# Behavioral Responses to Changes in Enforcement Priorities

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**Abstract**

A criminal behavior is often measured as a binary: whether or not a person commits a crime. The marginal deterrence theory expands the behavior to where a potential criminal chooses the degree of harmful activity along a spectrum of severity of punishment. An offender may progress from weapons possession to acts of intimidation to actual violence. This study empirically tests the effectiveness of criminal sanction in the context of the marginal deterrence. In 2013, the Justice Department of the United States instructed federal prosecutors to decline to charge the mandatory minimum sentence for drug criminals who do not possess a weapon. This policy lowers the expected punishment of a drug offense conditioning on the offender does not carry weapon. A rational criminal would choose not to carry weapon. The Regression Discontinuity Design estimates the probability of weapon possession of drug offenders fell by 11 percentage points in counties near the state line. This can be interpreted as a harsher sanction on more harmful activity lowers the degree of harmful activity that criminals choose.

**Keywords:** Marginal deterrence, Drug crime, Weapon, Holder Memo

**JEL Codes:** H75, I38, K42

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# 1 Introduction

Economic studies of crime has been testing and developing utility maximizing criminal model where criminal act is determined by the benefits and costs of crime. Despite it popularity, the empirical studies find different result from the theoretical prediction. For instance, a harsher sanction should deter future crimes of utility maximizing criminals by increasing the expected cost of crime. However, the deterrence literature finds no conclusive empirical evidence on the deterrence effect of criminal sanctions.[1] Tonry (2008) and Levitt & Miles (2007) point out that a series of enhanced criminal punishments during 1990s, such as three-strikes laws in California or zero-tolerance policing in New York City, do not always reduce criminal activities. Doob & Webster (2003) says "no consistent body of literature has developed over the last twenty-five to thirty years indicating that harsh sanctions deter".

These inconclusive results prevail in studies using a binary measure of criminal behavior: whether or not a person commits a crime (the "single-act" framework). In other words, economists have been testing whether a harsher sanction stops crime from happening. This single act framework, however, may be an extreme measure given that criminals can choose the degree of a harmful activity. A drug trafficker may choose how many drugs to smuggle or progress from trafficking to weapons possession to acts of intimidation to actual violence. When it comes to what is the socially affordable enforcement, the single act framework and the continuous measure of criminal behavior would draw a different conclusion.

This study incorporates an idea of criminal behavior where (1) realistically, offenders can choose the degree of illegal activity (2) the social cost of crime depends on the degree they choose. Stigler (1970) and Mookherjee & Png (1994) explain the idea of marginal deterrence where a potential criminal is choosing from a range of mutually exclusive actions along a spectrum of increasing severity.[2] Optimal enforcement then requires calibrating the gradient

[1] There had been a heated debate on the deterrence effect of capital punishment since mid-1990s (Dezhbakhsh et al. 2003, Donohue & Wolfers 2005, and Fagan 2006). Tonry (2008) argues that there is no credible evidence of deterrence effect of capital punishment. Katz et al. (2003) even finds that the execution increases homicide through brutalization effect.

[2] Shavell (1992) explains the notion of marginal deterrence quoting Bentham (1996): "to induce a man

of sanctions along the range of actions.

I show that government policy may influence the degree of a harmful activity. In 2013, Attorney General Eric Holder and President Barack Obama argued that the federal cost of imprisoning low-level drug offenders had increased beyond their control. In the United States, the number of inmates in Federal Bureau of Prisons (BOP) operated prison increased from 125,560 in fiscal year 2000 to 176,849 in fiscal year 2013. As a result, BOP's enacted budget increased by $6.8 billion in fiscal year 2014, 10% increase from fiscal year 2010.[3]. With the aim of promoting the efficiency of the federal justice system, the Justice Department of the United States implemented the so-called Smart on Crime Initiative, also known as the Holder Memo. The Holder Memo instructed prosecutors to decline to charge the mandatory minimum sentence for drug criminals who (1) do not use a credible threat of violence, (2) do not possess a weapon, or (3) do not belong to a criminal organization.

The Holder Memo provides a unique quasi-experiment for evaluating the criminals' choice of the degree of harmful activity when they face different levels of expected sanction between more harmful and less harmful acts. The Holder Memo satisfy the requirement of testing the marginal deterrence. Possession of weapon or not are adjacent on the range of possible criminal acts, with the weapon possession more severe act. The Holder Memo is an exogenous shock on the punishment for non-weapon possession, but does not change the punishment for weapon possession by the offender. For the empirical strategy, this study applies Regression Discontinuity Design (RDD) using the date of crime as the running variable. The main identifying assumption of RDD requires unobservable determinants of weapon possession are continuous around the cut-off date. I argue that the timing of the Holder Memo is an exogenous shock for criminals.

The results indicate that drug offenders significantly reduce weapon possession after the Holder Memo. The decline in weapon possession was largest in drug offenders in counties

to choose always the least mischievous of two offenses; therefore where two offenses come in competition, the punishment for the greater offense must be sufficient to induce a man to prefer the less."

[3]See Wroblewski & Hoffman (2015)

near the state lines (border counties): an 11 percentage point reduction. This can be interpreted as a harsher sanction on more harmful activity changes the degree of harmful activity that criminals choose. However, criminals' behavior toward the Holder Memo is asymmetric: no behavioral change when the Holder Memo repeals (in other words, no change when the expected sanction increases). On May 10, 2017, a new Attorney General Jeff Sessions reversed all guidelines of the Holder Memo set in 2013 and returned to impose the mandatory minimum sentence to drug offenders, which increases the expected cost of crime. The graphical evidence of RDD shows a modest changes in gradient of the linear projections in the border counties. However, such change is not statistically significant.

The result is robust under a hypothetical sample selection issue where federal law enforcement reduces their investigation on low-profile drug offenders. The purpose of the Holder Memo is to reduce the maintenance cost of retaining low-profile drug offenders in federal prisons. The inability to enforce the mandatory minimum sentence to non-violent drug offenders may disincentivize federal investigation and eliminates a large number of low-profile drug offenders in federal offender data. This study uses state inmate data to avoid the sample selection issue while exploiting the fact that drug criminals are subject to investigation by both federal and state law enforcement. However, federal investigators may hand over unarmed criminals to state investigators after the Holder Memo, which leads state data to contain more unarmed offenders. I use Dong (2019)'s method to estimate the lower and upper bounds of the estimated coefficients assuming unarmed drug offenders in the state data are transferred from the federal law enforcement. Even assuming this extreme sample selection issue, the results remain robust.

This findings contribute to the marginal deterrence literature designed by Stigler (1970). Despite its theoretical extension including Kramer (1990) and Wilde (1990), empirical evidence on marginal deterrence is in demand. One of the empirical studies in the marginal deterrence tests the effect of an enhanced sanction regarding murder on kidnapping. Detotto et al. (2015) finds that the enhanced sanction on kidnapping associating death decreases the

number of kidnapping resulted in death in Italy. Detotto et al. (2015)'s finding explains a sequential behavior of criminals in the context of marginal deterrence where a kidnapper commits a first crime (kinapping) and then later decide whether or not to murder the victim. Friehe & Miceli (2014) suggests another context of marginal deterrence where an offender chooses between two different acts given different levels of harm. This study empirically tests the non-sequential behavior of marginal deterrence: an offender can commit a drug crime with or without weapon, which leads the different levels of harm.[4]

In the following sections, Section 2 provides detailed information on the Holder Memo and some indirect evidence that individuals in Florida drug crimes were aware of the Holder Memo. Section 3 explains the details of OBIS data and benefits of using this data. Section 4 reports the main results using a Regression Discontinuity Design (RDD). The robustness of tests are performed in Section 6 including the upper and lower bounds of RDD estimate, placebo test, and sample period restriction. Lastly, Section 7 concludes.

# 2 The Holder Memo and Illicit Drug Market

## 2.1 What does exactly Holder Memo say?

The Holder Memo came into effect on August 12, 2013 with the aim of reducing the federal budget and also ensuring fair federal law enforcement. The five principles were: (1) setting priority of prosecutions on the most serious cases; (2) reforming sentencing guidelines to eliminate unjust disparities and reduce the burden of prison expenses; (3) providing alternatives to incarceration for low-level, non-violent offenders; (4) prevent recidivism and re-victimization; and (5) use more resources to prevent violence and secure the safety of the most vulnerable populations.

[4]Although not explained based the marginal deterrence theory, some studies find the add-on law, an enhanced sentence for criminals using firearms, affects the binary measure of criminal behavior. Abrams (2012) finds the sentence enhancement on the possession of weapon deter gun robbery by 5 percent. Marvell & Moody (1995) attempts to answer whether the mandating minimum sentences for crimes with guns reduce crime and finds modest reduction in a few states.

Specific instructions in Holder (2013) are as follows:

> [I]n cases involving the applicability of Title 21 mandatory minimum sentences based on drug type and quantity, prosecutors should decline to charge the quantity necessary to trigger a mandatory minimum sentence if the defendant meets each of the following criteria:
>
> - The defendant's relevant conduct does not involve the use of violence, the credible threat of violence, the possession of a weapon, the trafficking of drugs to or with minors, or the death or serious bodily injury of any person
> - The defendant is not an organizer, leader, manager or supervisor of others within a criminal organization;
> - The defendant does not have significant ties to large-scale drug trafficking organizations, gangs, or cartels; and
> - The defendant does not have a significant criminal history. A significant criminal history will normally be evidenced by three or more criminal history points but may involve fewer or greater depending on the nature of any prior convictions.
>
> Prosecutors should decline to file an information pursuant to 21 U.S.C. § 851 unless the defendant is involved in conduct that makes the case appropriate for severe sanctions. (Similar to Title 21 mandatory minimum sentences)

I choose the possession of a weapon as the main behavioral variable for the criminals. Hulsey (2015) provides charging and sentencing recommendations for the Holder Memo. G. Scott Hulsey, Assistant United States Attorney, argues that in applying the Holder Memo, the criminals' possession of a weapon or engaging in violence are self-evident and do not need for extensive discussion as to whether or not the Holder Memo is applicable, whereas the meaning of others, such as "significant ties to large-scale drug trafficking organization" are

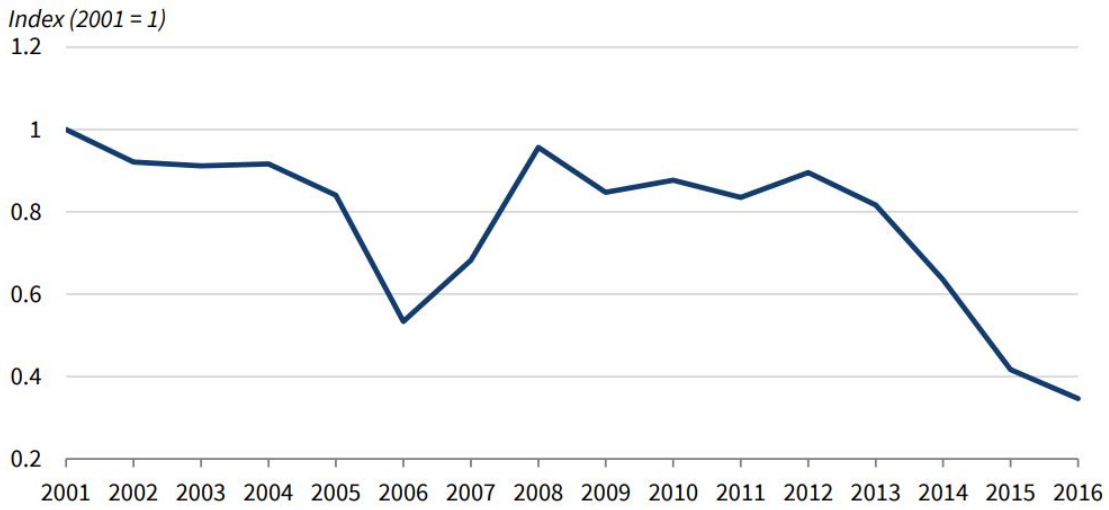
less obvious. Criminals leaving their organization after the Holder Memo can be an excessive assumption and not likely to happen.

## 2.2 Impact on Illicit Drug Market

We can infer the behavior of drug offenders from how the drug market has changed since the Holder Memo came into effect. Mulligan (2020) analyzes the behavior of drug dealers after the Holder Memo through the illicit drug market. The main argument in the paper is that the 2013 Holder Memo reduces the cost of crime for drug offenders because the federal prosecutors no longer investigate non-violent drug related crimes. For criminals, prison time is a considerable cost. Therefore, if the cost is reduced after the Holder Memo, such reduction affects the price and purity of drugs.

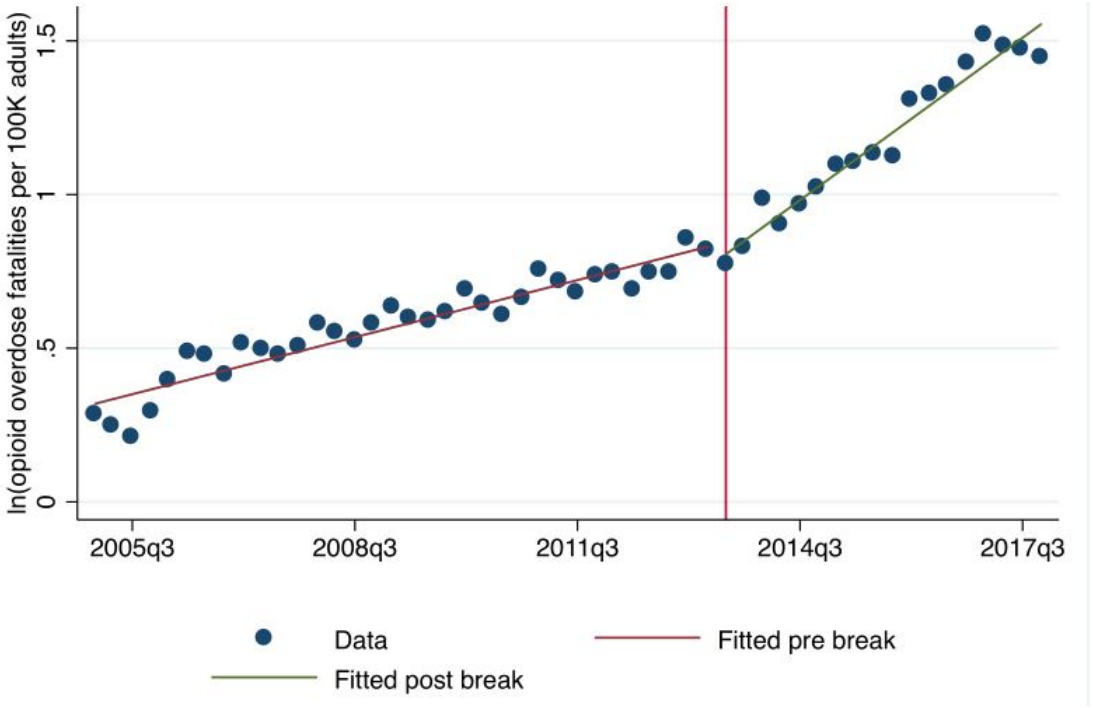
Murphy et al. (2006) argues that for the drug industry with low price elasticity, a stronger punishment on criminals increases the input cost of drug suppliers and results in a rise in drug prices in the market. On the contrary, the Holder Memo lowers the likelihood of getting a punishment for drug suppliers, which in turn leads to a decrease in the market drug price.

Figure 1: Real Price Index of Potency-Adjusted Illicit Opioids, 2001-2016




Source: The Council of Economic Advisors, CEA (2019)

Figure 2: 2013 Q3 A Structural Break For Opioid Fatalities




Source: Mulligan (2020)

The analysis of Mulligan (2020) shows that the inflation-adjusted price of illegally produced opioids fell by 9% in 2013 compared to the prices of 2008 and 2012. According to a report released by The Council of Economic Advisors, the price of potency-adjusted illicit opioids has dropped sharply since 2013 (Figure 1). Mulligan (2020) claims that the Holder Memo caused a structural break at the start of the drug's popularity. For example, Figure 2 shows that the mortality rate from opioid use has increased dramatically since the third quarter in 2013.

In conclusion, according to the analysis by Mulligan (2020), the decline in market prices and the increase in the number of illicit drugs circulating in the market since the Holder memo have been attributed to reduced costs for drug offenders. Reducing costs for drug crimes is only applicable when these offenders choose not to use a weapon or violence. A testable hypothesis is whether the Holder Memo gives drug offenders incentives not to carry

a weapon.[5]

## 2.3 Are criminals aware of the Holder Memo?

Although there is no survey on whether criminals are aware of the Holder Memo, it is reasonable to assume that the more information shared, the more it spreads. Empirical studies suggest that criminals are relatively well aware of the risk of committing crime. Herrnstein & Wilson (1985) shows that offenders are better informed of the risk of imprisonment. Using survey data of over two thousand inmates in jails and prisons in California, Michigan and Texas, Herrnstein & Wilson (1985) finds a close correspondence between the actual and perceived risk of imprisonment in Michigan and Texas.

I focus my analysis on the behavior of drug offenders in Florida which is the most active state in information sharing and practicing the Holder Memo. Table 1 shows the percentage of drug offenders sentenced with no mandatory minimum sentence increase is the highest in Florida. The actively practicing Holder Memo requires developing and updating any district policies or other guidance documents to reflect the policy changes. The more information sharing there is in the state, the more likely such information flows to criminals.

Table 1: Percentages of Drug Offenders Sentenced with No Mandatory Minimum, by Region FY 2012 – FY 2014

| Region | FY 2012 | FY 2014 | %Δ |
|---|---|---|---|
| Southwest Border | 47.00% | 59.80% | 27% |
| South | 43.40% | 55.30% | 27% |
| East | 42.90% | 52.60% | 23% |
| Pacific Northwest | 36.60% | 49.70% | 36% |
| Midwest | 32.30% | 42.60% | 32% |
| West | 30.70% | 41.30% | 35% |
| California | 28.00% | 38.40% | 37% |
| ***Florida*** | ***27.00%*** | ***40.70%*** | ***51%*** |
| Hawaii and Island Territorie | 21.00% | 22.10% | 5% |

Source: Office of Inspector General (OIG) analysis using United States Sentencing Commission (USSC) data OIG (2017)

[5]In fact, Quinones (2015), an investigation of the unprecedented spread of heroin addiction, describes the guidance of the heroin business model emphasizes as "never violent" and drug dealers don't carry guns.

Certain drug crimes are automatically federal drug crimes. When a drug crime occurs on federal property, whether it is a military base or a national park, it is a federal drug crime and will be tried as such. If a drug crime involves crossing state borders, it will also be tried as a federal drug crime.

Figure 3: Counties in FL


Drug offenders near state lines transport and sell drugs to other states. The counties near the state line in Florida are Baker, Columbia, Escambia, Gadsden, Hamilton, Holmes, Jackson, Jefferson, Leon, Madison, Nassau, Okaloosa, Santa Rosa, and Walton County. These 15 counties take up 7% of Florida's population in 2007 and 2013. However, Table 2 shows that the cases of acquisition of illicit drug by the federal agent is almost double

[6]The fraction is multiplied by 1000

Table 2: Federal vs State

| | (1) Border Counties | (2) Non-Border Counties |
|---|---|---|
| $\frac{\text{Federal Drug Acquisition Cases 00-07}}{\text{County Pop 07}}$ [6] | 0.110 | 0.057 |
| | (0.116) | (0.148) |
| $\frac{\text{Federal Drug Acquisition Cases 00-07}}{\text{Num of Drug Offenders in OBIS 00-07}}$ | 0.317 | 0.189 |
| | (0.398) | (0.488) |
| Population 2007 | 189,682 | 923,449 |
| | (113,305) | (664,698) |
| Observations | 8,160 | 77,256 |

than other counties. According to The System to Retrieve Information from Drug Evidence (STRIDE) data, acquisitions of illegal drugs by undercover agents and DEA informants is .110 per capita in border counties, whereas .057 in non-border counties.[7] Although the measurements are not identical, the federal drug acquisition –proxy for the degree of federal drug investigation– divided by the number of offenders committing drug offense –proxy for state investigation– is also higher in border line counties than other counties.

[7]STRIDE data set is collected by Drug Enforcement Administration (DEA) containing information on drug seized by the federal agents. The dataset includes information on the amount of seized drugs, the seizing location, the total amount paid when the transaction involved a purchase, the type of illicit substance, etc. The STRIDE dataset is agent and city-level data, which does not have sensitive personal information.

Table 3: State Investigation

| | 2000-2007 | | 2010-2012 | |
|---|---|---|---|---|
| | Border | Non-Border | Border | Non-Border |
| All Drug Offense | 0.327 | 0.372 | 0.282 | 0.318 |
| | (0.469) | (0.483) | (0.450) | (0.466) |
| Selling/Mfg/Dist. Drug Offense | 0.161 | 0.172 | 0.149 | 0.158 |
| | (0.367) | (0.377) | (0.356) | (0.365) |
| Drug Trafficking Offense | 0.029 | 0.049 | 0.037 | 0.062 |
| | (0.168) | (0.215) | (0.190) | (0.242) |

Source: Florida Offender Based Information System (OBIS)

Table 2 may indicate that the counties near the state border line are generally drug crime prone areas. Table 3 shows that the proportion of each crime recorded by the state in border and non-border counties. The table shows that border counties actually have a lower proportion of drug crime than other counties. This is either because the drug crime rate is actually low or because much of the drug crime is being investigated by the federal government, not by the state.

# 3 Data

The Florida Department of Correction (FL DOC 2017) maintains OBIS. This data is publicly available and provides state prison inmate information including types of offense, date of offense, gender, race, year sentenced, and prison release date for every inmate in a Florida state prison or under state supervision since October 1997. The number of observations is 1,178,138 and observations in 2013, the year the Holder Memo was implemented, are 45,520. The data includes information on multiple charges for the offenders, thus I can identify if a

drug offender carries a weapon or not at the crime.[8] Drug crimes include drug possession, drug sales, manufacturing, distribution (SMD), and drug trafficking. The data contains sensitive information such as inmates' names and unique identifiers for cases and inmates. I delete the name of the prison inmates and change the existing ID number to a randomly generated number before the analysis begins.

The advantage of using OBIS is that it is less affected by potential selection bias caused by the Holder Memo. The data only includes offenders sentenced to state prison or under state supervision. The use of data with federal criminal records causes sample selection bias because the Holder Memo limits the investigation of non-violent drug crimes to federal investigators. Potentially, federal data will contain less the number of unarmed drug offenders, not because of actual changes in criminals' behavior, but because fewer investigations into drug offenders who do not carry weapons under the Holder Memo guidelines. Under the U.S. dualism justice system, state law enforcement authorities are not directly subjected to the Holder Memo. However, state-level data may have issues with sample selection due to the cooperative nature between state and federal investigators. Many drug crimes are subject to both federal and state investigations, and which crimes are subject to federal investigation is largely determined by the judgment of the federal prosecutors. The Holder Memo prevented imposing the mandatory minimum sentence for violent drug offenders and additional penalties to ex-felons, which potentially reduced incentives for federal agents to investigate less serious drug crimes. It is also possible that marginal criminals will be investigated by state agents after the Holder Memo. I will discuss more on the sample selection issue in Section 6.

I restrict sample duration to two years before and after the Holder Memo starts, although the sample period of OBIS data begins in 1997. This is due to the characteristics of the running variable. In Regression Discontinuity in Time (RDiT), where time is used as running variable, increasing the number of samples causes some problems. Hausman & Rapson (2018)

[8]Criminals can have weapons-related charges for various reasons such as possession of weapon, manufacturing, or selling weapon. According to the 2013 records of OBIS data, 97% of the weapon charges are possession related. "Possession of a Firearm, Ammunition, or a Concealed Weapon by a Convicted Felon" is the highest at 80%, followed by "Carrying Concealed Firearm" at 17%

mentions that increasing the duration of a sample in RDiT is likely to cause bias, especially in the presence of an auto-regressive process or disparity in short-run and long-run effects. I also see that the MSE-optimal bandwidth is estimated to be over 500 days long when the sample period is not restricted, while 267 days for the restricted case. A period of more than 500 days may cause bias in coefficients as indicated by Hausman & Rapson (2018). The same sample restriction applies when the Holder Memo rescinds on May 10th, 2017. I restricted the duration of the sample year to two years before and two years after. The unrestricted results are Table 10 in Section 6.3.

Table 4 shows summary statistics in the restricted sample period. The variables include weapon possession indicator, race, age, gender, and sentenced year. The table reports mean and variance of these variables of all counties and border line counties. Panel A shows the summary statistics before and after the Holder Memo initiated. Some variables differ before and after the Holder Memo. The probability of weapon possession generally increases after the Holder Memo, indicating a possibility that the Holder Memo may not deter criminals carrying weapon. However, it may simply catch the upward trend of weapon possession. After the Holder Memo, the proportion of black offenders increased. For border counties, it increases from 0.489 to 0.455. In addition, the number of samples in the border counties that are tentatively heavily affected by federal investigations increased from 1611 to 1720. This suggests the possibility that federal investigators have decided not to pursue cases of several drug offenders and state investigators took these offenders after the Holder Memo. Similarly, in Panel B, the number of samples of the border counties have increased after the Holder Memo rescinded. The number of years sentenced in border counties decreased by 0.4 years (or 4.8 months) since the beginning of Holder Memo. This can be seen as federal policy influencing state law enforcement authorities imposing weaker penalties on drug offenders, but the sentence years after the Holder Memo repeal (Panel B) has not rebounded, but has decreased as well. Although differences in variables before and after the Holder Memo may be simply due to trends, I will address the risk of selection bias after the implementation of

the policy.

# 4 Empirical Analysis

## 4.1 Methodology

For the empirical method, I use a Regression Discontinuity Design (RDD) that utilizes the characteristics of data and the Holder Memo. OBIS includes the date of which the drug crime was committed, and the Holder Memo applies after August 12, 2013. I use the date as a running variable to address the possible effect of the Holder Memo. The estimating sample only includes drug crimes. The estimating equation is as follows.

$$\mathrm{Y}_{it} = \alpha + \beta After_{it} + g(\mathrm{DaysFromCutoff}_{it}) + g(\mathrm{DaysFromCutoff}_{it}) \times After_{it} + \epsilon_{it} \qquad (1)$$

$Y_{it}$ is a dummy variable that equals to 1 if the offender possesses a weapon. If $Y_{it}$ equals zero, the crime is a drug crime but the criminal did not possess weapon. In other words, a sample of this analysis contains only offenders who committed drug-related crimes. $i$ represents the criminal and $t$ is time by day. The variable $After$ is an indicator variable that is equal to 1 if $t$ is after the Holder Memo was enforced, August 12, 2013. $g$(DaysFromCutoff) represents the difference between the date the offender $i$ committed the crime and August 12, 2013: RDD's running variable. The interaction term, $g(\mathrm{DaysFromCutoff}_{it}) \times After_{it}$ allows running variables and drug-related violent crimes to change before and after cutoff. Control variables are not included in this empirical analysis. Heteroskedasticity-robust error are used to estimate variance-covariance matrix.

Bandwidth selection in RDD analysis is a critical part, and this study uses one common MSE-optimal bandwidth selector proposed by Calonico et al. (2014a) and Calonico et al. (2014b). I use the first-degree polynomial function in the analysis given the graphical observation. However, I will show that the main analysis in alternative bandwidths and polynomials.

Table 4: Summary Statistics

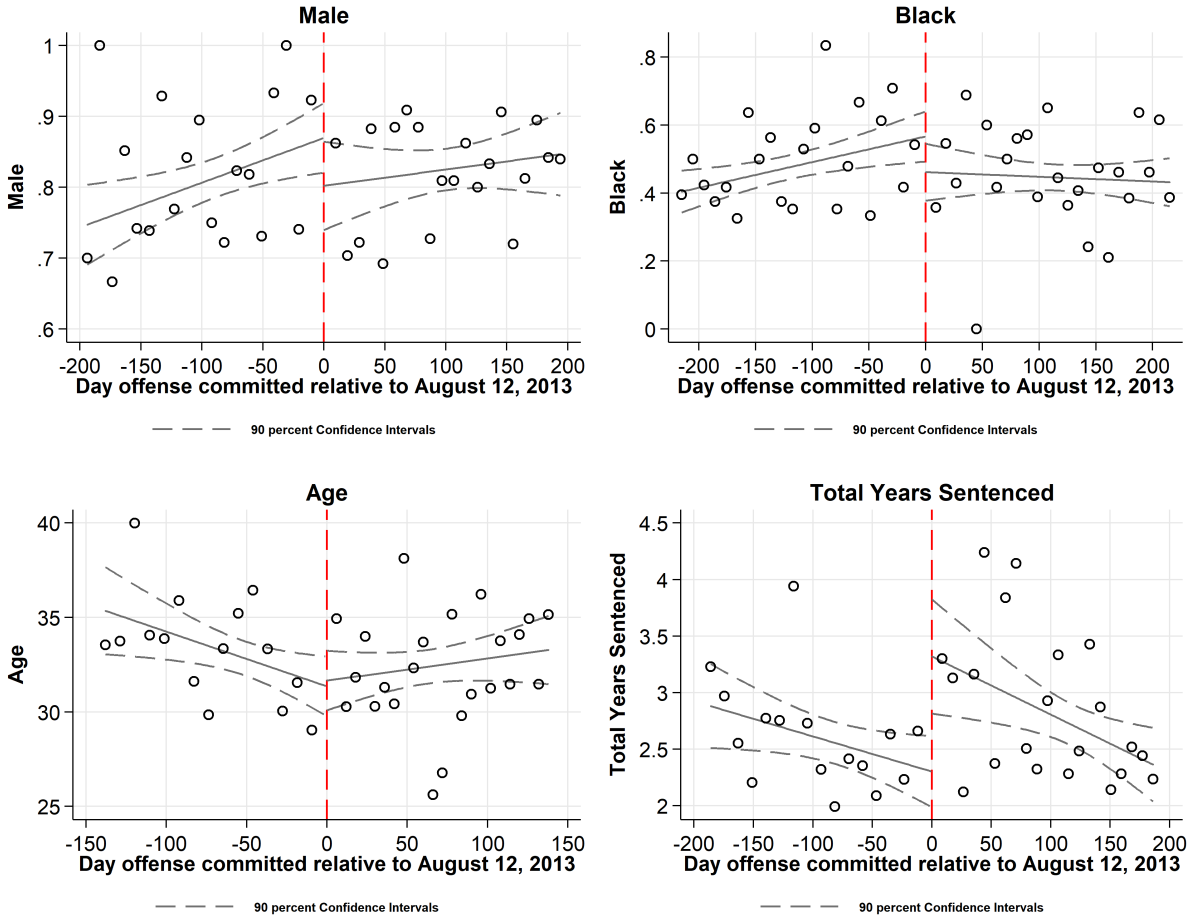
| | All County | | Border County | |
|---|---|---|---|---|
| | Before | After | Before | After |
| *Panel A. Holder Memo Starts (8/29/2013)* | | | | |
| Weapons Possession | 0.087 | 0.110 | 0.100 | 0.128 |
| | (0.283) | (0.312) | (0.301) | (0.335) |
| Black | 0.503 | 0.478 | 0.489 | 0.455 |
| | (0.500) | (0.500) | (0.500) | (0.498) |
| Hispanic | 0.035 | 0.037 | 0.005 | 0.006 |
| | (0.185) | (0.188) | (0.070) | (0.076) |
| Age | 33.774 | 34.457 | 32.716 | 33.247 |
| | (10.308) | (10.408) | (9.915) | (9.733) |
| Male | 0.841 | 0.845 | 0.805 | 0.828 |
| | (0.366) | (0.362) | (0.396) | (0.378) |
| Total Year Sentenced | 3.328 | 3.377 | 3.410 | 3.090 |
| | (8.968) | (9.746) | (12.102) | (9.649) |
| Observations | 16662 | 15514 | 1611 | 1720 |
| *Panel B. Holder Memo Ends (5/10/2017)* | | | | |
| Weapons Possession | 0.130 | 0.141 | 0.149 | 0.147 |
| | (0.336) | (0.348) | (0.356) | (0.354) |
| Black | 0.443 | 0.417 | 0.434 | 0.377 |
| | (0.497) | (0.493) | (0.496) | (0.485) |
| Hispanic | 0.040 | 0.044 | 0.007 | 0.009 |
| | (0.197) | (0.205) | (0.085) | (0.095) |
| Age | 34.709 | 36.061 | 33.881 | 35.976 |
| | (10.303) | (10.325) | (9.356) | (10.023) |
| Male | 0.837 | 0.839 | 0.816 | 0.826 |
| | (0.369) | (0.368) | (0.388) | (0.379) |
| Total Year Sentenced | 3.271 | 2.934 | 2.848 | 2.706 |
| | (8.280) | (6.696) | (3.000) | (2.884) |
| Observations | 15169 | 11888 | 1932 | 1664 |

*Note:* In the Panel A, The sample period in "Before" is from 8/29/2012 to 8/29/2013. The sample period in "After" is from 8/29/2013 to 8/29/2014. In the Panel B, The sample period in "Before" is from 5/10/2015 to 5/10/2017. The sample period in "After" is from 5/10/2017 to 5/10/2019.

The identifying assumption of the RDD is that unobserved determinants of drug related violent crimes are continuous at the threshold. Although this assumption cannot be tested, at least we can see the continuity of observable variables around the cutoff day. Figure 4 shows whether there are significant changes in offenders' gender, race, age, and sentenced years before and after the threshold.[9] Although linear estimates show a certain degree of discontinuity, the scatter plot does not show meaningful discontinuity. In particular, the discontinuity of total year sentenced seems to be caused by a small number of outliers before the cutoff date. Table 5 shows the estimated coefficient values with and without control variables. Regardless of the control variable, the RDD coefficients are significant at the 5% significance level with the range between -.11 and -.09.

[9]Among the independent variables, the graph of Hispanics was not included, because the Hispanic ratio was low (3% in summer statistics) and most of the scatter plots were zero. The Hispanic graph is in Figure A.1 in the Appendix.

Figure 4: Smoothness Test of the Observable Characteristics of Offenders




*Notes*: In these figures, the dependent variables are offenders' gender, race, age, and total years sentenced. The estimating equation is identical to Equation 1 and the MSE-optimal bandwidths are selected for each dependent variables. Bin sizes are selected by using Mimicking Variable Average-spaced Method (ESMV)

Table 5: Use of Weapon of Drug Offense in Border County with Control Variables

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Control Vars | None | Male | Black | Age | Total Years Sentenced | All |
| Holder Memo | -0.110** | -0.102** | -0.100** | -0.111** | -0.102** | -0.0909** |
| | (0.0456) | (0.0453) | (0.0453) | (0.0456) | (0.0453) | (0.0449) |
| Observations | 2,929 | 2,929 | 2,929 | 2,929 | 2,929 | 2,929 |
| Bandwidth | 197.8 | 197.8 | 197.8 | 197.8 | 197.8 | 197.8 |
| Eff Obs Below Cutoff | 372 | 372 | 372 | 372 | 372 | 372 |
| Eff Obs Above Cutoff | 431 | 431 | 431 | 431 | 431 | 431 |

*Notes*: The estimating equations are identical to Equation 1 except the dependent variable. The bandwidth is fixed as the same level of MSE-optimal bandwidth with no control varibles, column 1. First order polinomial function is used to estimate limites around the threshold.

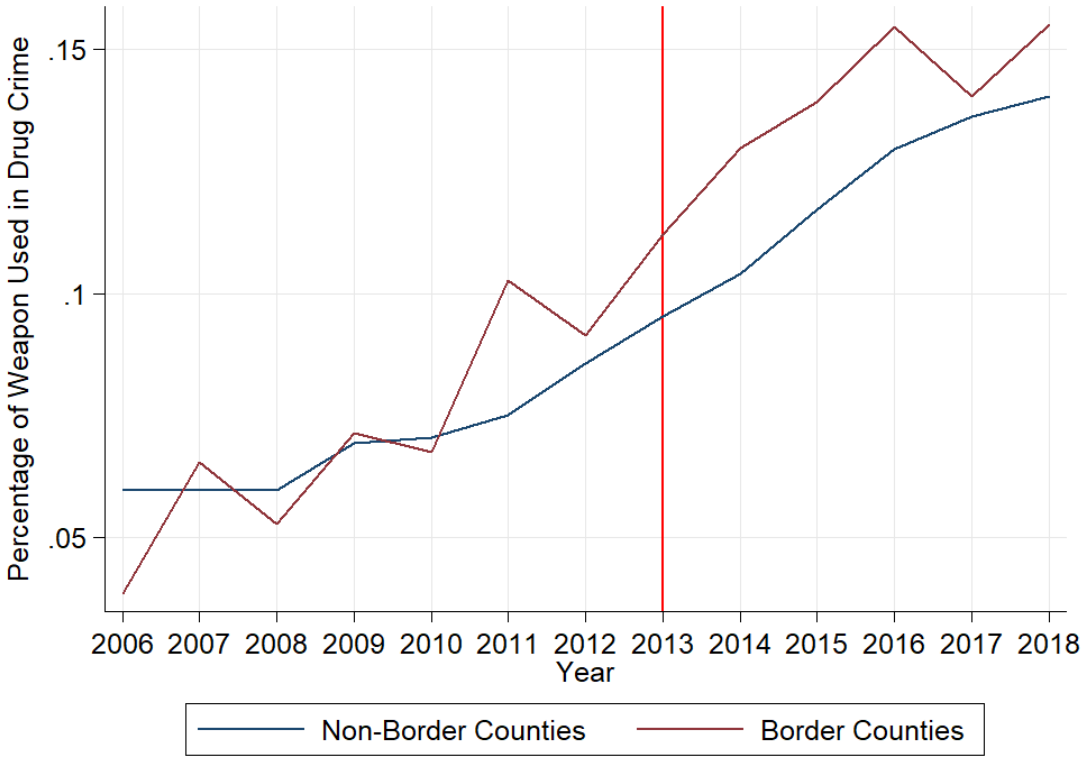
# 5 Main Result

Figure 5 shows that the trend of weapon use for drug offenders is increasing. It is important to note that the criminal's decision of carrying a weapon is determined by many factors other than the severity of punishment. The graph shows the average percentage of possession of weapons each year, and it has continued to increase since 2006. Given this upward trend of weapon possession, the question is whether the Holder Memo can slow down the upward trend of weapons used in the short run.

## 5.1 Holder Memo Starts

Table 6 shows the RDD estimating result in Florida around the Holder Memo start date, August 12, 2013. The dependent variable is the possession of a weapon and the running variable is the date. The $\beta$s is from Equation 1. Panel A and Panel B are the result of only using samples from border counties, and Panel C is the result of using samples from all counties in Florida. First of all, we can see that the values of the coefficients vary depending on the sample range. When only border counties were sampled, the rate of possession of drug

Figure 5: Percentage of Weapon Used in Drug Related Crime




Source: Florida Offender Based Information System (OBIS)

offenders decreased by 11% (Panel A and Panel B Column 1) but, no significant reduction in possession in all counties (Panel C Column 1). As mentioned in Section 2, counties near the state border lines are areas that are heavily affected by federal investigations and the result can be interpreted as the Federal Investigation Policy, the Holder Memo, has exerted a valid influence on criminals in this area. This assumption implies that the Holder Memo should have more impact on more serious drug criminals because they are more exposed to federal investigation. In the second and third columns of panels A and B, the rate of weapon possession has been reduced more for drug dealers, manufacturers and distributors (SMD) than drug-carrying offenders. The criminals charged with drug possession shows a 9.5% reduction in weapon possession, whereas SMD offenders a 13.8% reduction. Not to mention that 43% of SMD offenders in the border counties are also charged with drug possession

during ± 2 years of the Holder Memo.

The border county shows a consistent result under two different bandwidths. Panel A uses 197 days from MSE optimal bandwidth and Panel B uses narrower bandwidth of 150 days. However, I find that the narrower the bandwidth, the clearer behavioral changes.

Figure 6 and Figure 7 present visual evidence of the result of Table 6. The figures show a linear fitted line and scatter plots of weapon possession outcomes collapsed to 8.8-day bin averages for Figure 6 and 7.6-days (below cutoff date) and 8.3-days (above cutoff date) for Figure 7.[10] Figure 6 uses 150 days bandwidth and Figure 7 uses 100 days bandwidth.

The ratio of weapon possession has more clearly decreased in the case of 100 days bandwidth. Prior to the Holder Memo, the rate of possession of weapons varies widely by bin, from 0% up to 40%. After the Holder Memo, the rate of weapon possession is mostly 10% or lower. In Figure 6 50 days after adding the Holder Memo, bins that were added before the Holder Memo show relatively higher variance, but the trend is gradually increasing. When the Holder Memo is implemented, the scatter plots show a discontinuity, a sharp drop, but it gradually increased after 100 days of the Holder Memo.

In sum, as we saw earlier in Figure 5 the rate of possession of weapons by year is increasing, and by day, there is a greater reduction in the possession of weapons from the 100 days before and after the Holder Memo than 150 days. This information suggest that the Holder Memo has reduced the rate of possession of weapons for criminals, but its effect seems be short-run. The visual evidence of Panel C in Table 6 where all counties are in the sample, does not show any discontinuity of the weapon of a possession around the date the Holder Memo was implemented (see Figure 8).

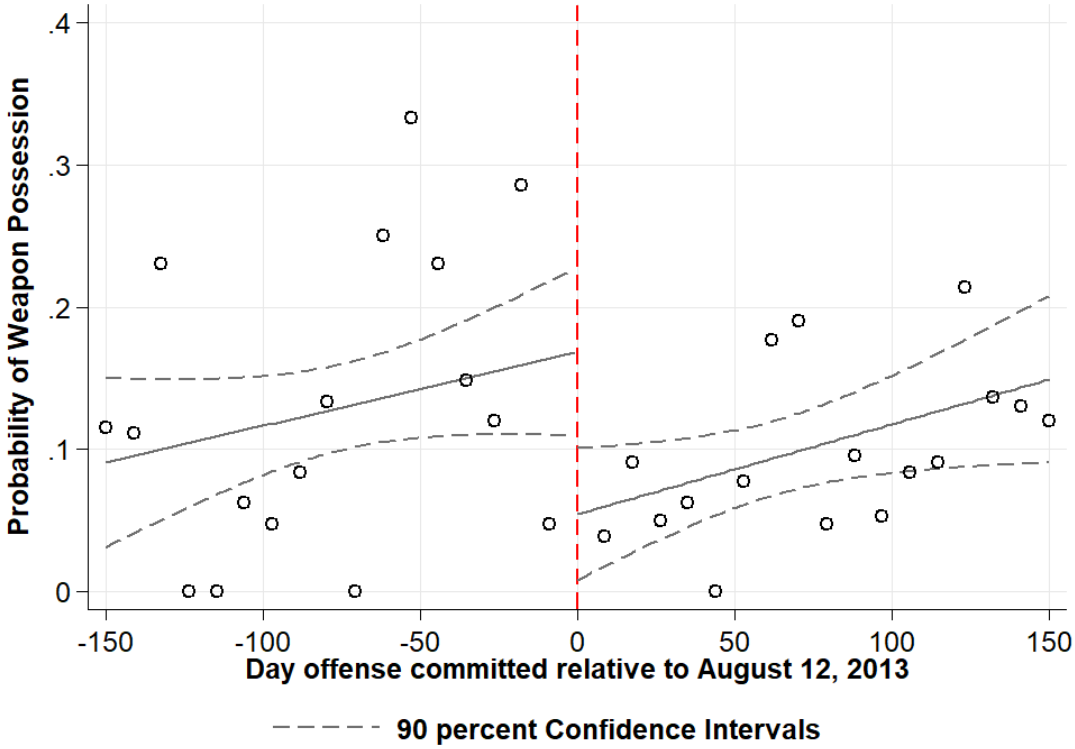
[10]For the bin estimation, I use the Stata default method of Mimicking Variable Average-spaced Method(ESMV).

Table 6: RD: Main Result

| | All Drug Offense | Possession Drug | SMD |
|---|---|---|---|
| *Panel A. Border Counties* | | | |
| $\beta$ | -0.110** | -0.0867* | -0.120** |
| | (0.0456) | (0.0473) | (0.0604) |
| Observations | 2,929 | 2,118 | 1,374 |
| Eff Obs Below Cutoff | 372 | 406 | 239 |
| Eff Obs Above Cutoff | 431 | 473 | 278 |
| MSE-optimal Bandwidth (Days) | 197.8 | 300.1 | 271.5 |
| *Panel B. Border Counties (Constant BW)* | | | |
| $\beta$ | -0.117** | -0.0951 | -0.138* |
| | (0.0502) | (0.0603) | (0.0767) |
| Observations | 2,929 | 2,118 | 1,374 |
| Eff Obs Below Cutoff | 277 | 200 | 120 |
| Eff Obs Above Cutoff | 337 | 250 | 166 |
| Constant Bandwidth (Days) | 150 | 150 | 150 |
| *Panel C. All Counties* | | | |
| $\beta$ | -0.0117 | 0.00740 | -0.0221 |
| | (0.0140) | (0.0156) | (0.0206) |
| Observations | 28,697 | 19,957 | 13,627 |
| Eff Obs Below Cutoff | 4103 | 3587 | 1598 |
| Eff Obs Above Cutoff | 4133 | 3701 | 1551 |
| MSE-optimal Bandwidth (Days) | 207 | 259.3 | 174 |

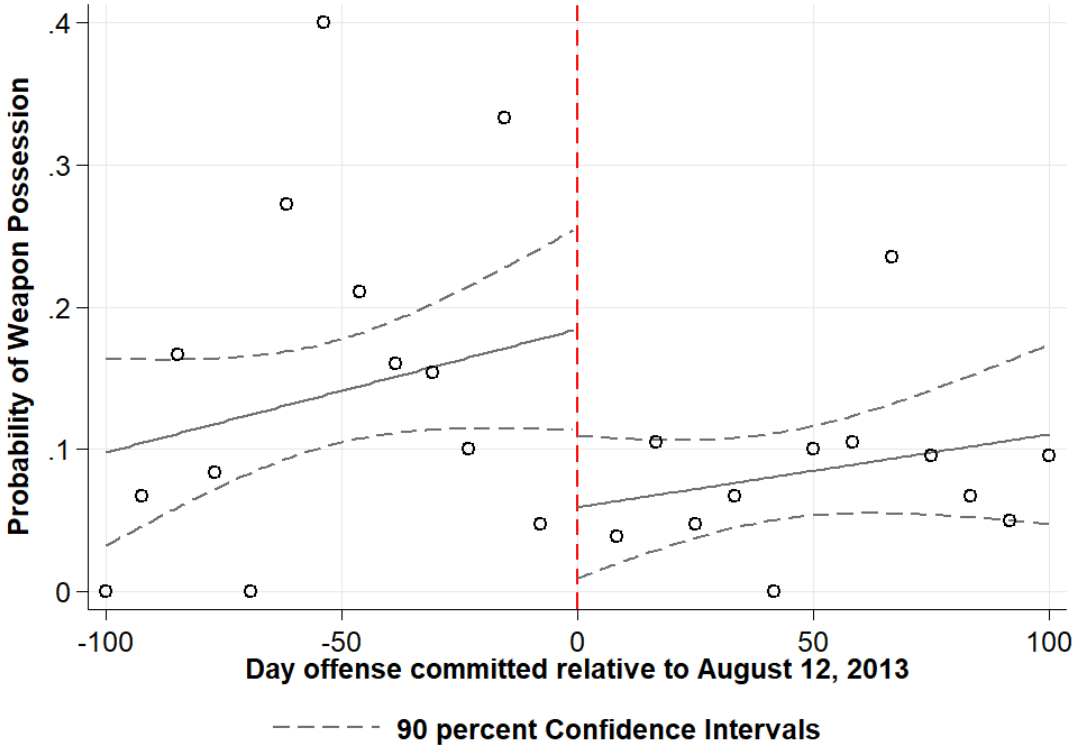
*Notes*: The tables reports RDD estimates from Equation 1 and heteroskedasticity-robust standard errors are estimated in parentheses. Panel A and Panel B use offenders data in the border counties. Panel A uses MSE-optimal bandwidth, whereas Panel B uses a constant bandwidth of 150 days before and after the onset of the Holder Memo, Aug. 12, 2013. Panel C uses offenders data in all counties. Poloynominal degree one is selected based on graphical evidence of Table 6 (see Figure 6 & Figure 7). The results with alternative polonomial degree are in Table A.1. All analysis use poloynominal degree one and triangular kernel (Stata default). The result is robust with alternative choice of kernels (see Table A.2).

Figure 6: RDD Any Drug Offense: Border Counties ±150 Days




*Notes*: This figure is a graphical evidence of the result in Table 6 Panel B with bandwidth 150 days. Bin sizes are estimated using Mimicking Variable Average-spaced Method(ESMV). The lines in the figure above display two linear regressions, estimated seperately by the cut off daty, Aug. 12, 2013. The scatter plots shows the bin average of the dependent variables. The running variables in the figure is the number of days between the offence date and the date of the Holder Memo starts, Aug 12, 2013. The negative distance indicates that the offenders committed a crime before the Holder Memo and the offences committed after the Holder Memo have positive distance from 0

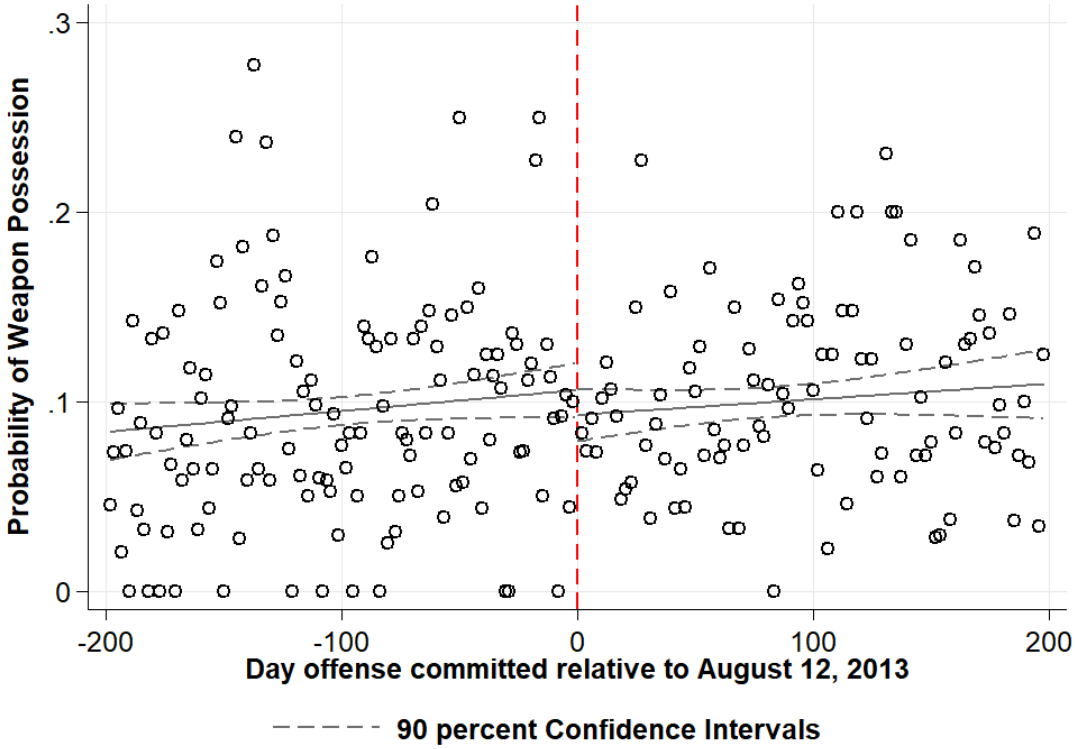
Figure 7: RDD Any Drug Offense: Border Counties ±100 Days




Bin size: BW:100

*Notes*: This figure is a graphical evidence of the result in Table 6 Panel B with an alternative bandwidth of 100 days. See Figure 6 for a general description of the RD plots.

Figure 8: RDD Any Drug Offense: All Counties




*Notes*: This figure is a graphical evidence of the result in Table 6 Panel C. See Figure 6 for a general description of the RD plots.

## 5.2 Holder Memo Ends

On May 10, 2017, Attorney General Jeff Sessions reversed the Holder Memo and issued harsher sanctions for the U.S. Department of Justice. In his memo Sessions (2017), he wrote "It is of the utmost importance to enforce the law fairly and consistently.". The memo denotes "Any inconsistent previous policy of the Department of Justice relating to these matters is rescinded, effective today".

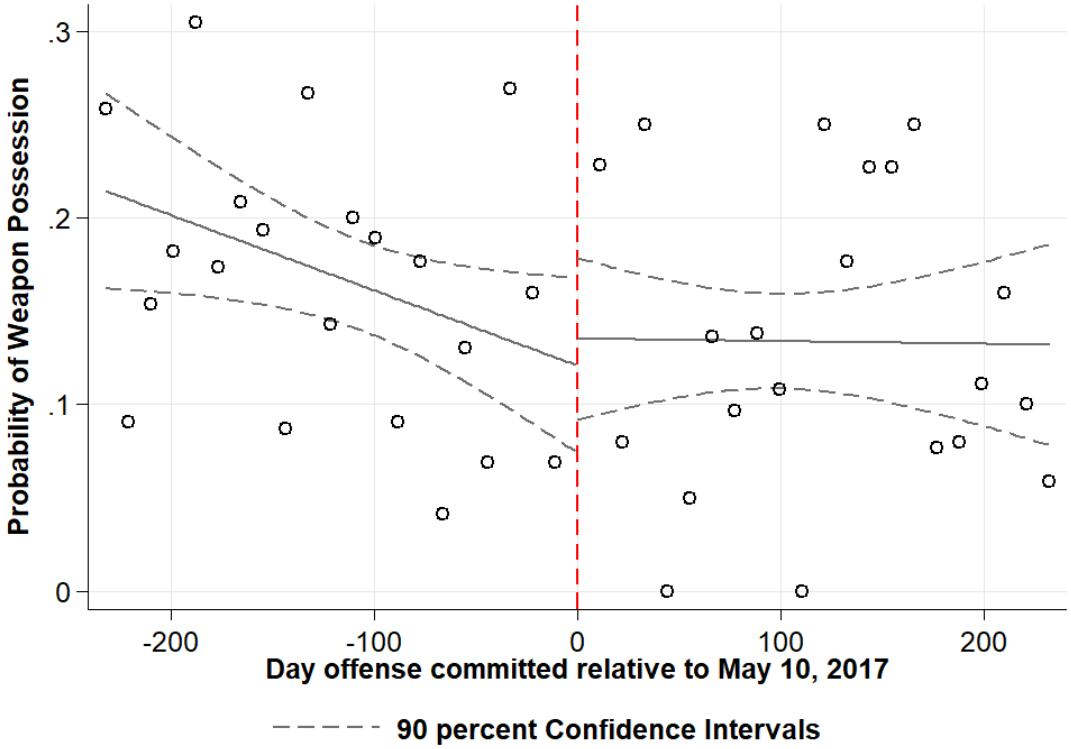
Table 7 shows the RDD result when the Holder Memo rescinds. I cannot find any significant changes. Figure 9 and Figure 10 present the visual evidence of Table 7. Conservatively, there is no change of weapon possession before and after the rescission. At the best, for the border counties, the downward trend of weapon possession is nullified after the rescission.

Table 7: RDD: Holder Ends Main Result

| | Any Drug Offense | Possession Drug | SMD |
|---|---|---|---|
| *Panel A. Border Counties* | | | |
| $\beta$ | 0.0154 | -0.00749 | 0.00242 |
| | (0.0451) | (0.0474) | (0.0984) |
| Observations | 2,921 | 2,286 | 888 |
| Eff Obs Below Cutoff | 584 | 490 | 174 |
| Eff Obs Above Cutoff | 531 | 425 | 159 |
| MSE-optimal Bandwidth (Days) | 246.1 | 258.3 | 252.5 |
| *Panel B. Border Counties (Constant BW)* | | | |
| $\beta$ | 0.0440 | -0.00189 | 0.0461 |
| | (0.0592) | (0.0632) | (0.142) |
| Observations | 2,921 | 2,286 | 888 |
| Eff Obs Below Cutoff | 373 | 302 | 113 |
| Eff Obs Above Cutoff | 335 | 261 | 94 |
| Constant Bandwidth (Days) | 150 | 150 | 150 |
| *Panel C. All Counties* | | | |
| $\beta$ | 0.0120 | 0.0132 | 0.0140 |
| | (0.0165) | (0.0185) | (0.0265) |
| Observations | 23,584 | 17,953 | 9,053 |
| Eff Obs Below Cutoff | 4042 | 3164 | 1547 |
| Eff Obs Above Cutoff | 3849 | 2983 | 1390 |
| MSE-optimal Bandwidth (Days) | 222 | 223.8 | 215.1 |

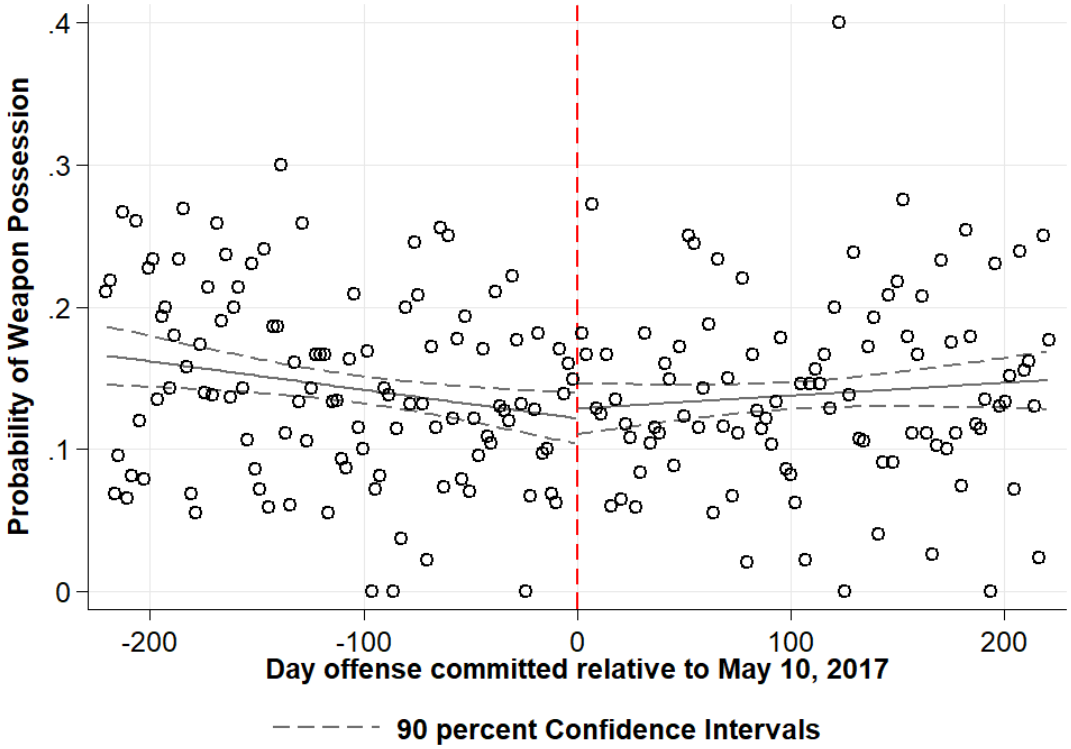
*Notes*: The tables reports RDD estimates from Equation 1 and heteroskedasticity-robust standard errors are estimated in parentheses. Panel A and Panel B use offenders data in the border counties. Panel A uses MSE-optimal bandwidth, whereas Panel B uses a constant bandwidth of 150 days before and after the rescission of the Holder Memo, May. 10, 2017. Panel C uses offenders data in all counties. See Table 6 notes for a general description of the RDD estimating options including the choice of polynomial and kernel.

Figure 9: Holder END, RDD Any Drug Offense: Border Counties




*Notes*: This figure is a graphical evidence of the result in Table 7 Panel A. See Figure 6 for a general description of the RD plots.

Figure 10: Holder END, RDD Any Drug Offense: All Counties




*Notes*: This figure is a graphical evidence of the result in Table 7 Panel C. See Figure 6 for a general description of the RD plots.

# 6 Robustness Check

## 6.1 Sample Selection

RDD requires comparability of observations above and below the RDD threshold. Empirical studies often face the issue of sample selection or missing values after the treatment. The Holder Memo can also cause sample selection issues due to the cooperative nature among federal and state law enforcement authorities. Although the Holder Memo only applies to federal investigators, non-violent drug offenders, previously investigated by federal investigators, can be investigated by state investigators since the Holder Memo. Therefore, this study, although using Florida state inmates' data, needs to address this potential sample

selection issue. In Table 6 Panel A posits an indication for potential sample selection. The number of drug crimes (effective observation within bandwidth) committed at the bandwidth of 197 days before and after the onset of the Holder Memo increased from 372 to 431. The increase could be seen as the transfer of cases to state governments that should be otherwise investigated by the federal investigators.

I use the sample selection model in RDD proposed by Dong (2019) to estimate the upper and lower bound of the RDD coefficient assuming there is a sample selection. The advantage of the Dong (2019)'s model is that, as with Lee (2009), identification does not require exclusion restrictions nor any underlining selection mechanism.

To intuitively explain how the model of Dong (2019) works, first of all, I classify the number of samples into four groups, as suggested by Frangakis & Rubin (2002). For $S_t$ where $S$ is an binary sample selection indicator that equals to 1 if the true observation $Y^*$ is observable, $Y = Y^*$ and $S = 0$ if $Y$ is missing and $t$ is a binary treatment indicator, so $t = 1$ when an individual commits a drug crime after the Holder Memo and 0 otherwise. Based on $S_t$, I can classify the following: new participants ($S_0 = 0, S_1 = 1$), quitters ($S_0 = 1, S_1 = 0$), never participants ($S_0 = S_1 = 0$), and always participants ($S_0 = S_1 = 1$). Dong (2019) estimates sharp bounds based on always participants. First, it estimates the fraction of always participants among always participants and new participants, $q \equiv Pr(S_0 = 0, S_1 = 1|S_1 = 1, C, R = r_0)$ using fuzzy RDD. For $Y_t^*$ where $Y, t = 0, 1$, the low bounds are estimated using samples with $Y_1^*$ less than $1-q$ quantile values of $Y_1^* \leq Q_1(1-q)Y_1^*$ and upper bound are with $Y_1^*$ that is greater than the $q$ quantile values of $Y_1^* \geq Q_1(q)Y_1^*$. Mathematically,

$$\begin{aligned}
\beta_{lower} &= \frac{1}{1-q} E[1(Y_1^* \leq Q_1(1-q)Y_1^*)|S_1 = 1, R = r_0, C] \\
&\quad - E[Y_0^*|S_0 = 1, R = r_0, C], \text{ and} \\
\beta_{upper} &= \frac{1}{1-q} E[1(Y_1^* \geq Q_1(q)Y_1^*)|S_1 = 1, R = r_0, C] \\
&\quad - E[Y_0^*|S_0 = 1, R = r_0, C]
\end{aligned} \tag{2}$$

The specific formula derivation process and estimation method are in Dong (2019). This

estimation requires monotonicity assumption, which is

$$\text{Assumption (Monotonic Selection)}: Pr(S_0 \leq S_1) = 1$$

The monotonic assumption tells that treatment can only cause sample selection in one direction, in the case of the Holder Memo, everyone is more likely to participate under treatment. This assumption reflects that the Holder Memo may cause a transition of non-violent drug crime cases from federal to state investigators.

In the OBIS data set, it is unidentifiable whether an individual would have been investigated by federal investigators without the Holder Memo. However, this paper assumes an extreme case of a sample selection issue: federal investigators hand over only unarmed drug criminal cases to state investigators. In the implementation, I randomly select 10 to 30% of unarmed drug criminals after the Holder Memo and create an indicator variable marking these selected individual as the new participants (observation with sample selection)and then I use Dong (2019)'s strategy to estimate the upper and lower bound of the always participants.

Table 8 shows the upper and lower bounds of $\beta$ (Table 6 Panel A column 1) under the sample selection. Without this hypothetical sample selection, we have found earlier that the possession of a weapon in drug criminals in the border counties is reduced by 11 percentage points (-.11). Regardless of the percentage of sample selection, the upper and lower bounds contain -.11 within their intervals. Assuming 10% to 30% of sample selection, the number of observations with sample selection is 38.6 in 10% of sample selection, 77.2 in 20%, and 115.5 in 30%. Note that the difference of effective observation before and after the Holder Memo is 59 in the main analysis.

The upper bounds where only use $Y$ with $Y_1^* \geq Q_1(q)$ have a constant negative coefficient in all hypothetical sample selection degrees. Given that there are 59 additional samples after the Holder Memo, it is reasonable to assume that 10% and 20%, or 38 or 77 samples, are cases handed over from the federal government. The estimated coefficient value is -.109

and -0.099 are very similar to -0.11 when there is no sample selection. The lower bound is constant at -.168. The reason is that the lower bounds select $Y_1$ only with $Y_1^* \leq Q(1-q)$ to estimate $E[Y_1^*|S_1 = 1, R = r_0]$ where $q$ is proportion of new participants. For $q = 0.1, 0.2, 0.3$, $E[1(Y_1^*|S_1 = 1, R = r_0, C]$ is zero meaning offenders lowest 70 to 90 percent of quantile are not carrying weapon after the Holder Memo.

Table 8: Sample Selection: Upper and Lower Bound of Weapon Use in Drug Offense Border County

| % of New Participants | 10% | 20% | 30% |
|---|---|---|---|
| Lower Bound | -0.168*** | -0.168*** | -0.168*** |
| | (0.0482) | (0.0447) | (0.0444) |
| Upper Bound | -0.109* | -0.0999 | -0.0611 |
| | (0.0611) | (0.0646) | (0.0883) |
| Number of Obs with Sample Selection | 38.6 | 77.2 | 115.8 |
| Observations | 3,326 | 3,326 | 3,326 |

*Notes*: The table reports the upper and lower bounds of coefficient of $\beta$ in Equation 1 using the method introduced in Dong (2019). The RDD estimating options is identical to Table 6 (see the notes). Standard errors are estimated by bootstrapping with 500 replication. The numbder of observation with sample selection indicates the number of samples that is randomly selected and assigned as the new participants. The first column 10% assumes the 10% of all unarmed drug offenders after the Holder Memo are the new participants. For the second and third column assumes that the proportion of new participants are 20% and 30%.

## 6.2 Seasonality

In this chapter, I implement a placebo test assuming seasonality in the weapon possession. Another way to interpret the result in Section 4 is that August is the month that usually drug offenders decide not to carry a weapon. Therefore, I changed the year of cutoff dates, in which instead of using August 12, 2013 as the cutoff date, I use August 12, 2000 to August 12, 2012. If there is a significant change in possession of weapons similar to -0.11 when the year of implementation of the Holder Memo has been changed, the negative coefficient

simply catches the seasonal trend of possession of weapons, not the change in behavior of criminals caused by the Holder Memo.

Table 9 estimates the change in the probability of weapon possession of drug offenders in the border county when the year the Holder Memo was deliberately changed. Each column represents the artificially assigned Holder Memo year. For example, Year 2000 (column 1) assumes that the Holder Memo implemented on August 12, 2000 and the coefficient is estimated using Equation 1 and indicates the changes in probability of possession of weapons in drug offenders. The bandwidth in RDD is identical to Panel A in Table 6.

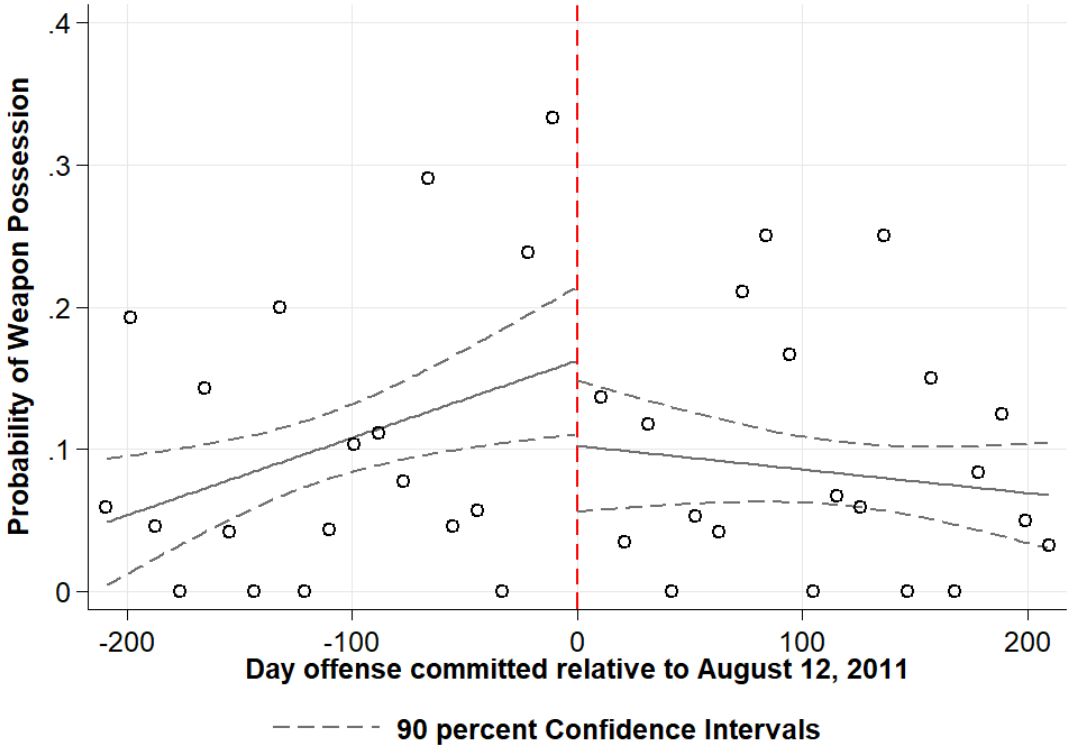
This placebo test reveals that the average coefficient from year 2000 to 2012 is -0.00263, which shows no evidence that the result of Section 4 is driven by seasonality, except year 2011 with coefficient of -.117 which is similar to non-placebo test result of -.110. However, Figure 11, the graphical evidence of year 2011 shows that there is no significant change in the scatter plot. Before and after August 12, 2011, the probability of weapon possession among drug offenders is ranged between 0 and 0.3. The coefficient seems to be driven by three bins right before the cutoff date leading the linear projection before August 12, 2011 upward sloping. On average, the probability of weapon possession around August 12, 2011 does not show a structure break nor a discontinuity.

Table 9: Seasonality: Weapon Use in Drug Offense in Border County

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Year | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 |
| $\beta$ | 0.00754 | -0.0388 | 0.00228 | -0.0183 | 0.00679 | 0.0217 | 0.00997 |
| | (0.0335) | (0.0295) | (0.0316) | (0.0156) | (0.0138) | (0.0212) | (0.0207) |
| Observations | 2,788 | 3,165 | 3,588 | 4,063 | 4,389 | 4,440 | 4,263 |
| MSE-optimal Bandwidth | 197.8 | 197.8 | 197.8 | 197.8 | 197.8 | 197.8 | 197.8 |
| Eff Obs in Given Bandwidth | 339 | 460 | 474 | 537 | 587 | 723 | 679 |
| Year | 2007 | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 |
| $\beta$ | 0.0325 | 0.0159 | 0.0149 | 0.0277 | -0.117** | 0.000618 | -0.110** |
| | (0.0340) | (0.0339) | (0.0425) | (0.0326) | (0.0538) | (0.0428) | (0.0456) |
| Observations | 3,847 | 3,453 | 3,172 | 2,988 | 2,906 | 2,928 | 2,929 |
| MSE-optimal Bandwidth | 197.8 | 197.8 | 197.8 | 197.8 | 197.8 | 197.8 | 197.8 |
| Eff Obs in Given Bandwidth | 552 | 473 | 462 | 399 | 403 | 400 | 372 |

*Notes*: The table reports the coefficient of $\beta$ in Equation 1 with an articial date of the Holder memo. Each column represents the artificial year of the Holder Memo, thus it changes from Aug. 12, 2000 to Aug. 12 2013. The month and day of the artifical Holder Memo is fixed to Aug. 12. The bandwidth is fixed as 197.8, the same level of the estimation 2013. The RDD estimating option is idential to Table 6 (see the notes).

Figure 11: RDD Drug Offense: Border Counties Cut Off Date 8/12/2011




*Notes*: This figure is a graphical evidence of the result in Table 9 when the artificial year of the Holder Memo is 2011. The cut off date is Aug. 12, 2011. See Figure 6 for a general description of the RD plots.

## 6.3 Sample Period Restriction

The sample period of this study restricts both before and 2 years after the Holder Memo started. In cross-sectional analysis, more samples reduce standard error and find more robust estimate, but in RDiT where the running variable is time, more observations around the cutoff date may cause a bias estimate due to capturing auto-regressive, or due to disparity of the short-run and long-run effect (see Hausman & Rapson 2018).

Another issue that I have found is that MSE-optimal bandwidth tend to be too large when a longer period of a sample is used. Table 10 shows the RDD estimating coefficient, $\beta$ in Equation 1 across different sample periods. Column 1 shows that the change in probability of weapon possession of drug offenders (Same as Panel A Column Table 6) in border counties

with a restricted sample period of ±2 years of the onset of the Holder Memo. The coefficients for ± 3 years and the non-restricted case (using sample from 1996 to 2019) are different from that of ± 2 years. This disparity can attribute to the different range of MSE-optimal bandwidth. For ± 2 years, the optimal bandwidth is 197.8 days whereas, 375.9 days for ± 3 years and ± 574.6 days for the non-restricted case. The longer period of bandwidth dampens the short-run effect of the Holder Memo.

The earlier works indicate that the effect of the Holder Memo is short lived. Figure 5 shows the annual trend of weapon possession is upward sloping. However, in a short run, RDD estimates that 10% point decrease of the possession of weapon within ± 197.8 days. Such a drop is more prominent in the shorter period of ± 100 days (see Figure 7). This may explain the reason why Table 10 shows the absolute value of RDD coefficient decreases from -0.11 to -0.0553 as the optimal bandwidth increases.

Table 10: Data Restriction: Weapon Use in Drug Offense Border County

| | (1) | (2) | (3) |
|---|---|---|---|
| Restriction Period | ± 2 years | ± 3 years | No Restriction |
| $\beta$ | -0.110** | -0.0679* | -0.0553** |
| | (0.0456) | (0.0351) | (0.0281) |
| Observations | 2,929 | 1,501 | 18,564 |
| Eff Obs Below Cutoff | 372 | 737 | 1170 |
| Eff Obs Above Cutoff | 431 | 820 | 1278 |
| MSE-optimal Bandwidth | 197.8 | 375.9 | 547.6 |
| Restriction Date | 8.12.2011-8.12.2015 | 8.12.2010-8.12.2016 | No Restriction |

*Notes*: The table reports the coefficient of $\beta$ in Equation 1 with different sample period ranges. Each column represents the ranges of the sample period. The first column uses the sample two years before and after the Holder Memo, from Aug. 12, 2011 to Aug. 12, 2015. The second column uses 3 years of sample before and after the Holder Memo. Last column does not restrict the sample period, thus the sample starts Oct. 1, 1995 and ends Feb. 28. 2020. The RDD estimating option is idential to Table 6 (see the notes).

# 7 Conclusion

The Holder Memo is a policy that the federal law enforcement agencies does not apply minimum mandatory sentence to unarmed, nonviolent and non-organized drug offenders. Using Florida OBIS data, this study has found that drug offenders' probability of weapon possession decreased by 11 percentage points after Holder Memo came into effect, whereas when Holder Memo was rescinded, there was no change in weapon possession drug offenders.

These findings suggest that criminal sanction affects the degree of harmful activity that criminals choose. After the Holder Memo, criminals faces a lower expected sentence if they do not possess weapons at the crime scene. The policy leads drug crime less harmful; criminals to be less likely to posses a weapon. In the context of single act framework, Doob & Webster (2003) find a harsher sanction do not deter crime. However, in the context of marginal deterrence, a harsher sanction is an effective policy tool in minimizing social cost that crime generate, making crime less harmful.

Taking into account the standard producer theory, the result can be interpreted as criminals re-optimizing their input selection by not carrying a weapon in order to minimize the cost of committing a crime. The economics of crime rarely discusses input of crime such as weapon or violence. The Holder Memo changes the price of input; the expected sentence on possessing weapons at the crime scene. The result poses an optimistic view on the rational criminal model and standard producer theory can be applied to explain criminal behavior. Additionally, this re-optimization of behavior is asymmetric: criminals show the behavioral change when the input cost of crime decreases (the onset of the Holder Memo), but no change when the cost of crime increases (the repeal of the Holder Memo).

However, this study finds the effect on sanction or cost of crime is rather short lived. Criminals reduce their possession of weapon for about 100 days but then they continue to carry weapon in the end. This is an indication of other forces or the existence of main driving sources of criminal behavior that is not about the cost of crime.

From the policy perspective, the disparity of behavioral change across regions in Florida

indicates that information sharing about the cost of committing crime is an important factor in crime deterrence. The only region that has actual impact is border line counties where I have provided pieces of evidence that criminals in these counties are more likely to be aware of the Holder Memo. The goal of sanction is eventually deterrence of future crimes thus, more information sharing may be required to achieve this goal.

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Table A.1: RD: Main Result with Alternative Polynomial Degrees

| Dep. Vars | Drug Offense | Possession Drug | SMD | Drug Offense | Possession Drug | SMD |
|---|---|---|---|---|---|---|
| *Panel A. Border Counties* | | | | | | |
| $\beta$ | -0.123** | -0.0908 | -0.157** | -0.121* | -0.0708 | -0.170* |
| | (0.0521) | (0.0619) | (0.0738) | (0.0618) | (0.0732) | (0.0882) |
| Observations | 2,929 | 2,118 | 1,374 | 2,929 | 2,118 | 1,374 |
| Eff Obs Below Cutoff | 581 | 430 | 318 | 637 | 409 | 372 |
| Eff Obs Above Cutoff | 644 | 504 | 353 | 697 | 480 | 429 |
| MSE-optimal Bandwidth | 304.9 | 318.2 | 350.1 | 327.2 | 304 | 405.9 |
| Degree of Polynomial in | 2 | 2 | 2 | 3 | 3 | 3 |
| *Panel B. Border Counties with 150 BW* | | | | | | |
| $\beta$ | -0.0758 | -0.0314 | -0.140 | -0.0196 | 0.0769 | -0.188 |
| | (0.0676) | (0.0737) | (0.109) | (0.0807) | (0.0796) | (0.133) |
| Observations | 2,929 | 2,118 | 1,374 | 2,929 | 2,118 | 1,374 |
| Eff Obs Below Cutoff | 277 | 200 | 120 | 277 | 200 | 120 |
| Eff Obs Above Cutoff | 337 | 250 | 166 | 337 | 250 | 166 |
| MSE-optimal Bandwidth | 150 | 150 | 150 | 150 | 150 | 150 |
| Degree of Polynomial in | 2 | 2 | 2 | 3 | 3 | 3 |
| *Panel C. All Counties* | | | | | | |
| $\beta$ | -0.0146 | 0.0168 | -0.0281 | -0.0107 | 0.0247 | -0.0347 |
| | (0.0157) | (0.0214) | (0.0242) | (0.0209) | (0.0264) | (0.0262) |
| Observations | 28,697 | 19,957 | 13,627 | 28,697 | 19,957 | 13,627 |
| Eff Obs Below Cutoff | 6671 | 3860 | 2467 | 6131 | 4136 | 3684 |
| Eff Obs Above Cutoff | 6936 | 4060 | 2498 | 6481 | 4398 | 3684 |
| MSE-optimal Bandwidth | 347.4 | 284.8 | 270.4 | 322.8 | 306.2 | 396.3 |
| Degree of Polynomial in | 2 | 2 | 2 | 3 | 3 | 3 |

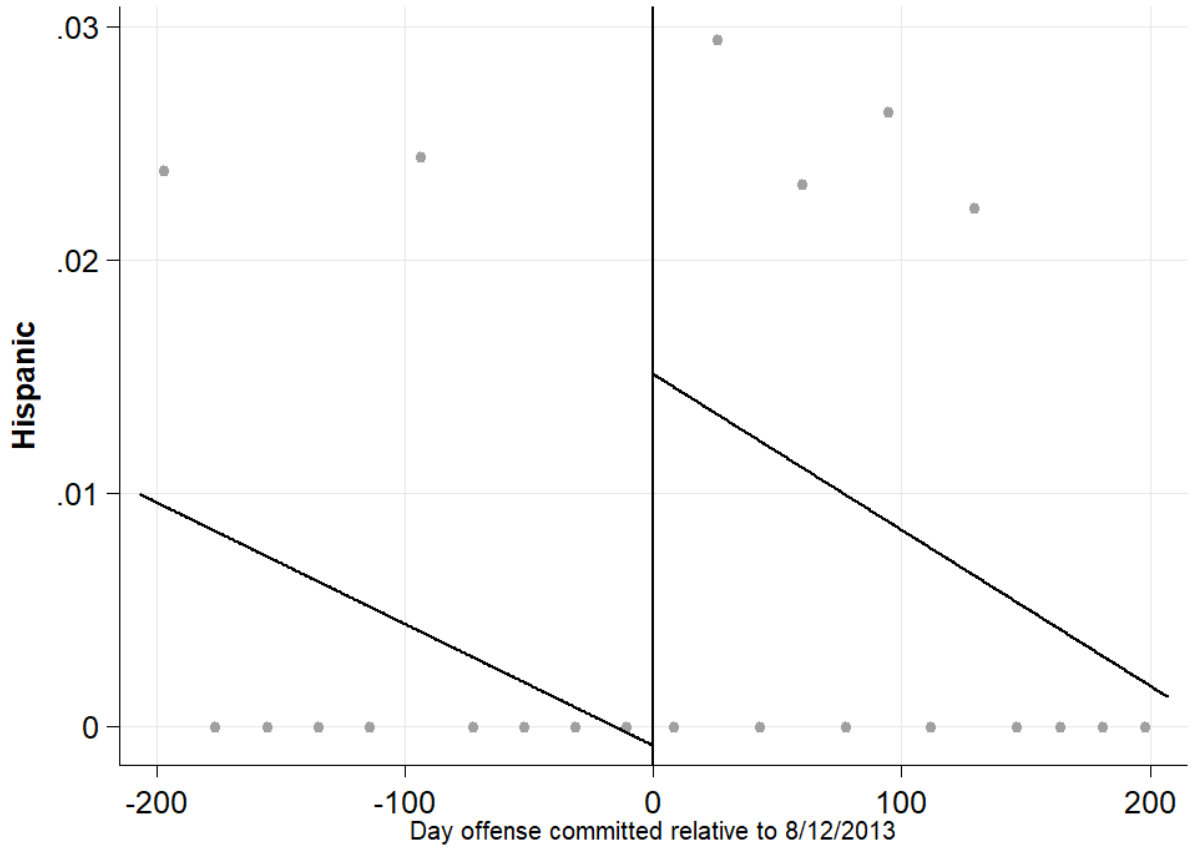
*Notes*: The tables reports RDD estimates from Equation 1 and heteroskedasticity-robust standard errors are estimated in parentheses. Panel A and Panel B use offenders data in the border counties. Panel A uses MSE-optimal bandwidth, whereas Panel B uses a constant bandwidth of 150 days before and after the onset of the Holder Memo, Aug. 12, 2013. Panel C uses offenders data in all counties. First three columns report the estimated coefficients in Table 6 with oloynominal degree two and the last three columns report those of polymonial degree three. All analysis use triangular kernel (Stata default).

Table A.2: RD: Main Result with Alternative Kernels

| Dep. Vars | Drug Offense | Possession Drug | SMD | Drug Offense | Possession Drug | SMD |
|---|---|---|---|---|---|---|
| *Panel A. Border Counties* | | | | | | |
| $\beta$ | -0.125** | -0.0824 | -0.112* | -0.114** | -0.0870* | -0.120** |
| | (0.0493) | (0.0542) | (0.0590) | (0.0465) | (0.0476) | (0.0603) |
| Observations | 2,929 | 2,118 | 1,374 | 2,929 | 2,118 | 1,374 |
| Eff Obs Below Cutoff | 269 | 253 | 210 | 345 | 387 | 230 |
| Eff Obs Above Cutoff | 332 | 300 | 256 | 401 | 447 | 264 |
| MSE-optimal Bandwidth | 146.4 | 185.2 | 239.1 | 182.3 | 280.5 | 256.6 |
| Kernel Function | | Uniform | | | Epanechnikov | |
| *Panel B. Border Counties with 150 BW* | | | | | | |
| $\beta$ | -0.114** | -0.0881 | -0.145** | -0.123** | -0.105* | -0.138* |
| | (0.0486) | (0.0583) | (0.0727) | (0.0498) | (0.0605) | (0.0747) |
| Observations | 2,929 | 2,118 | 1,374 | 2,929 | 2,118 | 1,374 |
| Eff Obs Below Cutoff | 282 | 205 | 120 | 277 | 200 | 120 |
| Eff Obs Above Cutoff | 338 | 251 | 166 | 337 | 250 | 166 |
| MSE-optimal Bandwidth | 150 | 150 | 150 | 150 | 150 | 150 |
| Kernel Function | | Uniform | | | Epanechnikov | |
| *Panel C. All Counties* | | | | | | |
| $\beta$ | -0.00358 | 0.00131 | -0.0101 | -0.0113 | 0.00787 | -0.0214 |
| | (0.0146) | (0.0149) | (0.0199) | (0.0138) | (0.0161) | (0.0207) |
| Observations | 28,697 | 19,957 | 13,627 | 28,697 | 19,957 | 13,627 |
| Eff Obs Below Cutoff | 3082 | 3303 | 1457 | 3918 | 3186 | 1501 |
| Eff Obs Above Cutoff | 3129 | 3425 | 1420 | 3919 | 3222 | 1463 |
| MSE-optimal Bandwidth | 159.2 | 240.2 | 159.1 | 198.8 | 227.4 | 164.3 |
| Kernel Function | | Uniform | | | Epanechnikov | |

*Notes*: The tables reports RDD estimates from Equation 1 and heteroskedasticity-robust standard errors are estimated in parentheses. Panel A and Panel B use offenders data in the border counties. Panel A uses MSE-optimal bandwidth, whereas Panel B uses a constant bandwidth of 150 days before and after the onset of the Holder Memo, Aug. 12, 2013. Panel C uses offenders data in all counties. First three columns report the estimated coefficients in Table 6 using uniform kernel and the last three columns report those of Epanechnikov kernel. All analysis use polynomial degree one.

Figure A.1: Smoothness Test of the Hispanic of Offenders




*Notes*: In this figure, the dependent variables is an indicator variables that equals one if the offender s race is hispanic, and zero otherwise. The estimating equation is identical to Equation 1 and the MSE-optimal bandwidths are selected. Bin sizes are selected by using Mimicking Variable Average-spaced Method (ESMV)