Optimal Punishment Lengths and Recidivism: Evidence from Sex Offender Registries

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Abstract

To estimate the causal effect of longer sex offender registry periods, 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 individuals were removed from the registry after 10 years, while others stayed on it. Results indicate that there is little evidence that an increase in registry length decreases sex crime recidivism as intended. I also use duration models to compare the recidivism trajectories over the first 10 years of registry for those who were required to registry to those who were not (based on prison release or sentencing date). I find that 15 percent of those required to register are later convicted of registry-related offense, and I show that registry actually decreases the time before an individual commits a recidivist offense.

1 Introduction

Although incarceration, probation and monetary penalties are the most common forms of punishment for criminal offenses, legal and social systems exact a variety of other punishments as well. Convicted individuals can be sentenced to other programs (e.g. community service or drug treatment) and have various privileges revoked like voting rights, gun-ownership, or driving – often even after having completed their initial formal sentences. The growing public availability of criminal records, including sex offender registries, means that ex-offenders are known as such in society, and status as a former offender carries numerous social and economic costs not regulated by the legal system.[1]

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[1]For example, there are employment penalties associated with having a past conviction. See the literature on "Ban the Box" policies for evidence of employers' aversion to hiring ex-offenders. (Doleac and Hansen, 2019; Agan and Starr, 2017)

All of these kinds of punishment have negative effects on those on whom they are imposed, some of them, like decreased employment opportunities, can even lead to recidivism. Ideally policy-makers and court officials consider these costs in addition to any crime-reducing effects when assigning punishment and setting the lengths of said punishments. Conditional on assigning a particular kind of punishment, its length can determine the magnitude and even the direction of effects on recidivism. Longer incarceration sentences appear to reduce recidivism and have ambiguous impacts on deterrence.[2,3]

Much less is known about optimal sentence length for alternative forms of punishment. These conditions frequently occur outside of the corrections system after official sentences are complete when we expect ex-offenders to be working towards reintegration into society. Often, these other forms of punishment have no given length. For example, in some states, individuals convicted of a felony are never restored voting rights or are required to go through a lengthy appeals process to regain them.

Sex offender registration is one of the few punishments that can be required for the remainder of the individual's life. Registry laws stipulate that individuals convicted of certain sexually-oriented offenses submit and update physical descriptions and address information as well as photos to local authorities for public dissemination. Registry is required for a set amount of time – usually between 10 years and for the rest of the individual's life. In this paper, I undertake the first explicit, causal analysis of sex offender registry length. I exploit a natural experiment in which a group of sex offenders were removed from North Carolina's sex offender registry in 2006, and others were not. These individuals had been registered for exactly 10 years before their removal, so I am comparing the effect of removal after 10 years of registry compliance with remaining on the registry for an indeterminate amount of time. I find that there are no reductions in recidivism attributable to registration after 10 years. Instead, I show that those who remain on the registry are frequently charged with offenses related to registry itself, imposing costs on the criminal justice system and the registrants.

Longer registry lengths are intended to deter sex offense recidivism by restricting the access that registered offenders have to potential victims. While this paper is the first to consider registry length specifically, there is mixed evidence on the effect of registry on recidivism more generally. Comparing those individuals who were required to register to those who were not based on prison release date, sentencing date, or offense date, 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, Miller, Walker, and Marshall, 2011; Schram and Milloy, 1995; Adkins, Huff, Stageberg, Prell, and Musel, 2000; Zgoba, Veysey, and Dalessandro, 2010).[4]

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[2]Both increased sentence length, itself, (Kuziemko, 2013) and incapacitation (Owens, 2009; Barbarino and Mastrobuoni, 2014, e.g.) reduce recidivism.

[3]The literature on deterrence is more mixed, with Helland and Tabarrok (2007); Abrams (2012); Drago, Galbiati, and Vertova (2009) and Hansen (2015) showing that increased sentences deter crime, and Lee and McCrary (2005) showing that they do not.

[4]Increased registry lengths can also be justified as increasing the cost of committing an offense for would-be offenders. There is similarly mixed evidence on the effect of implementing (or expanding) a registry in a state on aggregate crime levels. Many studies of state-level changes find no consistent effects either way (Ackerman, Sacks, and Greenberg, 2012; Sandler, Freeman, and Socia, 2008; Vasquez, Maddan, and Walker, 2008;

The increase in registry length that I study here was applied retroactively, creating an exogenous source of variation. State legislators extended registry length from 10 years to 30 years, applicable to all current registrants. Around 900 individuals had already been removed because their registry had expired (and they had not been convicted of any new sex offenses), so whether an individual'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 registrants and authorities could not manipulate on which side of the cutoff individuals fell. There is little reason to believe that this type of manipulation is possible. Each sex 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 individual 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. I verify that the density of the registry date is smooth, and I test whether there are discontinuities across the cutoff in observable individual characteristics (including criminal record). If a party had manipulated registry dates to make sure that the restriction applied to more individuals, or at least the most dangerous individuals, then one would expect the groups on either side of the cutoff to differ in quantity or observable characteristics. I also estimate a difference-in-differences model to examine differences in recidivism behavior across these groups, leveraging individual fixed effects to control for time-invariant characteristics related to recidivism.

I find no evidence that the registry extension reduces sex offense recidivism, which is the stated goal of the extension. 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, Miller, Walker, and Marshall, 2011; Zgoba, Veysey, and Dalessandro, 2010). This non-effect is a result of the fact that recidivism rates are nearly zero after 10 years for individuals who have not already committed a recidivist offense – out of the nearly 1600 individuals in this study, less than 15 recidivist sex offenses were committed in the 10-15 years after initial registry.

Given that there are no substantial differences in recidivism after 10 years for those who remain registered compared to those who are removed, I apply duration models to the first 10 years of registry (the period before removal) to determine if registry has any effect on earlier recidivism. If there is an amount of time over which registry does appear to reduce recidivism, it could suggest an optimal registry length. For this analysis, I compare the group of individuals who were released from prison or sentenced to probation before the registry was created to those released or sentenced after. I find that registry leads to more recidivism overall. There is also some evidence that those required to register

Walker, Maddan, Vásquez, VanHouten, and Ervin-McLarty, 2005; Maurelli and Ronan, 2013), though others report that registries reduce aggregate sex crimes (Prescott and Rockoff, 2011; Letourneau, Bandyopadhyay, Armstrong, and Sinha, 2010).

also commit more violent and financially-motivated offenses, but sex crime recidivism is unaffected.

This study makes three main contributions to the existing literature. First, to my knowledge this is the first study to explicitly explore the issue of sex offender registry length, 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, which is underscored by the existence of federal mandates on this topic. Second, results are informative in the debate on sex offender registries more generally, especially the results from duration models that indicate that registry can lead to more non-sex crime recidivism, possibly in response to the social and economic costs of the registry for registrants. Third, while sex offender registries are important in their own right, these results can also inform our understanding of other stigma-based punishments for ex-offenders. As the availability and ease of access of public criminal records has grown, ex-offenders face more social and economic repercussions that could prevent them from reintegrating into society and lead to more recidivism.

The particular question of optimal registry length is important from a policy perspective as well because the Adam Walsh Act, passed in 2006, increased the federally-mandated minimum required registry time to 15 years for the least serious offenses, and to lifetime for the most serious (McPherson, 2007). Under these federal stipulations, all registrants would be required to register for 15 years at least, so my results imply that even the shortest registry length required by the Adam Walsh Act may be inefficiently long. The evidence presented here and in the literature suggesting the ineffectiveness of sex offender registries at deterring recidivism is striking in light of the significant costs incurred by both law enforcement and registered 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 individuals on the registry for an extended period of time may not be fully justified by reductions in recidivism, although there is room for other benefits such as deterring would-be first time offenders.

2 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. It was first posted online in a searchable format on May 11, 2000 (Agan, 2011).

In 2006, the North Carolina state legislature voted to extend the registry period for sex offenders from 10 years to an indefinite length (presumably resulting in lifetime registration), 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 individuals whose registry period expired before the law took effect – those who registered between January 1, 1996, and November 30, 1996 (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 individuals will form the basis for my identification strategy in the registry extension models, described in detail in the next section.[5]

Not all offenders whose registry was extended will fulfill the lifetime registry requirement.[6] 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 individual can petition to have his or her name removed from the registry after spending 10 years on it.[7] This means that the results here will compare automatic removal to petition-based removal. Reported results are intent to treat, including all ex-offenders whose initial registry date would have qualified for extension, regardless of their actual extension status, as I am unable to observe it at the relevant time.

Economic theory is ambiguous as to whether the individuals to whom the registry requirement applied 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 the probability that an offender is caught.[8]

On the other hand, individuals may be more likely to commit crimes if registered. 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 can diminish an ex-offender's quality of life reduce this benefit. For example, these restrictions make it difficult for ex-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 individual's quality of life and potentially reduce the opportunity cost of crime, disincentivizing law-abiding behavior. These economic roadblocks may also drive individuals to commit financially-motivated crimes such as theft.

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[5]Other changes were made to the registry over time, including adding new offenses and creating elevated classifications based on crime severity and recidivism. Satellite-based monitoring was instituted in 2007, but only applied to convictions or releases after January 1, 2007. Residency restrictions were also instituted in 2007, and applied to all registered offenders. Although the implementation of residency restrictions is around the time of the extension, it provided a grandfather clause such that registered offenders living in violation were not required to move. If a registered offender did move after its application, then his or her new residence needed to be compliant. Kang (2017) uses the grandfather clause to show that these restrictions may work to reduce recidivism for some recently-released offenders.

[6]Two years later, the state reduced the maximum registry length to 30 years, but the reduction only applied to offenses committed after December 1, 2008. Some policy documents may say that the registry length in North Carolina is 30 years, but the sample in this study is subject to lifetime registration.

[7]All but those convicted of the most serious offenses are allowed to petition for removal starting 10 years from their original registry dates. For a petition to be successful, the registrant 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).

[8]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, 2017) and variation in the severity of punishment (e.g. Hansen, 2015; Abrams, 2012; Drago, Galbiati, and Vertova, 2009).

3 Identification and Methods

3.1 Registry Extension

3.1.1 Regression Discontinuity Models

I identify the effect of the sex offender registry extension on recidivism by comparing those whose registry was extended to those whose registry was allowed to expire. It is important to emphasize that whether an individual's registry was extended or allowed to expire depends on what date the individual 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 over which individuals would get removed and which would continue to stay on the registry. In addition, it would have been impossible for judges, prosecutors, or registrants to predict 10 years earlier that the registry date of December 1, 1996, would determine whether an individual'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; i.e. the estimates will compare the individuals just after the cutoff to those just before. Formally, I estimate the model:

$$Outcome_i = \alpha + \beta_1 Registry\ Extended_i + f(RegistryDate_i) + u_i \tag{1}$$

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 RD 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, this cutoff is in all likelihood exogenous to individuals and their characteristics.

Although the running variable in the regression discontinuity model is a date, the model should not be categorized as an interrupted time series model. Here, the date acts as an assignment variable for individuals, not as the date that a policy went into effect. Although the tests for identification support will address concerns relevant for a regression discontinuity in time (RD-i-T) model, it should not be classified as such either. In fact, Hausman and Rapson's recent work on RD-i-T models suggests that natural experiments relying on date-based eligibility for a program shouldn't be classified as RD-i-T models, giving the example of Ito (2015) which focuses on a start-of-service date threshold applied retroactively in an electricity program. Similarly, in this study eligibility for automatic removal from the registry depends on initial registry date 10 years earlier. Nonetheless, I implement a number of the tests suggested by Hausman and Rapson including displaying residuals of raw data after removing covariates, displaying the raw and residualized data with various polynomial orders and bandwidths, testing for discontinuities in covariates, and

estimating a "donut" RD.

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 individual'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 individual appeared on the registry on November 13, 2012, as the outcome variable.[9] Whether an individual 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 2006-2011 period. It likely declined over that time, and I will provide out-of-sample evidence (registrants from later years) to suggest what portion of the individuals whose registry was extended were still registered over time.

I estimate the main outcome models using an MSERD-optimal bandwidth (as suggested by Calonico, Cattaneo, Titiunik, et al. (2014)) and also show the results for a range of bandwidths. In addition to testing for sex crime recidivism, I also test for an effect on the likelihood of recidivating with any type of crime (excluding registry-related offenses, probation revocations and procedural-type offenses), financially-motivated crime, violent crime, drug crime, and registry-related offenses. The models focus on recidivism within 5 years of the extension, which is 10-15 years from initial registry.[10] The Adam Walsh Act mandated that the federal minimum registry length for the least serious category of offenses is 15 years, so this range is particularly important from a policy perspective.

3.1.2 Difference-in-Differences Models

As an alternative approach to measuring the effects of registry extension, I leverage the fact that I have data from the period before registry removal in a difference-in-differences model. I use a panel of convicted offenses for the 5 years before either registry expiration or extension and the 5 years after for each individual. The identifying assumption in

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

[10]Estimates of recidivism within 3 years similarly show an increase in registry-related offenses and provide no evidence that sex crime recidivism falls due to the registry extension.

this model is that those whose registry expired and those whose registry was extended would have continued to have the same trends over time in recidivism absent the change in sex offender registry length.

I estimate the following:

$$Outcome_{it} = \alpha + \beta_1 Registry\ Extended_{it} + \gamma_i + \lambda_t + u_{it} \tag{2}$$

The coefficient of interest is β_1, where $Registry\ Extended_{it}$ is equal to one for the individuals in the "registry extension" group in the 5 years after extension. I use individual fixed effects (γ_i) to account for time-invariant individual characteristics that may affect recidivism. I also control for fixed effects for years since initial registry (λ_t) because recidivism falls over time in general and to account for any changes to the judicial system that may impact ex-offenders in general.

I also estimate the analogous event study model of the form:

$$Outcome_{it} = \alpha + \sum_{j=-2}^{4} \beta_j Years\ Since\ Registry\ Extended_{it-j} + \gamma_i + \lambda_t + u_{it} \tag{3}$$

The summation in the event study specification includes lags of the treatment variable (when $j \geq 0$) and leading indicators of treatment ($j < 0$). The term $Years\ Since\ Registry\ Extended_{it-j}$ is an indicator variable equal to one when an individual's registry was extended j years ago. In the results section, I also generate event study graphs of the effects of registry extension over time. The leading indicators are particularly important as a means of supporting the identifying assumption - if they are not statistically different from zero, that suggests that the two groups were on a similar trajectory before the 2006 registry extension.

3.2   Initial Registry Length

To complement the analysis of the registry extension, I also consider the effect of initial registration on recidivism over the first 10 years of registry. I do so by comparing individuals who were released from prison or sentenced to probation for their first sex offense between January 1, 1994, to June 30, 1995, (did not have to register), and July 1, 1996, to December 1, 1997, (did have to register). The cutoff date, January 1, 1996, was first discussed in 1995, giving district attorneys, defense attorneys, and parole boards the opportunity to manipulate which individuals were required to register and which were not. For this reason, I do not include those closest to the cutoff (6 months on either side).

I use duration models in order to compare the likelihood that an individual recidivates at a given point in time conditional on having not done so yet. This is important in the policy environment - only those who have not recidivated with a sex offense are eligible for removal. First, I present graphical evidence using survival curves for each type of offense, and I nonparametrically test for the equality of survival functions using a Wilcoxon test.

Second, I present evidence using Cox proportional hazard models to estimate:

$$\lambda(t|x) = \lambda_0(t)exp(\beta x) \tag{4}$$

Where $\lambda(t|x)$ is the hazard rate conditional on covariates x, and $\lambda_0(t)$ is the baseline hazard rate common to all sex offenders. The vector x always includes a binary indicator for whether the individual was required to register as a result of the date he or she was released or sentenced. In other models, I also control for individual demographic and criminal history variables as well as fixed effects for the quarter the individual was released from prison or sentenced (to deal with seasonality and control flexibly for trends over time). I also include a specification where I control for whether the registry has been posted online.

For the effect of the initial registry to be interpreted as causal, we have to believe that the individuals who were not subject to registry are an appropriate counterfactual for those who were subject to it. That is less likely in this scenario than in the registry extension analysis. Again, for that reason, I omit individuals who would have registered July 1, 1995, to June 30, 1996, as those are the individuals whose status' are most likely to have been manipulated. As support for the identification assumption, I test for differences between these two groups in observable characteristics. To the extent that relevant unobservable characteristics are correlated with these observables, similarity between these two groups can be interpreted as support for the identifying assumption.

4   Data

Data on sex offenders and their criminal histories come from the North Carolina Department of Public Safety's Offender Public Information website. 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. At the time of download, 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 individuals registered at the time of download. Most information can be obtained from the DPS data, but the individuals' initial registry dates are only available in the registry data for the individuals who remain on the sex offender registry. Since my research design also requires a registry date for individuals who are no longer registered and those who never 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). For this reason, I also perform a "donut" RD in which I drop the first 10 days before the registry cutoff.

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These two dates are reported in the DPS records, and I use them to proxy for the registry date for all individuals. I will simply refer to this date as the "registry date" going forward.

In the registry extension models, I use this date to designate which individuals are classified as "registry expired" and "registry extended." Because the registry began on January 1, 1996, only individuals who registered within the first 11 months of the registry can belong to the "registry expired" group. This makes an 11 month bandwidth (22 months total) the largest possible. Individuals with release or sentencing dates between January 1, 1996, and October 31, 1997, serve as the main study group in the registry extension models, although in practice I will use data-driven bandwidth selection procedures and perform bandwidth sensitivity analysis.

In the initial registry models, I separate the individuals into "registered" and "not registered" groups. I omit July 1, 1995, to June 30, 1996, so the "not registered" group is those whose registry date is January 1, 1994 to June 30, 1995, and the "registered" group is those whose registry date falls between July 1, 1996 and December 1, 1997. There is some overlap between the two samples, but all classifications are made based on the *first* time an offender is convicted of a sex offense in North Carolina, so there should be no changes to the registry date based on later offenses.

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 the number of offenses for which an individual was convicted before he or she registered. I am also able to control for whether an individual has been incarcerated, whether they committed a crime with a child victim and whether they committed a serious offense (a class D felony or above, requiring imprisonment under North Carolina's structured sentencing guidelines). 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. For the registry extension analysis, I determine the number of convictions for sex offenses within 5 years after removal from the registry or registry extension for each individual. I build a similar measure for offenses of any type, financially-motivated offenses, violent offenses, drug offenses, and registry-related infractions.[11]

In order to confirm that the group of individuals 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 individuals for whom there is no listed Department of Corrections number in the registry data.

Table 1 contains summary statistics;[12] it shows means, standard deviations and differences in means of recidivism measures and control variables for the two sets of control and treatment groups: "registry expired" vs. "registry extended" and "registry not required" vs. "registry required." The first row of the table corresponds to the measure of

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[11]I include a full list of the offenses in each category in the appendix. Notably, registry-related offenses and parole/probation revocations are not included in the "any crimes" measure as they don't indicate a new crime.

[12]Although I will use MSERD-optimal bandwidths for the RD models, I report summary statistics and tests of identification for the full bandwidth because those bandwidths vary by outcome.

continued registry discussed in the previous paragraph. The difference in means indicates that the "registry expired" group is on average 37.5 percentage points less likely than the "registry extended" group to appear on the registry in 2012. Similarly, the "registry not required" group is on average 31.3 percentage points less likely than the "registry required" group to appear on the registry in 2012. These differences are significant at the 1 percent level.

Most registrants are white, but nearly 40 percent are black. Less than 1 percent of registrants are Hispanic. Nearly 99 percent of registrants are male and the average age is around 35 at the time of initial registry. On average, individuals had between 2 and 3 previous convictions and about half have been incarcerated previously. Over 70 percent committed a crime with a child victim,[13] and around 25 percent committed a relatively serious offense requiring incarceration.

In terms of outcome variables, although some individuals are convicted of another crime later (on average around 0.3 of them), few of them are sex offenses (less than 0.04 on average). For the registry extension sample groups, the only statistically significant difference between the groups of interest is that the "registry extended" group is convicted of more registry-related offenses. This is reasonable given that the other group is not subject to registry rules in this period.

The group required to register initially (compared to those who did not) are similarly convicted of more registry-related offenses, but they are also convicted of less crimes overall and financially-motivated crimes specifically.

5 Results

5.1 Registry Extension

5.1.1 Support for identifying assumption

The identifying assumption of the registry extension model is that the determinants of recidivism vary smoothly across the extension cutoff. There are few ex ante reasons to doubt this assumption in this context. It would be violated if judges, prosecutors, or registrants were able to affect which individuals 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 individuals' prison releases until after the cutoff or scheduled more sentencing hearings after the cutoff in order to maximize the number of individuals 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 10 day intervals in Figure 1. The vertical line denotes the cutoff date and the

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[13]This may seem particularly high, and the prevalence of the charge "indecency with a child" appears to provide a reason. At this time in North Carolina, "statutory rape" did not exist as a charge, so many of these charges could be the outcome of scenarios involving non-violent, willing acts with an individual too young to legally give consent.

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 that corresponds to the winter court holidays.[14]

However improbable given the required foresight, we could also worry that authorities attempted to rearrange sentencing dates or prison release dates to extend the registry length for individual at higher-risk of re-offending. In order to demonstrate that there are no compositional changes in the types of individuals across the threshold, I test whether 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 RD 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, of individuals who registered in the same month. The vertical line marks the cutoff date for registry extension. The first row of figures corresponds to demographic variables. The remaining subfigures in Figure 2 are generated using the constructed criminal history variables.

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 use the maximum bandwidth (11 months on either side of the cutoff) and are estimated using ordinary least squares.

All estimates are statistically indistinguishable from zero with the exception of the binary indicator for whether the offender is black. While it is zero when the running variable is fit linearly, the quadratic model estimates that there are less black offenders on the registry extended side of the cutoff. This discontinuity seems to be driven by the fact that in the first 30 or 60 days (the first 1 or 2 dots) directly after the cutoff, there are relatively few registering black offenders. 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 registered in the first 30 days after the effective date are omitted, there is no longer a discontinuity in the racial composition of offenders, and results are not statistically different from the main results (reported in columns 3 and 4 in Appendix Table A1).

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[14]I also perform a test for manipulation in registry dates (using a methodology from Cattaneo, Jansson, and Ma (2018)), which tests for problematic fluctuations in density. The result of this test does support the hypothesis that there is manipulation. The winter court holidays are likely contributing if not causing this result, as the week of Thanksgiving (week 47) and the week of Christmas and New Years (week 52) consistently have the lowest number of new registrants. Using historical data to assign would-be registry dates to individuals not in the sample group, I do a placebo test in which I perform this same test to check for discontinuities annually at December 1 from 1990-2012. Results are shown in Appendix Figure A1. Of the 22 years tested, 7 are statistically significant, so the result for the actual treatment threshold is less worrisome.

5.1.2 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. Individuals in the "registry extended" group are 37.5 percentage points more likely to remain on the registry until at least 2012, but, again, this difference is likely larger for the period over which the outcomes are measured (2006-2011).

The first panel in Figure 3 contains an RD graph showing the effect of registry extension on continued sex offender registry status. The y-axis represents the proportion of individuals registered in 2012 for 30 day bins. It is clear from the graph that the discontinuity is large at nearly 20 percentage points. Any individuals 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. I drop those who have been convicted in the first 10 years post registry for a sex offense, but the local averages on the left side are not all zero, perhaps due to out-of-state convictions. Some also did not register as required by state law or may have died or left the state, which could explain that not all expected registrants appear in the registry, even within the required registry period.

Table 3 contains results indicating the effect of registry extension on continued registry obtained by estimating equation 1 using continued registry as the outcome variable. Reported coefficients are for the variable "registry extended" and indicate the difference in the probability that an individual was registered in 2012 at the cutoff. Columns 1 and 2 use the maximum bandwidth (11 months on either side of the cutoff) and are estimated using ordinary least squares. Columns 3 and 4 use an MSERD-optimal bandwidth and are estimated for uniform and triangular kernels with a linear fit. Estimates range from 0.188 to 0.228; all are significant on the 1 percent level.

Only 53 percent of individuals in the registry extended group were still registered in 2012 (their 16th year of registry); this likely understates the proportion of offenders who were registered during their 10th to 15th years of registry, the time over which outcomes are measured. To quantify how much the discontinuity is understated, I use the entire set of sex offenders required to register before November 12, 2012, to provide out-of-sample evidence on the magnitude of the discontinuity at the time of registry expiration and during the period over which the outcomes are measured.

The lower panel of Figure 3 displays the proportion of individuals (in 60 days bins) who were registered relative to their time on the registry as of November 12, 2012. Offenders who were registered after November 12, 2002, are represented by markers left of the leftmost vertical line, and they were still ineligible for removal in 2012. Approximately 83 percent of them were registered. Individuals who registered between December 1, 1996, and November 12, 2002, were eligible for removal, and they are represented in the area between the two vertical lines. The leftmost bins in this range correspond to those recently eligible as of 2012, and those to the right have been eligible for up to 6 years. Those to the right of the rightmost vertical line belong to the group automatically removed (i.e. the "registry

expired" group).

There is a steady decline in registry over time as offenders have been eligible for longer with a precipitous drop-off for the group subject to automatic expiration. Over the period during which I measure recidivism, I will suppose that the proportion registered dropped from around 80 percent to around 50 percent for the "registry extended" group and around 20 percent for the "registry expired" group. Therefore, the maximum difference in registration status is around 60 percentage points.

5.1.3   RD Model: Effect of registry extension on recidivism

Figure 4 contains RD figures and indicates that registry extension does not appear to have any effect on most kinds of recidivism, including sex offense recidivism. The figures display binned means of raw data (for 30 day bins) as it is the most transparent way to display the data. Recognizing that the regression models will leverage linear and quadratic terms of the running variable and covariates, I also include figures with linear and quadratic fits of the underlying raw data and data that his had the effects of covariates removed in Appendix Figures A2 through A5. In Figure 4, I have also shaded the MSERD-bandwidths (for a linear model with covariates estimated using a uniform kernel) for each outcome. These bandwidths are notably smaller than the maximum bandwidths, and in some circumstances could plausibly lead to different conclusions compared to the maximum bandwidth. For this reason, I perform a bandwidth sensitivity analysis to provide evidence on which outcomes can considered to be robust to bandwidth selection.

I report corresponding RD results in Table 4. I estimate regression discontinuity models using both a uniform and triangular kernel, with and without controls, using linear and quadratic functions of the running variable. Columns 1-3 present results for the uniform kernel, and columns 4-6 present triangular kernel estimates. Both of these sets of results are estimated according to methods suggested by Calonico, Cattaneo, Titiunik, et al. (2014). To highlight the stability of estimates, I use the same bandwidth for each outcome within kernel type (the MSERD bandwidth for the linear model with controls). Results using the maximum bandwidth and estimating equation 1 using OLS (with standard errors clustered on the running variable) are in Appendix Table A1. In each set of results, I first present a linear model without controls, a linear model with controls, and then a quadratic model with controls.

I also report results where those released or sentenced between November 21 and 31 are dropped because their official registry date is ambiguous. These results are reported in Appendix Table A1 in columns 1 and 2, and results are very similar.

For sex offenses, the bandwidth selection procedure selects a bandwidth for the uniform kernel that is rather small. In fact, there are no sex offenses committed by either group for the individuals whose registry extension/expiration date is within 32 days of the cutoff. The sex offense recidivism rates are low for both groups even over the maximum bandwidth - out of 784 individuals in the group whose registry automatically expired, only 5 committed a recidivist

sex offense. Similarly, for the group of 788 whose registry was extended, only 10 committed one or more. Although it is notable that the group who stayed registered committed more recidivist sex crimes, the low occurrence of this kind of recidivism leads to varied and imprecise estimates of the effects on sex offense recidivism. Estimates range from -0.002 to 0.001, and none of these estimates are statistically distinguishable from zero. Interpreting the magnitude of the coefficient for the triangular kernel estimation with controls and a linear fit indicates that I can only rule out an increase of over 30 percent in sex offenses for those whose registry was extended, and I can only rule out reductions of more than 10 percent.[15] Because reducing sex offenses is a stated goal of sex offender registries, the lack of convincing evidence of their effectiveness at reducing sex crimes is important for policy analysis.

In some specifications, the "registry extended" group is more likely to be convicted of a violent offense later. In fact, the effect is always positive and similar across models. The magnitude of the effect for the preferred specifications (columns 2 and 5, linear with covariates) are very large, indicating a doubling in violent crime arrests. Because the visual evidence is not particularly compelling in Figure 4 and the increase is not statistically significant in all models, this result should be taken lightly.

Sex offender regulation offenses also appear to increase for the "registry extended" group. Recall that there was a difference in the mean of this outcome between the groups. The effect sizes range from 0.092 to 0.214 additional offenses. From Figure 4, we can tell that there appears to be a change in slope around the cutoff and that the results are reasonable given the small bandwidth selected. These offenses, while seemingly compliance-related in nature, do carry significant punishments, including incarceration, and in the hazard models, I will establish that the high rate of conviction for such crimes starts well before the individuals are eligible for removal.

Rather than exclusively relying on one bandwidth selection technique or another, I also estimate the main models over a variety of bandwidths and plot the resulting coefficients. To isolate the effects of the bandwidth specifically, I estimate equation 1 controlling for the running variable linearly and including controls (like columns 2 and 5 of Table 4). I vary the bandwidth in increments of 5 days from 30 to 300.

Appendix Figure A6 shows the results for each outcome, plotting the coefficient on "registry extended" and the corresponding 90 percent and 95 percent confidence intervals for each bandwidth. The effect on sex offenses is never statistically significant, and the magnitude and sign of the coefficients vary. Also, it is not possible to estimate a coefficient for bandwidths under 60 days because no sex offenses are committed by offenders whose registry dates fall within 58 days of the cutoff.

The coefficients for violent offenses are consistently positive and often rather large. They fall in magnitude as the bandwidth increases, but are close to traditional levels of statistical significance for all bandwidths. The coefficients for registry offenses similarly fall as bandwidth increases, but they are much closer to zero. There is a reasonable

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[15]The results using a quadratic fit of the running variable as also imprecise. They can rule out an increase of over 20 percent and a decrease of more than 60 percent.

explanation for this phenomenon. When all of the individuals in this sample initially registered, they expected to be removed in 10 years. The abrupt nature of the registry extension may have caught some individuals off guard, particularly those who just barely missed the cutoff for automatic removal. For example, if an individual expected to be removed in December, they may not have submitted the required registry update information when it was next due. Likely, local authorities did a better job notifying those further from the cutoff of their continued need to register because they had more notice of the policy change.

5.1.4 Difference-in-Differences Model: Effect of registry extension on recidivism

Table 5 contains results from the difference-in-differences models. In these models, I use the full bandwidth of 11 months of registration dates on either side of the cutoff and create an individual by year (measured as years since registry) panel for the 5 years on either side of the registry expiration/extension date. Panel A presents results from a model estimating equation 2, where treatment effects are averaged over the post-period. Only registry-related offenses are affected by the registry extension. On average, an individual whose registry period was extended committed an additional 0.013 registry-related offenses a year. During the 5 year period before the registry was extended (the "pre-period" here), individuals from both the "registry extended" and "registry expired" groups committed around 0.018 such crimes on average per year. In the period after extension, the "registry extended" group continued to commit around 0.019 of these offenses per year on average, but the "registry expired" group only committed 0.007 per year on average.

Panel B in Table 5 reports the event study coefficients for this difference-in-differences specification. The first 5 coefficients separately report treatment effects of registry extension over the first 5 years of the extension. In the first year after the registry extension, those in the registry extension group are more likely to commit crimes of any type (recall that this does not include registry-related offenses) and registry-related offenses. The effects in the year after extension for any type of offense are only statistically significant on the 10 percent level, but they are sizable. The fact that some of the largest effects on registry-related offenses occur in the first year post-extension suggests that some members of the "registry extended" group may not have been fully aware of the extension. The effects for registry-related offenses are also concentrated in the 4th and 5th year after the registry. There are no other statistically significant effects after the first year of extension for other types of crimes.

The last 2 rows report coefficients on leading indicators. They serve as support for the identifying assumption. None of them are statistically different from zero, indicating that there is no evidence that these two groups were deviating in trends before the extension. The leading indicators for sex offenses are both not estimable because I do not include anyone who recidivates with a sex crime before extension/expiration due to their ineligibility for expiration.

Appendix Figure A7 also represents these event study results. In each figure, I plot the coefficients for lagged

treatment indicators and leading indicators with their corresponding 95 percent confidence intervals.

5.1.5 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 individuals most likely to recidivate were to leave the state as a result of remaining on the registry, then the estimates might overstate a decrease in recidivism caused by the registry. (Although recidivism results generally go in the opposite direction.) 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 instead 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.[16]

Appendix Table A2 summarizes the results. I looked up a total of 338 offenders, of whom 167 (approximately half) came from the "registry expired" group. Critically, the number of offenders who are registered in another state is almost equal across registry types at around 9 percent. This suggests that there is no evidence of differential attrition from the sample.

5.2 Initial Registry

5.2.1 Support for identifying assumption

Whether an individual was required to register as a sex offender in the state of North Carolina depends on the date of their release from incarceration or sentencing date. Those whose relevant date fell before January 1, 1996, were not required to register, and those whose date fell after were required to register.

Because there is some scope for parole boards, district attorneys and judges to influence these dates, the groups on either side of the registration date cutoff are likely to be different. To address this issue, I do not consider the individuals who were released or sentenced within 6 months of the cutoff. While court actors have some sway in these dates, they are less likely to be able to make such large changes.

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[16]I used the SSA's 200 most common baby name lists from the 1950s, 1960s, 1970s and 1980s 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) 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.

In Table 1, I compare the difference in means for the "registry required" and the "registry not required" groups, and I do in fact show that the "registry required" group has been convicted of 0.51 more previous crimes. They had also committed more crimes with child victims.

These two differences in criminal history across the groups do speak to the groups being different from each other, but they also work against each other. At this time, North Carolina lumped in many victim age-based sex offenses with "indecency with a child," which can explain its prevalence. Most non-child offenses are the most serious in the data - things like rape and first-degree sex offense. So, in effect, "child offenders" are the less-serious group. The charge of "indecency with a child" also often only leads to probation. If sentencing is easier to manipulate than release from prison, we would expect to see more individuals only serving probation in the "registry required" group.

Because the support for the identification strategy here is less clear-cut and there is some evidence of manipulation across groups, the following results need to be considered carefully. I endeavor to couch the results in the appropriate level of caution.

5.2.2 Effect of initial registry on recidivism

Figure 5 plots survival functions for the "registered" and "not registered" groups separately (indicated with a solid line and a dashed line, respectively). The survival functions represent the proportion of individuals in each group over time that have not yet recidivated with that type of offense. If there is no systematic difference across the groups, the curves will lie nearly on top of each other. For both sex offenses and drug offenses, this appears to be the case. Notably, even after 10 years, less than 10 percent of the registered offenders had committed a recidivist sex crime.

The figures indicate that registered individuals recidivate with offenses of any type, financially-motivated offenses, violent offenses and registry-related offenses sooner than those who do not have to register. After 10 years, the proportion who have not recidivated is also higher for the non-registered group for these categories of crimes. The most visually striking figure is subfigure (f) for registry-related offenses. Almost as soon as the registry began, registrants began committing this type of offense. It is possible for the "not registered" group to commit one of these offenses because the group assignment is based on the first sex offense. If someone committed another sex offense, was required to register, and then failed to comply with regulations, then they could be convicted of a registry-related offense as a member of the not registered group. Other than this group of recidivists who had to register later, the "not registered" group cannot commit a registry-related offense because they are not registered. While this result is mechanical in nature, it is non-trivial in terms of costs to the justice system.

After around 5 years, the groups are nearly indistinguishable across all other crime types in trends. The survival curves have also flattened out a bit by that point, indicating that once an individual hasn't committed a type of crime within 5 years, they are unlikely to do so within 10.

Each figure also includes a p-value from a Wilcoxon test for equality of the survival curves. This test confirms what the visual evidence indicates - the duration of time without recidivism is shorter for any type of offense, financially-motivated offenses, violent offenses and registry-related offenses for registered offenders.

Table 6 contains results from the Cox proportional hazard model described in the methods section. Each column represents a different empirical specification estimated for the types of offenses listed separately, and the reported values are hazard ratios indicating how much more or less likely a registered individual is to commit a crime of each type. Standard errors are clustered on the individual level.

Column 2 adds demographic and criminal history controls, and column 3 includes fixed effects for the quarter of release or sentencing (to control for things like labor market conditions at that time). Coefficients are generally stable after adding controls and the fixed effects, which supports that the strategic sorting effects should be limited in scope. In column 4, I also control for whether the registry website had gone live yet.

For the "any offense" general recidivism category, I find that there is an increase in the hazard rate for those individuals required to register. The effect is consistent across models and statistically significant on the 1 percent level in 3 out of 4 models (and on the 5 percent in the 4th). Only sex offenses have a coefficient less than 1, indicating a reduction, but the results are not statistically significant. Although every other crime type except sex offenses is more likely for the group required to register, the largest effects are for registry-related offenses. These results are also consistently statistically significant. Both violent and financially-motivated offenses are no longer statistically significant after adding the registry quarter fixed effects, but the coefficients are reasonably stable.

6   Discussion

In this study I analyze the effectiveness of sex offender registries at deterring recidivism. First, I consider 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 individuals from the registry when they had been registered for 10 years, but then abruptly stopped this practice. The individuals 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 they 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.

I find no evidence that registry extension has the intended effect of reducing sex crime recidivism. Sex crime recidivism is a low frequency occurrence for individuals who have been registered as sex offenders for 10 years and have not yet committed a recidivist sex crime. For this reason, I cannot rule out medium-sized relative effects, but the group whose registry expired committed 3 *less* recidivist sex offenses than the group whose registry was extended (8 vs. 11 offenses).

I do find some evidence that those required to remain registered continue to be convicted of violating the registry rules. Over the 5 year period they are observed, 5.2 percent of the individuals whose registry was extended end up in prison for a registry infraction, costing the state over $2.9 million dollars. Another 3.6 percent are sentenced to probation. I also find some weak evidence that those whose registry was extended are more likely to commit a violent crime. Although this result is not always statistically significant at conventional levels, it could hint at the social costs faced by these individuals due to registration.

Second, I compare the recidivism patterns over time for individuals who were required to register to those who were not to determine whether their recidivism trajectories were impacted by the registry. I again find no evidence that those required to register are less likely to commit a recidivist sex offense. I do find large effects indicating that sex offender registry-related offenses are affected by the registry - around 9.3 percent of offenders who were required to register were later incarcerated for registry-related offense for 680 days on average. Another 9.4 percent were sentenced to probation for such an offense, and in total around 15 percent of offenders ended up serving a punishment related to their registry status. Based on the average cost of incarceration in North Carolina in 2006, these incarcerations cost around $6.2 million.

Registrants also recidivate with other types of crimes more quickly according to the duration models. The survival curves differ for financially-motivated and violent offenses, but the most convincing evidence showing that those required to register commit recidivist crimes after a shorter duration is for the "any offense" category, which *excludes* all probation violations and regulation-related offenses. Over the initial 10 year registry period, 20 percent of those who were required to register committed a recidivist crime of some type, while only 15 percent of those who were not required to register did so. The registered group cost the State of North Carolina $1.1 million more dollars in incarceration costs than the non-registered group.

The incarceration costs only represent some of the costs associated with registries. They are costly for law enforcement to operate and costly to the offenders themselves in terms of compliance and quality of life. Policy-makers must decide whether potentially reducing recidivism in these contexts sufficiently justifies long registry periods. The federal Adam Walsh Act now requires that states have a minimum registry period of 15 years, but I have shown that registry has no detectable effect on sex offenses after 10 years. Because some states still have not adopted all of the provisions of the Act, this result is particularly important from a policy perspective.

While these results are clearly relevant for the current federal policy recommendations for sex offender registries, they can also speak to the more general issue of optimal punishment length for convicted individuals. Deterring recidivism is only a small piece of the puzzle, but these results indicate that there are few gains to be made from keeping individuals on sex offender registries for lengthy periods. I find that no types of offenses are reduced due to initial registry or the extension, and registry leads to distinguishable increases in crimes related to the registry itself, as well as other types of offenses. Like other public information campaigns to alert communities of ex-offenders' criminal

histories, sex offender registries create hurdles to social reintegration. In this case, the complex set of regulations that have to be met also create a challenge for registrants to maintain compliance, resulting in many being sent back to prison or sentenced to probation.

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Figures and Tables

Figure 1: Density of the Running Variable
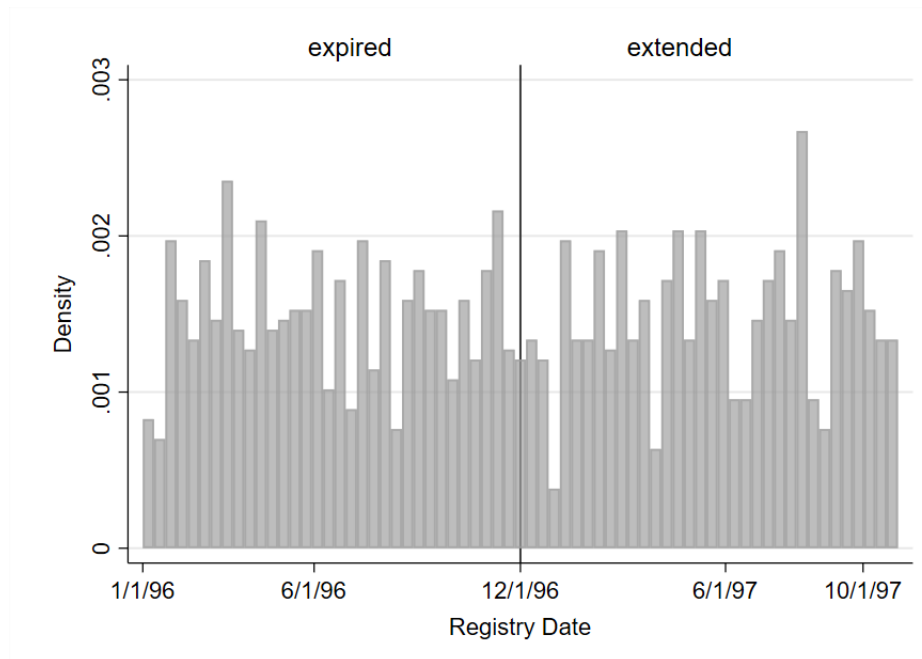
Notes: For individuals sentenced to incarceration, the registry date is estimated using release date. For individuals sentenced to probation, it is sentencing date. The vertical line denotes the effective date of the legislation. Histogram bins are 10 days.

Figure 2: Tests of RDD Specification
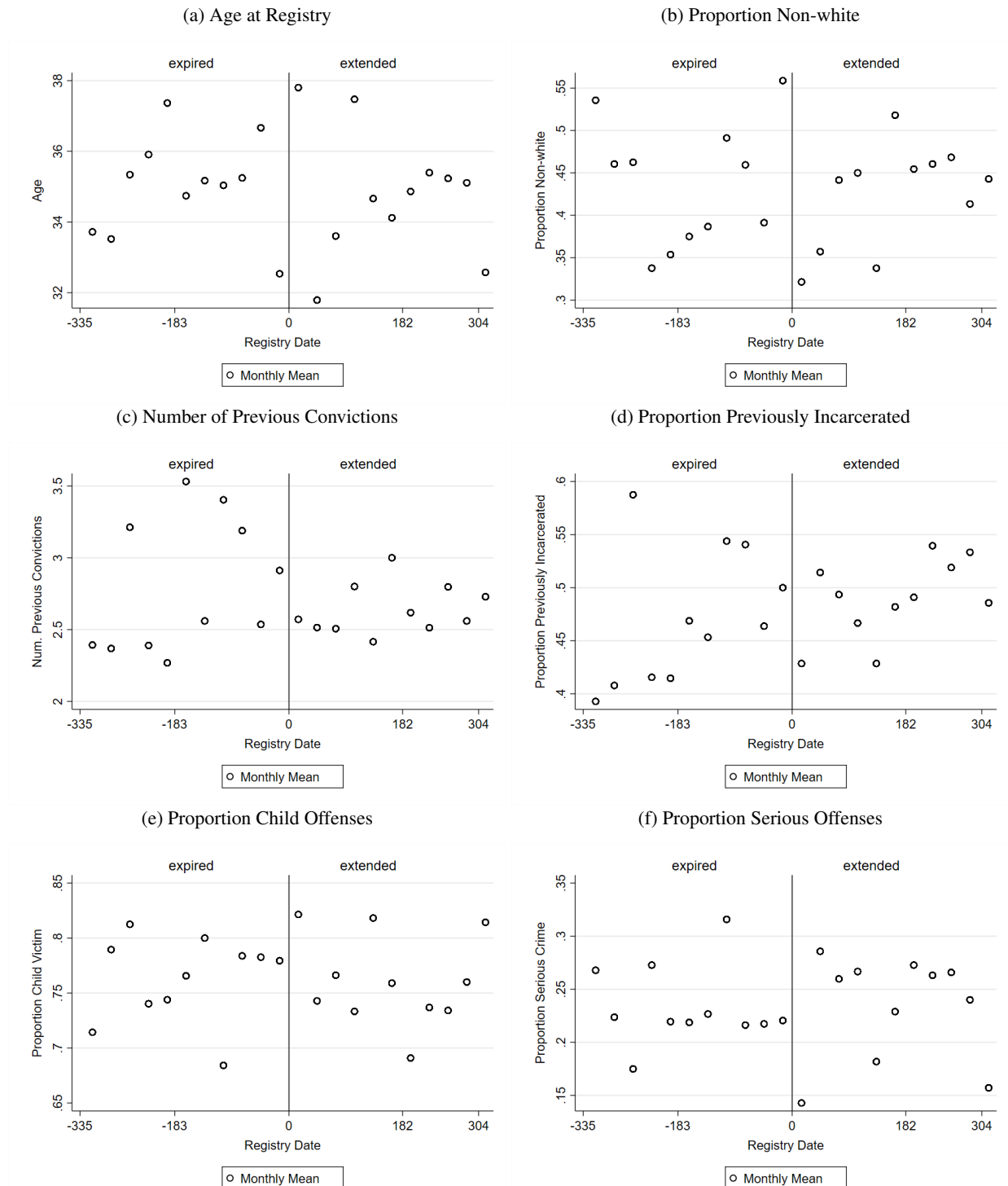
Notes: The running variable is the estimated registry date. Local averages are reported for 30 day bins. "Previous offenses" is the number of offenses for which an offender was convicted before he or she was required to register. "Previously incarcerated" is a binary indicator for whether the offender has been incarcerated before registry. "Child offenses" indicates that the individual was convicted of a crime with a child victim. "Serious offenses" are Class D or higher felony sex offenses (e.g. rape), and for both "child offenses" and "serious offenses" the outcome variable is whether the individual was convicted of one or more offenses of those types.

Figure 3: Effect of Registry Extension on Continued Registry

(a) Sample Group: Proportion Registered
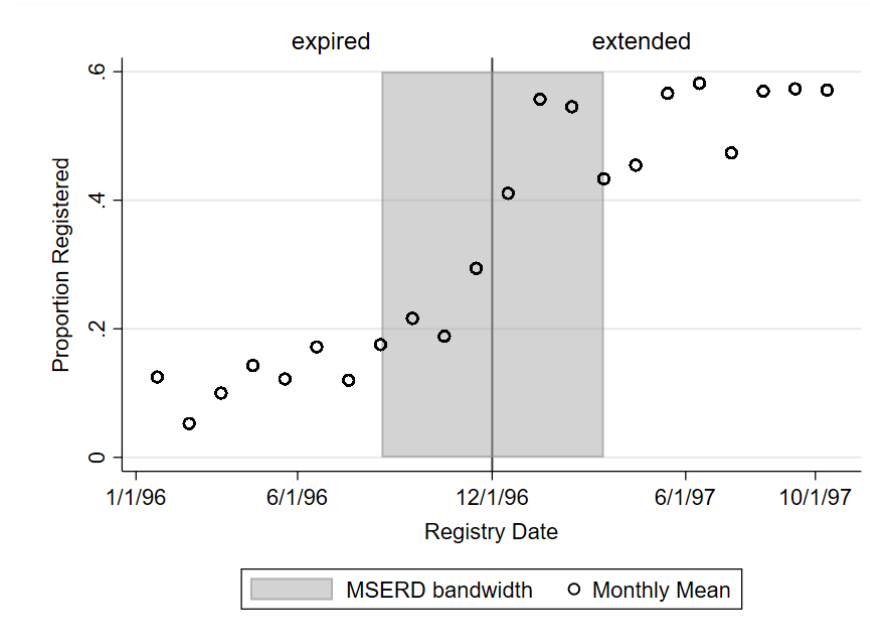
(b) Proportion Registered by Time Since Initial Registry
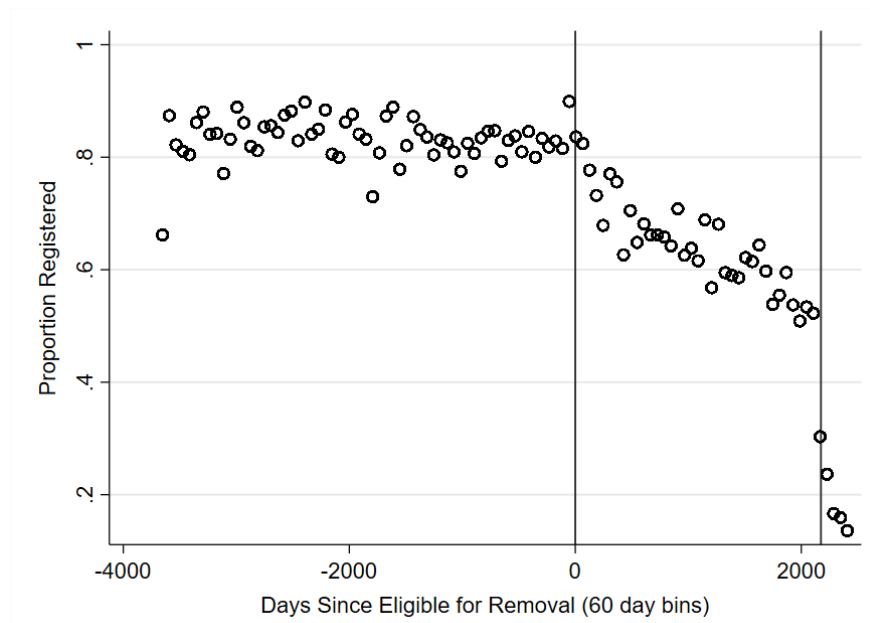
Notes: Here "registered" denotes that the individual was registered on Nov. 13, 2012. In panel (a) the vertical line denotes the law's effective date, and the x-axis is the estimated registry date. Averages are for 30 day bins. Panel (b) includes out-of-sample evidence on continued registry status of individuals registered up to Nov. 13, 2012. The leftmost vertical line separates those eligible for removal (right) from those ineligible (left) as of Nov. 13, 2012, and the rightmost separates those eligible as of Dec. 1, 2006 (the automatic expiration group). The x-axis is how many days an individual has been eligible for removal (and negative values indicate that they are not yet eligible). Averages are for 60 day bins.

Figure 4: Effect of Registry Extension on Recidivism Within 5 Years
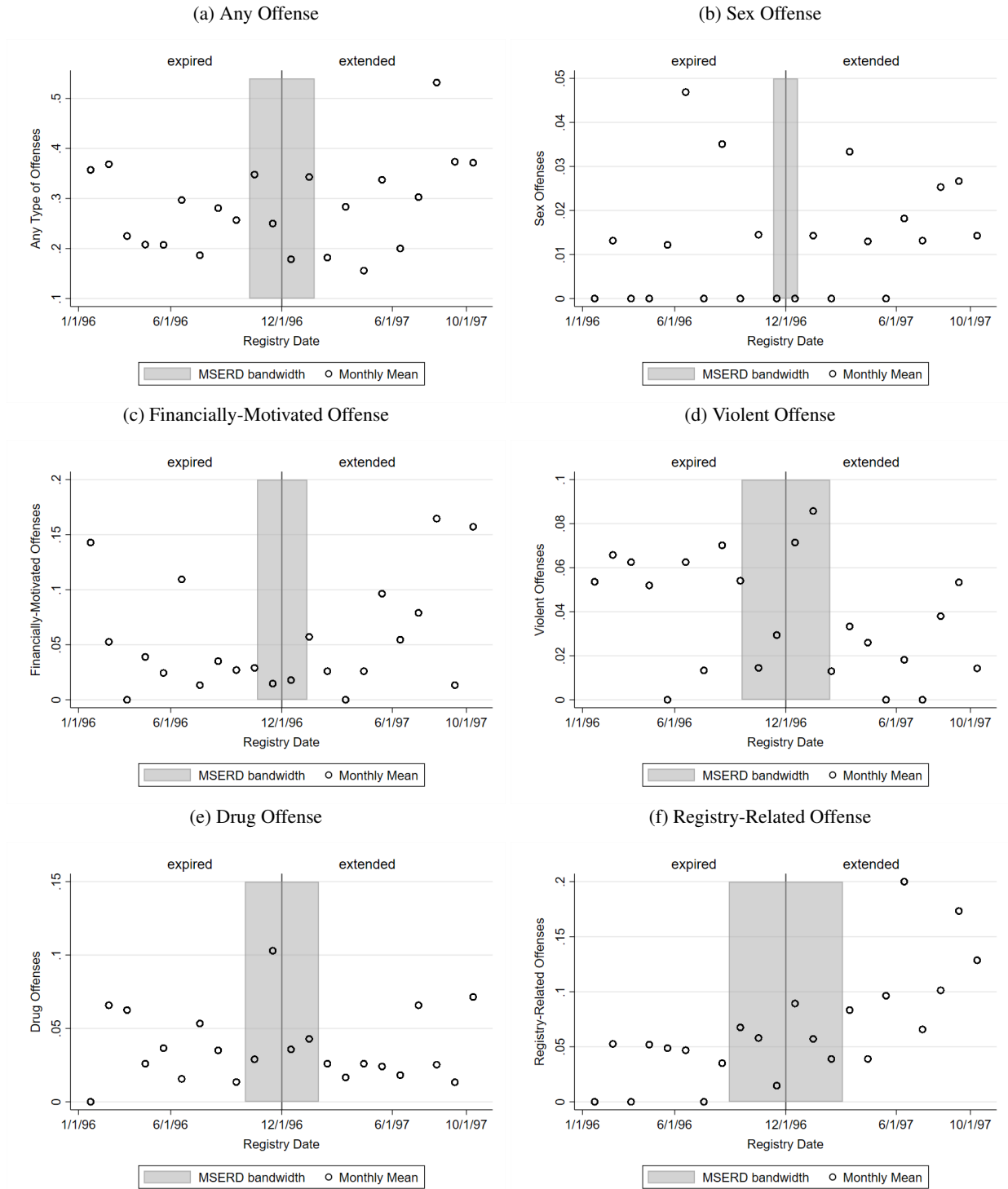
(a) Any Offense

(b) Sex Offense

(c) Financially-Motivated Offense

(d) Violent Offense

(e) Drug Offense

(f) Registry-Related Offense

Notes: Recidivism is calculated for the 5 years after registry expiration or extension. The running variable is the individual's estimated registry date. The vertical line denotes the law's effective date, and the shaded area represents the MSERD-optimal bandwidth for a linear model with controls estimated using a uniform kernel. Local averages are for 30 day bins.

Figure 5: Effect of Registry on Recidivism Over First 10 Years

(a) Any Offense

(b) Sex Offense

(c) Financially-Motivated Offense

(d) Violent Offense

(e) Drug Offense

(f) Registry-Related Offense
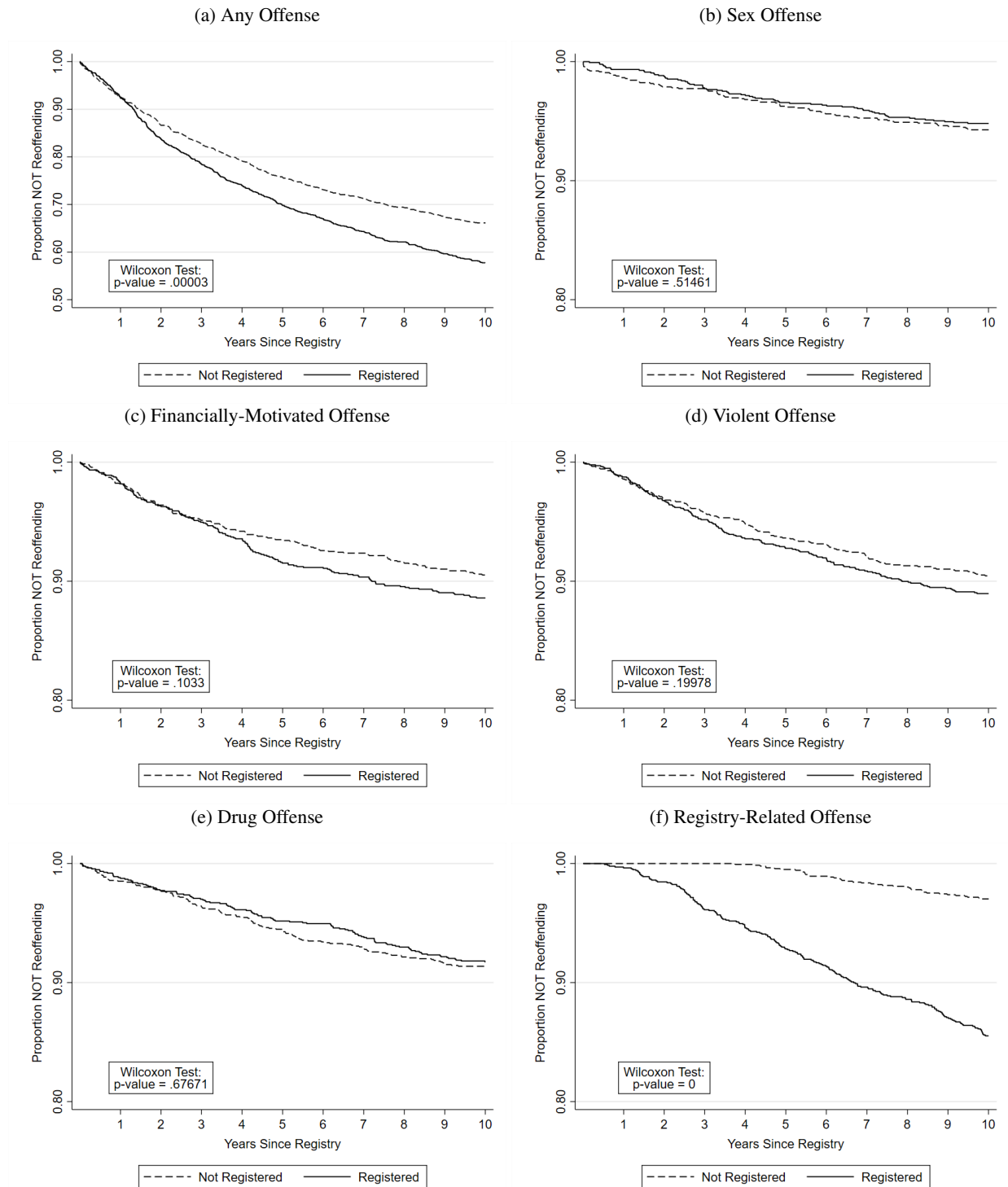
Notes: Recidivism is measured in days since registry or would-be registry. Survival curves are calculated separately for the "not registered" (dashed lines) and "registered" (solid lines) groups. P-values from a Wilcoxon test for equality between the survival curves for the two groups is reported in the bottom left corner of each graph.

Table 1: Summary Statistics

	registry expired	registry extended	difference		NOT required	registry required	difference	
First stage and controls								
Registered	0.152	0.527	0.375	***	0.141	0.454	0.313	***
	(0.36)	(0.50)			(0.35)	(0.50)		
Proportion Black	0.399	0.368	-0.031		0.366	0.382	0.016	
	(0.49)	(0.48)			(0.48)	(0.49)		
Proportion Hispanic	0.003	0.003	0		0.005	0.001	-0.004	
	(0.05)	(0.05)			(0.07)	(0.04)		
Proportion Male	0.991	0.984	-0.007		0.982	0.986	0.004	
	(0.09)	(0.13)			(0.13)	(0.12)		
Age at Registry	35.096	34.702	-0.394		34.923	34.592	-0.331	
	(11.91)	(12.83)			(12.32)	(12.39)		
Num. Previous Convictions	2.792	2.647	-0.145		2.269	2.779	0.51	***
	(3.26)	(2.93)			(2.51)	(3.15)		
Previously Incarcerated	0.476	0.492	0.016		0.471	0.501	0.03	
	(0.50)	(0.50)			(0.50)	(0.50)		
Child Victim	0.765	0.763	-0.002		0.729	0.758	0.029	*
	(0.42)	(0.43)			(0.44)	(0.43)		
Serious Crime	0.232	0.232	0		0.261	0.239	-0.022	
	(0.42)	(0.42)			(0.44)	(0.43)		
Outcomes: in 5 years post extension/registry								
Any Offense	0.265	0.302	0.037		0.344	0.287	-0.057	*
	(0.75)	(0.89)			(1.01)	(0.82)		
Sex Offense	0.01	0.014	0.004		0.039	0.015	-0.024	
	(0.14)	(0.13)			(0.40)	(0.13)		
Financially-Motivated Offense	0.041	0.066	0.025		0.073	0.048	-0.025	*
	(0.31)	(0.41)			(0.46)	(0.33)		
Violent Offense	0.042	0.03	-0.012		0.053	0.034	-0.019	
	(0.22)	(0.20)			(0.27)	(0.20)		
Drug Offense	0.041	0.036	-0.005		0.064	0.037	-0.027	
	(0.23)	(0.23)			(0.34)	(0.23)		
Registry-Related Offense	0.034	0.094	0.06	***	0.039	0.083	0.044	***
	(0.20)	(0.33)			(0.22)	(0.31)		
num. of observations	784	788			1415	1368		

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

Notes: Rows represent different covariates and report means, the difference in means across relevant groups and standard deviations (in parentheses).

Table 2: Identification Test: Discontinuity in Controls at Cutoff

	(1)	(2)
Age	-0.172	1.149
	(1.234)	(1.831)
Proportion Non-white	-0.067	-0.251***
	(0.051)	(0.077)
Num. Previous Convictions	-0.494	-0.550
	(0.310)	(0.436)
Proportion Previously Incarcerated	-0.058	-0.074
	(0.054)	(0.085)
Proportion Child Victim	-0.002	0.033
	(0.041)	(0.061)
Proportion Serious Crime	0.008	-0.043
	(0.044)	(0.062)
num. of observations	1572	1572
controls	no	no
time polynomial	linear	quad

$^{*}\, p < .10,\ ^{**}\, p < .05,\ ^{***}\, p < .01$

Notes: 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: Discontinuity in Registry at Cutoff

	(1)	(2)	(3)	(4)
Registered in 2012	0.229***	0.223***	0.220**	0.187**
	(0.045)	(0.069)	(0.086)	(0.084)
num. of observations	1572	1572	1572	1572
left obs			249	306
right obs			237	291
MSERD bandwidth			105	134
time polynomial	linear	quad	linear	linear
kernel	-	-	uniform	triangular

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

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." Control variables include race, gender, age, the number of pre-registry convictions for any type of offense, and binary indicators for whether the individual had been previously incarcerated, committed a crime against a child or committed a serious offense. Robust standard errors are reported in parentheses and clustered on the running variable. Columns 1 and 2 use the full (22 month) bandwidth.

Table 4: Effect of Registry Extension on Recidivism

	Uniform Kernel			Triangular Kernel		
	(1)	(2)	(3)	(4)	(5)	(6)
Any Offense						
Extension	-0.156	0.036	0.047	-0.151	-0.009	0.060
	(0.168)	(0.162)	(0.245)	(0.142)	(0.136)	(0.185)
num. of observations	1572	1572	1572	1572	1572	1572
left obs	131	131	131	177	177	177
right obs	114	114	114	170	170	170
MSERD bandwidth	55	55	55	79	79	79
Sex Offense						
Extension	0.000	0.000	0.000	0.001	0.002	-0.003
	(.)	(.)	(.)	(0.002)	(0.002)	(0.005)
num. of observations	1572	1572	1572	1572	1572	1572
left obs	53	53	53	135	135	135
right obs	53	53	53	126	126	126
MSERD bandwidth	22	22	22	60	60	60
Financially-Motivated Offense						
Extension	-0.016	-0.007	0.034	-0.006	0.006	0.010
	(0.029)	(0.029)	(0.034)	(0.024)	(0.024)	(0.024)
num. of observations	1572	1572	1572	1572	1572	1572
left obs	99	99	99	207	207	207
right obs	90	90	90	202	202	202
MSERD bandwidth	43	43	43	89	89	89
Violent Offense						
Extension	0.056	0.081*	0.058	0.070	0.092**	0.065
	(0.048)	(0.047)	(0.063)	(0.045)	(0.044)	(0.055)
num. of observations	1572	1572	1572	1572	1572	1572
left obs	169	169	169	261	261	261
right obs	163	163	163	260	260	260
MSERD bandwidth	74	74	74	114	114	114
Drug Offense						
Extension	-0.116	-0.090	-0.072	-0.092	-0.068	-0.088
	(0.077)	(0.076)	(0.115)	(0.069)	(0.067)	(0.100)
num. of observations	1572	1572	1572	1572	1572	1572
left obs	146	146	146	228	228	228
right obs	130	130	130	222	222	222
MSERD bandwidth	62	62	62	97	97	97
Registry-Related Offense						
Extension	0.092*	0.101*	0.213**	0.125**	0.132**	0.204**
	(0.053)	(0.053)	(0.090)	(0.063)	(0.063)	(0.097)
num. of observations	1572	1572	1572	1572	1572	1572
left obs	224	224	224	231	231	231
right obs	221	221	221	223	223	223
MSERD bandwidth	95	95	95	99	99	99
controls	no	yes	yes	no	yes	yes
time polynomial	linear	linear	quad	linear	linear	quad

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

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism

Table 5: D-i-D Model: Effect of Extension on Recidivism

	Any Offense	Sex Offense	Financially-Motivated Offense	Violent Offense	Drugs Offense	RSO Offense
Panel A:						
registry extended	0.012	0.001	0.004	-0.001	0.000	0.013***
	(0.010)	(0.001)	(0.005)	(0.004)	(0.004)	(0.004)
Panel B:						
year after extension	0.036*	0.001	-0.005	0.001	0.008	0.023***
	(0.021)	(0.004)	(0.011)	(0.005)	(0.008)	(0.008)
2nd year	-0.001	0.003	-0.006	-0.001	0.002	0.009
	(0.019)	(0.002)	(0.007)	(0.006)	(0.008)	(0.008)
3rd year	0.031	-0.001	0.016	-0.006	0.005	0.009
	(0.021)	(0.005)	(0.012)	(0.006)	(0.005)	(0.007)
4th year	-0.004	0.003	0.009	-0.008	-0.005	0.014***
	(0.019)	(0.002)	(0.009)	(0.006)	(0.005)	(0.006)
5th year	0.003	-0.001	0.001	-0.004	0.002	0.025***
	(0.017)	(0.001)	(0.007)	(0.006)	(0.006)	(0.007)
year before	0.000	0.000	-0.002	-0.005	0.002	0.013
	(0.019)	(.)	(0.010)	(0.007)	(0.007)	(0.009)
2 years before	0.006	0.000	-0.003	-0.005	0.008	0.003
	(0.021)	(.)	(0.010)	(0.008)	(0.007)	(0.008)
num. of observations	15720	15720	15720	15720	15720	15720
year FEs	yes	yes	yes	yes	yes	yes
individual FEs	yes	yes	yes	yes	yes	yes

$^{*}\ p < .10,\ ^{**}\ p < .05,\ ^{***}\ p < .01$

Notes: Panel A includes the results from average effects difference-in-differences models. Each value is from a separate regression. Panel B reports results from an event study difference-in-differences model, and each column within the panel is generated by a single regression. Column labels denote the outcome variable used, and fixed effects for individuals and years since initial registry are included. Standard errors are clustered on the individual level and included in parentheses.

Table 6: Duration Models: Effect of Registry on Recidivism

	(1)	(2)	(3)	(4)
Any Offense				
Registered	1.313***	1.326***	1.469*	1.489*
	(0.081)	(0.083)	(0.329)	(0.338)
Registry Online				0.964
				(0.127)
Sex Offense				
Registered	0.902	0.892	0.741	0.763
	(0.147)	(0.146)	(0.425)	(0.445)
Registry Online				0.929
				(0.342)
Financially-Motivated Offense				
Registered	1.214	1.205	1.087	0.872
	(0.143)	(0.143)	(0.432)	(0.355)
Registry Online				1.739**
				(0.430)
Violent Offense				
Registered	1.165	1.177	1.111	1.121
	(0.138)	(0.142)	(0.479)	(0.498)
Registry Online				0.978
				(0.227)
Drug Offense				
Registered	0.951	0.914	0.710	0.806
	(0.124)	(0.120)	(0.331)	(0.382)
Registry Online				0.722
				(0.207)
Registry-Related Offense				
Registered	5.227***	5.228***	19.384***	20.233***
	(0.883)	(0.886)	(14.646)	(15.431)
Registry Online				0.914
				(0.286)
num. of observations	5500	5496	5496	5496
controls	no	yes	yes	yes
registry quarter FEs	no	no	yes	yes
control for online access	no	no	no	yes

$^* p < .10,\ ^{**} p < .05,\ ^{***} p < .01$

Notes: Results are hazard ratios from Cox proportional hazards models where the outcome variables are each of the 6 listed crime types. Control variables include race, gender, age, the number of pre-registry convictions for any type of offense, and binary indicators for whether the individual had been previously incarcerated, committed a crime against a child or committed a serious offense. Column 4 also reports the coefficient on a binary indicator for whether the registry has yet been posted online. Robust standard errors are reported in parentheses and clustered on the individual-level.

Appendix

Appendix Note: List of Offenses by category

Any Offenses: all charges (except probation revocation, registry offenses and "procedural offenses")

Sex Offenses: rape, sex offenses, incest, sexual exploitation of a minor, indecent liberty with a minor
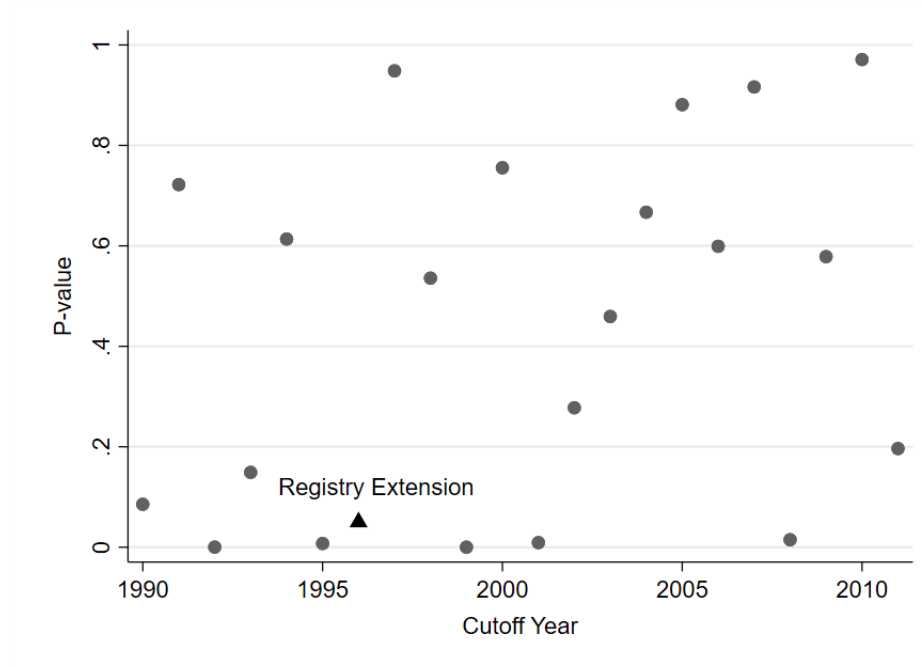
Financially-Motivated Offenses: theft, fraud, breaking and entering, burglary

Violent Offenses: murder, assault, robbery

Drug Offenses: trafficking, possessing, manufacturing drugs

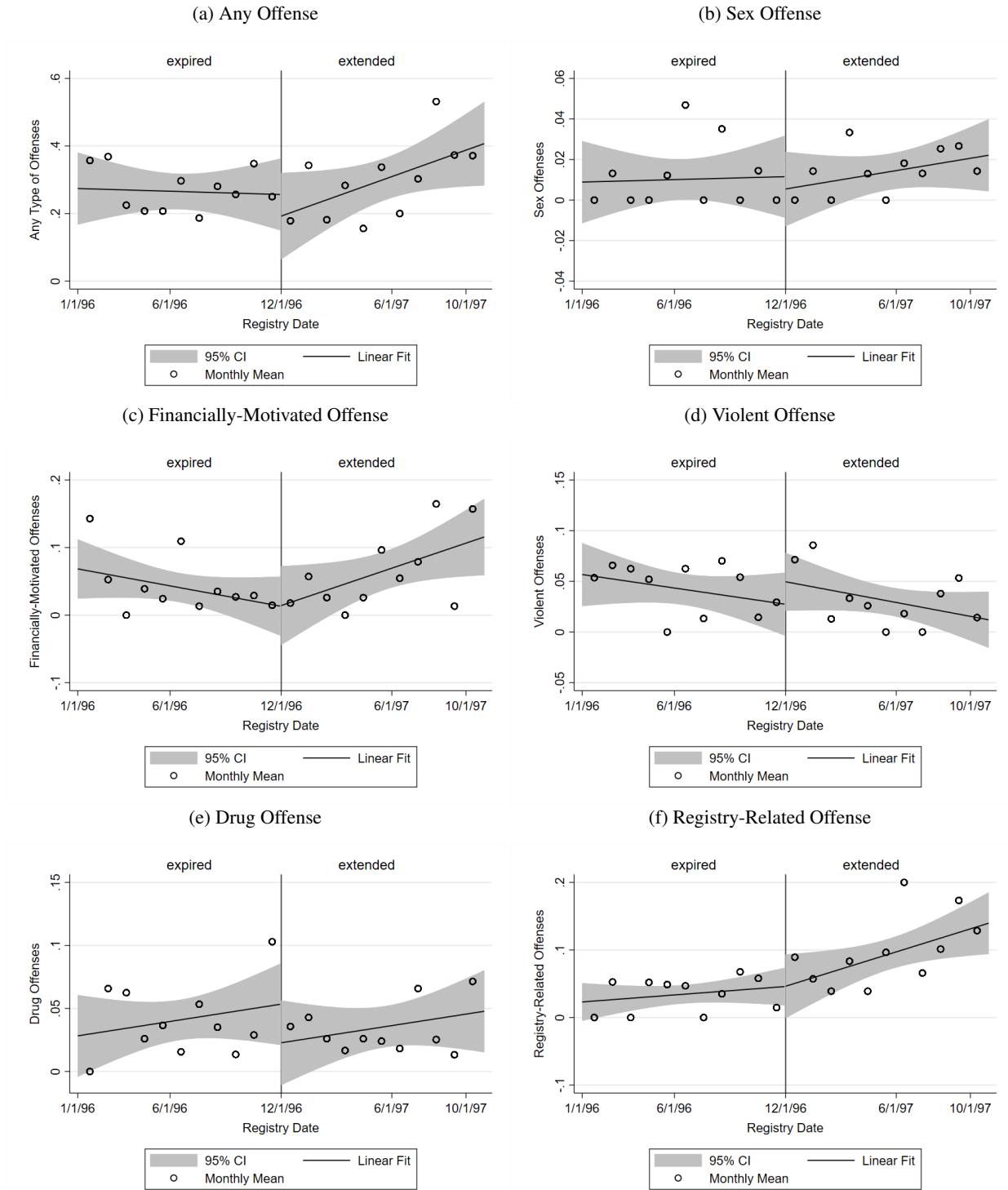**Registry Offenses:** failure to register, update address or comply with other rules

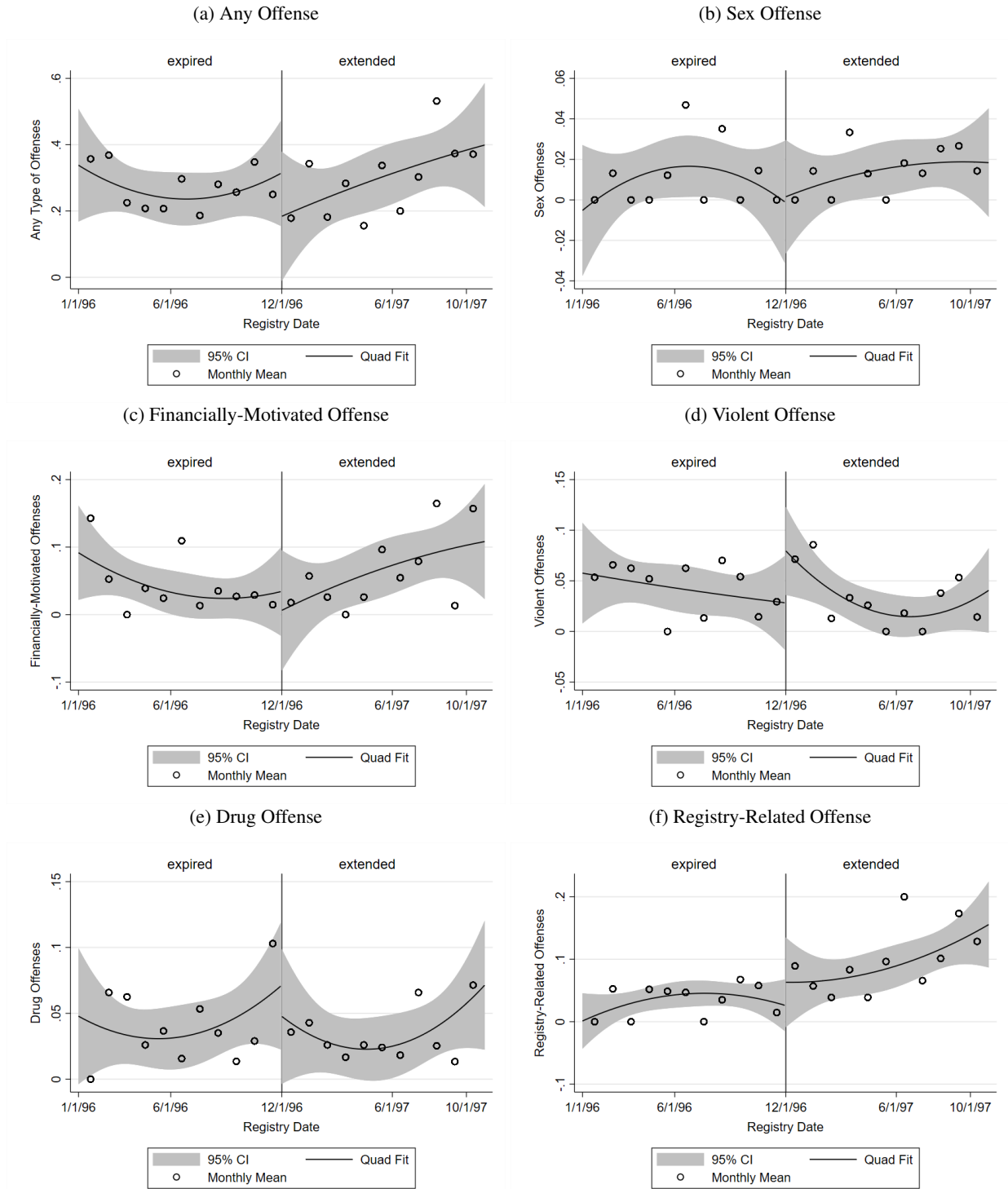Figure A1: Placebo Test for Cutoff Manipulation

Notes: Each point represents the p-value from a test for manipulation around an RD cutoff. The placebo cutoffs are represented with circles and they are plotted against the year they were calculated for on the x-axis. The real cutoff was in 1996, and that point is represented with a triangle.

Figure A2: Effect of Registry Extension on Recidivism Within 5 Years: Linear Fits

(a) Any Offense

(b) Sex Offense

(c) Financially-Motivated Offense

(d) Violent Offense

(e) Drug Offense

(f) Registry-Related Offense

Notes: Recidivism is calculated for the 5 years after registry expiration or extension. The running variable is the individual's estimated registry date. The vertical line denotes the law's effective date, and the shaded area represents the MSERD-optimal bandwidth for a linear model with controls estimated using a uniform kernel. Local averages are for 30 day bins.

Figure A3: Effect of Registry Extension on Recidivism Within 5 Years: Quadratic Fits

(a) Any Offense

(b) Sex Offense

(c) Financially-Motivated Offense

(d) Violent Offense
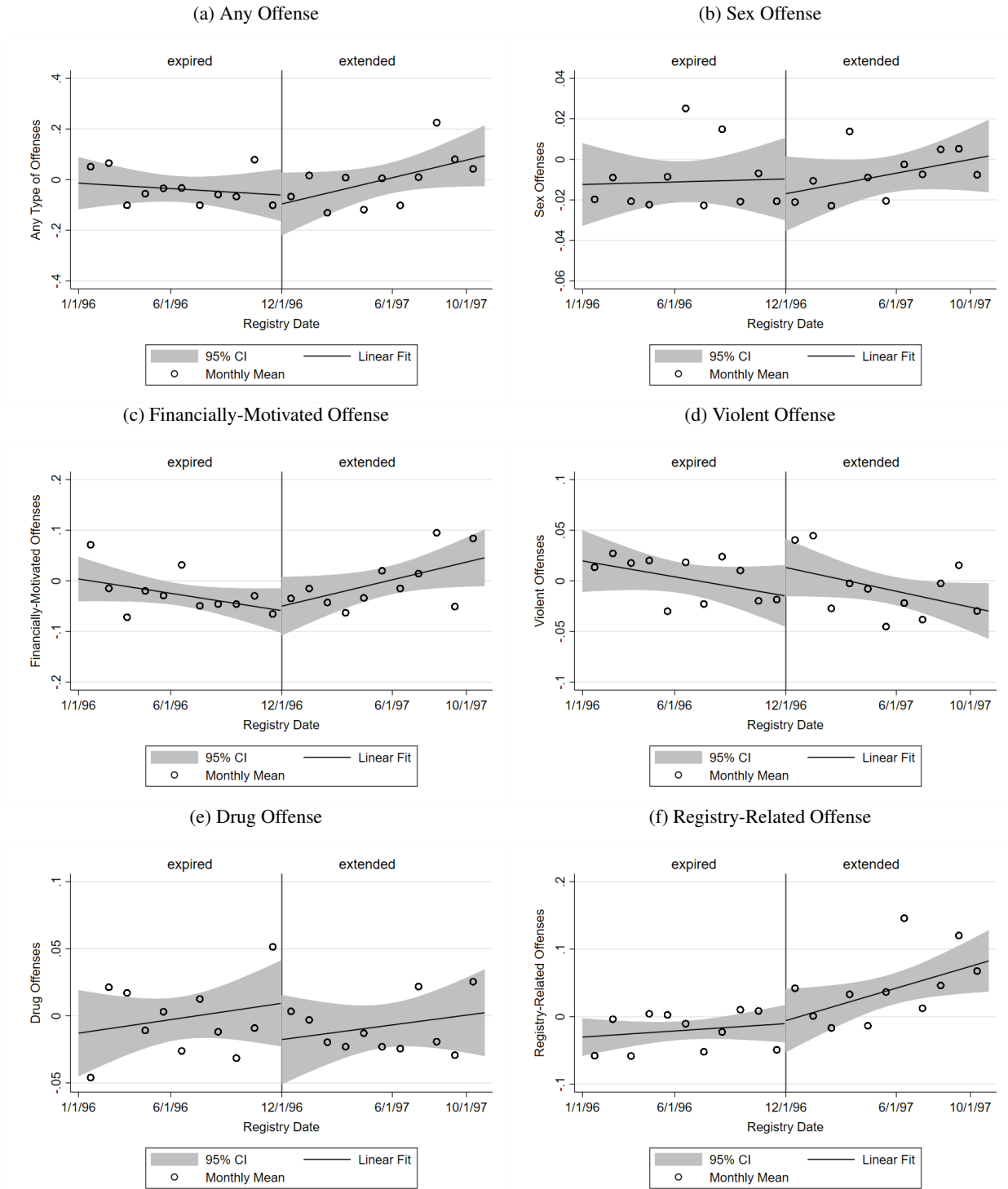
(e) Drug Offense

(f) Registry-Related Offense

Notes: Recidivism is calculated for the 5 years after registry expiration or extension. The running variable is the individual's estimated registry date. The vertical line denotes the law's effective date, and the shaded area represents the MSERD-optimal bandwidth for a linear model with controls estimated using a uniform kernel. Local averages are for 30 day bins.

Figure A4: Effect of Registry Extension on Recidivism Within 5 Years: Residualized with Linear Fits

(a) Any Offense

(b) Sex Offense

(c) Financially-Motivated Offense

(d) Violent Offense

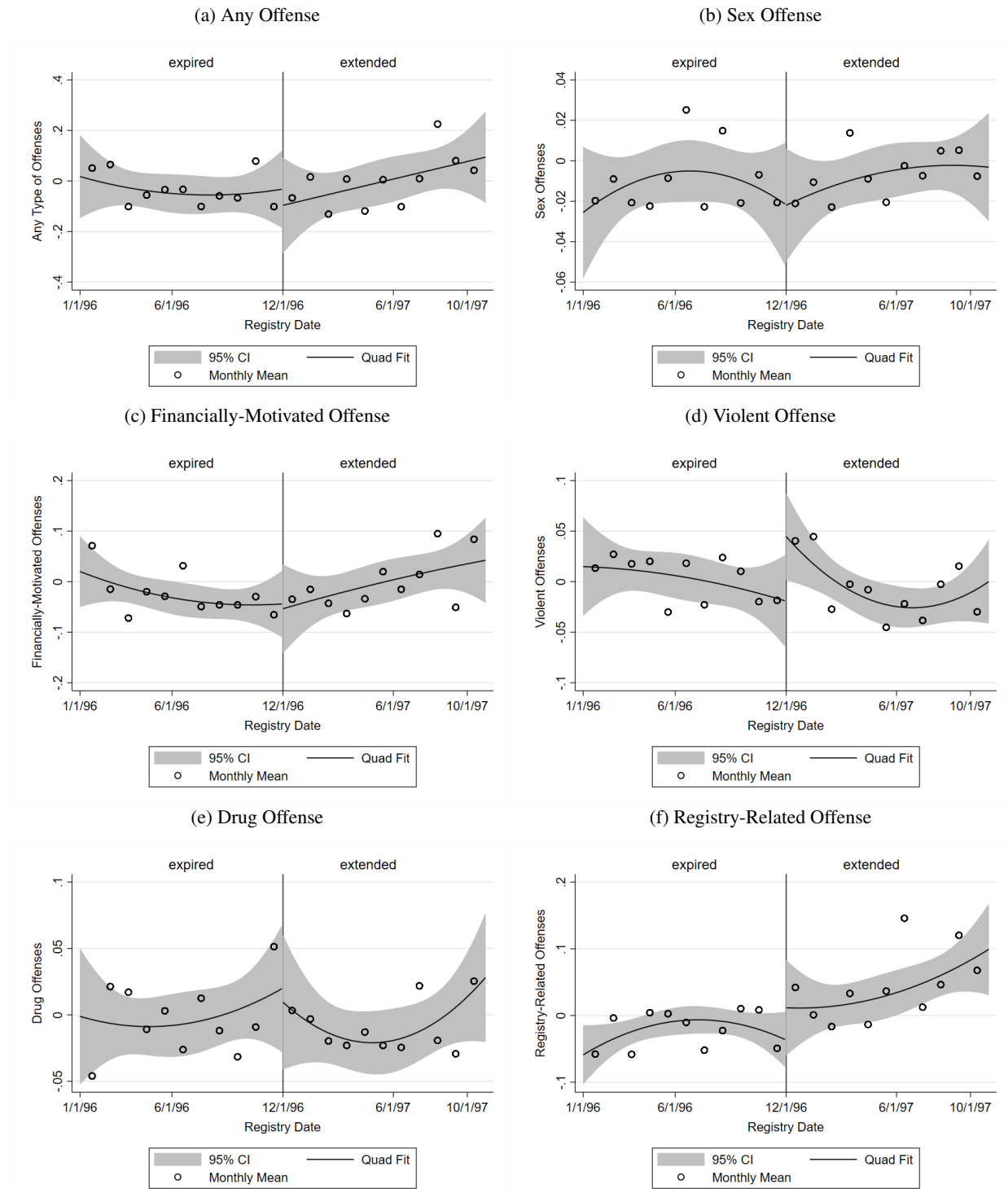(e) Drug Offense

(f) Registry-Related Offense

Notes: Recidivism is calculated for the 5 years after registry expiration or extension. The running variable is the individual's estimated registry date. The vertical line denotes the law's effective date, and the shaded area represents the MSERD-optimal bandwidth for a linear model with controls estimated using a uniform kernel. Local averages are for 30 day bins.

Figure A5: Effect of Registry Extension on Recidivism Within 5 Years: Residualized with Quadratic Fits

(a) Any Offense

(b) Sex Offense

(c) Financially-Motivated Offense

(d) Violent Offense

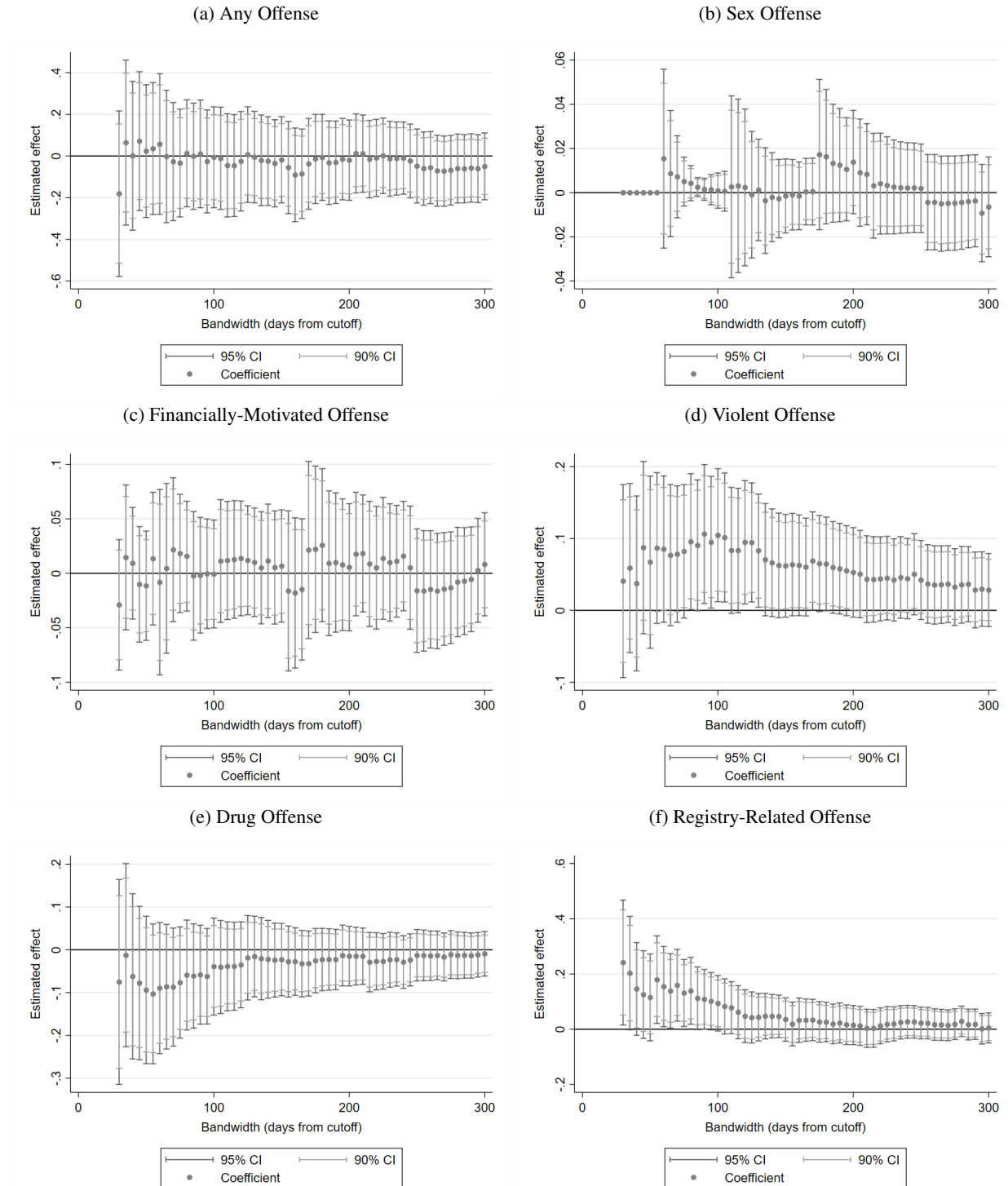(e) Drug Offense

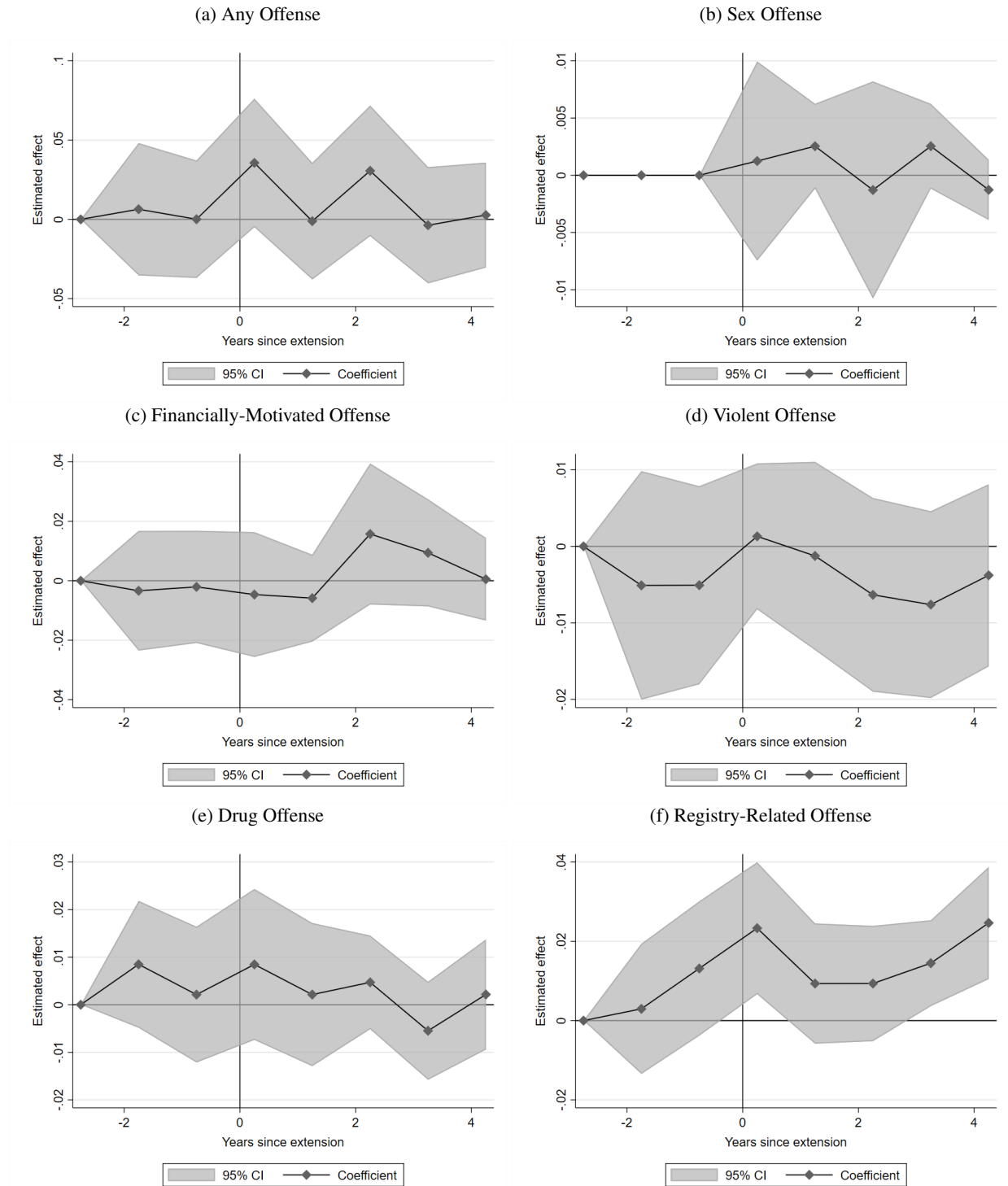(f) Registry-Related Offense

Notes: Recidivism is calculated for the 5 years after registry expiration or extension. The running variable is the individual's estimated registry date. The vertical line denotes the law's effective date, and the shaded area represents the MSERD-optimal bandwidth for a linear model with controls estimated using a uniform kernel. Local averages are for 30 day bins.

Figure A6: Bandwidth Sensitivity Analysis

(a) Any Offense

(b) Sex Offense

(c) Financially-Motivated Offense

(d) Violent Offense

(e) Drug Offense

(f) Registry-Related Offense

Notes: Each figure plots the results (coefficients, 95% confidence intervals and 90% confidence intervals) from RD regressions with controls and a linear function of the running variable, like column 2 of Table 4. In each subfigure, the bandwidth varies from 30 to 300 days in increments of 5 days.

Figure A7: Difference-in-Differences Figures

(a) Any Offense

(b) Sex Offense

(c) Financially-Motivated Offense

(d) Violent Offense

(e) Drug Offense

(f) Registry-Related Offense

Notes: These figures plot results (coefficients and 95% confidence intervals) for event study difference-in-difference models. The models include fixed effects for individuals and the number of years since the individual originally registered as a sex offender. Effects are measured compared to 3 or more years before the registry extension. All coefficients to the right of the horizontal line represent treatment effects, and those to the left represent leading indicators.

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Table A1: Effect of Registry Extension on Recidivism - Varying Samples

	Drop 10 Days		Drop December		Max Bandwidth	
	(1)	(2)	(3)	(4)	(5)	(6)
Any Offense						
Extension	-0.026	-0.077	-0.176	-0.117	-0.036	-0.040
	(0.207)	(0.174)	(0.633)	(0.271)	(0.079)	(0.082)
num. of observations	1558	1558	1516	1516	1572	1572
left obs	118	164	131	177	784	783
right obs	113	169	58	114	788	788
MSERD bandwidth	55	79	55	79	335	335
Sex Offense						
Extension	0.000	0.003	0.008	-0.016	-0.006	-0.002
	(.)	(0.003)	(0.014)	(0.018)	(0.011)	(0.009)
num. of observations	1558	1558	1516	1516	1572	1572
left obs	40	122	235	135	784	783
right obs	52	125	174	70	788	788
MSERD bandwidth	22	60	100	60	335	335
Financially-Motivated Offense						
Extension	-0.021	0.001	-0.467	-0.014	0.010	0.001
	(0.047)	(0.032)	(0.432)	(0.052)	(0.028)	(0.022)
num. of observations	1558	1558	1516	1516	1572	1572
left obs	86	194	99	207	784	783
right obs	89	201	34	146	788	788
MSERD bandwidth	43	89	43	89	335	335
Violent Offense						
Extension	0.065	0.084*	0.179	0.123	0.028	0.042
	(0.052)	(0.048)	(0.131)	(0.095)	(0.024)	(0.027)
num. of observations	1558	1558	1516	1516	1572	1572
left obs	156	248	169	261	784	783
right obs	162	259	107	204	788	788
MSERD bandwidth	74	114	74	114	335	335
Drug Offense						
Extension	-0.113	-0.083	-0.140*	-0.090	-0.028	-0.019
	(0.090)	(0.078)	(0.081)	(0.069)	(0.026)	(0.030)
num. of observations	1558	1558	1516	1516	1572	1572
left obs	133	215	146	228	784	783
right obs	129	221	74	166	788	788
MSERD bandwidth	62	97	62	97	335	335
Registry-Related Offense						
Extension	0.101*	0.138**	0.053	0.077	0.005	0.020
	(0.056)	(0.066)	(0.067)	(0.072)	(0.026)	(0.029)
num. of observations	1558	1558	1516	1516	1572	1572
left obs	211	218	224	231	784	783
right obs	220	222	165	167	788	788
MSERD bandwidth	95	99	95	99	335	335
controls	yes	yes	yes	yes	yes	yes
time polynomial	linear	linear	linear	linear	linear	linear
kernel	uniform	triangular	uniform	triangular	uniform	triangular

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

Table A2: 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 sub-sample 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.