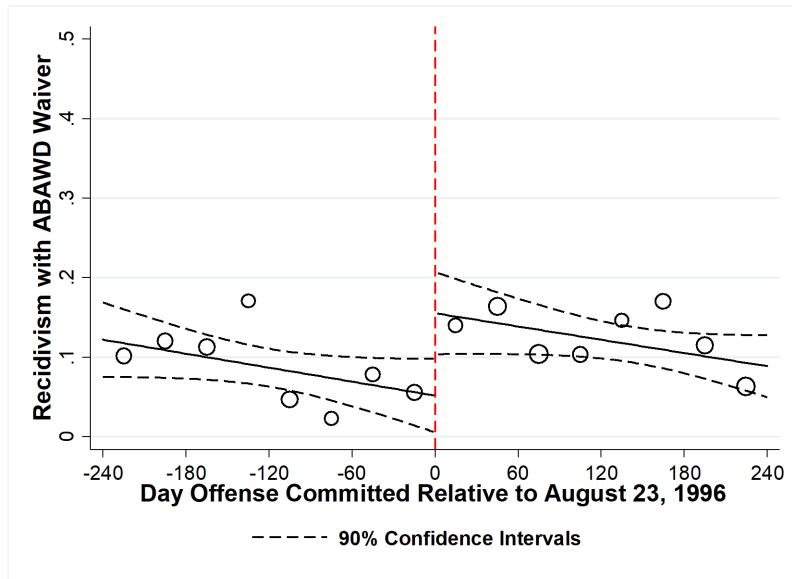
Notes: The first figure above displays the coefficient from ten separate regressions to illustrate how the SNAP ban affects timing of re-incarceration. For example, the coefficient plotted at "1" on the x-axis is the coefficient from a regression of whether or not the offender returns to prison within 0-1 years after release on whether or not the offender is banned from SNAP (committed a drug-trafficking offense on or after Aug 23, 1996). Similarly, the coefficient plotted at "5" is the coefficient from a regression of whether or not the offender returns to prison within 0-5 years after release on whether or not the offender is banned from SNAP. The second figure displays ten coefficients from similar regressions that use timing of financial recidivism as the dependent variable instead of timing of any recidivism. All regressions use a linear polynomial of the running variable, uniform kernel, and a bandwidth of ±240 days. See Table A3 for a definition of financially and non-financially motivated crimes and the associated recidivism measures.

Figure A9. Geographic Variation in ABAWD Work Requirement Waivers, 1996-2008
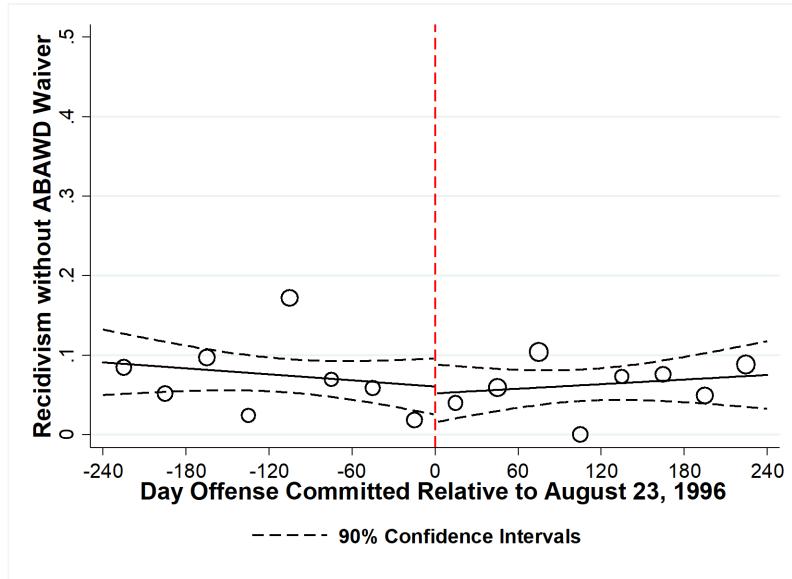
Notes: The figures above display which Florida counties have an ABAWD work requirement waiver at any point in a given year. When a county is filled in with red, it has an ABAWD work requirement waiver at some point in that year. When a county is filled in with white, it never has an ABAWD work requirement waiver in that year. I display every even-numbered year starting in 1996 and ending in 2008. I do not display years past 2008 since there is a nationwide ABAWD work requirement waiver in place from 2009-2016. Also, there is a nationwide ABAWD work requirement waiver in place from 2001-2003, so I do not display the map for 2002. An animation showing the above maps for every month-year combination from 1996-2009 is available here: https://www.dropbox.com/s/kufg1ieiwtjm0b6/Waivers%20by%20County-Month.gif?dl=0

Figure A10a. Effect of SNAP Ban on Recidivism in Time/Place with ABAWD Work Waiver

90% Confidence Intervals
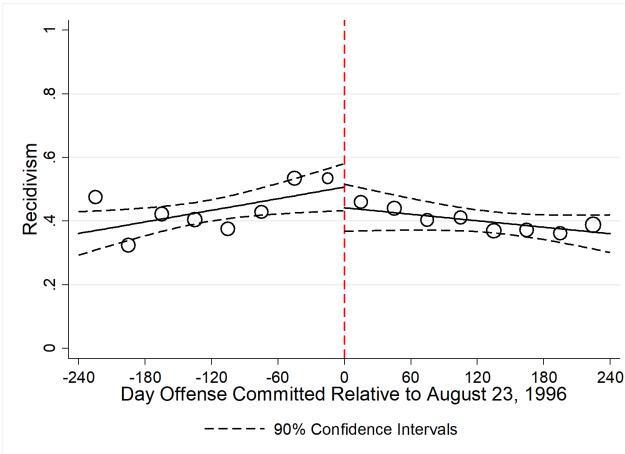
Figure A10b. Effect of SNAP Ban on Recidivism in Time/Place without ABAWD Work Waiver

90% Confidence Intervals
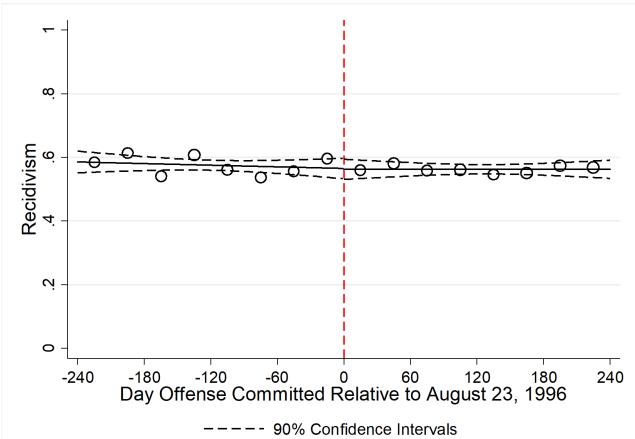
Notes: The figures above (and the following RD plots more generally) display the lines from two local linear regressions, estimated separately on each side of the cutoff using the offense-level micro data. I also overlay a scatter plot of 30-day bin averages of the dependent variable weighted by the number of offenses in each 30-day bin. See Figure A1 for notes about the running variable. The dependent variable in Figure A10a is whether or not the offender returns to prison for a crime committed in a time and place when an ABAWD work waiver was in effect. The dependent variable in Figure A10b is whether or not the offender returns to prison for a crime committed in a time and place when an ABAWD work waiver was not in effect. See Table A19 for more detail about this estimation and the ABAWD work requirement more generally.

**Visual Evidence of Main Result: Offenders Subject to SNAP Ban are More Likely to Recidivate**
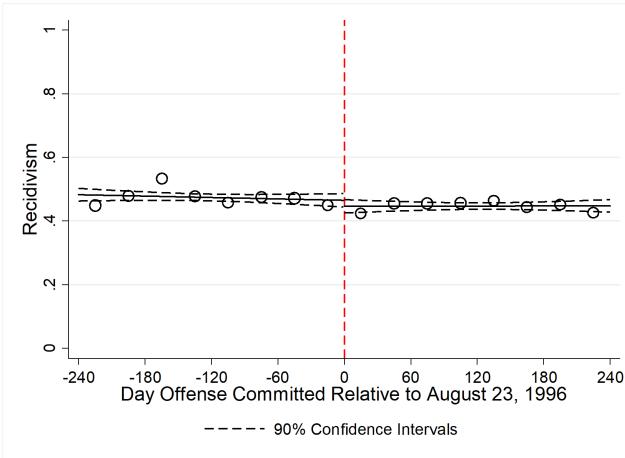
Figure A11a. DUI or Revoked License

90% Confidence Intervals
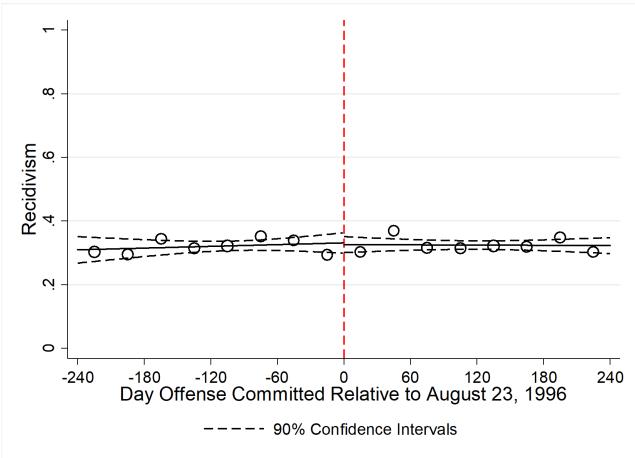
Figure A11b. Drug Possession

90% Confidence Intervals

Figure A11c. Property Crime

90% Confidence Intervals

Figure A11d. Violent Crime

90% Confidence Intervals

Notes: The figures above plot the lines from local linear regressions of recidivism outcomes on the running variable (days before and after August 23, 1996), estimated separately on each side of the cutoff for several different "placebo" crimes (crimes that do not lead to permanent ban from SNAP in Florida). All figures are overlaid with a scatter plot of recidivism averaged in 30-day bins. See Figures A1a-A1b for notes about the running variable. See Figure A4 for general notes about the creation of the RD plots for drug traffickers. These plots employ the same method but on a sample of offenders who do not commit drug trafficking but instead commit the following crimes: DUI/driving with a revoked license, drug possession, property crime, and violent crime.

Figure A12. Drug Traffickers in Other Years are Not More Likely to Recidivate

1997

1998

1999

2000
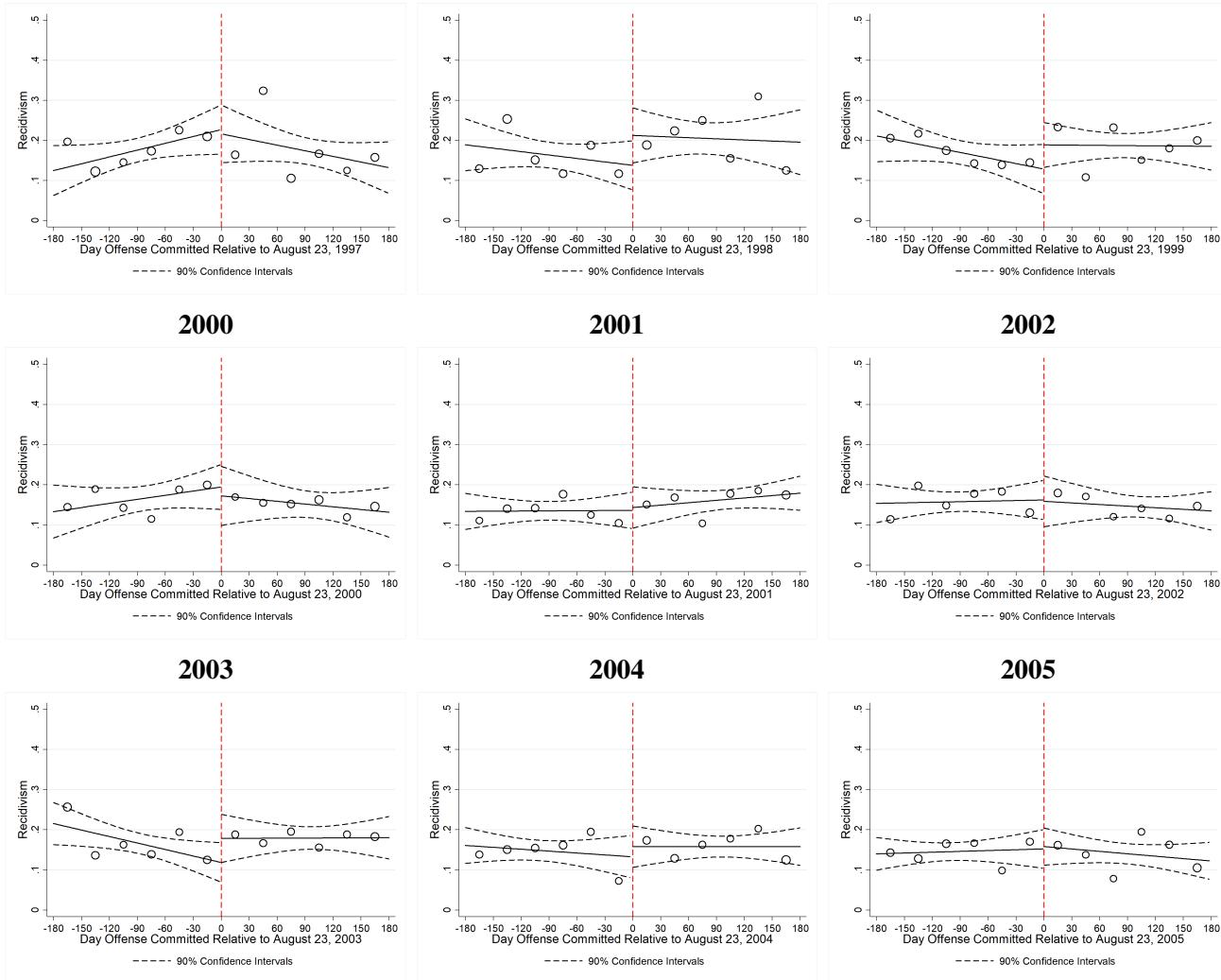
2001


2002

2003

2004
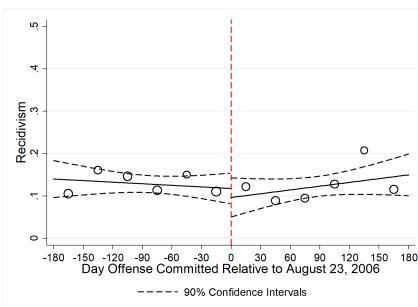
2005
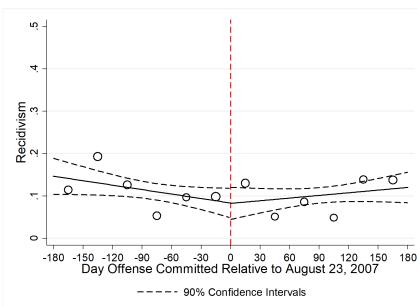
Notes: The figures above plot lines from local linear regressions of recidivism outcomes on the running variable (days before and after August 23 of a given year), estimated separately on each side of the cutoff. All figures are overlaid with a scatter plot of the recidivism averaged in 30-day bins. In these figures, the running variable is centered around placebo dates (dates that do not determine ban status). See Figure A4 for general notes about the creation of the RD plots for drug traffickers around August 23, 1996. These plots employ the same method but on a sample of offenders who commit drug trafficking around August 23 in the years 1997-2012.

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Figure A12. Drug Traffickers in Other Years are Not More Likely to Recidivate


Notes: The figures above plot lines from local linear regressions of recidivism outcomes on the running variable (days before and after August 23 of a given year), estimated separately on each side of the cutoff. All figures are overlaid with a scatter plot of the recidivism averaged in 30-day bins. In these figures, the running variable is centered around placebo dates (dates that do not determine ban status).

Figure A13. Test for Other Significant Breaks in Bandwidth

Note: The figure above follows Card, Mas, and Rothstein (2008) in identifying the "true" cutoff as determined by the data. To do this, I construct 120 placebo cutoffs (one for each of the 60 days before and after August 23, 1996). I then code placebo dummy variables for whether or not the offender committed their offense on or after each placebo date. Finally, I run 120 regressions of financial recidivism on each placebo dummy and plot the R-squared from each regression (no controls included). The "true" cutoff should have the highest R-squared. I detect the "true"cutoff at August 29, 1996 which is only six days from the date of the policy cutoff. The 15 days with the highest R-squared are all within nine days of August 23, 1996 and August 23, 1996 itself has the fifth highest R-squared.

Figure A14. Ganong-Jaeger Randomized Cutoffs Placebo Test

Note: The figures above follow a randomization inference test outlined in Ganong & Jaeger (2015). To create these figures, I calculate the 5th-95th percentiles of the running variables—days before or after August 23, 1996. At every percentile, I construct a placebo cutoff and run 46 separate regressions of recidivism on a dummy for whether or not the offender committed the offense on or after the placebo date. From here, I plot the coefficient estimates and confidence intervals on the y-axis against the running variable on the x-axis in the first figure. In the second figure, I plot a histogram of the coefficient estimates (most are near zero) and highlight the estimates which are significant. In addition, I plot a vertical red line indicating the value of the coefficient at the true cutoff (August 23, 1996). Less than 10% of the placebo estimates are positive and significant.

Appendix B. Additional Information

I. Further Review of Related Literature. *A. Offender Reentry.* Former offenders face a number of challenges when looking for legal work. First, many employers require employees to disclose criminal backgrounds on job applications and/or agree to criminal background checks. Pager, Western, and Sugie (2009) conduct an audit study in which they randomly assign a criminal background to some applicants. They find that applicants with criminal histories are half as likely to be called back by interviewers—this gap is even wider for black applicants. In recent years, offender advocates have encouraged cities and states to adopt laws that "ban the box" that asks applicants about criminal background. In fact, Shoag and Veuger (2016) show that after a city enacts "ban the box" legislation, employment from high-crime Census tracts increases.[1] In many cases, state occupational licensing laws only serve to exacerbate the troubles former offenders have in the legal labor market. Ex-felons are subject to more than 3,000 restrictive occupational licensing exclusions according to the American Bar Association (Council of Economics Advisors (CEA) 2016).

While the employment consequences associated with simply having a criminal background are

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[1]Agan and Starr (2018) find similar results to Pager, Western, and Sugie (2009) with a field experiment in which they sent applications to employers in New Jersey and New York City before and after "ban the box" went into effect. Employers who asked about criminal history in their sample were 62% more likely to call back applicants if they did not have a criminal record. The authors also point out the importance of statistical discrimination in this setting. Before "ban the box" went into effect, employers were 7% more likely to call back white applicants than black applicants, but this number balloons to 45% after "ban the box." It appears that "ban the box" may help offenders find work, but in doing so, it can diminish the employment prospects for young black men in general. This statistical discrimination spillover of "ban the box" policy is also explored by Doleac and Hansen (2016) who find that employment of young, low-skilled Black and Hispanic men decreases after "ban the box" takes effect in a metropolitan area.

large, incarceration and the prison experience can also negatively affect employment outcomes. For one, even if offenders are not explicitly tagged with their criminal backgrounds in the application process, many are left with large gaps in their work history as a result of their incarceration (Raphael 2011). Kroft, Lange, and Notowidigdo (2013) show that long-term unemployment in itself is penalized by potential employers. Incarceration may also prevent human capital accumulation, deteriorate bonds with legal job-finding networks, and/or create bonds with illegal job-finding networks (Bayer, Hjalmarrson, and Pozen 2009; Schmitt and Warner 2010). Mueller-Smith (2015) finds that an extra year of incarceration leads to a 4 percentage point drop in employment after release and a 30 percent decline in formal earnings. The stigma of a criminal background, the occupational restrictions, and the negative effects of incarceration are piled onto people who tend to have low education and low formal work experience even prior to incarceration, rendering them even less equipped to find legal work post-release (Raphael 2011).

Finding a job is not the only hurdle waiting for offenders as they transition back into their community. Once released, many offenders must navigate complicated and restrictive parole conditions that, if violated, could land them back in prison. Even more, offenders with families may return to a poverty-stricken or fractured homes—a family is 40% more likely to be in poverty when the father is incarcerated and incarceration increases probability of divorce or separation (CEA 2016). These stressors, among others, may contribute to the elevated mortality rate of offenders in the first couple of weeks after release, the majority of which is the result of drug overdoses (Schanzenbach et al. 2016)

Since offenders struggle to find legal work upon release, many reentry programs focus on increasing the employment prospects of offenders. In general, research has found mixed results on whether or not these programs are effective in curbing recidivism. Berk (2007) finds that work release does increase post-release earnings and that these earnings gains correlate with lower rates of re-incarceration but only for those offenders originally convicted of financially motivated crimes.[2] Another popular approach for helping offenders find legal work is through transitional employment programs. The National Supported Work (NSW) Demonstration, for example, provided a minimum wage job to ex-offenders for 12-18 months. Uggen (2000) finds the program decreased 3-year re-arrest rates for offenders above the age of 26 at the start of the program by about 20%. For younger offenders, however, the program was ineffective.[3]

Still, other work has consistently found that offenders who face better labor market conditions upon release are less likely to recidivate. Schnepel (2018), for example, finds that the availability of "good jobs" (manufacturing and construction work) reduces recidivism for offenders released in California whereas availability of other low-wage jobs has no effect. Yang (2017) also finds that being released in a time and place with good labor market conditions decreases probability of recidivism.

B. Financial Need and Crime. I find that offenders who are denied access to SNAP have higher rates of reincarceration. This result contributes to the literature above on prisoner reentry and recidivism, but it also adds to a long literature in economics and criminology that argues that financial motivations often underlie criminal behavior. In a seminal theoretical paper on criminal

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[2]Berk evaluates a work release program in Florida by comparing minimum custody inmates who participated in the program to minimum custody inmates who did not.

[3]Uggen evaluates the impact of the NSW by analyzing a randomized controlled trial in which some offenders were assigned to receive transitional employment while others were simply required to self-report employment and criminal information.

behavior, Becker (1968) points out the trade-off between participation in the legal labor market and the illegal labor market. Becker discusses how increased opportunities in the legal labor market could decrease participation in the illegal labor market. Most recent empirical investigations of the Becker model confirm this—Gould, Weinberg, and Mustard (2002) find that unemployment and wages for low-skilled men in a county are significantly related to crime in that county.[4]

Other empirical work also suggests that legal and illegal sector jobs may be substitutes. Mastrobuoni and Pinotti (2015), for example, find that recidivism (rearrest) and overall criminal activity decreases once immigrants become legal citizens, presumably because with citizenship comes many new job opportunities. The theoretical and empirical literature about legal opportunities and crime or recidivism suggests that financial need is a determinant of criminal behavior.

A nascent subset of this literature explores the effects of transfer programs on crime, and supports the claim that financial need is a catalyst for criminal behavior. Chioda, Mello, and Soares (2015) estimate the effect of a conditional cash transfer in Brazil named Bolsa Familia. They find that as the number of children receiving the cash transfer from Bolsa Familia increases, crime decreases.[5] Similarly, Das and Mocan (2016) show that short-term employment from a public works program in India insures against negative income shocks, and as a result, decreases crime.

C. Transfer Programs and Labor Supply. In addition to the work on labor supply effects covered in the main text, Moore (2014) examines a PRWORA policy that removed drug and alcohol addictions as qualifying disabilities for DI. Moore uses this policy change in a difference-in-difference framework to determine the effect of DI on labor supply. Specifically, he compares people thrown off the DI rolls by this policy to people who had drug and alcohol addictions but were able to stay on DI for another condition. Moore finds that 22% of people removed from DI increase their labor supply to levels beyond the DI eligibility threshold. The effects of PRWORA and pre-PRWORA welfare waivers on outcomes such as labor force participation, welfare caseloads, and fertility/family structure are further documented (Blank 2002).

Hoynes and Schanzenbach (2012) also estimate labor supply effects for groups other than female-headed households. They find that the introduction of Food Stamps in a county causes a imprecisely estimated decrease in head of household annual earnings in noneldery households with low education. However, the authors find no change in hours worked and an increase in labor force participation. Focusing on female-headed households, the authors show that for those households all measures of labor supply decrease after the introduction of Food Stamps. For female-headed households, labor force participation falls by about 6 percent and this decline is even sharper for nonwhite female heads. The authors also find evidence of changes in labor supply along the intensive margin with female-headed households decreasing both hours worked and annual earnings. 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 limited, and there is no consideration of illegal labor supply.

Finally, I draw inspiration from Deshpande (2016), who estimates labor supply effects of Supplemental Security Income (SSI) child disability support. PRWORA required that children receiving SSI undergo a medical review at age 18 if their birthday occurred on or after August 23, 1996. Deshpande demonstrates that undergoing a medical review caused many kids to lose SSI benefits.

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[4]Using Bartik-style instrumental variables, they show that higher unemployment leads to more crime and higher wages leads to less crime.

[5]The authors use the expansion of Bolsa Familia in 2008 and the demographic composition of schools to instrument for the number of children receiving funds from Bolsa Familia.

Using the August 23, 1996 cutoff in a regression discontinuity design, she finds that 18-year-olds who lose SSI do increase their labor supply but not by enough to offset the loss of SSI. Her paper also uses one impactful piece of PRWORA to estimate the effect of transfers on labor supply.

II. Miscellaneous Details.   Throughout the paper, I focus on one specific definition of recidivism–return to prison. Recidivism has many definitions in the criminology literature. For example, recidivism can be defined as re-arrest, re-conviction, re-offense, and so on (Maltz 1984). In addition, recidivism is often defined with respect to some time frame (such as the 3-year or 5-year re-arrest rate). The definition I use in this paper is a return to a Florida prison for a new offense. I do not observe re-arrest, re-offense, or re-conviction. These events all occur more often than re-incarceration for a new offense. In appendix Table A3, I show the results are robust to using 10-year, 8-year, and 5-year recidivism rates.

It's also worth noting that the crime for which an offender is convicted can feasibly differ from the crime which an offender committed. I observe the crime(s) for which the offender is convicted, which may not be the crime(s) they committed. For example, conviction crime and true offense crime may differ as a result of plea bargaining. That said, for the measure of treatment (the SNAP ban), only conviction crime and the date the offense was committed matters. In addition, the classifications financial and non-financial are broad—it is unlikely that slippage from offense crime to conviction crime will move a person from the financial to non-financial category (or vice versa).

Since the SNAP ban can be modified and repealed at the state level, offenders subject to the ban in one state could, in principle, move to another state and become eligible for SNAP. I do not find evidence that drug traffickers subject to the ban are more likely to migrate out of Florida and move away from the ban. Using the residence each offender plans to live at upon release (as reported on their release plan), I test for a change in the probability of that residence being outside of Florida. Offenders subject to the SNAP ban are not more likely to report a planned residence outside the state of Florida. Still, it is possible that offenders move to a place not listed on their release plan. In that case, the estimates in this paper will be attenuated.

While I provide numerous summary statistics on the offender population in Tables 1 and A1, I do not report the marital status of offenders because that information is not made publicly available in the OBIS database. This is potentially important for understanding how the SNAP ban affects ex-offenders. In 2013 and 2014, about 15% of Broward County jail inmates in Florida reported being married or having a significant other while the remaining 85% reported being single, divorced, separated, or widowed (ProPublica 2017). Unfortunately, to my knowledge, that is the best information available about marital status of Florida inmates.

In interpreting the main results, it is also important to consider the state's reentry policies/strategies. Florida abandoned its traditional parole system prior to 1995 and moved to a fixed sentencing system. With fixed sentencing (also known as structured sentencing or truth-in-sentencing), offenders must serve a certain percentage of their sentence (typically 80-90%). About 31% of offenders have some form of post-release supervision in Florida.

Finally, the regression used to create the risk score has a McFadden's R^2 of 0.20 and correctly predicts the recidivism outcome in 79% of drug trafficking cases within 212 days of August 23, 1996 (the IK optimal bandwidth for any recidivism). I can also calculate the risk score based on only those offenders subject to the ban and not in the ± 212 day IK bandwidth–the results do

not change. I also test for heterogeneity in the effect by sentence length and by risk score. The coefficients are not statistically different from zero, but the point estimates imply that the effect of the ban on any recidivism is muted for riskier offenders and for offenders who serve longer sentences.

Appendix C. Conceptual Model of SNAP and Illegal Labor Supply

To more clearly illustrate the mechanisms described in the main body of the paper, I present a simple conceptual model. In the traditional static labor supply model with transfers, individuals choose $c =$ consumption and $l =$ leisure subject to $h =$ hours worked and $wh + y^{transfer} =$ total income to maximize utility:

$$\max_{c,l} u(c,l) \; s.t. \; c = wh + y^{transfer} \tag{1}$$
$$l = 1 - h$$

This model is agnostic about whether h is supplied in the legal or illegal sector. For ex-offenders, this is an important distinction because they have ties to the illegal labor market, and they have difficulty finding work in the legal labor market. To highlight this distinction, I expand the model above to include $h^I =$ hours worked in the legal labor market and $h^L =$ hours worked in the illegal labor market. In addition, I assume that individuals must satisfy a fixed level of consumption $\bar{c}$.

$$\max_{h^I,h^L} u(w^I h^I + w^L h^L + y^{transfer}, 1 - h^I - h^L) \; s.t. \; w^I h^I + w^L h^L + y^{transfer} \geq \bar{c} \tag{2}$$
$$1 - h^I - h^L \geq 0$$

For simplicity, I further assume that ex-offenders face no additional cost of supplying illegal hours relative to legal hours. This implies that ex-offenders will optimally allocate all working hours to one sector. In general, I assume ex-offenders command a higher wage in the illegal labor market (w^I) than they command in the legal labor market (w^L)–this is a reduced form way of representing the difficulty of finding legal work versus illegal work for ex-offenders. When $w^I > w^L$ the maximization problem above reduces to the following:

$$\max_{h^I,h^L} u(w^I h^I + y^{transfer}, 1 - h^I) \; s.t. \; w^I h^I + y^{transfer} \geq \bar{c} \tag{3}$$
$$1 - h^I \geq 0$$

Assuming that neither of the constraints binds, then differentiating the first order condition of the problem above with respect to $y^{transfer}$ and h^I yields the following comparative static[6]:

---

[6]The denominator of $dh_I/dy^{transfer} = \dfrac{-(w^I \times u_{11} - u_{21})}{w^I \times (w^I \times u_{11} - u_{12}) - (w^I \times u_{21} - u_{22})}$ is negative based on the second order condition.

$$dh_I/dy^{transfer} < 0 \; iff \; w^I \times u_{11} - u_{21} < 0 \qquad (4)$$

Thus, for ex-offenders optimally consuming above $\bar{c}$ and working $h^I < 1$, a decrease in transfers will lead to an increase in hours worked in the illegal sector if leisure is a normal good.

For ex-offenders optimally consuming at $\bar{c}$ and working $h^I < 1$, we recover the following comparative static:

$$dh_I/dy^{transfer} < 0 \; iff \; w^I > 0 \qquad (5)$$

Notice that for these individuals, the response of h^I to a change in $y^{transfer}$ does not depend on preferences. For offenders consuming at $\bar{c}$, a decrease in transfers always leads to a increase in hours worked in the illegal sector.

Finally for those ex-offenders who are optimally working at $h^I = 1$, a decrease in $y^{transfer}$ will not induce an change in h^I; while these offenders may desire to increase h^I when $y^{transfer}$ falls, they cannot because of the constraint on their total time. In a more complex model, perhaps, even these offenders could respond by increasing the severity or "riskiness" of the crimes they choose to commit.

For drug traffickers in Florida who committed their offense prior to August 23, 1996, total income is the sum of earned income and transfer income (including SNAP). Those drug traffickers who committed their offense on or after August 23, 1996 are denied SNAP benefits. Because of this, transfer income for those committing an offense prior to the cutoff date is higher than transfer income for those committing an offense on or after the cutoff date. The comparative statics above yield a clear prediction: ex-offenders who are banned from SNAP will optimally choose to work more hours in the illegal sector (when possible) than ex-offenders who are not banned from SNAP. I empirically test whether or not offenders denied SNAP increase illegal labor supply (measured as whether or not they are re-incarcerated for a financially motivated crime), and I find evidence that suggests that they do.

This model motivates two heterogeneity tests I conduct. I began the model by assuming that $w^I > w^L$ to represent the difficulty that ex-offenders have in finding legal work versus illegal work. However, finding legal work (or increasing hours in the legal labor market) is more feasible for some ex-offenders than for others. For one, ex-offenders released during good legal labor markets may enjoy higher legal wages or may have an easier time finding legal work in general. Similarly, recall that Pager, Western, and Sugie (2009) find that offenders who are black face greater discrimination in the legal labor market than offenders who are white. To capture this in the model above, I assume that offenders released in good legal labor markets and offenders who are white are more likely to face $w^L > w^I$. The SNAP ban does not affect illegal labor supply in the model above when $w^L > w^I$, and thus, it should have less of an affect for groups more likely to face $w^L > w^I$.

To test the prediction regarding offenders released in good legal labor markets, I estimate the interaction between access to SNAP and state-level unemployment rate at the time of the offender's release. Taking this the data, I find noisy but positive estimates of the effect of state-level unemployment on offenders subject to the ban. This is consistent with the model above. When the unemployment rate is high, offenders are more likely to face $w^I > w^L$ and thus, the effect of the ban should be larger. To test the prediction regarding race of the offender, I estimate the interaction between access to SNAP and whether or not the offender is black. In testing for heterogeneity by race, I find noisy results that suggest black offenders subject to the ban are more likely to recidivate

than white offenders subject to the ban. Although these estimates are not statistically different than zero, the magnitude and direction are consistent with the model above.

Finally, the model suggests that when the disparity in $y^{transfer}$ between banned and non-banned offenders is greater, we should observe that the ban has a stronger effect. I use county-by-month variation in the work requirement imposed on Able-Bodied Adults Without Dependents (ABAWDs) to test how the effect of the ban differs when benefit generosity for the non-banned offenders is higher. The work requirement stipulates that unemployed ABAWDs may only receive SNAP benefits for three months out of every three years. If the ABAWD is employed more than 20 hours per week or is enrolled in a SNAP employment and training program, then they may receive SNAP benefits for more than three months. This requirement was waived nationally from 2009-2016. In addition, the requirement is waived for Labor Surplus Areas (counties in Florida with especially high unemployment) and for counties where Florida chooses to apply a special exemption that allows states to exempt 15% of the state's caseload from the requirement (the 15% exemption) (USDA 2016).

Using information from the Florida Department of Children and Families from 1996-2016, I create a measure for each month and county in Florida indicating whether or not the work requirement for ABAWDs is waived. I then estimate the effect of the ban on the probability an offender recidivates at a time and place where the ABAWD work requirement is waived versus the probability an offender recidivates at a time and place where the ABAWD work requirement is in effect. I find that the effect of the ban is strongest when benefit generosity for the non-banned offenders is high, which is consistent with the conceptual model above.

The static labor supply model can be extended to a dynamic setting in which offenders search for jobs over time. In the dynamic model, suppose offenders face a cost of job search that decreases with time out of prison, but that they also receive financial support from family members that decreases with time out of prison (Western et al. 2015). If the cost of the job search is highest immediately after release, then SNAP benefits may be most vital in this transition period. However, if family support is also highest immediately after release, then SNAP benefits may be more important years later when family support has waned. The model yields an ambiguous prediction about when support from SNAP is most important. In addition, once the cost of searching is incorporated, the model predicts increased recidivism among banned offenders via two channels: (1) the banned offenders are given less transfer income and thus have an incentive to increase labor supply and (2) the non-banned offenders are given more transfer income and thus have assistance that may mitigate the cost of legal job search. In this paper, I do not distinguish between these two channels. However, given that over half of all offenders (many of which have access to SNAP) are unemployed even a year after release, it does not appear that the second channel plays much of a role.

Appendix D. Cost-Benefit Analysis of the SNAP Ban

Recall, cost per offender is defined as:

$$
\begin{aligned}
\text{Cost per Offender} \quad = \quad & [(\text{Marginal Cost of Year of Incarceration}) \times (\text{Mean Years Sentenced}) \\
& \times (\text{Marginal Increase in Probability of Offending due to the Ban})] \\
& + [(\text{Victim Cost}) \\
& \times (\text{Marginal Increase in Probability of Offending due to the Ban})]
\end{aligned}
$$

In columns (1)-(4) of Table D1, I estimate the total societal cost of the SNAP ban. To be clear, this cost estimate is intended to highlight the potential benefit of reducing recidivism by providing SNAP or other financial support post-release. 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. In this calculation, I only include the cost of incarcerating the offenders and the cost of victimization. To start, I assume that drug traffickers who return to prison are sentenced to about 3 years, a statistic supported by the data from Florida Department of Corrections. I use an estimate of the marginal cost of incarcerating an inmate for one year from the US Department of Justice ($9,600 per year) (US DOJ 2011) and I use an estimate of victimization costs from the National Institute of Justice ($11,000) (Miller, Cohen, and Wiersema 1996). All dollar values in this section are adjusted to 2016 dollars.

In columns (5) and (6), I estimate the net cost for taxpayers. In other words, I ignore the private benefit drug traffickers get from SNAP benefits. Introducing this assumption requires an additional assumption about how long a drug trafficker would spend on SNAP if given the opportunity. The average length of time spent on SNAP is about 10 months (USDA 2011). I assume that drug traffickers would spend about the same amount of time on SNAP as the average recipient. I also assume the average SNAP benefit for men in Florida is about $150—this is consistent with the summary statistics on SNAP benefits in Table 2. Again, in columns (5) and (6), I treat the SNAP funds not disbursed to drug traffickers as a benefit, this is a highly conservative assumption which assumes an extra dollar of SNAP would have no effect on the welfare of a former drug trafficker. In other words, we ignore the benefit of SNAP to drug traffickers and estimate only the cost to non-banned taxpayers. In that case, the benefit per offender is defined as the following:

$$
\text{Benefit per Offender} = \text{Monthly Food Benefit} \times 12 \times \text{Mean Time on SNAP}
$$

Table D1. Cost-Benefit Analysis of SNAP Ban

	Societal Cost				Taxpayer Cost	
	(1)	(2)	(3)	(4)	(5)	(6)
Mean Time Served for Recidivating Offenders	3 years	3 years	3 years	3 years	3 years	3 years
Marginal Cost of Incarceration	$9,600	$9,600	$9,600	$9,600	$9,600	$9,000
Mean Months on SNAP	-	-	-	-	**12**	**12**
Monthly SNAP Benefit	-	-	-	-	**$150**	**$150**
Mean Cost of Victimization	**0**	**$11,000**	**0**	**$11,000**	**$11,000**	**$11,000**
Effect of SNAP Ban	**1.7 pp**	**1.7 pp**	**9.5 pp**	**9.5 pp**	**1.7 pp**	**9.5 pp**
Net Cost per Offender	**$490**	**$677**	**$2,736**	**$3,781**	**-$1,123**	**$1,981**

Notes: In the exercise above, "Net Cost per Banned Offender" is equal to the cost per banned offender minus the benefit. When calculating the taxpayer cost in (5) and (6), Benefit per Offender includes $Monthly Food Benefit \times 12 \times Mean Time on SNAP$ since taxpayers save that amount by denying drug traffickers SNAP benefits.

I assume that the effect of the SNAP ban on recidivism is approximately 1.7 percentage points in columns (1) and (2). In other words, for every 100 drug traffickers banned from SNAP, about 2 will recidivate because of the ban. This is the lower bound of the confidence interval on the main result in Table 3. This assumption yields my most conservative, traditional cost-benefit estimates. In the two columns that follow, I assume the effect of the SNAP ban is 9.5 percentage points—this is the point estimate from column (1) in Table 3, Panel B. In columns (2) and (4) above, I assume the cost of victimization is about $11,000 dollars on average. This cost of victimization is within the range of victimization costs for burglary, robbery, and theft provided by the National Institute of Justice (Miller, Cohen, and Wiersema 1996). The National Institute of Justice does not estimate a cost of victimization for drug crimes. Since the National Institute of Justice focuses on the material costs of crime and risk of death in these estimates, this number is an underestimate of the true costs of victimization (which also includes psychic costs, such as fear or trauma). Again, this yields conservative estimates of the net cost per offender.

In most cases, I find that the SNAP ban costs the state of Florida a substantial amount of money per offender banned. Even assuming the lower bound for the effect of the SNAP ban, I find the societal cost of the ban in Florida is about $677 per banned offender. With approximately 19,000 banned offenders, this implies the ban has cost Florida over 12 million dollars to date. Assuming the ban increases recidivism by 9.5 percentage points (the point estimate from the main results), I find the societal cost of the ban in Florida is about $3,781 per banned offender or approximately 70 million dollars to date. This estimate ignores the cost to the families of drug traffickers, all costs of crime for Florida citizens, and many other criminal justice costs (enforcement, trials, etc.). It also assumes the ban has zero deterrence effect for potential drug traffickers and no effect on the legal employment margin for those banned.

To drive the estimated net cost to zero, we must focus on the cost to taxpayers, ignoring the private benefit that drug traffickers and their families receive from the transfer. If I assume that the drug traffickers, if not banned, would spend about 1 year on SNAP and that the SNAP ban increases recidivism by about 1.7 percentage points (the lower bound estimate), then the SNAP ban has a net benefit of $1,123 per banned offender. However, if we assume the SNAP ban increases recidivism

by 9.5 percentage points (the point estimate), we recover a net cost of the SNAP ban of $1,981 per banned offender.

An important question that is beyond the scope of this paper is whether SNAP is the most efficient means of post-release financial support for reducing recidivism. Hendren (2017) suggests SNAP is highly inefficient in that it has potentially large negative labor supply effects. Hendren applies the estimate from Hoynes and Schanzenbach (2012) to the marginal value of public funds formula and finds that SNAP funds have a lower marginal value than funds spent on other programs. While a large decrease in legal labor supply may make SNAP less efficient than other programs, in general, it is not clear what the implication of that result is for SNAP and offender reentry. This paper argues that the decrease in recidivism is driven by a decrease in illegal labor supply. In that way, what makes SNAP less efficient generally (large labor supply response) may make it more efficient for reentry policy if the labor supply response of offenders is primarily on the illegal labor supply margin.

Appendix E. Data Construction

I use six separate datasets. First, the "Inmate Release Offenses CPS" and "Inmate Release Offenses Prpr" data include information about current and prior offenses, respectively, of released inmates. In addition, I link this data to the "Inmate Release IncarHist" dataset that details the admit and release date for each prison spell—this allows me to accurately calculate the time between release and the next offense. I also use data on active inmates, "Inmate Active Offenses" and "Inmate Active Offenses Prpr", to determine recidivism for those offenders who were released but returned and are currently serving a sentence. Finally, demographic information (age, sex, race) comes from the "Inmate Release Root" data. All datasets are publicly available from the Florida Department of Corrections. For the purposes of this paper, offender information such as full name, exact birthdate, or Florida offender ID are not necessary. Before beginning the data construction described below, I de-identify the data by assigning a new unique ID to each offender and by stripping the data of name and exact birthdate.

To construct the sample of offenders for the recidivism analysis, I start by combining the de-identified versions of "Inmate_release_offenses_CPS", "Inmate_release_offenses_prpr", "Inmate_active_ offenses_CPS", and "Inmate_active_offenses_prpr" from the FL OBIS Access database available here: http://www.dc. state.fl.us/pub/obis_request.html (downloaded on April 7, 2016). The combination of these tables is the totality of information that FL provides about released inmate offense history.[7] Next, I remove duplicate observations and offenses for which the adjudication was withheld.

After that, I manually tag drug trafficking offenses. I identify drug trafficking crimes by tagging offense types that contain the string "TRAFF" but do not contain the string "STOLEN PROPERTY", "HUMAN" or "SEX." Other crime categories are identified using a combination of manual string matching and an official categorization of offenses provided by the Florida DOC here: http://www.dc.state.fl.us/AppCommon/offctgy.asp. Exact strings used to identify specific crime categories are included in the data construction code.

Next, I collapse the data by offender ID, date of adjudication, and county of conviction, keeping

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[7]Florida also provides records about which offenders are currently under community supervision. Very few drug traffickers in my sample are in this dataset and offenders committing an offense after August 23, 1996 are not more ot less likely to be under community supervision. For this reason, I do not consider community supervision as a pertinent outcome.

the minimum date at each level and the maximum sentence length. For the trafficking offenses, I keep both the minimum and maximum offense date to insure that I am accurately classifying offenders as banned or not banned. If the resulting offense date for the offender does not equal the trafficking date, I replace it with the trafficking date—this is important since trafficking date determines treatment status, so I must measure this correctly. That said, this line of code affects a small number of observations. In general, I use the minimum trafficking offense date when necessary. However, I have estimated the main results using the maximum trafficking offense date, and this distinction does not matter. After that, I collapse further to the level of offender id, date of offense, and county of conviction. Again, I keep the minimum date and the maximum sentence length. And again, for trafficking offenses, I keep both the minimum and maximum date.

Next, I bring in the "Inmate_release_incarhist" table that includes information about the exact receipt and release date from prison. Since the previous data tables do not include receipt or release date to prison, I have to match offenders based on adjudication year and receipt year. This, naturally, will lead to some mistakes but I expect it is negligible. To do this matching, I drop duplicates at the level of offense ID and receipt year. Essentially, this means I leave out offenders who enter prison twice in the same year. This is not a big portion of drug traffickers or felony offenders in general. Next, I collapse the data by offender ID and receipt date. This yields a dataset in which each observation is a unique prison stay and in which the variables indicate all of the offenses associated with a given stay. I drop all observations with offense years before 1950 or after 2016.

Using this, I can calculate amount of time after release before an offender recidivates. I calculate "time until recidivism" as the difference between the release from prison stay[t] and the offense date for offense[t+1], if offense[t+1] exists. A small number of observations have a negative time to recidivism because offenders occasionally are arrested for crimes committed prior to stay[t] after they are released. This is not correlated with treatment. I remove these offenses and recalculate time to recidivism.[8] Since the data only includes inmates released after October 1, 1997, I exclude any observations with release dates prior to October 1, 1997 when doing recidivism analyses. I also drop all offenders with reported "race" as "Hispanic" due to special restrictions non-citizen immigrants face after committing a felony and after PRWORA's restrictions on SNAP receipt. Unfortunately, immigrant status is not available in the data. Outside of this, there are no other major data cleaning steps, only variable construction and analysis. Data and code necessary to reproduce all analyses (in the main text and in the online appendix) are available on the AEA website for this paper.

Finally, when providing the public database of released offenders, Florida includes the following disclaimer which I pass along here, "The Florida Department of Corrections updates this information regularly, to ensure that it is complete and accurate; however this information can change quickly. Therefore, the information in this file may not reflect the true current location, status, release date, or other information regarding an inmate. This database contains public record information on felony offenders sentenced to the Department of Corrections. This information only includes offenders sentenced to state prison or state supervision. Information contained herein includes current and prior offenses. Offense types include related crimes such as attempts, con-

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[8]In the code, I keep a variable that codes recidivism based on whether or not an offender has a prison stay after they are released (even if that stay is for a crime committed before stay[t]. Using this variable as the dependent variable, I get the same results. Offenders who commit drug trafficking on or after August 23, 1996 are more likely to have a stay[t+1].

spiracies and solicitations to commit crimes. Information on offenders sentenced to county jail, county probation, or any other form of supervision is not contained. The information is derived from court records provided to the Department of Corrections and is made available as a public service to interested citizens. The Department of Corrections makes no guarantee as to the accuracy or completeness of the information contained herein. Any person who believes information provided is not accurate may contact the Department of Corrections. The Florida Department of Corrections is not responsible for misinterpretation or inaccurate reporting by entities or persons utilizing this information."

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