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 Aug. 23, 1996 (Banned)	0.1099**	0.0965**	0.0950**	0.1026***	0.0436	0.0748**
	(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 Aug. 23, 1996 (Banned)	0.0894*	0.0914**	0.0909**	0.0998***	0.0581	0.0685**
	(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 Aug. 23, 1996 (Banned)	0.0787*	0.0922**	-0.0049
	(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 Aug. 23, 1996 (Banned)	0.0873**	0.0882**	-0.0010
	(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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A5. 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 Aug. 23, 1996 (Banned)	0.0922*	0.1064***	-0.0037
	(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 Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After Aug. 23, 1996 (Banned)	0.1053**	0.1043***	0.0010
	(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 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. 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 Days from Aug. 23, 1996	1	1	1
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 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 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)	$\pm$212	$\pm$243	$\pm$255
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of $\pm$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)	$\pm$240	$\pm$240	$\pm$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)	$\pm$214	$\pm$271	$\pm$233
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of $\pm$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)	$\pm$240	$\pm$240	$\pm$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 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 Days from Aug. 23, 1996	1	1	1
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 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. 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 Days from Aug. 23, 1996	1	1	1
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 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). 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 ±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)	±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 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 Days from Aug. 23, 1996	1	1	1	1
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 Days from Aug. 23, 1996	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. 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 Aug. 23, 1996 (Banned)	0.0996**	-0.0039
	(0.0415)	(0.0292)
Control Group Mean	0.0874	0.0761
Observations	936	990
Bandwidth (in Days)	±242	±256
Degree of Polynomial in Days from Aug. 23, 1996	1	1
Panel B. Consistent Bandwidth of ±240 Days		
Offense Committed After Aug. 23, 1996 (Banned)	0.1037**	-0.0087
	(0.0418)	(0.0306)
Control Group Mean	0.0880	0.0764
Observations	918	918
Bandwidth (in Days)	±240	±240
Degree of Polynomial in Days from Aug. 23, 1996	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 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 Aug. 23, 1996 (Banned)	0.0997**	0.0002
	(0.0420)	(0.0317)
Control Group Mean	0.0833	0.0797
Observations	918	997
Bandwidth (in Days)	±240	±258
Degree of Polynomial in Days from Aug. 23, 1996	1	1
Panel B. Consistent Bandwidth of ±240 Days		
Offense Committed After Aug. 23, 1996 (Banned)	0.0997**	-0.0048
	(0.0420)	(0.0335)
Control Group Mean	0.0833	0.0810
Observations	918	918
Bandwidth (in Days)	±240	±240
Degree of Polynomial in Days from Aug. 23, 1996	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 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 $\pm$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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A22. Placebo Test: Recidivism for Sell/Mfg/Dist 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 Aug. 23, 1996 (Banned)	0.0109	-0.0235	0.0366
	(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 Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After Aug. 23, 1996 (Banned)	0.0056	-0.0313	0.0369
	(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 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 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 Aug. 23, 1996 (Banned)	0.0088	0.0065	-0.0002
	(0.0143)	(0.0126)	(0.0092)
Control Group Mean	0.3930	0.2425	0.1505
Observations	26375	29232	21928
Bandwidth (in Days)	$\pm$506	$\pm$595	$\pm$373
Degree of Polynomial in Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of $\pm$240 Days			
Offense Committed After Aug. 23, 1996 (Banned)	0.0062	0.0072	-0.0010
	(0.0187)	(0.0164)	(0.0109)
Control Group Mean	0.3933	0.2427	0.1506
Observations	15166	15166	15166
Bandwidth (in Days)	$\pm$240	$\pm$240	$\pm$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 Aug. 23, 1996 (Banned)	0.0088	-0.0669	0.0040	-0.0238	-0.0085
	(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 Days from Aug. 23, 1996	1	1	1	1	1
Panel B. Consistent Bandwidth of ±240 Days					
Offense Committed After Aug. 23, 1996 (Banned)	0.0062	-0.0560	0.0082	-0.0225	-0.0085
	(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 Days from Aug. 23, 1996	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. 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*	0.1046**	-0.0060
	(0.0532)	(0.0468)	(0.0316)
Offense Committed After Any Aug. 23 1996-2012	0.0040	0.0042	-0.0002
	(0.0094)	(0.0086)	(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 Aug. 23, 1996 (Banned)	0.0197	0.0919**	0.0415**	0.0549***
	(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 Days from Aug. 23, 1996	1	1	1	1
Panel B. Consistent Bandwidth of ±240 Days				
Offense Committed After Aug. 23, 1996 (Banned)	0.0110	0.0892**	0.0526**	0.0586***
	(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 Days from Aug. 23, 1996	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. 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 Aug. 23, 1996 (Banned)	-0.0071	0.1168**	0.3195**
	(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 Aug. 23, 1996 (Banned)	-0.0118	0.1067**	0.2464*
	(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 Aug. 23, 1996 (Banned)	0.0195	-0.0599	0.0876**
	(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 Days from Aug. 23, 1996	1	1	1
Panel B. Consistent Bandwidth of ±240 Days			
Offense Committed After Aug. 23, 1996 (Banned)	0.1136	-0.0223	0.1359**
	(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 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 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

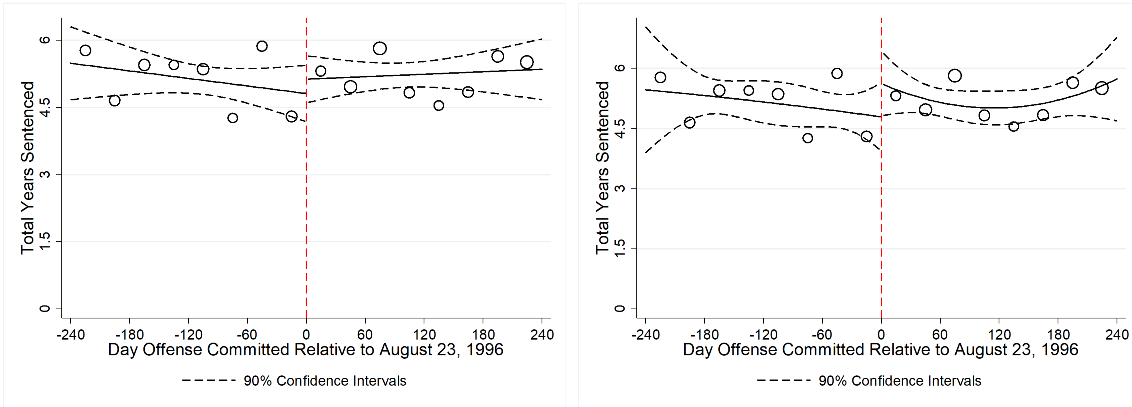
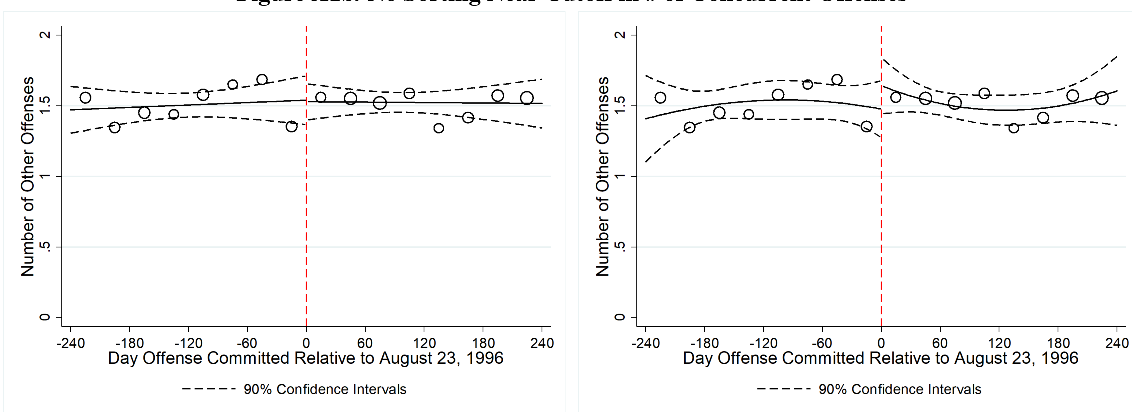


Figure A1b. No Sorting Near Cutoff in # of Concurrent Offenses



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

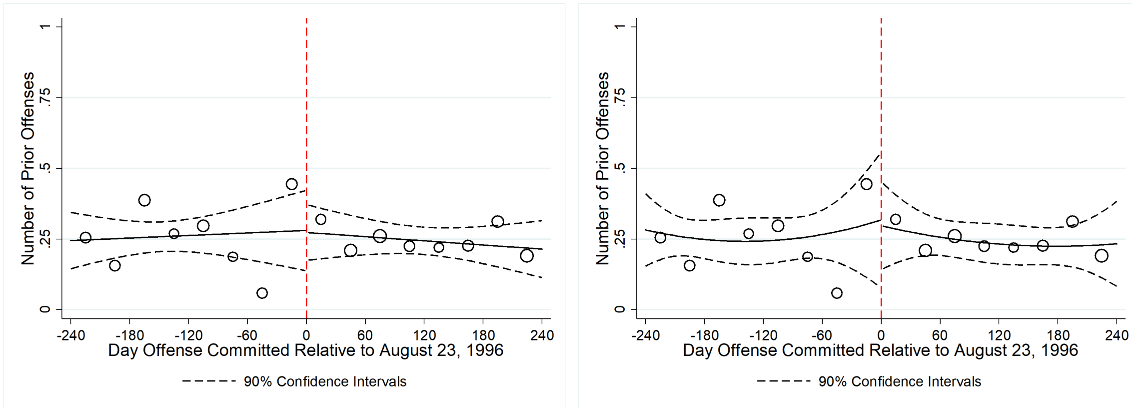
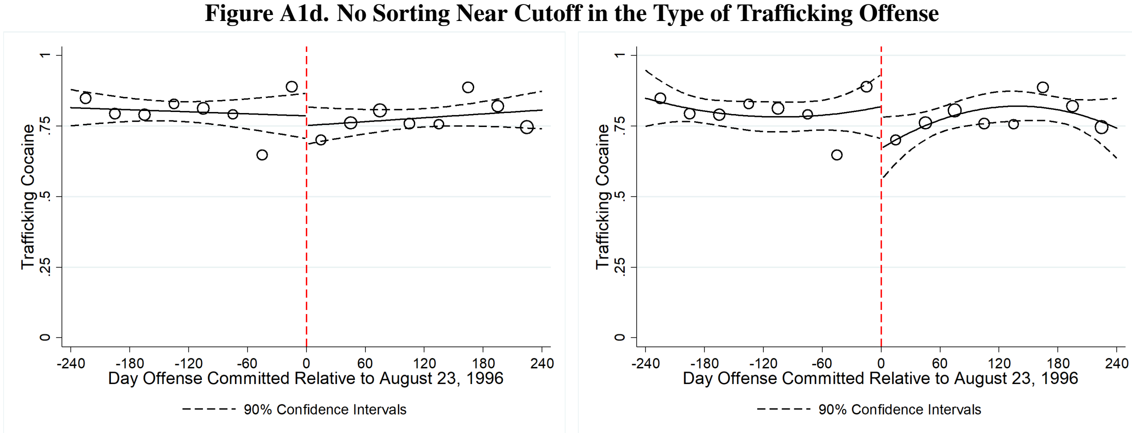


Figure A1d. No Sorting Near Cutoff in the Type of Trafficking Offense



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

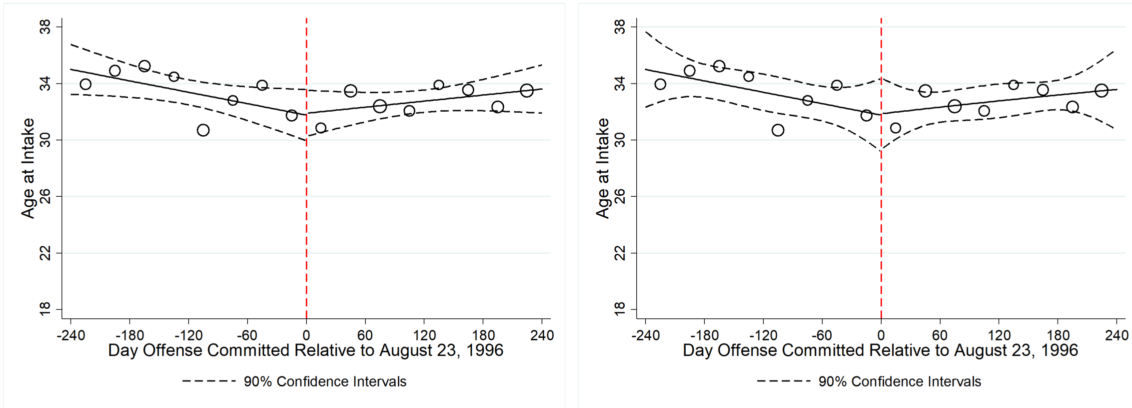
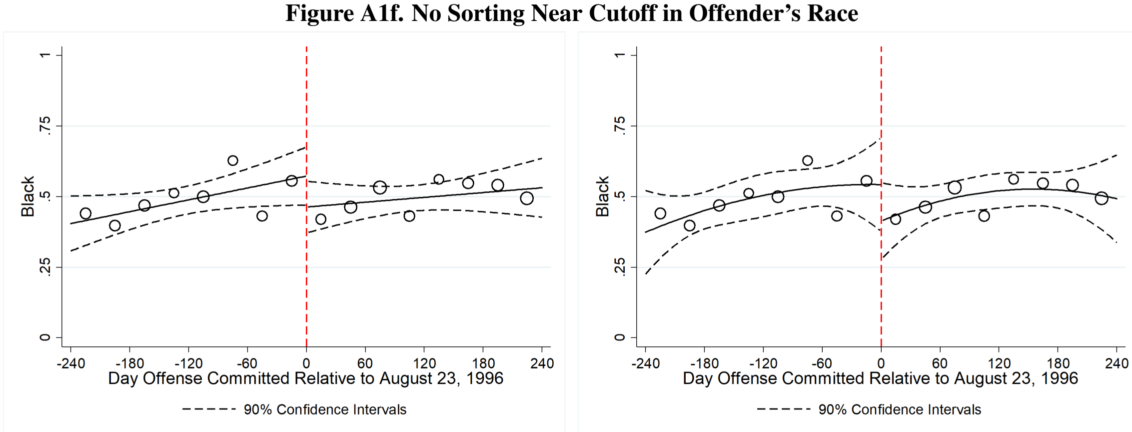


Figure A1f. No Sorting Near Cutoff in Offender's Race



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

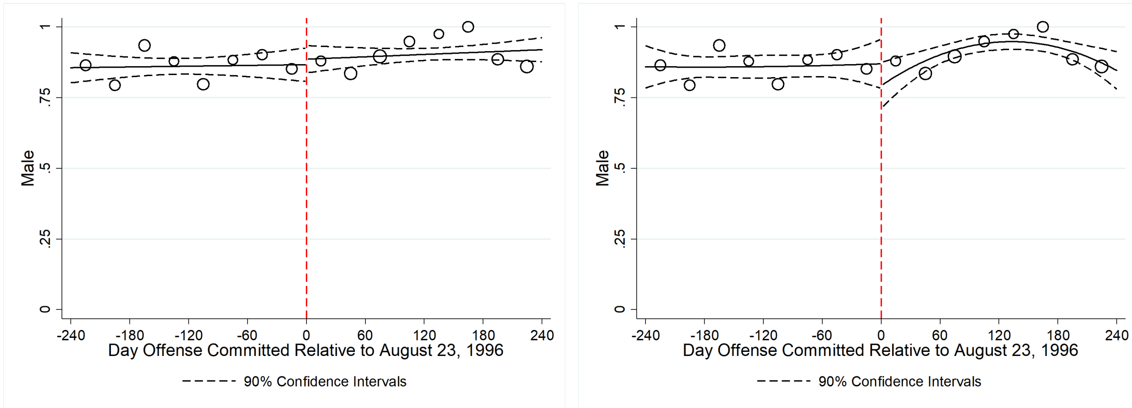
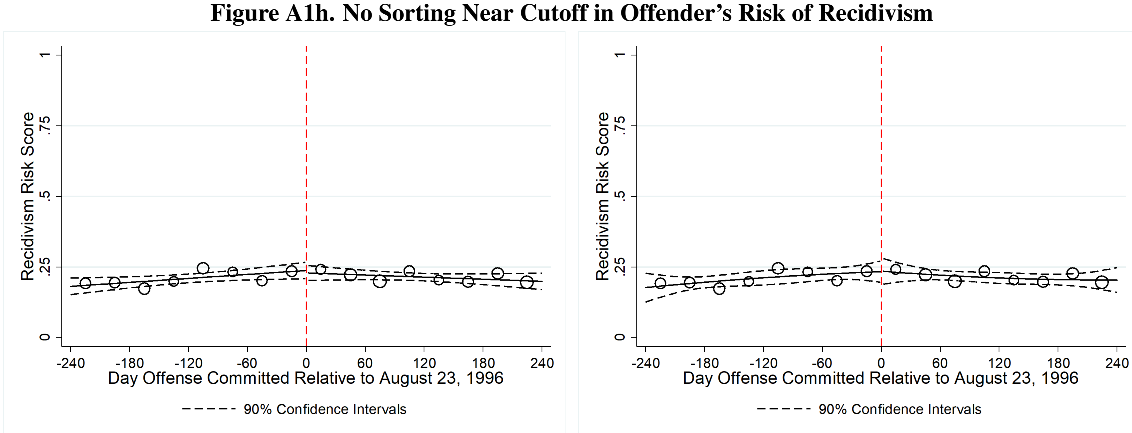
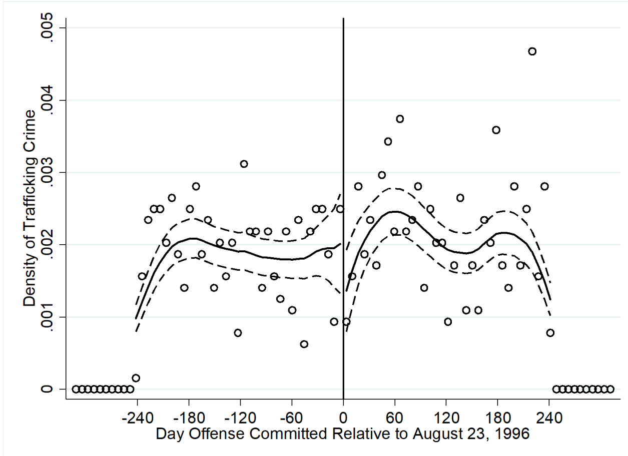


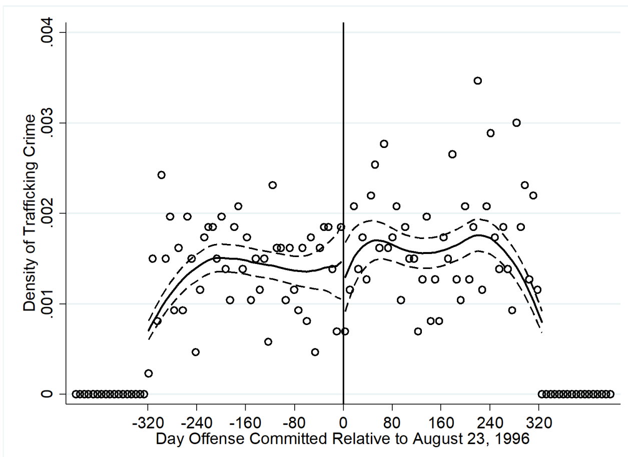
Figure A1h. No Sorting Near Cutoff in Offender's Risk of Recidivism



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

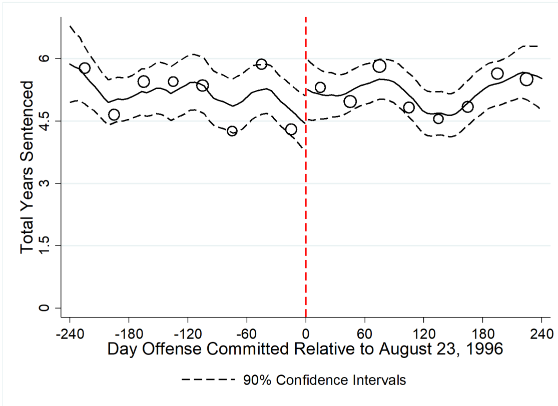


Figure A3b. No Sorting in # of Concurrent Offenses

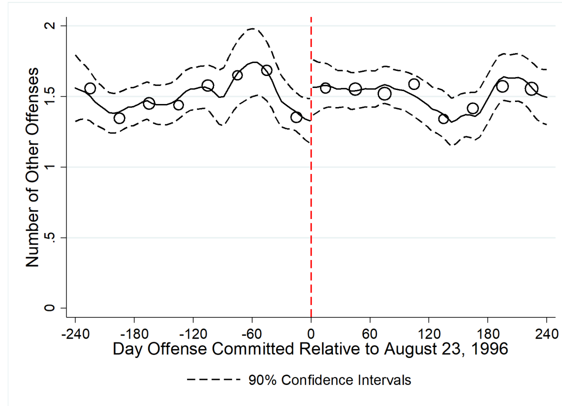


Figure A3c. No Sorting in Type of Trafficking

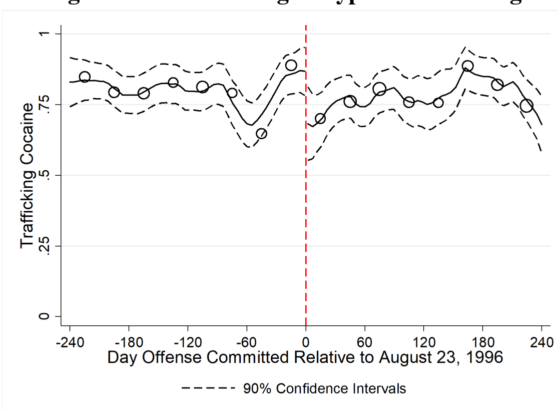
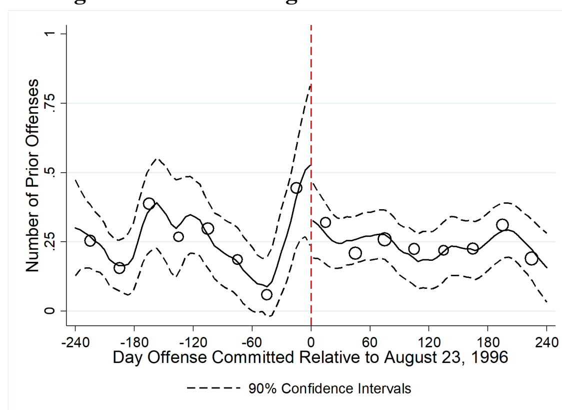


Figure A3d. No Sorting in # of Prior Offenses



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-Alb 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

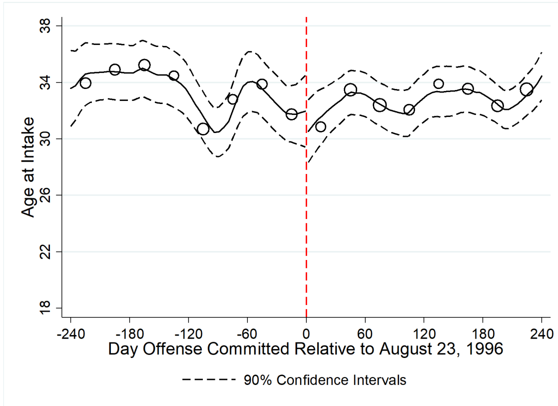


Figure A3f. No Sorting in Race

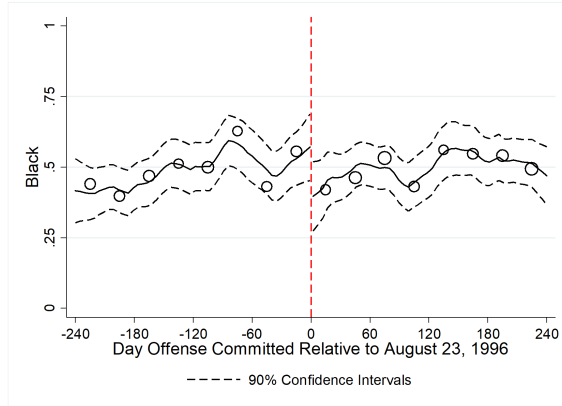


Figure A3g. No Sorting in Sex

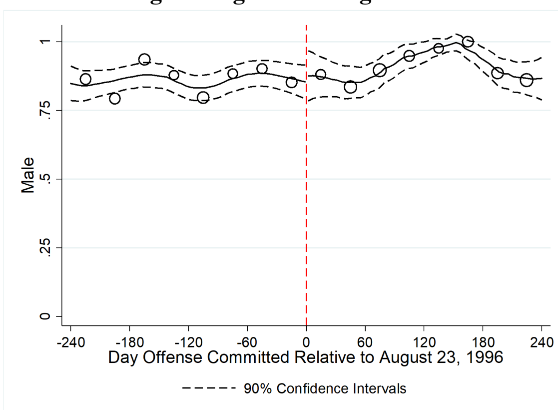
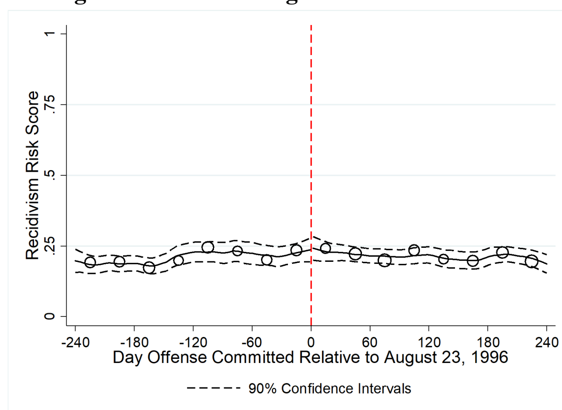


Figure A3h. No Sorting in Risk of Recidivism



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

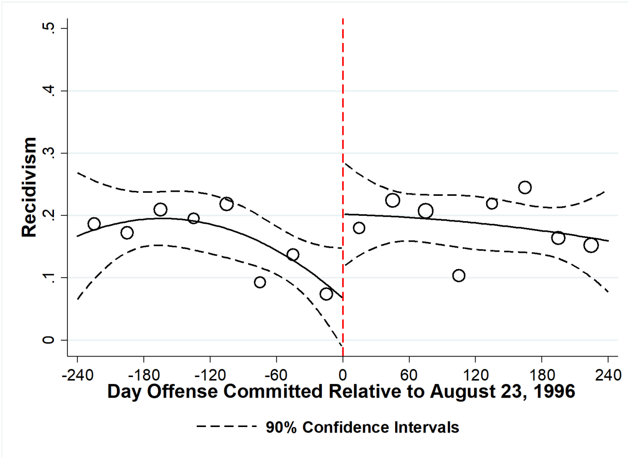


Figure A4b. Any Recidivism, Nonparametric

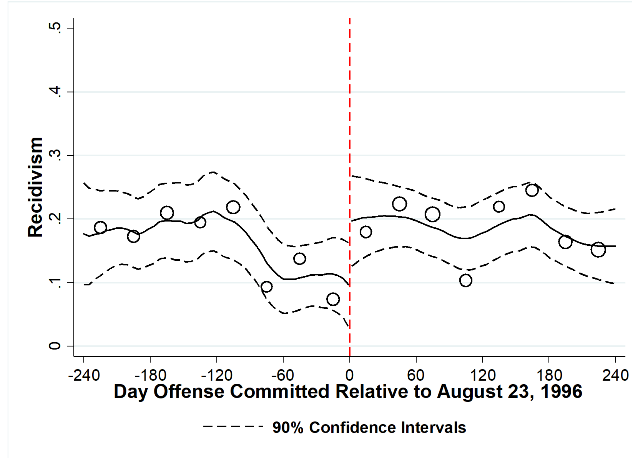


Figure A4c. Financial Recidivism, Quadratic

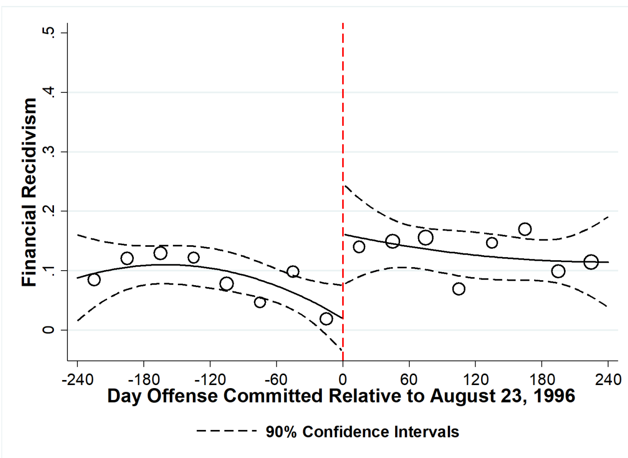


Figure A4d. Financial Recidivism, Nonparametric

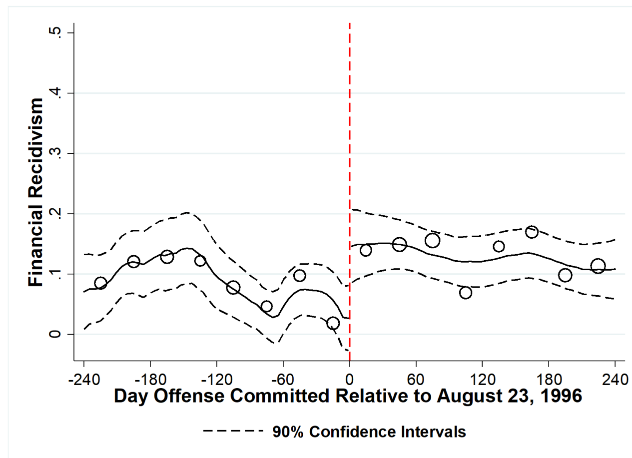


Figure A4e. Non-Financial Recidivism, Quadratic

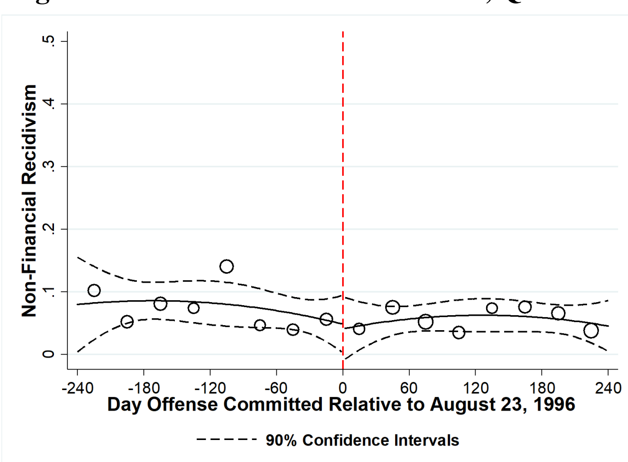
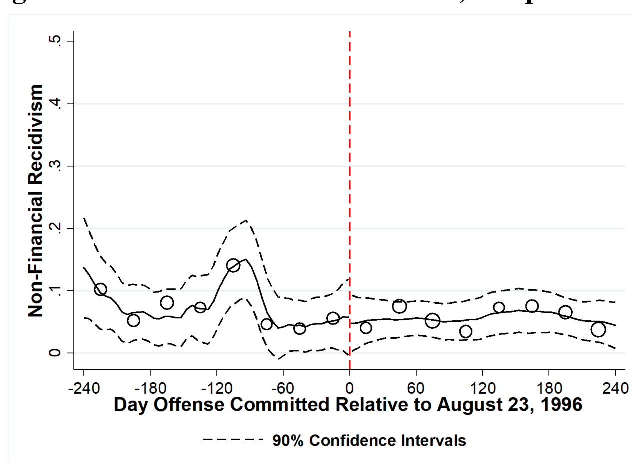


Figure A4f. Non-Financial Recidivism, Nonparametric



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.

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

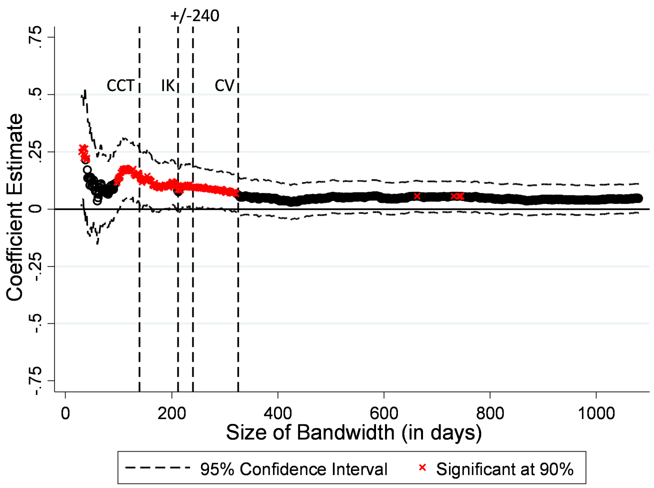


Figure A5b. Estimate of Effect over Many Bandwidths, Quadratic Polynomial

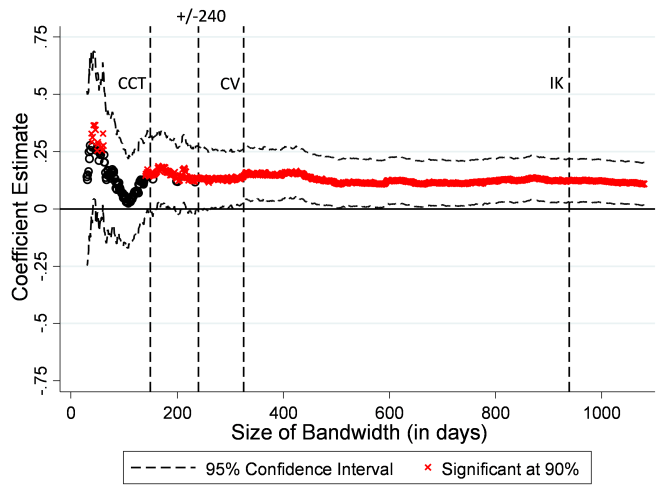
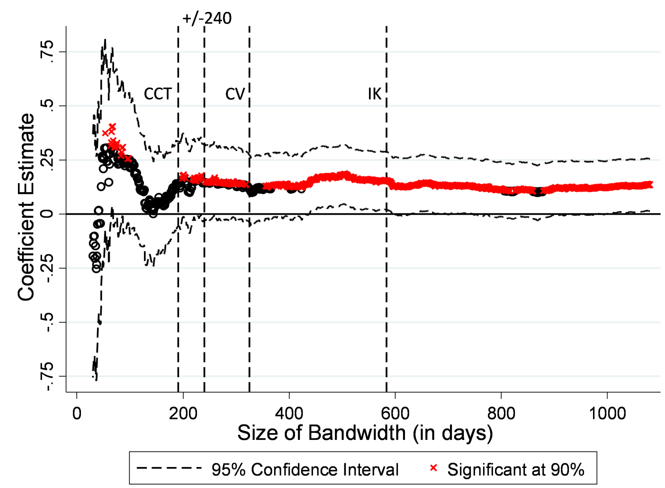


Figure A5c. Estimate of Effect over Many Bandwidths, Cubic Polynomial



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

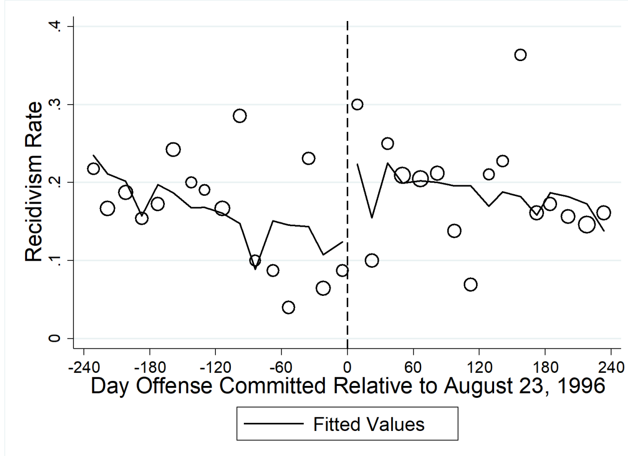


Figure A6b. Financial Recidivism

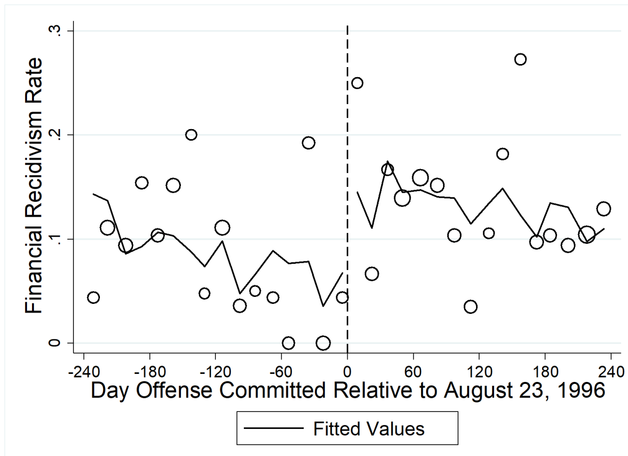
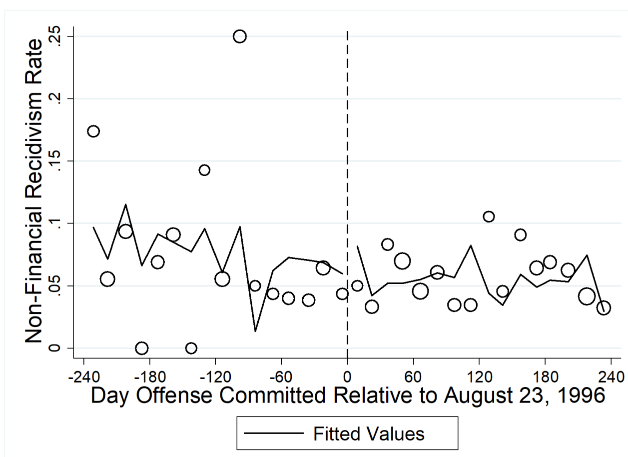
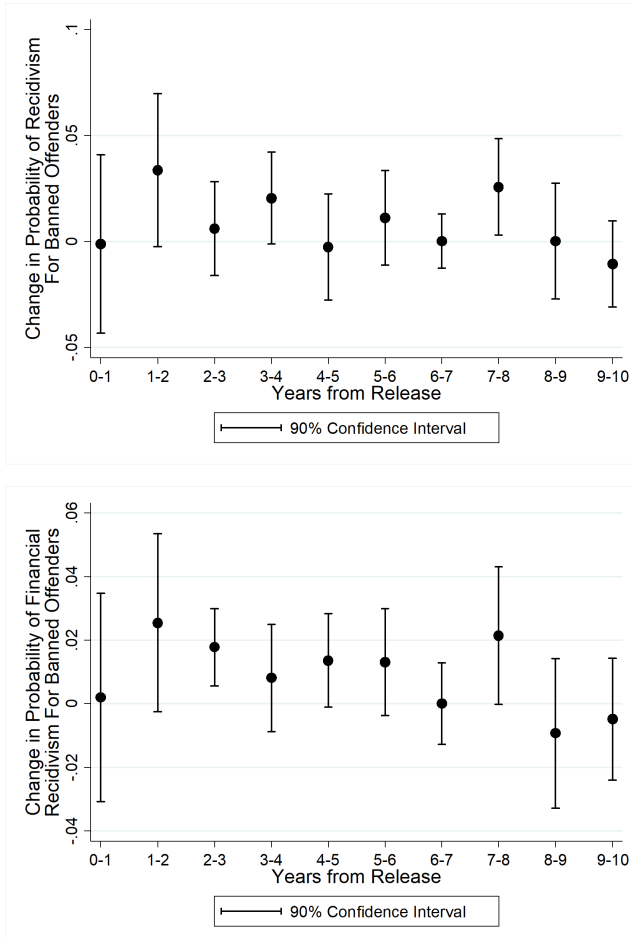


Figure A6c. Non-Financial Recidivism



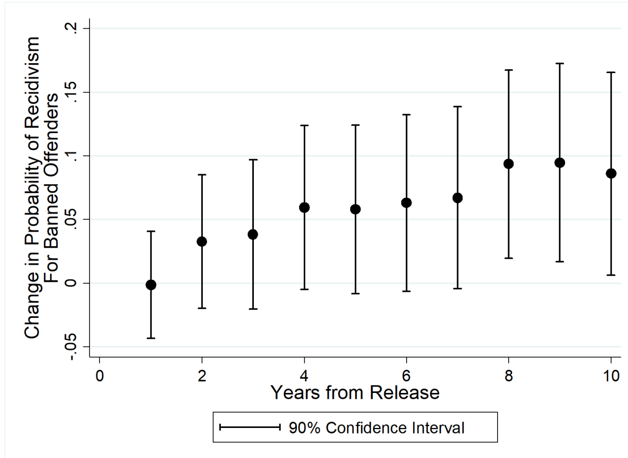
Notes: The figures above plot the lines of fitted values from time-series regressions modeling recidivism rates as an AR(1) process (number of lags chosen using the model with the highest AIC). All figures are overlaid with a scatter plot of the dependent variable averaged in 15-day bins. See Figures 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.

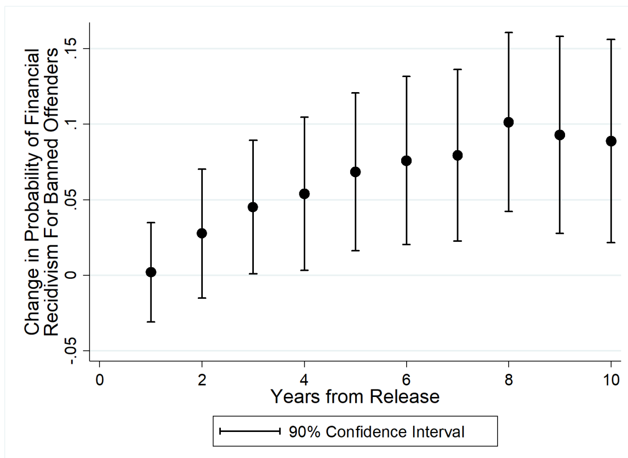
Figure A7. Effect of SNAP Ban on Timing of Re-incarceration



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.

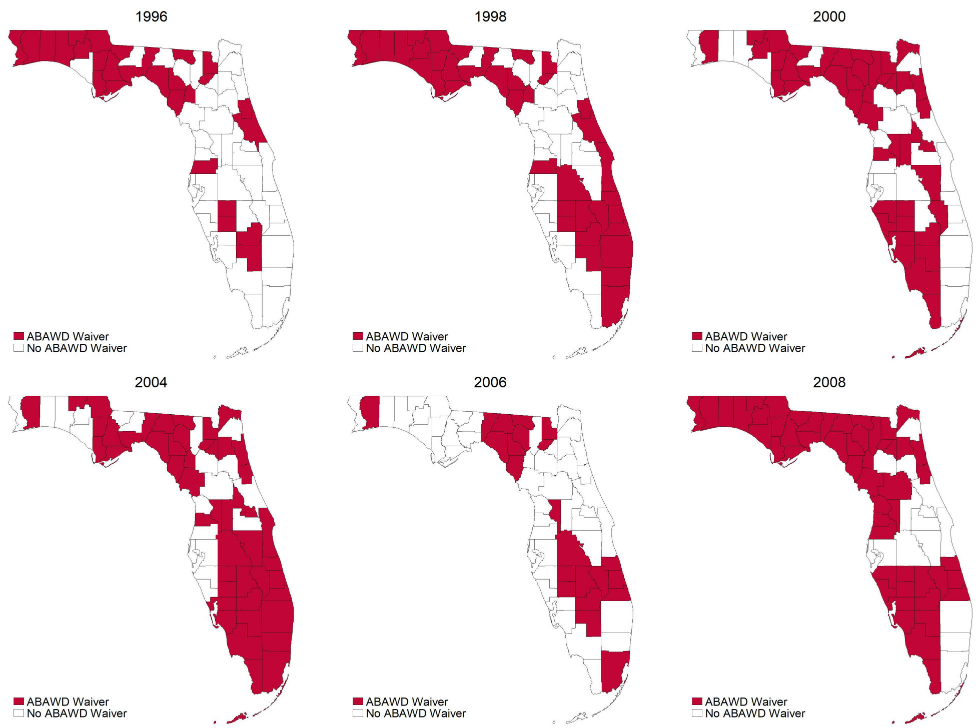
Figure A8. Effect of SNAP Ban on Timing of Re-incarceration, Cumulative





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 $\pm$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

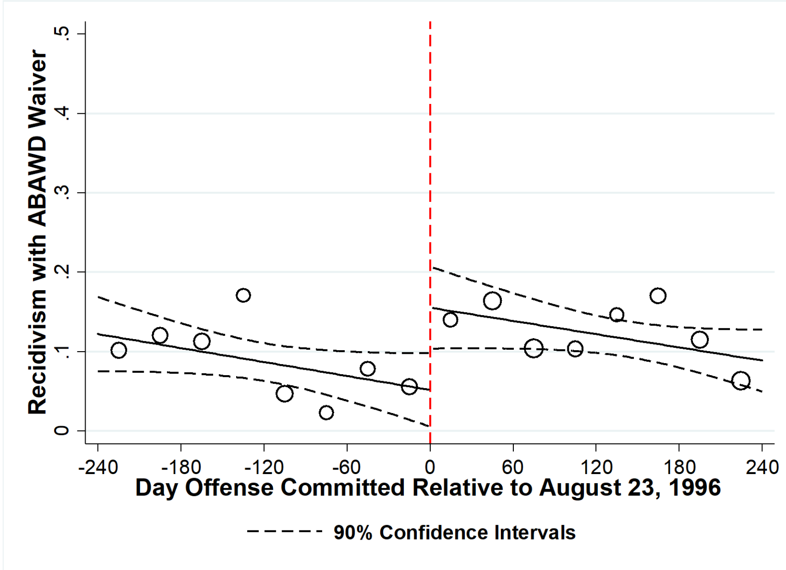
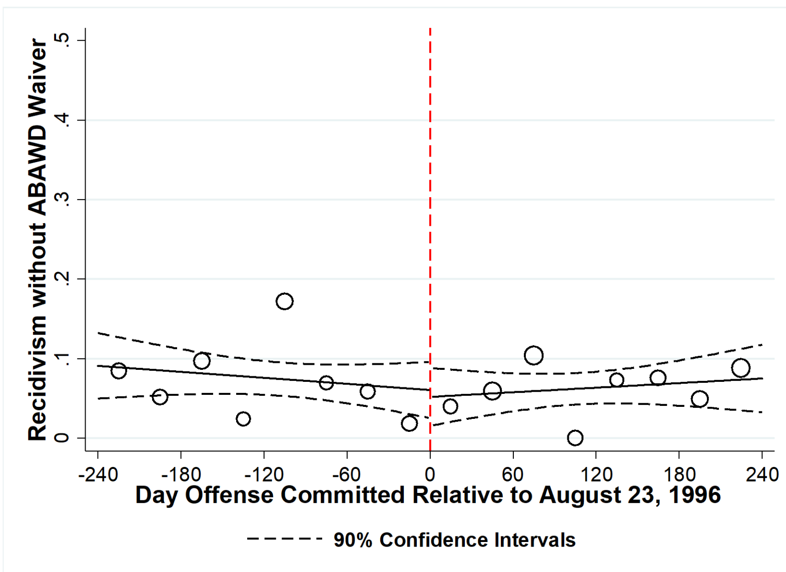


Figure A10b. Effect of SNAP Ban on Recidivism in Time/Place without ABAWD Work Waiver



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

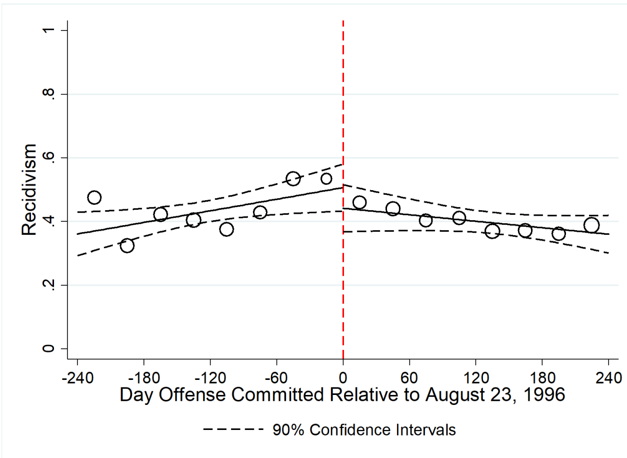


Figure A11b. Drug Possession

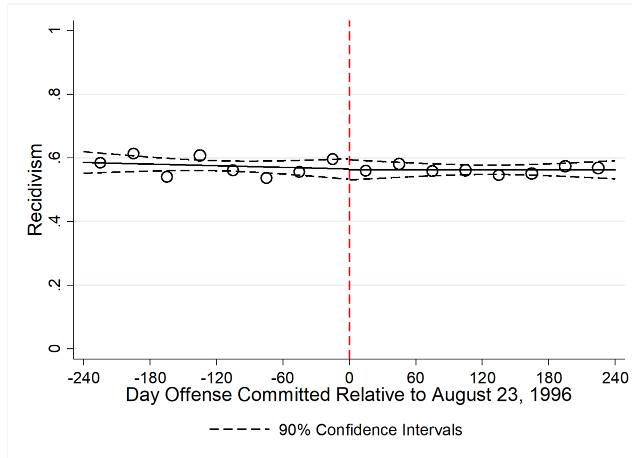


Figure A11c. Property Crime

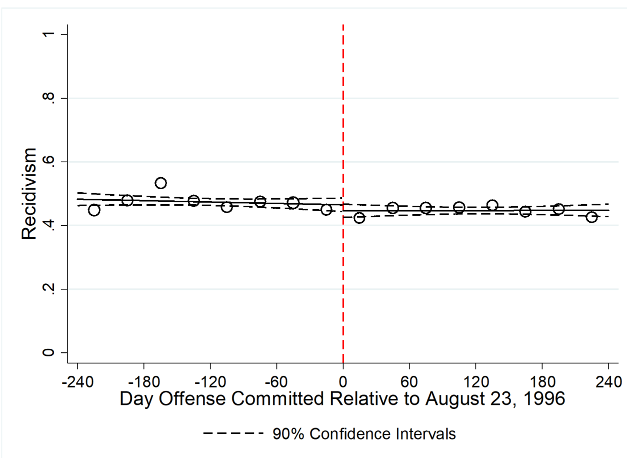
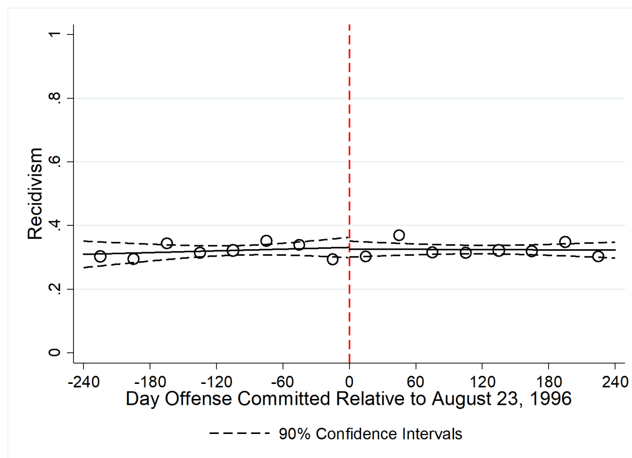
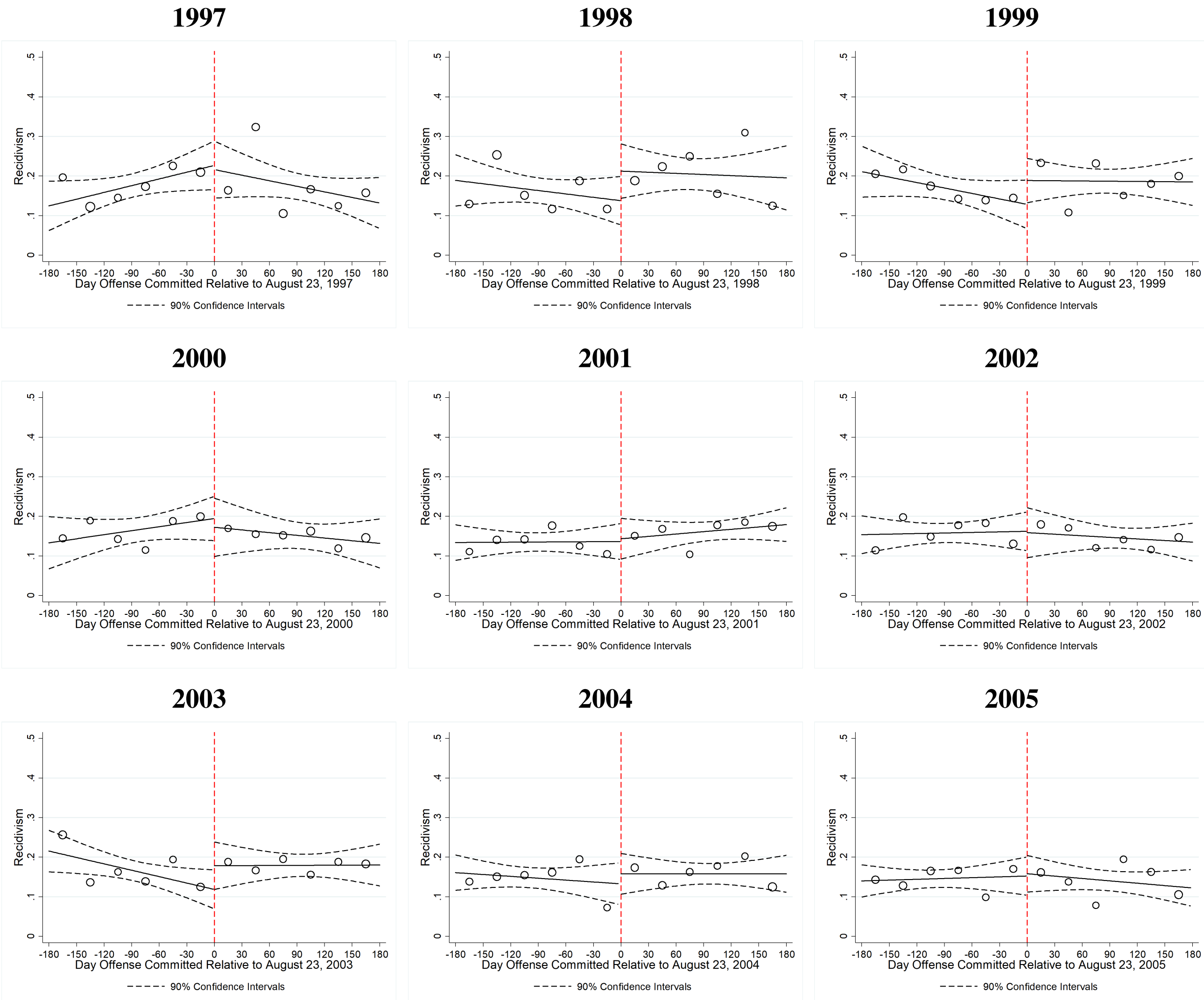


Figure A11d. Violent Crime



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.

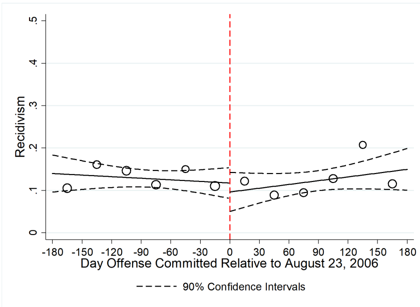
Figure A12. Drug Traffickers in Other Years are Not More Likely to Recidivate



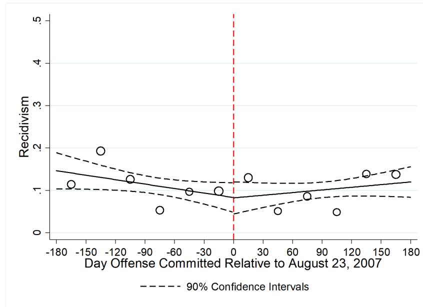
Notes: The figures above plot lines from local linear regressions of recidivism outcomes on the running variable (days before and after August 23 of a given year), estimated separately on each side of the cutoff. All figures are overlaid with a scatter plot of the recidivism averaged in 30-day bins. 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.

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

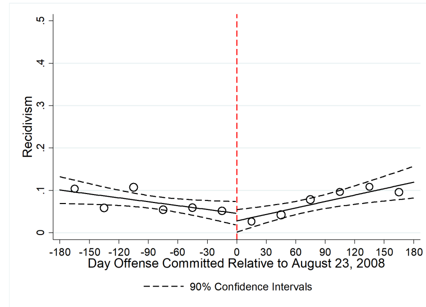
2006



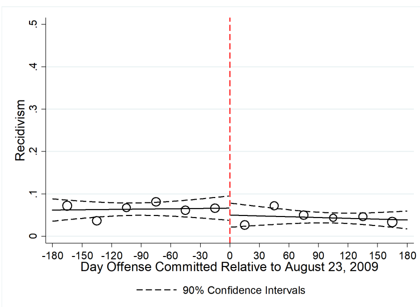
2007



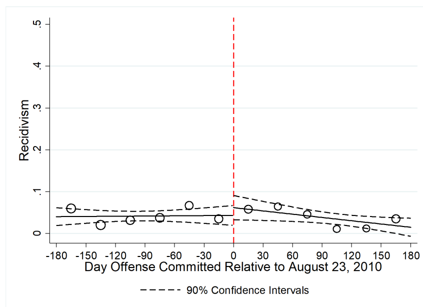
2008



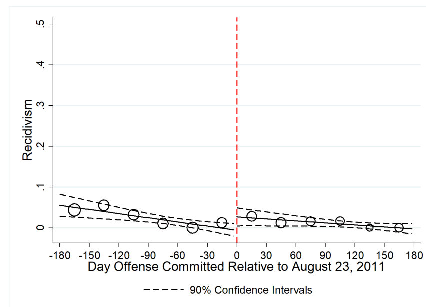
2009



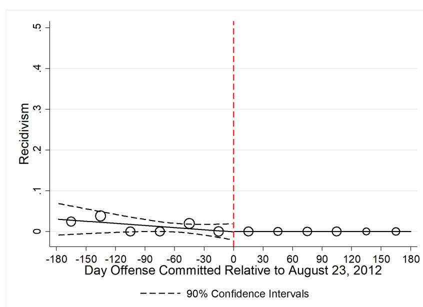
2010



2011

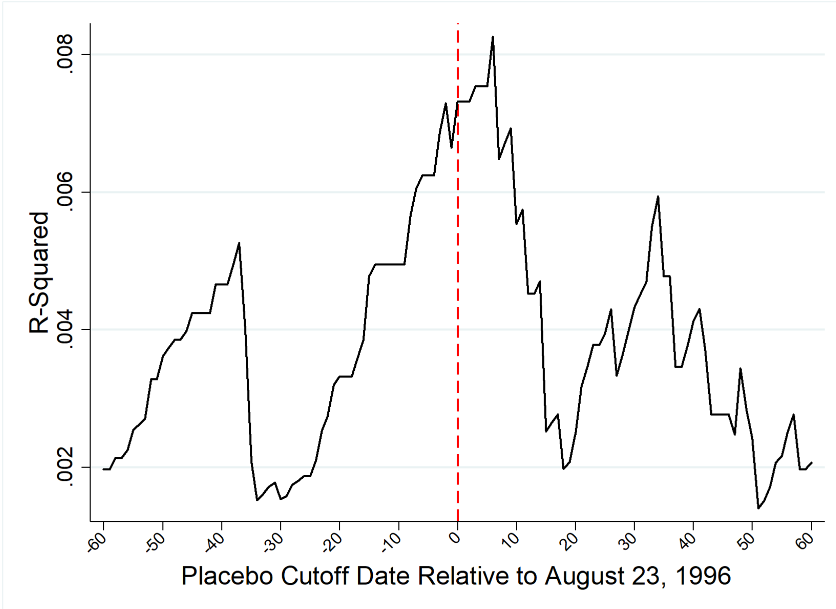


2012



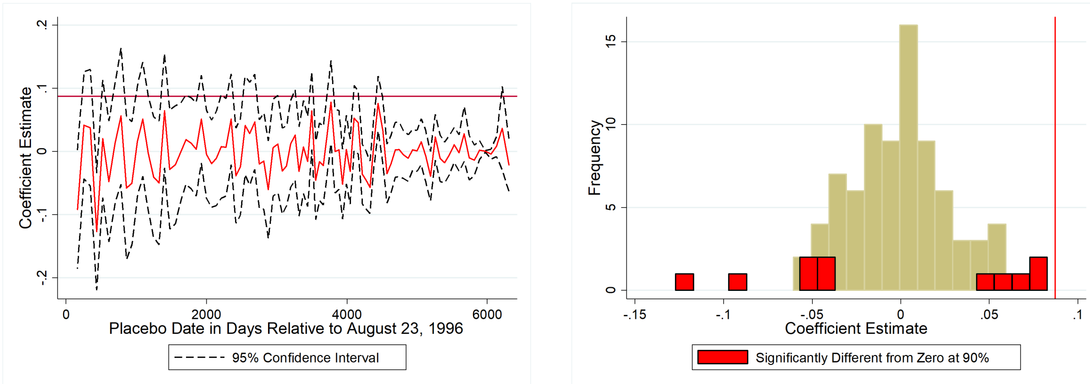
Notes: The figures above plot lines from local linear regressions of recidivism outcomes on the running variable (days before and after August 23 of a given year), estimated separately on each side of the cutoff. All figures are overlaid with a scatter plot of the recidivism averaged in 30-day bins. 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

[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

[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.

[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:

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

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 $\bar{c}$.

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

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:

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

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.

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

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:

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

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

[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-

[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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