The Effect of Sex Offender Registries on Recidivism: Evidence from a Natural Experiment

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Abstract

This paper examines the effectiveness of sex offender registries at reducing recidivism using administrative data from North Carolina. To estimate the causal effect, I use a regression discontinuity design to exploit variation induced by the fact that small differences in the date of initial registry meant that some offenders were removed from the registry after 10 years, while others stayed on it. Results provide little evidence than an increase in registry length decreases sex crime recidivism as intended, although there is some evidence that it reduces an offender's probability of recidivating with a court procedure-related infraction such as violating parole.

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Sex offender registries have been instituted across the US in the previous few decades in hopes of reducing sex crimes. Registry laws stipulate that offenders convicted of certain sexually-oriented offenses submit and update physical descriptions and address information as well as photos to local authorities. Current federal law requires that states host registry websites containing this information that can be used by the public to search for offenders. Offenders are required to register for lengthy periods, sometimes even for life. Sex offender registries aid law enforcement in pinpointing likely recidivist offenders for new crimes and make offenders known within their communities. Both of these forces should lead to fewer sex crimes. Conversely, as a registered offender's quality of life decreases due to the stigma and other costs associated with prolonged registry, he or she may be more likely to recidivate.

The existing empirical evidence on this question is inconclusive. Studies typically follow one of two approaches. One set of studies compares the outcomes of those required to register to those who were not required to register. Often, which offenders are required to register is determined by criteria such as previous sex offense severity. Some existing studies compare across these groups (for example, Duwe and Donnay (2008)). Other studies compare those offenders who were required to register to those who were not based on prison release date, sentencing date, or offense date (Agan, 2011; Duwe and Donnay, 2008; Zgoba, Veysey and Dalessandro, 2010; Maddan et al., 2011). In some states, there is room for parole boards, judges and offenders, to manipulate these dates, introducing the possibility for bias.[1]

Given lingering concerns about selection into the registry, another literature uses state-level variation in policies to identify effects, and similarly finds mixed results.[2] While these studies are well-suited for providing credible evidence on

[1]These studies find that either registries reduce recidivism (Barnoski, 2005; Duwe and Donnay, 2008; Zgoba, Veysey and Dalessandro, 2010) or that they have no effect on recidivism (Agan, 2011; Maddan et al., 2011; Schram and Milloy, 1995; Adkins et al., 2000; Zgoba, Veysey and Dalessandro, 2010).

[2]Many of the state-level studies find no consistent effects either way (Ackerman, Sacks and Greenberg, 2012; Sandler, Freeman and Socia, 2008; Vasquez, Maddan and Walker, 2008; Walker et al., 2005; Maurelli and Ronan, 2013), though others report that registries reduce aggregate sex crimes (Prescott and Rockoff, 2011; Letourneau et al., 2010). Ack-

the effect of registries on state-level crimes, it is more difficult to disentangle the effects on offenders from the effects on non-offenders. One exception is Prescott and Rockoff (2011), who interact the number of registered offenders with treatment indicators to estimate this effect.

In this paper, I overcome the selection issue by exploiting a natural experiment in which a group of sex offenders were removed from North Carolina's sex offender registry in 2006. State legislators extended registry from 10 years to 30 years, applicable to all current registrants. Because around 900 offenders had already been removed because their registry had expired, whether an offender's registry was extended depends on the date on which he or she originally registered 10 years earlier. This allows for a comparison between these two groups using a regression discontinuity design, where the original date of registration (ten years earlier) is the running variable.

This empirical model hinges on the assumption that the offenders and authorities could not manipulate on which side of the cutoff offenders fell. There is little reason to believe that this type of manipulation is possible. Each offender's registry date was set in 1996 or 1997 when he or she first registered, while the cutoff date for the registry extension was not announced until 2006. In order to manipulate whether an offender was removed from the registry, a party would have had to not only anticipate that the registry date would affect registry length, but also predict the cutoff date 10 years prior to its announcement.

Empirically, there is no evidence of such manipulation. I verify that the density of the registry date is smooth, and I test whether there are discontinuities across the cutoff in observable offender characteristics (including criminal record). If a party had manipulated registry dates to make sure that the restriction applied to more offenders, or at least the most dangerous offenders, then one would expect the groups on either side of the cutoff to differ in quantity or observable characteristics.

This study makes two main contributions to the existing literature. First,

erman, Sacks and Greenberg (2012) and Prescott and Rockoff (2011) employ difference-in-differences methodologies, and the other studies mentioned use time series methods.

to my knowledge this is the first study to explicitly analyze the effect of extending time on the registry, which contrasts with the existing literature that focuses on the impact of being registered at all. Registry length is an important aspect of sex offender registry policies. In fact, the Adam Walsh Act mandated federal minimums across states in 2006 (McPherson, 2007). Although results are specific to the group of previously registered offenders, and represent a local average treatment effect, they are still informative in the debate on sex offender registries more generally. The second contribution of this study is that I am able to use a simple yet compelling research design that under reasonable identifying assumptions can distinguish the effect of registry extension from confounding factors.

I find no evidence that registry extension reduces sex offense recidivism, which is the stated goal of the extension. I do find suggestive evidence that registry extension may cause a reduction in the likelihood of recidivating with regulatory infractions such as post-release revocations, possession of a firearm by a felon and obstructing justice. These results support the ineffectiveness of sex offender registries at preventing serious offenses, particularly sex offenses, and are in line with a significant portion of the literature on sex offender registries (Agan, 2011; Maddan et al., 2011; Zgoba, Veysey and Dalessandro, 2010). They may suggest, though, that additional contact with law enforcement can help keep previous offenders compliant with various regulations.

The evidence suggesting the ineffectiveness of sex offender registries is striking in light of the significant costs incurred by both law enforcement and sex offenders as a result of keeping individuals on the registry. That is, evidence here suggests that the significant social and logistical costs associated with keeping offenders on the registry for an extended period of time may not be fully justified by the benefits.

1 Background

North Carolina's sex offender registry went into effect on January 1, 1996. Offenders who were convicted of a qualifying offense or released from a penal institution for one of the applicable offenses after that date were required to register for 10 years (Senate Bill 53, S.L. 1995-545). From the start, the North Carolina sex offender registry was public information.

In 2006, the North Carolina state legislature voted to extend the registry period for sex offenders from 10 years to 30 years, and they applied the extension to all active registrants as of December 1, 2006 (House Bill 1896, S.L. 2006-247). The timing of the law's passage created a subset of offenders, those who registered between January 1, 1996, and November 30, 1996, whose registry period expired before the law took effect (Markham, 2013; Rubin, 2007). In contrast, offenders who had registered on or after December 1, 1996, remained on the registry. Comparing across these two groups of sex offenders will form the basis for my identification strategy, described in detail in the next section.

Not all offenders whose registry was extended will fulfill the 30 year registry requirement. Offenders who die or move to another state are removed from the North Carolina registry. Additionally, the same legislation that extended the registry created a means by which an offender can petition to have his or her name removed from the registry after spending 10 years on it.[3] The results section contains more detailed analysis on the effect of such petitions on registry, but I estimate that no more than about 20% of offenders are removed through successful petitions within 3 years of eligibility.

Economic theory is ambiguous as to whether the offenders for whom the registry period was extended in 2006 should be less likely to commit crimes. On one hand, keeping their information on the registry makes it more likely that their sex offender status is known to social contacts, which could limit access to potential victims. Additionally, the registry serves as an immediate aid to law enforcement in child abduction or abuse emergencies in identifying likely suspects and their whereabouts, potentially deterring recidivism by increasing

[3] All but the most serious offenders are allowed to petition for removal starting 10 years from their original registry dates. For a petition to be successful, the offender must have not been arrested for a registry-qualifying offense since he or she registered, and a trial court must determine that he or she is not a "current or potential threat to public safety" (Markham, 2013).

the probability that an offender is caught.[4]

On the other hand, offenders may be more likely to commit crimes after the registry period is extended. Prescott and Rockoff (2011) suggest that public notification of sex offender status can increase recidivism by decreasing the opportunity cost of crime. In this setting, the opportunity cost of crime is the benefit received from abiding by the law. Regulations that diminish an offender's quality of life reduce this benefit. For example, these restrictions make it difficult for offenders to build social connections due to the stigma. In addition, a number of surveys of sex offenders have confirmed difficulty in obtaining housing (for example, Mercado, Alvarez and Levenson, 2008; Levenson, 2008) and jobs (for example, Levenson and Cotter, 2005; Tewksbury, 2005). All of these effects decrease an offender's quality of life and potentially reduce the opportunity cost of crime, disincentivizing law-abiding behavior. These economic roadblocks may also drive offenders to commit financially-motivated crimes such as theft.

There are a number of competing influences that may cause offenders to either commit more or less offenses when their registry is extended. The net effect of registry extension on recidivism will have to be determined empirically.

2 Identification and Methods

I identify the effect of sex offender registry extension on recidivism by comparing those whose registry was barely extended to those whose registry was barely allowed to expire. It is important to emphasize that whether an offender's registry was extended or allowed to expire depends on what date the offender originally registered as a sex offender 10 years before the extension. This is critical since it means that policymakers in 2006 did not exercise choice

[4] The criminal cost-benefit decision making process is a staple in the economics of crime literature, stemming from Becker's seminal economics of crime paper (1968) in which he suggested that criminals have an additional cost consideration that other economic actors may not - the probability of detection and the resulting punishment. Two parallel literatures exist on the effects of changing the probability of punishment (e.g. Levitt, 1997; Doleac, 2012) and variation in the severity of punishment (e.g. Hansen, 2014; Abrams, 2012; Drago, Galbiati and Vertova, 2009).

over which offenders would get removed and which would continue to stay on the registry. In addition, it would have been impossible for judges, prosecutors, or sex offenders to predict 10 years earlier that the registry date of December 1, 1996, would determine whether an offender's duty to register expired after 10 years or was extended.

I use a regression discontinuity design to estimate the effect of remaining on the registry. This experimental design will identify the effect of registry extension at the cutoff; the estimates will compare the individuals just after the cutoff to those just before. Formally, I estimate the model:

$$Outcome_i = \alpha + \beta_1 RegistryExtended_i + \beta_2 f(RegistryDate_i) + u_i \qquad (1)$$

I allow the polynomial function of the release date ($f(RegistryDate_i)$) to vary on either side of the cutoff by using separate polynomials for the "registry extended" group.

The identifying assumption in the RDD model is that all other determinants of recidivism vary smoothly across this time threshold. Because the running variable was assigned 10 years before the cutoff was set, the timing of this cutoff is in all likelihood exogenous to offenders and their characteristics.

To support the validity of this empirical strategy, I perform a number of tests designed to detect any evidence that assignment to the groups is not exogenous. I first verify that the registry date does not exhibit signs of manipulation. One method is to check for signs of displacement in the distribution of registry dates. If there is manipulation in the expected direction, there would be a trough in the density just before the effective date and a peak just after. Manipulation could take another form, though - rather than the number of individuals changing discontinuously, the composition could be changing. To test for this type of manipulation, I check for discontinuities at the cutoff in observable characteristics. Discontinuities could signal that the groups close to the cutoff are not merely different in whether their registry expired, but in other ways that may bias estimates. Additionally, I estimate all models with and without control variables. This tests whether these observable factors appear to be correlated with whether an offender's registry was extended. If the estimates do not change with the addition of these controls, it can be taken as support that registry extension is in fact exogenous.

In order to confirm that the registry date does in fact indicate whether an individual was removed from the registry, I compare whether the "registry expired" group is less likely to appear on the registry after the extension than the "registry extended" group. I estimate equation 1 using an indicator variable for whether the offender appeared on the registry on November 13, 2012, as the outcome variable.[5] Whether an offender was registered in 2012 is unlikely to accurately reflect continued registry during the period over which recidivism is measured because I measure recidivism over the 2006-2009 period. I use samples of offenders registered at different times to provide evidence to suggest what portion of the offenders whose registry was extended were still registered after various periods of time.

I estimate the main outcome models by estimating equation 1 using ordinary least squares. In addition to testing for sex crime recidivism, I also test for an effect on the likelihood of recidivating with any type of crime, property crimes, violent crimes, drug and alcohol crimes and court-related procedural infractions.

3 Data

Data on offenders and their criminal histories come from the North Carolina Department of Public Safety's Offender Public Information website (North Carolina Department of Public Safety, 2013). Demographic, sentence, and punishment information on all individuals convicted since 1972 (for all types of offenses) is available for download in bulk from this website. Below, I refer to these data as the "DPS data."

Data from the North Carolina Sex Offender and Public Protection Registry were downloaded from the North Carolina Department of Justice website (North Carolina Department of Justice, 2013). At the time of download,

[5] November 13, 2012, is the date on which the data were downloaded.

the website contained information on all offenders registered on November 13, 2012. Throughout the paper, I will call these data the "registry data."

The registry data have one obvious shortcoming – they only exist for offenders registered at the time of download. Most information can be obtained from the DPS data, but the offenders' initial registry dates are only available in the registry data for the offenders who remain on the sex offender registry. Since my research design also requires a registry date for individuals who are no longer registered, I exploit the fact that North Carolina law required that offenders register within 10 days of release from prison or sentencing to probation (SB 53, S.L. 1995-545). These two dates are reported in the DPS records, and I use them to proxy for the registry date for all offenders. I will simply refer to this date as the "registry date" going forward. I use this date to designate which offenders are classified as "registry expired" and "registry extended," and I study samples that include offenders registered within 6 months and 11 months of the cutoff. Because the registry began on January 1, 1996, only offenders who registered within the first 11 months of the registry can belong to the "registry expired" group. This makes the 11 month bandwidth (22 months total) the largest possible. Individuals with release or sentencing dates between January 1, 1996, and October 31, 1997, serve at the main study group.

Because the DPS data include all convictions in the state of North Carolina, I can construct criminal history variables to use as controls. I create measures for both the number of offenses and the number of sex offenses of which an offender was convicted before he or she registered. I am also able to construct a count of the number of times an offender has been incarcerated and the total amount of time he or she spent incarcerated before registry. These measures, along with offender age, race, and ethnicity, are empirically-supported predictors of recidivism (Langan and Levin, 2002).

Similarly, I generate outcome variables using this dataset by determining whether an offender was convicted of any sex offenses within 3 years after removal from the registry or registry extension. I measure the outcomes for the first 3 years because it is a standard in recidivism studies and as such will allow for comparison.[6] I replicate the main results table for recidivism within 1 to 5 year ranges in Appendix 7.1.[7] I build a similar measure for offenses of any type, violent crimes, property crimes, drug and alcohol offenses, and regulation-based infractions.[8]

In order to confirm that the group of offenders whose registry expired in 2006 were removed from the registry, I match the DPS data to the registry data using an identification number assigned to individuals by the North Carolina Department of Corrections. I also perform a secondary match on name and birthday for offenders for whom there is no listed Department of Corrections number in the registry data.

Table 1 contains summary statistics; it shows means and standard deviations of recidivism measures and control variables for the "registry expired" group and the "registry extended" group. The first row of the table corresponds to the measure of continued registry discussed in the previous paragraph. The difference in means indicates that the "registry expired" group is on average 36.7 percentage points less likely to appear on the registry in 2012. This difference is significant at the 1% level.

For both groups, around 1.5% recidivate with another sex offense, whereas around 16% of offenders recidivate within 3 years by committing a crime of any type. Most offenders are white, but nearly 40% are black. Less than 1% of offenders are Hispanic. Nearly 99% of offenders are male and the average age is around 35 at time of registry. On average, offenders have 3 previous convictions and 1.4 previous sex offense convictions (including the offense that qualified him or her for the registry). They have been to jail 1.8 times and have spent just over 2 years total incarcerated.

No differences in these means are significant even at the 10% level, which indicates that at least the observable determinants of recidivism do not vary

[6]Many recidivism studies use data collected by the Bureau of Justice Statistics on prisoners released in 1994. These data contain recidivism information for the first 3 years after release (Langan and Levin, 2002).

[7]Results for various recidivism time frames generally support the finding that sex offender registries do not effect recidivism with the exception of the two year window.

[8]I include a full list of the offenses in each category in Appendix 7.2.

systematically across the two groups. Nevertheless, to identify effects I will compare those whose registry was barely allowed to expire to those whose registry was barely extended to allow for any time or age effects that could be different across these groups

4 Results

4.1 Tests of identifying assumption

The identifying assumption of the model is that the determinants of recidivism vary smoothly across the cutoff. There are few ex ante reasons to doubt this assumption in this context. It would be violated if judges, prosecutors, or sex offenders were able to affect which offenders were subject to the restriction and which ones were not. It is worth emphasizing that manipulation along these lines seems implausible, if not impossible, given that the running variable was defined 10 years earlier, but nonetheless I test for evidence of strategic behavior.

One example of such behavior is that authorities could have delayed offenders' prison releases until after the cutoff or scheduled more sentencing hearings after the cutoff in order to maximize the number of offenders subject to the extension. If this were the case, upon examining the density of registry dates, we would see a dip just before the effective date and a peak just after. In order to support that this is not the case, I show the density of the registry date for the full 11 month sample binned by week in Figure 1. The vertical line denotes the cutoff date and the x-axis is the registry date. There is no evidence of this type of strategic behavior, but there is a slight dip a few bins after the cutoff which corresponds to the winter court holidays.[9]

However improbable given the required foresight, we could also worry that authorities attempted to rearrange sentencing dates or prison release dates to

[9] Using historical data to assign would-be registry dates to offenders released starting in 1972, I find that only 1,812 offenders are assigned dates during the last week of the year, which is the lowest for any week. The mean (excluding the week in question) is 2,683 offenders.

extend the registry length for higher-risk offenders. In order to demonstrate that there are no compositional changes in the types of individuals across the threshold, I verify that no covariates exhibit a discontinuity at the cutoff. If I were to detect a discontinuity, it could indicate that the individuals whose registry dates fell just before the cutoff (whose registry expired) are not a good counterfactual for the individuals whose registry was extended.

Figure 2 displays RDD graphs using each covariate as the dependent variable. The running variable (and x-axis) is the registry date, and each figure contains local averages, denoted by circles, and linearly-fitted estimates for a different control variable. The vertical line marks the cutoff date for registry extension, and the maximal 11 month bandwidth is displayed. The first row of figures corresponds to the race and ethnicity dummies. The first figure in the second row relates to the gender composition of offenders, and the second figure is produced using the offenders' ages. The remaining figures in Figure 2 are generated using the constructed criminal history variables.[10]

Table 2 contains the corresponding regression estimates, which were obtained by estimating equation 1 with each control variable serving as the outcome variable. The rows of Table 2 are labeled with the control variable being used as the dependent variable, and the reported values are the coefficient on "registry extended."

All estimates are statistically indistinguishable from zero with the exception of the binary indicator for whether the offender is identified as black in estimates for the 6 month bandwidth. This discontinuity seems to be driven by the fact that in the first 60 days (the first dot) directly after the cutoff, there are relatively few registering black offenders. Only 29.7% of offenders registered during that time were black, compared to an average of around 35%. This reduction in the proportion of black offenders is likely statistical noise. If it were due to manipulation of registry dates around the cutoff, offenders registered just after the cutoff could drive biased results. When the offenders

[10] Although some of the figures in Figure 2 exhibit jumps at the cutoff, none of these discontinuities are statistically significant. Additionally, it is important to think about whether fitting one model across both sides together would be more convincing than the fitting them separately — in many parts of this figure, this seems to be true.

registered in the first 60 days after the effective date are omitted, there is no longer a discontinuity in the racial composition of offenders. Importantly, omitting this group does not affect the main results, indicating that these offenders do not drive the results.[11]

4.2 Effect of registry extension on continued sex offender registry status

Before exploring whether there is a discontinuity in recidivism at the cutoff date, I document that there is in fact a significant discontinuity in continued registry at the cutoff. Offenders in the "registry extended" group are 36.7 percentage points more likely to remain on the registry until at least 2012, but, again, this is likely to be understated for the period over which the outcomes are measured (2006-2009).

Figure 4 contains RDD graphs showing the effect of registry extension on continued sex offender registry status for the full 11 month bandwidth in the left panel. The dependent variable is whether the individual was registered in 2012. It is clear from the graph that the discontinuity is quite stark at nearly 30 percentage points. The local averages on the left side are not all zero because any offenders who recidivated with another sex offense are still registered even if their original registry date would have qualified them for expiration in the absence of their later convictions. Some also did not register within 10 days of release from prison, as required by state law.

Table 3 contains results indicating the effect of registry extension on continued registry obtained by estimating equation 1 using ordinary least squares. Reported coefficients are for the variable "registry extended" and indicate the difference in the probability that an offender was registered in 2012 at the

[11]Omitting this group, the effect of registry extension on whether the offender was convicted of any type of offense is -0.048, which is comparable to the -0.042 obtained when they are included. As in the full sample models, this point estimate is occasionally, but not consistently statistically significant. The effects on sex crimes and property crimes are statistically indistinguishable from zero when this group is omitted. The coefficient for procedural crimes is -0.030 (compared to -0.023 including this group) and statistically different from zero.

cutoff. Estimates range from 0.283 to 0.294; all are significant on the 1% level. Importantly, the results change little based on bandwidth or the inclusion of controls, indicating that being in the "registry extended" group is uncorrelated with observable and (hopefully) unobservable factors that may affect registry in 2012.

Only 55% of offenders in the registry extended group were still registered in 2012 (offenders' 16th year of registry); this likely understates the proportion of offenders who were registered during their 10th to 13th year of registry, the time over which outcomes are measured. To quantify how much the discontinuity is understated, I use a sample of offenders released later than my main sample group to provide "out of sample" evidence to suggest the magnitude of the discontinuity at the time of registry expiration and during the period over which the outcomes are measured. Obtaining an estimate of the discontinuity in registry at the time over which the outcomes are measured is important for understanding the true magnitude of the treatment and allows for more accurate interpretation of results on recidivism. I do so by estimating the following equation:

$$Registry_i = \alpha + \beta_1 f(RegistryDate_i) + u_i \tag{2}$$

Using ordinary least squares, I estimate this model using 2 separate samples of offenders: those who had been registered between 10 and 11 years and between 13 and 14 years as of November 13, 2012.[12] In order to generate a comparable figure and estimates, I treat November 12, 2002, and November 12, 1999, as registry extension cutoffs; I estimate models and generate figures using the would-be "registry extended" group according to those cutoffs. The coefficient α will therefore denote the likelihood of registry at the would-be cutoff for those groups, and controlling for the running variable will allow for simple graphical comparison.

The lower panel of Figure 4 replicates the in-sample first stage graph (solid line) and displays the out-of-sample evidence (dashed line for the 10 year

[12]This should be analogous to the "registry extended" group in the main models.

sample; dashed and dotted line for the 13 year sample). Results for the 10 year and 13 year samples represent likely bounds of the true first stage. Visually, this evidence suggests that the discontinuity in registry may have been between 40 and 60 percentage points during the 10th to 13th year after initial registry (the period for which recidivism is measured).

The second part of Table 3 contains estimates for the 10 year and 13 year samples. The estimated intercept from these models represents the proportion of offenders who are registered at the cutoff. Subtracting 0.245 (the proportion of offenders registered just before the cutoff in the main sample) from this coefficient will give us estimates of the suggested 10 year and 13 year discontinuities. The 10 year intercept is at least 84% across models, suggesting a discontinuity of 59.5 percentage points. Estimates of the 13 year intercept are mostly near 65%, suggesting a discontinuity of around 40.5 percentage points. The discontinuity in registry for the sample group during the time over which outcomes are measured is likely somewhere between 40 and 60 percentage points. In order to recover a local average treatment effect from the reduced form results presented, this discontinuity implies that we would have to approximately double the estimates.

4.3 Effect of registry extension on recidivism

Figure 5 contains RDD figures and indicates that registry extension has no effect on sex offense recidivism. There is no visual evidence of a discontinuity in recidivist sex offenses. I report corresponding RDD findings in Table 4. I estimate equation 1 using OLS, and robust standard errors are clustered on the running variable. RDD estimates are obtained using a linear function of the running variable and with and without covariates. For sex offenses, the estimates range from -0.0003 to -0.0020, indicating that offenders in the "registry extended" group are less than a quarter of a percentage point less likely to be convicted of a sex crime at the cutoff, and none of these estimates are statistically distinguishable from zero. The point estimate for the 11 month bandwidth with controls is -0.0003, indicating a reduction of 1.88%. Because reducing sex offenses is a stated goal of sex offender registries, the lack of evidence of their effectiveness at reducing sex crimes is important for policy analysis.

In Figure 5, recidivism of any type exhibits a discontinuity at the cutoff. The discontinuity appears to be around 5 percentage points. Point estimates in Table 4 indicate that the registry extension makes offenders between 2.7 and 5.8 percentage points less likely to be convicted of an offense. This result is not sufficiently precise to rule out that point estimates are statistically different from zero, but they are all negative and rule out an increase of over 2.1 percentage points with 95% confidence.

Results for all crime types combined indicate that there may be a decrease in recidivism for any type of crime, but that decrease is not driven by sex offenses. To elucidate this finding, I look for effects on various other types of crimes.

Recidivist infractions related to court procedures is the only type of recidivism that exhibits a statistically significant decrease at the cutoff. In Figure 5, the RDD graph for this type of recidivism is in the bottom, right panel. This figure presents visually compelling evidence that there is a decrease in this type of recidivism due to the registry extension. The last row of Table 4 contains estimates for this outcome variable; these results indicate that registry extension may cause around a .5 to 3 percentage point decrease in such recidivism. The estimates are all negative across all specifications and are not statistically different from each other, but are only statistically different from zero in the models for the 11 month bandwidth.

In contrast, there is no evidence that violent offenses, property offenses, or drug and alcohol offenses are affected by the registry extension. Visually, there is little evidence of an effect on any of these types of crimes in Figure 5. Generally, point estimates for violent crimes in Table 4 are small, negative and not statistically different from zero. The effect on property crimes is small, positive, and statistically insignificant. If registries affect offenders primarily by limiting their employment options, we might expect to see an increase in property crimes for those offenders whose registry was extended. I find no strong evidence of this. For drug and alcohol crimes, point estimates are also statistically indistinguishable from zero, but they vary in sign and magnitude.

4.4 Test for differential attrition

Because I use recidivism data from only North Carolina, it is important to know whether registry extension is likely to be correlated with leaving the state. If the worst offenders were to leave the state as a result of remaining on the registry, then the estimates might overstate the decrease in recidivism caused by the registry. Because many states require immigrant offenders from other states to register as sex offenders regardless of whether their duty to register in the state of conviction has expired, one might worry that offenders in the "registry expired" group were more likely to stay in North Carolina.

To address whether or not this was likely to be the case, I searched the National Sex Offender Public Website for a subset offenders to determine if one group or the other is more likely to be registered out of state. The search tool returns all records from state registries for offenders with the entered name and similar names. For efficiency, I only looked up a subset of names near the cutoff that are relatively less common using an index of name uniqueness to determine which offenders should be omitted. This index was based on the Social Security Administration's lists of common baby names by decade and the US Census Bureau's data on surname frequency.[13]

Table 5 summarizes the results. I looked up a total of 338 offenders (out of 2005), of whom 167 (approximately half) came from the "registry expired" group. Critically, the number of offenders who are registered in another state

[13] I used the SSA's 200 most common baby name lists from the 1950s, 1960s, 1970s and 1980s (United States Social Security Administration, 2013) because most offenders were born during that time. I calculated the proportion of babies born during those decades given these common names. I converted the Census Bureau's count of all surnames occurring at least 100 times (in the 2000 Decennial Census, United States Census Bureau, 2013) to a percent of the population, and then I multiplied the first name percent by the last name percent to get a rough probability of having each name. I eliminated all names from the list that occurred more than .15 times for every 1 million people. For example, the names "Roger Brown" (25 records nationally) and "David Holmes" (19 records nationally) have index values just above this threshold and were excluded from the selection for lookup.

is almost equal across registry types at around 9%. This suggests that there is no evidence of differential attrition from the sample.

5 Discussion

In this study I analyze the effect of extending the registry period for sex offenders on recidivism. I do so using a natural experiment in which the state of North Carolina purged offenders from the registry when they had been registered for 10 years, but then abruptly stopped this practice. The offenders who had originally registered just before December 1, 1996, saw their registration expire in 2006, while those registered just after did not. Because this cutoff was designated 10 years after the offenders initially registered, registry extension is plausibly exogenous. I use this source of exogenous variation to estimate regression discontinuity models to distinguish the effect of being on the registry from confounding factors.

Importantly, I find no evidence that registry extension has the intended effect of reducing sex crime recidivism. I also find little evidence in favor of a major criticism of sex offender registries – that sex offenders will commit property crimes because their labor market outcomes are limited by formal restrictions and social stigma.

I find that the only type of crime that is affected by registry extension is a type that has little effect on public safety — regulation-based crimes, such as parole revocations, possession of a firearm by a felon, and obstructing justice. Any reduction is likely due to the additional supervision that continued registry imposes on offenders. Registrants are required to keep the local authorities up to date on their address, and may be more aware of other regulations due to the additional supervision that they receive in doing so.

Parole revocations are of interest from a cost perspective, though, as they result in reincarceration. According to the North Carolina Department of Public Safety, custody in a state prison in 2007 costed between $57.48 and $88.93 per day, and parole costed only $2.09 per day (North Carolina Department of Public Safety, 2007). In North Carolina in 2007, individuals were generally not eligible for parole until they had 9 months or less of their sentence remaining, and upon parole revocation, they simply completed the original sentence (Markham, 2011). Based on these cost estimates and the maximum time an offender can be reincarcerated, the upper bound on the additional cost to the state of a single parole revocation is $23,772.45 per year.

Overall, these results suggest that registries may deter criminals from committing infractions, but not sex offenses as intended. They also suggest that the deterrent effects may be isolated to lower priority types of crimes. Registries are costly for law enforcement to operate, and policy-makers must decide whether potentially reducing recidivism in these contexts sufficiently justifies long registry periods. The major benefits to law enforcement are not likely in increases in public safety, but instead in stemming the flow of former criminals back into the penal system, which comes with considerable cost savings. If agencies do determine that this is an appropriate objective, then my results suggest that they would reap the largest benefits by working to increase the salience of sex offender supervision.

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6 Tables and Figures

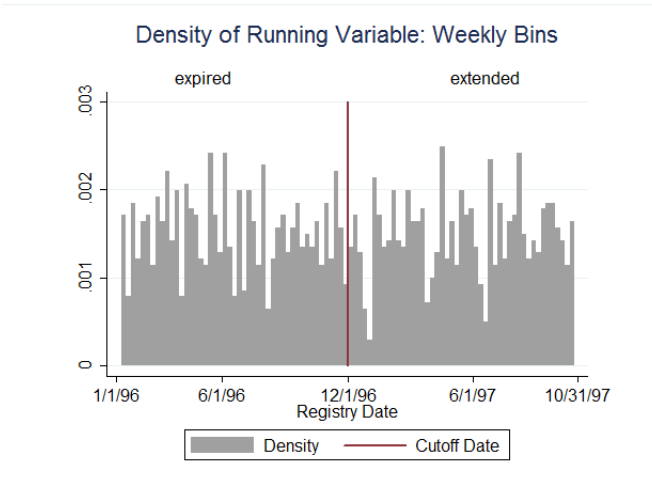

Figure 1: Density of the Running Variable

Notes: For offenders sentenced to incarceration, the registry date is estimated using release date. For individuals sentenced to probation, it is sentencing date. The vertical line denotes the effective date of the legislation.

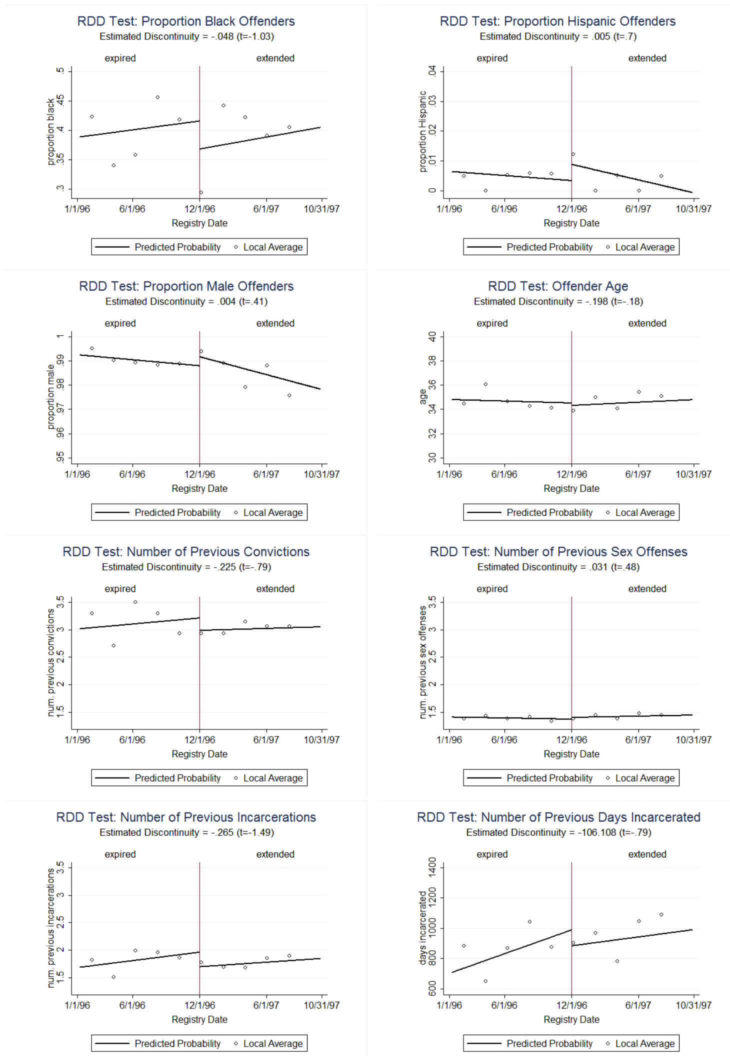

Figure 2: Tests of RDD Specification

Notes: The running variable is the estimated registry date. Local averages are reported for 60 day bins. Race, ethnicity and gender are measured by dummy variables. "Previous offenses" is the number of offenses and "previous sex offenses" is the number of sex offenses for which an offender was convicted before he or she was required to register. "Previous incarcerations" is the number of separate incarceration sentences before registry and the "previous years incarcerated" is the total years served before registry.

Figure 3: Effect of Registry Extension on Continued Registry

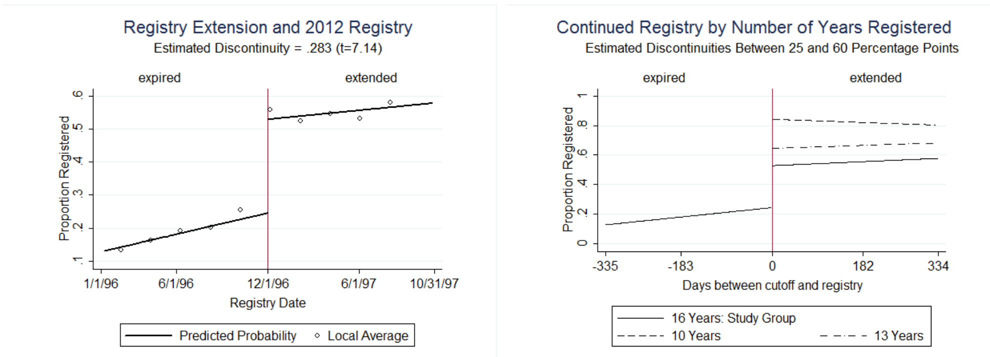

Figure 4: Effect of Registry Extension on Continued Registry

Notes: The dependent variable is whether the offender was registered on November 13, 2012. The running variable is the offender's estimated registry date. The vertical line denotes the law's effective date. Local averages are for 60 day bins. Figure 3b includes out-of-sample evidence on continued registry status of the groups of offenders who had been registered for 10 years and 13 years as of November 13, 2012. For these two groups, I treat November 13, 2002, and November 13, 1999 (respectively) as "cutoffs" and only graph the would-be "registry extended" group.

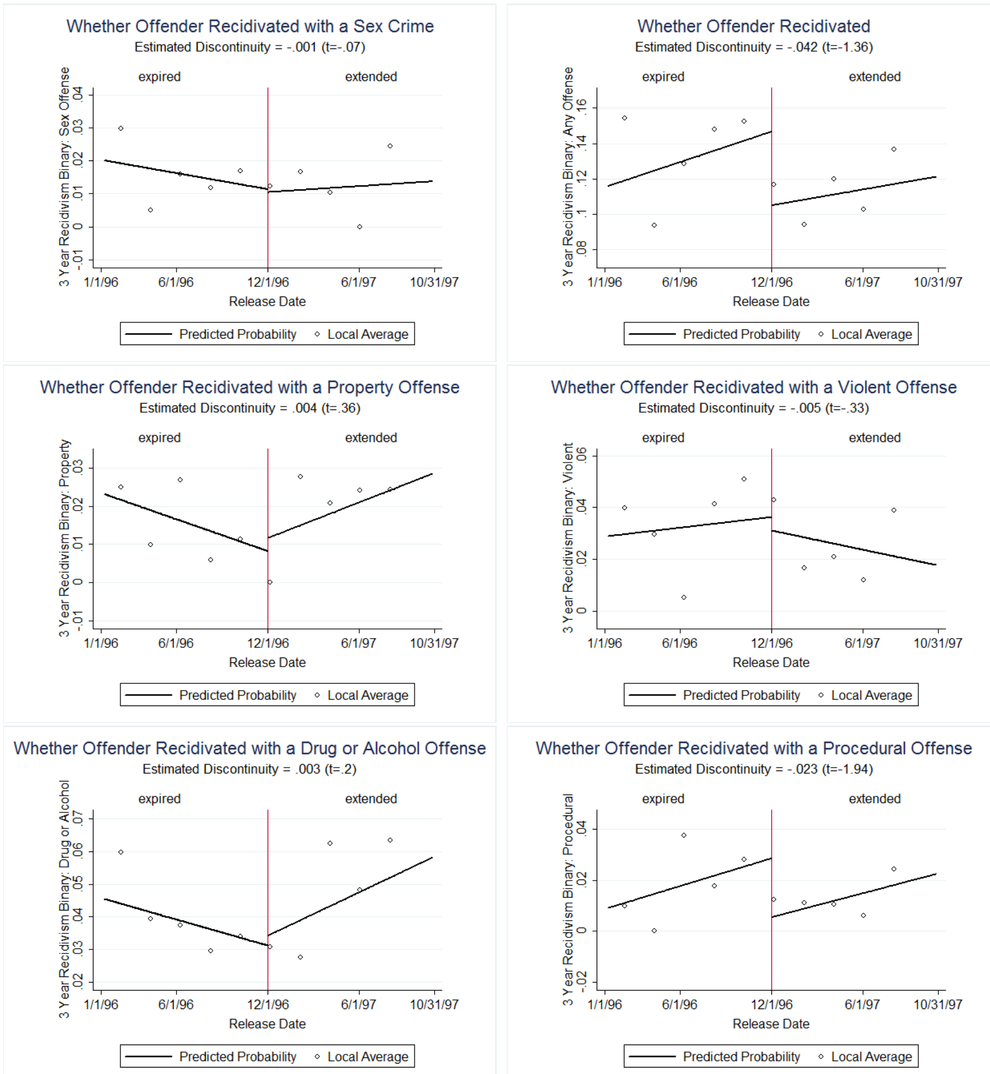

Figure 5: Effect of Registry Extension on Recidivism

Notes: Recidivism is calculated for the 3 years after registry expiration or extension. The running variable is the offender's estimated registry date. The vertical line denotes the law's effective date. Local averages are for 60 day bins.

Table 1: Summary Statistics

	registry expired	registry extended
registered in 2012	0.187	0.554
	(0.390)	(0.497)
any offense type: whether offender recidivated	0.131	0.113
	(0.338)	(0.317)
sex offense: whether offender recidivated	0.016	0.012
	(0.125)	(0.109)
property offense: whether offender recidivated	0.016	0.020
	(0.125)	(0.141)
violent offense: whether offender recidivated	0.033	0.024
	(0.177)	(0.154)
drug or alcohol offense: whether offender recidivated	0.038	0.046
	(0.192)	(0.211)
procedural offense: whether offender recidivated	0.019	0.014
	(0.136)	(0.118)
proportion black	0.402	0.387
	(0.491)	(0.487)
proportion Hispanic	0.005	0.004
	(0.070)	(0.063)
age	34.655	34.553
	(11.921)	(12.514)
proportion male	0.990	0.985
	(0.099)	(0.122)
num. previous convictions	3.109	3.016
	(3.302)	(3.309)
num. previous sex offenses	1.394	1.427
	(0.741)	(0.774)
num. previous incarcerations	1.825	1.775
	(1.942)	(1.966)
days incarcerated	846.480	937.123
	(1468.354)	(1490.555)
num. of observations	1015	990

Notes: Summary statistics are reported for the "registry expired" group (estimated registry dates from January 1, 1996, to November 30, 1996) and the "registry extended" group (estimated registry dates December 1, 1996, to October 31, 1997). For offenders sentenced to incarceration, the registry date is estimated using release date. For individuals sentenced to probation, it is sentencing date. Recidivism measures are for the 3 years after an offender's expected registry expiration date. Standard deviations are reported in parentheses.

Table 2: Tests of RDD Specification

	11 month bandwidth	6 month bandwidth
	(1)	(2)
proportion black	-0.048	-0.178***
	(0.046)	(0.060)
proportion Hispanic	0.005	0.008
	(0.008)	(0.013)
proportion male	0.004	0.004
	(0.009)	(0.012)
age	-0.198	0.163
	(1.097)	(1.475)
num. previous convictions	-0.225	-0.168
	(0.283)	(0.361)
num. previous sex offenses	0.031	0.045
	(0.065)	(0.084)
num. previous incarcerations	-0.265	-0.193
	(0.178)	(0.236)
days incarcerated	-106.108	-54.163
	(134.311)	(174.171)
num. of observations	2005	1069
controls	no	no
time polynomial	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Standard errors are in parentheses. Each value in the table is generated by a separate regression in which the control variable for which the row is named is the dependent variable, and the independent variables are "registry extended" (for which the coefficient is reported) and a polynomial function of estimated registry date (the running variable). The polynomial date function is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and are clustered on the running variable.

Table 3: Effect of Registry Extension on Continued Registry

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
Study Group: Discontinuity				
registered after 16 years				
registry extended	0.283***	0.291***	0.289***	0.294***
	(0.040)	(0.038)	(0.056)	(0.056)
num. of observations	2005	2005	1069	1069
Out of Sample Evidence: Continued Registry				
registered after 10 years				
intercept	0.842***	1.029***	0.846***	0.984***
	(0.024)	(0.095)	(0.032)	(0.100)
num. of observations	888	888	463	463
registered after 13 years				
intercept	0.646***	0.668***	0.631***	0.369***
	(0.032)	(0.105)	(0.046)	(0.132)
num. of observations	1025	1025	545	545
controls	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: The outcome variable is whether an individual was registered as a sex offender in 2012 in North Carolina. The reported coefficients are for the variable "registry extended." For the 10 year and 13 year samples, I estimate an equation where the only regressor(s) is the time polynomial. I report the intercept which represents the proportion of offenders registered at the cutoff. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. Robust standard errors are reported in parentheses and clustered on the running variable.

Table 4: Effect of Registry Extension on Recidivism

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0009	-0.0003	-0.0020	-0.0005
	(0.011)	(0.011)	(0.015)	(0.015)
any crime type: whether offender recidivated				
registry extended	-0.0419	-0.0269	-0.0583	-0.0392
	(0.031)	(0.029)	(0.042)	(0.039)
violent offenses: whether offender recidivated				
registry extended	-0.0052	-0.0015	-0.0183	-0.0129
	(0.016)	(0.015)	(0.022)	(0.021)
property offenses: whether offender recidivated				
registry extended	0.0035	0.0078	0.0040	0.0073
	(0.010)	(0.010)	(0.012)	(0.012)
drug and alcohol offenses: whether offender recidivated				
registry extended	0.0032	0.0096	-0.0148	-0.0064
	(0.016)	(0.016)	(0.020)	(0.021)
procedural offenses: whether offender recidivated				
registry extended	-0.0231*	-0.0213*	-0.0044	-0.0033
	(0.012)	(0.012)	(0.017)	(0.018)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the 3 years after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

Table 5: Test for Differential Attrition

	registry expired	registry extended
offender current status:		
registered in North Carolina	13.17%	35.67%
registered in another state	8.98%	8.19%
not registered anywhere	67.07%	43.27%
number checked	167	171

Notes: The subsample of offenders for this test was selected from those closest to the cutoff and restricted using a name uniqueness index. Offender current status was confirmed using the National Sex Offender Public Website.

7 Appendices

7.1 Alternative Recidivism Windows

Table 6: Effect of Registry Extension on Recidivism within 1 Year

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0045	-0.0041	-0.0083	-0.0069
	(0.008)	(0.008)	(0.012)	(0.012)
any crime type: whether offender recidivated				
registry extended	-0.0013	0.0081	-0.0243	-0.0124
	(0.022)	(0.021)	(0.030)	(0.029)
violent offenses: whether offender recidivated				
registry extended	-0.0049	-0.0030	-0.0050	-0.0026
	(0.010)	(0.009)	(0.013)	(0.013)
property offenses: whether offender recidivated				
registry extended	0.0066	0.0091	-0.0025	-0.0007
	(0.008)	(0.008)	(0.009)	(0.009)
drug and alcohol offenses: whether offender recidivated				
registry extended	0.0009	0.0040	-0.0104	-0.0066
	(0.010)	(0.011)	(0.014)	(0.014)
procedural offenses: whether offender recidivated				
registry extended	0.0014	0.0023	0.0086	0.0094
	(0.009)	(0.008)	(0.013)	(0.013)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the first year after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

Table 7: Effect of Registry Extension on Recidivism within 2 Years

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0053	-0.0051	-0.0175	-0.0155
	(0.009)	(0.009)	(0.014)	(0.013)
any crime type: whether offender recidivated				
registry extended	-0.0376	-0.0251	-0.0579*	-0.0413
	(0.025)	(0.024)	(0.034)	(0.032)
violent offenses: whether offender recidivated				
registry extended	-0.0121	-0.0097	-0.0128	-0.0101
	(0.013)	(0.012)	(0.018)	(0.017)
property offenses: whether offender recidivated				
registry extended	0.0029	0.0064	0.0056	0.0076
	(0.009)	(0.009)	(0.011)	(0.011)
drug and alcohol offenses: whether offender recidivated				
registry extended	-0.0016	0.0032	-0.0256	-0.0198
	(0.014)	(0.014)	(0.018)	(0.018)
procedural offenses: whether offender recidivated				
registry extended	-0.0118	-0.0104	0.0038	0.0055
	(0.010)	(0.010)	(0.015)	(0.015)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the 2 years after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

Table 8: Effect of Registry Extension on Recidivism within 4 Years

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0003	0.0005	0.0016	0.0036
	(0.011)	(0.011)	(0.016)	(0.015)
any crime type: whether offender recidivated				
registry extended	-0.0332	-0.0155	-0.0559	-0.0347
	(0.032)	(0.030)	(0.043)	(0.039)
violent offenses: whether offender recidivated				
registry extended	-0.0000	0.0047	-0.0105	-0.0047
	(0.016)	(0.016)	(0.022)	(0.021)
property offenses: whether offender recidivated				
registry extended	0.0107	0.0153	0.0091	0.0121
	(0.010)	(0.010)	(0.013)	(0.013)
drug and alcohol offenses: whether offender recidivated				
registry extended	0.0026	0.0102	-0.0264	-0.0179
	(0.018)	(0.017)	(0.022)	(0.022)
procedural offenses: whether offender recidivated				
registry extended	-0.0207*	-0.0188	-0.0114	-0.0110
	(0.012)	(0.012)	(0.018)	(0.018)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the 4 years after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

Table 9: Effect of Registry Extension on Recidivism within 5 Years

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0016	-0.0005	0.0016	0.0036
	(0.011)	(0.011)	(0.016)	(0.015)
any crime type: whether offender recidivated				
registry extended	-0.0322	-0.0119	-0.0474	-0.0260
	(0.032)	(0.030)	(0.043)	(0.039)
violent offenses: whether offender recidivated				
registry extended	0.0042	0.0098	-0.0089	-0.0029
	(0.017)	(0.016)	(0.023)	(0.021)
property offenses: whether offender recidivated				
registry extended	0.0084	0.0139	0.0040	0.0067
	(0.011)	(0.011)	(0.015)	(0.015)
drug and alcohol offenses: whether offender recidivated				
registry extended	-0.0110	-0.0023	-0.0342	-0.0253
	(0.020)	(0.020)	(0.026)	(0.025)
procedural offenses: whether offender recidivated				
registry extended	-0.0206	-0.0182	-0.0146	-0.0123
	(0.013)	(0.013)	(0.019)	(0.019)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the 5 years after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

7.2 Classification of Conviction Crime Types into Categories

1. Violent Offenses - assault, attempted murder, kidnapping, manslaughter, murder, robbery
2. Property Offenses - breaking and entering, burglary, forgery, fraud, theft
3. Drug and Alcohol Offenses - DUI, manufacture or sale of controlled substances, possession of controlled substances, possession of drug paraphernalia
4. Procedural Offenses - contempt of court, failure to appear (felony), obstructing justice, possession of firearm by felon, post release revocation